Policy Emulation or Policy Convergence? Potential Ambiguities in the Dyadic Event History Approach to State Policy Emulation

Frederick J. Boehmke

Associate Professor of Political Science

University of Iowa

Department of Political Science

341 Schaeffer Hall

Iowa City, IA 52242

frederick-boehmke@uiowa.edu

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Abstract

I demonstrate a source of bias in the common implementation of the dyadic event history model as applied to policy diffusion. This bias tends to severely overstate the extent to which policy changes depend on explicit emulation of other states rather than on a state's internal characteristics. This happens because the standard implementation conflates policy emulation and policy adoption: since early adopters are policy leaders, later adopters will appear to emulate them, even if they are acting independently. I demonstrate this ambiguity analytically and through Monte Carlo simulation. I then propose a simple modification of the dyadic emulation model that conditions on the opportunity to emulate and show that it produces much more accurate findings. An examination of state pain management policy illustrates the inferential differences that arise from the appropriately modified dyadic event history model.

How do public policies diffuse from one political entity to another? Research on this question has witnessed a resurgence in the last few years as scholars bring new statistical approaches and new ways of thinking about it (see Karch 2007 for a recent review). As early work that focused on the general pattern of diffusion (e.g., Walker 1969; Gray 1973; Collier and Messick 1975) gave way to the use of event history analysis to study the influence of both internal and external characteristics on policy diffusion (Berry and Berry 1990), current research has also progressed through the use of more sophisticated measures and models (Boehmke 2009). These advances have helped us develop a better understanding about how cross-border pressures influence diffusion (e.g., Berry and Baybeck 2005, Boehmke and Witmer 2004), how policies diffuse across different levels of government (Shipan and Volden 2006, 2008), and how diffusion operates through peer networks that go beyond contiguity (Brinks and Coppedge 2006).

Perhaps the most exciting development in this area is the dyadic event history approach proposed by Volden (2006).[1] While traditional diffusion studies focus on contiguity as a pathway for diffusion (e.g., Mooney 2001), the dyadic approach expands this by considering all pairs of states and then explicitly estimating through which pathways diffusion processes flow. This approach mimics the structure of directed dyad models used to study international conflict. In the policy context, rather than study whether one country initiates conflict with another, the dyadic approach evaluates whether policy in one state moves closer to policy in a second state, allowing scholars to study patterns of policy diffusion between all pairs of states.

This new approach makes a valuable advance by allowing a vastly richer specification of the diffusion process between pairs of states and, consequently, a more precise comparison of the role of external forces with internal political and demographic characteristics. For example, an important debate in this literature involves the use of contiguity or regional proximity as a proxy for peer groups. Scholars employing a monadic approach must define a peer group and then measure average ideological similarity (e.g., Grossback, Nicholson-Crotty, and Peterson 2004) or average program success (e.g., Meseguer 2006) within that peer group. In contrast, a dyadic analysis allows scholars to more accurately and flexibly measure the peer relationship between each pair of states, perhaps with ideological distance (e.g., Volden 2006), or with shared organizational memberships and trade flows (e.g., Holzinger, Knill, and Sommerer 2008) or Meseguer (2006).

Importantly, Volden's (2006) application of dyadic event history analysis to state Children's Health Insurance Program policy finds that internal state characteristics have little effect on program modifications whereas external characteristics of leader states, such as wealth, and relative characteristics, including similar government ideology, have statistically and substantively large effects on emulation. Appropriately, then, the dyadic event history approach brings the literature closer to Walker's (1969) original focus on policy leadership rather than merely policy adoption. Not surprisingly, then, scholars have quickly applied the dyadic approach to understand policy diffusion or convergence at the subnational level both in the United States (Shipan and Volden 2007) and in other countries (Gilardi and Füglister 2008) as well as at the national level across countries (Gilardi 2008).

Given the potential theoretical and empirical value of this relatively new approach and its widening application across subfields, it is important to know the extent to which the conclusions that we draw are based on true policy learning and diffusion processes or are possibly statistical artifacts of model specification. Unfortunately, as I show in this paper, the dyadic event history approach has the potential to greatly overstate the role of external forces relative to internal forces. This tendency varies with characteristics of the policy in question and the course of adoption, but under fairly common circumstances can produce evidence of policy emulation even when none exists.

Intuitively, this bias can be understood with a simple example. Assume that wealthier states are more likely to adopt a policy. At first, a few wealthy states will become policy leaders by adopting. Then, after a few years, most wealthy states will have the policy while most poorer states will not. Wealthy states that adopt after the few early adopters will appear to emulate those leaders when, in fact, they are merely responding in similar ways to their own characteristics. In the next section, I analytically demonstrate that because the dependent variable in the dyadic event history model depends on both policy change in the potential emulator state and the existence of leader states for them to consider emulating, characteristics of leader states that influence the opportunity to emulate also influence the probability of apparent emulation.[2]

A remedy to this problem follows by conditioning on emulation opportunity by eliminating dyads in which leader states do not exist because their policies do not differ. I refer to this as the conditional dyadic event history approach and compare its performance to the usual (i.e., unconditional) dyadic event history model through Monte Carlo analysis. The simulations indicate that emulation bias can be a serious concern — over 90% of the trials produce statistically significant evidence of emulation when none exists — and that the conditional dyadic model performs much better. Both approaches are then applied to state adoption of pain management policy for end of life care and the results indicate that conditioning on opportunity greatly reduces the estimated effect of external forces on diffusion.

A Source of Potential Bias in the Dyadic Event History Model

Before analytically demonstrating the source of bias in the dyadic event history model, I first define some terms. Studies that apply the dyadic event history model generally seek to test for policy emulation, which I define as a situation in which a state intentionally changes its policy in a way to more closely conform with existing policy in another state. Policy convergence, on the other hand, occurs whenever a state's policy moves closer to policy in another state. Convergence can therefore arise from emulation, which requires intention, or coincidence, which does not. As in Volden (2006), the dependent variable in a dyadic event history analysis measures policy convergence and one typically includes independent variables to explicitly test for active emulation. As currently implemented, the dyadic model may produce evidence of emulation even in its absence, an outcome that I refer to as apparent emulation or emulation bias since it may or may not result from intentional emulation.

In order to demonstrate the potential for bias in the dyadic event history model, I start with a standard state policy adoption scenario, with the data generated at the state-year level. Each year, states without the policy in question choose whether or not to adopt it based on their internal characteristics and some unobserved random component. For simplicity, I include only one component and one internal characteristic whose value varies across states, but not over time. Based on this standard monadic policy adoption process I then write out the corresponding dyadic model. In this setup observations consist of pairs of states with each pair including a laggard state (i.e., the state deciding whether to change its policy) and a leader state (i.e., the state whose policy it may choose to emulate). The directed dyadic approach includes each pair of states twice, switching the identity of the leader and laggard states in the second observation. I show that with this structure the probability of policy convergence depends on both the laggard and leader states' characteristics, despite the fact that only internal state characteristics determine the data generating process: the dyadic event history model is capable of generating evidence of emulation even when none exists. An appendix extends these results to policies with multiple components and characteristics that vary over time.[3]

To be precise, let Y_{it} indicate whether state i ($1 \leq i \leq N$) adopts the policy in question at time t ($1 \leq t \leq T$). Assume for convenience that once a state has adopted the policy, it is no longer in the risk set at time t, which, following convention, is denoted by $R(t)$. Let L_{it} indicate whether state i has already adopted the policy in question (i.e., $L_{it} = 1 \Rightarrow i \notin R(t)$). Let the probability of adoption increase with the value of an internal state characteristic, D_i, which I assume is continuous and constant over time.[4] For example, D_i might measure the degree of Democratic control of a state's government.

Following the usual, monadic state-year approach to policy adoption, write the probability of adopting the policy as:

$$\pi_{it} \;=\; \Pr(Y_{it} = 1 | D_i). \qquad (1)$$

Importantly, note that the probability that state i adopts does not depend on any characteristics of any leader state j ($\forall j \neq i$). Since D_i does not vary over time, I drop the time subscript and simply refer to the probability of adoption as π_i.

Now consider the directed dyad approach to modeling policy diffusion. With the simple dichotomous policy adoption variable Y_{it} described above, I say that policy convergence occurs if state i adopts a policy ($Y_{it} = 1$) that state j has already adopted ($L_{jt} = 1$). Policy in state i may converge with policy in all other states (i.e., $\forall j \neq i$), which results in $N - 1$ observations per state for each year for which it is in the risk set. If a state adopts the policy in question in year t, its policy therefore converges with all states that have adopted before that year and does not converge with any state that has not adopted before that year (including states that adopt in year t).

Estimating a model of policy convergence requires a new unit of observation, the directed dyad-year, and a new dependent variable. Let C_{ijt} indicate whether policy in state i converges with policy in state j in year t. This variable takes on the value one if $Y_{it} = 1$ and $L_{jt} = 1$. Note that in the directed dyad approach, policy in state i converging with that in state j is different than policy in state j converging with that in state i. Thus C_{ijt} is not the same as C_{jit}: the order of the subscripts matters.

Now, write out the probability that C_{ijt} equals one in terms of the state-year variables:

$$\Pr(C_{ijt} = 1 | D_i, D_j) \quad = \quad \Pr(Y_{it} = 1, L_{jt} = 1 | D_i, D_j). \tag{2}$$

Note that there are only four mutually exclusive combinations of Y_{it} and L_{jt} in a given year and that only one of them results in the convergence variable taking on the value one. This is crucial for understanding the source of ambiguity in the dyad event history approach. Intuitively, the probability of policy in state i converging with that in state j depends both on whether state i adopts and whether state j has already adopted, so any variable that increases the chance of a state adopting can increase the chance of apparent emulation.

To see this, start by writing out the four combinations of Y_{it} and L_{jt} and the associated values of C_{ijt} (where $1\{X = 1\}$ means the event that $X = 1$):

$$1\{Y_{it} = 1, L_{jt} = 1|D_i, D_j\} \Rightarrow C_{ijt} = 1, \quad (3)$$

$$1\{Y_{it} = 1, L_{jt} = 0|D_i, D_j\} \Rightarrow C_{ijt} = 0, \quad (4)$$

$$1\{Y_{it} = 0, L_{jt} = 1|D_i, D_j\} \Rightarrow C_{ijt} = 0, \quad (5)$$

$$1\{Y_{it} = 0, L_{jt} = 0|D_i, D_j\} \Rightarrow C_{ijt} = 0. \quad (6)$$

Only the first of these corresponds to policy convergence, whereas the latter three correspond to a lack of policy convergence, either due to a failure to adopt the policy (in the third case), to a lack of an opportunity to do so (in the second case), or to both (in the fourth case). Since these mutually exclusive events constitute all possible cases (assuming $i \in R(t)$), I can write out the probability of policy convergence:

$$\Pr(C_{ijt} = 1|D_i, D_j) = \Pr(Y_{it} = 1, L_{jt} = 1|D_i, D_j). \quad (7)$$

Because policy adoption occurs independently in the two states, I can rewrite the probability of convergence as the product of two independent probabilities that depend on policy adoption in each state separately, which will demonstrate that convergence depends on unrelated actions in both states. To do so, rewrite the joint probability as the product of the associated marginal probabilities, which are independent given the assumption of no policy emulation.

$$\Pr(Y_{it} = 1, L_{jt} = 1|D_i, D_j) = \Pr(Y_{it} = 1|D_i) \Pr(L_{jt} = 1|D_j). \quad (8)$$

The first piece is just the probability of adoption in state i, which can be written as π_i while the second piece represents the probability of convergence opportunity, which is just the probability that state j has adopted the policy in question before year t:

$$\begin{aligned} \Pr(L_{jt} = 1|D_j) &= 1 - \prod_{k=1}^{t-1}(1-\pi_j), \qquad (9) \\ &= 1-(1-\pi_j)^{t-1}. \qquad (10) \end{aligned}$$

I can now restate the probability of convergence in Equation 7 as the product of the probability of policy adoption in state i and the probability that state j has adopted the policy by year t:

$$\Pr(C_{ijt} = 1|D_i, D_j) = \pi_i \left(1-(1-\pi_j)^{t-1}\right). \qquad (11)$$

This equation makes it apparent that the probability of policy convergence depends on the probabilities of policy adoption in both states, despite the assumption of independence, which in turn means that the probability of convergence depends on characteristics of both states. The following claim states this more precisely.

Claim 1 *The probability of policy in state i converging with policy in state j increases with both D_i and D_j.*

This claim asserts that anything that increases the probability of state j adopting the policy also increases the probability of policy convergence in state i, thereby providing evidence of apparent emulation (see Appendix A for a proof). This happens despite the fact that the probability that state i adopts the policy depends on neither D_j nor L_{jt}. What is the intuition behind this finding? Simply put, state i appears to emulate state j not because it looks to state j as a policy leader, but because both are independently headed in the same direction and state j may just happened to get there first. Since, by definition, neither emulation nor convergence can occur if state j has not adopted the policy, factors that increase the probability of adoption also increase the opportunity for policy convergence and therefore the probability of convergence, which would be incorrectly interpreted as evidence of emulation.[5]

The following claim concerns the appearance of apparent policy emulation based on the characteristics of state i relative to state j, for example whether states with large democratic majorities emulate other states with a similar partisan leaning.

Claim 2 *The effect of D_i on the probability of policy in state i converging with policy in state j increases with D_j.*

The proof of the second claim, also contained in Appendix A, shows that the marginal effect of D_i on the probability of policy in state i converging with that in state j is greater when D_j is larger. A variable that independently increases π_i and π_j will have a greater effect on π_i when it takes on larger values in state j. This effect will not be distinctly captured unless the effect of variables that increase π_i are allowed to depend on the value of variables that increase π_j.

To illustrate these two claims, consider an example in which states become more likely to adopt a policy as the proportion of Democrats in the legislature increases. The first claim demonstrates that the probability of policy convergence is greater not just when state i is more Democratic, but also when state j is more Democratic. The second claim shows that the effect of increasing Democratic control is larger in state i when state j is more Democratic. In practice, this means that if one conducts a dyadic event history analysis to study policy convergence by including D_i and D_j in an analysis, they will both have positive coefficients. Further, if one includes an interaction between them — i.e., $D_i \times D_j$ — it will also have a positive coefficient. In both cases, evidence of policy emulation could emerge despite the fact that it should not.

A critical assumption behind these results is that the value of D_j does not change over time within a state. This links the probability of opportunity for policy convergence to current year values of the independent variable, and those values end up capturing changes in the probability of opportunity rather than true emulation. For example, if the independent variables had the same mean and variance across all states, then today's values in leader states would have no relation to previous values and no bias would result. This situation is unlikely to occur in most applications, however, since richer states tend to stay richer and conservative states tend to stay conservative. Fortunately, the results do not depend on D_j being constant over time: Appendix B shows that the results hold if independent variables change over time, as long as that change preserves their relative order across states (e.g., states with greater income in one year tend to have greater income in subsequent years).

Other assumptions can be relaxed as well. First, analogous results will hold if the effect of D is negative for both states: states with greater values of D would be less likely to adopt and the effect of increasing D_i on apparent emulation would be even more negative when D_j is larger. This would produce a negative interaction term. Second, similar results would hold for dichotomous or ordinal independent variables. Third, Appendix C proves the main results for policies consisting of multiple components.

An Alternative: The Conditional Dyadic Event History Approach

The potential bias discussed in the previous section arises due to the conflation of policy convergence and the opportunity for convergence. Conditioning on opportunity eliminates this by removing observations that currently have the same policy and are therefore not at risk of policy convergence. Since the probability of convergence without opportunity is precisely zero, one loses no information about convergence by omitting these observations. To see that this approach removes the problem of emulation bias, write out the conditional convergence model and rewrite it in terms of adoption by the two states:

$$\begin{aligned}
\Pr(C_{ijt} = 1 | D_i, D_j, L_{jt} = 1) &= \Pr(Y_{it} = 1, L_{jt} = 1 | D_i, D_j, L_{jt} = 1), && (12)\\
&= \Pr(Y_{it} = 1 | D_i, D_j, L_{jt} = 1), && (13)\\
&= \Pr(Y_{it} = 1 | D_i). && (14)
\end{aligned}$$

With one binary component the probability of policy convergence given opportunity is the same as the probability of adoption given opportunity. Coupled with the independence of leader and laggard states' actions, this reduces to the probability of adoption.

To implement this alternative model, one need only limit the analysis to the set of observations for which policy convergence is possible. In general, this involves eliminating dyad-year observations in which both states had the same policy in the previous year. Further, if policy moves in one direction (e.g., if one studies a period of time in which states are adopting lotteries but none are getting rid of them), then policy can not converge by one state "unadopting" a policy it already has in place. As in a monadic model, the general principal involves defining the risk set appropriately.

With multiple components convergence given opportunity will not necessarily be equivalent to adoption, but researchers should still condition on opportunity since, by definition, neither convergence nor emulation can occur without opportunity.[6] Monte Carlo evidence (discussed later) supports this recommendation. An alternate approach that I do not develop in detail due to space constraints involves moving from a dyadic notion of policy convergence to a component-by-component conception of convergence. By measuring convergence at the dyad-year-component level, opportunity is again achieved in only one way and the single component results, including the conditional convergence approach, would directly apply in this alternate setting with only minor modifications.[7]

Monte Carlo Analysis

To explore the consequences of the potential bias demonstrated above, I perform a series of Monte Carlo experiments. These experiments generate data according to a monadic model of state policy adoption, so that the probability that each state adopts the policy in question depends only on its own characteristics and not on those of any other state. I then transform this state-year data set into a directed dyad-year data set and estimate a number of models of policy convergence to determine the existence and severity of emulation bias and whether conditioning on opportunity resolves it. In order to understand how this bias depends on characteristics of the data, I vary the over-time correlation in the independent variable within each state. The results demonstrate that the dyadic event history approach produces biased coefficients for external variables (i.e., those of the leader state and interactions), that one would incorrectly and frequently reject the null hypothesis of no effect on the probability of policy convergence, and that the extent of the bias increases with the amount of autocorrelation in those external variables. Dyadic event history models that condition on opportunity, while not perfect, produce more accurate estimates and a much lower Type I error rate.[8]

I generate monadic policy adoption data with fifty states over a twenty-five year period with one continuous independent variable. I generated this variable to mimic the structure of real per capita income in the fifty states from 1975 to 2000, changing one parameter to control the amount of autocorrelation in order to highlight its consequences for emulation bias. I repeated the Monte Carlo for five different values of this parameter, producing average autocorrelations of 0.85, 0.65, 0.5, 0.2, and 0.05 with a standard deviation across states of about 0.2 (see Appendix D for more information). I also ran the Monte Carlo using the observed values of real income, which has a correlation of 0.97 with its lag, in order to evaluate the degree of emulation bias that might occur with actual U.S. state policy adoption data.

For each set of independent variables, I generated the dependent variable according to a logit model:

$$Y^*_{it} = -4 + 0.1 \times X_{it} + \epsilon_{it}; \tag{15}$$

$$Y_{it} = \begin{cases} 1 & \text{if } Y^*_{it} > 0, \\ 0 & \text{otherwise.} \end{cases} \tag{16}$$

This data generating process results in about three and a half states adopting per year during the first ten years, and the remaining states slowly adopting over the remaining fifteen years. The average number of observations in the risk set decreases from 454 with a correlation of 0.85 to 424 with a correlation of 0.05.[9]

I then converted the data into a directed dyad data set, with two observations each year for each state pair — one in which policy in state i can converge with policy in state j and another in which policy in j can converge with policy in i. Each observation includes the value of the independent variable in each state in the pair and I created an interaction by multiplying the two values.[10] The dependent variable, C_{ijt}, is equal to one if state i adopts and state j has already adopted:

$$C_{ijt} = \begin{cases} 1 & \text{if } Y_{ijt} = 1 \text{ and } L_{ijt} = 1, \\ 0 & \text{otherwise.} \end{cases} \tag{17}$$

Thus L_{ijt} indicates whether the leader state, j, has already adopted the policy question, thereby providing an opportunity for policy convergence in the laggard state, i. As with a standard state policy adoption model, once state i has adopted it is no longer in the risk set and I remove observations with it as the laggard state from the dyadic analysis.[11] This setup results in about twenty thousand observations in the risk set with the largest correlation and seventeen thousand with the smallest correlation. Five to six percent of the observations in the risk set have C_{ijt} equal to one; this jumps to thirteen or fourteen percent when conditioning on opportunity.

I estimated a total of six different dyadic models and one monadic EHA for comparison. The first three dyadic models include all observations, corresponding to the standard dyadic event history analysis, while the second three condition on the opportunity for convergence by removing observations for which the leader state has not yet adopted. For each approach the first model includes only variables for the laggard state, the second adds measures of the leader state and the third adds the interaction between the values of these variables in the two states. All models cluster the standard errors on state i.

[Table 1 Here.]

I ran the models presented in Table 1 one thousand times for each set of parameters, generating only new errors for each of the trials. I report the average values of the estimated coefficients associated with the included variables, the averages of their estimated standard errors, the standard deviations of the estimated coefficients and the percentages of the estimated coefficients that are significantly different from zero at the .05 level.[12] The final quantity is of particular interest since it addresses the question of how often one would reach an erroneous conclusion regarding the effect of external or joint variables. Given that these variables have no role in the adoption process, one would expect to commit a Type I error only 5% of the time at this level.

[Figure 1 Here.]

Figure 1 summarizes the main findings with kernel density plots of the distributions of the estimated coefficients for the models that include both leader and laggard state characteristics. To ease interpretation, I only plot results for four different values of the correlation parameter. The models that include only laggard state characteristics produce similar results for that variable while the interactions complicate interpretation in those models (since they must be accounted for when determining the effect of income in the potential emulator). Four features stand out. First, the standard dyadic event history model produces biased coefficients for income in the leader state, as expected, but it also produces biased coefficients for income in the laggard state. This latter result is surprising, but emerges even for the model that omits leader state characteristics. I return to this finding in a moment. Second, the amount of bias depends on the correlation in income over time. As it decreases, the bias for the leader state variable also decreases. Similarly, the estimated values of the laggard state coefficient decrease as the correlation decreases, though the bias moves from positive for large correlations to negative for small ones. Third, the coefficients from the models that condition on opportunity show no bias, though the results in Table 1 indicate a small amount in both variables with large correlations. Fourth, the standard deviation of the estimated coefficients increases as the correlation gets larger, since this produces greater variation in the independent variables and therefore more precise estimates.

With respect to points one and three, the bias in the laggard state coefficient appears to emerge partly from the over time correlation, but also from the structure of the dependent variable: additional simulations (not presented) show that a dyadic event history model with adoption rather than convergence as the dependent variable produces an average laggard state coefficient within one percent of the true value. This is unsurprising since a dyadic model with only laggard state variables is equivalent to the monadic model with forty-nine identical copies of each observation and the reported results for the monadic model indicate a similar level of bias with low correlation. Thus the bias appears to result not from the change in the level of analysis, but rather from the process of changing the conception and measurement of the dependent variable from policy adoption to policy convergence.

Table 1 demonstrates two additional results. Perhaps most importantly, the Type I error rate for the unconditional dyadic event history model is quite large. The model that includes leader and laggard characteristics produces an error rate of over 90% for leader state coefficients. This drops with the inclusion of the interaction, but still greatly exceeds 5%. Further, the coefficients for the interaction have Type I error rates between 10 and 17%. Two features of the results cause this undesirable outcome: the bias in the coefficient, but also the apparent underestimation of the standard errors. The latter is evidenced by the fact that the average reported standard errors for leader state coefficients are about 60% of the sampling standard deviations. The bias does not appear to be as serious for coefficients for the laggard state, as their standard deviations are much closer to the average standard error. Some slight upward bias emerges with small correlations while a downward bias emerges with large correlations, though neither exceeds 7%.

Second, while the conditional dyadic event history model almost completely eliminates the bias in leader state coefficients and the interaction coefficients, the Type I error rate for both remains larger than 5%, with reported values between 8% and 15%. This results partly from a continued, though reduced overconfidence in the precision of the estimated coefficients, particularly for large correlations. The relative difference between the standard errors and standard deviations is now between 0 and 5%, however, which vastly improves upon the results for the unconditional dyadic event history model. Additional simulations that clustered on the leader state or on the dyad did not resolve but rather exacerbated this problem, suggesting a need for additional work.

The Diffusion of State Pain Management Policy

In this section I apply the lessons of the previous sections to the study of state pain management policy (hereafter, PMP) for end-of life care. First, I review the context and forms of PMP, then I discuss some of the independent variables used to model its adoption and diffusion, followed by the presentation of various conditional and unconditional dyadic event history models of state adoption of PMP.

State Pain Management Policy

One of the objectives of promoting a good PMP is to provide patients with a "good death": "a dying experience characterized by respect for patients' preferences regarding their clinical care; attention to patients' psychosocial, spiritual and emotional needs; and the provision of pain and symptom management" (Byock 1997; Imhof 2006).[13] Due to a lack off federal activity in this area (Imhof 2006), states took matters into their own hands in the 1990s and developed their own policies to promote good end of life care. The University of Wisconsin's Pain and Policy Studies Group (PPSG), which has become recognized as the national resource on state pain management policy (Joranson and Dahl 1989), has identified a number of components that are part of a balanced policy consistent with current clinical practices (PPSG 2000). Following Imhof (2006) I focus on seven of these indicators: 1. Pain management is part of medical practice; 2. Opioids are part of professional practice; 3. Encourages pain management; 4. Addresses fear of regulatory scrutiny; 5. Prescription amount alone does not determine legitimacy; 6. Physical dependence or analgesic tolerance is not confused with addiction; 7. Other provisions that may enhance pain management.

The first PMP activity occurred in 1989 in Massachusetts, which sets that year as the beginning of the period of study. The adoption pattern shows little activity in the early 1990s, but picks up between 1996 and 2001, with 1999 as the watershed year with fifty-three components adopted. The activity in this year is partly driven by the release of the Federation of State Medical

Boards (FSMB) model guidelines for state PMP in July, 1998. There is great variation across states in the number of components adopted and each component is adopted by anywhere from nineteen to thirty states.

Explaining State Pain Management Policy Adoption

In order to develop a model of state PMP adoption, I build on Imhof's (2006) monadic event history analysis of states' adoptions of the seven components described above. Since a dyadic event history analysis requires values of key independent variables for laggard states, leader states and their relative values, I focus on a handful of key variables in order to keep the analysis relatively parsimonious. These variables are divided into two categories: characteristics of state medical boards, which control the adoption of state PMP, and political characteristics of the states commonly employed in policy diffusion studies.

I focus on three features of state medical boards.[14] The first measures the number of full-time legal counsel on the board's staff. As Imhof (2006) argues, legal counsel may play an important role in helping boards develop new PMP proposals given the complex legal issues involved. The second variable captures a medical board's statutory authority, measured by boards' licensing and disciplinary powers, with a four point scale that increases with the board's ability to implement new policies on its own.[15] The third variable measures the proportion of board members that do not have an medical degree. Boards with a greater proportion of non-physicians may be more likely to adopt policies that prioritize patients' needs.

To measure state political environment, I include two commonly deployed variables: state-level ideology (Erikson, Wright and McIver 1993) and real income per capita, both of which have been employed in previous studies of PMP adoption (e.g., Boehmke 2007; Imhof 2006). If states look to similar states when setting policy, then I would expect that liberal and wealthy states will tend to imitate similarly liberal and wealthy states. I also control for policy diffusion effects (see, e.g., Berry and Berry 1990; Mooney 2001) by constructing a variable that counts the number of the seven PMP components already adopted in the leader state. As in Volden (2006), I include this variable as well as its interaction with contiguity in order to explicitly test whether the configuration of policy in contiguous states has a greater effect than policy in noncontiguous states. A significant coefficient on the latter variable suggests that contiguity matters for the diffusion of state PMP adoption. Because there is no economic competition between states on PMP, I do not expect to find a strong effect of contiguity — diffusion should be driven mostly by learning in this policy area (Boehmke and Witmer 2004).

The final variables control for time trends. These are shaped by the release in July 1997 of the Federation of State Medical Boards' (FSMB) report on model guidelines for state PMP, which advocated adoption of all seven components of PMP. To control for the possible effect of this report I include an indicator variable, Post-FSMB Guidelines, which takes on the value one in 1999 and subsequent years. In addition, I include separate linear time trends before and after the release of the FSMB guidelines. These variables are constant across states and dyads within a given year.[16]

The Diffusion of Pain Management Policy

Using the variables described in the previous section, I estimate four dyadic event history models and one monadic event history model for comparison; for the monadic model I create a dependent variable that takes on the value one in the first year in which any component is adopted. The first two dyadic models constitute a standard dyadic event history analysis whereas the second two correspond to the conditional dyadic event history model, which conditions on the opportunity for policy convergence by constructing a variable for whether the leader state has already adopted at least one component that the laggard state has not adopted. For each pair of models I estimate one model that includes interactions between all of the variables, measured with the absolute difference between them, and one that omits these interactions, which are generally insignificant. Finally, I omit states after they have adopted all seven components.[17]

[Table 2 Here.]

A few findings stand out in the results presented in Table 2. First, consider the variables measuring characteristics of the laggard state. All but one have significant effects on convergence in the dyadic diffusion models and all but two have such effects on adoption in the monadic model. These results indicate that policy convergence is more likely in liberal, poorer states with more legal counsel and fewer non-physicians on their state medical boards. Second, conditioning on opportunity has little effect on the magnitudes of the corresponding coefficients, suggesting actual policy emulation rather than coincidental convergence.

Now turn to the effect of variables in the leader state. Two of these — medical board legal authorization and the number of legal counsel — have significant effects on convergence: states are more likely to adopt policies similar to those in states that have greater values for both of these. These results hold for both the unconditional and the conditional dyadic event history models, suggesting the presence of true emulation. Two additional variables — ideology and income — have significant coefficients in the unconditional models that become insignificant once I condition on opportunity. These results illustrate the problem of emulation bias: since both variables affect adoption they also affect the opportunity for policy convergence, but once I condition on this opportunity, their apparent effect on emulation disappears. Note further that the magnitudes of both coefficients decrease quite a bit in the models that condition on opportunity while the standard errors change much less. This is consistent with emulation bias leading to the difference in findings rather than the change in sample size.

Next, consider the variables measuring the characteristics of the leader state relative to the laggard state. None of these approaches standard significance levels, with the exception of non-physicians on state medical boards. The negative coefficients for this variable indicate that policy in laggard states tends to converge with policy in leader states that have similar board structures more so than with leader states that have dissimilar board structures. While the coefficient for this variable decreases by thirty-three percent in the models that condition on opportunity, it is still significant at the .05 level.

Finally, turn to the results for the variables that measure policy diffusion. Policy in laggard states converges with that in with leader states that have adopted a greater number of the seven provisions, as evidenced by significant coefficients in both the unconditional and conditional models. Whether diffusion from contiguous states matters more, however, varies across models: the effect is significant in the unconditional dyadic model but not in the conditional model. Note that the monadic model (which measures the number of contiguous states that have adopted at least one component) also indicates no effect of diffusion from contiguous states.

Emulation in State PMP

The analysis of state PMP reveals the potential consequences of emulation bias: internal characteristics generally emerge as significant influences on policy emulation in all models, but the set of significant external factors shrinks in the dyadic event history models that correctly condition on opportunity. Since the Monte Carlo analysis shows that the latter can reduce bias, the persistence of significant coefficients for characteristics of state medical boards suggests the possibility of true policy emulation. The loss of significance for general state characteristics indicates that those results are likely illusory and due to emulation bias.

These findings suggest, albeit in only one policy area, that policy leadership may be based more on policy-specific factors rather than on general state characteristics. While early PMP movers may, in fact, be poorer and more liberal states, the empirical analysis suggests that these variables do not influence policy convergence. That is, poorer, more liberal states are more likely to expand their pain management policy, but they are not more likely to do so based on active imitation of poorer, liberal states that have already expanded their policy. On the other hand, increasing medical boards' access to legal counsel increases the chances of policy emulation in two ways: first, policy in states with greater legal counsel tends to converge to policy in leader states more quickly; second, given a choice of two policies to emulate, states prefer to emulate the one in the state with more legal counsel.

Conclusion

What are the general lessons to be drawn from these findings? First, scholars applying the dyadic event history analysis approach to study state policy convergence should be sensitive to how well it may apply to a specific policy area. In particular, policy areas with components that tend to move in a uniform direction (i.e., states tend to adopt rather than unadopt components) are susceptible to providing false evidence of policy emulation. Analytic results and Monte Carlo simulations support this conclusion. Second, they also show that one can largely avoid emulation bias by conditioning on convergence or emulation opportunity. An example using real-world data demonstrates that substantively different inferences result from the two approaches.

Researchers should keep in mind that in any empirical application the assumptions of the model I use for the analytic and Monte Carlo analyses may not hold: the data generating process may, in fact, be characterized by emulation. The lesson here is not that all evidence of emulation originates from emulation bias. Rather, it is that evidence of emulation may appear in the absence of actual emulation. The current paper does not address the possibility of emulation bias in the context of actual emulation; it may be the case, though, that failure to condition on opportunity will lead to inflated estimates of emulation. At the very least, one should exclude observations that are not at risk of emulation since they contain no information about possible emulation.

It is important to note, therefore, that the opportunity for policy convergence or emulation will vary quite a bit across policy areas. In particular, policy areas with many observations that are not eligible for policy convergence likely suffer from a greater risk of emulation bias. The number of such observations will likely increase for policy areas that have components that tend to move in a uniform direction. Intuitively, when policy is moving in the same direction across states, it makes it harder to determine whether changes result from convergence or from actual emulation. Conditioning on opportunity reduces the chances of incorrectly inferring the latter in such situations. For example, in Volden's (2006) analysis of state Children's Health Insurance Programs states start out with very different policies in response to the creation of a federal program. Because of this initial policy heterogeneity, conditioning on emulation opportunity eliminates only 136 out of ten thousand observations and produces no important changes in the magnitude or significance of estimated coefficients.[18]

Finally, the valuable advances that it has and will likely continue to produce warrant additional analytic and simulation diagnostics of the dyadic event history model. The results here underscore one particular issue and offer a simple modification to avoid it, but other complexities still exist. First, the Monte Carlo results in this paper produced evidence of bias in the estimated standard errors for both the unconditional and conditional approaches (though the bias in the latter is much smaller). One solution to this problem could emerge from Gilardi's (2008) discussion of the use of multilevel models to model the nonindependence of observations. Of course, the monadic data generating process used for the Monte Carlo in this paper was not designed to study this issue specifically — generalization on this finding should be done with caution. Second, while the analytic and Monte Carlo results extend to policies with multiple components, future work should explore in more detail the conditional dyadic event history model in this context. While additional Monte Carlo results show that conditioning on opportunity generally eliminates the bias in leader state variables, whether performed at the dyad-year or dyad-year-component level, these two levels of analysis correspond to different conceptions of policy convergence. Does convergence occur when policies become more similar overall? Or does it occur as on a component by component basis? Third, and more generally, future work should focus on developing an estimator that includes monadic adoption and dyadic emulation as special cases, allowing us to derive hypotheses about when emulation might occur, and then rely on the data rather than modeling assumptions to tell us whether policy changes are driven merely by convergence or by true emulation.

Appendices

A Proofs for Claims 1 and 2

Proof of Claim 1: Let $\pi'_k = \partial \Pr(Y_{kt} = 1|D_k)/\partial D_k$, with $k \in \{i, j\}$. Take the derivative of $\Pr(C_{ijt} = 1|D_i, D_j)$ with respect to D_i and D_j, respectively:

$$\Pr(C_{ijt} = 1|D_i, D_j) = \pi_i \left(1 - (1 - \pi_j)^{t-1}\right); \quad (18)$$

$$\frac{\partial \Pr(C_{ijt} = 1|D_i, D_j)}{\partial D_i} = \pi'_i \left(1 - (1 - \pi_j)^{t-1}\right), \quad (19)$$

$$\frac{\partial \Pr(C_{ijt} = 1|D_i, D_j)}{\partial D_j} = (t-1)\pi'_j \pi_i (1 - \pi_j)^{t-2}. \quad (20)$$

Since $t - 1 > 0; \pi_i, \pi_j \in (0, 1);$ and $\pi'_i, \pi'_j > 0$, then the two partial derivatives above must be positive. Note that $t > 1$ since convergence can not occur until $t = 2$ and that the rest of the inequalities are true by assumption. ∎

Proof of Claim 2: Take the mixed partial derivative of $\Pr(C_{ijt} = 1|D_i, D_j)$ with respect to D_i and D_j:

$$\Pr(C_{ijt} = 1|D_i, D_j) = \pi_i \left(1 - (1 - \pi_j)^{t-1}\right), \quad (21)$$

$$\frac{\partial^2 \Pr(C_{ijt} = 1|D_i, D_j)}{\partial D_i \partial D_j} = (t-1)\pi'_j \pi'_i (1 - \pi_j)^{t-2}. \quad (22)$$

Since $t - 1 > 0; \pi_j \in (0, 1);$ and $\pi'_i, \pi'_j > 0$, then the mixed partial derivative above must be positive whenever $t > 1$, which it is since convergence can not occur until $t = 2$. ∎

B Extension for Time-Varying Independent Variables

The assumption that D is constant over time for each state can be relaxed. In particular, a sufficient condition on the change in D over time for the proof of Claim 1 to go through is that

$$\partial \Pr(L_{jt} = 1|\mathbf{D}_j)/\partial D_{jt} \quad > \quad 0, \tag{23}$$

where $\mathbf{D}_j$ is the vector of values of D_{jt} from time 1 to $t-1$. When this condition is met, the probability that state j has already adopted by time period t is increasing in the value of D_{jt} at time t, which implies that the probability of apparent emulation increases with D_{jt} as well.

One way to achieve this condition is to write out how D_{jt} evolves over time. When this relationship satisfies certain conditions, one can extend the proof in a straightforward manner. For example, if one lets $\mathbf{D}_j$ change over time such that $D_{jt} = g(D_{jt-1}) + \delta\epsilon_{jt}$, where ϵ_{it} is a random variable, then, if g^{-1} exists and is differentiable, similar results would obtain. The main results correspond to the special case $g(D_{jt-1}) = D_{jt}$. Briefly, here is a sketch of the proof. First write out the probability of policy convergence to account for the fact that D_{kt} changes over time:

$$\begin{aligned}
\Pr(Y_{it} = 1, L_{jt} = 1|D_{it}, D_{jt}) \quad &= \quad \Pr(Y_{it} = 1|D_{it}) \Pr(L_{jt} = 1|\mathbf{D}_j), &(24)\\
&= \quad \Pr(Y_{it} = 1|D_{it}) \left(1 - \prod_{k=1}^{t-1}(1 - \Pr(Y_{jk} = 1|D_{jk}))\right). &(25)
\end{aligned}$$

Consider the simplest case of $t = 2$. For the partial derivative of this to be increasing in D_{i2}, it suffices to show that the derivative of the second term with respect to D_{i2} is positive, since the first probability is positive by assumption. This can be accomplished by inverting g, (i.e., $g^{-1}(D_{j2} - \delta\epsilon_{i2}) = D_{j1}$), substituting its inverse in for D_{j1}, and taking the partial derivative with respect to D_{j2}:

$$\begin{aligned}
\Pr(L_{j2} = 1|\mathbf{D}_j) \quad &= \quad 1 - \prod_{k=1}^{1}(1 - \Pr(Y_{jk} = 1|D_{jk})), &(26)\\
&= \quad \Pr(Y_{j1} = 1|D_{j1}), &(27)
\end{aligned}$$

$$
\begin{aligned}
&= \Pr(Y_{j1} = 1 | g^{-1}(D_{j2} - \delta\epsilon_{i2})); && (28)\\
\frac{\partial \Pr(L_{j2} = 1|\mathbf{D}_j)}{\partial D_{j2}} &= \frac{\partial g^{-1}(D_{j2} - \delta\epsilon_{i2})}{\partial D_{j2}} \frac{\partial \Pr(Y_{j1} = 1|g^{-1}(D_{j2} - \delta\epsilon_i))}{\partial g^{-1}(D_{j2} - \delta\epsilon_{i2})}, && (29)\\
&= \frac{\partial g^{-1}(D_{j2} - \delta\epsilon_{i2})}{\partial D_{j2}} \frac{\partial \Pr(Y_{j1} = 1|D_{j1})}{\partial D_{j1}}. && (30)
\end{aligned}
$$

Since the second term is positive by assumption, the probability of convergence opportunity, thus the probability of convergence, is increasing in D_{j2} whenever the first term is positive as well. Since $g(x)$ is strictly increasing, then $g^{-1}(x)$ is as well, so the proof holds. Increasing t would add more terms to the partial derivative, but would not change the results. Note that this relationship includes cases where D_{jt} is constant over time and where D_{jt} is trending upwards or downwards (e.g., $g(D_{jt}) = 0.5D_{jt-1} + \epsilon_{it}$), possibly with stochastic variation from period to period. The critical feature is that $g(x)$ preserves the relative order of D across states in different years. It therefore does not include cases for which $g'(x) \leq 0$, which correspond to either completely stochastic variables or ones that systematically jump from positive to negative from year to year (e.g., $g(D_{jt}) = -0.5D_{jt-1} + \epsilon_{it}$). Many common variables in state politics would fall into the former category rather than the latter.

C Extension for Policies with Multiple Components

In this section I provide a quick proof that extends the main results to policies with multiple components (Boehmke 2009). This setup is common in the empirical literature that employs the dyadic approach to studying diffusion (e.g., Volden 2006, Shipan and Volden 2008) and corresponds to policies that have more than one component, e.g., anti-smoking policies that involve any or all of youth access restrictions, bans on smoking in government buildings, or bans on smoking in resturuants (Shipan and Volden 2008).

Here I merely extend the main results to policies with two components; allowing for additional components would proceed analogously. As before, I assume that components are discrete and write the probabilities of adopting them as $\Pr(Y_{it1} = 1|D_i) = \pi_{i1}$ and

$\Pr(Y_{it1} = 1|D_i) = \pi_{i2}$. Assume that these probabilities are independent given D_i and let π'_{i1} and π'_{i2} denote their derivatives with respect to D_i. In this scenario, convergence occurs whenever state i adopts a component that state j has previously adopted. This creates multiple forms of convergence: state i can just adopt component one if state j already has it, state i can just adopt component two if state j already has it, or state i can adopt both components if state j has either of them. Under the assumption that adoption is independent across states and across components, the probability of policy convergence is written as:

$$\begin{aligned}\Pr(C_{ijt} = 1|D_i, D_j) &= \pi_{i1}(1-\pi_{i2})\left(1-(1-\pi_{j1})^{t-1}\right) + (1-\pi_{i1})\pi_{i2}\left(1-(1-\pi_{j2})^{t-1}\right) \\ &\quad + \pi_{i1}\pi_{i2}\left(1-(1-\pi_{j1})^{t-1}(1-\pi_{j2})^{t-1}\right). \qquad (31)\end{aligned}$$

To demonstrate emulation bias, take the derivative with respect to D_j:

$$\begin{aligned}\frac{\partial \Pr(C_{ijt} = 1|D_i, D_j)}{\partial D_j} &= \pi_{i1}(1-\pi_{i2})(t-1)\pi'_{j1}(1-\pi_{j1})^{t-2} + (1-\pi_{i1})\pi_{i2}(t-1)\pi'_{j2}(1-\pi_{j2})^{t-2} \\ &\quad + \pi_{i1}\pi_{i2}(t-1)(1-\pi_{j1})^{t-2}(1-\pi_{j2})^{t-2} \\ &\quad \times \left[\pi'_{j1}(1-\pi_{j2}) + \pi'_{j2}(1-\pi_{j1})\right]. \qquad (32)\end{aligned}$$

Note that whenever the probabilities that state j adopts components one and two are increasing in D_j, the above derivative is positive, so the probability of convergence increases in D_j (since $t \geq 2$ and everything else is a probability and therefore positive); similarly, when the effect of D_j on component adoption in state j is negative, the probability of policy convergence decreases in D_j. Further, emulation bias can still obtain when D_j has opposite effects on the adoption of component one and component 2, with the sign determined by the relative magnitude of the two. In fact, emulation bias disappears only under one specific condition that balances π'_{j1} and π'_{j1} just right (this condition is derived by setting the above derivate equal to zero and solving for π'_{j1}, which will be a linear function of π'_{j2}).

D Process Used to Generate Independent Variable

I generated the independent variable in the Monte Carlo analysis based on the structure of real per capita income from 1970 to 2000, varying one parameter in order to control autocorrelation. I set up initial values and subsequent observations as follows:

$$
\begin{aligned}
X_{i1} &\sim N(17, 3^2); &(33)\\
X_{it} &= X_{i1} + \gamma_i \times time + \epsilon_i; &(34)\\
\gamma_i &\sim N(0.3, 0.1^2); &(35)\\
\epsilon_i &\sim N(0, \lambda^2 \sigma_i^2); &(36)\\
\sigma_i^2 &\sim N(0.7, 0.2^2). &(37)
\end{aligned}
$$

Starting values were drawn from the first distribution to create heterogeneous states. Subsequent value were generated as a function of the initial value, a time trend, and a stochastic term. Each state was assigned a different baseline amount of variation, and this variation is parameterized by λ^2, so that as λ increases, the autocorrelation decreases for all states.

Notes

[1]See Gilardi and Füglister (2008) for an excellent explication and guide to using the dyadic diffusion model.

[2]Note that the bias does not result from the dyadic nature of the data, but from the definition of the dependent variable, suggesting studies of truly dyadic events (e.g., war) should not suffer from this bias.

[3]This setup differs a little from Volden's (2006) analysis of CHIP policies, which contains both dichotomous and continuous measures of the various policy components. Note, however, that Volden's study measures policy convergence by whether policy moves closer to the leader state, which effectively dichotomizes the continuous measures. Other studies have relied on dichotomous components, e.g., Shipan and Volden's (2007) study of anti-smoking policy.

[4]The assumption of continuity is easily relaxed, but is made for notational convenience; the assumption that D_i is constant over time is relaxed in the appendix; and the assumption of a positive effect on adoption is without loss of generality.

[5]Note also that nothing in the proof requires that the variables influencing adoption by states i and j measure the same concept or even that they are correlated.

[6]For example consider a policy with three components and a leader state that has adopted one of them. If the laggard state adopts only that component, convergence occurs. If it adopts all three components, convergence does not occur.

[7]See Boehmke 2009) for information on the analysis of policies with multiple components at the monadic level. I thank an anonymous reviewer for suggesting this alternate approach.

[8]While space does not permit presenting them, additional results indicate that these conclusions hold when there are multiple components, whether estimated at the dyad-year or dyad-year-component level. These results are available from the author upon request.

[9]The average decreases because the small correlation results from more stochastic variation, which increases the dispersion of the independent variable. Given the low probability of a success,

this in turn makes it more likely that a state will have a large value early on and end up adopting earlier.

[10]Similar results also obtained if I generated an interaction that was the absolute difference between the two values. Both approaches may be used in an empirical application, depending on whether one wishes to measure similarity or leadership (see, e.g., Volden 2006 or Boehmke 2007).

[11]Of course, in a year in which a state adopts, its policy is coded as converging with any other state that has adopted and not converging with any state that has not adopted by that year.

[12]I calculate the average standard error by taking the average estimated variance and then taking its square root rather than using the average of the reported standard errors.

[13]This section draws heavily on Imhof (2006) for its discussion and modeling of state PMP — I appreciate the use of the data and information in her study. Any mistakes in this interpretation are mine.

[14]These data were gathered from Imhof and are based on reports by the Federation of State Medical Boards "Exchange Section 3" series. Reports were issued in 1988; 1989-1990; 1992-1993; 1995-1996; and 1999-2000. Data for missing years were filled in with the values from the most recent report.

[15]This is coded one for boards that play a purely advisory role; two for boards that have some limited authority to act; three for boards that are able to act independently in general, but still depend on other administration officials for some tasks and decisions; and four for boards that have full policy autonomy.

[16]In the case of PMP adoptions, it would be difficult to control for the success of a state's policy profile (unlike with children's health care programs à la Volden (2006)). given the fact that there is no clear outcome variable other than patients' experiences during their EOL care.

[17]I also estimated dyad-year-component versions of these models and they produced broadly similar conclusions, with leader state characteristics significant in the standard model, but insignificant when conditioning on emulation.

[18]I thank Craig Volden for sharing his data with me to investigate this.

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Table 1: Monte Carlo Results for Models of State Policy Convergence and Adoption, Varying Autocorrelation of the Independent Variable

	Average Values		$\hat{\beta}$ **for Income in State 1 (Laggard)**				$\hat{\beta}$ **for Income in State 2 (Leader)**				$\hat{\beta}$ **for Income Interaction (Income$_1$ $\times$ Income$_2$)**			
	$\bar{\rho}$	N	Avg.	SD	SE	Sig.	Avg.	SD	SE	Sig.	Avg.	SD	SE	Sig.
EHA Model	0.05	378	0.101	0.020	0.021	100%								
	0.20	422	0.101	0.031	0.031	91%								
	0.50	431	0.104	0.041	0.040	77%								
	0.65	436	0.105	0.047	0.045	66%								
	0.85	454	0.110	0.048	0.046	67%								
	0.99†	434	0.109	0.050	0.046	67%								
Unconditional Dyadic EHA	0.05	16790	0.085	0.017	0.018	100%								
	0.20	19295	0.105	0.028	0.028	96%								
	0.50	19945	0.124	0.034	0.035	97%								
	0.65	20169	0.137	0.038	0.038	96%								
	0.85	19125	0.177	0.053	0.048	95%								
	0.99†	18812	0.153	0.048	0.046	93%								
	0.05	16790	0.085	0.017	0.018	100%	0.013	0.006	0.004	77%				
	0.20	19295	0.104	0.028	0.029	96%	0.045	0.015	0.009	95%				
	0.50	19945	0.119	0.035	0.035	94%	0.072	0.022	0.014	97%				
	0.65	20169	0.129	0.040	0.039	92%	0.088	0.026	0.016	97%				
	0.85	19855	0.145	0.048	0.044	91%	0.100	0.030	0.020	97%				
	0.99†	18812	0.138	0.048	0.045	88%	0.093	0.026	0.018	95%				
	0.05	16790	0.080	0.016	0.018	100%	0.007	0.012	0.011	16%	0.000	0.000	0.000	11%
	0.20	19295	0.089	0.037	0.036	72%	0.028	0.044	0.036	21%	0.001	0.002	0.002	13%
	0.50	19945	0.107	0.069	0.062	41%	0.059	0.083	0.066	25%	0.001	0.004	0.003	12%
	0.65	20169	0.128	0.097	0.081	38%	0.085	0.114	0.086	28%	0.000	0.005	0.004	14%
	0.85	20347	0.159	0.126	0.099	41%	0.119	0.144	0.104	33%	−0.001	0.007	0.005	16%
	0.99†	18812	0.103	0.152	0.125	21%	0.055	0.161	0.123	20%	0.002	0.008	0.006	15%
Conditional Dyadic EHA	0.05	8354	0.104	0.024	0.024	100%								
	0.20	9323	0.104	0.036	0.035	86%								
	0.50	9539	0.106	0.045	0.043	71%								
	0.65	9601	0.110	0.052	0.047	66%								
	0.85	9646	0.112	0.057	0.050	62%								
	0.99†	9088	0.115	0.063	0.056	58%								
	0.05	8354	0.104	0.024	0.024	100%	0.001	0.005	0.005	8%				
	0.20	9323	0.104	0.036	0.035	86%	0.002	0.011	0.011	11%				
	0.50	9539	0.106	0.045	0.043	71%	0.004	0.016	0.016	12%				
	0.65	9601	0.110	0.052	0.047	66%	0.005	0.019	0.018	14%				
	0.85	9646	0.112	0.057	0.050	62%	0.006	0.021	0.020	13%				
	0.99†	9088	0.115	0.064	0.055	58%	0.005	0.023	0.022	15%				
	0.05	8354	0.103	0.024	0.024	100%	0.000	0.014	0.014	8%	0.000	0.001	0.001	7%
	0.20	9323	0.104	0.049	0.048	58%	0.001	0.048	0.043	12%	0.000	0.002	0.002	10%
	0.50	9539	0.105	0.085	0.079	27%	0.002	0.084	0.076	11%	0.000	0.004	0.004	11%
	0.65	9601	0.104	0.112	0.101	20%	−0.002	0.112	0.097	11%	0.000	0.006	0.005	11%
	0.85	9646	0.106	0.136	0.120	17%	−0.002	0.135	0.116	11%	0.000	0.007	0.006	11%
	0.99†	9088	0.092	0.166	0.152	12%	−0.019	0.166	0.145	10%	0.001	0.008	0.007	10%

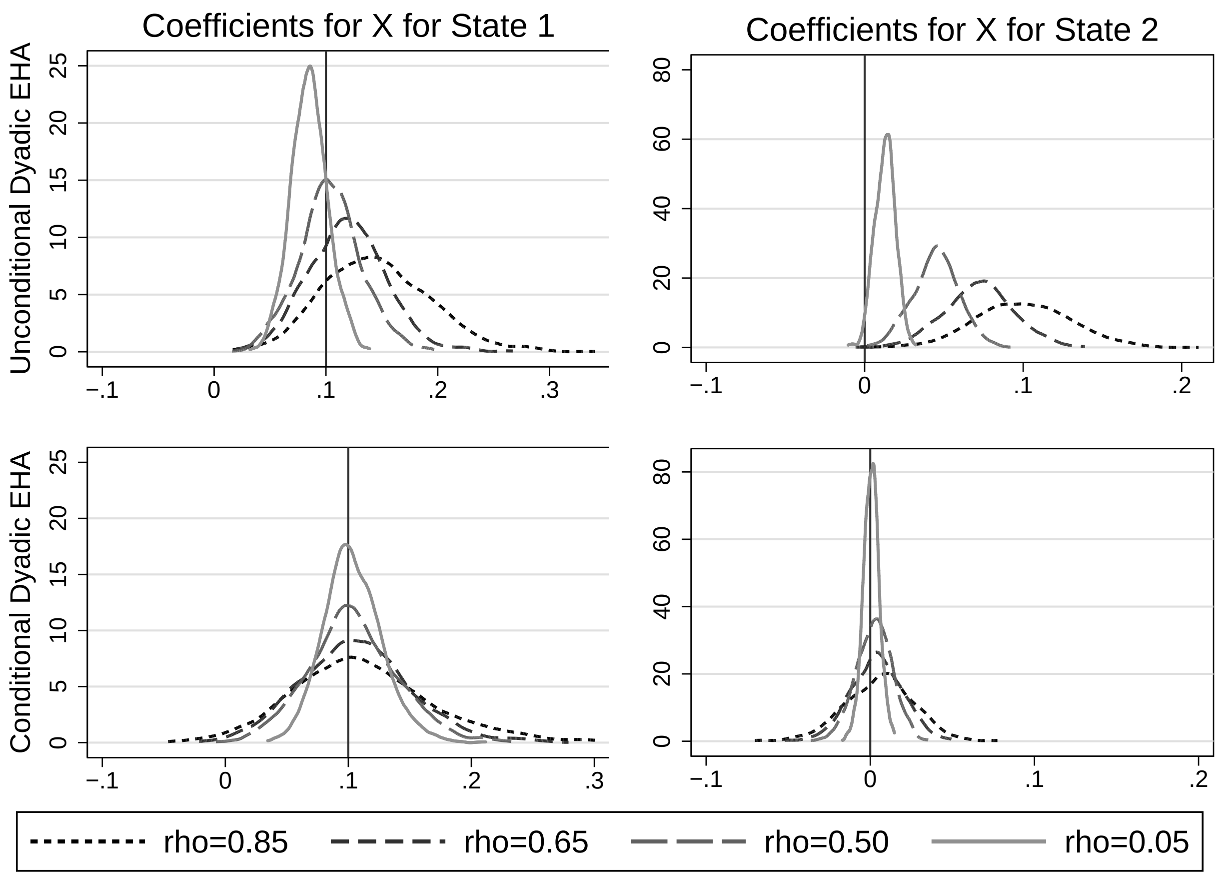
Note: 1000 replications performed per set of parameter values for each model. Standard errors clustered on State 1. $\bar{\rho}$ represents the average across states of the correlation between X_{it} and its lag in the monadic data set; N indicates the average number of observations in the analysis. See Appendix D for more information on how this correlation was constructed. † Results based on observed values of real per capita income in the states, 1975-2000.

Table 2: Event History Analyses of State Pain Management Policy Convergence and Adoption, 1989-2002

	Dyadic				**Monadic**
	Unconditional		*Conditional*		
MB Legal Counsel - State 01	0.098	0.096	0.126*	0.121	0.177*
	(0.054)	(0.052)	(0.063)	(0.062)	(0.045)
MB Authorization - State 01	−0.076	−0.171	−0.040	−0.173	−0.143
	(0.217)	(0.170)	(0.229)	(0.181)	(0.156)
MB Non-M.D. Members - State 01	−4.337*	−3.836*	−4.027*	−3.652*	−0.732
	(1.068)	(0.914)	(1.045)	(0.989)	(0.775)
Income - State 01	−0.170*	−0.162*	−0.172*	−0.164*	−0.176*
	(0.067)	(0.064)	(0.066)	(0.063)	(0.049)
Liberal Ideology - State 01	6.419*	6.728*	6.282*	6.298	3.076
	(2.984)	(3.035)	(3.122)	(3.232)	(1.667)
MB Legal Counsel - State 02	0.217*	0.203*	0.072*	0.049*	
	(0.036)	(0.023)	(0.029)	(0.019)	
MB Authorization - State 02	0.340*	0.245*	0.505*	0.378*	
	(0.160)	(0.105)	(0.190)	(0.138)	
MB Non-M.D. Members - State 02	0.640	−0.022	−0.673	−1.049	
	(0.600)	(0.444)	(0.832)	(0.731)	
Income - State 02	−0.075*	−0.063*	0.005	0.013	
	(0.031)	(0.030)	(0.033)	(0.032)	
Liberal Ideology - State 02	2.121*	2.744*	1.107	1.335	
	(1.001)	(0.983)	(1.426)	(1.336)	
MB Legal Counsel - Difference	−0.022		−0.031		
	(0.039)		(0.037)		
MB Authorization - Difference	0.108		0.174		
	(0.152)		(0.162)		
MB Non-M.D. Members - Difference	−2.206*		−1.527*		
	(0.712)		(0.727)		
Income - Difference	0.030		0.013		
	(0.036)		(0.038)		
Liberal Ideology - Difference	1.192		1.082		
	(1.354)		(1.469)		
Total Provisions - State 02 Contiguous	0.105*	0.093*	0.022	0.014	−0.018
	(0.044)	(0.044)	(0.038)	(0.038)	(0.110)
Total Provisions - State 02	0.485*	0.482*	0.571*	0.571*	
	(0.026)	(0.026)	(0.050)	(0.049)	
Post FSMB Guidelines	3.966*	3.829*	3.764*	3.609*	10.861*
	(0.591)	(0.557)	(0.593)	(0.559)	(3.152)
Time - Pre FSMB	0.326*	0.314*	0.121	0.110	0.383*
	(0.093)	(0.091)	(0.088)	(0.086)	(0.107)
Time - Post FSMB	−0.052	−0.046	−0.222*	−0.218*	−0.657*
	(0.086)	(0.082)	(0.081)	(0.078)	(0.272)
Constant	−2.210	−1.728	−2.162	−1.428	−0.028
	(2.373)	(2.049)	(2.470)	(2.126)	(1.358)
	26,234		5993		636

Note: Standard errors clustered on state 01. * indicates $p \leq .05$ with a two-tailed test. Emulation models start in 1990 rather than 1989 to allow one year for states to set their policies for emulation; monadic EHA model starts in 1989.

Figure 1: Kernel Density Plots of Coefficients From Monte Carlo Results, Varying Autocorrelation in Independent Variable



Note: rho represents the average across states of the correlation between X_{it} and its lag in the monadic data set. The vertical lines indicate the true parameter value. Results based on 1000 trials per set of parameter values.