Beyond Logit and Probit: Some Alternative Duration Modeling Strategies for State Policy Adoption

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Introduction

Models of state policy adoption are ubiquitous in the literature. Owing to the important work of Berry and Berry (1990, 1994), researchers across a wide domain of public policies have collected longitudinal data accounting for when a state adopts a particular type of public policy. Typically, these data record observations on a state from time t_0 until time t_k, the point at which the state adopts the policy. Hence, it is natural to measure the time until policy adoption and treat this quantity—or some function of it—as the response variable in statistical models of state policy adoption. Given this kind of response variable, duration models, or event history analyses are commonly applied to these data.[1]

Moreover, policy adoption data are often measured discretely. That is, a state is initially observed at some time t and then is measured at discrete points, $t = 1, 2, 3, \ldots, k$, until adoption occurs. In the parlance of event history analysis, policy adoption is the "event" and the discretely measured time intervals leading to the event connotes the "history." Given the discrete nature of the data, the event history is then usually recorded as a binary sequence where "0" denotes the nonoccurrence of an event and a "1" denotes the occurrence of an event. Thus, if a state legislature is observed over 10 years and policy adoption occurs in the last period, then the binary sequence would be nine zeroes followed by a one. This kind of data set-up is what Beck, Katz, and Tucker (1998) refer to as "binary time-series cross-sections" (hereafter, BTSCS).

Allison (1982, 1984), Beck, Katz, and Tucker (1998) as well as Box-Steffensmeier and Jones (1997, 2004) demonstrate that BTSCS data are equivalent to duration data. As such, models for binary dependent variables, for example logit or probit, may be applied to such data. In the policy adoption literature, the application of logit or probit models has been widespread (c.f. Balla 2001; Berry and Berry 1990, 1992; Hays and Glick 1997; Mintrom 1997a,b; Mintrom and Vergari 1998; Mooney and Lee 1995; see also True and Mintrom 2001).

[1] We use the terms "duration analysis" and "event history analysis" interchangeably.

In this paper, we discuss some implications of the "logit-probit" approach and consider some alternative modeling strategies for state policy adoption. Our paper builds on and extends recent work by Buckley and Westerland (2003), who also consider issues inherent in the application of standard logit or probit models. The paper proceeds as follows. In the next section, we discuss some implications for duration dependency in the application of logit or probit models on policy adoption data. In this section, we provide some motivation for the consideration of the Cox model. Following this, we discuss the issue of multiple events data and in particular, point out that most duration models of policy adoption tend to focus on "single-spell" processes. We illustrate our points with several examples.

Some Issues Regarding Duration Dependency

Models for policy adoption data are frequently motivated by questions pertaining to risk: given that a state has not adopted a policy to some point, what are the chances it will adopt the policy in the current period? In general, event history data contain information allowing the researcher to assess risk by considering event occurrences as well as the length of survival times prior to the event (if an event occurs). In terms of probability, analysts are therefore interested in determining the following quantity:

$$\Pr(t \leq T \leq t + \Delta t \mid T \geq t), \tag{1}$$

which is the probability a duration T will end in some time interval given that the duration has persisted up to or beyond some time t.

If the random variable T in (1) is assumed to be continuous, this implies that change (or the event's occurrence) may occur anywhere in time. As such, by the definition of a continuous random variable, the probability in (1) is 0. Therefore, for a continuous-time event history process, T is assumed to be absolutely continuous. By dividing the probability in (1) by Δt, a ratio is established of the probability per time unit *to* the time unit. If the

limit of this ratio is taken as Δt approaches 0, then we obtain

$$h(t) = \lim_{\Delta t \to 0} \frac{\Pr(t \leq T \leq t + \Delta t \mid T \geq t)}{\Delta t},$$

which is the hazard rate for a continuous random variable.

In contrast, if the random variable T is assumed to be discrete, this implies that change occurs at some observable time point. Event history models presuming a discrete-time process specify the probability in (1) directly. Therefore, the hazard "rate" for a discrete-time process is

$$h(t) = \Pr(T = t \mid T \geq t),$$

which is a probability. Distinguishing continuous-time from discrete-time processes is *only* important insofar as the quantities of interest (the hazard rate, survivor function, density or probability mass function) in event history analysis are defined differently, mathematically, thus leading to different kinds of modeling strategies. Nevertheless, while the distinction between the two kinds of processes may seem clear-cut, in fact, they are not. While many processes may be absolutely continuous, techniques for observation and/or measurement fail to approximate the continuous nature of change. Data for continuous-time processes often are collected at discrete intervals, for example, by fiscal periods, months, quarters, or even years. Change may occur anywhere in the interval, but the data are only "observed" at predefined periods. So while the continuous-time process presumes knowledge of when change occurred in time, we sometimes only have an approximation as to when the change or transition actually occurred.

Moreover, some longitudinal processes may conceivably be continuous-time processes, but knowledge of precisely when in time change occurred is largely unimportant. To illustrate, consider the example of state adoption of public policy. Presumably, a legislature could adopt a policy anytime within a legislative session. Because state legislatures routinely record votes, we could easily discern precisely when change occurred. In most analyses of state policy adoption, however, the crucial issue is not knowing exactly when adoption

occurred within a legislative session, but rather when adoption occurred relative to other states. In such analyses, the year in which a policy was adopted may be sufficient to mark the occurrence of an event. Therefore, while policy adoption may be a continuous-time process in principle, a discrete-time model may be suitable to the research question. Indeed, this is precisely the motivation that has led to the widespread use of logit and probit type models in this kind of research. Thus, given the discretized nature of policy adoption data, logit and probit have become *de rigueur*; however, it does not naturally follow that because such data are discrete, the menu of duration modeling strategies is limited to binary link models (like logit or probit). To explain and help motivate what follows, we briefly consider the issue of duration dependency.

Apart from characterizing event history processes as being continuous or discrete, another way researchers often think about the range of event history models is in terms of whether or not the distribution of failure times (or survival times) is specified. If this distribution is unspecified, the model (or statistic) is *nonparametric* or "distribution-free." In these kinds of models, the shape of the hazard rate, or analogously, the form of duration dependency, is not directly specified. In contrast, if the distribution of failure times is specified by a distribution function, then the model can be said to be parametric. In general, if the parameterization is accurate, estimates of the median duration (for example) in parametric models will be somewhat more precise than estimates produced by nonparametric methods (Collett 1994); on the other hand, if the distributional assumptions are wrong, the inferences generated from parametric approaches can be misleading (Bergström and Edin 1992). Larsen and Vaupel (1993) make this point more strongly noting that "if the functional form of the hazard has the wrong shape, even the best-fitting model may not fit the data well enough to be useful" (96).

Our discussion of parametric duration models may seem out of place; after all, the literature on policy adoption almost always forgoes standard parametric duration models in

lieu of logit or probit models.[2] Nevertheless, one important point is often overlooked in applied research on state policy adoption: *there is an implicit distributional assumption made regarding duration dependency* (Box-Steffensmeier and Jones 2004; Buckley and Westerland 2003). This overlooked assumption frequently leads to a logit (or probit) equivalent of a duration model parameterized in terms of the exponential distribution, perhaps the most unrealistic assumption that can be made in applied political science duration modeling (see Box-Steffensmeier and Jones 2004 for further discussion).

To demonstrate this, we estimate a logit "duration" model where the dependent variable is a state's adoption of restrictive abortion legislation after the 1973 Supreme Court decision *Roe v. Wade* (see Brace and Langer, n.d. and Brace, Hall, and Langer 1999, 2001 for fuller analyses of these data). The results from this model are shown in the first column of Table 1, labelled "Logit 1". The adoption model is treated as a function of four covariates. The covariate denoted as "pre-*Roe*" is the 5-point Mooney index of pre-*Roe* abortion permissiveness. Higher scores on the index indicates the state was most permissive of abortion prior to the *Roe v. Wade* decision. The second covariate, denoted as "ideology" in the table, is a composite measure of the average of the ideology score of the state legislature, the Supreme Court, and the public (see Brace and Langer 2001 for more details). The measure is scored such that higher values denote greater levels of liberalism and lower values denote more conservative states. The covariate labelled "divided" is a binary indicator scored 1 if a condition of divided government exists and 0 if not. The covariate labelled "neighbor" is a binary indicator scored 1 if a bordering state adopted restrictive abortion legislation, and 0 if not.[3]

The results are interpreted as in any logit model, where the sign on the coefficient indicates how the log-odds are increasing or decreasing with changes in the covariate. Hence,

[2]Standard parametric models include the exponential, Weibull, log-normal, log-logistic, or gamma models; see Box-Steffensmeier and Jones (2004) for a discussion of these models as used in political science research.

[3]The pre-*Roe* and ideology covariates were mean-centered in order to more easily compute baseline hazard functions; see Box-Steffensmeier and Jones 2004 for a discussion of mean-centering and baseline hazard estimation.

Table 1: Logit Models of Adoption of Restrictive Abortion Legislation

	Logit 1	Logit 2	Logit 3
Variable	Estimate (s.e.)	Estimate (s.e.)	Estimate (s.e.)
Pre-*Roe*	−.26 (.09)	−.22 (.09)	−.22 (.08)
Ideology	−.01 (.009)	−.012 (.008)	−.012 (.008)
Divided Gov.	.73 (.31)	.68 (.31)	.67 (.31)
Neighbor	.59 (.37)	.54 (.37)	.53 (.38)
Time	–	−.35 (.20)	6.70 (3.48)
Constant	−2.74 (.42)	−2.16 (.50)	−3.54 (.59)
N	386	386	386
Log-Likelihood	−125.71	−123.86	−123.50

Data are from Brace, Hall, and Langer (1999) The "Time" covariate for the model in the second column ("Logit 2") is $log(t)$; the "Time" covariate for the model in the third column ("Logit 3") is a lowess function.

we see that as pre-*Roe* permissiveness increases, the log-odds of a state adopting restrictive abortion legislation decreases. However, the more central point for our purposes centers on duration dependency. To explain, for this model let the probability of state adoption be $\Pr(y_{it} = 1) = \lambda_i$, and the probability of a nonadoption be $\Pr(y_{it} = 0) = 1 - \lambda_i$. Under the logit model, the log-odds are given by

$$\log\left(\frac{\lambda_i}{1-\lambda_i}\right) = \beta_0 + \beta_1 x_{1i} + \beta_2 x_{2i} + \ldots + \beta_k x_{ki}, \tag{2}$$

and the hazard rate is obtained by reexpressing (2) directly in terms of the probability,

$$\hat{\lambda}_i = \frac{e^{\beta'\mathbf{x}}}{1 + e^{\beta'\mathbf{x}}},$$

where $\exp(\beta'\mathbf{x})$ represents the exponentiated logit parameters for a given set of covariates.[4] What is not obvious from (2) is how duration dependency enters the model. It turns out that this model is directly analogous to a parametric duration model specified in terms of the exponential distribution. Note that under the exponential, the hazard rate is flat with respect to time. Substantively, this means that the risk, or probability of a state adopting some kind of policy is invariant to time: the hazard rate at time t is identical to the hazard

[4]The probit model is not discussed, although the results discussed below apply equally to the probit case (we refer the reader to Buckley and Westerland 2003 for an excellent discussion of logit vs. probit).

rate at any other time t_k. In the logit example above, the same inference regarding the baseline hazard that comes from the exponential model holds for the logit (or probit) case. To see this, reconsider the logit model. Suppose we estimate

$$\log\left(\frac{\lambda_i}{1-\lambda_i}\right) = \beta_0 + \beta_1 x_{1i} + \beta_2 x_{2i},$$

where x_{ki} are two (mean-centered) covariates and β_0 is the constant term. As Box-Steffensmeier and Jones (2004) show, the "baseline" hazard under this model would be equivalent to

$$\hat{\lambda}_i = h_0(t) = e^{\beta_0},$$

which is a constant. The hazard probability is flat with respect to time. This is probably an unrealistic restriction, though one that is commonly made in applied research of policy adoption.

The point to emphasize here is that the use of binary link models (like logit) does not obviate the issue of duration dependency—an issue usually thought of as applying only to standard parametric models. In fact, any garden variety logit or probit model can pose serious analytical problems if duration dependency is not accounted for in some way. Fortunately, as Beck, Katz, and Tucker (1998), Box-Steffensmeier and Jones (2004), and Buckley and Westerland (2003) variously point out, solving the "exponential" problem is easy: simply include a duration dependency parameter(s) in the binary link model. These authors discuss a variety of approaches, including the use of log transformations, spline functions, locally weighted scatterplot smoothing (lowess), and piecewise functions (see also Beck and Jackman 1998).

In the second and third columns of Table 1 we reestimate the logit model and include a covariate to account for duration dependency. The model in the second column (labelled "Logit 2") includes a log transformation of the duration time and the model in the third column (labelled "Logit 3") includes a lowess function of time. Either of these two models provide a slightly better fit than the exponential equivalent given in column 1.[5] As such,

[5]It has been our experience that garden variety logit models usually fit *much* worse than models including

characterizations of the baseline hazard will be substantially different for these models than for the exponential. Note also that once duration dependency is accounted for, the parameter estimates for the covariates change. Thus, estimates of the hazard probability—the fundamental quantity of interest—will be affected by how (or if) duration dependency is accounted for in the data. This example illustrates a more general point that the parameter estimates will sometimes be sensitive to the specification of the duration dependency parameter.[6]

To visually see the differences in the baseline hazard across the three logit models, consider Figure 1. The flat line labelled "Logit 1" gives the estimated hazard probability from the standard logit model; the hazard probability labelled "Logit 2" gives the baseline estimate for the model including $\log(t)$ in its specification; finally, the hazard probability labelled "Logit 3" gives the hazard for the model including a lowess function for t. The differences between the models accounting for duration dependency and the one that does not are substantial. In terms of "substantive" interpretation, if we were to solely look at the exponential equivalent, we would conclude the rate of adoption is constant. For the other models, the results suggest that the rate (or probability) is relatively higher immediately after the *Roe* decision and decreases over time.[7]

The extent to which one should ascribe substantive interpretation to the baseline hazard is a matter of debate (see Bennett 1998 for one view; Box-Steffensmeier and Jones 2004 for another). Our view is that duration dependency, or the particular shape of the baseline hazard is more appropriately viewed as a nuisance. Nevertheless, *failure* to account for duration dependency has adverse affects no matter how the parameter is interpreted: ignoring it all together leads to a model with an awkward and substantively unnatural interpretation. The central point we want to make is that ignoring duration dependency in a binary link model leads to an exponential equivalent. As such, issues pertinent to standard parametric

a duration dependency parameter. In this particular example, the difference is statistically significant, but not dramatically different.

[6]This is also an issue in the application of standard parametric models.

[7]This kind of hazard exhibits "negative duration dependency."

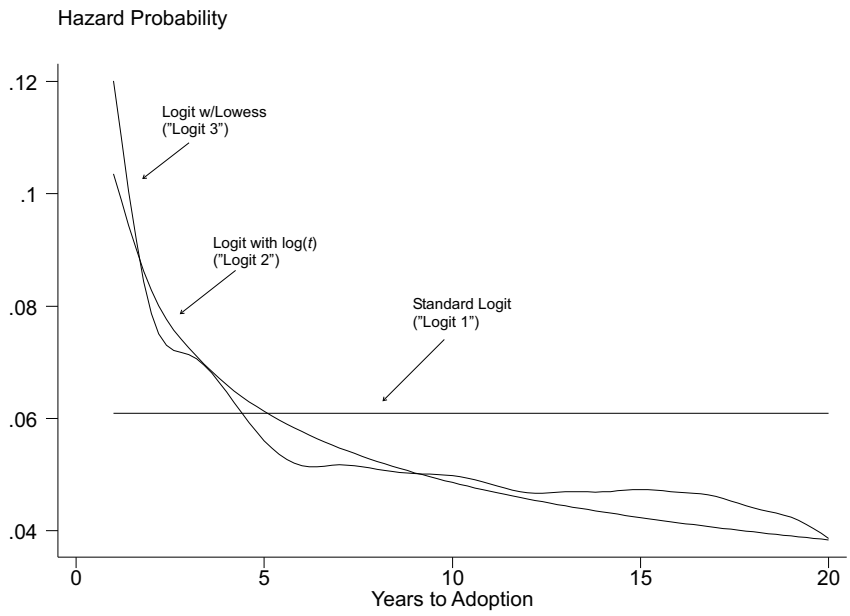

Figure 1: *This figure illustrates the baseline hazard functions from the three logit models shown in Table 1.*

duration models—i.e. the shape of the baseline hazard—become equally relevant for the logit and probit approach.

Consideration of a Cox Model

An approach used far less often in this literature is the application of Cox duration models. In our view, the Cox model has many desirable features that make it, in many instances, more applicable to the kinds of problems posed by state politics researchers. The Cox model departs most substantially from the logit-probit approach (or from approaches using standard parametric models) in that the form of the baseline hazard rate is left *unspecified*. More precisely, the hazard rate can take any form and may, in fact, be flat in between successive time points (Collett 1994). But although the baseline hazard is not directly specified (though as we will see, it can be retrieved from Cox estimates), covariate parameters are. Thus, the researcher can estimate "effect parameters" without having to make assumptions (which

could be wrong) about the shape of the baseline hazard rate. In the context of the logit-probit approach, this implies that one can forgo the issue of finding a function for t that best characterizes duration dependency.

The Cox model is often eschewed because of alleged substantive importance of the baseline hazard. That is to say, if one ascribes substantive meaning to the baseline hazard, then presumably models that estimate it directly will be preferred to models that do not. Hence, at least in political science applications, parametric models (or binary link models with duration dependency parameters) have been more widely used than Cox models. Box-Steffensmeier and Jones (2004) argue, however, that duration dependency should be less emphasized relative to coefficient estimates of theoretically relevant parameters. Since the nature, or "shape" of the baseline hazard will be highly sensitive to both its parameterization *and* to included covariates, a sequence of models on a set of data could produce, in principle, *both* positive *and* negative duration dependency. Since we view duration dependency as mostly a nuisance, it seems natural to consider a model where this dependency need not be specified. This leads to the Cox model. Therefore, it is worth describing the model briefly (see Collett 1994 or Box-Steffensmeier and Jones 2004 for much more details; see of course Cox 1972 as well).

The Cox model gets its name from the statistician who derived it, Sir David Cox. The importance of this model, as Allison (1995) and Box-Steffensmeier and Jones (2004) note, cannot be overstated. It is the "workhorse" model for most disciplines in which duration (or survival) data is commonly used. Under the Cox, the hazard rate is given by,

$$h_i(t) = h_0(t) \exp(\beta' \mathbf{x}),$$

where $h_0(t)$ is the baseline hazard function and $\beta'\mathbf{x}$ are covariates and regression parameters. In the Cox model, the form of the baseline hazard, $h_0(t)$ is not parameterized. Because of this, the Cox model is sometimes referred to as a "semi-parametric" model because the hazard rate is parameterized as a function of covariates but the particular distributional

form of the duration times is *not* parameterized. Owing to this result, Cox models do not have an intercept term. This can be seen by writing the Cox model as

$$h_i(t) = \exp(\beta_1 x_{1i} + \beta_2 x_{2i} + \ldots + \beta_k x_{ki}) h_0(t). \tag{3}$$

Expressing the model in terms of the log of the hazard ratios, we obtain

$$\log \left\{ \frac{h_i(t)}{h_0(t)} \right\} = \beta_1 x_{1i} + \beta_2 x_{2i} + \ldots + \beta_k x_{ki}. \tag{4}$$

For either (3) or (4), the constant term is "absorbed" into the baseline hazard function, and hence, is not directly estimated. The lack of a direct estimate of the baseline hazard function, as noted above, is often cited as a reason for forgoing the Cox model; however, if duration dependency is viewed as a nuisance, then this feature of the Cox model is actually desirable. It is also worth noting that a nonparametric estimate of the baseline hazard *can* be backed out of Cox model estimates, although this estimate will be highly adapted to the observed data. We return to this issue shortly.

Estimation of the Cox model requires the use of "partial likelihood" (Cox, 1972). Consideration of partial likelihood estimation would take us far beyond the scope of this paper and we refer the reader to Cox (1972), Collett (1994), or Box-Steffensmeier and Jones (2004). Nevertheless, there is one issue worth mentioning. As these (and many other) authors note, "tied" events—that is, the co-occurrence of two or more events at the same time—poses estimation problems for the Cox model. In the state adoption literature, tied events will be common given the discrete nature of the data used in this research. The prevalence of "ties" in duration data has been one oft-cited reason for avoiding the Cox model.[8] Nevertheless, several approximation methods have been developed to "sort out" the problems posed by tied events. Indeed, as Box-Steffensmeier and Jones (2004) note, one approximation method for the partial likelihood function results in an estimator that is *equivalent* to a conditional

[8]Indeed, Box-Steffensmeier and Jones (1997) state that data with more than 5 percent tied cases is probably unsuitable for the Cox model. This assertion is no longer valid.

logit estimator—a model perfectly suited for discretized duration data.[9] As such, the issue of ties in duration data is a far less important issue than it once was. Put differently, justification for use of the logit or probit models based solely on the grounds that these models can accommodate tied data is not, on its own, a reason to forgo the Cox model.

To illustrate an application of the Cox model to the abortion data, we reestimate the model from Table 1 and present the results in Table 2. The coefficient estimates in this table are expressed in terms of the hazard rate. Hence, a positively signed coefficient indicates that the hazard rate is increasing as the covariate increases in value; a negatively signed coefficient implies the opposite. Thus, an increasing hazard rate implies the "survival time," or time-until-adoption, is decreasing (i.e. shorter); a decreasing hazard rate implies the time-until-adoption is increasing. For this model, we see that the Mooney pre-*Roe* index of abortion permissiveness is negatively associated with the hazard rate. This suggests that the pre-*Roe* context of abortion rights in a state had a subsequent impact on state legislative behavior in the post-*Roe* era. Similarly, the measure of state ideology is also associated with the risk a state will adopt restrictive abortion legislation. We find that more liberal states are less likely to adopt restrictive abortion legislation than are more conservative states. Interestingly, the condition of divided government is positively related to the risk of adoption. That is, in states where the governor's party differs from the state house party, the risk, or likelihood of adoption increases.[10] Finally, the Cox model results suggest there is no relationship between a state's adoption of restrictive abortion legislation and neighboring states' adoption of such legislation.

In terms of the magnitude of the covariate effects, one useful way to interpret the coefficients is in terms of how much the hazard changes for a unit change in the covariate. If

[9] This approximation method is known as an "exact" method. In the software package Stata, one references this estimator by specifying the option `exactp`. Interestingly, one could obtain the same results by specifying the model as conditional logit (though some modifications may need to be made to the input data set in some applications).

[10] A more richly developed model would probably resolve this issue; that is, controlling for partisan control of the statehouse might explain the result. This information was not available in the data used for this illustration.

Table 2: Cox Model of Adoption of Restrictive Abortion Legislation

Variable	Estimate (s.e.)
Pre-*Roe*	−.23 (.09)
Ideology	.02 (.01)
Divided Gov.	.71 (.35)
Neighbor	.36 (.42)
N	386
Log-Likelihood	−92.25

Data are from Brace, Hall, and Langer (1999)

we exponentiate the coefficients, the hazard ratio is obtained. Hence, for the divided government covariate, the hazard ratio is $\exp(.71) = 2.03$. This ratio has a natural statistical interpretation: states for which the divided government condition holds are about two times more likely to adopt restrictive abortion legislation than states where the condition does not hold. The important thing to note is that even though the Cox model does not have an intercept term (which is typically used in regression-type models to give the baseline condition for covariates with a meaningful zero category—like dummy variables), full interpretation of the covariate is possible by looking at the hazard ratios. Under the Cox, the hazard ratio for the baseline case of 0 is exactly 1 (i.e. $\exp(.71 \times 0) = 1$). The contrast between a "1" and "0" on a binary variable is easy to compute, then, by simply calculating the hazard ratio. For the divided government variable, the ratio is $2.03 : 1$.

For continuous or semi-continuous covariates, the hazard ratio may also be used to describe the covariate effects. To illustrate how the hazard changes as a function of pre-*Roe* permissiveness and average state ideology, consider Figure 2. Here, we compute the change in the hazard across the range of these two covariates. The downwardly sloping line indicates that increases in the covariates are related to decreases in the hazard. For the pre-*Roe* covariate, we see that in states where abortion rights were most permissible, legislatures are about three times less likely to adopt restrictive abortion legislation than legislatures in states with a restrictive pre-*Roe* abortion environment. For the ideology covariate, we see that a legislature in the most liberal state is about five times less likely to adopt restrictive

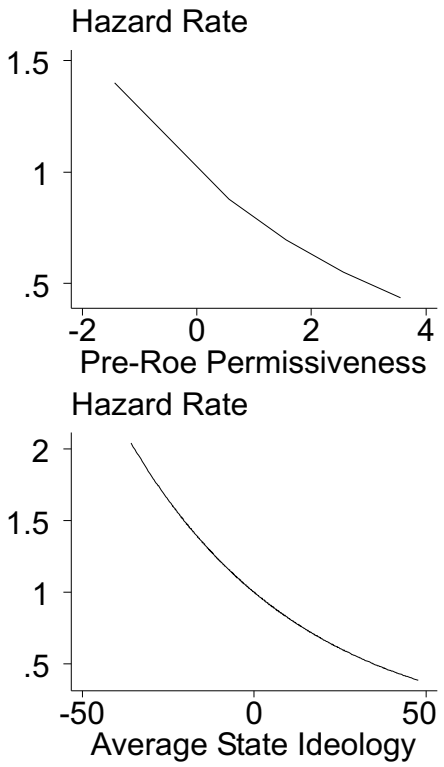

Figure 2: *This figure illustrates the change in the hazard for changes in the pre-Roe and ideology covariates.*

abortion legislation than a legislature from the most conservative state. The figure nicely illustrates the impact of these two covariates on the hazard.

In comparing the Cox results to the logit models from Table 1, we see there are differences in the results. For the logit models, the coefficient estimates for the state ideology covariate in the models with a duration dependency parameter are marginally significant ($p \approx .07$, 1-tail). Further, the neighbor covariate is marginally significant ($p. \approx .07$, 1-tail) in the logit models but insignificant in the Cox model. The differences across the models are most likely attributable to two features of the models: 1) differences in the partial likelihood estimator versus the maximum likelihood estimator; and 2) differences in how the baseline hazard is accounted for in the Cox vs. logit models (which is obviously related to the first point). In the logit models, duration dependency has to be directly accounted for (or if ignored, the

exponential equivalent must be accepted) while in the Cox model, this feature of the data is not estimated. Since parameter estimates may be sensitive to how duration dependency is parameterized, this can lead to differences between Cox estimates and estimates from other duration models (like logit or any standard parametric model). Because the Cox estimator is flexible insofar as the form of the duration dependency need not be specified, we generally prefer the Cox approach over the "logit-probit" approach. From the Cox estimates, we obtain our "effect parameters" which have a direct connection to the hazard rate without having to worry about accounting for duration dependency. Further, if this dependency is regarded as nuisance rather than substance, then this is a desirable feature of the Cox estimator.

If one is interested in examining the baseline hazard from a Cox model, this function is retrievable from Cox estimates. Collett (1994) and Box-Steffensmeier and Jones (2004) explain in some detail how to obtain this estimate. It is important to note that since the Cox model only uses information on the ordered failure times (or adoption times), then any resulting baseline hazard function will be highly adapted to the observed data; that is to say, it will be hard to visualize. To see this, consider Figure 3, where the baseline hazard function is generated from the Cox results presented in Table 2. The nonsmooth function (which looks like a city skyline) is characteristic of Cox baseline hazard functions. Since information on the ordered failure times is what is used to generate the partial likelihood estimates, the resultant baseline hazard is sensitive to the location of the observed event times.

To easier visualize the Cox baseline function, it is sometimes useful to smooth the function. In this figure, we used lowess to smooth the function and overlayed it on the nonsmoothed estimate. If one were interesting in ascribing substantive interpretation to this function, the smoothed baseline hazard would be considerably easier to interpret. Because we maintain the baseline hazard is of less substantive interest than are covariate parameters, however, our recommendation is to spend more time interpreting the theoretically relevant coefficients and less time interpreting the baseline hazard—a function that will be sensitive

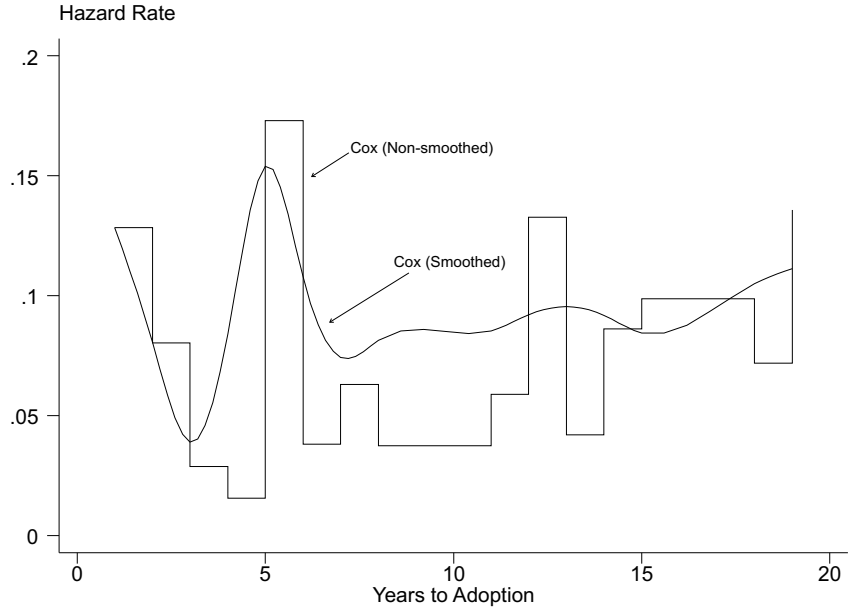

Figure 3: *This figure gives the non-smoothed and smoothed estimates of the Cox baseline hazard from a model of restrictive abortion legislation.*

to included covariates. Moreover, the baseline hazard should be of even *less* interest, in our view, to researchers using duration models for state adoption data. As the data are highly discretized, the function will be generally quite noisy. Indeed, this is another advantage of the Cox model. In the logit-probit approach, inclusion of duration dependency parameters probably create a false sense of precision about the shape of the baseline hazard, as a smooth function is generated on a political process that is otherwise "discrete".[11]

To conclude this section, we have provided some motivation for the consideration of the Cox model for use in state policy adoption research. As we noted, the garden variety logit and probit models produce exponential-equivalent models. Yet, even if one accounts for duration dependency (through transformations of t), the fact remains that the form of the baseline hazard must be specified in some way. In this sense, the logit-probit approach suffers from some of the same problems that Box-Steffensmeier and Jones (2004) point out

[11] We mean "discrete" in the sense that adoption of policy only occurs during legislative sessions, which are punctuated with periods of legislative recess and adjournments.

for parametric models. Moreover, if duration dependency is regarded as a nuisance, then a preferred modeling strategy might be one where covariate parameters are estimated but the baseline hazard is left unestimated. This kind of strategy is the essence of the Cox model. As we saw in our examples, substantive results can be easily produced by the Cox model while simultaneously avoiding the problem of interpreting a duration dependency parameters (or parameters if one uses spline functions or piecewise parameters [like year dummies; see Mintrom 1997]). Finally, because the Cox model has been readily extended to account for more complicated event structures, the basic model will be of considerable use for us as we move on to more difficult challenges that are often not dealt with in the policy adoption literatures. Specifically, we turn attention now to the problem of multiple events data.

Multiple Events Data and State Policy Adoption Research

A common duration modeling strategy in the policy adoption literature, and for that matter, in political science generally, is to focus on so-called "single-state" processes or "one-way" transition models. In a single-state process, attention is focused on the occurrence (or non-occurrence) of a singular event. Further, after the event's occurrence, it is assumed that the observation (for example, the state) "exits" the risk set. In the language of state policy adoption, models of single-state processes imply the researcher is *only* interested in a specific, singular kind of event (policy adoption). Moreover, once a state government adopts this policy, the state is assumed to be *no longer* at risk of adopting subsequent policies of the same or similar type. From this perspective, the only event of interest is the first event. Models that account for multiple events are sometimes called "repeatable events models." Additionally, researchers examining state policy adoption often do not account for the fact that *different* kinds of events can occur. For example, a state may adopt a certain type of restrictive abortion legislation. The usual practice is to categorize any particular type of policy in some domain as "an event" and make no attempt to discriminate between or

among event types. Duration models that do account for the occurrence of different kinds of events are sometimes called "competing risks" models. In this section, we consider these two types of models in conjunction with policy adoption data.

Repeatable Events Data

Suppose that states could adopt and *readopt* policies in the same legislative domain? To illustrate with example, consider Table 3. In this table, we reproduce a portion of a data set that records when and whether or not a state adopted obscenity legislation.[12] In this data set, which spans the period 1991 to 1998, states were recorded as either adopting or not adopting legislation pertaining to pornography and the adult entertainment industry. As with typical data sets on state policy adoption, the data are measured discretely and at each period, an event is either observed or not observed. In Table 3, data for two states are presented. For the first state, Colorado, we see that no event occurred during the time-span (i.e. the event indicator is "0" throughout). In terms of the "timing" of legislation, Colorado goes 8 periods (years) without policy adoption; hence this state is fully right-censored.

Contrast Colorado with Florida. In the second "risk period" (1992), we see Florida adopts legislation. Its recorded duration time is "2," because in the second year adoption occurs. Note, however, that in the third observation year ($t = 3$), Florida adopts another piece of obscenity legislation. In the column labelled "conditional" time, we note that the timing of this event, *conditional* on the previous event, is "1" indicating the second event occurs in the risk period following the first event. Hence, the "clock" starts "reticking" (i.e. set to 0) after the previous event occurs. Florida then goes three years until 1996, when yet another piece of legislation is adopted. In terms of the overall duration time, the event occurs in the sixth observation year; in terms of the conditional time—i.e. the time since the previous event—the timing occurs at $t_C = 3$, where the subscript C denotes this timing is conditional. Finally, after the third piece of legislation is adopted, the conditional time

[12]These data were graciously made available to us by Laura Langer.

Table 3: Example of Repeatability of Obscenity Legislation: A Tale of Two States

Year	State	Event	Time	"Conditional" Time	Risk Sequence
1991	CO	0	1	1	1
1992	CO	0	2	2	1
1993	CO	0	3	3	1
1994	CO	0	4	4	1
1995	CO	0	5	5	1
1996	CO	0	6	6	1
1997	CO	0	7	7	1
1998	CO	0	8	8	1
1991	FL	0	1	1	1
1992	FL	1	2	2	1
1993	FL	1	3	1	2
1994	FL	0	4	1	3
1995	FL	0	5	2	3
1996	FL	1	6	3	3
1997	FL	0	7	1	4
1998	FL	0	8	2	4

Laura Langer made these data available to us.

counter resets and two years pass and no further legislation is adopted (as of the end of the observation period). Hence, $t_C = 2$ and is right-censored at the last observation point.

The event history of obscenity legislation considerably differs between Florida and Colorado. Apart from the fact that Florida "experiences" the event, it experiences the event three different times. In contrast, an event is never recorded for Colorado. Hence, in terms of risk, Colorado, over the span of the study, is perpetually "at risk" of adopting its first piece of legislation. Thus, the last column of Table 3 (labelled "Risk Sequence") indicates the ordering of events for which the state is "at risk." For Colorado, the state is always in the risk set of adopting the first piece of legislation. In the Florida case, the state is only at risk of adopting the first piece of legislation for two periods. After first adoption, it then becomes at risk of adopting the second piece of legislation. After second adoption, it then is at risk of a third adoption, and so on.

The data in this example are illustrative of repeated events data. Repeated events data

pose several challenges for standard duration modeling techniques. Moreover, these challenges are highly relevant for state adoption data where it may be very likely, in some policy domains, for states to repeatedly adopt a certain kind of legislation (as in the example above). Unfortunately, the standard practice in much of the extant literature is to focus on single-state or single-spell processes. Models of single-state processes typically focus on the timing until the first event. This is troublesome if events can occur repeatedly.

There are at least two implications of focusing on time-until-first-adoption. First, there is an implicit assumption made that the first event is representative of *all* events; second, focusing on the first event results in an obvious loss of information. For the data in the previous example, a single-state duration model would explicitly treat Florida as exiting the risk set after first adoption, even though Florida repeatedly adopts obscenity legislation after the first event occurs. In a single-spell model, all of this information is lost.

The basic problem repeated events pose for policy adoption researchers is that the unit of observation—the state—*never* leaves the risk set for many conceivable policy domains. The question naturally arises as to how repeated events can be accounted for in the duration model. Fortunately, modeling repeated events is straightforward and requires some relatively simple modification to the standard Cox model considered in the previous section. While several modifications to the Cox model to account for repeated events have been proposed (c.f. Andersen and Gill, 1982; Therneau and Grambsch 2000, Wei, Lin, and Weissfeld, 1989; see also Box-Steffensmeier and Jones 2004 and Box-Steffensmeier and Zorn 2002), we focus on one type of model that is most applicable for policy adoption data (and probably political science data generally): the conditional "gap time" model.[13]

[13]We should note that the "simple modifications" to the Cox model that we refer to in the text are derived from extremely important work done on so-called "counting processes" (see Andersen and Gill 1982, Fleming and Harrington 1991, and especially Therneau and Grambsch 2000). Counting process theory has led to extensive developments on Cox model diagnostics as well as extensions of the Cox model into the domain of multiple events data (Therneau and Grambsch 2000; see also Box-Steffensmeier and Jones 2004). So while the modifications to the Cox model are relatively straightforward, requiring little more than redefining the composition of the risk set, the statistical theory from which these "simple" modifications come from is extremely rich and important for reasons far beyond the issues dealt with in this paper.

The "gap" in the conditional gap time model refers to the time interval between successive repeated events. Under this model, the duration time is assumed to reset after the occurrence of an event. Thus, after the first event is experienced, the observation then becomes at risk of the second event, at which time the duration time starts anew. Hence, the conditional model is "based on the idea that an observation is not at risk for the kth event until the $k-1$ event has occurred" (Box-Steffensmeier and Jones 2004, p. 159). Finally, to account for the sequencing, or ordering of events, the model is stratified by the event number. In Table 3, the event number corresponds to the "risk sequence" indicator. If T in a duration data set is constructed in terms of the conditional gap time (in Table 3, the column "conditional time" gives the gap time) and a Cox model is estimated that stratifies on event number (i.e. "risk sequence"), then a conditional gap time Cox model is obtained (see Box-Steffensmeier and Jones 2004 or Box-Steffensmeier and Zorn 2002 for more details). The implication of stratifying on event number is that the baseline hazard is allowed to vary by event number, but the covariate parameters are assumed to be the same across the ordered events. Hence, unobserved heterogeneity is "swept" out of the model into the k baseline hazards and what remains are estimates of the covariate parameters. Moreover, stratification allows for the preservation of the ordering of events, whereas nonstratified estimates presume ordering of events is unimportant.

To summarize, the conditional gap time model is easily estimated using the Cox model. The only modifications to the model is to adjust the risk set to account for repeatability of events and to alter the duration time so it measures the timing from the previous event. The data in Table 3 illustrate this kind of coding (Cleves 1999 provides an excellent overview on the risk set modifications that are necessary to estimate a variety of repeated events models).

To illustrate the conditional gap time model, we use the data on obscenity legislation adoption. This data set, though spanning a relatively short period of time, is replete with multiple events. Across the 50 states, there are 94 event occurrences producing a per-state average of a little under two events. Clearly, one-way transition models, if applied to these

data, would see a considerable loss of information. The models of obscenity legislation adoption include six covariates. The first is a measure of the percentage of citizens in a state who claim to be Fundamentalists. This measure is used as an indicator of a state's level religious conservativeness. Presumably, the greater the number of fundamentalists in a state, the more prone the state government may be to respond to this constituency with obscenity legislation. The second covariate is a binary indicator denoting Southern vs. non-Southern states. Data on state-wide education levels is also included. These data are based on U.S. Census data and is a measure of the percentage of college educated citizens in the state. Two measures of ideology are also included. The first is a 100-point scale measuring the ideology of the state legislature. Higher scores denote a more liberal legislature while lower scores denote a more conservative legislature. The second ideology covariate measures citizen ideology. Here, higher scores denote a more liberal public; lower scores denote a more conservative public. The percentage of women in the state legislature is also included as a covariate. The idea here is that legislatures with relatively more women may be more prone to adopt obscenity legislation. Finally, a covariate measuring the states' murder rate was included in the models. We included this covariate as an indicator of the general level of criminal violence in the state. The idea is that states with a greater prevalence of violent crime may also be more likely to adopt legislation pertaining to pornography and the adult entertainment industry.

To illustrate the implications of repeated events data on models of state policy adoption, we estimated three separate Cox models. The results are shown in Table 4. The first column of estimates correspond to a standard single-spell Cox model (like the one discussed in the previous section). The implication of this model is that *only* the first event is of substantive interest. Consequently, instead of using information on all 94 events, this model only takes into account the first event, of which there are 37 adoptions. Thus, less than 40 percent of the actual events are used in this analysis. Unfortunately, much of the literature on state policy adoption focus singularly on the first event. In some settings, this may be theoretically

Table 4: Adoption of Obscenity Legislation: Cox Conditional Gap Time Model

	Single-Spell Model	Non-Stratified Events Model	Repeated Events Model
Variable	Estimate (s.e.)	Estimate (s.e.)	Estimate(s.e.)
Fundamentalist	-.02 (.21)	.18 (.13)	.22 (.10)
South	.79 (.56)	.19 (.46)	.04 (.42)
% College Ed.	.11 (.07)	.12 (.06)	.12 (.05)
Leg. Ideo.	-.07 (.01)	-.03 (.01)	-.02 (.01)
Citizen Ideo.	.09 (.02)	.05 (.02)	.04 (.01)
% Women Leg.	-.07 (.04)	-.01 (.03)	.00001 (.02)
Murder Rate	.24 (.06)	.16 (.04)	.18 (.04)
N	226	400	400
Log-Likelihood	-147.23	-447.21	-334.18

The first column of estimates are from a Cox Single Event model; the second from a Cox Independent Events model; the third from a Stratified Cox model with Repeated Events.

warranted; however, if it is done merely out of convenience (i.e. because it is easier to estimate a single-spell model than a multi-spell model), then the implications could be severe.

How severe? In the third column of Table 4 (we will visit the second column of estimates shortly) we estimate the Cox conditional gap time model discussed above. Here, we stratify on the event number and as such, maintain information on the ordering of events. Moreover, because repeatability of events is directly modeled, all 94 events enter the model: there is no loss of information. In comparing the single-spell estimates (first column) to the repeated events model (third column), we see substantial differences across the coefficient estimates. For the single-spell model, the South covariate seems to increase the hazard rate; however, across all events, the South covariate seems to have *no* impact on obscenity legislation adoption. Additionally, in looking at the single-state model, we would conclude that the prevalence of fundamentalists in a state has *no* impact on obscenity legislation. Yet when looking at a model accounting for all events, we find this characteristic of the population is strongly related to the hazard of adoption. States with more fundamentalists are more prone to adopt this kind of legislation.

Regarding the ideology covariates, we see that between the single-state model and the

repeatable events model, the coefficients are statistically significant; however, the magnitude of the effect is much less strong in the repeatable events model. The conclusions one would draw from a single-state model versus a repeatable events model would differ: focusing on the first event, ideology (both citizen and legislative) has a substantial impact on the risk of adoption. Taken over all events, the effects are significant, but less pronounced.[14] Additionally, we "learn" from the single-state model that the percentage of the legislature that is comprised of women is associated with a lower hazard of *first* adoption. Yet comparing this "finding" to the repeatable events model, this counter-intuitive result does not hold. We see that the percentage of women in the legislature has no impact on the risk of a state adopting obscenity legislation. Finally, we find the effect of the murder rate covariate (which is included as an indicator of the prevalence of violent crime) is substantial across either model (though the magnitude of the effect is a bit smaller for the repeatable events model).

The central point of this illustration is to demonstrate that one's results can and very likely will vary, depending on how the event history process is characterized. Estimation of single-state models in the face of repeatable events data results in an obvious loss of information. Yet even worse, the conclusions from the single-state model do not, for this application, naturally map onto the results from the repeatable events model. Put differently, conclusions about the political process would differ substantially from one model to the next. Because the conditional gap time Cox model uses all of the information on the events and further, uses information on the ordering of events, we would strongly prefer this model to the single-state Cox model.

Finally, to make another comparison to the conditional gap time model, we estimated a Cox model that ignored the ordering of events. This model is presented in the second column of Table 4. Specifically, this model is an unstratified Cox model.[15] In this model, informa-

[14]Interestingly, the effect of citizen ideology on adoption differs from legislative ideology (i.e. one increase the hazard, the other decreases the hazard). We intend to investigate this result further in a more substantively oriented paper.

[15]We should note that all of the models in Table 4 account for clustering using the Lin-Wei variance estimator.

tion on all 94 events is used, but the ordering of the events is assumed to be unimportant. An additional implication of this model is that the baseline hazard function is assumed to be identical across all events that occur within a state. This is in contrast to the stratified model, where the baseline hazard for each event number is allowed to take its own shape. In comparing the fit of the two models (looking at the log-likelihood), it is clear the stratified Cox model provides a superior fit to the unstratified model, though both use information on the repeatable events. The covariate estimates are similar across the two models though certainly not identical (though the South coefficient is much different between the two models). Given the fit of the stratified Cox model over the unstratified model, we would again prefer this model.

The approach outlined in this section provides the policy adoption researcher with a method to account for repeatable events. Preserving the elegance of the Cox model, estimation to account for repeatable events requires only slight modifications to the duration times and to the definition of the risk set. Further, it is our view that this approach is superior to logit or probit attempts to account for repeatable events through the inclusion of an "event counter" (see Beck, Katz, and Tucker 1998). Therneau and Grambsch (2000) and Wei and Glidden (1997) have expressed skepticism about this approach. Moreover, use of the logit or probit model in this setting still does not obviate the problem posed by duration dependency. The model proposed here, like any Cox model, avoids the specification of the baseline hazard. This feature of the Cox model is, in our view, even more desirable in the repeated events setting, a setting where the baseline hazard may substantially vary over the k ordered events. In the state policy adoption literature, the possibility of repeatable events would seem to be important to consider. In this section, we have hoped to demonstrate an approach to deal with this issue and moreover, convey the implications of *ignoring* repeated events. In the next section, we turn attention to a different type of problem posed by multiple events: competing risks.

Competing Risks Data

In most applications of duration models in political science, including the policy adoption literature, there is a tendency to focus on single events. Indeed, this focus gives rise to some of the problems discussed in the previous section in the context of repeated events. However, another problem with the focus on singular events can arise. Earlier, we used data on the adoption of restrictive abortion legislation. In that model, we made no attempt to distinguish among the different types of legislation that could be adopted (indeed, in our repeated events model, we only accounted for ordering of events, and not for the different types of obscenity legislation). Frequently, defining events in a broad fashion will be suitable for the problem under study. This will of course always be the case when the researcher is mostly interested in state adoption of any kind of legislation within some policy domain. However, in other research settings, it may be interesting and substantively natural to consider event occurrences in a more refined way.

Returning to the restrictive abortion example, in the post-*Roe* era, it turns out that state policies restricting abortion tend to fall into one of four categories: policies that required spousal consent; policies requiring informed consent; policies that required parental consent; and policies that limit funding for providers of abortion rights (Brace and Langer 2001). Generically, duration models that explicitly account for multiple kinds of events are sometimes referred to as "competing risks" models or "multi-state" processes. In this section, continuing our theme of developing the Cox model, we discuss two variants of the Cox model that can be applied to problems of competing risks. We do note that the literature on competing risks problems is vast and a number of models have been proposed for these kinds of data; our focus is on Cox extensions (for a fuller overview of different types of competing risks models, see Box-Steffensmeier and Jones 2004 or Gordon 2002).

The first variant of the Cox competing risks model we consider has considerable similarity to the Cox conditional gap time model discussed in the previous section. In terms of policy

adoption processes, it is assumed that states are "at risk" of adopting one of m type policies. Moreover, because states may adopt multiple policies, it is assumed that they never leave the risk pool. Using a substantive example, suppose that there four kinds of restrictive abortion policies a state could adopt: informed consent, parental consent, spousal consent, and limited funding. Moreover, suppose that the adoption of one policy did not preclude the adoption of another type of policy. In this scenario, if a state adopted policy m, it would still be at risk of adopting any of the remaining $m - 1$ policies.

For many policy domains, this seems like a plausible condition. Fortunately, accommodating competing events in situations such as this entails a relatively straightforward modification of the standard Cox model. Under this variant of the Cox model, each state is assumed to be at risk of adopting any of m policies. There are no restrictions on when or which of the m policies a state adopts; however, since the state is in the risk set for each m policy, the physical data set used for the analysis requires that each state in the analysis appears m times in the data set, once for each possible event (Cleves 1999 discusses in detail the data set-up necessary to estimate this variant of the Cox model). Once each state is recorded as being at risk of each of the m events, a stratified Cox model can be applied to the data, where the stratification variable indicates the event type. Under this model, it is assumed that the covariate effects are the same for each event type but that the baseline hazard for each event (or m policy types) can uniquely vary across the competing events. In this model, then, the stratified Cox model gives estimates of a single set of parameters while the m baseline hazards can be backed out of the Cox estimates. In this sense, stratification serves the same purpose as that in the repeatable events model: heterogeneity not accounted for by the covariates is "swept" into the m baseline hazards. As Box-Steffensmeier and Jones (2004) note, "this approach to the competing risks problem may be appropriate for many kinds of competing risks problems that emerge in political science, especially where the occurrence of an event does not imply the observation exits the sample" (p. 174). This situation would seem highly applicable to state policy adoption data.

To illustrate the stratified Cox competing risks model, we consider the example of restrictive abortion legislation. In this data, states are assumed to be at risk of adopting any of the four possible kinds of restrictive abortion legislation described previously. The data we use in this application are the same data used in our previous models of restrictive abortion legislation. The principal difference is that we now are explicitly accounting for the type-specific policy that is adopted. Results from a stratified Cox competing risks model as well as a standard Cox model (i.e. one that does not discriminate among event types) are presented in Table 5. The covariates used in this illustration include a binary indicator denoting whether or not the state was a southern state. The covariate denoted as "ideological distance" is a measure of the difference between the state legislature's ideology score and the state's high court's ideology score (see Brace and Lange 2001 for more details on the coding of this covariate). The scale of this variable is such that higher scores indicate the legislature is more liberal than the court while lower scores indicate the court is more liberal than the state legislature. The covariate is expected to be negatively related to the hazard (c.f. Brace, Hall, and Langer 2001 and Brace and Langer 2001). Since legislatures may anticipate court intervention, as the ideological distance between the legislature and the court increases, the risk the legislature incurs of adopting restrictive abortion legislation decreases (i.e. the hazard rate should decrease and hence the time until adoption (of any type) should increase). The covariate labelled "neighbor" in the table corresponds to the proportion of bordering states that have adopted one of the m policy types. The pre-*Roe* abortion permissiveness covariate discussed earlier is also used in this illustration as an indicator of the climate for support or opposition to abortion rights. The covariate "unified government" is scored 1 if the upper and lower chambers in the state house are controlled by the same party and 0 if not. Finally, the covariate denoted as "constitutional right" in Table 5 indicates whether or not a state explicitly had a construed constitutional right to abortion.

Of principal interest in this illustration is to ask whether or not accounting for competing risks has any value-added for our statistical estimates. In comparing the competing risks

Table 5: Cox Models of Restrictive Abortion Legislation

	Competing Risks Cox	Standard Cox
Variable	Estimate (s.e.)	Estimate (s.e.)
South	.66 (.26)	.50 (.26)
Ideology Distance	−.14 (.07)	−.13 (.08)
Neighbor	.11 (.16)	.07 (.16)
Pre-*Roe*	−.19 (.06)	−.19 (.06)
Unified Gov.	.11 (.22)	.01 (.22)
Const. Right	−.92 (.28)	−.88 (.28)
N	2554	2554
Log-Likelihood	−455.23	−544.09

Data are from Brace, Hall, and Langer (2001)

estimates (given in column 1) from Table 5 to the standard Cox estimates (given in column 2), we see that the differences across the parameter estimates are somewhat different. Not accounting for event-type differences, the standard Cox model seems to understate the effect of the south covariate (though both models produce a statistically significant result). The other covariates are fairly similar across the two models. The major difference between the two models lies in the overall fit. The stratified Cox seems to fit the data better (in terms of the log-likelihood) suggesting that accounting for competing events produces a model preferable (on statistical grounds) to a model that does not. Additionally, under the standard Cox model, the baseline hazard is constrained to be identical over the four possible event types, while in the competing risks model, this assumption need not be maintained.

In Figure 4, we compute the baseline hazard estimates from the stratified Cox model and present the results in the first four panels. The top left panel corresponds to the baseline hazard for informed consent; the top right panel gives the baseline hazard for parental consent; the middle left panel gives the baseline result for limited funding; and the middle right panel gives the hazard for spousal consent. The bottom panel corresponds to the baseline hazard estimates from the standard Cox model presented in Table 5. It is immediately clear that the type-specific baseline hazards markedly depart from the unstratified Cox estimates. This is especially true for the parental consent event type, where the hazard is clearly rising

over time. While we do not advocate ascribing much substantive interpretation to baseline hazard functions, Figure 4 *does* reveal that there is considerable heterogeneity in the baseline hazards. Allowing them to vary—which is a result of the stratified Cox competing risks model—-produces a model superior to one where the baseline hazards are constrained to being equal across event types. Thus, although the differences between the standard Cox model (which is in the spirit of typical applied research on state policy adoption) and the competing risks model were not so stark as our repeatable events models, accounting for multiple events seems to produce a superior model, at least in this example. As a general modeling strategy for policy adoption, this approach would seem fruitful, particularly given the fact that states can, presumably, adopt a wide range of policies within a given legislative domain. Ignoring this variability has the implication of placing perhaps unrealistic assumptions on the baseline hazard function. This, in turn, could have the effect of inducing unaccounted-for heterogeneity into the estimates.

An alternative competing risks model is one where the covariate parameters are allowed to vary over the m-specific events. This model is a considerable departure from the stratified Cox model from above. In this model, the covariates are assumed to operate the same for each of the m outcomes. Under a "type-specific" hazard model, the covariates can have different effects for each of the m outcomes. A Cox variant of this type of model may be estimated in the following way. Estimate a Cox model for the mth event type and treat the remaining $m-1$ outcomes as being right-censored. Doing this for each m policy type will produce m Cox models with $k \times m$ sets of covariates, where k corresponds to the number of covariates. Crowder (2001), David and Moeschberger (1978) and Hougaard (2000) demonstrate that this modeling strategy essentially decomposes the overall likelihood (or partial likelihood) into m subcontributions, each corresponding to the unique event type (see also Diermeier and Stevenson (1999) for an excellent application of this model to a political science problem; see also Box-Steffensmeier and Jones (2004) who describe this model in more detail).

We estimate the m-specific hazards (or subhazards as Crowder (2001) refers to them)

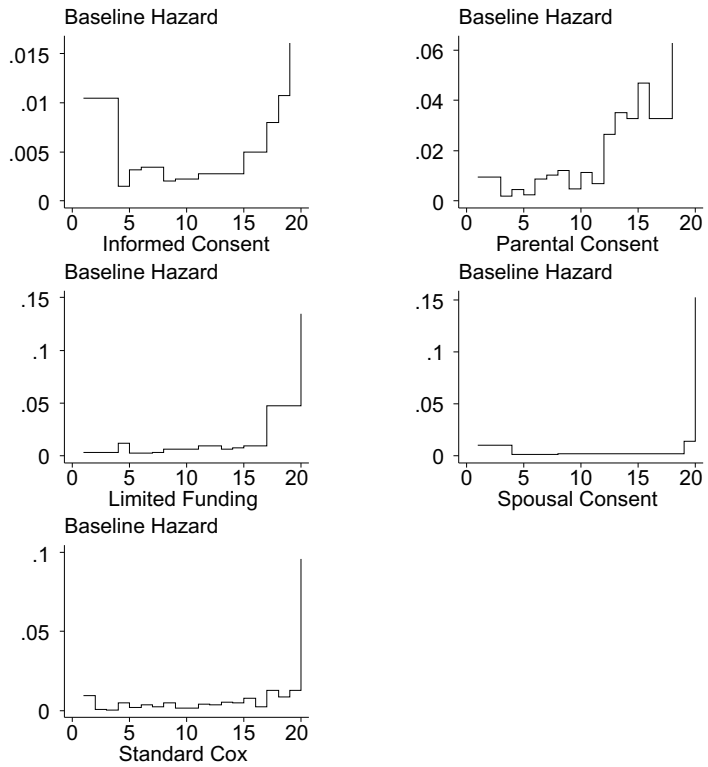

Figure 4: *This figure gives the estimates of the Cox baseline hazards. The first four panels correspond to baseline estimates from a stratified Cox competing risks model; the bottom panel comes from a standard Cox model.*

using the abortion adoption data and present the results in Table 6. The same covariates used in Table 5 are used in this illustration. The utility of this model over the stratified model is that the coefficients can assume different values for the m-specific outcomes. In Table 6, each column corresponds to one of the possible policy types the state can adopt. Looking at the parameter estimates, we see that in some cases, there is considerable variability. For example, the ideological distance covariate seems to be most strongly related to the parental consent outcome and not related to the other $m-1$ policy types. Why this is the case is not immediately clear from theory and might suggest that further consideration on model specification is necessary. Likewise, the pre-*Roe* permissiveness index is strongly (and negatively) related to the risk of informed consent and spousal consent adoption, moderately

Table 6: Cox Competing Risks Models of Adoption of Restrictive Abortion Legislation

	Informed Consent	Parental Consent	Limited Funding	Spousal Consent
Variable	Estimate (s.e.)	Estimate (s.e.)	Estimate (s.e.)	Estimate (s.e.)
South	.32 (.59)	.31 (.46)	.93 (.49)	.35 (.63)
Ideology Distance	−.17 (.16)	−.32 (.14)	.08 (.14)	−.05 (.18)
Neighbor States	.05 (.36)	−.05 (.28)	.01 (.31)	.38 (.36)
Pre-*Roe*	−.26 (.14)	−.14 (.10)	−.12 (.11)	−.29 (.16)
Unified Gov.	.34 (.47)	−.24 (.36)	.21 (.44)	−.34 (.53)
Const. Right	−1.06 (.65)	−.93 (.47)	−.82 (.53)	−.66 (.63)
N	386	386	386	386
Log-Likelihood	−132.75	−222.67	−157.08	−91.25

Data are from Brace, Hall, and Langer (1999)

related to parental consent adoption, and not related to limited funding policy adoption. This may be the case because consent legislation may be viewed as far more restrictive than funding legislation. Hence the pre-*Roe* environment on abortion rights has its strongest lingering effects when it comes to adoption of the most restrictive types of legislation. The one consistent covariate across the four policy types is the constitutional right indicator. States having a construed constitutional right to abortion are far less likely to adopt any type of the legislation.

Given that the covariates seem to be sensitive to the event type, this model may provide some evidence that the stratified Cox assumption of equal covariate effects is unwarranted. Users of this model should be aware of some caveats, however. Because the overall hazard function is partitioned into m subhazards, the type-specific models may be highly sensitive to the number of events that are actually observed within each category. If the number of m-specific events is small, then the parameter estimates will obviously be sensitive to this. That is to say, with few events per category, the Cox model has less information to work with in deriving estimates of the coefficients. For this illustration, this issue may be relevant, especially for the spousal consent category where there are only 17 events in the data set.

Nevertheless, the major point we want to emphasize is that *ignoring* the possibility

that different kinds of events can occur may induce substantial heterogeneity problems into the estimates. Moreover, this practice—which is common in policy adoption literature—may result in a considerable loss of information. If one has reason to think that adoption of different kinds of events within a policy domain is substantively and theoretically interesting, then the modeling strategies discussed here seem naturally applicable. The choice between the stratified competing risks model and the type-specific competing risks model may be regarded as an empirical question, although theory may suggest one model over the other in some applications. Yet as a way to enrich analyses of state policy adoption, models that account for different kinds of events would seem, on the face of it, to be preferable to models where events are defined singularly.

Conclusion

The primary goal of this paper has been to present some alternative modeling strategies for researchers using duration models for the problem of policy adoption. As noted, the typical research strategy in this research has been the application of binary link models like logit or probit. While there is nothing "wrong" with the use of these models—indeed they are appropriately applied to BTSCS duration data—we argue issues inherent in this approach may sometimes lead the researcher to a Cox model. The Cox model avoids the basic issue of having to parameterize the baseline hazard function. Because covariate parameters—presumably the feature of the model researchers are most interested in—can be highly sensitive to the baseline hazard parameterization, we contend a model that leaves this function unspecified will often be preferable to the standard logit-probit approach. Since Cox estimates are easily interpretable in terms of the hazard rate, the same kinds of substantive information forthcoming from a binary link model is produced by the Cox model.

Moreover, modifications to the standard Cox model leads to models that effectively deal with multiple events problems, which was another major issue of this paper. It is incontrovertible that states, in some policy domains, can adopt and readopt legislation. Further,

states, in some domain, may adopt multiple kinds of policies. The usual practice in the policy adoption literature has been to focus on single events in single-spell models. As we demonstrated, this practice can have adverse effects. Apart from an obvious loss of information, this approach can lead to contradictory conclusions when results from a single-spell model are compared to results from multiple events models. As such, we discussed some variants of the Cox model that allows for repeatable and competing events.

Several issues, of course, have not been discussed. Our focus has been on the Cox model and its variants. An issue inherent with any Cox application is the proportional hazards assumption (see Box-Steffensmeier and Jones 2004 or Box-Steffensmeier and Zorn 2001 for more details). Researchers should be aware of this assumption and test for it. There are several easily applied tests for this assumption and resolving violations to the assumption are quite straightforward (again, see Box-Steffensmeier and Jones 2004 or Box-Steffensmeier and Zorn 2001 for more details).[16] Additionally, several other modeling strategies that could be fruitfully applied to policy adoption data were not discussed, due to space limitations. Among these include so-called "flexible parametric models" (Royston and Parmar 2001, 2002; see also Box-Steffensmeier and Jones 2004), dependent risks models (Gordon 2002), and frailty models (or random coefficients models). Future work will discuss the application of these models to state adoption data. Yet despite the omission of these topics, we think the problems discussed herein and the solutions proposed offer a wider menu of modeling strategies to researchers examining state policy adoption.

[16] In all of the Cox models presented in this paper, we assessed the proportional hazards assumption using Harrell's ρ and in each case, we found this assumption to hold.

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