DOING TIME: DYNAMICS OF IMPRISONMENT IN THE
REFORMIST STATE*

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Most explanations of official social control point either toward reform movements or
the imperatives of the social system as dominantly influencing imprisonment rates, with
little attempt to integrate these distinct causal processes. This study aims to disentangle
these effects by arguing that the strategic behavior of official state actors plays an
intervening role in the punishment process that determines the relative salience of
reform and systems effects. The empirical analysis focuses on the expansion of prisons
and jails in the American states between 1880 and the early 1920s. Treating reform in
terms of the adoption of probation, parole, and indeterminate sentencing legislation,
and treating the social system as a store of resources likely to affect institutional
expansion, the analysis pursues a series of dynamic additive and interactive models.
The findings support the argument that reforms introduced discretion into the social
control system and allowed official actors greater freedom to adjust their behavior to
shifting bureaucratic and political constraints.

What factors determine a society’s capacity for sanctioning deviant behavior? In the last 20
years or so, this question has reemerged as a central issue in the sociology of deviance,
supplanting to some degree traditional research on individual causation. A major impetus for
this shift has been the conviction, derived in large part from labeling theory, that deviance is
produced primarily by rules that define inappropriate behavior and prescribe legitimate societal
responses and only secondarily by prior differences between deviant and nondeviant actors
(Becker 1963; Erikson 1966; Lemert 1967). As a result, studies have increasingly concentrated on
the historical origins and consequences of the legal rules and institutions that constitute
modern strategies of control, such as the criminal law, the prison, the asylum, and the
welfare system.

Yet research in this area has approached a theoretical impasse from two different direc-
tions. As Humphries and Greenberg (1981) have argued, accounts of official social control fall
into two broad types: “systems theories,” in

which legal controls arise in response to functional need (whether of society as a whole
or, in the Marxist variant, of the ruling class), and “theories of agency,” in which entreprene-
ural movements create new forms of deviance independent of structural context. These ac-
counts differ sharply in their choice of dependent variables, explanatory models, and predic-
tions about long-term trends in punishment rates. Systems models concentrate on the
development of legal systems and dominant modes of sanctioning (Parsons 1951, 1964;
Schwartz and Miller 1964; Rusche and Kirchheimer 1969; Chambless 1964). Whether in-
spired by Durkheim or Marx, they treat sanctioning as a mechanism of societal equili-
bration. In this view, intermittent reforms serve only to achieve short-term adjustments to
long-term socioeconomic imperatives. Thus, sanctioning rates are predicted to be stable over
time, net of a variety of structural effects.

Agency models, on the other hand, are typically applied to such millenial and parochial
reforms as Prohibition (Gusfield 1963), marijuana legislation (Becker 1963), and the juvenile
court (Platt 1969). They tend to emphasize the arbitrary and socially constructed nature of
many control reforms, often invoking a diffuse “status politics” argument that fails to address
issues of power or explain why some reforms are more successful and enduring than others.
They frequently predict, but seldom demonstrate, that reform causes abrupt increases in
rates of sanctioning.1

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1 See, e.g., critiques of Platt’s (1969) analysis of juvenile court reform by Hagan and Leon (1977) and
Systems and agency models approach the issue of control from nearly opposite directions. From one perspective, social systems evolve solutions to problems of social disorganization as they arise; from the other, social movements invent problems to fit their preconceived solutions. To this point there has been little direct comparison of the effects of systemic constraints and entrepreneurial deform, and the clear differences in the two perspectives prevent easy synthesis. The research reported here will focus on one factor emphasized in research by Berk and his colleagues (Berk et al. 1981, 1983; Messinger et al. 1985) that strict systems and agency models tend to ignore: the strategic and organizationally self-serving behavior of official actors who administer the social control apparatus. While conventional models tend to treat the state and its agencies as passive recipients of externally generated imperatives, a growing, diverse literature suggests that they may have a decisive impact on the generation of reform initiatives as well as their implementation. At the most general level, this approach rests on a view of modern states as complex and loosely coupled systems of organizational entities that develop their own agendas and competencies and devote considerable resources to self-legitimation (Skocpol 1985). In the politically volatile area of criminal justice, official actors face a special problem of legitimation: given a chronic scarcity of resources, they are expected to appear both humanitarian and competent in response to crime. Therapeutic reforms address this problem by expanding discretionary decision making and enhancing the self-regulatory capacity of the legal system. Reforms increase the flexibility of the system as a whole by creating new agencies, policies, and sanctioning options that permit greater selectivity in the use of coercion (Abel 1981; Garland 1985; Hagan et al. 1977; Rothman 1980; Spitzer 1975; Turk 1969). Police, court, and correctional officials use their discretion to regulate the flow of offenders in response to changing bureaucratic and political exigencies (Berk et al. 1983; Bittner 1967; Black 1980; Sudnow 1965). Reforms may increase discretion further by incorporating professional entrepreneurs and their scientific diagnoses into the official control system (Becker 1963; Foucault 1977; Hagan 1979; Scull 1977). This approach suggests that sanctioning rates are determined by the ways in which official actors use strategic discretion to manage their domains of action and only indirectly by reform or socioeconomic imperatives.

This study will explore these arguments by analyzing the mutual effects of legal reform and socioeconomic resources on imprisonment in a specific historical context. I focus on three watershed criminal justice reforms—probation, parole, and indeterminate sentence laws—and on changes in inmate populations in the American states between 1880 and 1923. These reforms are the highest achievements of the Progressive ideal of "socialized" and "individualized" justice (Boyd 1917; Mead 1918) in the area of criminal law. According to Rothman (1980), all three emerged amid growing skepticism about the efficacy of 19th century prisons and asylums. Professional organizations like the National Prison Association, the National Conference of Charities and Correction, and the National Probation Association sponsored these policies as the keys to a more scientific and therapeutic legal system. While these reforms fell far short of their therapeutic goals (U.S. National Commission 1931; U.S. Department of Justice 1939; Rothman 1980), they nevertheless increased the variability of criminal sentences and enhanced the legitimacy of the embattled criminal justice system.

While these reforms shared a common ideological foundation, they had different implications for the distribution of discretion in the criminal justice system. Probation laws had the unique potential to affect the rate of prison admissions; in effect, they increased and formalized judges' discretion to withhold sentences. Parole legislation, by contrast, created specialized bureaucratic agencies to control the rate of prison releases. If Messinger et al.'s (1985) study of California law is generalizable, parole was not initially viewed as an instrument of rehabilitation. Instead, it began as a means to relieve the governor of incessant demands for executive pardons. Later it was used routinely to relieve prison overcrowding (Berk et al. 1983). Indeterminate sentencing laws tended to follow parole. Sentencing reform was promoted as a means to invest parole with rehabilitative potential, indeed to make imprisonment itself therapeutic by making release contingent evidence of reform (Wines 1919, ch. 10). In practice, mandatory indeterminate sentencing reduced judges' control over the length of sentences and increased the discretionary authority of prison bureaucrats. In reaction to the 19th-century heritage of harsh sentences and chronically overcrowded and underfunded prisons, these policies introduced legitimate discretion into the sentencing process, but each in a different way and with potentially varying consequences.

The literature contains few detailed hypotheses about the relative effects of agency and

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2 This seems to have been a common pattern in the emergence of parole laws. See the historical notes in the U.S. Justice Department's Survey of Release Procedures (1939).
systems factors. On logical grounds, however, it is possible to suggest three alternative causal scenarios that approximate the arguments just discussed. Following the implicit argument of agency theory, the first is that the enactment of reform legislation has a direct and independent effect on imprisonment rates. A second possibility is that imprisonment rates respond to broad structural imperatives and that reform effects operate only in service of a more general process of equilibration. This is the core of the systems theory argument; in this study I follow Stinchcombe (1965) in conceptualizing the social system as a store of resources required to mount an organized response to deviance. The third possibility, corresponding to a "strategic discretion" approach, is that reform institutionalizes new interests within the social control regime in the form of offices, roles, and responsibilities, creates new options for the deployment of resources, and thus refracts the influence of systemic factors. In this formulation, therefore, reform and resource effects are interactive. Subsequent empirical analysis will treat these scenarios as nested models. As listed here, each is a more complex specification, as well as a falsification, of the one preceding.

Two special features of the analysis deserve mention before moving on. First, I will conceptualize systemic resources from three different theoretical perspectives. Following the standard distinction made in the social control literature (Chambliss 1976; Hagan et al. 1977; Hopkins 1975; Meier 1982), I will distinguish between a Durkheimian "consensus" approach, which emphasizes general societal complexity as the driving force behind criminal sanctions, and a Marxian "conflict" approach, which emphasizes the disruptive effects of capitalist market relations and class cleavages. In addition to these models, I pursue the logic of the strategic discretion approach by exploring the organizational capacity of the state as a determinant of sanctioning rates. The argument here is that official actors adjust their behavior to local resource flows rather than to general conditions of societal development.

The second feature of the analysis is the empirical comparison of growth rates in different kinds of institutions. To gain the broadest possible picture of official sanctioning behavior, I analyze imprisonment both in state prisons and local jails (including workhouses and lock-ups). To maintain a standard baseline for comparisons, I consider only sentenced inmates in both settings, ignoring, for example, inmates held in jail pending trial.

DYNAMIC MODELS OF IMPRISONMENT

Hypotheses and Estimation Procedures

Studies of imprisonment in the U.S. suggest that levels of incarceration are constrained not so much by actual rates of deviance as by societal capacities for processing and confirming deviant actors (McKelvey 1936; Pontell 1984). My empirical analysis treats imprisonment as an organizational issue; that is, I assume that prisons and jails tend to grow to the limits set by environmental constraints. Following previous literature on organizational expansion (Freeman and Hannan 1975; Nielsen and Hannan 1977), the first task is to define a hypothetical ceiling on the imprisonment capacity of a state, \( I^* \), and to set out a model that describes its dependence on exogenous conditions. Because we lack any a priori evidence to the contrary, we can assume that the relationship is linear:

\[
I^* = a' + c'X + d'POP
\]

Equation (1) proposes that the maximum "capacity to punish" (Pontell 1984) is set by the size of the population (POP) and some exogenous variables (X), representing both reform measures and resource constraints.

One advantage of this model is its flexibility. Following Durkheim ([1938] 1964) and Blumstein et al. (1977), one may interpret equation (1) as an equilibrium model in which imprisonment adjusts in stable ways to population and, perhaps, to other exogenous factors. Recent research by Berk and colleagues (Berk et al. 1981, 1983; Rauma 1981; see also Greenberg 1977) has shown both the logical and empirical weaknesses of this "stability of punishment" hypothesis. In this research, I will not address it directly. But it is not necessary to treat equation (1) as a stable equilibrium model. As Tuma and Hanna (1984, pp. 338–39) have argued, \( I^* \) can be interpreted more simply as the maximum level of punishment that can be sustained by exogenous factors. Because I am concerned mainly with exogenous effects, my primary attention will be to the parameters associated with the variables in X.

In historical terms it is impractical to assume that \( I^* \) is ever achieved, since exogenous conditions vary over time, and officials cannot respond instantaneously to reform legislation or shifts in the availability of resources. It is more realistic to suggest that at any given time systems of control are expanding or contracting to close the gap between the current level of imprisonment, \( I_t \), and the target, \( I^*_t \). For convenience one may assume that the task of adjustment is a continuous-time process, even though in practice it may be driven by annual funding cycles or decisions to admit or release
inmates made weekly or daily. This process can be portrayed in a linear partial-adjustment model:

\[ \frac{dI_t}{dt} = b' (I_{t-1} - I_t) \]  (2)

The parameter \( b' \) gives the rate at which the system expands or contracts to meet its carrying capacity. The higher the absolute value of \( b' \), the more responsive the system is to external contingencies. Implicit in this formulation is the notion that different systems of incarceration vary in their ability to respond to changes in the environment. In subsequent analysis it will be important to compare the relative responsiveness of prisons and jails.

Note, however, that equation (2) suggests no explicit role for environmental factors. They can be incorporated in the form of exogenous variables by substituting (1) into (2), yielding the full dynamic model:

\[ \frac{dI_t}{dt} = b'a' - b'1 + b'c'X + b'd'POP \]  (3)

In substantive terms, equation (3) proposes that the instantaneous rate of growth (or decline) in imprisonment is a function of the current number of inmates, some exogenous factors, and population. Since instantaneous rates of change are unobservable, equation (3) cannot be estimated directly. Coleman (1968) has shown that one solution to equation (3) is a linear regression equation with a lagged dependent variable on the right-hand side:

\[ I_t = a + bI_{t-k} + cREF_{t-k} + dPOP \]  (4)

Model (4) can be estimated by pooling a minimum of three equally spaced panels of observations into a set of time-series. It yields parameters that can be used to calculate coefficients for the dynamic equation, but, as Nielsen and Rosenfeld (1981) have argued, one can also interpret equation (4) directly and substantively. While the parameter \( b' \) in equations (2) and (3) is an indicator of system responsiveness, \( b \) in equation (4) is its inverse—an indicator of inertia, or resistance to change, due to internal structural characteristics. The \( c \) and \( d \) parameters estimate the marginal impact of exogenous constraints, net of inertial effects.

Readers familiar with the research of Berk et al. (1981, 1983) will note clear differences between their strategy of estimating a partial-adjustment model of imprisonment and that given in equation (4). Part of this difference arises from the fact that I have conceptualized imprisonment as an organizational phenomenon and seek to take advantage of a modeling approach that is already widely used in the macro-organizational literature. Another difference is that, as explained above, I do not address a strict "stability of punishment" hypothesis, an argument to which the Berk et al. models are explicitly directed. But most of the difference concerns our different sources of data. This study uses four waves of panel data, and theirs a much longer time-series from a single state. While Berk et al. use a series of lagged regressors to tame the residuals in their models, this strategy is impractical in a panel study where only a few observations are available. I describe below an alternative strategy for dealing with autocorrelation.

Earlier discussion identified three models of imprisonment that are of interest: one containing reform effects only; a second containing reform and resource effects; and a third in which reform and resources interact. These models can be specified as direct extensions of equation (4). The reform effects model has the form

\[ I_t = a + bI_{t-k} + cREF_{t-k} + dPOP \]  (5)

in which \( I \) is the number of sentenced inmates in prisons or jails, \( REF \) signifies the existence of a probation, parole, or indeterminate sentence law, and \( POP \) is a control for total state population. \( REF \) represents a set of dummy variables that take the value 1 if a given law is in effect at \( t-k \), and 0 otherwise. The \( REF \) dummies are lagged by one observation (10 years) on the grounds that any major reform requires some lead time before its effects are felt. Supplementary tests using five-year lags and no lags (instantaneous effects) showed that this specification achieves the strongest results.

It is not clear a priori what effects to expect from reform variables. Reforms could have slowed the growth of imprisonment by providing alternatives to incarceration, routinizing early releases, and shortening minimum sentences. On the other hand, each of these reforms expanded the net surveillance capacity of the state; thus they could have encouraged imprisonment if offenders who otherwise would have remained free were imprisoned for infractions of probation and parole rules. The best empirical evidence of this is in Berk and his colleagues' studies of imprisonment in California. They found no effect of probation (Berk et al. 1981) and a negative effect of parole on rates of prison expansion (Berk et al. 1983). Since, as the authors note, California may be unusual in its administration of sentencing reforms, and since they did not study jails at all, their findings are only suggestive. In general, I will treat equation (5) as an exploratory model. But, based on what is known about the content of these reforms, it is possible to hypothesize that the magnitude of their effects—whether positive or negative—
will vary systematically between prisons and jails. Since probation is a sentencing option at the discretion of the judge, it is likely that it had its greatest impact (if any) on local jail populations. This effect could have worked in two ways. First, probation was probably awarded more frequently to minor offenders who would otherwise have received short sentences to local institutions. Second, since most judges were local officials, they were probably more responsive to overcrowding in jails than in prisons. Conversely, since parole legislation was administered by state parole boards and prison administrators, it is likely that its primary effect was on penitentiary capacities. Indeterminate sentence laws specified a range of time rather than a fixed sentence for a variety of major and minor crimes. Since these laws were intended as a supplement to parole laws, their major effect was probably on prison releases.  

A second set of models adds to equation (5) a term that represents systemic resources, here represented as RES:

\[ I_t = a + b I_{t-k} + c_1 R E F_{t-k} + c_2 R E S_{t-k} + d P O P_t \]  

(6)

Note that in this equation, the added resource measures are also lagged. My expectation is that decisions to build or expand institutions, like changes in sentencing policy, have a delayed effect on punishment rates. But it is well known that, beyond their projected capacity, institutions always have some elastic capacity that is realized through under- or overcrowding. The equation includes lagged values of resource measures to account for the long-term expansion of intended prison and jail capacities, as well as contemporaneous population measures to control for short-term variation in elasticity.

If, as a systems argument suggests, legal reforms are a means by which punishment rates are equilibrated to resource availability, any effects of reform variables observed in equation (5) should be washed out in this model by the overriding effects of the resource variables. In practice I will be concerned with three sets of resources that represent the alternative consensus, conflict, and strategic discretion arguments.

From the consensus argument I will focus on urbanization and the spread of literacy in the population as salient resources for the expansion of control institutions. Again following Stinchcombe (1965), it may be argued that these variables capture a generalized level of competence for organizing activity: urbanization contributes to the concentration of wealth and the increasing density of exchange networks, and growing literacy signifies an increased capacity for learning, communication, and launching collective projects. Urbanization also suggest a more practical influence on sanctioning. In the terms of consensus theory, urban life erodes traditional normative constraints on behavior and creates new opportunities for deviance—surely an important resource. Under either interpretation, we would expect these variables to be positively associated with imprisonment.

The conflict model is a special case of the consensus argument. Here the relevant context is not society in general, but a specifically capitalist society. The most important resources are those that signify a potential for class-based discord. The analysis to follow examines immigration and the size of the industrial wage-labor force as indicators of inequality and the growth of capitalist market relations. The substantive argument that underlies this model is a standard one in the conflict literature. Immigrant groups were perceived by elites to be in special need of containment and socialization, and, in fact, they make up a disproportionate share of the institutionalized population at the turn of the century. Wage workers are presumed by definition to be cut loose from the affective controls of kinship and community. In a developed capitalist economy, a massed working class poses a threat to economic stability that is likely to elicit a punitive response.

This model ignores the effect of unemployment on imprisonment, even though this factor is emphasized by many conflict theorists (Rusche and Kirchheimer 1969; Rusche 1978; Box and Hale 1982; Greenberg 1977; Jankovic 1977). The practical reason for this omission is that state-level data on unemployment are not available for the period covered by this study. But I doubt that such data would be very helpful, given the structure of the analysis. The panel design of the models, with observations on independent variables spaced every 10 years, by its very nature emphasizes long-term effects on the expansion of prison capacity. Unemployment cycles are likely to have their strongest effects over the short term, on the use of existing capacity. It would be interesting to test this effect, but it is of a different order than the others explored here. By excluding it I am...
admittedly limiting the comprehensiveness of the analysis, but not impairing the validity of the results.

Finally, I offer two substantive hypotheses about the strategic behavior of official actors. First, it seems likely that the expansion of the social control system depends not just on general resources, but on the ability of the state to extract wealth for its own purposes. Put simply, wealthy governments are more likely to build new prisons than poor governments. I will explore the effects of state government revenue on institutional expansion. I expect that state revenue will have a major positive impact on state prison growth; its effect on local jail capacity will probably be relatively weak, or perhaps negative. Second, extending the argument by Berk et al. (1983) that prisons are “self-regulating” systems, I will explore whether prisons and jails adjust their capacities to each other. The most straightforward hypothesis is that large inmate populations in one sector will contribute to expansion in the other, as judges and other administrators attempt to even out population pressures. But it is also possible that the two sectors will show inverse patterns of growth. The construction of state prison systems, for example, may represent a trend toward the centralization of control that relieved local institutions of some of their clientele. Conversely, a strong system of jails may have inhibited the growth of prisons.

Estimates of the interrelationships of reform and resource effects will be obtained by adding a multiplicative dummy-variable interaction term to the previous additive equations:

\[ I_t = a + bI_{t-k} + c_1 REF_{t-k} + c_2 RES_{t-k} + c_3 (REF \times RES)_{t-k} + dPOP_t \]  

In substantive terms, equation (7) suggests that the effects of resource constraints vary, depending on whether states have enacted probation, parole or indeterminate sentencing laws. But again it is difficult to suggest in advance what direction those differences might take. It could be argued that reform increased the social control system’s legitimacy and administrative efficiency, permitting a more effective pursuit of available resources, or that new policies created new loci of decision making that buffered the system from external contingencies. Messinger et al. (1985) argue that sentencing reform offered California officials a strategic opportunity to reduce overcrowding, and the historical literature suggests that prison overcrowding was a chronic problem nationwide. Thus we would expect the latter direction to be the case.

Here are two last notes on estimation. First, it is widely known that in pooled regression models, OLS techniques yield inconsistent estimates because they fail to correct for autocorrelated disturbances. In models with lagged dependent variables, parameter estimates are also likely to be biased. My analysis uses a variant of a generalized least-squares approach described by Kmenta (1971, pp. 509–12), in which it is assumed that errors are first-order autoregressive, but cross-sectionally independent. Estimation proceeds in three steps. First, OLS is applied to the original data, and the resulting residuals are used to calculate estimates of \( \rho_1 \), the autocorrelation coefficient for each case. In the second step, the \( \rho_1 \)'s are used to weight the data and remove its autoregressive characteristics. The last step is to reestimate the model using OLS on the transformed data. 6

A second estimation problem arises from heteroskedastastic residuals caused by the enormous size differences among the states. Here I follow Johnston’s (1963, pp. 207–11; see also Firebaugh and Gibbs 1985) suggestion of dividing through the entire equation, with the exception of the legal reform dummy variables, by population. Thus, for example, the weighted form of equation (5), with the addition of an error term \( u_t \), becomes

\[ \frac{I_t}{POP_t} = a(1/POP_t) + b(1/POP_t) \\
+ c_1 REF_{t-k} + d(1/POP_t) + (u_t/POP_t) \]  

Scatterplots show that the residuals from such models are reasonably well behaved.

Data

This study uses three kinds of state-level longitudinal data: counts of inmate populations, dates of reform legislation, and resource measures. To fulfill the requirements of pooled regression techniques, the data were collected and organized into four panels of observations spaced according to decennial census intervals from 1890 to 1920, with lagged indicators reaching back to 1880. With four panels of data...

6 Potential bias due to contemporaneously correlated errors are not dealt with directly here. Problems of this sort may arise whenever all the individuals in the sample are affected by a single time-specific event. For example, wars or depressions might influence the rate of imprisonment across all states. As a simple check for such effects, models reported here were re-estimated with the inclusion of dummy variables for each time period—a variant of the well-known “least-squares with constants” procedure (Kmenta 1971, p. 516; Tuma and Hannan 1984, pp. 434–38). Results were not affected in any substantial way. The estimation program used here was written using the Statistical Analysis System MATRIX procedure.
on 48 states and territories, the resulting data set contains 192 observations.

Inmate data are drawn from a series of special institutional censuses conducted by the U.S. Census Office (1888, 1895) and the Bureau of the Census (1907, 1918, 1926) in 1880, 1890, 1904, 1910, and 1923. All figures are from enumerations made on a single day, thus population figures are independent of variation in rates of admission or average sentence length. Since officials had been counting penitentiary convicts since 1850, there are some grounds for trusting the completeness of these data. Jail enumerations were probably less thorough, but I see no evidence of systematically biased counts. While criteria for enumerating and reporting different categories of inmates varied from one census to another, it was possible to recover counts of one set of inmates—sentenced prisoners, exclusive of those held for nonpayment of fine and those held in insane asylums or military or Federal prisons—for penitentiaries and jails at all intervals except 1890. I estimated the 1890 figures by interpolating state counts from adjacent censuses, and scaling the result to the aggregate U.S. figure (reported in U.S. Bureau of the Census 1926, p. 7, Table 1). Finally, data for 1904 and 1923 were scaled to decennial years of 1900 and 1920 by linear interpolations.

Table 1 shows inmate data aggregated at the national level from 1880 to 1923, both in absolute numbers and as a proportion of the population. According to these figures, U.S. prison capacity more than doubled over 40 years of fairly stable growth. Per capita, prisons grew 16 percent between 1880 and 1890, and remained at about the same level after that. Jails grew only slightly during this period, with a notable surge around 1910, and their capacity declined relative to population growth. These different patterns of expansion underline the importance of distinguishing between these two imprisonment strategies.

Dates of probation, parole, and indeterminate sentence laws are taken from an exhaustive historical survey of state policies conducted by the U.S. Department of Justice (1939). As described earlier, these data are transformed into dummy variables that indicate whether each state had adopted each reform at each time (t – k). This dummy variable approach seems inevitable given the panel structure of the data, but it is imprecise because states did not enact reforms at convenient 10-year intervals. Imagine two states, one of which passed a probation law in 1901, and the other in 1910. Both would be coded the same on the probation variable, with zeros at 1880, 1890, and 1900, and ones at 1910 and 1920. When imprisonment in 1920 is regressed on the value of the probation dummy in 1910, both states appear to have identical probation policies even though one has had 19 years to respond to the new legislation and the other only 10. Dummy coding introduces some error into the models, but it is unlikely that this error systematically biases the data, since this could occur only if states showed a patterned preference for reform earlier or later in the decade.

Systems variables are drawn from regular decennial census reports (USCO 1883, 1892; USBC 1904; 1913a, 1913b, 1923, 1933). Urbanization, literacy, immigration, and wage labor are measured respectively as the number of persons who lived in cities over 25,000 population, who were literate over age 10, who were foreign born, and who were employed for wages in manufacturing industries. State government revenue is expressed in terms of constant 1967 dollars. All resource variables as well as total population are

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7 The 1910 census reported sentenced inmates only in state totals, omitting separate counts for prisons and jails. Breakdowns by specific institutions did not distinguish sentenced from nonsentenced inmates. I estimated separate counts for prisons and jails on the assumption that all nonsentenced inmates and all serving time for nonpayment of fines were in local institutions (an assumption supported by other, more detailed, censuses). Subtracting total prison inmates from sentenced inmates thus leaves (estimated) sentenced jail inmates.
coded in units of 100,000. Descriptive statistics on independent and dependent variables at each decade (excluding reform dummy variables) are reported in Appendixes A and B.

FINDINGS

Reform Effects

The first step was to explore the simple effects of reform on prison and jail expansion, controlling only for population. Results from this step are reported in equation form in Table 2 (a key to variable names appears in Appendix C).

Two reform effects emerge from this table. First, parole reform (PAR) shows a significant negative effect on the rate of prison expansion. Neither probation (PROB) nor indeterminate sentencing (IND) appear to have independent effects on prisons. This result confirms the California findings of Berk et al. (1981, 1983), and suggests that parole had a decelerative effect on incarceration nationwide. The second reform effect does not, however, fit so neatly with expectations: it appears from the jail model that the adoption of indeterminate sentencing policies significantly slowed the growth of local institutions. Probation has no apparent effect. The simplest and most plausible account of this result is that indeterminate sentence laws shortened the minimum penalty for many crimes, and speeded the flow of inmates through local institutions. Whether inmates routinely served only minimum terms, or whether jail officials actively adjusted the rate of release—to alleviate overcrowding, for example—cannot be determined from the present data.

One other pattern of effects in Table 2 deserves mention. The parameters for the lag dependent variables run counter to the prediction that prison systems are slower to respond to exogenous constraints than jails. The lag jail parameter is significantly higher than the lag prison parameter, signifying higher inertia in the more decentralized institutions. How will these estimates behave when resource measures are added into the equations?

Additive Resource and Reform Effects

In the second step of the analysis, indicators of resource constraints added to equations contain-

8 Indicators were chosen with an eye toward reducing multicollinearity. In the models reported below, the highest pairwise correlation is .52 (between state revenue and lag prison inmates). Belsley, Kuh, and Welsch (1980) collinearity diagnostics were estimated for all models shown subsequently. Unless stated otherwise in the text, condition index values were below 20, indicating no degradation of the estimates due to multicollinearity.

ing the reform dummy variables. Results from the consensus models are reported in Table 3, from conflict models in Table 4, and from models focusing on state-specific resources in Table 5.

In Table 3, note the performance of the reform measures when resource effects are added to the models. In the model of prison expansion, the previously observed effect of parole drops below significance, as predicted by a systems perspective. In the jail model, however, the negative impact of indeterminate sentencing persists, and appears to be independent of resource effects. A systems interpretation is complicated further by the substantive influence of resource measures. Urbanization shows no effect on prison growth, but a strong positive effect on jails. Literacy, on the other hand, shows a powerful negative effect on prison expansion, and no influence on jails. Finally, note the parameters for the lag dependent variables. Here prisons appear slightly more inertial than jails, but this effect is insignificant because the estimates are less than two standard errors apart.

It is premature at this point to offer general interpretations of these results, but I can make two observations. First, Table 3 offers no particular support for systems theory in general, or a consensus model in particular. Second, and more substantively, it appears that prisons and jails occupied different niches than those popularly assumed. Prison systems grew fastest in the least developed states, and jails persisted longest in the most urbanized states. These findings suggest that city jails were a major source of institutional growth during this period. For a more precise definition of the niche parameters of prisons and jails, we must explore additional models.

Conflict models displayed in Table 4 show equally surprising results with resource effects. The size of the immigrant population had no effect on expansion in either sector. The wage-labor parameter in the prison equation is negative, indicating that prisons grew fastest where the industrial labor market was least developed. This effect is reversed in the jail model: the association between wage labor and jail expansion is positive and significant. This suggests a need to rethink the conflict argument. Why should prison expansion decline and jails prosper in states with large industrial working classes? One explanation may lie in the activities of the early labor movement. McKelvey (1936) observes that around the turn of the century, unions, especially the powerful Knights of Labor, opposed the use of convict labor in prison industries where it could compete with free labor, and legislators in many large states responded by severely limiting the
Table 2. Pooled GLS Estimates of Reform Effects on Prison and Jail Expansion (Standard Errors in Parentheses, $N = 192$)

<table>
<thead>
<tr>
<th></th>
<th>PRIS</th>
<th>LPRIS</th>
<th>PROB</th>
<th>PAR**</th>
<th>IND</th>
<th>POP***</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>31.79**</td>
<td>+ .59</td>
<td>- 3.64</td>
<td>- 12.42</td>
<td>+ .01</td>
<td>+ 45.04</td>
</tr>
<tr>
<td></td>
<td>(5.50)</td>
<td>(.05)</td>
<td>(5.06)</td>
<td>(4.78)</td>
<td>(5.45)</td>
<td>(3.57)</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th></th>
<th>JAIL</th>
<th>LJAIL</th>
<th>PROB</th>
<th>PAR</th>
<th>IND*</th>
<th>POP***</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>6.17**</td>
<td>+ .78</td>
<td>+ 3.11</td>
<td>+ .53</td>
<td>- 7.70</td>
<td>+ 10.48</td>
</tr>
<tr>
<td></td>
<td>(2.34)</td>
<td>(.04)</td>
<td>(2.55)</td>
<td>(2.58)</td>
<td>(3.01)</td>
<td>(1.57)</td>
</tr>
</tbody>
</table>

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

Table 3. Pooled GLS Estimates of Urbanization, Literacy, and Reform Effects on Prison and Jail Expansion (Standard Errors in Parentheses, $N = 192$)

<table>
<thead>
<tr>
<th></th>
<th>PRIS</th>
<th>LPRIS</th>
<th>URB</th>
<th>LIT***</th>
<th>PROB</th>
<th>PAR</th>
<th>IND</th>
<th>POP***</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>27.90**</td>
<td>+ .73</td>
<td>- 6.65</td>
<td>+ 3.26</td>
<td>- 7.68</td>
<td>+ 4.39</td>
<td>+ 75.31</td>
<td>(7.09)</td>
</tr>
<tr>
<td></td>
<td>(5.92)</td>
<td>(.05)</td>
<td>(14.03)</td>
<td>(5.32)</td>
<td>(4.95)</td>
<td>(5.57)</td>
<td>(7.09)</td>
<td>(5.57)</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th></th>
<th>JAIL</th>
<th>LJAIL</th>
<th>URB***</th>
<th>LIT</th>
<th>PROB</th>
<th>PAR</th>
<th>IND*</th>
<th>POP*</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>11.64**</td>
<td>+ .69</td>
<td>+ 32.25</td>
<td>- 2.62</td>
<td>- .94</td>
<td>- 6.23</td>
<td>+ 6.80</td>
<td>(3.25)</td>
</tr>
<tr>
<td></td>
<td>(2.50)</td>
<td>(.05)</td>
<td>(8.98)</td>
<td>(7.10)</td>
<td>(2.65)</td>
<td>(3.11)</td>
<td>(3.25)</td>
<td>(3.11)</td>
</tr>
</tbody>
</table>

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

Table 4. Pooled GLS Estimates of Immigration, Wage-Labor, and Reform Effects on Prison and Jail Expansion (Standard Errors in Parentheses, $N = 192$)

<table>
<thead>
<tr>
<th></th>
<th>PRIS</th>
<th>LPRIS</th>
<th>IMM</th>
<th>WAGE*</th>
<th>PROB</th>
<th>PAR</th>
<th>IND</th>
<th>POP***</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>33.30**</td>
<td>+ .58</td>
<td>- 10.38</td>
<td>+ 1.32</td>
<td>- 7.05</td>
<td>- 5.03</td>
<td>+ 52.87</td>
<td>(4.27)</td>
</tr>
<tr>
<td></td>
<td>(6.47)</td>
<td>(.05)</td>
<td>(28.49)</td>
<td>(5.48)</td>
<td>(5.04)</td>
<td>(5.57)</td>
<td>(4.27)</td>
<td>(4.27)</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th></th>
<th>JAIL</th>
<th>LJAIL</th>
<th>IMM</th>
<th>WAGE***</th>
<th>PROB</th>
<th>PAR</th>
<th>IND*</th>
<th>POP***</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>11.51**</td>
<td>+ .64</td>
<td>+ 7.35</td>
<td>- .98</td>
<td>- .09</td>
<td>- 7.20</td>
<td>+ 6.64</td>
<td>(1.55)</td>
</tr>
<tr>
<td></td>
<td>(2.97)</td>
<td>(.05)</td>
<td>(13.43)</td>
<td>(2.59)</td>
<td>(2.44)</td>
<td>(2.87)</td>
<td>(1.55)</td>
<td>(1.55)</td>
</tr>
</tbody>
</table>

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


employment of inmates. Union pressure destroyed any possibility that penitentiary regimes would pay for themselves, and may indirectly have slowed the growth of prison systems. This interpretation is tentative, however, since it is far from clear that this effect is captured by an indicator of the sheer number of industrial workers.

Reform effects in Table 4 are similar to those in Table 3. When resource effects are controlled, reforms appear to have had no significant effect on prison expansion. Indeterminate sentencing still shows a significant decelerative effect on jails. Again, inertia effects are not significantly different across sectors.

Table 5 shows tests of the effects of state-specific resources on imprisonment. Again, results on the resource measures defy expectations. State revenue is negatively associated with prison expansion, and shows no effect on jails. Nor is there any sign that the two sectors influence each other: large jail populations do not appear to have encouraged prison growth, nor did large prisons encourage the expansion of jails. Once again the prison model shows no significant reform effects, while the negative effect of indeterminate sentencing on jails remains strong.

Findings from these additive models run contrary to almost all the hypotheses with which the analysis began. Jails grew fastest (or declined least) in the most urbanized and industrialized states. If there is any truth to the consensus or conflict arguments, it appears that local jails rather than prisons served as the first line of defense against social disorganization. In the three prison models, none of the resource measures is positively associated with the expansion of state prison systems. Indeed, three of them—literacy, wage labor, and state revenue—show significant negative effects. On the face of it, this means that the worst educated, least industrialized, and poorest states expanded their prisons most rapidly. This conclusion suggests that we are focusing on the wrong sets of resources, or, less plausibly, that resources of any kind are not meaningful determinants of institution-building capacity.

Another way to approach this issue is to think not just about absolute levels of resources, but also about patterns of temporal change in resource availability. One suggestive piece of evidence is that, according to Appendices A and B, literacy rates, wage-labor markets and especially state revenues all grew rapidly during this period; across all states they grew faster than imprisonment rates or population. A second clue concerns the ranking of the states on these variables. In the early years of the study, the cases ranking lowest in literacy, wage labor, and revenue were territories and new states in the mountain and far western regions—places where obviously there was little or no opportunity for institution building. In later observations, these cases tended to move out of the lowest ranks, to be replaced often by southern states. Third, parallel analyses of regional tendencies show that prisons expanded slowest in the north Atlantic and New England states and fastest in the western regions. Taken together, these clues suggest a cohort effect in which newer states, starting from nothing, moved more rapidly than older states in building up their social, economic, and administrative infrastructures.

One simple way to test for this cohort effect is to create a variable, called TIME, which indicates the number of years a state was in the Union at each observation, and to enter that variable, along with relevant resource and reform measures, into equations predicting prison growth. Because I want to compare frontier states with all others (i.e., I am less concerned with differences among older states), TIME is logged. For simplicity, estimated models contained only one resource variable, PAR, and TIME, along with population and the lag dependent variable. If the negative effects of literacy, wage labor, and state revenue are, in fact, byproducts of a cohort process, these effects should fade when TIME is included in the models, and the parameter associated with TIME should be negative. Results are shown in Table 6.

These estimates generally support the cohort argument. In all three, the TIME parameter is negative and significant. In the first model, literacy maintains a significant negative effect; apparently this variable is not tied to the institutional age of states. However, models 2 and 3 show no effects of wage-labor or state revenue once TIME is controlled. Together, these results suggest two conclusions. First, as hypothesized, frontier states expanded their prison systems faster than more established core states. Second, they apparently did so on the basis of anticipated rather than actual resources. In subsequent analyses, I will continue to use the WAGE and REV variables rather than the simpler, but less revealing, TIME measure. Keep in mind, however, that these variables represent important time-dependent effects as well as cross-sectional variation among states.

In sum, analysis of linear models has yielded no categorical support for either agency or systems theories in their pure form. The negative association between parole and prison expansion may be spurious, since it does not ______

---

9 These are average tendencies. Extensive tests of regional effects using dummy variables to represent regions showed no significant associations.
Table 5. Pooled GLS Estimates of Revenue, Inmate Populations, and Reform Effects on Prison and Jail Expansion (Standard Errors in Parentheses, N = 192)

<table>
<thead>
<tr>
<th>PRIS =</th>
<th>35.03***</th>
<th>+.72</th>
<th>.175</th>
<th>REV**</th>
<th>+.11</th>
<th>LJAIL</th>
<th>+1.37</th>
<th>PROB</th>
<th>-6.38</th>
<th>PAR</th>
<th>+.23</th>
<th>IND</th>
<th>+43.18*</th>
<th>POP***</th>
</tr>
</thead>
<tbody>
<tr>
<td>(6.15)</td>
<td>(.07)</td>
<td>(.64)</td>
<td>(.11)</td>
<td>(.57)</td>
<td>(.28)</td>
<td>(1.95)</td>
<td>(5.95)</td>
<td>(5.73)</td>
<td>(10.73)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>JAIL =</td>
<td>4.34</td>
<td>+.78</td>
<td>-.05</td>
<td>REV**</td>
<td>+.03</td>
<td>LPRIS***</td>
<td>+3.11</td>
<td>PROB</td>
<td>+.80</td>
<td>PAR</td>
<td>-7.94</td>
<td>IND**</td>
<td>+9.27***</td>
<td>POP***</td>
</tr>
<tr>
<td>(2.71)</td>
<td>(.05)</td>
<td>(.29)</td>
<td>(.03)</td>
<td>(.66)</td>
<td>(.33)</td>
<td>(2.63)</td>
<td>(3.02)</td>
<td>(1.67)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

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

Table 6. Pooled GLS Estimates of Time-Dependence Models of Prison Expansion (Standard Errors in Parentheses, N = 192)

1. PRIS = 23.01*** + .74 LPRIS*** - .5983 LIT*** - 4.69 PAR - 4.67 TIME* + 85.36 POP***
   (6.89) (.05) (14.58) (3.66) (2.18) (8.23)
2. PRIS = 19.43** + .67 LPRIS*** - 62.91 WAGE - 8.86 PAR* - 7.59 TIME*** + 75.47 POP***
   (7.05) (0.06) (46.20) (3.70) (2.23) (7.98)
3. PRIS = 19.07* + .76 LPRIS*** - .74 REV - 6.52 PAR - 8.83 TIME*** + 75.37 POP***
   (7.56) (0.06) (53) (4.10) (2.10) (8.27)

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

Table 7. Pooled GLS Estimates of Significant Interaction Effects on Prison Expansion (Standard Errors in Parentheses, N = 192)

1. PRIS = 28.26*** + .61 LPRIS*** - 56.23 URB** - 18.54 PAR*** + 57.09 (URB × PAR)* + 48.92 POP***
   (5.90) (.05) (19.93) (5.57) (25.48) (4.15)
2. PRIS = 27.92*** + .62 LPRIS*** - 190.25 WAGE** - 21.45 PAR*** + 196.36 (WAGE × PAR)** + 51.60 POP***
   (5.67) (.05) (59.71) (5.63) (79.74) (4.14)
3. PRIS = 38.23*** + .78 LPRIS*** - 2.29 REV** - 32.75 PROB* + 2.95 (REV × PROB)* + 44.06 POP***
   (6.04) (.07) (59) (15.61) (1.36) (3.67)
4. PRIS = 36.51*** + .73 LPRIS*** - 2.02 REV*** - 20.92 IND* + 1.75 (REV × IND)* + 46.91 POP***
   (5.81) (.06) (52) (8.77) (8.0) (3.74)

* p < .05.
** p < .01.
*** p < .001.
appear independent of resource effects. Under all specifications, however, indeterminate sentencing reform shows a stubborn negative association with jail expansion. Probation shows no impact on either sector. Resource measures have some profound effects on rates of incarceration, but their effects are often the reverse of what was anticipated: prison growth seems to have slowed in the older core states as it accelerated on the frontier, and jails grew most in urban-industrial states. The next step is to explore potential interactions between sentencing reforms and resource effects.

**Interactive Resource and Reform Effects**

To isolate the interactions of specific variables, I estimated simplified models that contained only one reform term, one resource term, and one interaction term, in addition to the lag dependent variable and population. Given three reforms, six resource variables, and two sets of penal institutions, 36 possible interactions were investigated. For simplicity, I report only interactions that are significant (at $p \leq .05$) and that appear to be free of multicollinearity. Significant interaction effects on prison expansion are shown in Table 7, and on jail growth in Table 8.

Table 7 shows four significant interaction effects on prison expansion, involving three resource variables and all three reforms. I begin the discussion with model 1, which contains urbanization and the parole dummy variable. The first point of interest in the model is the main effects: parole once again shows a significant negative main effect, and the main effect of urbanization is now significantly negative. This tells only a part of the story, however. The main effect parameter for URB is an estimate of the relationship between urbanization and prison growth when the parole variable equals zero—that is, before the adoption of parole legislation in each state. The interaction parameter estimates the difference between the prereform resource effect and its effect after parole was adopted. This can be clarified by deriving two separate equations, one for all observations in which PAR equals zero, and the other for all observations in which PAR equals one. Following Kerlinger and Pedhazur (1973, pp. 251–55), the preparole equation for model 1 is found by dropping the parole and interaction terms:

$$\text{PRIS} = 28.68 + .61 \text{LPRIS} - 56.23 \text{URB} + 48.92 \text{POP} \quad (9)$$

These estimates show that, in the absence of parole legislation, urbanized states expanded their prison systems at a significantly lower rate than nonurban states.

To derive the equation for observations in which PAR equals one, the PAR parameter is added to the intercept and the interaction parameter is added to URB. This gives

$$\text{PRIS} = 10.14 + .61 \text{LPRIS} - .86 \text{URB} + 48.92 \text{POP} \quad (10)$$

Here the intercept drops sharply, and the parameter estimate for urbanization is not significantly different from zero (using the standard error associated with the interaction term in model 1, Table 7). Thus equation (10) shows that the enactment of parole laws slowed the rate of prison growth generally, and voided prior differences between rural and urban states. The only plausible interpretation of this latter effect is that parole had a particularly strong decelerative effect in rural states.

Model 2 shows a similar dynamic with regard to the interaction of parole and industrial wage labor. Here again, the main effect of parole is significantly negative. Wage labor shows a significant negative association with prison expansion in the absence of parole. After the adoption of parole, this association disappears (when PAR equals one, the WAGE parameter is 6.11, or effectively zero). In models 3 and 4, state revenue interacts with probation and indeterminate sentencing. The pattern of the interactions is by now familiar: both reforms had direct decelerative effects on imprisonment, and revenues were inversely related to prison expansion in the prereform period. The adoption of sentencing reforms erased differences between high- and low-revenue states.

Models 3 and 4 are difficult to interpret. For the first time, probation and indeterminate sentencing show significant effects on prisons; parole seems to drop away. The first question is whether these two interaction effects are, in fact, independent. The simplest way to test this is to insert both PROB and IND into a single equation, along with REV and their respective interactions. Equation (11) shows the results of such a test (standard errors appear in parentheses, and all significance levels are as noted in the tables):

10 Results shown here exclude four significant interactions, all involving the literacy variable, that showed troublesome collinearity diagnostics (Belsley et al. 1980). Condition indexes in these models were well over 20, and ranged as high as 35. In each case it appears that the distributions of literacy, the reform dummies, and their products are so closely intertwined that it is impossible to assess their independent effects. Thus we can draw no reliable conclusions about more complex effects of literacy on either prisons or jails.
Table 8. Pooled GLS Estimates of Significant Interaction Effects on Jail Expansion (Standard Errors in Parentheses, N = 192)

<table>
<thead>
<tr>
<th></th>
<th>JAIL =</th>
<th>LJAIL*** +</th>
<th>URB*** -</th>
<th>IND -</th>
<th>(URB × IND)* +</th>
<th>POP***</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>12.06*** + .69</td>
<td>37.74</td>
<td>1.37</td>
<td>37.40</td>
<td>6.95</td>
<td>1.62</td>
</tr>
<tr>
<td></td>
<td>(2.39)</td>
<td>(8.54)</td>
<td>(3.34)</td>
<td>(15.50)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>2</td>
<td>2.52</td>
<td>40.37</td>
<td>IMM** +</td>
<td>PAR -</td>
<td>6.71</td>
<td>1.50</td>
</tr>
<tr>
<td></td>
<td>(.247)</td>
<td>(12.43)</td>
<td>2.22</td>
<td>51.29</td>
<td></td>
<td></td>
</tr>
<tr>
<td>3</td>
<td>3.22</td>
<td>39.26</td>
<td>IMM*** +</td>
<td>IND -</td>
<td>7.14</td>
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<tr>
<td></td>
<td>(.222)</td>
<td>(11.07)</td>
<td>5.15</td>
<td>82.30</td>
<td></td>
<td></td>
</tr>
<tr>
<td>4</td>
<td>12.52*** + .66</td>
<td>137.04</td>
<td>WAGE*** +</td>
<td>IND -</td>
<td>5.42</td>
<td>1.52</td>
</tr>
<tr>
<td></td>
<td>(2.18)</td>
<td>(21.21)</td>
<td>5.03</td>
<td>201.38</td>
<td></td>
<td></td>
</tr>
<tr>
<td>5</td>
<td>6.36*</td>
<td>LJAIL*** +</td>
<td>REV +</td>
<td>PROB** -</td>
<td>8.96</td>
<td>1.60</td>
</tr>
<tr>
<td></td>
<td>(.29)</td>
<td>.00</td>
<td>29.00</td>
<td>2.45</td>
<td></td>
<td></td>
</tr>
<tr>
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<td></td>
<td></td>
<td>(8.96)</td>
<td>(.73)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>6</td>
<td>4.82</td>
<td>LJAIL*** +</td>
<td>REV +</td>
<td>PAR* -</td>
<td>7.92</td>
<td>1.70</td>
</tr>
<tr>
<td></td>
<td>(.29)</td>
<td>.43</td>
<td>9.21</td>
<td>1.37</td>
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</tr>
<tr>
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<td></td>
<td></td>
<td>(4.03)</td>
<td>(.39)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>7</td>
<td>4.92*</td>
<td>LJAIL*** +</td>
<td>REV +</td>
<td>IND -</td>
<td>8.79</td>
<td>1.65</td>
</tr>
<tr>
<td></td>
<td>(.24)</td>
<td>.43</td>
<td>3.51</td>
<td>1.22</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(4.32)</td>
<td>(.40)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>8</td>
<td>8.21**</td>
<td>LJAIL*** -</td>
<td>LPRIS -</td>
<td>PROB** +</td>
<td>7.62</td>
<td>1.54</td>
</tr>
<tr>
<td></td>
<td>(.27)</td>
<td>.00</td>
<td>37.29</td>
<td>.62</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(7.35)</td>
<td>(.12)</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

* p<.05.
** p<.01.
*** p<.001.
PRIS = 37.05*** + .80 LPRIS***
       (5.97)  (.07)
       − 2.62 REV***
       (.65)
       − 31.30 PROB*
       (15.90)
       + 2.85 (PROB × REV)*
       (1.38)
       − 19.49 IND*
       (8.93)
       + 1.68 (IND × REV)*
       (.82)
       + 46.44 POP***         (11)
       (3.75)

Coefficients here are almost exactly the same as those in models 3 and 4, Table 7; probation and indeterminate sentencing appear to have had separate, but similar, influences on the effect of state revenue. At this point, it is not clear why the revenue variable operates in this fashion. The only interpretation available is the simplest one: low-revenue states had high rates of prison growth only as long as they lacked any sentencing reforms. Poorer state governments discouraged prison growth either by restricting the rate of prison admissions (through the use of probation), or by increasing the release rate (through the use of indeterminate sentencing).

To interpret the results from Table 7, it will be useful to invoke the cohort hypothesis offered earlier. Recall that the wage-labor and state revenue variables reflect developmental differences between frontier and core states. The appearance of interactions involving these factors suggests that reforms had age-dependent effects. This argument will be stronger if it can be shown that urbanization is also implicated in variation across cohorts. As a test, I only add the TIME variable to model 1 in Table 7:

PRIS = 16.84* + .70 LPRIS***
       (7.07)  (.05)
       − 21.95 URB
       (20.35)
       − 15.60 PAR**
       (5.49)
       + 34.23 (URB × PAR)
       (26.11)
       − 7.86 TIME**
       (2.08)
       + 73.20 POP***         (12)
       (7.78)

The addition of TIME diminishes the effects of URB and the interaction term to near zero. It appears that urbanization, like wage-labor market size and state revenues, primarily reflects differences among states in their rate of incorporation into the national polity.

The estimates in Table 7 reveal a number of effects that were obscured in linear-additive models of prison expansion; yet, the results all point in roughly the same direction. By the turn of the century, prison growth had slowed in the older core states where urban populations, industrial labor markets, and official administrative capacities were best developed. The main sources of expansion were the booming new states to the west where, it appears, policymakers moved aggressively to construct penal systems that were on a par with those in more established sections of the nation. Sentencing reforms—especially parole, but also including probation and indeterminate sentencing—provided the means to decelerate prison growth across all states, but the effects of reform were particularly strong in new states. On the frontier, postreform growth rates tended to fall to a national norm.

Table 8 shows eight significant interactions related to jail expansion. These interactions involve four resource variables and, again, all three reforms. Model 1 suggests a pattern somewhat the inverse of that seen in the prison analyses: where indeterminate sentence legislation was not in effect, jails expanded faster in urban states. After the adoption of indeterminate sentencing policies, the urban-rural difference disappeared (where IND equals one, the URB parameter drops to .34). This pattern is repeated in the next three models. Models 2 and 3 reveal a previously unobserved, positive main effect of immigration on jail expansion. This effect, however, operates only in the absence of reform. It appears that among states with parole legislation (model 2) or indeterminate sentencing (model 3), immigration had no effect. In fact, however, the parole-immigration interaction is spurious. When a supplementary model (not shown) was estimated that contained both reforms and their interactions with immigration, the parole effect washed out and the indeterminate sentencing-immigration interaction remained significant. Model 4 shows that the effect of the wage-labor variable was positive only among states that lacked indeterminate sentence laws. After the adoption of such policies, states with large industrial labor markets expanded their jails at a slightly slower rate than average (or allowed their jails to decline more rapidly). These models boil down to one fairly simple conclusion: the adoption of indeterminate sentencing laws

11 This effect is probably not significant. Where IND equals one, the WAGE parameter becomes −63.34, a figure less than twice the standard error associated with the interaction term.
counteracted the accelerative influence of urbanization, immigration, and industrial labor-market expansion.

Models 5, 6, and 7 show that, as in the prison interactions, revenue effects were influenced by different reforms. Here all three reforms are implicated. Each enactment appears to have decelerated the growth of jails; the higher the revenue, the greater the deceleration. Again it is important to test whether these effects are independent. The first approach to this question was to enter all three reforms and their interactions with REV into one equation. Results showed that only the probation interaction is significant. Because indeterminate sentencing has consistently been associated with jail expansion to this point, another model was estimated that contained PROB, IND, and their interactions with REV:

\[
\text{JAIL} = 5.34^* + .76 \text{LJAIL}^{***} \\
\quad (2.57) \quad (0.06) \\
+ .44 \text{REV} \\
\quad (0.24) \\
+ 27.70 \text{PROB}^{**} \\
\quad (8.80) \\
- 2.25 (\text{PROB} \times \text{REV})^{**} \\
\quad (0.72) \\
+ 3.11 \text{IND} \\
\quad (4.40) \\
- 1.09 (\text{IND} \times \text{REV})^{**} \\
\quad (0.42) \\
+ 8.37 \text{POP}^{***} \quad (13)
\]

Both interactions are significant, suggesting again that parole had no effect on jailing independent of indeterminate sentencing. The revenue effect again appears to be influenced by the enactment of more than one sentencing reform: in the absence of any reform, jails in high-revenue states grew slightly (but insignificantly) faster than average. State adoption of either reform turned growth rates sharply downward. One anomaly remains in this model: the main effect of probation is significant and, contrary to expectation, positive. I will return to this below.

Model 8 presents what might be the most interesting result of this analysis. This model shows a significant interaction between probation and prison populations. Probation here shows a significantly negative main effect on jail expansion, as was hypothesized early on. More important, the interaction term shows that, after the enactment of probation legislation, the number of prison inmates exerted an upward pressure on jail populations (when PROB equals one, the prison coefficient becomes .62, and the parameter is significant). A scenario to account for this finding is that by lowering jail growth across the board, probation introduced slack into the system that permitted local officials to maintain some proportion of inmates in jails when prison populations grew too large.

The true direct effect of probation on jails remains mysterious. In equation (13) probation appears to encourage jail expansion when the interaction with revenue is controlled. Model 8 in Table 8 suggests a decelerative effect when prison growth is controlled. The test of these contradictory findings is to estimate a single model containing both interactions. Results (not shown here) are inconclusive: the probation coefficient in the combined model is negative but insignificant, and collinearity diagnostics suggest there may be some degradation of the estimates. One possible interpretation is that probation had different direct effects in different states. In some it may have provided a safety-valve to reduce the flow of inmates into jails; in others it may have been a means to widen the social control net and place more offenders at risk of incarceration. Results, in short, point to higher-order interactions that cannot be confidently explored using the present data.

DISCUSSION

This study was motivated by a theoretical critique of dominant systems and agency models of imprisonment. This critique states that imprisonment rates are not directly determined either by social-systemic imperatives or entrepreneurial reforms. Rather, they follow from the ways in which official actors use their discretionary authority to control the flow of inmates through institutions. The analyses addressed these arguments with three causal scenarios: (1) reform independently produces sharp discontinuities in punishment rates; (2) reforms intervene only to adjust sanctioning rates to long-term systemic processes; and (3) the effects of resources and reforms are mutually contingent.

The empirical results support this critique. Linear models of reform and resource effects account poorly for changes in imprisonment rates; they yielded ambiguous results and frequently obscured important relationships. More complicated interactive models revealed that reforms worked differently on prisons and jails and across subsets of states. Moreover, these results are largely compatible with the strategic-discretion argument. Prison growth was decelerated mainly by parole reform, which routinized the discretionary release of inmates. This decelerative effect was strongest in states where prisons were expanding the fastest. Jail capacities were influenced by probation legislation, which increased discretion over rates of admission, and by indeterminate sentencing, which probably encouraged faster releases. The effect of indeterminate sentencing was also strongest where jails were most expansive. It
appears that reforms were used to contain explosive growth and relieve overcrowding. Substantively, these findings suggest two important and largely unanticipated conclusions: prisons and jails showed nearly opposite patterns of expansion, and these patterns reveal an overriding cohort effect. Prisons grew fastest in frontier states. Despite their lack of developed resources, frontier states enjoyed significant growth advantages because of their late incorporation into the national system. They avoided the painful and expensive process of evolving an original penal strategy by simply borrowing highly rationalized ideological, administrative, and even architectural models from older states. I believe reforms had especially powerful decelerative effects in new states because they were adopted at a relatively early stage of the institution-building process. Newer prison systems were probably more reformist from the outset—that is, frontier states enacted reforms as ideologies rather than as expedients, and inherited with them the therapeutic discourse and administrative machinery that signified an advanced approach to penalty. The pattern of jail growth was the reverse: jails endured best in the core, where prison growth was most tightly constrained, and grew least in the frontier. Where probation laws were in force, large prison populations directly increased the flow of inmates into jails. Finally, jail expansion was also encouraged by the presence of large immigrant populations, a factor that did not affect prisons at all.

The findings have two larger implications for the study of social control. First, they amplify our understanding of the history of imprisonment in the U.S. As McKelvey (1936) has written, states that had been among the pioneers of prison reform in the 1800s lost their political will by the end of the century. Legislators responded to the antagonism of unions, which feared wage competition from prison labor, and the public, which had doubted that the penitentiary was rehabilitative. The consequences in the older northeastern states were a squeezing-off of prison appropriations, a trend toward smaller, less expensive penitentiaries, and more flexible surveillance strategies such as parole. My results are compatible with this account, but they also suggest the need to treat jails as an integral component of the penal system, not as a residual set of institutions. In particular, the findings of this study imply that local jails supplemented the limited capacity of centralized prison systems, especially in urban areas. While this interpretation is largely inferential, it underscores the need to explicate the role of jails using more detailed historical sources, focusing on the ways local officials responded to the enactment of probation and indeterminate sentencing laws.

Second, the findings contain a number of theoretical implications. The analysis attempted to extend the longitudinal research of Berk et al. (1981, 1983) by drawing comparisons across states and between prisons and jails. As I have already suggested, results support their argument that prisons (and jails) are largely “self-regulating” systems of punishment. The apparent complementarity between the two sectors suggests further that self-regulation operates not only through the formal linkages that define the structure of state prison systems, but also through informal links between local and state actors. Finally, the discovery of cohort effects on imprisonment rates expands our theoretical perspective in yet another way. The different patterns of expansion in frontier and core states show that imprisonment rates are influenced not only by management strategies enacted by social control officials, but by larger strategies of state-building as well.

12 Such convergent growth of prison systems is an example of what DiMaggio and Powell (1983) refer to as “mimetic isomorphism.” Huntington (1973) has written more specifically about the tendency of U.S. states to reproduce each others’ institutional structures.

Appendix A. Mean Absolute Values of Dependent and Independent Variables, for States and Territories, by Decade (Percent Change in Parentheses, N = 48)

<table>
<thead>
<tr>
<th>Variable</th>
<th>1880</th>
<th>1890</th>
<th>1900</th>
<th>1910</th>
<th>1920</th>
<th>Mean change</th>
</tr>
</thead>
<tbody>
<tr>
<td>PRIS</td>
<td>638.64</td>
<td>386.06</td>
<td>1.64</td>
<td>6.34</td>
<td>1.38</td>
<td>+25%</td>
</tr>
<tr>
<td>JAIL</td>
<td>927.96</td>
<td>463.21</td>
<td>2.69</td>
<td>8.52</td>
<td>1.92</td>
<td>15.69</td>
</tr>
<tr>
<td>URB</td>
<td>+45%</td>
<td>(+20%)</td>
<td>(+64%)</td>
<td>(+34%)</td>
<td>(+39%)</td>
<td>(+54%)</td>
</tr>
<tr>
<td>LIT</td>
<td>+19%</td>
<td>(+4%)</td>
<td>(+50%)</td>
<td>(+26%)</td>
<td>(+12%)</td>
<td>(+25%)</td>
</tr>
<tr>
<td>IMM</td>
<td>1104.96</td>
<td>482.29</td>
<td>4.04</td>
<td>10.74</td>
<td>2.15</td>
<td>+22%</td>
</tr>
<tr>
<td>WAGE</td>
<td>132.50</td>
<td>132.50</td>
<td>1.10</td>
<td>1.10</td>
<td>1.10</td>
<td>+15%</td>
</tr>
<tr>
<td>REV</td>
<td>87.64</td>
<td>87.64</td>
<td>13.00</td>
<td>13.00</td>
<td>13.00</td>
<td>+13%</td>
</tr>
<tr>
<td>POP</td>
<td>59.90</td>
<td>59.90</td>
<td>13.00</td>
<td>13.00</td>
<td>13.00</td>
<td>+13%</td>
</tr>
</tbody>
</table>

Note: Figures for prison and jail inmates are in units of 1; all others in units of 100,000.
Appendix B. Mean Per Capita Values of Dependent and Independent Variables, for States and Territories, by Decade (Percent Change in Parentheses, N = 48)

<table>
<thead>
<tr>
<th>Year</th>
<th>PRIS</th>
<th>JAIL</th>
<th>URB</th>
<th>LIT</th>
<th>IMM</th>
<th>WAGE</th>
<th>REV</th>
</tr>
</thead>
<tbody>
<tr>
<td>1880</td>
<td>58.20</td>
<td>36.29</td>
<td>.102</td>
<td>.590</td>
<td>.145</td>
<td>.042</td>
<td>5.06</td>
</tr>
<tr>
<td>1890</td>
<td>80.48</td>
<td>34.11</td>
<td>.148</td>
<td>.650</td>
<td>.162</td>
<td>.055</td>
<td>7.22</td>
</tr>
<tr>
<td></td>
<td>(+ 38%)</td>
<td>(- 6%)</td>
<td>(+ 45%)</td>
<td>(+ 10%)</td>
<td>(+ 12%)</td>
<td>(+ 31%)</td>
<td>(+ 48%)</td>
</tr>
<tr>
<td>1900</td>
<td>81.74</td>
<td>29.30</td>
<td>.177</td>
<td>.684</td>
<td>.140</td>
<td>.060</td>
<td>9.55</td>
</tr>
<tr>
<td></td>
<td>(+ 1%)</td>
<td>(- 14%)</td>
<td>(+ 20%)</td>
<td>(+ 5%)</td>
<td>(- 14%)</td>
<td>(- 9%)</td>
<td>(+ 32%)</td>
</tr>
<tr>
<td>1910</td>
<td>80.86</td>
<td>36.03</td>
<td>.217</td>
<td>.713</td>
<td>.140</td>
<td>.062</td>
<td>12.94</td>
</tr>
<tr>
<td></td>
<td>(- 1%)</td>
<td>(+ 23%)</td>
<td>(+ 22%)</td>
<td>(+ 4%)</td>
<td>(+ -0%)</td>
<td>(+ 3%)</td>
<td>(+ 35%)</td>
</tr>
<tr>
<td>1920</td>
<td>76.98</td>
<td>21.60</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(- 5%)</td>
<td>(- 40%)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Mean change</td>
<td>+8%</td>
<td>-9%</td>
<td>+29%</td>
<td>+6%</td>
<td>-1%</td>
<td>+14%</td>
<td>+37%</td>
</tr>
</tbody>
</table>

Appendix C. Variable Labels and Definitions

Variable | Definition
--- | ---
PRIS | Sentenced prison inmates
LPRI | Sentenced prison inmates (lag)
JAIL | Sentenced jail inmates
LJAIL | Sentenced jail inmates (lag)
PROB | Adoption of probation (lag dummy)
PAR | Adoption of parole (lag dummy)
IND | Adoption of indeterminate sentencing (lag dummy)
URB | Persons in cities over 25,000 population (lag)
LIT | Literate persons over age 10 (lag)
IMM | Foreign-born residents (lag)
WAGE | Wage workers in manufacturing industries (lag)
REV | State government revenue in constant dollars (lag)
POP | Total population
TIME | Years since statehood at each observation

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Politics of the American Probation Movement."  
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