Imprisonment and Social Classification in Five Common-Law Democracies, 1955–1985[1]

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Rates of imprisonment have risen in many Western democracies over the past few decades, most dramatically in the United States. The development of a systematic and general explanation of imprisonment trends has been impeded because of the theoretical and methodological limitations of prior quantitative studies. Most use data from a single country or jurisdiction and focus narrowly on the Rusche-Kirchheimer "labor surplus" hypothesis, with little attention to alternative explanations. This study takes a cross-national perspective, using longitudinal data from Australia, Canada, New Zealand, the United Kingdom, and the United States to offer an institutional account of imprisonment rates. The labor surplus effect is treated as a special case of a more general process by which individuals are allocated among alternative life-course paths. This allocation process is likely to be influenced by macrolevel institutional arrangements and contests for political power. Hypotheses are tested using pooled time-series cross-section regression techniques. Results support this broadened perspective: prison growth is driven not only by crime rates and unemployment rates, but also by welfare spending and the power of right parties.

INTRODUCTION

In the post–World War II period, dramatic changes have occurred in the penal regimes of many Western democracies. Most conspicuously, im-

prisonment rates in the United States declined gradually until the mid-1970s, then rose fourfold over the next two decades. Imprisonment rates have multiplied in the United Kingdom as well, but at a more consistent pace. The trend in most other Western democracies has been toward moderate expansion, and in only a few countries have imprisonment rates remained level or declined. We have a wealth of explanations of the U.S. case (e.g., Beckett 1997; Blumstein 1988; Caplow and Simon 1999; Mauer 1994; Tonry 1999), which focus on both the supply and demand sides of the punishment equation. On the supply side, the maturation of the baby boom generation contributed to a rise in the incidence of some kinds of crime. On the demand side, the structural vulnerability of the American polity to moral panics, the opportunistic use of crime and welfare as "wedge" issues by conservative politicians, and the increasing magnification of crime by the news media have led to the enactment of stricter sentencing policies (especially with regard to drug offenses), tighter restrictions on parole release, and a sharp increase in punishment capacity. Many of these factors are not unique to the United States, however. For example, Canada, New Zealand, and the Netherlands experienced larger baby booms than did the United States, and the United Kingdom's was nearly as large. Crime, along with welfare, race, and immigration, has been politicized elsewhere as well—especially in the United Kingdom (Hale 1989) and even in New Zealand (Pratt 1988)—but with much less apparent effect on inmate populations.

Far from being incomplete, our understanding of punishment trends in the United States is overdetermined: we have too many explanations, some undoubtedly spurious, and too few degrees of freedom to distinguish among them. The appropriate move in this situation is to approach the issue of punishment from a comparative perspective—not to focus on the United States or any other particular country in isolation, but to begin the search for a general explanation of punishment trends in the modern West. This raises a different kind of problem. While scholarly interest in this issue has increased recently and theoretical arguments have moved in interesting new directions, the existing empirical work focuses on a narrow range of explanations. The dominant neo-Marxist tradition, based on the work of Rusche and Kirchheimer (1968) in the 1930s, has pursued the argument that prisons in capitalist societies are mechanisms for controlling surplus labor. An impressive number of studies, beginning with Greenberg (1977) and Jankovic (1977), have explored the derivative hypothesis that imprisonment rates rise and fall with the downward and upward movement of the business cycle. Most studies support the Rusche-Kirchheimer hypothesis (Chiricos and DeLone 1992), but for both empirical and theoretical reasons, it is premature to accept it as convincing. The empirical weakness of this tradition is that most studies use time-

series data from a single country or jurisdiction. Indeed most use data from the United States or the United Kingdom, where imprisonment rates have grown most dramatically, raising questions about the generalizability of their findings.[2] Time-series models tend to be narrowly focused because multicollinearity makes it difficult to test a wide range of competing hypotheses in a single model. Thus the suspicion remains that the business cycle–imprisonment association is spurious, or at least overestimated. The theoretical weakness of the Rusche-Kirchheimer hypothesis is that, even if it is valid and generalizable, no one knows how it works or what it means—its theoretical significance remains, as Melossi (1993, p. 259) says, a riddle.

Interpretations of the Rusche-Kirchheimer hypothesis have mirrored more general trends in neo-Marxist scholarship: while some scholars continue to hew to an instrumentalist interpretation, others, influenced in varying degrees by Gramsci (1971) and Foucault (1979), argue that the causal link is ideological hegemony—what Melossi (1985) terms a "discursive chain" that coordinates ideas and practices in the prison, the economy, and the polity. In this interpretation, the prison becomes a cultural artifact, a symbolic arena that gives form and meaning to class struggle. Economic interest retains its causal priority, but its influence is filtered and attenuated. Other scholars, looking more to Durkheim and Weber than to Rusche and Kirchheimer, have pushed more aggressively toward accounts of punishment practices that give primary emphasis to culture and politics (Garland 1992; Jacobs and Helms 1996; Savelsberg 1994; Sutton 1987). Studies of this sort emphasize widely varying causal processes, and they do not as yet add up to a coherent theoretical program. But taken together, they suggest an institutionalist account in which punishment plays a reflexive role in the structuration of the moral order: on the one hand, the prison offers a potent symbol of dominant conceptions of deviance and morality, and on the other, it provides a site for the ongoing negotiation of normative boundaries and political authority.

This article moves forward in two steps. First, I outline an institutionalist argument that frames punishment within a more general set of classificatory discourses and practices used in modern societies to manage socially marginal populations. This argument is institutional in the sense that it emphasizes the politics of moral order rather than economic instrumentality as the driving force behind prison expansion. As I will show, the Rusche-Kirchheimer hypothesis can be subsumed under this argument

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[2] In fact, Greenberg's (1977) seminal paper is based on Canadian data. Other well-known confirmatory studies use data from France (Godefroy and Laffargue 1991) and Italy (Melossi 1985), and one disconfirmatory study uses data from Poland (Greenberg 1980).

as a special case. Second, I test the hypotheses generated from this argument using data from five Common Law democracies—Australia, Canada, New Zealand, the United Kingdom, and the United States—over the period 1955–85. My intent in using this sample is to move beyond the limitations of single time-series designs, but in a judicious way. Pooling data from several countries captures more variation, permits tests of a broader range of hypotheses, and encourages more robust generalizations. As the cross-national sample size increases, however, data comparability may become problematic and results may be difficult to interpret—particularly in areas like imprisonment, where there are so few precedents in the literature.[3] As a first approach to the problem, it seems appropriate to restrict the sample to this "reference group" of countries that, while showing substantial variation in political systems, economic trajectories, and penal policies, nonetheless shares a common language and legal history and counts phenomena of interest in consistent ways.

TOWARD AN INSTITUTIONAL ACCOUNT OF IMPRISONMENT

Theoretical Background

The brief for an institutionalist account of imprisonment has been argued most compellingly by Garland (1992), who draws on the work of Elias (1978, 1982) to suggest that penal practice reflects a diffuse set of sentiments and dispositions arising from long-term civilizing processes in Western societies. In substantive terms, an Eliasian perspective suggests that the modern prison arose as the historical analogue to a more diffuse and pervasive set of changes in social life, including the pacification of interpersonal conflict, the development of genteel manners, and the privatization of sexuality. To Elias's argument that civilization is built on repression, Garland adds that the prison is the most conspicuous expression of that repressive impulse. This essentially interpretive argument offers a persuasive account of the origins of Western penality, but it offers little leverage for theorizing about variation in punishment trends among modern Western societies. The subjective sentiments and aggregated *mentalités* that animate Eliasian theory make for interesting concepts but elusive

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[3] The few cross-national empirical studies I have been able to find include Biles (1983; England-Wales and Australia), Farrington, Langan, and Wikström (1994; United States, England, and Sweden), Kommer (1994; eight European countries), Lynch (1987, 1988; England, Canada, West Germany, and the United States), Lowman and Menzies (1987; Canada and the United States), Savelsberg (1994; United States and Germany), Waller and Chan (1974; 15 countries), Wolpin (1980; California, England, and Japan), and Young and Brown (1993; 15 countries). These are for the most part descriptive analyses—few offer causal arguments or conduct multivariate tests (Savelsberg and Wolpin are valuable exceptions).

variables. Moreover, they do not begin to encompass the kinds of institutional dynamics that are of interest to many contemporary cultural sociologists. The institutional account of modern penality that Garland calls for requires a more structural conception of culture as a set of practices and exteriorized symbolic objects—including public discourses, rituals, organizational recipes, and material artifacts—that are produced in patterned ways that vary observably over time and space.

The account developed here begins by juxtaposing Durkheim's early argument that crime and punishment are indispensable components of moral order (1982, chap. 3) with his later work on totemic symbolism and classification systems (1965; Durkheim and Mauss 1963) and by reading both through contemporary theories of culture (Bourdieu 1977, 1984; Griswold 1994; Wuthnow 1987). The starting point is the idea that the structure of the moral order arises from a primarily cognitive process of classification. Institutions play a crucial role in this process by providing schemas that individuals use to sort events, people, and things into apparently natural categories (Douglas 1986). Sorting creates meaning and value by enacting taken-for-granted notions of the proper relations among objects in the world. More to the point of this study, institutions also provide schemas for sorting people into various developmental pathways, tracking their progress, and certifying their achievement of appropriate institutionalized identities. The moral order acquires its shape both from the juxtaposition of life courses and status positions—forming what Bourdieu (1984) calls "distinctions"—and from the allocation of persons among those positions.

As Durkheim recognized, the distinction between criminals and noncriminals is the most fundamental classificatory distinction in modern societies. The moral resonance of classification is in this case obvious: crime is constituted as a social fact through public rituals of punishment and exclusion; as such it is not a social pathology, but rather a normal means by which healthy societies reinforce their normative boundaries (Durkheim 1982, chap. 3). We can push a bit farther by recognizing that criminality is only one among several conspicuous criteria of moral distinction. Modern societies support a range of institutions, including mental health systems, welfare agencies, unemployment bureaus, homeless shelters, and substance abuse programs, that constitute and manage more or less specific forms of social marginality. Analysis of the prison should, at the very least, take into account the fact that crime is only one of many possible ways of being deviant. I would argue further that if we take seriously the idea that classification is a generic element of social organization, then what are commonly understood as "social control" agencies turn out to be a subset of a much broader array of institutions that manage individuals' movement through the life course—including families,

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schools, hospitals, the military, labor markets, and so on. Societies have a finite capacity to produce various kinds of identities, and the production of particular reputable or disreputable selves is dependent on the shape of the institutional terrain (Meyer 1988). The opportunity to become a "professional" depends on the availability of higher education, just as the opportunity to become "insane" depends on the availability of psychiatric expertise. It is consistent with Durkheim to argue that the production of crime is dependent on the capacity to punish.

What determines that capacity? To Durkheim and his best-known interpreters (Douglas 1970; Erikson 1966; Foucault 1979), punishment is a functional response to a systemic need for moral integration. This is a well-known theoretical dead end: since moral integration is unobservable, we can only recognize it through shifts in the style and volume of punishment; the argument, thus, is circular. A better approach would be to suggest that punishment practices, and the institutional forces they express, are outcomes of historically contingent political projects. Organizations, professions, and social movements form institutional coalitions—what Bourdieu (1990) terms social "fields"—that compete for jurisdiction over the life course, and particularly over the terrain of social problems. This is fundamentally a discursive process in which competitors seek to expand their domains by adapting their own developmental taxonomies to new forms of social trouble. Whether a particular case of trouble is classified under the criminal code, the public welfare manual, or the Diagnostic and Statistical Manual of the American Psychiatric Association (DSM-IV) is determined not only by the facts of the case, but also by the relative scope and reach of the associated fields. A conspicuous example of cross-societal variation in this regard is hard drug use, which in the United States is classified as a criminal violation to be dealt with (increasingly so) by courts and prisons and in Britain is classified as a medical problem to be treated under the auspices of the National Health Service (Nadelmann 1993). As this example illustrates, patterns of institutional dominance influence the allocation of individuals among various official agencies and in and out of reputable and disreputable roles. In doing so, they give publicly recognizable shape to the moral order. Following Wuthnow (1987), moral order is in this sense a practical accomplishment, not the aggregation of individual subjectivities.

Hypotheses: Main Effects

I have argued so far that (*a*) the prison should be viewed as one terminus in the complex classificatory system that makes up the moral order of society and (*b*) classificatory outcomes—including the rise and fall of imprisonment rates—are influenced by competition for institutional

dominance. The most straightforward implication of this argument is that, given finite human and fiscal resources, the expansion of prisons is inversely related to the expansion of other institutionalized means for managing the life course. I explore this empirically at three levels.

Life-course patterns.—Competitive relations should be most obviously apparent in the way bodies flow along alternative life-course paths. This might not seem at first like an empirically useful approach to imprisonment, since the societies in this study have large populations and the paths available to individuals are numerous and complex. This problem is made more tractable by the fact that both personal and property crimes are committed disproportionately by young males (Cohen and Land 1987; Gartner and Parker 1990; Hirschi and Gottfredson 1983; Pampel and Gartner 1995), and young males therefore make up a preponderant share of prison inmates (Berk et al. 1983). We can narrow our focus considerably by assuming that young men in modern societies face a limited set of broad life-course alternatives: if they are not in prison, they are likely to be in school, at work, or in the military. Of course this set of alternatives is neither mutually exclusive nor exhaustive. A young man can, for example, work and attend school at the same time. But we can realistically expect that this would reduce his risk of crime and incarceration more than either work or school by itself, thus preserving the logic of the argument. The residual category mainly comprises young men whose risk of imprisonment is on average higher: in this group, the proportion who are on probation or parole, employed in the illicit economy, or simply on the street is probably large, and the proportion who are economically and socially independent is probably inconsequentially small.

There is a great deal of evidence—from Hirschi (1969) to Sampson and Laub (1993)—to show that, for any given individual, involvement with legitimate institutions lowers the probability of criminal activity, and probably also incarceration. The implication at the aggregate level is that as opportunities for legitimate attachments expand, rates of crime and incarceration will tend to decline. It is important to stress that this argument requires no restricting assumptions about the values, subjective motivations, or exogenous interests of individuals. More specifically, it is not necessary to assume that individuals join the military, enroll in school, or take jobs because they have internalized more conformist values or because they are by nature more attached to legitimate institutions. This may be the case, but it is equally likely that conformist values and attachment arise as *by-products* of involvement, operating as a set of retrospective accounts and predispositions that encourage

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movement along a particular path.[4] As such, emergent values may add to the path dependence of the classification process, but they do not change its basic structure. The causal logic underlying these hypotheses is stochastic rather than deterministic: the institutional orders of work, education, and the military can plausibly be taken as the major defining features of the opportunity space through which young men move, and it is reasonable to suspect that the structure of that space influences the production of criminality.

The literature suggests two ways in which this effect may operate, neither of which invokes exogenous subjective motivations. At the routine activities level (Cohen and Felson 1979), people who work, go to school, or serve in the military have less unstructured time than do unattached individuals and also have fewer occasions to interact with unattached others; as a result, their opportunities for criminal involvement are reduced. It may be at least as important that individuals with stable institutional attachments are less visible to police, net of their propensity to crime. At the organizational level, there is abundant evidence that social control agents use strategic classificatory logics that are also likely to produce trade-offs. The practical task for such officials is to handle ambiguous cases of trouble efficiently and in ways that can be accounted for in terms of official rules and local organizational cultures. Standard taxonomies such as criminal codes, psychiatric nosologies, and welfare eligibility criteria offer detailed classificatory schemas but leave substantial amounts of discretion about how abstract rules apply to a concrete case. Given chronic shortages of information and resources, officials typically suffice by attempting to match the ascriptive characteristics of particular troublesome persons with the stereotypical attributes of different classes of offenders. If the match is successful, the individual and his circumstances become a "type of case" for which a classificatory outcome follows "naturally." The studies in this genre suggest in general that persons who lack legitimate institutional attachments are more likely than others to be classified as criminals and more likely to receive harsh sentences, net of the behavior that brought them to official attention in the first place.[5]

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[4] They are, thus, elements of a habitus (Bourdieu 1990) characteristic of a specific stage in a specific life course.

[5] For classic examples in criminal and juvenile justice, see Bittner (1967), Emerson (1969), Feeley (1979), Sudnow (1964), and Skolnick (1966, chap. 4). Examples involving other forms of "dirty work" include Scheff and Culver (1964) and Emerson and Pollner (1976) on mental hospitals, Jeffery (1979) on hospital emergency rooms, and McBarnet (1991) on tax law enforcement. Heimer and Staffen's (1995) study of neonatal intensive care units offers an interesting counterexample to the generalization that individuals with weaker institutional attachments are more easily stigmatized: NICU staff expect low-status mothers to be less cooperative and less attentive to their children, so staff are more solicitous and less likely to diagnose child abuse in these cases.

The allocation argument yields five hypotheses.[6] First, the rate of imprisonment is likely to be influenced by the "supply-side" effect of the age and gender distribution of the population:

HYPOTHESIS 1.—*The greater the proportion of young males in the population, the higher the rate of prison growth.*

"Young" in this case means ages 15–24, a window that captures peak rates of crime, arrest, and first convictions in Britain and the United States (Hirschi and Gottfredson 1983).

Second, it is also useful to control for the more direct effect of the volume of crime on prison growth. For most crimes, comparable data are not available for all of the countries and years in the sample. Homicide rates, however, are measured with a fair degree of accuracy across modern societies and over time (Monkkonen 1989). Homicide is an attractive candidate for inclusion here for two reasons: (*a*) because homicides are so conspicuous, they are likely to have an exaggerated influence on public perceptions of crime and on crime-control policies, and (*b*) such a measure might help account for the U.S. case—U.S. homicide rates, like imprisonment rates, are two to ten times higher than those in the other countries throughout the observation period. The effect here should be positive:

HYPOTHESIS 2.—*The higher the homicide rate, the higher the rate of prison growth.*

Three other hypotheses identify anticipated trade-offs involving the military, schools, and the labor market. The military recruits disproportionately from the population of young males, and enlistment effectively removes them from the risk of civilian punishment. Evidence from the United States suggests that the military provides a particularly attractive life-course option during economic downturns (Griffin, Wallace, and Devine 1982), especially for the most disadvantaged young men (Mare and Winship 1984). Prior studies using U.S. data by Berk et al. (1981) and Cappel and Sykes (1991) found significant negative associations between military growth and imprisonment, while another by Inverarity and Grattet (1989) yielded mixed results—no effect of enlistments, but a negative effect of battle deaths. I expect a negative effect of enlistments on prison growth:

HYPOTHESIS 3.—*Expansion in military enlistments reduces the rate of prison growth.*

Schooling does not categorically immunize young men from civilian punishment, but it occupies at least some of their energy and provides them with a legitimate identity. To match the expected supply-side effect of the male cohort ages 15–24, the empirical focus here will be on the expansion of enrollments in secondary and tertiary institutions. Even in

[6] Specific indicators and their sources are described in appendix table A1.

this sample of advanced industrial societies there is a fair amount of variation in secondary enrollments, mainly in the 1950s and early 1960s. Variation at the tertiary level persisted much longer, much of it coming from differential investments in advanced vocational training, not just restrictions on general university education (Ryan 1991). The resulting hypothesis therefore captures not just the contemporaneous "routine activities" effects of schooling, but also class-based effects over the life course: enrollments at these levels signify the breadth of the institutionalized pathways that connect adolescent males with the legitimate labor market, and more specifically, they signify the capacity of those paths to accommodate nonelites as well as those traditionally destined for higher degrees.

HYPOTHESIS 4.—*Expansion in young men's secondary and tertiary school enrollments reduces the rate of prison growth.*

The final life-course hypothesis focuses on the capacity of the labor market itself:

HYPOTHESIS 5.—*Growth in unemployment rates increases the rate of prison growth.*

This is the Rusche-Kirchheimer hypothesis, which now appears as a special case of a general allocative process. In that context, unemployment is not a proxy for criminal motivation or the felt class interest of criminal justice decision makers. Rather it serves more directly as a measure of slack capacity in the labor market—and particularly in the market for wage labor, which is the most viable port for young men who would otherwise run the greatest risk of imprisonment.

Patterns of policy trade-offs.—Emerson (1983) has offered two arguments suggesting that allocation patterns are structured by higher-level organizational and policy constraints. First, he observes that official gatekeepers monitor their decisions reflexively and relationally: what constitutes an appropriate or inappropriate disposition in a given situation depends on the typical pattern of dispositions in a particular agency and on the official's assessment of where her record fits in that pattern. Organizations thus develop classificatory histories that are used to make sense of specific cases and justify specific outcomes. Second, and more important, these classificatory histories are influenced by resource constraints—specifically, the availability of suitable placement options. For example, police might in principle classify a fractious homeless man as drunk, insane, criminally dangerous, or simply in need of a place to sleep. Each of these classifications implies a different organizational destination, but some destinations may have limited capacities. Local-level capacities are influenced in turn by societal "deployment patterns" (Erikson 1966)—patterns of differential attention to various social problems signified by decisions to build prisons, close down mental hospitals, outlaw

or decriminalize various kinds of drug use, revise welfare eligibility rules, and so on. This can be restated in more general theoretical terms: Jurisdictional competition among social fields shapes the flow of symbolic and material resources to official agencies and occupational groups, thus determining the breadth and accessibility of alternative life-course paths. The most obvious and measurable expression of institutional competition is government spending in sectors that might be expected to offset prison expansion. I focus on two sectors in particular.

First, there is strong historical evidence to suggest that a trade-off between prison and social welfare is endemic to Anglo-American societies. In the late Victorian period, for example, the British government offered more generous social benefits and moderated the severity of the penal system as means to ease the inclusion of working-class voters into the polity (Garland 1985). At about the same time, the United States made trade-offs of a different sort. Almshouses, which had for much of the 19th century served as all-purpose repositories for the poor, petty criminals and vagrants, the aged and senile, unwed mothers, and homeless children, were gradually closed down. Political opposition precluded a compensating rise in "outdoor relief" payments; instead, the decline of almshouses contributed directly to waves of expansion among juvenile reformatories, prisons, jails, and mental hospitals (Sutton 1987, 1990, 1991). There have been few attempts to explore linkages between imprisonment and welfare using contemporary data. The most important of these is a study by Inverarity and Grattet (1989), who tested the effects of Aid to Families with Dependent Children (AFDC) caseloads and mental hospital populations on state prison admissions in the United States. They found no effect of either variable, but the acknowledged limitations of their data (pp. 365–66) suggest that further analysis using a broader sample is warranted.

Modern welfare regimes comprise several different kinds of benefit programs targeted at different groups of citizens. Pensions, for example, are the largest single category of welfare expenditures in most industrialized democracies, but pensions are unlikely to influence imprisonment because they are distributed mainly to the aged—a group that commits few crimes and makes up a minuscule proportion of prison populations. Instead, I focus on programs that are targeted at two overlapping groups: wage workers and families with children. Unemployment compensation and work injury benefits provide the primary safety net for working families in the legitimate labor market during times of economic hardship. These programs are relevant because they are likely to mediate the effects of business cycles on crime and imprisonment rates. In addition, Pampel and Gartner (1995) show that more generous spending on benefits targeted at families with children reduces the impact of young male cohorts on

homicide rates. This association holds, they argue, because these benefits provide mechanisms to support the transition from youth to adulthood. It is reasonable to suspect that they would influence the imprisonment rate as well. I include two such programs here: public assistance provides benefits on a means-tested basis for the nonworking poor, and family allowances are benefits payable to families with children, regardless of family income or employment status. Taken together, these four programs provide a rough measure of the level of collective protection available to working and nonworking families. The measure used here is the sum of expenditures on these programs as a percentage of the gross domestic product.[7]

HYPOTHESIS 6.—*Growth in welfare spending aimed at workers and families with children decreases rates of prison expansion.*

The second field that deserves attention is education. Enrollments are likely to be constrained by investments in education, particularly at the tertiary level where schooling is more discretionary, so it is useful to include a measure of spending as a control variable. But the educational field carries theoretical freight of its own. Public education has been a central part of the nation-building strategies of the United States (Meyer et al. 1979) and, in somewhat different ways, of other modern nations as well (Ramirez and Boli-Bennett 1981; Soysal and Strang 1989). A common justification for these strategies has been that, beyond its contribution to human capital, schooling produces competent citizens. Thus education expenditures provide a measure of public commitment to legitimate life-course development, and this effect may be independent of the allocative effects of enrollments. Education spending is also related theoretically and empirically to other variables of interest in the model: in some countries, especially the United States, education operates as a functional equivalent to welfare entitlements, and cross-national evidence indicates that education spending is influenced by party politics (Castles 1989; Heidenheimer 1981). The general expectation is that expansive investments in the field of education tend to narrow the developmental path toward criminality:

[7] There are other types of benefits that might have similar effects but must be excluded here for practical reasons. Support for low-income housing is an obvious candidate, but these programs come in so many different forms—capital outlays for public housing, subsidies for private-sector redevelopment, and subsidies to needy individuals—that it has proven impossible to gather consistent data. Another is the portion of disability and health care spending that goes toward the treatment and maintenance of drug and alcohol abusers and the mentally ill. For lack of any other support, members of these groups often wind up in jails and prisons in the United States, and perhaps in other countries as well (Adler 1986; Belcher 1988; Biles and Mulligan 1973; Chafetz, Goldman, and Taube 1983); but published sources do not allow such expenditures to be disaggregated from the much larger categories of pension and medical benefits.

HYPOTHESIS 7.—*Growth in public spending for education decreases rates of prison expansion.*

Political domination.—It will also be important examine the political processes that are antecedent to policy commitments. Evidence from the United States suggests that political factors shaped the expansion of prisons, jails, asylums, and reformatories around the turn of the century (Sutton 1987, 1990, 1991) and that partisan dominance influences spending on criminal justice, the adoption of punitive policies, and ultimately prison admissions (Caldeira and Cowart 1980; Chambliss 1994; Jacobs and Helms 1996). There is cross-national evidence as well. Savelsberg (1994) argues that different structures of political domination account for widely varying rates of imprisonment in the United States and Germany, largely through the influence of politics on the production of knowledge that informs criminal justice decision making. Hale's (1989) analysis of British politics in the 1970s shows the crucial importance of law and order discourse in Tory campaigns against unions and welfare.

In this analysis, political domination is treated in terms of partisan control of government. The countries in the present sample have roughly parallel sets of party alignments. Left labor parties, which draw their support from working-class, urban, and ethnically marginal voters, tend to define social problems in structural terms and to support tighter regulation of the economy and more expansive programs of social benefits. Right parties attract older, more affluent, rural, and native-born voters; they tend to define social problems in individual moral terms and to promote free-market economics and a law and order approach to social disruption. Thus party rule is not only likely to affect imprisonment directly, but indirectly as well through welfare, labor market, and education policies. I focus on the influence of right parties, in part because these ideological linkages are more explicit in their platforms, and also because the United States has no left-party equivalent to the Labour in Australia, New Zealand, the United Kingdom or to the New Democratic Party in Canada.

HYPOTHESIS 8.—*Prison populations expand more rapidly when right parties are in power than when parties of the left and center are in power.*

A note on directionality.—Hypotheses 3–7 anticipate that prison growth is inversely associated with growth in labor markets, education, welfare, and the military. Prior research and theory justify this anticipation, but only in bivariate terms—that is, in terms of the relationship between prisons and each of the other institutional fields. Borrowing from organizational theory and cultural sociology, I synthesized these bivariate arguments into a more general model of competitive relations among institutional fields. But while competition is useful as a general expec-

tation, it is unlikely to hold in every case. Particular relations among fields are negotiable, fluid, and emergent; at any given time, they may be competitive or cooperative and mutualistic (Friedland and Alford 1991). Thus, it is extremely unlikely, and perhaps impossible, that prison expansion will prove to be inversely associated with growth in all of the other life-course fields. This would imply that all of the other fields are mutualistically related to each other. This cannot be true, since we know from research already cited that labor-market capacity is inversely related to both military enlistments and welfare spending.

The competition model serves here as a theoretical starting point, not as a complete description of a closed and static system. In evaluating the usefulness of the institutional approach taken here, it will be important to attend to the overall pattern of associations, not just to individual coefficients. Indeed, the data contain some surprises in this regard, but these unanticipated results can only be interpreted post hoc, as a contribution to future theory.

Hypotheses: Interaction Effects

The theoretical framework presented earlier generated hypotheses at several levels of analysis and suggested that higher-level effects might influence those below—for example, the capacities of various life-course paths are likely to depend on the antecedent influences of policy trade-offs or partisan politics. So far I have discussed these multilevel influences only in additive terms, but it is also reasonable to suspect that they operate interactively. Two types of interactions in particular suggest themselves. First, life-course and policy effects may be conditional upon partisan dominance. Evidence already cited suggests that right parties in the United Kingdom, the United States, and New Zealand have raised the political salience of crime, welfare, and the economy by forging ideological linkages among these issues. The tighter the linkages, the more the various policy initiatives are dependent on each other: if the demand for law and order is understood simultaneously as a coded demand for reducing welfare benefits and unfettering the economy, then the association of prison expansion to welfare spending and unemployment should be stronger. Thus in general, trade-offs are likely to be sharper when right parties are in power.

Second, I am interested in whether the effects of all exogenous variables—including right-party dominance—are different in the United States than in the other countries in the sample. The reasons for this interest are both methodological and theoretical. Any general explanation of imprisonment rates requires consistent effects across the countries in the sample, and inconsistencies are most likely to arise from the fact that the

United States is an outlier with respect to the dependent variable. The obvious danger is that U.S. observations are producing spuriously significant coefficient estimates and perhaps suppressing other associations. Tests of interactions should identify the limits of the model. The theoretical issue is that the United States is exceptional in many respects besides high imprisonment rates. The United States has the most decentralized and fragmented polity, as well as the least bureaucratized central state administrative apparatus, of any modern democracy. Moreover, prisons and other social control institutions have historically been central to American debates about citizenship, social policy, and the meaning of democracy itself (Beaumont and Tocqueville 1964; Hamilton and Sutton 1989). As a result of both structural conditions and historical legacies, the United States may be qualitatively distinct in its propensity to politicize crime and punishment. This suggests that the *mechanisms* of prison expansion are different in the United States than elsewhere—an issue we can begin to sort out by testing for interactions.[8]

The framework I have outlined implies tests of 11 interactions: five variables (military enlistments, school enrollments, unemployment, welfare spending, and education spending) each with the right-party dominance variable and a dummy variable representing the United States, plus the interaction of right-party dominance and the U.S. dummy. All of the interaction terms are easy to interpret since the right-party variable takes on values from 0 to 1 and can for convenience be treated as a binary. The expectation in each case is that the coefficient for the interaction term will be in the same direction as the original main effect—substantively, that all effects are stronger under right parties and in the United States.

DATA, MEASUREMENT, AND ESTIMATION

Imprisonment as a Dependent Variable

Quantitative cross-national analysis of punishment raises difficult problems of conceptualization and measurement. This is particularly true for the outcome variable used here, imprisonment rates. Statistical reporting practices in different countries do not reflect natural categories of inmates,

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[8] An alternative strategy would be to use measures of institutional structure that vary across countries but not (usually) over time, such as Hicks and Swank's (1992) factor scales or the constitutional structure variable created by Huber, Ragin, and Stephens (1993). These variables carry a substantial amount of theoretical content, but their use is prohibited in the fixed-effects models used here, which contain dummy variables for $N - 1$ countries (see below). In separate analyses, I tested these variables in OLS as well as random-effects models and found no effects. It appears that the variation across this small sample of countries cannot be ordered in terms of a single underlying dimension.

but rather the administrative structures of prison systems. These administrative differences can yield inmate counts that are not comparable across countries. U.S.-based studies, for example, typically count only inmates in state and federal penitentiaries, ignoring large numbers of unconvicted inmates and petty criminals in local jails.[9] In studies of European and Commonwealth countries with more centralized prison systems, counts of prison populations typically include less serious offenders, who in the United States would be held in local jails, and sometimes include a substantial proportion of unconvicted prisoners as well.

As a first approach to the problem, this study focuses on trends in aggregate imprisonment rates. The definition of imprisonment used here comprises all inmates, unconvicted as well as convicted, incarcerated in adult prisons and jails administered by national, state, provincial, and local authorities. Imprisonment rates are defined in the conventional way as the proportion of inmates per 100,000 in the general population. This operational definition, while comprehensive and consistent across the countries in the sample, is nonetheless flawed because it aggregates demographic processes that are probably causally distinct. Imprisonment rates at any given time are the product of (*a*) rates of admission, which are determined by the scope of existing criminal statutes and the case-processing capacity of the courts, and (*b*) the average time served by inmates, which is determined by the severity of sentences imposed and the available prison capacity. Some scholars recommend using admission rates to measure levels of imprisonment because they are more responsive to short-term shifts in exogenous variables, and less likely to be influenced by the decisions of prison officials themselves, than prison populations; a second-best option is to use admissions as a control in models of prison populations (Jacobs and Helms 1996; Young and Brown 1993). Neither option is available here because none of the countries in the sample publishes admissions figures—especially for unsentenced inmates—for anywhere near the full three decades covered by this analysis. This study proceeds on the assumption that the sheer scope of imprisonment is a worthy object of investigation in its own right. It would certainly be useful to know how imprisonment rates in particular countries and particular times are produced, ideally using data not just on prison admissions but also on arrests, convictions, and parole releases. The goal of this analysis is to broaden our empirical understanding of imprisonment, and given the limitations of available data, the choice before us is between learning more about something or nothing about everything.

Imprisonment trends in the five sample countries are displayed graphically in figure 1. The graph expresses imprisonment rates in log form to

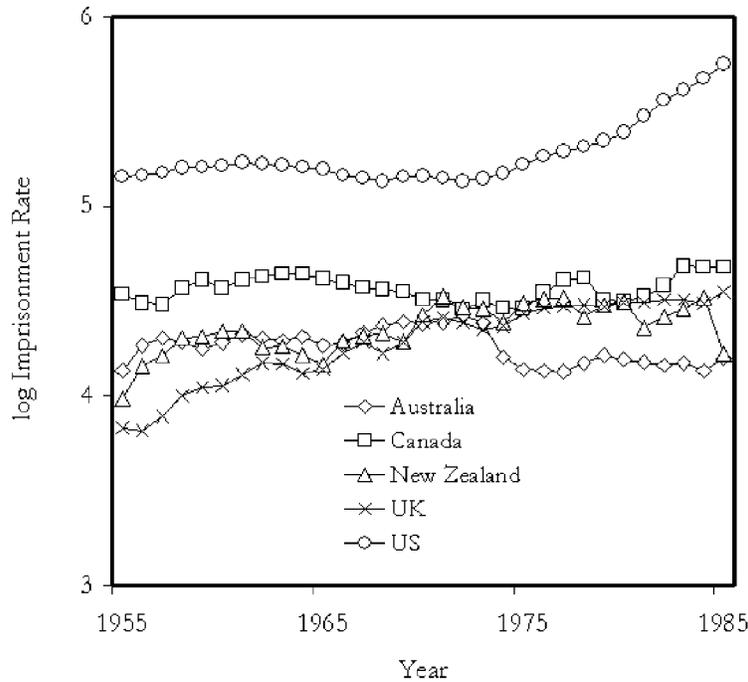[9] For a recent exception, see D'Alessio and Stolzenberg (1995).

Fig. 1.—Prison inmates per 100,000 population (log)

show more clearly the substantial variation that exists among Australia, Canada, New Zealand, and the United Kingdom. Incarceration rates in the United States are higher than in the other four countries by a factor of two to three, depending on the year. But prison *growth* in the United Kingdom was greater in percentage terms than in the United States over the entire 30-year period (104% to about 80%). A key difference is that growth in the United Kingdom was relatively steady from 1955 to 1985, while virtually all prison growth in the United States occurred after 1972. New Zealand shows the third highest rate of growth (26% overall, or 68% if we ignore the steep drop in 1985), followed by Canada (14%). Australia shows a net decline in the imprisonment rate of 5%, mostly the result of a drop that occurred in the early 1970s.

The imprisonment data used in this study were drawn primarily from national statistical yearbooks. Canadian imprisonment data were provided directly by the Canadian Centre for Justice Statistics; for Australia, yearbook data were supplemented with data published by the Australian Institute of Criminology (Mukherjee et al. 1989) and in Biles (1982).

Sources for independent variables, which I have already described, are detailed in appendix table A1; descriptive statistics and a correlation matrix for all variables in the analysis are in appendix table A2.

Estimation

The present data comprise 31-year time series for five nations. The most efficient approach to these data is through pooled time-series cross-section regression techniques that analyze variation occurring over both time and space, yielding a single set of effect parameters. This study will complicate the basic pooled regression approach by estimating models in partial-adjustment form.[10] In practice, a partial-adjustment model requires that a lagged value of the dependent variable is used as a predictor in the regression equation. The logic of partial adjustment suggests that exogenous variables set a "ceiling capacity" on imprisonment but that prison populations do not respond immediately to changes in exogenous variables. Prisons, perhaps more so than other organizations, are highly inertial in terms of both the policies that govern their actions and the speed at which inmate populations change. Thus the rate of imprisonment in a given year is likely to be heavily dependent on the rate of imprisonment in prior years, and models that omit a lagged dependent variable are likely to be misspecified.

Choice of a lag interval required some experimentation. Early results showed that models with one-year lags are extremely weak in terms of individual coefficients and overall fit, but longer lags (up to five years) produced progressively stronger models. This is a sensible result considering the "policy lags" (Dornbusch, Startz, and Fischer 1998) that are likely to delay the effects of exogenous variables—such as the lead times required to build and staff new facilities, for officials to develop new routines, and to clear the system of old cases. The following analysis uses a lag interval of three years for the lag-dependent variable and most independent variables. This interval is a satisfactory choice: shorter lags obscure some interesting effects, and longer lags shorten the time series without yielding any empirical payoff. The resulting data set contains 28 observations on five countries, yielding a total N of 140 observations.

Pooled models raise thorny estimation problems. Autocorrelated error can produce biased regression coefficients; heteroscedasticity is likely to

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[10] My use of partial adjustment models follows a number of prior studies in organizational growth and decline (Freeman and Hannan 1975; Meyer and Hannan 1979; Nielsen and Hannan 1977) and welfare-state development (Hicks and Misra 1993; Hicks and Swank 1992), as well as the analysis by Berk et al. (1982) of prison expenditures in California and Sutton's (1987, 1990, 1991) work on the expansion of prisons, juvenile institutions, and mental hospitals in the United States.

arise when one is comparing nations, such as the United States and New Zealand, with widely different population sizes; heterogeneity bias may arise from unmeasured time-invariant characteristics of individual countries. Heterogeneity is an obvious danger in these data because of the consistently high imprisonment rates in the United States. Another potential problem is nonstationarity: when variables move in the same direction over time, coefficients may show spurious effects. WGLS estimators that are typically used to address many of these problems (described in Hicks [1994]) raise as many problems as they solve (Beck and Katz 1995, 1996). In the present analysis, inclusion of a lagged dependent variable corrects for AR1 autocorrelation as well as, or better than, weighting on the lagged residuals, and also yields valuable substantive information about the dynamics of the model (Beck and Katz 1996). To correct for heterogeneity bias, I include dummy variables for four of the five countries in the sample in each model.

The standard solution for nonstationarity is to difference the variables, thus shifting attention to short-term changes around the trend rather than the trend itself. Pyle and Deadman (1994) argue that most prior time-series studies of imprisonment are suspect because of their failure to correct for nonstationarity and recommends differencing in all cases. Differencing also has salutary effects in terms of heteroscedasticity, heterogeneity, and multicollinearity. But as Jacobs and Helms (1996) argue in some detail, differencing is a drastic remedy, and the available diagnostic tests are controversial. They chose to difference their imprisonment data because nonstationarity was indicated by the appropriate Dickey-Fuller tests (Greene 1993, pp. 563–65) *and* because levels models were unstable. The present situation is similar. Test results were inconsistent, and models using levels variables were destabilized by multicollinearity among the regressors. In the models to follow, most continuous variables are expressed in log first-difference form, $\log(Y_{(t)}/Y_{(t-3)})$ for the dependent variable and $\log(X_{(t)}/X_{(t-3)})$ for independent variables. There are three exceptions. First, following the conventional logic of partial-adjustment models, the lag-dependent variable is expressed in log-level form as $\log(Y_{(t-3)})$. Second, indicators of the young male population and homicide rates are also expressed in log-level form. In both cases, first-difference measures produced coefficients that were always negative and usually statistically significant, thus difficult to interpret in a theoretically fruitful way. Third, right-party cabinet dominance is expressed, following Hicks and Swank (1992), as a four-year running average.

With these corrections, models are amenable to OLS estimation—with one proviso. As Beck and Katz have shown (1995), WGLS and OLS estimates of pooled models tend to underestimate the standard errors of the regression coefficients. Following their recommendation, models pre-

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sented below are estimated using OLS with more conservative *panel corrected* standard errors.

RESULTS

Main Effects

Results from tests of hypotheses 1–8 are presented in table 1. The stepwise model-building process depicted there is designed to reveal the kinds of indirect effects discussed above. Model 1 presents the simplest and most instrumental account of prison growth, comprising only controls for prior imprisonment rates, the relative size of the young male population, the prevalence of homicide, and stable country-specific effects. Model 2 adds life-course effects of schooling, the military, and labor markets; model 3 adds spending on welfare and education; and model 4 adds right-party dominance. For reasons that will become clear, model 5 is a trimmed model that omits the young males variable and presents standardized as well as metric coefficients.

I begin with some observations about control variables and model fit, then discuss results that address substantive hypotheses. Coefficients for the lag-dependent variable are negative and significant in all models, as we expect from the logic of partial adjustment. Coefficients for the young males variable in models 1–4 are nowhere near significant. The estimated effect of homicide rates is positive and significant in the first three models, but coefficients diminish in size as other variables are added to the equation. In model 4, the homicide effect drops below statistical significance, but it emerges again in model 5 when the young males variable is dropped. Model fit improves significantly each time variables are added (at $P <$ .001), and omission of the young males variable in model 5 causes only an insignificant loss of fit. These results suggest that whatever effect young male cohorts may have on prison growth, it operates mainly through the more proximal influence of homicide rates—suggesting some support for an instrumental explanation over an institutional life-course explanation. But a closer look suggests that that would be an overinterpretation of what turns out to be a trivial difference. Separate tests (not shown here) reveal that when homicide rates are omitted from models 1–4, the coefficients for %young males are consistently positive and significant, and overall chi-square values are only slightly lower than in models that omit %young males and retain homicide rates. In model 5, %young males can be substituted for homicide rates with no loss of fit at all and no meaningful changes to the other coefficients. Keeping in mind that these variables are offered primarily as statistical controls, their interchangeability

TABLE 1

OLS Estimates of the Effects of Selected Variables on Imprisonment Rates
in Five Common-Law Countries, 1955–1985

	b					β
	1	2	3	4	5	
Log lag dependent	−.302***	−.308***	−.314***	−.306***	−.294***	−1.18
	(.0570)	(.0569)	(.0550)	(.0534)	(.0525)	
Log %young males	−.0882	.00673	.0182	.117		
	(.112)	(.109)	(.106)	(.108)		
Log homicide rates	.170*	.148*	.120*	.0752	.127**	.727
	(.0702)	(.0660)	(.0668)	(.0667)	(.0468)	
Δ male school enrollments		.0968	.0538	.0542	.0398	.0524
		(.0608)	(.0596)	(.0579)	(.0566)	
Δ military enlistments		.181***	.179***	.195***	.186***	.275
		(.0499)	(.0494)	(.0482)	(.0476)	
Δ unemployment rates		.0288**	.0374***	.0362***	.0362***	.221
		(.0120)	(.0118)	(.0115)	(.0116)	
Δ welfare spending			−.122***	−.105**	−.106**	−.214
			(.0349)	(.0344)	(.0345)	
Δ education spending			−.0304	−.0241	−.0332	−.0458
			(.0644)	(.0625)	(.0622)	
Right-party dominance				.0605**	.0535**	.218
				(.0207)	(.0198)	
Canada	.110***	.118***	.123***	.155***	.151***	
	(.0273)	(.0266)	(.0256)	(.0272)	(.0270)	
New Zealand	.105***	.0738**	.0564	.0430	.0572*	
	(.0290)	(.0285)	(.0287)	(.0283)	(.0251)	
United Kingdom ...	.127***	.143***	.139***	.150***	.151***	
	(.0275)	(.0267)	(.0296)	(.0290)	(.0291)	
United States of America	.164	.207*	.247*	.308**	.234**	
	(.103)	(.0977)	(.0976)	(.0970)	(.0697)	
Constant	1.30***	1.14***	1.18***	.932**	1.08**	
	(.262)	(.259)	(.262)	(.268)	(.230)	
χ^2	49.65***	77.02***	96.10***	110.5***	108.4***	

Note.—Metric coefficients, with panel corrected SEs given in parentheses and standardized coefficients given for model 5.
* $P < .05$.
** $P < .01$.
*** $P < .001$.

is good news: it suggests that, one way or another, supply-side effects on imprisonment are adequately specified.[11]

Country dummy variables allow each country in the sample to have a different intercept. The omitted category comprises observations from Australia, so one can read the constant term as the Australia intercept and read other intercepts as the sum of the constant term and the appropriate coefficient. As we might expect, the U.S. intercept is consistently the largest: while the coefficient is nonsignificant in model 1, it becomes significant and grows larger in the more complex models. Superficially, this might suggest that adding substantive explanations does not tame U.S. exceptionalism, but instead makes it more dramatic. But coefficients for the Canada and U.K. dummies also grow larger; only the New Zealand coefficients decline after model 1. In models 4 and 5, the coefficients for Canada, the United Kingdom, and the United States do not differ significantly—hence they effectively have the same intercept.[12] This is somewhat misleading, since the U.S. coefficient represents an average of declining imprisonment rates before 1973 and the undeniably unique rates of growth thereafter. Still, these results highlight the exceptionally *low* rates of growth in Australia (and to a lesser degree in New Zealand) as much as they do exceptionally *high* rates in the United States

I turn now to results concerning substantive variables in models 2–5. In model 2, unemployment growth is the only life-course variable that performs as expected. The coefficient is positive and significant, showing that a 1% rise in unemployment rates corresponds to about .03% growth in imprisonment rates. Male secondary and tertiary school enrollment rates have no apparent association with prison growth. The association between prison growth and military enlistments is unexpectedly *positive* and clearly significant—on average, when military enlistments grow 1%, imprisonment grows nearly two-tenths of a percent. This not only fails

[11] I chose to retain the homicide variable in model 5 and in subsequent interaction tests (rather than %young males) for two reasons: to maintain continuity with previous research in the Rusche-Kirchheimer tradition, in which the standard model includes an indicator of business cycles and a control for crime, and to include a substantive variable that could in an easily interpretable way capture some of the uniqueness of the U.S. case. In these data, the United States observations are outliers with respect to both homicide rates and prison growth, but it ranks only fourth in the relative size of its young male cohorts.

[12] In model 4, the lower bound of the 95% confidence interval for the U.S. coefficient is 0.118, and in model 5 it is 0.0946—in both cases overlapping substantially with point estimates for Canada and the United Kingdom When model 5 is reestimated with an Australia dummy included and the U.S. dummy omitted, coefficients for Canada and the United Kingdom are not significant; that for New Zealand is significant (at $P < .05$).

to support the hypothesis that military expansion drains the pool of likely prison inmates, it points to quite the opposite association.

Model 3 includes measures of changes in social welfare and education spending. The welfare effect is significant, and the coefficient is in the anticipated negative direction, supporting the hypothesis that higher welfare spending translates into lower rates of prison growth. But the coefficient for education spending is effectively zero: economic investments in education seem to have no more bearing on imprisonment than do school enrollments. Specification of the welfare effect causes the coefficient for unemployment rates to grow about 30% over its level in model 2. This shift is readily interpretable. Welfare spending tends to grow in response to rising unemployment, if only because newly laid-off workers are likely to apply for unemployment insurance benefits. Thus when the effect of welfare is omitted, as in model 2, the unemployment coefficient is biased downward because it represents both a real positive effect and the offsetting negative effect of welfare.

In model 4, we see that the coefficient for right-party dominance is positive and significant, supporting the hypothesis that prison growth accelerates during periods of conservative rule. Inclusion of the right-party variable changes the other coefficients only modestly: as previously noted, the homicide rate drops below significance, the military enlistment coefficient rises slightly, the unemployment coefficient declines slightly, and the coefficient for welfare spending declines about 14%. This suggests that party politics may have indirect effects that operate primarily through welfare policy and perhaps through economic policies that influence unemployment rates.

Model 5 drops the %young males variable but retains the two (apparently redundant) education variables to provide a baseline for subsequent tests of interaction effects. Since most prior research has focused almost exclusively on the influence of business cycles, it would be useful at this point to draw some conclusions about the relative impacts of the broader set of variables tested here. This model offers two ways to approach this issue. The first approach is to compare directly the metric coefficients for variables that are expressed as proportional changes. These coefficients are unit-free elasticities, and by this criterion, it appears from model 5 that the impact of unemployment is tiny compared to those of military enlistments and welfare spending. But elasticities can be misleading. One problem is that they ignore differences in the historical variability of the independent variables: the substantive meaning of a 1% change depends both on the level of a variable at a particular point in time and on its variability over time. The usual strategy is to compare the impact of different variables at their means, but this yields conclusions only of a very stylized sort. Another problem is that no comparison is possible with

variables expressed in terms of levels, such as homicide rates and right-party dominance. Thus, the second approach is to look to standardized coefficients for a more conservative, general, and arguably more appropriate gauge. By this criterion, control variables predominate: prison growth is constrained most severely by the prior size of the prison population ($\beta = -1.18$) and next by the incidence of homicide ($\beta = 0.727$). Setting aside school enrollments and education spending, the standardized effects of other substantive variables are roughly similar in absolute value. In particular, the coefficient for military enlistments is still the largest, but not remarkably so, and that for unemployment rates is far from trivial.

Results from the main-effects models support four tentative conclusions. First, there is some evidence that prison growth is in part an instrumental response to crime but that evidence is equivocal: imprisonment rates not only rise faster where homicide rates are higher, but also where there are relatively more young males. Second, while the persistent effects of unemployment rates lend the broadest support so far to the Rusche-Kirchheimer hypothesis, the models show that it is by no means a complete explanation. The unemployment effect appears to be only one aspect of a complex set of institutional processes: political dynamics, welfare spending, and (perplexingly) military expansion show effects that are as strong as that of growth in unemployment. Third, results so far suggest that the observed significant effects are for the most part direct and independent. The introduction of the welfare variable did not wipe out the effects of military enlistments or unemployment, and the inclusion of party dominance changed other coefficients only slightly. The next step in the analysis is to challenge these conclusions with the introduction of interaction terms.

Interaction Effects

The 11 hypothesized interactions were tested by adding them one at a time to model 5 in table 1. The four interactions appearing in table 2 are those that produced both statistically significant b coefficients and significant improvements to the fit of the model. The rows of the table show coefficients for main effects of the two variables involved in each interaction, coefficients for the interaction terms themselves, and χ^2 values for improved fit (with one added parameter). The expectation in each case was that interaction effects, if any, would be in the same direction as the main effects—in other words, that effects would be stronger under right-party rule and in the United States. Empirical results turn out to be more complex.

Consider first the interaction of unemployment growth and right-party dominance, shown in the first row. In this specification, the coefficients for both main effects are considerably larger than in the baseline model

TABLE 2
SUMMARY OF STATISTICALLY SIGNIFICANT INTERACTION EFFECTS

X variable	b_X	b_USA	$b_\text{right party}$	$b_\text{interaction}$	χ^2
Δ unemployment rates ...	$.114^{***}$		$.0703^{***}$	$-.0963^{**}$	12.32^{***}
Δ welfare spending	$-.0596$	$.279^{***}$		$-.238^{**}$	13.55^{***}
Δ education spending	$.0208$	$.286^{***}$		$-.293^{**}$	11.56^{***}
Right-party dominance ...		$.198^{***}$	$.0261$	$.0854^{*}$	5.15^{*}

* $P < .05$.
** $P < .01$.
*** $P < .001$.

5 in table 1; the interaction coefficient is negative and almost the same (absolute) size as the coefficient for the main effect of unemployment. This indicates, contrary to expectations, that right parties blunt the effect of unemployment (driving it to zero) rather than sharpening it. Clearly there is no support here for the idea that right parties politicize the workings of business cycles. Rather the most reasonable interpretation (since the main effect of right parties remains positive and significant) is that right parties raise the baseline rate of prison growth to such a point that fluctuations in the business cycle no longer matter.

The second significant interaction is between welfare spending and the U.S. dummy variable. Results in this case are closer to expectations: the main effect of welfare disappears, the positive effect of the U.S. variable remains significant, and the interaction coefficient is negative—suggesting substantively that the decelerating effect of welfare spending on prison growth is confined to the United States. The model in the third row describes the interaction of education spending and the U.S. dummy. This reveals a remarkable effect: there seems to be a trade-off between education spending and prison growth after all—one that was suppressed in the main-effects models in table 1 because it is unique to the United States. This sensitivity may arise from the central role that education has always played in the U.S. nation-building project, and it may as well have to do with the fact that education and criminal justice are both primarily local functions in the United States and thus are particularly likely to covary. The fourth significant interaction is between right-party dominance and the U.S. variable. Observe in the bottom row that the main effect for right parties has effectively disappeared, and the coefficient for the interaction term is positive. Substantively, this suggests that only in the United States do right parties effectively encourage punitive policies.

Taken at face value, the findings in table 2 suggest a strong case for American exceptionalism: the United States appears to be the only country in the sample where welfare spending, education spending, and conservative politics matter. This conclusion leaves us very little ahead of where

TABLE 3
Conditional Effects of Growth in Unemployment and Welfare
Spending for Different Values of Right Party and U.S. Variables

	Intercept	$b_{\text{unemployment}}$	b_{welfare}
Right party = 0, U.S. = 0	1.19***	.119***	−.0687*
Right party = 1, U.S. = 0	1.27***	.0179	−.0687*
Right party = 0, U.S. = 1	1.50***	.119***	−.305***
Right party = 1, U.S. = 1	1.58***	.0179	−.305***

* $P < .05$.
** $P < .01$.
*** $P < .001$.

we began the analysis. But it is almost certainly wrong, since it rests on the assumption that all four interactions operate jointly. The logical implausibility of this assumption is apparent when we try to integrate the interaction of unemployment and right parties with the others: How can right parties have a positive main effect only in the United States *and* an across-the-board negative interaction with unemployment growth? In an attempt to narrow the field, I estimated a set of models with various combinations of interaction terms. In the end, only two interactions are clearly nonspurious: those between unemployment and right parties and those between welfare and the United States. Interpretation of these results is fairly straightforward. The most effective way to describe them is to decompose the interactions to show conditional intercepts and effects of unemployment and welfare under different values of the right-party and U.S. variables. These results are shown in table 3.[13]

Several observations are called for. First, the main effects of right parties and the United States are independent and additive. The lowest baseline rates of prison expansion are in countries other than the United States when right parties are out of power, and the highest rates are in the United States when the right party (Republicans) controls the cabinet. Second, the maximum effect of unemployment is more than three times higher than that estimated in additive models, but that effect disappears when right parties are completely in control of government. Third, the offsetting influence of welfare spending is more than four times stronger in the United States than elsewhere, but the effect remains significant among the other four countries. Finally, it is important to add that in-

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[13] Procedures for calculating conditional coefficients and their associated standard errors are given by Friedrich (1982).

clusion of these interactions does not materially change any of the other findings from table 1.

SUMMARY AND INTERPRETATION

This article was motivated by two problems in the existing research on imprisonment, one empirical and one theoretical. The empirical problem is that we lack rigorous analyses of the glaring differences in recent imprisonment trends among Western societies. More specifically, past research explained the divergence between the United States and other countries mostly with exceptionalist accounts that are so far untested, and perhaps untestable, using cross-national data. The theoretical problem arises from the limitations of the venerable Rusche-Kirchheimer hypothesis. Results supporting the hypothesis are suspect, but until recently there was little apparent interest in searching for a more comprehensive explanation of imprisonment trends. Indeed, the time-series designs that have commonly been used in this line of research inhibit tests of wide-ranging models. Together these problems contributed to a widening gap between our concrete knowledge of particular countries and our capacity to develop a general theory of punishment.

In this analysis, I have sought to bridge that gap by outlining a theoretical framework that nests the labor market/imprisonment association within a broader system of social classification and by testing hypotheses derived from that framework on a sample of countries observed over time. In constructing that framework, I took seriously Garland's (1992) injunction to treat the prison as an institution, not simply as an instrument of social control or class oppression. Drawing on contemporary institutionalist theory, I argued that classification patterns are the outcomes of institutional projects that operate at three levels: through the allocation of persons among life-course paths, trade-offs among social programs that manage various socially marginal populations, and contests for political dominance.

Results supported hypotheses at all three levels of analysis. Confirming much past research, the dominant life-course effect comes from fluctuations in labor markets: when opportunities for legitimate employment expand, prison growth slows. The observed effect of unemployment is not surprising, but it is noteworthy because it appears for the first time in a cross-national analysis using stationary data series and controlling for a range of competing explanations. Analysis revealed no other countervailing allocation effects. Whether young men's school enrollments are rising or declining seems to make no difference for imprisonment, suggesting reasonably that schools and prisons do not draw from the same undifferentiated pool.
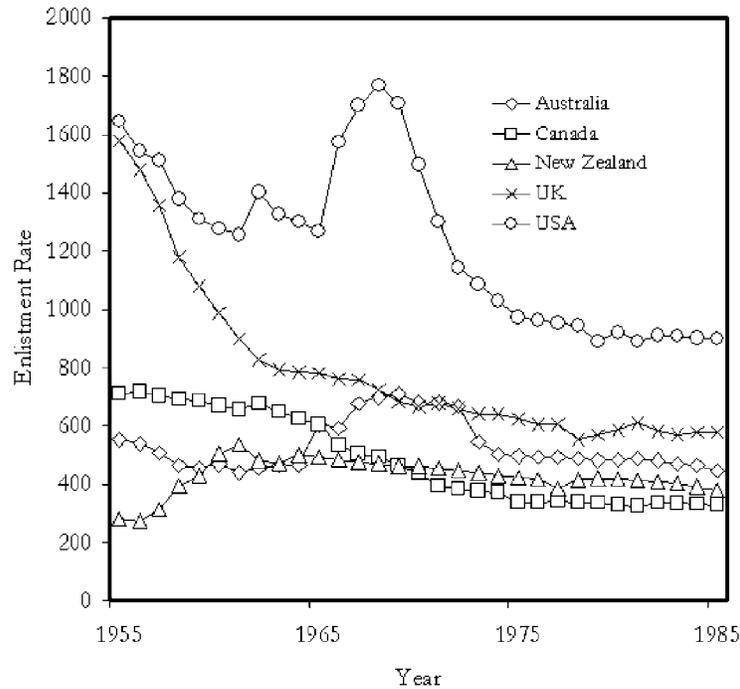
FIG. 2.—Military enlistments per 100,000 population

There is strong evidence of one policy trade-off: declines in welfare spending are associated with growth in imprisonment rates—an association that has long been suspected but never shown. The welfare/ imprisonment trade-off is much stronger in the United States, but it remains significant across the rest of the sample. Finally, there is convincing evidence that politics matters. The consistently positive effects of right-party rule offer grounds for generalizing Jacobs and Helms's (1996) findings about the punitive tendencies of conservative politics beyond the borders of the United States. Indeed, the observed interaction between right-party rule and unemployment growth suggests that politics not only matters, it trumps economics.

The observed effect of military enlistments is a conspicuous anomaly, suggesting that military expansion contributes to the expansion of prisons. If this is a real effect, it must operate in diffuse and long-term ways because enlistment and imprisonment trends are not parallel. As the enlistment data displayed in figure 2 show, conspicuous episodes of military growth occurred only in the United States and the United Kingdom during the

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Korean War and again in the United States during the Vietnam War (though it does not show in the figure, both countries thoroughly demobilized after World War II). As we saw earlier in figure 1, the most dramatic episode of prison expansion was in the United States after 1973, and prisons in the United Kingdom grew at a high rate for the entire observation period. The association appears due not to military expansion, but to slow demobilization. After each war, enlistments in both countries declined at a decreasing rate: from about 1975 on, rates in the United States settled at a level about twice as high as those in Australia, Canada, or New Zealand; rates in the United Kingdom remained about a third higher. This suggests that the association between enlistments and imprisonment is a sustained echo of Korea and Vietnam. But does this statistical association signify a real causal effect? It is tempting to find in these results evidence of a diffuse ideological mutualism, reflecting an elective affinity between coercive forms of problem solving in domestic as well as international relations. But the present analysis cannot take us beyond the level of speculation in this regard.

These findings add considerable depth to our understanding of imprisonment trends, at least in the five countries under examination: net of the effects of supply-side factors, prisons grow more rapidly during economic downturns, when social spending is constrained, when conservative parties control the policy agenda, and perhaps under conditions of long-term military mobilization. Moreover, these results are fairly theoretically coherent: taken together they undercut an instrumental account of imprisonment and move us decisively toward an account that emphasizes the politics of moral order. But there is also persistent evidence of American exceptionalism—the analysis has whittled away at it, but it has not eliminated it. Can this "U.S. effect" be theoretically housebroken?

We can gain some leverage on the problem by revisiting the analysis of interaction effects, since it tells us something about *how* the United States is different. The effect of welfare spending is exaggerated in the United States; so are the effects of education spending and right parties, though in the end not significantly so. These findings fit a pattern that was predicted early on: under the administratively fragmented and decentralized structure of American government, social policy tends to be highly politicized, localized, and particularistic. When policy deliberations are conducted through election campaigns rather than in government ministries, the symbolic stakes are higher; in times of perceived disorder, discursive linkages among different policy arenas are drawn more explicitly (Sutton 1988). Themes of administrative fragmentation, particularism, and moralism run through the literature on the American welfare state (e.g., Skocpol 1995; Weir, Orloff, and Skocpol 1988), and they also appear in the literature on public schooling in America (Tyack and Hansot

1982). They apply with even greater force to American penal policy. Observers since Beaumont and Tocqueville in the 1830s have argued that America's dispersed polity and its historical obsession with punishment are two sides of the same coin (Beaumont and Tocqueville 1964; Hamilton and Sutton 1989; Mead 1918; Melossi 1990). More specifically and concretely, police functions in the United States are organized locally, policy-making authority is dispersed among the states, and judges and prosecutors must constantly renew the approval of voters—structural conditions that create a chronic vulnerability to moral panics (Beckett 1997; Caplow and Simon 1999; Tonry 1999). From this perspective, a weak state makes moral order precarious.

Subsequent research will extend this line of research in two directions. First, it will be useful to explore the distinction between sentenced and unsentenced inmate populations. This distinction is typically muddied or ignored in the existing literature, but it can be turned to theoretical account. Populations of unsentenced inmates are formed at the intake end of the criminal justice process and might be sensitive to allocative constraints—the sheer supply of bodies—that are so far latent. Sentenced inmate populations are produced by a lengthy and complex filtering process and thus are likely to be more sensitive to higher-order effects of policy trade-offs and political shifts. Second, the sample will be expanded to include a wider range of developed Western democracies—specifically, Scandinavian and continental European societies. One obvious benefit of this will be to introduce more fine-grained statistical variation into the analysis. But more important, it will introduce greater qualitative variation in polity structures, legal systems, and welfare regimes. There is no reason to assume that the causal processes operating in the common law world are the same as those elsewhere, and we know little about how corporatist politics or social-democratic welfare policies, for example, are related to punishment. Thus, the results presented here should be seen as the first steps toward a more nuanced account of the dynamics of imprisonment.

APPENDIX

<div align="center">

TABLE A1

VARIABLE DESCRIPTIONS AND SOURCES

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Variable	Description	Source
Dependent	Total inmates per 100,000 population (proportional change from $t - 3$ to t)	Various (see text)
Lag dependent	Total inmates per 100,000 population (log, at $t - 3$)	Various (see text)
Homicide rates	Number of homicides per 100,000 population (log, at $t - 3$)	WHO (1951–64; 1962–88)
Military enlistments	Active-duty military personnel as percent of total population (proportional change from $t - 3$ to t)	Faber (1989), with updates from IISS (1983–85)
Male school enrollments ...	Males enrolled in secondary and tertiary schools as percent of total population (proportional change from $t - 3$ to t)	UNESCO (1955–90)
Unemployment rates	Unemployed persons as percent of total working population (proportional change from $t - 3$ to t)	ILO (1955–90)
Welfare spending	Sum of expenditures on unemployment compensation, work injury benefits, family allowances, and public assistance as a percentage of GDP (proportional change from $t - 3$ to t)	ILO (1961–92)
Education spending	Total government expenditures on education as percentage of GDP (proportional change from $t - 3$ to t)	UNESCO (1955–90)
Right-party dominance	Proportion of total cabinet seats held by right parties (running average from $t - 4$ to t)	Hicks and Swank (1992)

NOTE.— Denominators for ratio variables (population, working population, GDP) are from Summers and Heston (1991). Monetary variables are 1985 \$U.S. using PPP exchange rates. WHO = World Health Organization; ILO = International Labour Organization.

TABLE A2

CORRELATIONS, MEANS, AND STANDARD DEVIATIONS FOR DEPENDENT AND
INDEPENDENT VARIABLES

	1	2	3	4	5	6	7	8	9
1. Δ imprisonment rates	. . .								
2. log lag dependent	−.0338								
3. log homicide rates	.0725	.888							
4. Δ school enrollments	.0731	−.130	−.231						
5. Δ military enlistments ...	.117	−.0292	−.0678	.090					
6. Δ unemployment rates ...	.222	−.192	−.127	−.083	−.00550				
7. Δ welfare spending	−.163	.0453	.0624	−.254	−.0721	.149			
8. Δ education spending	−.0808	−.0958	−.156	.270	−.130	.0138	−.0902		
9. right-party dominance ...	.174	−.240	−.141	.0930	.0910	.118	−.207	.0537	. . .
Mean	.0337	4.53	1.07	.0437	−.0448	.234	.068	.0547	.633
SD	.0964	.388	.554	.127	.143	.589	.194	.133	.394

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