2-1-1991

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DOI: https://doi.org/10.2307/2131723


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The New Era-New Deal Test

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Political scientists usually view the New Deal as transforming the American state. A sizable literature in economics and history, however, has cast doubt on the significance of the changes wrought by the FDR administration. In this paper we propose a model of the state, and then test important hypotheses about it. More specifically, we focus on the New Era-New Deal period to test how, if at all, the American state changed during this critical time in its history. We systematically analyze five state functions: stabilization, redistribution, regulation, police power, and administration. Within each category, quantitative policy measures are evaluated. Although the FDR administration produced many notable changes in government policy and structure, our analysis suggests that the American state, measured in these several ways, was not transformed from the New Era to the New Deal.

After considerable neglect, theories of the state have enjoyed a renaissance in political science (e.g., Benjamin and Elkin 1985; Bornstein et al. 1984; Evans et al. 1985). What this activity means is not altogether clear, but it has sparked lively debate (see Almond 1988, Nordlinger 1988, Lowi 1988, and Fabbrini 1988). In studying the changing American state, the events of the New Deal are regarded as critical. Theda Skocpol (1980, 157), a leading theoretician, asks the fundamental question: “How can we account for New Deal transformations in the American state and politics?” Beyond doubt, the FDR administration was responsible for laws which importantly altered American government. Recall the Wagner Act of 1935 or the initiation of Social Security in 1935, to name two outstanding examples. Of course, other major changes occurred during the period. The administrative apparatus of government became more federal. In the mass electorate, party realignment took place. As a response to the Great Depression, the New Deal took on symbolic as well as material importance.

While such shifts are profound, one should not forget that all political eras—the Square Deal, the New Freedom, the New Frontier, the Great Society—generate their own set of unique and vital policy changes. Some of

We thank Andrew Skalaban for research assistance and the Hoover Presidential Library Association for financial support.

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these changes may transform the state, but others surely do not. Our suggestion is that the state transforming properties of the New Deal have been much exaggerated. Beneath the dramatic program names, court cases, and political symbols of the New Deal, a rather remarkable policy continuity from Herbert Hoover and the New Era is visible. The state, as a functioning political organ, actually emerges as little changed by the New Deal. To demonstrate this point, we follow several lines of argument. Speaking broadly, the research unfolds from historical context, to state theory, to measurement and testing. At the end, conclusions about the state effects of the New Era-New Deal transition are drawn.

THE HISTORICAL CONTEXT

According to our survey of American government textbooks, the conventional wisdom is that the New Era and New Deal periods are polar opposites. The former is thought to have been governed by a decentralized polity at the mercy of the free market, while the latter was run by a bold federal government managing social and economic change. In general, the view is that, in contrast to Roosevelt, Hoover was an indecisive leader with a "hands-off" attitude toward business and society.

While some may find the conventional wisdom useful as a sort of historical shorthand, researchers in economics and history have had increasing difficulty with it. Since Herbert Stein (1969), there have been complaints from economists that FDR did not launch a "fiscal revolution," so much as elaborate old policies. In a recent and thorough treatment, William Barber (1985) concludes that there was, after all, a good deal of Keynesianism in the "new economics" of Hooverism. For some time, Hoover historians have been working to show that Hoover himself was humanitarian, progressive, even liberal (Clements 1984; Wilson 1975; Giglio 1983), and that his presidency is the victim of the distortions of hindsight generated by the cloud of the Great Depression (Burner 1979, 212; Leuchtenburg 1958, 251; Hawley 1986, 1088).¹

Our purpose is not to solve specific research quarrels in the Hoover-Roosevelt historiography. These quarrels are ongoing, as a new spate of historical works on FDR and the New Deal indicates (see Lash 1988; McJimsey 1987; Olson 1988; Schwarz 1987; Ferguson 1984; Hawley et al. 1988). Rather, for now, we wish merely to raise a doubt. Are the policy differences between Hoover and FDR often overdrawn?

What we find in examining the period is that, contrary to what is com-

¹The thrust of this argument is that Hoover's policies were more "liberal" than commonly thought, and therefore, much like Roosevelt's. Radical historians (i.e., Bernstein 1968; Rothbard 1972; Radosh 1972) have also concluded that Hoover and Roosevelt were similar but with a different twist: the New Deal was essentially conservative and not fundamentally different from what preceded it.
monly supposed, Hoover had a policy agenda not so different from Roosevelt (see also the useful essay by Arnold 1972). Several FDR programs seem to have gotten their start under Hoover. For example, the Federal Emergency Relief Administration (FERA) of Roosevelt had as its precursor direct unemployment relief through the Reconstruction Finance Corporation (RFC). Before the Agricultural Adjustment Administration (AAA), Hoover had initiated commodity price supports by means of national government purchases. A core idea of the National Recovery Administration (NRA), that businesses should band together to increase efficiency and prevent harmful competition, was forcefully advocated by Hoover as far back as 1921 (in a well-publicized unemployment conference). In banking, the Federal Deposit Insurance Corporation (FDIC) was foreseen in Hoover's special (January 1932) message to Congress, which called for the safeguarding of bank depositors. The newly-named Public Works Administration (PWA) grew out of the several increases in public works spending that Hoover had instigated prior to 1932. These examples suggest that much of the New Deal came from the New Era. Indeed Roosevelt braintruster Rexford Tugwell confessed, “We didn't admit it at the time, but practically the whole New Deal was extrapolated from programs that Hoover started” (in Burner 1979, 244; see also Romasco 1974). The New Deal programs were, to borrow Polsby's (1984) terms, “incubated,” not “acute,” policies.

THEORY AND MEASUREMENT OF THE STATE

The above review of historical evidence is provocative but incomplete. Hoover and Roosevelt may have shared many political principals and held to a policy agenda that was really not too dissimilar. Still, this does not mean that they were equally successful in getting their policy proposals through Congress. Further, certain policies, whether implemented or not, may have had little to do with transforming the state. Therefore, in comparing the policies of the period, we need to focus more systematically on real change (as opposed to possible or proposed change). As well, changes in the policies examined must have the potential of leading to state transformation. A search for the relevant policy arena, then, forces an explicit consideration of the meaning and measurement of “the state.”

Political scientists have long grappled with different definitions of the state (Mansfield 1983). There seems to be broad agreement that the state is distinct from society, analytically separate from individuals, groups, or parties. Beyond that, accord on the definition is spotty. Some students of the American state have abandoned, or avoided, the task. Stephen Skowronek (1982), in his seminal work Building a New American State, nowhere defines that central concept. In a recent critique, Theodore Lowi (1988, 891), while applauding the “return to the state,” believes it is a concept that “will inevitably be poorly defined.”
We take heart from these cautionary examples. To move forward the scholarly debate on the state, researchers must be as explicit about their definitions and measures as possible. In this way, the ground for agreement (and disagreement) becomes clear. As a starting point, we call up a Weberian notion of the state, as *the organization with legitimate final authority over a territory*. In the exercise of this authority, the state carries out certain *functions*, ranging from pure *control* (e.g., police) to pure *allocation* (e.g., welfare). Along this control-allocation continuum, at least five distinct functions can be arrayed, in order: *police power, regulation, administration, stabilization,* and *redistribution*. The realization of specific functions depends on *agents* (e.g., the bureaucracy, the Congress, the courts) which implement policies. In pursuit of these policies, the agents apply *instruments* and achieve certain *outcomes*. *Changed* policy instruments and *changed* policy outcomes represent shifts which can causally feedback upon the state organization. Significant shifts along critical functional dimensions tend to change the state organization, in the long run actually *transforming* it. In figure 1, a causal sketch summarizes this process of state transformation.

**Figure 1**

A Model of the State
The model in figure 1 seems to satisfy several criteria which state theorists have noted are important (Krasner 1984; Nettl 1968; Skocpol and Finegold 1982). First, the state can be seen as an autonomous actor. Put another way, the state may operate as an independent variable. For instance, a change in agents points to a change in policy. Second, rule and control functions, as well as the usual policy allocation functions, are considered. In other words, the punishment as well as the reward side of the modern state is incorporated. Third, the model postulates a longitudinal process. Thus, the historical dimension of state change is permitted full play. Fourth, the model allows for disequilibrium, as well as equilibrium. For example, conflict or shock might jolt the state agents, causing a jolt in policy. State change does not have to be harmonious in order to be detected. Fifth, the state, as it functions, is measurable. For instance, we can observe its varying regulatory activity over time. Because of such measurement, we can more rigorously test the notion of a New Era-New Deal transformation, a task to which we now turn.

HYPOTHESIS TESTING AND THE RESEARCH DESIGN

Was the state transformed from the New Era to the New Deal? An interrupted time-series (ITS) test design is used here to examine changes in the level and/or trend of a policy indicator, in response to the global independent variable of the New Era-New Deal shift (see Lewis-Beck 1986). We apply the design to annual policy observations from 1921–1939. An ITS procedure allows us to move beyond “eyeballing” of scatterplots, to the application of significance tests, which we suggest are useful despite the time-bound problem.

2The rough equation of the Hoover period and the New Era seems useful analytically. Hoover dominated Republican administrations from 1921 when he was appointed Secretary of Commerce.

3Significance tests may not appear needed because we have a population of observations, 1921–1939. However, in important respects, these observations still compose a sample. First, like all econometric time-series studies, the observations compose a finite sample from a temporal sequence. Further, these observations are no more than a sample from the entire population of public policies. In addition, this sample of policy variables is undoubtedly measured with some error. Fortunately, random measurement error in these dependent variables will not bias the parameter estimates. (The independent variables, as time counters in an ITS model, are measured without error; see the errors-in-variables discussion in Pindyck and Rubinfeld [1981, 176–77].) However, the presence of this measurement error may produce estimated parameter differences when none exist structurally. In this case, the application of a significance test allows us to rule out the possibility that observed differences might simply be due to such measurement error. More generally, observed differences in the parameter estimates might come from some unspecified random process. Then, nonsignificant results would suggest that the observed differences were generated by “chance” or “accident.” As Henkel (1976, 87) concluded, “the most general and consistently advanced supportive argument is that tests of significance are a means of guarding against according substantive importance to results which can easily be ex-
The actual statistical model for ITS can be conceived of as estimating the short-term and long-term changes in the trajectory of a dependent variable (here a measure on state policy), after an "interruption" (after 1932, with the change in administration). This equation captures these changes:

\[ S_t = b_0 + b_1X_{1t} + b_2X_{2t} + b_3X_{3t} + e_t \]  

where \( S_t \) = annual observations (from 1921–1939) on dimensions of state policy; \( X_{1t} - X_{3t} \) are dummies capturing the global New Era-New Deal administration shift as follows, \( X_{1t} \) = a counter variable for time from year 1 (1921) to year 19 (1939), \( X_{2t} \) = a dichotomous variable valued 0 for observations before 1933 and 1 for observations 1933 and after, \( X_{3t} \) = a counter variable of time scored 0 for observations before 1933 and 1 (at 1933), 2 (at 1934), \ldots 7 (at 1939); \( e_t \) = the error term; \( b_0, b_1, b_2, b_3 \) = the parameters to be estimated.

**Test Results**

The ITS methodology was applied to policy indicators on the five state functions, given in figure 1. Moving along this continuum, the identified functions are: to police, regulate, administrate, stabilize, and redistribute. For each of these functional areas, the measures located could be sorted into policy instruments or policy outcomes (Lowery 1985). In general, instruments are simply viewed as more specific actions taken to achieve a broader outcome, although these differences sometimes blur. More than 100 different policy variables were grouped according to their dimension, then subjected to test.

While the results are remarkably consistent across variables, certain ones appear especially representative. These estimates, presented according to their policy dimension, appear in table 1. The asterisked \( b_2 \) and \( b_3 \) coefficients suggest statistical significance at .05, in the usually expected direction of a positive New Deal effect.\(^4\)

The five functional areas, while not exhaustive, certainly are critical policy dimensions for the modern state in action. Further, it is important to emphasize that the interrupted time-series model provides a good statistical explanation for these variables, for across the 12 equations the average \( R^2 = .73 \). (The \( R^2 \) from the Cochrane-Orcutt type equations have been appropriately deflated.)

\(^4\)When the variables were logged, it was to overcome a nonlinearity problem. Estimation was ordinary least squares, unless significant first-autoregression was uncovered, in which case a Cochrane-Orcutt type procedure was generally applied. In those instances where the Durbin-Watson statistic fell into the uncertainty region ( 97 to 1.68, for \( N = 19 \) ), the OLS results were allowed to stand (Pindyck and Rubinfeld 1981, 181). This strategy, by avoiding an autocorrelation correction in the face of uncertainty, avoids the charge of biasing against New Deal effects.
TABLE 1
INTERRUPTED TIME-SERIES ESTIMATES,
NEW ERA-NEW DEAL EFFECTS ON STATE
(MODEL: \( St = b_0 + b_1X_{1t} + b_2X_{2t} + b_3X_{3t} + e_t \))

<table>
<thead>
<tr>
<th>State Functions</th>
<th>( \hat{b}_1 )</th>
<th>( \hat{b}_2 )</th>
<th>( \hat{b}_3 )</th>
<th>( R^2 )</th>
<th>D.-W.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Stabilization</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Real GNP Growth ((o))</td>
<td>.01</td>
<td>-.17</td>
<td>.03</td>
<td>.36</td>
<td>@</td>
</tr>
<tr>
<td></td>
<td>(1.2)</td>
<td>(-1.5)</td>
<td>(1.2)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Public Construction ((i))</td>
<td>.18</td>
<td>-.34</td>
<td>-.25*</td>
<td>.76</td>
<td>1.6</td>
</tr>
<tr>
<td></td>
<td>(6.4)</td>
<td>(-1.0)</td>
<td>(-3.6)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Regulation</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Mergers ((o))</td>
<td>26.6</td>
<td>-646.7*</td>
<td>-29.7</td>
<td>.53</td>
<td>.9</td>
</tr>
<tr>
<td></td>
<td>(1.2)</td>
<td>(-2.4)</td>
<td>(-.5)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>FTC ((i))</td>
<td>.05</td>
<td>-.18</td>
<td>.02</td>
<td>.82</td>
<td>1.4</td>
</tr>
<tr>
<td></td>
<td>(4.2)</td>
<td>(-1.3)</td>
<td>(.8)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Redistribution</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Tax Burden ((o))</td>
<td>-.3</td>
<td>5.0*</td>
<td>.3</td>
<td>.53</td>
<td>1.2</td>
</tr>
<tr>
<td></td>
<td>(-2.0)</td>
<td>(2.7)</td>
<td>(.8)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Farm Income ((o))</td>
<td>-89.3</td>
<td>-1,375.6</td>
<td>417.1</td>
<td>.17</td>
<td>1.0</td>
</tr>
<tr>
<td></td>
<td>(-.8)</td>
<td>(-1.0)</td>
<td>(1.5)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Employee Wages ((o))</td>
<td>13.6</td>
<td>-87.3*</td>
<td>4.7</td>
<td>.91</td>
<td>2.3</td>
</tr>
<tr>
<td></td>
<td>(10.0)</td>
<td>(-5.2)</td>
<td>(1.4)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Police Power</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Federal Prisoners ((o))</td>
<td>1,104</td>
<td>-4,038*</td>
<td>224</td>
<td>.92</td>
<td>2.0</td>
</tr>
<tr>
<td></td>
<td>(4.9)</td>
<td>(-3.1)</td>
<td>(.7)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Immigration Denied ((o))</td>
<td>-1,736</td>
<td>-3,220</td>
<td>2,146*</td>
<td>.90</td>
<td>1.8</td>
</tr>
<tr>
<td></td>
<td>(-7.6)</td>
<td>(-1.2)</td>
<td>(3.8)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Administration</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Government Growth ((o))</td>
<td>.03</td>
<td>-.07*</td>
<td>.01</td>
<td>.98</td>
<td>@</td>
</tr>
<tr>
<td></td>
<td>(15.7)</td>
<td>(-3.9)</td>
<td>(1.8)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Centralization ((i))</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>—in DC ((i))</td>
<td>-.00</td>
<td>.09</td>
<td>.09*</td>
<td>.89</td>
<td>1.2</td>
</tr>
<tr>
<td></td>
<td>(-.4)</td>
<td>(1.0)</td>
<td>(4.7)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>—outside DC ((i))</td>
<td>.01</td>
<td>.00</td>
<td>.06*</td>
<td>.97</td>
<td>1.2</td>
</tr>
<tr>
<td></td>
<td>(4.2)</td>
<td>(-.1)</td>
<td>(7.7)</td>
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Note: The letter "o" = policy outcome, the letter "i" = policy instrument. These variables are measured as follows: GNP Growth = annual spending at 1958 prices, logged to the base e; Public Construction = total federal public construction spending in 1957 dollars as a fraction of total federal revenue; Government Growth = total number of government employees (federal, state, local), logged to the base e; Centralization in DC = number of paid federal employees in Washington, logged to the base e; Centralization outside DC = number of paid federal employees outside Washington, logged to the base e; Mergers = annual mergers in manufacturing and mining; FTC = annual Federal Trade Commission appropriation, in thousands of dollars, logged to the base e; Tax Burden = the tax rate on incomes of more than $15,000; Employee
Stabilization. Consider first the goal of economic stabilization and efforts to achieve it with Keynesian-style infusions of public construction dollars (Tobin 1980). Evidently, FDR applied this key policy instrument even less than Hoover. In particular, take all types of real construction spending—on highways, harbors, military, sewer and water, conservation, smaller projects—lump them together, and treat them as a percentage of federal revenue. If Roosevelt is using this instrument package relatively more than Hoover, presumably to fight the Depression, then the ITS model should show a significant trend change. But it does not (see \( b_3 \)). Indeed, the coefficient is negative, suggesting that the Roosevelt administration might actually have been able to implement even fewer such policies. Further, the empirical picture does not really improve when the index is disaggregated, and analysis is conducted on the separate components.

Of course, while this policy set is complex, it is not exhaustive of available tools. Still other instruments, fiscal or monetary, might have been manipulated to achieve an improved economic outcome. What happens when one examines the macroeconomic results directly? Table 1 suggests that the New Deal efforts did not manage to accelerate significantly the real growth rate in GNP. Other macroeconomic indicators yield the same null result. An early economic study (Brown 1956) reached a similar conclusion. In sum, the findings suggest that fiscal and monetary manipulation under the New Deal had a net effect on the macroeconomy comparable to that under the New Era. This conclusion holds even with a control variable for the disastrous short-term effects of the economic crisis itself (see \( b_3 \)).

Regulation. What were the overall effects of the New Era-New Deal transition on the regulatory functioning of the state in the economic realm? A favored hypothesis does not clearly suggest itself. A general study of the impact of presidential partisanship on antitrust enforcement, covering the period 1895-1973, indicates no significant Republican-Democrat difference (Lewis-Beck 1979, 180). However, our analysis is for a different purpose and

Wages = real earnings in 1914 dollars; Farm Income = net income from farming; Federal Prisoners = the annual number of prisoners in federal institutions; Immigration Denied = the annual number of petitions for immigration denied.

The statistics are defined as follows: underneath the columns, respectively, of \( b_1 \), \( b_2 \), \( b_3 \) are the ITS parameter estimates (the intercept \( b_0 \) is not reported); below the parameter estimates are the \( t \)-ratios; \( * = a / t > 2.00 \), for \( b_2 \) or \( b_3 \); \( R^2 \) = the coefficient of multiple determination; D.-W. = the Durbin-Watson statistic, which is reported for the OLS equations; @ = estimation with SAS Autoreg, a Cochrane-Orcutt style procedure to correct for significant first order autocorrelation (thus no. D.-W.); \( N = 19 \), in general, except for the Cochrane-Orcutt style equations which necessarily have one less degree of freedom (also, the FTC variable has only 18 available observations, federal prisoners only 14).
focuses on a more historically bounded period. According to some, Coolidge and Hoover had a "hands-off" attitude toward the anticompetitive behavior of big business. In the words of economic critic Clair Wilcox (1975, 91), under these Republican administrations "industry enjoyed, to all intents and purposes, a moratorium from the Sherman Act. . . ." From that perspective, then, one might expect that cases against "conspiracies in restraint of trade" and "monopolization" would be more actively pursued by the Roosevelt administration. However, this notion must be balanced against FDR's support of the National Recovery Administration, which sponsored big business combination and collaboration.

Aggregate measures of anticompetitive behavior in the American economy are varied and controversial (Stigler 1957). One widely used measure is "mergers." Did the New Deal significantly alter any trend in anticompetitive behavior, as measured by aggregate numbers of corporate mergers in manufacturing and mining? No, on the basis of the ITS results. While there was an initial, significant short-term drop in mergers (see negatively significant \( b_2 \)), the long-term pattern in merger activity appears essentially unchanged (see \( b_3 \), with a t-value far from statistical significance).

This null finding repeats itself when antitrust policy is examined more directly. The principal means of enforcement for the nation's antitrust laws is the bringing of cases, by the Justice Department or the Federal Trade Commission. One general measure of agency enforcement activity, in some ways better than simple numbers of cases filed, is the actual dollars appropriated (Lewis-Beck 1979, 186; Posner 1970). Thus, in the ITS analysis of the FTC, the dependent variable is the annual budget. We observe no significant change in this indicator of antitrust enforcement after the intervention of the New Deal. In sum, state economic regulation, as measured by these global antitrust policy instrument and outcome variables, functioned with little change from one epoch to the next.

**Redistribution.** At the level of the mass public, the 1932 electoral realignment and formation of a "New Deal Coalition" has been rather well established (Andersen 1979; Burnham 1970; Clubb et al. 1980; Erikson and Tedin 1981). The coalition members might serve as new targets of government benefits, causing a redistribution of goods and services in their direction. Certainly, there is evidence that the Roosevelt administration targeted select federal programs in the states, so as to maximize their electoral impact (Wright 1974). Our next question is whether, under FDR, policies were implemented in favor of key groups in the New Deal Coalition and against groups clearly not in it.

Despite measurement difficulties, we have located useful indicators on two key groups in the New Deal Coalition—farmers and workers; and one key group outside—the upper income class. For farmers, we measure their
annual net income. One observes, in table 1, that net farm income did not rise significantly after FDR took office. The varied Roosevelt programs, such as the AAA and the related apparatus, apparently did not manage an overall increase in income to the farm community.

With respect to workers, we have the convenient dependent variable of real wages. The ITS analysis indicates the average real wage exhibited no significant positive shift from the New Era to the New Deal. The implication is not that FDR did "nothing" for workers. Such an assertion would have to ignore the Wagner Act. Rather, it suggests that Hoover was more pro-labor than commonly supposed (Zieger 1986), and that Roosevelt's support for workers was narrower than usually thought and perhaps exaggerated (Goldfield 1987).

What about punishing major groups not in the coalition, specifically the upper-income classes? We defined those with annual incomes of more than $15,000 to be this "upper class." The test question is whether this group experienced a higher tax rate under Roosevelt. The ITS estimates suggest otherwise, at least for the long run. That is, there was a temporary, short-term rate burst after FDR arrived in office (see the significant \( b_2 \)). However, after this initial shift, the pre-1932 rate structure reasserted itself (see the insignificant \( b_3 \)).

These findings, on in-groups and out-groups of the New Deal Coalition, suggest that FDR's policies did not succeed in redistributing state benefits and burdens. Of three important target groups—farmers, workers, and the upper class—none appears to have received any more (or less) from the state than they did during the New Era, at least along the critical variables examined here.

**Police Power.** All states "police" their citizenry. In some ways, exercise of the police power is the most basic of all state functions. Did the Roosevelt administration in its exercise of the police power yield a different overall "law-and-order" outcome? No one indicator of such a complex concept can be complete. However, a useful gross measure would seem to be the total number of prisoners in federal institutions. Did the legal apparatus of the national government under FDR incarcerate, for whatever reason, a greater or a lesser number of citizens? The answer is that, except for the temporary blip represented by \( b_2 \), not much changed. As can be seen from table 1, the post-1932 trend in the annual level of federal prisoners is not close to being significantly different from the pre-1932 period (see \( b_3 \)).

Throughout the time period under study, immigration was an item high on the political agenda (Leuchtenburg 1958, 205–209). Legal actions toward aliens did vary across the New Era and the New Deal. Were these systematic, with Roosevelt favoring immigration more than his predecessors? Apparently not, if number of "petitions for immigration denied" is examined in an
ITS equation. Indeed, under FDR the number of petitions denied actually increased significantly. Perhaps surprisingly, he appeared more willing to police the borders in this fashion than was Hoover.

Administration. A final critical function of the state is that of administration. The New Deal changed the administration of the state in countless ways, or so it has been argued (Higgs 1987, 194). We do not have measures on all, or even nearly all, of these possible ways. But two seem especially important, comprehensive, and measurable: centralization and size.

Let us first consider “size.” It is taken as axiomatic that the state grew in response to the New Deal. But is this true? To avoid a distorted picture of a changing public administration in the United States, it is necessary to examine all levels of state action. If only the national level is studied, inferences about overall New Deal effects will inevitably be biased. Suppose after the New Deal the national bureaucracy grows rapidly, but bureaucracy in the states shrinks correspondingly. Looking only at the national level of government organization, it would then appear public administration grew. But that inference would be faulty, for bureaucracy diminished correspondingly in the states. In this hypothetical example, net change in size of the public administration actually stands at zero.

What really happened? First, it is well-established that growth patterns at the various levels of American government were changing across the study period (Wallis 1985). After the New Deal, did these changes cancel each other out, leaving the overall trajectory of administrative growth unchanged from the New Era? In table 1 are the ITS estimates, using a dependent variable which counts bureaucrats at all levels of government.

These are fascinating results. Applying a conventional test ($t > 2.00$), the New Deal trend in bureaucratic growth is not significantly different from the New Era. Interpretations to the contrary would seem to depend, as much as anything, on a failure to consider the multiple-layers of public administration in the American system.

It is important to be clear about what occurred. Under the New Era, the total number of government employees increased at an annual rate of 3% (the interpretation of the $b_1$ coefficient made possible by the logging of the dependent variable). After 1932, this rate of administrative expansion continued more or less unchanged. FDR, and New Deal politics in general, did not really manage to accelerate (or decelerate) the ongoing trend in bureaucratic growth.

Now, turn to the issue of centralization. Did governmental activity become more centralized in Washington after the New Deal, as conventional wisdom would have it? A simple test of this question compares the growth rate of federal employees in Washington, DC, to those outside Washington,
DC. (Compare the $b_3$ coefficients from these variables in table 1.) We observe that there was significant growth in federal employees, both inside and outside Washington, after FDR came to power. However, the growth rate outside Washington is almost twice as significant statistically as inside Washington (e.g., dividing the t-ratios, $7.7/4.7 = 2$). The suggestion is that, while the federal government was growing, it was not necessarily becoming more centralized in DC.

A key question, for our purposes, is whether the growth was significantly faster inside or outside Washington. To help answer this, a confidence interval (95%, two-tail) can be constructed around the respective parameter estimates for $b_3$. For $b_3$ in the inside-DC-equation, the interval is .05 to .13 (i.e., .09 ± .04); for $b_3$ in the outside-DC-equation, the interval is .04 to .08 (i.e., .06 ± .02). Observe that the interval for the second equation essentially rests within the interval for the first equation. In other words, we cannot confidently say that the federal employees growth rates inside versus outside Washington are different. If they are not different, then centralization of state organization in Washington, so measured, did not really occur during the New Deal.5

5The pattern of almost no significant differences in table 1 does not change, even in the face of further economic controls. First, it must be recalled that the ITS variable $X_2$, a dichotomous 1932–1933 dummy, already imposes a strong control on the more immediate policy shock from the Depression economy. However, perhaps more subtle economic fluctuations need controlling as well. In that event, we included as control variables macroeconomic measures on annual GNP growth, inflation, wholesale prices, and employment. While the t-ratios of $b_2$ and $b_3$ shift some, as one would expect, the overall conclusions on significant New Era-New Deal differences remain unchanged.

These results speak directly to a controversy underlying our ITS approach: is it finally impossible to disentangle the effects of the FDR administration from the effects of the Great Depression? We think that we have largely untangled these effects for these reasons. First, empirically, as well as conceptually, the two are distinct. In the ITS design, four (1929–1932) of the 10 depression years are located in the pre-interruption, New Era period. Furthermore, in classic pre-test, posttest fashion, the New Era period (the 12 years from 1921) measures the policies before the interruption, thus providing the critical baseline of comparison to the Roosevelt years. With regard to these policy variables, our fundamental question is: do they look different under the New Deal, when compared to the New Era? Our essential conclusion is no. This indicates that, at least for these variables, the Roosevelt administration failed to change things.

A provocative criticism of this conclusion is the "suppressor effect" argument; i.e., FDR's efforts prevented the policy degeneration that the depression would have otherwise produced. In other words, the fact that there was no change (for the worse) in these policy measures implies that FDR's policies would have shown significant positive change, if it had not been for the drag caused by the depression. This suppressor argument can be tested by including in the model the suspected suppressor variables; i.e., by incorporating various economic measures of the depression into the ITS model. If the suppressor hypothesis is correct, then the ITS parameters would now indicate significance for the New Deal period. As already noted earlier, this significance fails to emerge, despite repeated experimentation with different economic indicators.
SUMMARY AND CONCLUSION

Did the New Era-New Deal transition transform the American state? In our interrupted time-series analysis of more than 100 policy variables, in the five functional areas, we found almost no statistically significant changes from the New Era to the New Deal. Regardless of the areas—police power, regulation, administration, stabilization, or redistribution—policy instruments and outcomes did not change much after Hoover. Of course, this does not mean that nothing changed. In the introduction, we noted a number of important changes, which need not be repeated here. Further, our measures of policy, while extensive, are not exhaustive. Thus, some change undoubtedly occurred in areas which eluded our analysis. Having issued these cautions, we cannot help but be impressed by the consistency of these null findings across such a wide spectrum of policy arenas.

This does not imply that FDR failed to try to change policy, for he certainly did. It does suggest that such efforts were largely unsuccessful. Why? Multiple reasons might be posed. A global one is the almost perfect coincidence of New Deal action with the existence of the Great Depression. It is possible to attribute the practical failure of these New Deal initiatives to the tremendous policy drag created by the enduring presence of economic hardship. Still, even after we risked the imposition of statistical controls on various relevant macroeconomic indicators, the null findings persisted.

One implication of our findings is that a great share of the supposed Hoover-Roosevelt differences rests on symbols rather than substance. In his classic study, Edelman (1964, 83) argued, "[The] dramaturgical difference between Roosevelt and Hoover was considerably more important politically than any difference in their actual attacks upon the depression." It should be noted that FDR’s much more successful use of symbols and rhetoric reflected his superior political and public relations skills rather than Hoover’s failure to attempt to mold public opinion (Leuchtenburg 1963, 42). Recall Hoover was the first president to designate an aide as his press secretary (Grossman and Kumar 1981, 22), and he held almost as many press conferences and made almost as many public addresses as did Roosevelt (Kernell 1986, 69, 86).

Another implication is that the New Era-New Deal transition wrought no tangible transformation of the American state. Not only, as already observed, did the policies not change. As well, and more subtly, the lack of policy change meant that the potential feedback to the state organization, perhaps leading to a long-run change, did not take place either. This suggests a final point. While we find very little policy change across the period under study, we do not infer the American state is necessarily static. What we do conclude is that the sources of most fundamental change in the American state lie outside the New Era-New Deal period. In the future, researchers on state
transformation should look to other epochs, as some have already done (Alston and Ferrie 1985; Bensel 1984; Metzer 1985; Skowronek 1982).

Manuscript submitted 9 August 1989
Final manuscript received 12 June 1990

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