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Welfare Caseload Change: An Alternative Approach

Lawrence M. Mead

In the last decade, caseloads in AFDC/TANF have shifted dramatically up, then down. Of existing studies based on time series or state panel data, some tend to underplay the role of welfare reform. All say little about what policies drove the decline or about the role of governmental quality. An approach using cross-sectional models explains interstate differences in caseload change rather than the national trend but allows more discussion about the role of policy and government. Results suggest that grant levels, work and child support requirements, and sanctions are important explainers of change, along with some demographic terms and unemployment. These policies in turn are tied to states' political opinion, political culture, and institutional capacity. Moralistic states seem the most capable of transforming welfare in the manner the public wants.

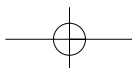
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Introduction

In this article, I suggest an alternative approach to explaining recent changes in welfare caseloads. Most existing research uses pooled or time series methods. These studies can account for change over time, but in some cases they come up with implausible findings. They also do not identify which specific policies caused change, and they ignore the role of governmental quality. These problems seem due largely to an inability to measure state policies accurately. I suggest a simpler, cross-sectional approach that gives up the time dimension but permits measuring policy and governmental influences more accurately. It leads to more plausible findings, specifies more policy details, and highlights the role of governmental quality in successful reform.¹

The national caseload of Aid to Families with Dependent Children (AFDC, since 1996 Temporary Assistance for Needy Families [TANF]) rose 34% between 1989 and 1994, then fell 56% through June 2000 (data from U.S. Administration for Children and Families). These are the sharpest changes in the history of the program. The fall predates the Personal Responsibility and Work Opportunity Reconciliation Act of 1996 (PRWORA), the federal law that recast AFDC as TANF, but PRWORA seems to have accelerated it.

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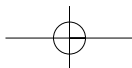


The research into the causes has been done mostly by economists using either time series or pooled analyses based on state panel data. These studies say something about the causes of change, but they pose several problems. Some authors admit that despite impressive statistical apparatus, they cannot explain the recent changes well (Blank, 1997; Wallace & Blank, 1999). Some also attribute the recent caseload changes mostly to economic conditions (Congressional Budget Office, 1993; Ziliak, Figlio, Davis, & Connelly, 1997; Bell, 2001). A phenomenon closely related to the caseload decline is that work levels among poor parents have risen sharply. In 1993, only 43% of poor female heads of family reported that they worked at all, but in 1999, 60% did so (U.S. Bureau of the Census, 1994–2000). One prominent study gives most of the credit to the Earned Income Tax Credit (EITC), a subsidy for the working poor that has risen recently (Meyer & Rosenbaum, 1999).

However, these findings conflict with what close observers of welfare reform say about the causes of change. Journalists tend to think welfare reform is at least as important as economic conditions in driving the rolls decline, and perhaps more so (e.g., DeParle, 1997). State and local officials carrying out the reform think they have mostly caused the decline, although they also credit the economy (Nathan & Gais, 2001). In a study that asked local Wisconsin officials about the reasons for caseload fall, welfare reform was mentioned most often, then the economy; no one mentioned the EITC (Mead, 1999, pp. 601–604). One reason to doubt that the economy is the leading cause of the decline is that tight labor markets in the late 1980s did not have the same effect on the rolls.²

Some economic studies credit the economy, welfare reform, and other factors more evenly (Bishop, 1998; Council of Economic Advisors, 1997, 1999; Ellwood, 1999), but then a further problem emerges: They cannot say exactly how government produced its effect. All the economic studies measure “welfare reform” using dummies that indicate whether states had “waivers” of normal federal welfare rules to run welfare reform experiments. These variables capture only crudely the pressures to work, to avoid welfare, and to pay child support that constitute welfare reform.

The research pays even less attention to the important role of governmental quality. Reform involves a difficult process of changing welfare from an income support for needy families to a more complex system that requires the adult recipients work in return for aid. Welfare must now not only pay aid to families but induct adults into work programs and provide them child care and other support services. Most states that have led the country in that change—Michigan, Minnesota, Oregon, and Wisconsin, for example—have strong good-government traditions. They are best able to decide contentious issues about how tough to be about enforcing work and then to implement the required programs. Other states with more conflicted politics and more troubled bureaucracies have had a much tougher time (Norris & Thompson, 1995; Author, 2002). But measures of governmental quality—however defined—play no role in any of the economic studies of change.



All these difficulties are ultimately due to measurement, or lack thereof. The fact that time series or panel analyses must specify explainers over time sharply limits the variables they can include. That is why few studies include many controls for the demographic features of caseloads. Especially, it is the reason these studies have to indicate welfare policy only approximately using waiver dummies. Meanwhile, economic determinants such as unemployment rates or the EITC are much better measured. That may be why the studies seem to privilege economic explanations while downplaying the role of policy and government.

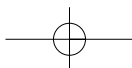
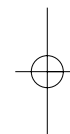
Approach

Fortunately, another, less noticed literature on caseload change suggests an alternative approach. Several papers model caseload changes using simpler cross-sectional analyses. Measures of change in caseload are regressed on various explainers with states as the units of analysis (Fein, 1996; Grady, 1999; Mead, 1999; Author, 2000). This approach has opposite strengths and weaknesses to those of the economic studies. By giving up a time dimension, these studies cannot, strictly speaking, speak to the causes of the national caseload trend. Rather, they model variations in caseload change among the states. At best, change over time is incorporated into the dependent terms and some of the explainers.⁵

But in return, these papers can measure many more of the determinants of change. They can specify demographic features of caseloads more easily. Above all, they can measure welfare policy more precisely, often using program data. They can disaggregate “welfare reform” into the many specific measures that it really consists of. And they can tap several measures of governmental quality. The findings do not apply to the same question as the economic studies, but they suggest much more about the forces that are probably driving change. They also suggest—though they cannot prove—that policy and governmental causes are the dominant causes of change.

One argument for the cross-sectional approach is that the principal variation in caseload change, and its causes, probably does lie across the states rather than across time. Although the caseload has come down sharply in virtually every state, the extent varies enormously state by state. All the possible determinants of change—welfare policy, economics, and the demographics of caseloads—vary much more sharply around the country than they have changed over time, at least during the last decade and a half, which has been the chief focus of inquiry. Thus, trying to explain caseload trends over time may not be worth the cost it imposes in underspecifying models. At the same time, my results are congruent with those of the more moderate economic studies, those that rank welfare reform equal to or more important than the economy in explaining change.

Initial hypotheses about which specific policies ought to influence caseloads come from the evaluation literature on welfare work programs. This suggests that the programs that raise employment and reduce dependency the most among adult recipients tend to enforce participation stringently and emphasize work in avail-



able jobs rather than education and training (Gueron & Pauly, 1991; Hamilton & Friedlander, 1989; Riccio, Friedlander, & Freedman, 1994; Freedman et al., 2000). There are also some statistical studies of program performance measures that suggest much the same: Work programs that work intensively with clients to raise participation and place people in jobs achieve the most job entries, among other outcomes (Chadwin, Mitchell, & Nightingale, 1981; Mead, 1988). One of the existing cross-sectional models of caseload change (Mead, 1999) also found that effective child support enforcement tended to depress caseloads, a factor neglected in most of the economic studies. The more families receive child support, the more single parents can live on this income rather than go on welfare.⁴

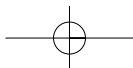
From all these studies, I hypothesize that caseload growth will be smaller, or caseload fall greater, in states that have the following features compared to the average state:

1. Low or falling welfare benefit levels, as these features reduce eligibility for aid.
2. High or rising participation levels in welfare work programs
3. High or rising assignment of clients in such programs to search for jobs or actually work, rather to place them in education or training
4. High or improving enforcement of child support
5. High levels of governmental quality
6. Caseload demographics tending to reduce dependency—less unwed pregnancy or greater employability
7. Favorable economic conditions, particularly low unemployment.

Theoretical guidance is insufficient to be more specific than this. I do not propose specific measures of these dimensions in advance of the analysis.

6 Another reason for that is limited degrees of freedom. While the economic models may be underspecified, their pooled structure gives them many observations, and they can easily estimate all possible determinants. In a cross-sectional structure, there are more possible measures, but there are only 50 observations with which to estimate them. The characteristic of limited degrees of freedom does not mean that the models lack power; they explain far more of the variation in the dependent term than the economic models. The problem is rather that there are many possible measures for each of the dimensions of change. There are even more measures of governmental quality, as noted below. In previous studies I have sometimes used factor analysis or 2SLS to combine terms.⁵

In this study, I simply tried the available measures of each of the above dimensions and retained those that were significant. Because of this fitting process, I limit what I claim for the results. It is the general model above that I set out in advance of the analysis, not the specific measures. So it is the general



model that is verified—mostly—rather than the specific results. Other results might be constructed with other measures that would look much the same and might be equally valid. In the results below, I show two models for each time period to suggest how the results evolved. The analysis is thus semiexploratory—guided by past research as to general structure but not as to the particular outcome. The point is more to suggest an alternative approach to explaining caseload change than to argue for a specific set of results.

I seek to explain two dependent terms: percent change in a state's AFDC/TANF caseload (measured in persons) over 1989–1994 and over 1994–1998. State caseloads generally rose in the first period and fell in the second. I do not model the change over the entire period, 1989–1998, because such a variable would net out much of the change up and down and leave little to explain. It is also reasonable to suppose that the determinants of change were different in the two periods, because the policy environment changed radically to favor reform. That supposition is confirmed below.

Although my main purpose is to suggest an alternative to the economic studies of caseload change, this analysis is also a contribution to implementation research. Political scientists have written valuable case studies about how states and localities have carried out reform (Nathan & Gais, 1999; Liebschutz, 2000; Weissert, 2000). This work suggests some of the same causes behind the reductions in caseloads as I list above, but it is entirely qualitative. No rigorous connection is made between policy features or changes and outcomes, either program performance measures or caseload changes. I suggest some of those connections here.

Caseload Change, 1989–1994

I first constructed the models for 1989–1994 shown in Table 1 (Author, 2000).⁷ The initial model was based on the above expectations and specifically on Mead (1999), a study of country-level caseload change in much the same period within Wisconsin.⁸

Results showed that states had higher caseloads if they added coverage for two-parent families, which was required under the terms of the Family Support Act. However, this expansive force was countered by enforcement pressures: States had lower caseloads if they had more welfare adults active in the Jobs Opportunities and Basic Skills (JOBS) program (the federally mandated welfare work structure in this period)⁶ and if they improved child support enforcement over 1989–1994. However, the percentage of JOBS clients in job search was unexpectedly positive, probably because it had become endogenous. In a period of rapid caseload growth, many states assigned more clients to job search, the cheapest activity in JOBS. The JOBS higher education variables and the initial child support level in 1989 were nonsignificant.⁷

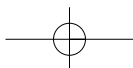
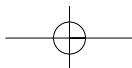


Table 1. Reduced Form Model of the Percent Change in AFDC Caseload (in Persons) in the American States, 1989–1994

	Initial model	Final model
Maximum AFDC grant (family of 3) in January 1989 (dollars)		0.060** (0.019)
State added Unemployed Parent coverage after fiscal 1989	14.8* (7.76)	12.3** (5.61)
32 Percent of welfare adults active in JOBS, fiscal 1991:	-0.654* (0.372)	-0.633** (0.209)
Change in percent of welfare adults active in JOBS, fiscal 1989–1994	-0.341 (0.435)	
Percent of JOBS clients assigned to job search, fiscal 1991	0.825** (0.278)	
Change in percent of JOBS participants in job search, fiscal 1991–1994		-1.18** (0.258)
Percent of JOBS clients assigned to higher education, fiscal 1991	0.383 (0.353)	
Change in percent of JOBS clients assigned to higher education, fiscal 1991–1994	0.442 (0.302)	
Percent of AFDC cases getting child support, fiscal 1989	-0.594 (0.362)	-0.706** (0.299)
Change in percent of AFDC cases getting child support, fiscal 1989–1994:	-0.869** (0.424)	
Score for implementing reinvention of government (0–4)		-1.53* (0.870)
Percent change in state population, 1989–1994		1.38** (0.503)
Percent of state population on AFDC in 1989	-11.3** (3.32)	-7.49** (2.32)
Percent of state households female-headed, 1990	11.5* (6.10)	
Average number children in female-headed families, 1990	-138.6** (44.1)	
Average AFDC case size (persons), fiscal 1989		-56.4** (20.0)
Percent of state population Black, 1990	-0.443 (0.409)	
Change in unemployment rate, 1989–1994	7.57** (2.81)	6.07** (2.05)
Constant	252.0** (72.9)	237.6** (57.9)
<i>N</i>	50	50
Adjusted R ²	0.67	0.76

Note: Data are unstandardized regression coefficients; standard errors are in parentheses. *Indicates significant at 0.10 or better, **at 0.05 or better. California is omitted from the initial model and the District of Columbia from the final model due to missing data. A statistical appendix giving description and sources of variables and statistical tests on the final model is available from the author.

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Of the demographic controls, the most important were the share of a state's population already on AFDC in 1989 (a high level here limited further increase), the share of state's households that were female-headed in 1989, and the average number of children in female-headed households. The latter is a proxy for the age of welfare mothers. Mothers who have more children are older, and thus there are fewer younger single mothers available to go on welfare. Other studies agree that rising unwed pregnancy helped drive the welfare boom (Blank, 1997, pp. 13–21; Congressional Budget Office, 1993). As expected, change was also positively tied to increases in unemployment.⁸

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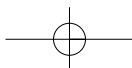
In the final model, I added the AFDC grant level (not a variable within Wisconsin) and dropped the nonsignificant JOBS terms. I also replaced the level of job search in 1991 with change in job search over 1991–1994, which was more independent.⁹ Now the level of child support enforcement, rather than the change over time, was significant, but the meaning is similar. I was able to add one significant government quality term: States that implemented “reinventing government” reforms more fully (e.g., performance measurement, decentralized decision making, a focus on consumer satisfaction), as measured by Brudney, Hebert, and Wright (1999), had less caseload rise (see further below).

Among the controls, I added increase in a state's population; it is positively tied to caseloads because more people means more recipients. I replaced number of children in female-headed families with average case size; The latter has a similar interpretation and is more valid because it pertains specifically to welfare.¹⁰ In the final model, neither a state's percent of female-headed households nor the Black percent of population was significant. Other measures of the racial or ethnic composition of the caseload or state population were also nonsignificant. This was apparently because the caseload rise was too sudden to associate well with the social features of states, which change much more slowly.

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The final model contains terms representing all seven of the causal dimensions hypothesized above. That is all I claim for it. A different model could easily have been constructed using different indicators, without much theoretical reason to prefer one over the other. At the same time, the results are robust. In basic structure, the final model is not greatly different from the initial one. The key policy dimensions—work and child support enforcement—carry over from one to the other. In the fitting process, these dimensions dominated throughout, along with change in unemployment. These terms were significant regardless of what other variables were in the model. That is consistent with the message of the more moderate economic studies—welfare reform and the economy together largely account for caseload change. The demographic dimension was much less important, and governmental quality least of all—at least in this reduced-form structure.

Perhaps because it is so broad based, the model is extremely strong, accounting for over three-quarters of cross-state variation in caseload change. Of course, the fitting process assures a stronger result than if I had tested just one set of specific measures chosen in advance.



Caseload Change, 1994–1998

I attempted to replicate this result for 1994–1998, when caseloads were falling, using the same variables or their closest equivalents. The first column of Table 2 shows the results.

The replication succeeded only in part. Grant levels and child support enforcement had much the same influence as before. The level of JOBS activity at the start of the period, however, was now positive. Like job search in the earlier model, it had apparently become endogenous; it reflected caseload levels rather than driving them down. Change in activity levels is negative as expected, however, paralleling change in job search levels in the earlier results. But both job search terms were now nonsignificant, as was the reinvention of government variable and all of the earlier demographic controls.

Most interesting, unemployment variables were still significant—but now with a negative sign. High or rising unemployment was now associated with *lower* rather than higher welfare rolls—the reverse of expectations. The likeliest reason is that unemployment, also, has become endogenous. Welfare decline had begun to drive joblessness, rather than the other way around. Lower welfare rolls meant more welfare recipients looking for work, producing a higher measured unemployment rate. This is the first evidence I have seen suggesting that the drive to enforce work in welfare may have a displacement effect on labor markets. I tried to instrument unemployment using other labor market terms (employment levels, labor force participation rates), so as to identify it net of backflow from the dependent term, but in vain. So I simply dropped these terms.

The determinants of caseload change differences were clearly distinct from those in the earlier period. However, I was able to replace the nonsignificant or invalid terms with others that sustained most of my hypotheses, if not all of them. In the final model, the grant level, change in JOBS activity, and child support terms resemble those in Table 1, confirming the idea that both benefits and enforcement pressures are key determinants of dependency.¹¹ But job search is replaced by the positive influence of assignment to higher education and the strong negative force of sanctions. States that told more clients to go to college had less caseload fall than others. This accords with the evaluations, mentioned above, showing that work programs oriented to education and training achieve smaller caseload reductions, at least in the short term, than those that stress “work first.”

Even more clearly, states that terminated recipients for noncooperation had more decline than those that did not. Sanctions were not measurable in the earlier period, but it is clear from other sources that states began to stress sanctioning only in the past several years (U.S. General Accounting Office, 1997, 2000). Relative to a reference category of states where the sanction only partially reduced the grant, states with a full-family sanction—when the case is closed completely—realized sizable caseload reductions. That was particularly true for states

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Table 2. Reduced Form Regression Model of the Percent Change in AFDC Caseload (in Persons) in the American States, 1994–1998

	Initial model	Final model
Maximum AFDC grant (family of 3) in January 1994 (dollars)	0.058** (0.014)	0.027* (0.014)
Percent of welfare adults active in JOBS, fiscal 1994	0.384* (0.226)	
Change in percent of welfare adults active in JOBS, fiscal 1994–1996	–0.970** (0.354)	–0.838** (0.294)
Percent of JOBS clients assigned to job search, fiscal 1994	0.355 (0.301)	
Change in percent of JOBS clients in job search, fiscal 1994–1998	–0.059 (0.295)	
Percent of JOBS clients assigned postsecondary education, fiscal 1994		0.369** (0.149)
State has delayed full-family sanction		–9.87** (3.33)
State has immediate full-family sanction		–15.9** (4.07)
Percent of AFDC cases getting child support, fiscal 1994	–0.585** (0.206)	–0.357** (0.132)
Score for implementing reinvention of government (0–4)	–0.712 (0.679)	
State's rank in legislative effectiveness (CCSL)		0.269** (0.115)
State is individualistic (Elazar)		5.38* (0.317)
Percent change in state population, 1994–1998	0.517 (0.530)	
Percent of state population on AFDC in 1994	2.96 (2.20)	
Average AFDC case size (in persons), fiscal 1994	–12.9 (16.0)	–30.4** (13.1)
Unemployment rate, 1994	–4.17* (2.06)	
Change in unemployment rate, 1994–1998	–4.81* (2.62)	
Constant	–2.48 (42.9)	38.1 (35.8)
<i>N</i>	49	49
Adjusted R ²	0.48	0.61

Note: See note to Table 1. Alaska and the District of Columbia are omitted due to missing data. Legislative effectiveness term is reversed so that higher values mean better rather than worse.

where the sanction was imposed immediately rather than delayed.¹² This result agrees with other research suggesting that sanctions have taken over much of the work-enforcing role earlier played by participation in work programs (Pavetti, 2002, pp. 247–254).

Two government quality terms are now significant. A state's legislative effectiveness (Citizens Conference on State Legislatures, 1971) *reduced* caseload fall, perhaps the reverse of expectations.¹³ The reason may be that welfare reform has generally been led by governors and welfare executives (Gais, 2000, pp. 174–178). Stronger legislatures may indicate weaker governors.¹⁴ The other governmental variable is a dummy for whether a state is individualistic in political culture (Elazar, 1984). It too is positive. According to Elazar, such states are more willing to tolerate individual preferences—here dependency—than states that are moralistic or traditionalistic.¹⁵

The only environmental explainer that is now significant is the same case size term as in 1989. A strenuous search for other demographic factors failed.¹⁶ Again, the change in caseloads was too sudden for state variation to be explained well by differences in social conditions.

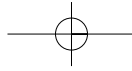
Again, this result is representative of many defensible models, not definitive. There is less continuity between the initial model and the final one, indicating less certainty about what is driving the results. Nevertheless, the first five of the seven causal dimensions that I hypothesized are represented. Only demographic explanation is weak, whereas economic forces are inexplicable or absent. The policy variables dominated throughout the fitting process, especially sanctions. Governmental quality terms were marginal at best. But despite the failure of most of the demographic and economic terms, the model still accounts for over three-fifths of the variation.

Government Quality

A limitation of the economic studies, mentioned at the outset, is that they neglect the important contribution of governmental quality to welfare reform. These reduced form specifications above show that some government quality factors can significantly affect caseloads even in direct competition with other explainers. That is, governmental features account for an important dimension of caseload change, regardless of what we assume the causal structure to be. It is plausible, however, that governmental quality affects welfare mainly via the policy variables. On that assumption, I produced structural versions of the caseload change models by withdrawing the government quality terms. A few other terms then became nonsignificant, and they too were dropped.¹⁷

When models are assembled partially on an exploratory basis, as was done here, results can easily reflect the idiosyncrasies of a particular run of data. The shift to the more frugal structural models offsets this. In the structural models, the coefficients of the remaining terms changed little from those shown in Tables 1 and 2, and the models remained potent. The remaining policy terms were those shown in Table 3.

I then ran correlations between these policy variables and the government quality variables, on the premise that the latter operate as background influences

**Table 3.** Policy Terms from Structural Models of Caseload Change**1989–1994**

Maximum AFDC grant (family of 3) in January 1989 (dollars)
 State added Unemployed Parent coverage after fiscal 1989
 Percent of welfare adults active in JOBS, fiscal 1991
 Change in percent of JOBS participants in job search, fiscal 1991–1994

1994–1998

Maximum AFDC grant (family of 3) in January 1994 (dollars)
 Change in percent of welfare adults active in JOBS, fiscal 1994–1996
 Percent of JOBS clients assigned postsecondary education, fiscal 1994
 State has delayed full-family sanction
 State has immediate full-family sanction
 Percent of AFDC cases getting child support, fiscal 1994

on policy. Tables 4 lists measures of political opinion, meaning views about public issues and political culture, which concerns a state's style of politics. The best known of these terms are the measures of state opinion developed by Erikson, Wright, and McIver (1993) and Berry, Ringquist, Fording, and Hanson (1998),¹¹ and the indicators of state political culture deriving from Daniel Elazar (1972)¹² and his later interpreters. Table 5 lists indicators of institutional quality, meaning the general modernization of government, and administrative quality, meaning the organization and resources of the bureaucracy. Most of these terms come from Barrett and Greene (1999); Bowman and Kearney (1988); and Brudney, Hebert, and Wright (1999).

This inquiry is more exploratory than the analyses above. Fewer expectations from past research suggest how these variables might affect welfare reform. It is merely apparent that states with good-government traditions are leading change. On the right-hand side of the tables, I indicate the links that I anticipate with caseload change. Generally, I expect the political opinion variables to associate positively. The scales of these terms run in a liberal direction, so higher levels on these scales should mean higher acceptance of dependency, and so should higher levels of Democratic party control or party competition (the Ranney variables). I expect the modernization and administrative quality terms to associate negatively, because capable government—in the sense of running programs tightly and efficiently—appears to be one of the forces restraining dependency.

The political culture variables are more complicated. I assume that states with more diverse or minority populations will favor higher welfare. But given the recent leadership of moralistic states in welfare reform, this type of culture—insistent on pursuing a public interest—must be seen as opposed to dependency. For these states, nonworking welfare affronts the public interest. Traditionalistic states, mainly in the South, should also oppose welfare, but in the name of inherited values.¹⁹ The individualistic culture probably promotes dependency, as this

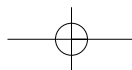
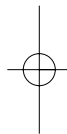
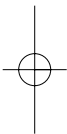


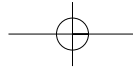
Table 4. Governmental Quality Explainers: Political Variables

Variable name	Variable label	Expected tie to caseload change
Political opinion		
partisan	State partisanship (Erikson, Wright, & McIver, 1993)	+
ideology	State ideology (Erikson, Wright, & McIver, 1993)	+
policlib	Policy liberalism (Erikson, Wright, & McIver, 1993)	+
govid	Government ideology (Berry et al., 1998)	+
dgovid	Change in government ideology (Berry et al., 1998)	+
citid	Citizen ideology (Berry et al., 1998)	+
dcitid	Change in citizen ideology (Berry et al., 1998)	+
ranney1	Ranney party control index (Bibby & Holbrook, 1996)	+
ranney2	Ranney party competition index (Bibby & Holbrook, 1996)	+
Political culture		
diverse	Social diversity index (Sullivan, 1973)	+
ethnic	Ethnic diversity index (Hero & Tolbert, 1996)	+
minority	Minority diversity index (Hero & Tolbert, 1996)	+
econdev	Economic development rank (Citizens Conference of State Legislatures, 1971)	+
elazarm	State is moralistic (Elazar, 1972)	-
moral1	Moralism index (Johnson, 1976)	-
moral2	Moralism index (Morgan & Watson, 1991)	-
civic	Civic culture score (Rice & Sumberg, 1997)	-
elazari	State is individualistic (Elazar, 1972)	+
indiv1	Individualism index (Johnson, 1976)	+
indiv2	Individualism index (Morgan & Watson, 1991)	+
elazart	State is traditionalistic (Elazar, 1972)	-
trad1	Traditionalism index (Johnson, 1976)	-
trad2	Traditionalism index (Morgan & Watson, 1991)	-
sharkan	Political culture score (Sharkansky, 1969)	?
south	State is in South	-
deep	State is in deep South	-

Note: For the 1989–1994 time period, govid and citid are measured in 1989, and dgovid and dcitid measure change over 1989–1994. For the 1994–1998 time period, govid and citid are measured in 1994; dgovid measures changes over 1994–1996, and dcitid measures change over 1994–1997, the last years available. Ranney measures cover years 1989–1994. For ease of presentation, variables expressed as ranks are reversed so that higher values mean better rather than worse. This also reverses their expected association with caseload change. South includes AL, AR, DE, DC, FL, GA, KY, LA, MD, MS, NC, OK, SC, TN, TX, VA, and WV. Deep South includes AL, AR, FL, GA, LA, MS, NC, SC, and VA.

mindset accepts various claimant groups getting what they can out of government. The positive finding in Table 2 supports this. But note that these expectations apply only to the overall effect on caseload change. Ties between state culture and specific welfare policies might be different.

The analysis is also exploratory in the sense that we seek associations without necessarily specifying a causal structure. Some aspect of political or institutional



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Table 5. Government Quality Explainers: Institutional Variables

Variable name	Variable label	Expected tie to caseload change
Modernization		
walker	Innovation score (Walker, 1969)	–
grayave	Overall average innovation rank (Gray, 1973)	–
graywel	Average welfare innovation rank (Gray, 1973)	–
grumm	Legislative professionalism score (Grumm, 1970)	–
legeffec	Legislative effectiveness rank (Citizens Conference of State Legislatures, 1971)	–
poldevel	Political development rank (Citizens Conference of State Legislatures, 1971)	–
account	Government accountability score (Bowman & Kearney, 1988)	–
represnt	Representation score (Bowman & Kearney, 1988)	–
Administrative quality		
exec	Executive centralization score (Bowman & Kearney, 1988)	–
avgrade	Overall public management grade (Barrett & Greene, 1999)	–
managav	Average of five public management grades (Barrett & Greene, 1999)	–
results	Managing for results grade (Barrett & Greene, 1999)	–
human	Human resources management grade (Barrett & Greene, 1999)	–
infotech	Information technology grade (Barrett & Greene, 1999)	–
reinvent	Reinvention of government score (Brudney, Hebert, & Wright, 1999)	–
reinvimp	Reinvention implementation score (Brudney, Hebert, & Wright, 1999)	–
statemps	Number of state government employees per 10,000 population (Morgan, Morgan, & Quitno, 1994)	–
locemps	Number of local government employees per 10,000 population (Morgan, Morgan, & Quitno, 1994)	–
staff	Staff/spending score (Bowman & Kearney, 1988)	–

Note: For ease of presentation, variables expressed as ranks are reversed so that higher values mean better rather than worse. This also reverses their expected association with caseload change. Avgrade is the overall grade given to the states by Barrett and Greene (1999); managav is the average of their five grades in five areas of public management.

background might influence welfare, not because one directly causes the other but because both features reflect the general politics, culture, or development of a state. I did not attempt to draw path diagrams linking certain government quality terms only with certain policy terms; theoretical guidance is insufficient to do this. Rather, I show all the associations. Although I am interested mainly in the associations with the policy terms from the structural models, I show also them for the dependent term (percent of caseload change) if a direct causal tie was assumed. I report correlation coefficients for associations that are significant at 0.10, otherwise only the signs.

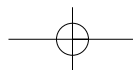


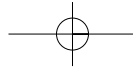
Table 6. Correlations of Governmental Quality Variables With the Dependent Term and Policy Explainers From Structural Model for 1989–1994: Political Variables

Variable name	% Δ in AFDC 1989–1994	AFDC grant 1989	State added UP	% adults active in JOBS 1991	Δ in % of JOBS in job search
Political opinion					
partisan	–	–0.36	+	–0.29	+
ideology	+	0.59	–0.64	–	–
policlib	–	0.78	–0.71	+	+
govid	–	0.35	–0.35	–	+
dgovid	0.24	–0.31	+	–	–
citid	–	0.53	–0.54	+	+
dcitid	+	–	–	–0.36	+
ranney1	–	–0.26	–	–	+
ranney2	+	0.35	–0.31	–	+
Political culture					
diverse	+	0.69	–0.44	+	–
ethnic	+	0.46	–0.38	+	–
minority	+	–0.40	+	–0.36	–
econdev	+	0.49	–0.44	–	–
elazarm	–	0.38	–	+	+
moral1	–	0.36	–	0.34	+
moral2	–	0.24	–	0.37	+
civic	–	0.67	–0.45	0.29	–
elazari	+	0.30	–0.38	+	+
indiv1	+	0.58	–0.46	+	+
indiv2	+	0.55	–0.53	–	–
elazart	+	–0.68	0.51	–	–
trad1	+	–0.67	0.42	–	–
trad2	+	–0.65	0.47	–0.25	+
sharkan	+	–0.77	0.38	–0.31	–
south	–	–0.62	0.31	–	–
deep	–	–0.50	0.32	–	–

Note: Correlations shown are significant at $p = 0.10$ or better. For other values, only signs are shown.

Tables 6 and 7 shows the correlations for the 1989–1994 structural model. Looking at the dependent term first, we find remarkably few ties to any of the governmental explainers. This confirms that the sharp jump in the caseload in this period was not a political act. No one willed it. The increase was driven largely by higher unwed pregnancy in the 1980s and higher unemployment in the 1990s, and it hit states of all politics and traditions. The rise varied across states according to the variables specified in Table 1, but only a few states escaped it entirely.²⁰

Turning to the policy terms, however, we see a very different picture. Higher AFDC grants are clearly linked to more liberal state politics, by most of the measures.²¹ They are also linked positively to social and ethnic diversity and to moral-



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Table 7. Correlations of Governmental Quality Variables With the dependent Term And Policy Explainers From Structural Model for 1989–1994: Institutional Variables

Variable name	% Δ in AFDC 1989–1994	AFDC grant 1989	State added UP	% adults active in JOBS 1991	Δ in % of JOBS in job search
Modernization					
walker	–	0.69	–0.46	+	+
grayave	+	0.66	–0.51	–	+
graywel	–	0.53	–0.38	+	+
grumm	–	0.27	–0.41	–	+
legeffec	–	0.46	–	+	+
poldevel	–	0.68	–0.53	+	+
account	–	0.40	–	+	–
represnt	0.36	0.44	–	+	–0.26
Administrative quality					
exec	+	0.30	–	+	+
avgrade	–0.24	–	–	+	–
managav	–0.24	–	–0.24	+	–
results	–	–	–0.25	–	–
human	–	–	–	–	–
infotech	–	–	–	+	+
reinvent	+	0.26	–0.26	+	–
reinvimp	–	–	–	+	–0.29
statemps	+	+	+	+	0.24
locemps	–0.24	–	+	0.27	–
staff	–	0.26	–0.25	–0.25	–

Note: Correlations shown are significant at p = 0.10 or better. For other values, only signs are shown.

istic or individualistic political cultures, but negatively to minority diversity and traditionalism (both of which probably denote the South). Grants are also tied positively to all the modernization measures and to a few of the administrative terms. In short, generous welfare is a feature of liberal and well-developed states. Turning next to adding Unemployment Parent coverage, the correlates here should be seen, not as causes of that policy decision (it was mandated under the Family Support Act) but rather of a preceding refusal to offer two-parent benefits. The associations are opposite to those just seen for benefits. Just as liberal, diverse, and developed states tend to be more generous with welfare benefits, more conservative and traditional ones tend to deny broad coverage.

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The percent of welfare adults active in JOBS shows a different pattern. Influencing this policy, political liberalism and moralistic political culture no longer cut in the same direction. Enforcing work is a conservative measure but nevertheless favored by states that are moralistic or civic. And although there are few significant ties to the institutional variables, most of the signs are positive. This confirms the predilection for well-run work programs that we see in states

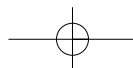


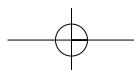
Table 8. Correlations of Governmental Quality Variables With the Dependent Term and Policy Explainers From Structural Model for 1994–1998: Political Variables

Variable name	% Δ in AFDC 1994–1998	AFDC grant 1994	Δ in % of adults active in JOBS	% JOBS ass higher ed 1994	Moderate sanctions	Strong sanctions	% of AFDC w/child support
Political opinion							
partisan	+	-0.36	-	-	-	-	-0.53
ideology	0.47	0.59	+	+	+	-0.38	-0.28
policlib	0.39	0.75	+	-	+	-0.36	-
govid	0.29	+	-	-	-	-	-
dgovid	+	+	-	+	+	-	-
citid	0.40	0.48	-	+	+	-0.41	-
dcitid	+	+	-	+	+	-	+
ranney1	+	-0.24	-	-	-	+	-0.37
ranney2	+	0.29	+	-	-	-	+
Political culture							
diverse	0.48	0.70	-	+	0.28	-0.49	-
ethnic	0.38	0.48	-	-	+	-0.24	-0.28
minority	+	-0.43	-	-	+	+	-0.51
econdev	0.35	0.46	+	-	+	-	-
elazarm	-	0.32	+	+	+	-	0.55
moral1	-	0.38	+	-	+	-	0.42
moral2	-	0.25	+	-	+	-	0.43
civic	+	0.64	+	+	+	-	0.34
elazari	0.38	0.33	-	-	+	-	-
indiv1	0.42	0.60	-	+	0.37	-0.50	-
indiv2	0.41	0.52	-	+	0.31	-0.39	-
elazart	-0.28	-0.65	-	-	-	0.34	-0.36
trad1	-0.28	-0.70	-	-	-0.30	0.47	-
trad2	-0.32	-0.63	-	-	-0.29	0.45	-
sharkan	-	-0.78	-	-	-	0.34	-0.43
south	-0.24	-0.63	-	-0.24	-	0.31	-0.29
deep	-0.27	-0.49	-	-	-	0.41	+

Note: Correlations shown are significant at $p = 0.10$ or better. For other values, only signs are shown.

such as Oregon or Wisconsin. For change in the percent of JOBS clients assigned to job search, there are few significant findings, but the pattern of signs does suggest a link to moralism and institutional development. Building up work programs appears to be the policy of states that want to restrain welfare, not by cutting aid but by promoting good behavior.

Tables 8 and 9 show the results for 1994–1998. Far more associations appear between caseload change and governmental antecedents than in the earlier period. The decline of welfare since 1994 definitely is a political act, or at least is affected by politics. The fall is less in the more liberal and individualistic states, and greater



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Table 9. Correlations of Governmental Quality Variables With the Dependent Term And Policy Explainers From Structural Model for 1994–1998: Institutional Variables

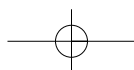
Variable name	% Δ in AFDC 1994–1998	AFDC grant 1994	Δ in % of adults active in JOBS	% JOBS assigned higher ed 1994	Moderate sanctions	Strong sanctions	% of AFDC w/child support
Moderniz							
walker	0.33	0.67	–	–	+	–0.34	–
grayave	+	0.65	+	+	+	–0.33	+
graywel	+	0.50	+	+	+	–	+
grumm	0.30	+	–	–0.25	+	–	–0.32
legeffec	0.30	0.45	+	+	+	–	–
poldevel	0.26	0.66	+	–	+	–0.35	+
account	+	0.38	+	–	–	–	+
represnt	+	0.43	–	0.29	+	–0.24	+
Admin quality							
exec	+	0.31	+	+	+	+	+
avgrade	–	–	+	–	–	+	+
managav	–	–	+	–	–	+	+
results	+	–	+	–	–	–	–
human	–0.31	–0.25	+	+	–	+	+
infotech	–	–	0.28	–	–	+	+
reinvent	+	+	+	–	+	–	–
reinvimp	–	–	0.34	–0.28	–	+	–
statemps	+	0.34	–	0.27	–	–	+
locemps	–0.30	–	+	–	–	0.27	0.27
staff	+	+	–	–0.27	–	–	–0.33

Note: Correlations shown are significant at $p = 0.10$ or better. For other values, only signs are shown.

in traditionalistic areas, including the South. For moralistic states the influence is less clear. Grant levels show much the same associations as in the earlier results.

Although few significant associations appear with change in the percent of adults active in JOBS, the pattern of signs again suggests that this is a conservative policy that, nevertheless, is favored by moralistic states rather than by states of the other cultures. There is also some association with modernization and administrative quality. For the percent of JOBS clients assigned to higher education, these linkages are less clear. The reason may be that enrolling recipients in JOBS but assigning them to college is politically ambiguous; it enforces activity aimed at self-reliance, but in a form that postpones an actual obligation to work.

As policies, the two sanction variables differ only in degree, but politically they emerge as virtual opposites. In the context of 1994–1998, when welfare politics everywhere was turning conservative, enforcing only a delayed full-family



sanction was a liberal policy. It was also favored by individualistic states and—to judge from the signs—moralistic ones, but not by traditionalistic states. Strong sanctions—an immediate full-family sanction—on the other hand was a conservative policy that was opposed in individualistic and, probably, moralistic areas but supported by traditionalistic states and the South. The signs in Table 9 suggest that moderate sanctions may be linked positively to modernization and negatively to administrative quality, whereas with strong sanctions the associations lean the other way.

The child support variable returns more strongly to the pattern seen with the level of JOBS activity in the 1989–1994 results. Getting child support paid on more child support cases is clearly a conservative policy. But it is favored by the moralistic states and not in the South. The reason may only be that the South has other, more direct ways of controlling welfare—low benefits and tough sanctions. Child support also shows some positive association with most of the institutional terms.

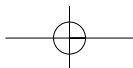
Discussion

14 The results largely sustain my hypotheses. All seven of the anticipated dimensions matter for explaining differences in caseload change among the states between 1989 and 1998, at least to some extent. The principle surprises were the inversion of the economic terms in 1994–1998, leading to their omission, and the weakness of demographic factors other than case size in both periods.

My findings are not inconsistent with the more moderate economic studies; but they reveal much more about the specific policies that are probably driving change, and they suggest some important ties to state politics and government. Because of incomplete theoretical guidance and multiple measures, there is some indeterminacy to the results. One could construct different models using these sources that might be just as persuasive. I contend only that, whatever the lineup, the same dimensions of causation would tend to emerge. There is no denying the influence of enforcement policies and governmental performance, in some form, on caseload change.

The models suggest that policy and government factors were nearly as important as economic or social differences in the first period and much more important in the second. In the final model in Table 1, policy terms explain 42% of the variation by themselves and the environmental terms by themselves 49%. But in the final model in Table 2, the policy terms account for 57% of the variation, the one environmental term for virtually none.²²

Whereas policy, government, and environmental factors are all important in both periods, there are some changes of emphasis. Policy becomes more important, and context less so, in 1994–1998 compared with 1989–1994. Enforcement pressures become the dominant force behind caseload fall, with low unemploy-

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ment secondary. Although unemployment did improve between 1994 and 1998, the rate fell nationwide only 1.2 points. Other studies agree that recent caseload decline goes well beyond what good economic conditions could explain (Council of Economic Advisors, 1999; Ellwood, 1999; Wallace & Blank, 1999).

Among the policy forces, benefit levels and the pressures to work and support families, stemming from JOBS and child support enforcement, are important in both periods. The main change is that sanctions become much more important in the period 1994–1998. It is also clear that more of the change from caseload growth to caseload decline is because of enforcement pressures than of benefits. States on average raised their benefits \$20 in nominal terms between 1989 and 1994. But in the first year after the passage of PRWORA in 1996, few changed their benefits at all (Gallagher et al., 1998, pp. 34–38).

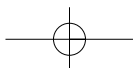
There also is variation among the states. They stressed, as it were, different policies from the menus listed in Tables 1 and 2. Three styles of welfare reform emerge from the government quality analysis, and they correspond to Elazar's prescient three cultures. Moralistic states tried to combine generosity with increased demands that recipients function in return for aid. They continued to pay high benefits, but they also toughened up work and child support enforcement. The goal was not so much to reduce caseloads but to *use* the welfare system to change the lifestyle of the poor.

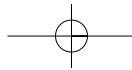
At the other extreme, traditionalistic states, mostly in the South, controlled dependency simply by keeping people off welfare, whether by paying low benefits, so that few families could qualify for aid, or by tough sanction policies. Individualistic states were more ambivalent. Their tradition was to pay welfare to satisfy a constituency, but they also faced impatience at growing rolls from the rest of the community. So they temporized, relying mainly on moderate sanctions to trim welfare. Not by accident, many of the urban states that reduced their rolls the least after 1994, for example Illinois, New Jersey, or New York, fall into Elazar's individualistic category.

To say that successful welfare reform is associated with good government, one must be willing to say which of these paths is preferable. We tend to identify the moralistic states with "good government," but does that hold for welfare reform? Is it best to reform welfare with a public purpose, to simply cut it, or to proceed incrementally by compromise? My own preference is for the first, and national polls make very clear that most Americans feel the same. What most voters want out of welfare reform is to require that adult recipients work and obey other civilities, yet for welfare to still aid families in clear need (Gilens, 1999, chaps. 2, 8). That is exactly what the moralistic states are trying to do.

The cross-sectional approach raises such issues just because it reveals more detail about what states have done, and how. We can ask not only why caseloads have fallen but whether welfare reform is a success or not in other terms.

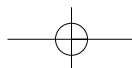
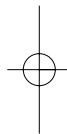
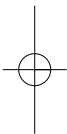
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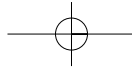




Notes

1. This research was supported by the Lynde and Harry Bradley, Annie E. Casey, and John M. Olin Foundations. I acknowledge valuable comments on earlier drafts from Michael Heaney, Paul Teske, David Autor, and confidential referees. The data files and diagnostic tests of the final regression models are available from the author.
2. Ziliak (2002) shows that caseload fall in the 1980s was greater in states with good economic conditions than those without. But that does not explain why the national trend did not follow the economy in that period, and the national trend is the subject of most of the recent studies.
3. One may regard my model as a two-period panel study in which the two points are the beginning and end years of the interval. I owe this insight to David Autor.
4. The only economic study to consider child support, finding a significant effect, is Huang, Garfinkel, and Waldfogel (2000).
5. Also in deference to limited N , I use an alpha level of 0.10 to assess significance rather than the more usual 0.05.
6. Note that JOBS first reported data in 1991.
7. In this and other models, explainers are positioned by preference at the beginning of the period studied. This minimizes endogeneity or the chance that the values of terms might be swayed by the caseload fall itself. In some cases, as with unemployment here, I included a change term when the level of the variable in 1989 was nonsignificant.
8. Among economic measures, I tried employment levels and labor force participation rates, but in the end, unemployment rates, or their change over time, proved most useful.
9. To test the independence of change in job search, I instrumented it (using the percent of AFDC adults judged mandatory for JOBS in 1991 and the percent of a state's federal JOBS allocation spent in 1991, and changes in both terms over 1991–1994) and reestimated using 2SLS. Results changed little.
10. The huge coefficient on case size shows how much caseload change would shift if case size rose by one person, but the range of this variable is only from 2.6 to 3.1. One might argue that case size term is endogenous, but I measure it in 1989, before the welfare boom begins. The caseload size term was also a more powerful explainer of caseload change than the percent of state households that were female-headed. The two terms correlated at 0.43.
11. To be sure that the change in JOBS activity term was not endogenous, I instrumented it using the percent of welfare adults mandatory for JOBS in 1994 and the proportion of their federal JOBS funds that states spent in 1994, and changes in both these terms over 1994–1996, then reestimated using 2SLS. The change in activity term now had a coefficient of -0.491 and a significance of 0.482. I decided to keep the term because little else changed and because it was implausible that the fall in caseloads was driving change in JOBS activity. Since caseload fall ushers the more employable recipients off the rolls first, it should, if anything, cause a decline in JOBS activity, implying a positive coefficient. But the mean for the change in activity was 4.5 points, showing that most states were in fact building up JOBS, as the law required them to do. It is thus more plausible that rising activity depressed caseloads than that falling caseloads drove up activity.
12. Admittedly, these sanction indicators are dummies for formal state policies—just the sort of variable that I criticize in the economic studies. However, they are more detailed than the waiver indicators in most of those studies.
13. Here and below, I reverse variables expressed as ranks so that higher, rather than lower, is better.
14. Here and below, I have been willing to use measures of governmental quality that are considerably out of date. One argument is simply that I know of no more recent measures. Another is that governmental features are much less volatile than welfare caseloads or even social conditions. They reflect enduring features of a state's political history and culture. So associations that show up with later events, including welfare reform, are probably valid. When I have multiple measures of the same feature, the findings tend to be similar for all, and this confirms the assumption I make.





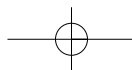
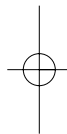
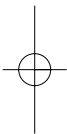
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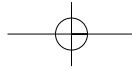
15. When substituted for the individualism measure, the measure for whether a state was moralistic neared significance ($p = 0.115$) and was negative, consistent with this interpretation.
16. The percentage of Black caseloads in 1994 was significant, and positive, when added to the reported model, but that version failed diagnostic terms. The coefficient suggested that each point increase in the Black share of caseloads reduced caseload fall by .131 points, a small effect.
17. In the model for 1989–1994, when I withdrew the reinvention of government term, one case had to be dropped to avoid heteroskedasticity. This made the variables for child support enforcement and the percent of population on AFDC in 1989 nonsignificant, so they too were dropped. For 1994–1998, when I withdrew legislative effectiveness and individualism, I had to omit a different case to avoid heteroskedasticity, and case size was omitted as nonsignificant. The revised models are available from the author.
18. Data used come from an update from the State Politics and Policy Data Archive at Florida State University, www.pubadm.fsu.edu/archives. This data covers 1960–1996 for government ideology and 1960–1997 for government ideology.
19. Since Sharkansky's version of Elazar links moralism and traditionalism, its expected tie to caseload change is unclear. In my results it operates largely as a measure of traditionalism. 16
20. The only states whose caseloads did not rise in this period were Arkansas, Louisiana, Mississippi, and Wisconsin.
21. Partisanship is inverse because many southern states are Democratic yet conservative. Democratic party control (ranney1) is also negative, probably for similar reasons.
22. These figures are for adjusted R^2 ; figures for unadjusted would be slightly higher.

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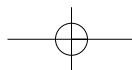
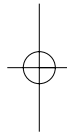
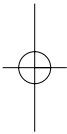


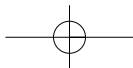
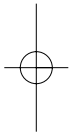
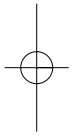
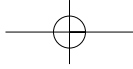
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