The Demand for Welfare Generosity*

By: David Ribar and Mark Wilhelm

David Ribar and Mark Wilhelm "The Demand for Welfare Generosity," *Review of Economics and Statistics*, 81:1 (February 1999), 96-108. doi:10.1162/003465399767923854

Abstract:

This paper estimates economic models of the determinants of state benefit levels in the Aid to Families with Dependent Children (AFDC) program using 1969-1992 data. These models have been extensively researched; however, the existing literature has produced an unacceptably wide range of estimates. Using alternative econometric procedures, this paper systematically examines both the specification assumptions underlying previous analyses as well as several additional specification issues. It is, therefore, able to replicate and reconcile estimates from previous studies and to provide updated, consensus estimates of the demand for welfare generosity. It finds that changes in the average level of income within states have small but statistically significant positive effects on benefits with the confidence bounds on the elasticity extending from 0.11 to 0.82. Changes in the effective price of redistribution are found to have, at most, weak negative effects with elasticities in the range of -0.14 to 0.02. These results are used to evaluate the effects of block grant provisions in the recently enacted welfare reform legislation.

Article:

I. Introduction

The controversy surrounding redistribution policy sustains considerable interest in the analysis of poverty relief. Until recently, the largest means-tested cash welfare program in the United States was Aid to Families with Dependent Children (AFDC). This program operated as a federal/state partnership with the federal government setting general guidelines and providing subsidies, but leaving the determination of benefit levels and most other day-to-day operational aspects to the states. Features of the AFDC program had been under sharp attack from politicians, analysts, and academics across the ideological spectrum for some time. This push for change culminated in 1996 with the enactment of sweeping reform legislation, the Personal Responsibility and Work Opportunity Reconciliation Act, which replaced AFDC with Temporary Assistance for Needy Families (TANF). TANF sets strict time limits on the receipt of benefits and increases work requirements among recipients. The relationship between the federal and state governments has also been fundamentally altered, with the new program relaxing some operating guidelines and changing the funding formula from a matching subsidy to a block grant.

The potential effects of these reforms raise numerous interesting questions and will without doubt be a major focus of policy research for years to come. One simple question, however, that economists should already be prepared to answer is: what specific effects will the change in the funding formula have on benefit levels.'? Recasting the state's benefit determination problem in a consumer-choice framework, the change from a

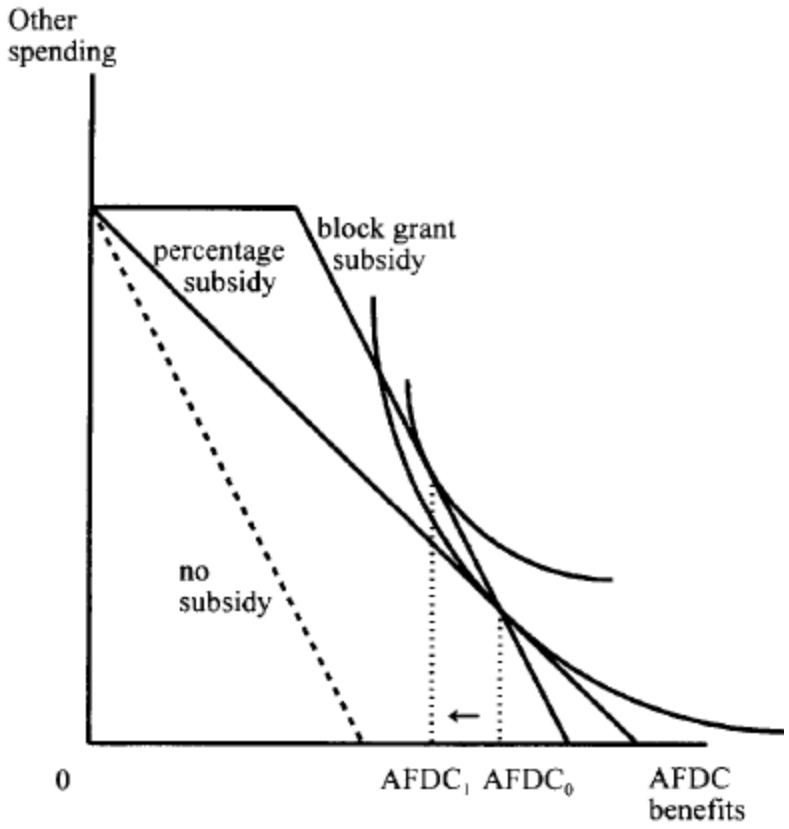
* An earlier version of this paper, "The Effects of Costs, Resources, Interstate and Interprogram Competition, and Redistributional Preferences on AFDC Expenditures," (Ribar and Wilhelm, 1994) was presented at the annual meetings of the American Economic Association, January 1994, in Boston. The authors thank Tammy Kolbe for expert research assistance and the Graduate Program in Policy Analysis at Penn State for financial support. The authors have received helpful comments from Joe Cordes, John Creedy, Ned Gramlich, Don Kenkel, Robert Plotnick, Jim Rodgers, Diane Rogers, Natalie Webb, workshop participants at Penn State and Carnegie Mellon University, and several anonymous referees. The authors also thank Lou Correra, Robert Moffitt, and Larry Orr for kindly providing various data.

matching subsidy to a block grant alters the state's budget constraint by both increasing its effective price of redistributing money to AFDC recipients and increasing its available income. Figure 1 shows the effect of a block grant equal in size to the state's original subsidy under the matching formula on the optimal level of AFDC benefits assuming no interactions with other assistance programs and no maintenance of effort requirement-essentially what has happened under TANF.(n1) As indicated in the graph, the policy change represents an income-compensated price increase with predictable negative effects on the demand for redistribution. Given suitable estimates of states' welfare-demand functions, predicting the impact of block grants on benefits is straightforward.

In fact, an extensive literature--dating back to Orr's (1976) pioneering study of AFDC expenditures as a public good---examines differences in the generosity of AFDC programs across states and over time. While these studies have considered additional issues including interactions among various types of public welfare programs such as Food Stamps and Medicaid, interactions between states in the determination of benefits, and residents' underlying preferences for redistribution, they have all also examined basic economic hypotheses involving the sensitivity of state AFDC benefits to changes in resources and costs. Unfortunately, these studies, which also differ in their data and econometric methodology, have produced widely varying results, and, to date, no systematic reconciliation of findings has been undertaken. Thus, as a practical matter, the existing results are not useful for evaluating the potential effects of the conversion from matching to block grants on benefit levels.

This study uses a long panel of state-level data to estimate these basic economic relationships. The analysis has several goals. First, it replicates and reconciles previous price and income results using a consistent set of data. In particular, the study examines whether the alternative fixed-effect methods employed to control for omitted-variables bias and instrumental-variable approaches used to control for endogeneity bias account for the differences in findings across studies. Second, the study performs a systematic analysis of these and other econometric specification issues such as instrument selection, variable measurement, structural shift, and functional form to obtain appropriate, defensible estimates of the demand parameters. Finally, the study uses its estimates to examine the likely impacts of the recent block grant legislation.

FIGURE 1.—EFFECT OF A BLOCK GRANT ON AFDC BENEFITS



From this analysis, the study is able to reproduce and reconcile the range of previous findings. It confirms that differences in methodology account for these results. As to issues of appropriate specification, it finds not only that the state-specific, fixed-effect controls that were used in some previous studies belong in the demand model, but that other types of fixed-effect controls belong as well. Interestingly, however, once these additional fixed-effect controls are included, endogeneity does not appear to be a significant problem. Estimates from regressions that withstand the various specification tests indicate that changes in income have modest positive effects on benefits levels with the confidence bound on the elasticity ranging from 0.11 to 0.82. Changes in price are found to have, at most, weak negative effects with implied elasticities no lower than -0.14. Accordingly, we predict that the change to block grants is likely to produce only a small decrease in the average level of benefits across states.

The remainder of the article is organized as follows. Section II briefly discusses the theoretical model and estimation methodologies used in previous studies of welfare expenditures. Findings from these studies are also summarized and evaluated in section II. Section III describes the longitudinal state-level data used in the analysis. Estimation results appear in section IV. The discussion in section IV includes comparisons with earlier findings as well as detailed specification analyses. Section V discusses the implications of our estimates for the newly enacted welfare reform legislation. Concluding remarks appear in section VI.

II. Theoretical and Empirical Models of Welfare Expenditures

A. Theory and Previous Findings

Most previous empirical research on AFDC generosity has been based on a median voter model of public expenditures.(n2) In this model, the median voter in state i is assumed to have preferences $U(c_i, a_i; Z_i)$ defined over the voter's own consumption, ci, and the consumption enjoyed by a representative AFDC recipient as a result of program benefits, a_i.(n3) Voter i's preferences are conditioned on a set of variables, Z_i, which might include demographic characteristics, attitudes, values, or other state-specific factors. The voter selects levels of c_i and a_i. subject to a budget constraint, $y_i = c_i + p_i a_i$, where y_i represents the voter's pretax income, and pi her

share of the cost of extending one additional dollar of benefits to each of the state's AFDC recipients. The price of benefits can be expressed

$$p_i = (1 - s_i)\frac{R_i}{N_i} \qquad (1)$$

where s_i is the portion of state expenditures matched by the federal government, R_i is the number of AFDC recipients, and N_i is the state's population. Because AFDC is federally subsidized and its costs are spread over all taxpayers, p_i is substantially less than one dollar.

Given these preferences and constraints, the voter has an optimal demand for benefits. Under the conditions of the median-voter theorem, this will also be the benefit level determined by majority voting. We write this optimal benefit level as

$$a_i^* = g(y_i, p_i; Z_i). \qquad (2)$$

The central concern of this study and much of the empirical literature on AFDC generosity is to estimate models like equation (2) to determine the sensitivity of benefits to changes in income and price.

TABLE 1.—SUMMARY OF PREVIOUS STATE-LEVEL WELFARE EXPENDITURE STUDIES

Study	Years (States)	Benefits	Price	Methodology	Price Elasticity	Income Elasticity
Orr (1976)	1963–67 (51) 1968–72 (51)	AFDC per recipient	R/N, (1 − s)	Lagged recipiency used as exog. proxy for recipiency Predicted state share	R/N: −0.12 (1 − s): −0.23	1963–67: 0.65 1968–72: 1.17
Orr (1979)	1975–76	AFDC per recipient	R/N, (1 − s)	Same as above	R/N: weaker than above (1 − s): stronger than above	Same as above (1968–72)
Hulten et al. (1982)	1969–80 (51)	AFDC per recipient	R/N, (1 − s)	Same as above	R/N: [−0.33, 0.10] (1 − s): [−0.42, 0.11]	[1.17, 2.09]
	1974–80 (50)	AFDC per recipient	R, (1 − s)	2SLS (Endogenous vars.: R, Food Stamps, and Medicaid expenditures)	R: −0.03 (1 − s): −0.14	1.68
Gramlich (1982)	1974–81 (33)	Max. AFDC less estimated income	(1 − s) adjusted for federal taxes	State-fixed effects; IV for Food Stamps; "neighbors" methodology using all other states	−0.957	1.02
Gramlich and Laren (1984)	1974–81 (33)	Same as above (Some models include Food Stamps)	(1 − s) adjusted for federal taxes	State-fixed effects; two-equation model of benefits and recipiency; "neighbor" methodology with various definitions of proximity	[−1.05, −1.24]	[−0.41, −0.64]
Moffitt (1984)	1971 (51)	AFDC per recipient	(R/N) × (1 − s)	MLE with nonlinear budget constraint	−0.15	1.57
Plotnick and Winters (1985)	1970–71 (48)	Combined Max. AFDC fam. 4, weighted Food, Stamps, and Medicaid	R/N, (1 − s) adjusted for FS benefit-reduction rate	Same as Orr (1976) plus LISREL modeling of political variables	similar to Orr (1976)	similar to Orr (1976)
Plotnick (1986)	Same as above	Combined Max. AFDC fam. 4, weighted Food Stamps	R/N, (1 − s) adjusted for FS benefit-reduction rate	Same as above	Same as above	Same as above
Moffitt (1990)	1960 (48)	Max. AFDC family of 4	(R/N) × (1 − s)	OLS with R lagged 3 years; forecast 1984 benefits using 1960 estimates	−0.17 (largest)	0.98
	1984 (48)	Combined Max. AFDC, Food Stamps, and Medicaid	(R/N) × (1 − s)	OLS with R lagged 3 years; backcast 1960 benefits using 1984 estimates	−0.12 (largest)	0.94
Shroder (1995)	1982–88 (51)	Max. AFDC family of 3 plus Food Stamps	R/N, (1 − s)	State-fixed effects; two-equation model of benefits and recipiency; "neighbor" methodology based on low-income migration	R/N: [−0.11, 0.12] (1 − s): [0.02, 0.58]	[−0.17, 0.39]
Ribar and Wilhelm (1996)	1988–91 (51)	AFDC per recipient	(R/N) × (1 − s)	State and time-fixed effects; IV for price; other redistributional preference controls	[−0.08, 0.20]	[−0.14, 0.46]
		Max. AFDC family of 3	(R/N) × (1 − s)	Same as above	[0.26, 0.31]	[0.54, 0.65]
Moffitt, Ribar and Wilhelm (1998)	1969–92 (50)	AFDC per recipient	(R/N) × (1 − s)	State- and time-fixed effects with independent variables lagged 1 year and controls for low-income wages; some regressions with state/trend effects; used alternative measures of median voter's characteristics	[−0.06, −0.05]	[−0.02, 0.22]

Table 1 summarizes the methodologies and results of twelve studies that have estimated state-level expenditure functions similar to equation (2). The studies differ in their definition of benefits, specification of price, econometric technique, and the particular states and time periods under study.(n4) Not surprisingly, these differences have led to substantial variation in the estimated price and income effects. Estimated price effects range from the negative and elastic results generated by Gramlich (1982) and Gramlich and Laren (1984) to the positive results reported by Shroder (1995), with the majority of studies reporting small and negative results. Income estimates also range from strongly negative (Gramlich &Laren, 1984) to strongly positive (Hulten, McCallum, Durman, & Michel, 1982). The studies, which are listed chronologically, display increasing technical sophistication and concern for various problems of misspecification. Unfortunately, this growing awareness of possible misspecification has not lead to convergence in results. We outline some of the fundamental specification issues below.

B. General Econometric Issues

Ordinary least squares (OLS) estimates of equation (2) will be biased if there are omitted preference or institutional factors that are correlated with the observed characteristics used to model benefits. For instance, if states with strong tastes for redistribution also tend to have high incomes, the effects of preferences on benefit levels, if not controlled for, will be erroneously attributed to income. The most common approach to reducing bias associated with omitted variables has been to include indirect controls, such as racial composition of the AFDC caseload or regional dummies, to proxy differences in voter and recipient characteristics across states. Estimates have been shown to be moderately sensitive to the inclusion of these controls (Orr, 1979; Hulten et al., 1982). A handful of studies (e.g., Gramlich, 1982; Gramlich & Laren, 1984; Moffitt, Ribar, & Wilhelm (1998), Ribar & Wilhelm, 1996; Shroder, 1995) have gone farther and included state- and in some cases time-specific fixed effects.(n5)As already noted, the results from several of these studies diverge from those in the rest of the literature as well as from each other.

In addition to omitted variables bias, OLS estimates of equation (2) may be biased due to endogeneity in the individual components of price, R_i/N_i *and* $(1- s_i)$. Endogeneity in the first component arises because of the direct effects of benefit guarantees and reduction rates on program eligibility and the incentive effects of benefits on recipiency and related behavior such as work, marriage, and fertility decisions. A simple model of recipiency can be expressed as

$$\frac{R_i}{N_i} = h(a_i, E_i), \qquad (3)$$

where E_i describes alternative economic and noneconomic opportunities for potential eligibles. Ignoring endogeneity may positively bias the estimate of recipiency's effect (through pi) on benefits in equation (2). To reduce endogeneity, some studies have used various lags of recipiency as proxies for the actual value in equation (2) and found that such respecification has little effect on the estimation results. A few other studies have used instrumental-variable methods (e.g., Gramlich, 1982; Gramlich & Laren, 1984; Ribar & Wilhelm, 1996; Shroder, 1995) and obtained much different price effects.

In the early data, the state financing share (1- si) may also be endogenous because, under the old AFDC reimbursement schedule, states simultaneously selected benefit levels and matching rates.(n6) An additional complication is multicollinearity caused by the matching rate's functional dependence on state per capita income.(n7)

C. Other Issues

Several studies--including Gramlich (1982), Gramlich and Laren (1984), Hulten et al. (1982), Moffitt (1990), Orr (1979), and Shroder (1995)--have suggested modifying the median voter's utility function and budget constraint to include other forms of public assistance (such as Food Stamps and Medicaid) that might be viewed as substitutes for AFDC benefits. Interprogram effects from additional forms of public assistance raise complex theoretical problems that are associated with modeling outcomes in multipleissue elections and which have not been addressed. There are econometric complications as well. First, there is the practical difficulty of defining a set of relevant alternatives. The literature's narrow categorization of other public assistance available to AFDC eligibles is justified on grounds of data availability and econometric tractability; yet, in principle, the definition could be expanded to include all social service, health, and education spending. Second, there is difficulty in finding structural variation in federally administered programs, like Food Stamps, that is independent of the variation in AFDC spending or other explanatory measures. In the case of Food Stamps, the basic regulations have been uniform across all states except Alaska and Hawaii since coverage became universal in 1975. Thus, subsequent structural variation in this program is virtually indistinguishable from a general time series. Third, if other forms of public assistance act as substitutes for AFDC, it is logical to expect that the reverse might also be true. Hence, benefits under these programs should be modeled as endogenous determinants of AFDC generosity with all of the attendant problems of identification.(n8)

Three studies (Gramlich, 1982; Gramlich & Laren, 1984; Shroder, 1995) also test hypotheses regarding interstate competition by specifying the vector of alternative opportunities, E_i, in equation (3) to incorporate benefit levels in neighboring states, an (i.e., include migration to and recipiency in other states as alternatives to recipiency in one's own state). These analyses have been motivated by variants of the "welfare magnet" hypothesis.(n9) The econometric complications associated with analyses of interstate competition are similar to those described for interprogram effects. With respect to definitions of relevant alternatives, theory provides little guidance in determining which states constitute the appropriate set of neighbors. As before, finding convincingly independent sources of variation is also a problem.(n10) Lastly, the possibility of reciprocal causality raises concerns regarding endogeneity bias.

While issues involving both interprogram and interstate competition are interesting and relevant to the ongoing policy debate, it is not clear whether either issue absolutely needs to be addressed to obtain useful estimates of price and income effects. Because these issues introduce so many additional econometric complications and because preliminary analyses indicated that our price and income estimates (which are the focus of the study) are not particularly sensitive to their being addressed, we do not directly examine interactions between AFDC and other programs or interactions between states.(n11)

III. Data

The empirical analysis uses longitudinal annual state-level data collected from a variety of sources. Short descriptions, sources, and simple statistics for the analysis variables are listed in table 2. The data extend from 1969 to 1992 and, thus, cover most of the time periods examined in previous studies. However, for reasons that are outlined below, our empirical investigation concentrates on the final eleven years of data. The earlier data are used in a few sensitivity analyses. All dollar amounts in the data set are converted to real (1987) values using the Personal Consumption Expenditure Deflator (PCD).

The analysis employs several alternative measures of welfare generosity. Our primary measure is the maximum monthly AFDC benefit for a family of four with no other income. While this measure does not capture all the dimensions of welfare generosity such as differences in payments by family size, reductions for recipients' own financial resources, and interactions with other assistance programs, it does represent a direct and easily interpreted policy outcome. Because of the shortcomings of the maximum-benefits variable, we also conduct sensitivity analyses using three other measures of welfare generosity: average monthly AFDC benefits per recipient, maximum combined AFDC and Food Stamp benefits for a family of four, and maximum combined AFDC, Food Stamps, and Medicaid for a family of four. The benefits-per-recipient measure is advantageous because it summarizes information about state policies and recipient characteristics into a single index of welfare generosity; however, unlike our other measures, it is not a direct policy outcome. The other two combined benefit measures incorporate interactions between the various major public welfare programs, and thus provide a more realistic description of the total benefits package available to poor families. The weakness of these measures is that neither is adjusted for recipient characteristics and both include a benefit (Food Stamps) that is not controlled by the states.

From equation (1), the price of AFDC benefits is the product of the state's financing share and its recipiency rate. The former depends on the federal matching rate, which, prior to 1983, the states selected from either the AFDC or FMAP schedules. We do not explicitly model the state's choice of schedule, which may lead to selectivity bias in some of our results from earlier years (Moffitt, 1984). Selectivity bias should not be a problem in the later data and should in any case be eliminated in the two-stage analyses where we instrument for price. Our recipiency variable for each year is computed by averaging the state's total number of AFDC recipients in July and December and dividing by the total population.(n12)

The income measure used in our analysis is annual per capita total personal income in each state. This measure, which is intended to serve as a proxy for the median voter's income, is adopted for reasons of comparability and data availability.(n13) Our analysis also includes several other state-and year-specific demographic and political

variables as controls. These variables, which are listed in table 2, all have straightforward interpretations as measures of either voter or recipient characteristics.

TABLE 2.—VARIABLE DEFINITIONS, SOURCES, AND DESCRIPTIVE STATISTICS

Variable	Definition [source]	1970–1981	1982–1992
AFDC4	Maximum AFDC for a family of four with no other income deflated by PCD [Characteristics of State Plans, Green Book, Social Security Bulletin]	551.857 (197.083)	412.655 (153.527)
AFDCPR	Total AFDC benefits divided by recipiency deflated by PCD [Social Security Bulletin]	126.387 (44.286)	110.030 (41.000)
AFDCFS4	Maximum combined AFDC and Food Stamp benefits for a family of four (=AFDC4 + FS Guarantee −0.3 Max (AFDC4 − standard deduction)) deflated by PCD [Characteristics of State Plans, Green Book, Social Security Bulletin, unpublished data from U.S. Food and Nutrition Service]	685.866[B] (123.571)	602.933 (107.330)
BENSUM4	Maximum combined AFDC and Food Stamp benefits plus average expenditures on Medicaid for a family of four (=AFDCFS4 + 0.368 Medicaid) [Medicaid figures come from unpublished data from U.S. Health Care Financing Administration and are deflated by the CPI for medical services]	768.025[B] (133.156)	685.643 (115.207)
Income	Per capita total annual personal income from Bureau of Economic Analysis Regional Economic Information System (REIS) deflated by PCD	12.043[A] (2.091)	14.519[A] (2.387)
State share	Data from Characteristics of State Plans for AFDC, Social Security Bulletin: Annual Statistical Supplement, Green Book, and unpublished tables from Administration for Children and Families	0.488 (0.234)	0.404 (0.100)
Recipiency	Recipiency data from Social Security Bulletin, Quarterly and Annual Public Assistance Statistics, and Green Book; population data from REIS	0.041 (0.015)	0.039 (0.014)
Price	State share × recipiency	0.020 (0.011)	0.016 (0.007)
Congressional A.D.A. rating	Annual index for entire House and Senate delegation [A.D.A. Today]	0.408 (0.219)	0.454 (0.218)
Proportion of pop. of African descent	[Decennial Census, Intercensal Estimates]	0.089 (0.091)	0.094 (0.091)
Proportion of pop. age 65 or older	[Decennial Census, Intercensal Estimates]	0.102 (0.021)	0.119 (0.022)
Proportion of pop. age 14 or younger	[Decennial Census, Intercensal Estimates]	0.263 (0.028)	0.224 (0.024)
Proportion of pop. with high school	Proportion of population over age 25 with high school [Decennial Census, intercensal figures interpolated]	0.597 (0.093)	0.734 (0.070)
Proportion of pop. with college	Proportion of population over age 25 with college degree [Decennial Census, intercensal figures interpolated]	0.130 (0.031)	0.186 (0.036)
Sex ratio	Ratio of men 15–54 to women 15–44 [Decennial Census, Intercensal Estimates]	1.234 (0.046)	1.206 (0.038)
Female unemployment rate	[Geographic Profile of Employment and Unemployment]	6.066[C] (1.735)	6.858 (2.291)
Retail earnings	Annual average wage and salary earnings of retail employees deflated by PCD [REIS]	14.070[A] (1.748)	12.497[A] (1.590)
Observations		600	550

Notes: Data from the fifty states. Unless otherwise noted, figures represent means and (standard deviations). All variables other than benefits lagged one year (i.e., measured from 1969–1980 or 1981–1991).
[A] Amounts in thousands of dollars.
[B] Figures for years 1978–1981.
[C] Figures for years 1976–1980.

As mentioned, most of our analyses are based on the last eleven years (1982 to 1992) of data. The principal reason for splitting our sample is that tests for structural shift (discussed in the next section) indicated that it was inappropriate to pool the data. We focus on the newer observations because of our interest in applying our findings to an analysis of the TANF provisions, but we acknowledge that the older data would have been more useful in replicating some of the older studies. The start of the selected period corresponds to the first year following the enactment of the Omnibus Budget Reconciliation Act (OBRA) of 1981. Among other things, OBRA dramatically cut welfare eligibility, decreased voluntary work incentives for welfare recipients, and allowed states to impose work requirements (workfare) on recipients through Community Work Experience Programs; Robins (1990) reports evidence that these changes substantially altered the behavior of welfare recipients. The start year also corresponds to a natural break in the periods considered by previous studies; only two of the studies listed in Table 1--Moffitt (1990) and Moffitt et al. (1998)--incorporate both pre- and post-OBRA data. Lastly, the start date reflects the point where almost all states were using the FMAP subsidy formula.(n14)

IV. Results

Tables 3.1 and 3.2 list estimates and specification test results from an initial series of longitudinal state-level benefit regressions that use a consistent set of 1982-1992 data and incorporate alternative features of the analyses described in table 1. Although they are not exact replications, the regressions do illustrate how

different specification assumptions can explain the variety of results reported in the literature. The first five regressions use the logarithm of the maximum AFDC benefit for a family of four as a dependent variable, while the last two use the log of the maximum combined AFDC and Food Stamp amount. All of the models include logarithms of the price and income variables as regressors as well as a consistent set of other demographic and political controls. Because of the double-log specification, the coefficients on price and income in each model can be directly interpreted as elasticities. The independent variables are all lagged one year to account for the presumed short delay between decisions regarding welfare policy and their implementation.

TABLE 3.1.—ALTERNATIVE SPECIFICATIONS OF THE DETERMINANTS OF WELFARE GENEROSITY

Model	Dependent Variable	Price	Income	State Effects	Time Effects	Estimation Method/Instruments
3A	AFDC4	0.260***	0.593***	no	no	OLS
		(0.028)	(0.133)			
3B	AFDC4	−0.022	0.357***	yes	no	OLS
		(0.022)	(0.096)			
3C	AFDC4	−1.752	−1.592	yes	no	2SLS/average benefits in all other states
		(1.247)	(1.440)			
3D	AFDC4	3.522**	5.784**	yes	yes	2SLS/average benefits in all other states
		(1.572)	(2.434)			
3E	AFDC4	−0.232***	0.120	yes	no	2SLS/sex ratio, female unemployment, retail income, average benefits, sex ratio, female unemployment, and retail income in contiguous states
		(0.061)	(0.117)			
3F	AFDCFS4	0.027	0.298***	yes	no	2SLS/same as 3E
		(0.023)	(0.047)			
3G	AFDCFS4	−0.027	0.182***	yes	yes	2SLS/same as 3E
		(0.025)	(0.056)			

Notes: Regressions use 1982–1992 data from the fifty states. Dependent variables, price, and income variables are expressed in logarithms. Controls for the A.D.A. ranking and proportions of the population who are black, under age 15, over age 64, over age 24 with high school, and over age 24 with college are included in all regressions but not reported. Standard errors appear in parentheses.

* Significant at 0.10 level.

** Significant at 0.05 level.

*** Significant at 0.01 level.

Following the methodology of the earliest studies, the first specification in table 3.1 is an OLS regression in which the longitudinal data for each state are pooled and treated as independent observations. As with the initial empirical studies, per capita income is estimated to have a strong positive effect on AFDC benefits. However, unlike those studies, the income response is less than unit elastic, and the price variable is estimated to have a significant positive effect. While the specifications do not reproduce the findings of the earliest studies which used pre-OBRA data, the results are similar to OLS estimates reported by Ribar and Wilhelm (1996), which used 1988-1991 post-OBRA data. Regressions (not shown) estimated with pre-OBRA data do replicate the initial studies' findings.

The second specification adds state-fixed effects to the model. The state controls are jointly significant, and their addition noticeably decreases the coefficient estimates for income and price. Though small and statistically insignificant, the coefficient on price becomes negative. The third model in table 3.1 contains the same observed controls as the second; however, the price measure is treated as an endogenous variable and is instrumented using the average benefit level in all other states. The specification of this model--in particular, its use of state-fixed effects and choice of instrument--resembles the procedure employed by Gramlich (1982). Like that study, the third specification produces a large negative price elasticity and, like the subsequent study by Gramlich and Laren (1984), a large negative income elasticity.

In the next row, we examine the sensitivity of the third specification to an issue not considered in the studies by Gramlich (1982) and Gramlich and Laren (1984)--the inclusion of general time effects--and see that the price and income estimates are sensitive to the point of the coefficients actually being reversed. Model 3E examines the sensitivity of the estimates from the third specification to an alternative set of instruments for price. It adopts a set of instruments that is similar to the set used by Shroder (1995) (i.e., uses the sex ratio, female unemployment rate, retail wage, and averages of the benefit level, sex ratio, female unemployment rate, and retail wage in contiguous states to predict the price variable). Once again, the results from the third model are found to be sensitive to alternative specification assumptions.

The sixth specification replaces the dependent variable from Model 3E with the maximum combined AFDC and Food Stamp amount. This combined measure is closer to the outcome examined by Shroder (1995). Because the Food Stamp Program effectively taxes AFDC benefits (after a standard disregard, Food Stamp awards are reduced thirty cents for each dollar of AFDC income), there is less variability in combined benefits across states than in AFDC benefits alone. Relative to the estimates from Model 3E, the price coefficient in this specification is closer to zero while the income coefficient is more strongly positive. The final specification adds a set of general time effects to the previous model; estimates reveal that the price and income coefficients are moderately sensitive to this respecification.

The results from table 3.1 are consistent with the range of estimates from the empirical literature and fairly well replicate the specific findings of the most recent studies. They also indicate that estimates of the effects of price and income on welfare generosity are sensitive to alternative fixed-effect controls for unobserved heterogeneity, procedures to account for the possible endogeneity of the price variable, and the specification of the dependent variable. Figures from table 3.2 further reveal that the specification test results themselves are sensitive to alternative specification issues. Overall, the results demonstrate that a more careful and methodical analysis of these specification issues is warranted.

TABLE 3.2.—SELECTED SPECIFICATION TESTS

Specifications	Null Hypothesis	p Value
3B vs. 3A	State effects are jointly insignificant	<0.001
3C vs. 3B	Price variable is exogenous	0.165
3D vs. 3C	Time effects are jointly insignificant	0.748
3E vs. 3B	Price variable is exogenous	<0.001
3G vs. 3F	Time effects are jointly insignificant	<0.001

For our formal sensitivity analysis, we take specification 3B from table 3.1 (the model with state-fixed effects) as our point of departure. In the first column of table 4.1, we report estimates from a regression which adds a general set of time effects to this model. The time effects are jointly significant, and adding these variables leads to modest changes in the estimated price and income coefficients.

TABLE 4.1.—REGRESSION ANALYSIS OF DETERMINANTS OF WELFARE GENEROSITY

Dependent Variable:	AFDC4			AFDCPR	AFDCFS4	BENSUM4
Estimation Method:	OLS	OLS	2SLS	OLS	OLS	OLS
Model:	4A	4B	4C	4D	4E	4F
Price	−0.007	−0.021	0.022	−0.035	0.010	0.001
	(0.022)	(0.021)	(0.047)	(0.023)	(0.009)	(0.011)
Income	0.521***	0.346***	0.415***	0.390***	0.237***	0.231***
	(0.102)	(0.122)	(0.129)	(0.131)	(0.053)	(0.063)
A.D.A. ranking	−0.004	0.017	0.013	0.027	0.002	0.005
	(0.034)	(0.028)	(0.025)	(0.030)	(0.012)	(0.014)
Proportion black	−4.820***	−9.025***	−9.445***	−3.401	−1.994*	−1.007
	(1.459)	(2.701)	(2.446)	(2.908)	(1.167)	(1.393)
Proportion age 65 or older	−2.277*	−6.506***	−7.455***	−7.920***	−2.676***	−3.081***
	(1.274)	(2.140)	(2.138)	(2.304)	(0.925)	(1.104)
Proportion age 14 or younger	1.276	−2.668**	−3.268**	−1.619	−1.831***	−2.133***
	(0.991)	(1.292)	(1.304)	(1.391)	(0.559)	(0.667)
Proportion with high school	0.016	3.566**	3.842***	1.847	2.406***	3.295***
	(0.356)	(1.610)	(1.462)	(1.733)	(0.696)	(0.830)
Proportion with college	0.423	−2.274	−2.232	−2.328	−0.433	−1.056
	(0.846)	(3.138)	(2.798)	(3.379)	(1.356)	(1.618)
State effects	yes	yes	yes	yes	yes	yes
Time effects	yes	yes	yes	yes	yes	yes
State/trend interactions	no	yes	yes	yes	yes	yes
R^2	0.982	0.993	—	0.992	0.993	0.989

Notes: Regressions use 1982–1992 data from the fifty states. Dependent variables, price, and income variables are expressed in logarithms. 2SLS regressions use sex ratio, female unemployment rate, and state (Medicaid) financing share as instruments. (See appendix A.) Standard errors appear in parentheses.

* Significant at 0.10 level.

** Significant at 0.05 level.

*** Significant at 0.01 level.

As an additional control for unobserved heterogeneity, the next specification in table 4.1 includes variables for state-specific linear trends (i.e., adds interactions of state dummies and linear time-trend variables to the existing controls for state- and year-specific effects). These variables account for slowly changing demographic, institutional, and preference factors within states, which may be jointly correlated with welfare generosity and its other observed determinants. Like the time effects, the state-specific trend variables are jointly significant, and, once again, their inclusion has modest effects on the estimated price and income results.

The third regression in table 4.1 adopts the parameterization of the previous model but uses two-stage least squares and exclusion restrictions on the sex ratio, female unemployment rate, and state financing share to account for the possible endogeneity of price. The sex ratio and unemployment rate are included to proxy women's marriage and work opportunities, respectively, and are assumed to affect voters' behavior only indirectly through their effects on recipiency. Similarly, the financing share is assumed to have no independent effect on benefits other than an indirect effect through the price variable. (We subsequently test this assumption.) First-stage results for the price regression (shown in appendix A) indicate that the sex ratio and financing share are solid individual predictors, while female unemployment is weaker; the three variables are also jointly significant. The second-stage results reveal that--conditional on the inclusion of the state, time, and state/trend effects--instrumenting price has little effect on the coefficient estimates. Thus, the null hypothesis that price is exogenous cannot be rejected, and Model 4B emerges as our preferred specification.

TABLE 4.2.—SPECIFICATION TEST RESULTS

Null Hypothesis	Dependent Variable:	p Value			
		AFDC4	AFDCPR	AFDCFS4	BENSUM4
Time effects are jointly insignificant		<0.001	<0.001	<0.001	<0.001
State/trend interactions are jointly insignificant		<0.001	<0.001	<0.001	<0.001
Price variable is exogenous		0.313	0.310	0.580	0.608

The sensitivity analyses for the inclusion of the time effects and state/trend variables and the endogeneity of price are repeated using the three other measures of welfare generosity. As in the previous analyses, the specification tests indicate that the time effects and state/trend variables should be included in the model and that the price variable can be treated as being exogenous. Results for specifications corresponding to Model 4B but using the other dependent variables are presented in the last three columns of table 4.1. The models all indicate that income has a small positive effect on welfare benefits and price has a negligible effect.

Table 5 lists estimates and test results from two-stage least squares (2SLS), maximum-benefit models that incorporate alternative exclusion restrictions to identify the effect of price. We use the results from these regressions to examine whether

(i) our price and income estimates are sensitive to alternative identification restrictions,
(ii) the restrictions in Model 4C from the previous table or additional restrictions similar to those used in previous studies lead to overidentification, and
(iii) our conclusions regarding the exogeneity of price are sensitive to alternative identification conditions.

The first row in table 5 displays the price and income estimates and exogeneity test results from Model 4C as well as test results for whether the exclusion restrictions on either the financing share or the sex ratio and female

unemployment rate lead to overidentification. Neither of these latter tests rejects the identifying assumptions of Model 4C.(n15) The second and third rows in table 5 display results from specifications that drop the financing share (Model 5A) and sex ratio and female unemployment rate (Model 5B) as identifying variables. A comparison of the results across rows reveals that including the financing share as an instrument reduces the price and income coefficient estimates and greatly reduces the associated standard errors.(n16)

The fourth row reports results from a 2SLS regression that adds retail income to the set of identifying variables from specification 4C. Along with female unemployment, retail income captures alternative economic opportunities among low-skill women who are potential AFDC recipients; Shroder (1995) included a similar low-skill wage variable in his set of instruments. Adding retail income as an instrument does not substantially alter the price and income coefficient estimates, their standard errors, or the exogeneity test results. It does, however, lead to overidentification.(n17)

In the fifth row, a variable for the average benefits in all other states is added to the other identifying variables from model 4C. The benefit level in other states was used by Gramlich (1982) as a measure of the incentives for welfare-induced migration across states. Adding this variable leads to slightly higher estimates of the price and income effects and a rejection of the exogeneity assumption for price, albeit based on a small and incorrectly signed coefficient. Like the addition of the retail income variable, it also leads to overidentification. Model 5E in the sixth row uses benefit levels and other characteristics of contiguous neighboring states as instruments; these variables are similar to the measures that Shroder (1995) employed to describe the migration incentives for low-income families. The use of these measures also leads to overidentification but no substantive changes in the estimates from Model 4C.

TABLE 5.—2SLS REGRESSIONS USING ALTERNATIVE INSTRUMENTS FOR PRICE

Model	Instruments	Coefficient Estimates: Price	Coefficient Estimates: Income	Test of Over-Identification Variable(s)	p Value	Exogeneity Test p Value
4C	Sex ratio, female unemployment, financing share	0.022 (0.047)	0.415*** (0.129)	i. financing share ii. sex ratio, female unemployment	0.303 0.274	0.313
5A	Sex ratio, female unemployment	0.167 (0.119)	0.647*** (0.222)	—	—	0.111
5B	Financing share	0.001 (0.051)	0.381*** (0.132)	—	—	0.670
5C	Sex ratio, female unemployment, financing share, retail income	−0.008 (0.045)	0.366*** (0.126)	retail income	0.054	0.749
5D	Sex ratio, female unemployment, financing share, average benefits in all other states	0.065 (0.048)	0.483*** (0.131)	average benefits in all other states	<0.001	0.044
5E	Sex ratio, female unemployment, financing share, average benefits, sex ratio and female unemployment in contiguous states	0.001 (0.045)	0.380*** (0.126)	average benefits, sex ratio and female unemployment in contiguous states	<0.001	0.590

Notes: Regressions use 1982–1992 data from the fifty states. Dependent variable is the maximum AFDC benefit for a family of four with no other income; dependent variable, price, financing share, and per capita and retail income variables are expressed in logarithms. Controls for the A.D.A. ranking, proportions of the population who are black, under age 15, over age 64, over age 24 with high school, and over age 24 with college, time effects, and state-specific fixed and trend effects are included in all regressions but not reported. Standard errors appear in parentheses.

* Significant at 0.10 level.

** Significant at 0.05 level.

*** Significant at 0.01 level.

Taken together, the results from table 5 indicate that the estimates and tests from Model 4C are based on an appropriate set of instruments and exclusion restrictions. Issues of overidentification notwithstanding, the results also indicate that our earlier substantive findings of modest positive income effects and weak price effects are not particularly sensitive to the choice of instruments.

In table 6, we list results from maximum benefit regressions that are specified like Model 4B but estimated over the entire time period 1970 to 1992 and over the pre-OBRA subperiod 1970-1981.(n18) We also present results for tests of stability for individual coefficients between the pre- and post-OBRA periods. Estimates of the price effects from regressions run using the pre-OBRA data are very similar to those based on the later data. However, estimates of the income effects from the earlier period are much weaker than estimates from the later period. The income coefficients are also weaker than the majority of studies that examined pre-OBRA data; our use of state-fixed effects accounts for this difference. Coefficients for several of the demographic control

variables as well as the joint set of state-specific effects and state/trend interactions also change significantly across time. Not surprisingly, a general test for structural shift strongly rejects the null hypothesis of coefficient stability. Thus, estimates based on the pre-OBRA data do not provide reliable predictions of behavior after 1981. In appendix B, we report results from additional sensitivity tests that show that our general findings and structural shift results are robust to the selection of other cut-off dates within two years on either side of the OBRA date.

TABLE 6.—COMPARISON OF THE DETERMINANTS OF WELFARE GENEROSITY ACROSS TIME

Variable	1970–1992 Coefficient	1970–1981 Coefficient	H_0: $\beta_{70\text{-}81} = \beta_{82\text{-}92}$ p Value
Price	−0.037***	−0.008	0.725
	(0.011)	(0.013)	
Income	0.041	−0.019	0.088
	(0.077)	(0.115)	
A.D.A. ranking	0.002	0.011	0.923
	(0.027)	(0.039)	
Proportion black	−3.588**	19.930***	<0.001
	(1.757)	(6.589)	
Proportion age 65 or older	−4.509***	1.371	0.035
	(1.319)	(1.980)	
Proportion age 14 or younger	−0.245	−2.372	0.935
	(0.786)	(3.690)	
Proportion with high school	1.120	−5.607	0.035
	(0.809)	(4.380)	
Proportion with college	0.358	15.117	0.130
	(1.620)	(12.838)	
State effects	yes	yes	<0.001†
Time effects	yes	yes	—
State/trend interactions	yes	yes	<0.001†
R^2	0.970	0.974	—

Notes: Regressions use data from the fifty states. Dependent variable is the maximum AFDC benefit for a family of four with no other income; dependent variable, price, and income variables are expressed in logarithms. Standard errors appear in parentheses.

* Significant at 0.10 level.

** Significant at 0.05 level.

*** Significant at 0.01 level.

† Joint coefficient test.

It is possible that the relatively small estimates for price and income in the preceding models result from measurement error bias that has been exacerbated by our use of fixed-effect controls. Measurement error could arise from our use of lagged rather than contemporaneous measures of the independent variables; contemporaneous measures might better reflect voters' expectations of the conditions under which their welfare decisions would be implemented. Measurement error might also occur if there are nonnegligible transactions costs associated with adjusting benefits such that voters respond only to permanent changes in the economic variables. To address these concerns, we reestimated Model 4B using contemporaneous measures of the independent variables and measures averaged over the previous three years. Results for these specifications appear in table 7.

When contemporaneous measures are used, the estimated income effect remains significantly positive but becomes weaker. The estimated price effect is also significantly positive, though small. The results may reflect increased endogeneity bias from the recipiency component of the contemporaneous price variable. In the regressions based on three years of lagged data, the estimated income and price effects become more strongly positive and negative, respectively, than the corresponding estimates from Model 4B. Both coefficients are also significantly different than zero. The results suggest that voters are more sensitive to permanent changes in the economic variables than to transitory changes, and we subsequently refer to these as our "high-end" price and income effect estimates.

As a final sensitivity check, we used a Box-Cox regression to examine the functional specification of Model 4B. In particular, we estimated a specification of the form

$$a_{jt}^{\gamma} = \beta_Y y_{jt}^{\gamma} + \beta_P p_{jt}^{\gamma} + \beta_X' X_{jt} + \epsilon_{jt}, \qquad (4)$$

where j indexes states, t indexes years, Xjt denotes the sociopolitical controls and fixed effects from Model 4B, Epsilonjt is the error term, the [3s are the usual regression coefficients, and Gamma is the Box-Cox coefficient. For brevity, detailed results are not reported. The estimated Gamma parameter from the regression was 0.485 and significantly different from both zero and one. Likelihood ratio tests also rejected both the double-log and linear models. Nevertheless, the estimated elasticities for price and income (0.013 and 0.441, respectively) were not very different from the estimates from Model 4B. To the extent that the square-root and log functions share the same general shape and produce similar substantive results, we feel that the double-log specification represents a reasonable approximation.(n19)

TABLE 7.—REGRESSIONS USING ALTERNATIVE MEASURES OF OBSERVED CONTROLS

Variable	Contemporaneous Measures	Measures Averaged Over Three Previous Years
Price	0.062***	−0.084***
	(0.023)	(0.026)
Income	0.219*	0.508***
	(0.122)	(0.161)
A.D.A. ranking	−0.010	−0.014
	(0.028)	(0.025)
Proportion black	−11.140***	4.419
	(2.470)	(4.739)
Proportion age 65 or older	−11.654***	−1.537
	(2.102)	(2.039)
Proportion age 14 or younger	−4.146***	0.658
	(1.246)	(1.463)
Proportion with high school	4.364***	2.438
	(1.445)	(1.889)
Proportion with college	−0.493	−7.666**
	(2.956)	(3.768)
State effects	yes	yes
Time effects	yes	yes
State/trend interactions	yes	yes
R^2	0.993	0.993

Notes: Regressions use 1982–1992 data from the fifty states. Dependent variable is the maximum AFDC benefit for a family of four with no other income; dependent variable, price, and income variables are expressed in logarithms. Standard errors appear in parentheses.

* Significant at 0.10 level.

** Significant at 0.05 level.

*** Significant at 0.01 level.

V. Predictions of the Effects of Block Grants on Benefits

We use the price and income elasticity estimates from Model 4B and the second specification in table 7 to examine the effects of the switch from matching to block grant subsidies in the new TANF program. The coefficients from Model 4B provide "low-end" point estimates of the price and income elasticities of -0.02 and 0.35, respectively, while the coefficients from the second model give us "high-end" point estimates of -0.08 and 0.51, respectively. To consider the full range of possible effects, we also consider estimates from the lower and upper boundaries of the 95% confidence intervals around these estimates. Thus, for price, we examine effects of a block grant assuming no price response up to a maximum elasticity of -0.14. For income, the corresponding range of elasticity estimates extends from 0.11 to 0.82.

In 1996, the average state received a subsidy of just over 60%. The average effective price increase from an immediate switch to block grants assuming no behavioral response on the part of recipients would be 165%. Using the point estimates described above, this implies benefit changes of either -3% or -14%, and, with the coefficient at the lower boundary of the confidence interval, the average decrease would be at most 22%.(n20) These figures are smaller than the effects predicted by Chernick (1996), whose computations adopted the larger price sensitivity estimates of previous studies.

To get a better feel for the possible range of effects, we can also consider the changes in particular states. For instance, in high-income states like California, Illinois, and New York with 1996 FMAP subsidies of 50%, the unadjusted price increase would be 100%, and the decrease in benefits would be at most 14%. For poorer states, the effective price increase will be much higher. For instance, in Mississippi, the 1996 FMAP subsidy was just over 78%. Block granting implies an effective price increase of 356% and a decrease in benefits of up to 48%.(n21)

As discussed earlier, the enacted reforms orders will have other effects on the AFDC program besides changing the funding mechanism. Several provisions (such as the new time limits for continuing and lifetime receipt of benefits, increased work requirements for recipients, and the recent executive order setting school attendance requirements for teenage recipients) will either reduce program participation directly or reduce the incentives for participation. These reductions will, in turn, lower the effective price of redistribution, mitigating some of the increase from the block grant provisions. Thus, the actual decrease in benefits may well be smaller than the figures projected above.

The presence of these provisions and the new flexibility that will be granted to states in running their welfare programs remind us that there are additional issues of structural shift that the present analysis does not address. Also, while prices similar to those that will prevail under the block grant formula appear in the historical record for the AFDC program, the projected price increases are nevertheless very large by recent standards (i.e., well off the support of the 1982-1992 data).(n22) Accordingly, our predictions should be interpreted with a fair degree of caution.

VI. Conclusion

This article examines differences in states' AFDC generosity using 1969-1992 data. Applying the statistical methods used in previous welfare expenditure analyses to a consistent set of data, the study reproduces the range of income and price results from that literature. The article carefully reexamines the specification assumptions used in these analyses, particularly the assumptions regarding omitted variables and price endogeneity. It finds that state- and year-specific fixed-effect and state-specific linear-trend controls are appropriate additions to the regression models and that, once these controls are included, the price variable can be treated as being exogenous. In other specification tests, the study examines issues involving instrument selection, variable measurement, structural shift, and functional form. It finds that the estimated effects of price and income on benefits are sensitive to many of these issues.

As a result of this extensive testing procedure, the study is able to reduce the range of acceptable price and income estimates to a relatively narrow band. Combining confidence bounds from various estimates, we place the range of price elasticities between -0.14 and 0.02. For income, the overlapping confidence bounds are wider, with the estimated elasticity ranging from 0.11 to 0.82. These consensus price and income results are at the low end of those reported in the literature and indicate that welfare generosity is much less sensitive to economic changes than many analysts had previously supposed.

To illustrate the substantive importance of our findings, we examine the implications they have for the change in the federal AFDC funding from matching subsidies to block grants that is occurring under the new reform legislation. While the effective price increases generated as a direct result of block granting will be enormous, ranging from 100% in high-income states to 356% in the poorest state, the very small price estimates imply that benefit reductions in most states will be relatively modest. Because the legislation entails other changes that will

almost surely reduce recipiency and thus partially mitigate the increase states face in their costs of redistribution, the actual decreases to individuals remaining on AFDC are likely to be even smaller still. Differences between our results and other studies' findings have other important implications for policy. For example, estimates from most previous studies indicated that, with or without federal encouragement, benefit growth would essentially keep pace with states' income growth. However, our findings of more modest income effects suggest that benefit growth will fall behind income growth. Coupling these results with the likely effects of block granting, the prospects for benefit growth in the near term appear dim.

Our investigation also points to several avenues for future research. One crucial area that we have not examined here is an underlying structural explanation for the various fixed-effects controls. We presume that these controls capture heterogeneity in voter attitudes and preferences, characteristics of the needy, and institutions across states and time; however, explicit tests of these conjectures are beyond the scope of the present study. Clearly, it is important to examine not only whether fixed effects belong in these models but why they belong there. Another issue that is not taken up here but explored in another of our studies (Moffitt et al., 1998) is whether the median-voter framework provides an adequate description of state welfare expenditure behavior or whether alternative models might offer better structural explanations. While our other work indicates that a more precise implementation of the median-voter approach is consistent with the results presented here, we have not developed similarly precise empirical representations of the alternative approaches. The explanatory power of the a theoretical fixed-effects controls in our regressions cautions us that we have left quite a bit of room for alternative structural models. Finally, the TANF program and its accompanying legislation create new research opportunities. The switch to block grant funding, lifetime restrictions on eligibility, and changes in other programs such as Food Stamps, Medicaid, Supplemental Security Income, child support enforcement, and other social service programs all represent large exogenous shocks from the perspective of the states. As data become available, these changes can be used by researchers to reexamine the effects that prices and other program features have on benefits.

Notes

(n1) Maintenance-of-effort (MOE) requirements set a floor on benefit levels as a condition of receiving the block grant. Chernick (1996) provides a detailed analysis of incentive effects of various block grant schemes, incorporating both MOE requirements and program-interaction effects.

(n2) Some authors motivate their analyses using alternative models of political competition. (See, e.g., Plotnick, 1986; Baumgardner, 1993.) However, with the exception of Baumgardner's study, estimating equations in these studies are indistinguishable from specifications in which political variables reflect preferences of the median voter. We adopt the median-voter framework mainly to introduce notation and generate rough hypotheses.

(n3) The utility function reflects interdependent preferences (Hochman & Rodgers, 1969; Becket, 1974).

(n4) Table 1 is not an exhaustive list of AFDC expenditure models. Several economic models are not easily comparable to those in table 1, either because they do not model price as depending on recipiency (e.g., Husted, 1989; Smith, 1991), define recipiency differently (e.g., Baumgardner, 1993), or focus on total welfare expenditures (Craig, 1994; Craig & Inman, 1986). In addition, there is a related political science literature (e.g., Peterson & Rom, 1990; Tweedie, 1991) that implicitly specifies dynamic models of AFDC benefits.

(n5) While fixed-effects techniques are useful in addressing omittedvariables bias, they do not identify which specific unobserved factors influence welfare generosity. Preference heterogeneity appears to be a reasonable candidate. Attitudes and values have occupied a central position in attempts to understand the public's dissatisfaction with welfare (e.g., Ellwood, 1988; Cook & Barrett, 1992). However, of the studies in table 1, only Moffitt et al. (1998), Plotnick and Winters (1985), and Ribar and Wilhelm (1996) emphasized the modeling of redistributional preferences.

(n6) In 1969, 36 states were on the old AFDC formula with the remainder on the Federal Medical Assistance Percentage (FMAP) schedule. By 1976, the number on the AFDC schedule had been reduced to ten and by 1982 had fallen to two. After 1982, all states were on the FMAP schedule. Most studies using the early data selected the si for states on the old formula, while some (Gramlich and Gramlich-Laren) dropped states still on the old formula. Moffitt (1984) modeled the simultaneous choice and found weaker price effects.

(n7) For instance, the formula under the FMAP schedule is (1- si) = Max [Min [.50, .45 x (yily)2], .17] where y represents national per capita income. Under the old AFDC schedule, the matching rate was 5/6 up to benefits of $18 per recipient per month, the state's "federal percentage" for benefits between $18 and $32, and zero for benefits exceeding $32. The federal percentage was also a function of per capita income.
(n8) There is actually also a structural source of endogeneity in the Food Stamp Program where individuals' benefits are reduced by thirty percent of their AFDC awards.
(n9) See Peterson and Rom (1990) and Corbett (1991).
(n10) Consider the identification questions present in the fixed effects analyses by Gramlich (1982) and Gramlich and Laren (1984) which use mean welfare benefits in all other states as measures of an. Once controls for a general time series are introduced, the residual variation in ai and an must be perfectly negatively correlated.
(n11) For a more detailed examination of these issues, the reader is referred to the working paper version of this study (Ribar & Wilhelm, 1994).
(n12) Twelve-month averages for monthly recipiency are unavailable for the early years of our sample and would have been prohibitively expensive to compute. Two-month averages should reduce problems of seasonality or excessive variability that might be associated with figures from a single month. For years in which comparisons can be made, there is little difference in results based on two- and twelve-month averages.
(n13) In a separate analysis (Moffitt et al., 1998), we determined that estimates of the benefit demand function were not particularly sensitive to the use of median versus average income.
(n14) Other analysts (e.g., Moffitt, 1987, 1990) have discussed the role played by structural shifts in public assistance policies in explaining the time-series variation in benefits and recipiency. Among these shifts were a number of changes in the AFDC program itself: a decrease in the benefit reduction rate (the rate at which outside income reduces program benefits) from 100% to 67% in 1967, a series of court decisions that expanded eligibility in the early 1970s, the 1981 OBRA changes, and reforms from the Family Support Act of 1988. Other sources of structural variation were the creation of additional public assistance programs, most notably Food Stamps in 1961 and Medicaid in 1965, and changes in these programs such as the effective taxation of AFD2 benefits and elimination of the purchase requirement in the Food Stamp program in 1977 and the expansion of Medicaid coverage for non-AFDC families in the mid-1980s. Finally, stigma associated with welfare participation may have diminished over time.
(n15) Because the coefficient on the log of the financing share in the first-stage price regression differs slightly from one (see appendix A), the test for overidentification from the financing share is not exactly equivalent to a test of the functional restrictions from equation (1). Exact tests (not shown) fail to reject the restriction that the coefficients on the log financing share and recipiency variables in the benefits model are equal. Note that all of our tests for overidentification are conditioned on the remaining exclusion restrictions.
(n16) We estimated a regression along the lines of Model 4B using the financing share in place of the price variable (i.e., the reduced form for Model 5B) and obtained results that were nearly identical to those reported for Model 5B. See Gramlich (1982) and Gramlich and Laren (1984) for other reduced-form results in the literature. Craig and Inman (1986) also estimated models using the financing share alone as the price; however, their analysis focused on welfare expenditures rather than maximum benefits per family.
(n17) Moffitt et al. (1998) discuss and report evidence of independent effects of low-skill wages on benefit determination.
(n18) Although the analysis data extend back to 1969, the use of lagged data for the independent variables prevents us from examining the determinants of benefits in that initial year.
(n19) Box-Cox regressions for the model with independent variables averaged over the previous three years produced similar results (specifically, rejection of both the linear and double-log models with an estimated Gamma of .475, estimated price elasticity of -0.015, and estimated income elasticity of 0.329).
(n20) The estimated effects incorporate both price and income effects. The income effects model the block grant subsidies; however, because the total amount of the subsidies are small relative to total income within each state, the income effects are negligible.
(n21) MOE provisions of the TANF program, which do not allow states to drop welfare expenditures by more than 20% to 25%, would seem to rule out such a large reduction in benefits by Mississippi. To reduce benefits

by 48%, the state would have to increase its expenditures in other areas such as child care, work training, or administration.

(n22) Recall that under the old federal percentage formula, states were not subsidized for the portion of monthly per-recipient benefits that exceeded $32.

APPENDIX A.—PRICE REGRESSION

Variable	Coefficient
Sex ratio	−5.485***
	(1.698)
Female unemployment rate	0.007
	(0.006)
State (Medicaid) financing share	1.121***
	(0.138)
Income	−0.974***
	(0.270)
A.D.A. ranking	0.039
	(0.059)
Proportion black	4.987
	(5.766)
Proportion age 65 or older	38.519***
	(4.798)
Proportion age 14 or younger	14.723***
	(2.661)
Proportion with high school	−7.707**
	(3.384)
Proportion with college	−4.558
	(6.648)
State effects	yes***
Time effects	yes***
State/trend interactions	yes***
R^2	0.974

Notes: Regressions use 1982–1992 data from the 50 states. Dependent variable, financing share, and income variables are expressed in logarithms. Standard errors appear in parentheses.

*Significant at .10 level.

**Significant at .05 level.

***Significant at .01 level.

APPENDIX B.—COMPARISON OF THE DETERMINANTS OF WELFARE GENEROSITY ACROSS TIME

Variable	1980–1992 Coefficient	1981–1992 Coefficient	1982–1992 Coefficient	1983–1992 Coefficient	1984–1992 Coefficient
Price	0.005	−0.011	−0.021	−0.030	0.015
	(0.020)	(0.020)	(0.021)	(0.024)	(0.033)
Income	0.450***	0.362***	0.346***	0.277**	0.305**
	(0.099)	(0.102)	(0.122)	(0.132)	(0.137)
A.D.A. ranking	−0.009	−0.002	0.017	0.006	−0.013
	(0.025)	(0.025)	(0.028)	(0.029)	(0.029)
Percent black	−9.078**	−10.230***	−9.025***	−8.325***	z−6.136**
	(2.583)	(2.686)	(2.701)	(2.696)	(2.582)
Percent age 65 or older	−6.123**	−6.065***	−6.506***	−8.697***	−9.000***
	(1.861)	(1.943)	(2.140)	(2.534)	(2.843)
Percent age 14 or younger	−2.181**	−2.159*	−2.668	−3.397**	−2.876**
	(1.077)	(1.134)	(1.292)	(1.383)	(1.434)
Percent with high school	3.247***	4.115***	3.566**	2.262	0.048
	(1.247)	(1.399)	(1.610)	(1.888)	(2.195)
Percent with college	0.789	−1.292	−2.274	−1.318	−2.875
	(2.461)	(2.761)	(3.138)	(3.658)	(4.218)
State effects	yes	yes	yes	yes	yes
Time effects	yes	yes	yes	yes	yes
State/trend interactions	yes	yes	yes	yes	yes
R^2	0.991	0.992	0.993	0.994	0.995
F test for structural shift	10.155	9.236	7.959	6.487	5.803

Notes: Regressions use data from the 50 states. Dependent variable is the maximum AFDC benefit for a family of four with no other income; dependent variable, price, and income variables are expressed in logarithms. Standard errors appear in parentheses.

*Signifciant at .10 level.

**Significant at .05 level.

***Significant at .01 level.

REFERENCES

Americans for Democratic Action. ADA Today, 1990 and previous volumes and titles, Washington, D.C.

Baumgardner, J., "Tests of Median Voter and Political Support Maximization Models: The Case of Federal/State Welfare Programs," Public Finance Quarterly 21(1) (1993), 48-83.

Becker, G., "A Theory of Social Interactions," Journal of Political Economy 82(6) (1974), 1063-1093.

Chernick, H., "Fiscal Effects of Block Grants for the Needy: A Review of the Evidence," mimeo, City College of New York (1996).

Committee on Ways and Means, U.S. House of Representatives (various years). Green Book: Background Material and Data on Programs within the Jurisdiction of the Committee on Ways and Means (Washington, DC: US GPO).

Cook, F., and E. Barrett, Support for the American Welfare State: The Views of Congress and the Public (New York: Columbia University Press, 1992).

Corbett, T., "The Wisconsin Welfare Magnet Debate: What Is An Ordinary Member of the Tribe To Do When the Witch Doctors Disagree?" Focus, 13(3) (1991), 19-28.

Craig, S., "Fiscal Relief or Programmatic Change: The Impact of Federal Low Income Assistance Aid on State Government Behavior," mimeo, University of Houston (1994).

Craig, S., and R. Inman, "Education, Welfare and the `New' Federalism: State Budgeting in a Federalist Public Economy," in H. Rosen (ed.), Studies in State and Local Public Finance (Chicago: University of Chicago Press, 1986) 187-222.

Ellwood, D., Poor Support: Poverty in the American Family (New York: Basic Books, 1988).

Gramlich, E., "An Econometric Examination of the New Federalism," Brookings Papers on Economic Activity 2 (1982), 327-360.

Gramlich, E., and D. Laren, "Migration and Income Redistribution Responsibilities," Journal of Human Resources 19(4) (1984), 489-511.
Hochman, H., and J. Rodgers, "Pareto Optimal Redistribution," American Economic Review 59(4) (1969), 542-557.
Hulten, C., J. McCallum, E. Durman, and R. Michel, "A Model of State Determination of AFDC Benefits," mimeo, the Urban Institute (1982).
Husted, T., "Nonmonotonic Demand for Income Redistribution Benefits: The Case of AFDC," Southern Economic Journal 55 (1989), 710-727.
Moffitt, R., "The Effects of Grants-in-Aid on State and Local Expenditures," Journal of Public Economics 23 (1984), 279-305.
----- "Historical Growth in Participation in Aid to Families with Dependent Children: Was There a Structural Shift?" Journal of Post Keynesian Economics 9(3) (1987), 347-363.
----- "Has State Redistribution Policy Grown More Conservative?" National Tax Journal, 43(2) (1990), 123-142.
Moffitt, R., D. Ribar, and M. Wilhelm, "State Welfare Policy in the United States," Journal of Public Economics, 68(3) (1998), 421-452.
National Opinion Research Center, General Social Survey, 1972-91: Cumulative Codebook (Chicago: University of Chicago Press, 1992)
Orr, L., "Income Transfers as a Public Good: An Application to AFDC," American Economic Review 66 (1976), 359-371.
----- "Food Stamps for AFDC Families: Income Supplementation or Fiscal Relief?" mimeo, U.S. Department of Health, Education and Welfare (1979).
Peterson, P., and M. Rom, Welfare Magnets: A New Case for a National Standard (Washington, D.C.: The Brookings Institution, 1990).
Plotnick, R., "An Interest Group Model of Direct Income Redistribution," Review of Economics and Statistics 68 (1986), 594-602.
Plotnick, R., and R. Winters, "A Politico-Economic Theory of Income Redistribution," American Political Science Review 79 (1985), 458-473.
Ribar, D., and M. Wilhelm, "The Effect of Costs, Resources, Interstate and Interprogram Competition, and Redistributional Preferences on AFDC Expenditures," mimeo, Pennsylvania State University (1994).
----- "Welfare Generosity: The Importance of Administrative Efficiency, Community Values, and Genuine Benevolence," Applied Economics 28 (1996), 1045-1054.
Robins, P., "Explaining Recent Declines in AFDC Participation," Public Finance Quarterly 18 (1990), 236-255.
Shroder, M., "Games the States Don't Play: Welfare Benefits and the Theory of Fiscal Federalism," Review of Economics and Statistics 77 (1995), 183-191.
Smith, P., "An Empirical Investigation of Interstate AFDC Benefit Competition," Public Choice 68 (1991), 217-233.
Tweedie, J., "Looking to the States: A State-Centered Model of Welfare Policymaking," mimeo, the State University of New York-Binghamton (1991).
U.S. Bureau of Economic Analysis, Regional Economic Information System (Washington, D.C.: U.S. Department of Commerce, 1992).
U.S. Bureau of Labor Statistics, Geographic Profile of Employment and Unemployment (Washington, D.C.: U.S. GPO, various years).
U.S. Bureau of the Census, Census of Population and Housing (Washington, D.C.: U.S. GPO, 1970, 1980, 1990).
U.S. Bureau of the Census, Intercensal Estimates of the Population of Counties by Age, Sex, and Race: 1970-1980 (Washington, D.C.: U.S. GPO, 1984).
U.S. Bureau of the Census, Revised Estimates of the Population of Counties by Age, Sex, and Race: 1980-1989 (Washington, D.C.: U.S. GPO, 1992).
U.S. Social Security Administration, Characteristics of State Plans for Aid to Families with Dependent Children (Washington, D.C.: U.S. GPO, various years).

U.S. Social Security Administration, Quarterly Public Assistance Statistics (Washington, D.C.: U.S. GPO, various years).
U.S. Social Security Administration, Social Security Bulletin (Washington, D.C.: U.S. GPO, various years).
U.S. Social Security Administration, Social Security Bulletin: Annual Statistical Supplement (Washington, D.C.: U.S. GPO, various years).