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THE EFFECT OF THE U.S. WELFARE SYSTEM ON MARITAL STATUS

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An issue of long-standing importance in the U.S. welfare system has been its lack of neutrality with respect to family composition, which generally provides payments only to female-headed families – that is, families with no able-bodied male present. Using data from 1969 to 1985 to examine the issue, this study finds that (1) the simple cross-sectional correlations between marital status and welfare benefits are almost always in the expected direction but are generally weak in significance; (2) that the magnitude and significance of the correlations have nevertheless grown over time; and (3) that the correlations for men are no weaker and usually stronger, especially for blacks, than those for women.

1. Introduction

Research on the economic effects of the U.S. welfare system on individual behavior has over a twenty-year history but has been undergoing a significant evolution in the last few years. In the literature as a whole, by far the most attention has been paid to the static work disincentives of transfer programs, both those of an experimental nature, such as a negative income tax, and those of the major cash program in the United States, the Aid to Families with Dependent Children (AFDC) program. The most important policy issue in this literature has always been the potential work-inducing effects of lowering the tax rate, also called the 'benefit-reduction rate', in such programs. This line of attack has for the most part played itself out, perhaps through sheer exhaustion, but also because it is now widely agreed by both economists and government officials that manipulation of the tax rate is likely to have little significant positive impact on labor supply, if not a negative impact [see the recent review of this literature by Moffitt (1987a)]. In the past few years, attention has turned instead to other issues, such as the effects of in-kind transfers, which have grown far more rapidly in the

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United States than cash transfers; to the study of the effects of work requirements and training programs for welfare recipients, which have been of great interest to government officials recently; and to the study of turnover in transfer programs and of other dynamic issues such as those related to human capital formation.

Another issue to which economists have been granting increasing attention – but with which demographers have long been concerned – is the effect of the United States transfer system on household structure, including effects on marital status, fertility, and the presence of multiple families within a household. The most well-known potential effect of this sort arises in the AFDC program, where families with an able-bodied male are generally ineligible. Although there are some exceptions in some states to this requirement, the caseload of the program is predominantly composed of female-headed families – that is, families headed by women with children under the age of 18 and with no able-bodied male present.[1] There is an obvious incentive in such a system to delay marriage or remarriage, and to hasten marital dissolutions.

The issue is important not only for the direct value which voters put on intact marriages when children are present, but also for its potential indirect effect on labor supply. Marital status is a powerful determinant of female labor supply, for example, for female heads have higher labor supply levels than married women with children [Moffitt (1987a)]. It is quite possible that the indirect effects of AFDC on labor supply working through marital status could dwarf the static labor supply effects of the program, and possibly in an opposite direction.

In this study I use the Current Population Survey (CPS) to further examine the issue. The monthly surveys of approximately 60,000 households that constitute the CPS are conducted by the Bureau of the Census and are based upon a representative sample of the U.S. non-institutional population. Income and earnings information is collected only once a year (in March), and the annual series of these surveys roughly constitutes a series of independent, repeated cross-sections of the population.[2]

Using any one of these cross-sections, reduced-form, marital-status equations can be estimated as a function of potential welfare benefits and other variables. A systematic examination of the results of such cross-sectional estimates – and how they change as they are obtained from cross-sections

[1]Structuring a transfer system in this way may seem absurd on the face of it in countries like the United Kingdom where benefits are provided regardless of household composition and marital status. In the United Kingdom benefits are provided to married couples who satisfy appropriate income and asset conditions although, until the spring of 1988, the benefit schedules for them and for female heads were not fully integrated.

[2]Families are kept in the survey for 16 months (though not continuously), so there is an overlap between adjacent years. However, this aspect of the data will not be used below, for adjacent CPS years will not be employed.

from different years – is the major contribution of this paper. A secondary contribution is an examination of estimates from marital-status equations for men as well as for women.

Estimates of this type are often criticized because the equations from which they are derived are implicitly based upon a static model of marital status when, in fact, the true process is clearly a dynamic one. For example, it is often argued that panel data are necessary to correctly study the dynamics of marital status. In one section of this paper it is shown that this argument is not correct and that data from a series of repeated cross-sections such as are available from the CPS can be used to estimate dynamic models of marital status. Estimates of such models are obtained and their results are compared with those of the purely static, cross-sectional models.

The outline of the paper is as follows. In section 2 several theoretical and empirical issues in the study of welfare effects on marital status are discussed. Section 3 provides a brief summary of some of the past research on the topic. The new cross-sectional evidence is presented in section 4 and the estimation of dynamic models follows in section 5. A summary concludes the paper.

2. Theoretical and empirical issues in the estimation of welfare effects on marital status

2.1. Theoretical considerations

Although the empirical work reported in the subsequent sections is reduced-form in character, it is worthwhile outlining several elementary models of marital-status formation and the role of AFDC in those models. Virtually all models of marital-status formation are based upon the utility differences model of Becker (1981). An example of one simple model, among many, is that which allows the utility function of an individual of age t to be $U(M_t, H_t, C_t)$, where M_t is a dummy equal to one if married and zero if not, H_t is the individual's hours of work, and C_t is his or her consumption. Ignoring capital markets and letting the budget constraint be $W_tH_t+N_t+fM_tY_t=C$, where W_t is the hourly wage rate, N_t is his or her own non-labor income, Y_t is potential spouse income, and f is the net fraction of that income spent on the individual's consumption, the quasi-indirect utility function can be written as $V(M_t, W_t, I_t^M)$, where $I_t^M=N_t+fM_tY_t$. The marriage choice is thereby made on the basis of the utility difference:

$$M_t^* = V(1, W_t, I_t^1) - V(0, W_t, I_t^0), \tag{1}$$

$$M_t = \begin{cases} 1, & \text{if } M_t^* \geq 0, \\ 0, & \text{if } M_t^* < 0, \end{cases} \tag{2}$$

In this simple model the AFDC program obviously increases potential income in the non-marriage state for women and hence lowers their marital probabilities.

While this theory has great intuitive appeal, it lacks many of the features that one would naturally desire in a model of marital-status determination. Most importantly, its static nature obscures the search and uncertainty aspects of the decision and the associated set of asymmetries in marital formation, on the one hand, and marital dissolution, on the other. Marital formations occur after a period of search over potential partners and marital dissolutions occur after a period of experience with a particular partner and after the uncertainty of the value of the match has been effectively resolved (negatively).

The sources of uncertainty in the search and dissolution processes are many, but a flavor for the types of models that are called for can be gained from what is perhaps the simplest of all such models, one with uncertainty arising only from random arrivals of new preference valuations for existing and potential spouses. Let a finite-lived individual face a choice at each t between marriage to a spouse with a certain income and non-marriage with certain non-marital income (including transfers), and let the period-specific utility from each be once again $V(M_t, W_t, I_t^M)$. Assume that all future spousal and non-marital incomes are known with certainty. To allow crudely for search-theoretic considerations, assume that there is a utility-equivalent cost of γ to dissolving an existing marriage and a search cost δ that is incurred if an individual is unmarried. Letting the random valuation shocks to the marital and non-marital options be v_t^m and v_t^n, respectively, the value functions for the two choices at time t are the following:

$$V_t^m = U(1, W_t, Y_t^1) - \delta(1-M_{t-1}) + \beta E_t(V_{t+1} | M_t = 1) + v_t^m, \tag{3}$$

$$V_t^n = U(0, W_t, Y_t^0) - \gamma M_{t-1} - \delta(1-M_{t-1}) + \beta E_t(V_{t+1} | M_t = 0) + v_t^n, \tag{4}$$

where the expectation is taken over the values of v_{t+1}^m and v_{t+1}^n and where $V_{t+1} = \max(V_{t+1}^m, V_{t+1}^n)$. The marriage criterion function to replace (1) becomes:

$$M_t^* = V_t^m - V_t^n = U(1, W_t, Y_t^1) - U(0, W_t, Y_t^0) + \gamma M_{t-1}$$
$$+ \beta[E_t(V_{t+1} | M_t = 1) - E_t(V_{t+1} | M_t = 0)] + v_t, \tag{5}$$

where $v_t = v_t^m - v_t^n$. When expanded, the difference in the expectations of V_{t+1} can be shown to equal the differences in the expected present values of all possible sequences of future marital formations and dissolutions across $M_t = 1$ and $M_t = 0$. An undesirable feature of this model is the strong Markovian assumption in (3) and (4) that only the value of M in the most recent period (M_{t-1}) matters, implying that the current M_t decision would affect future valuations for only one period.

While this model is oversimplified in many respects, it does allow for the presence of intrinsically unobservable (to the analyst) match-specific information, and for individual behavior resulting from future values of such information also being uncertain and unobserved. Formations and dissolutions in the model occur in much the same way as worker–firm attachments in the well-known model of Jovanovic (1979), and the same types of learning about the value of a match over time could be explicitly represented here.

Nevertheless, this model and more sophisticated variants of its type will not generate any different predictions of the effect of AFDC on marital probabilities. An increase in the level of present and future benefit levels will increase utility in both present and future periods of non-marriage regardless of present decisions. In fact, any simple model of marital-status determination that incorporates the major features of the processes involved will generate the same predicted effect of AFDC as that in the simple differences model in (1)–(2).

An important effect that these partial equilibrium models cannot capture is any effect on the marital-status decisions of males. To capture such effects in the most direct fashion requires an equilibrium marriage market model such as that based upon the optimal assignment model of Koopmans–Beckmann [see Becker (1981)] or something similar. An increase in AFDC benefit levels must be allowed to increase the reservation price of women and hence to force fewer marriages to form and more to dissolve. With such a change in the reservation price, the models above could be suitably modified to apply to males and the effect of AFDC would work through the equilibrium level of spousal income (i.e. potential female income) or some other matching characteristic of women. Consequently, in the empirical work below, tests for the effects of transfers will be conducted for men as well as women.

2.2. Econometric considerations

Assuming that one wishes to estimate a reduced-form version of the static model (1)–(2) on cross-sectional data, at least three empirical and econometric issues arise. First, there is the problem that a truly reduced form of (1) will contain few variables. Labor supply, earnings, and family unearned income as well as the presence and number of children are all obviously endogenous to the marital-status decision and consequently do not belong in a reduced form. The wage rate may be presumed exogenous as a first approximation, but a completely reduced form will contain only its determinants inasmuch as wage rates are unavailable for non-workers. The characteristics of a potential spouse are also unobserved and those of actual spouses observed only for married persons; hence only characteristics of the individual himself or herself may enter the equation.[3] As a result, the marital-status equations presented below will be quite parsimonious, conditioning only upon age, education, and a few other variables.

The endogeneity of children is particularly important, since AFDC may affect fertility decisions. There are actually four states instead of two in which women can locate: married with children, married without children, unmarried with children, and unmarried without children. AFDC provides an incentive to choose the third category relative to the other three. But since all four are potential choices, it would not be proper to exclude from the sample any women in the other three groups.

Second, since all women are potentially eligible for AFDC, a reduced-form analysis must include all women. Moreover, since all men are potentially affected by a change in the female reservation price, the male sample must include all men. However, since only 3–4 percent of all U.S. women aged 15–64 receive AFDC payments, estimates on total-population samples of men and women are likely to be relatively inefficient. To the extent that there are exogenous characteristics available (e.g. low education) which are thought on an a priori basis to be correlated with the utility difference in (1), these may be used to stratify the sample. Some attempts at this approach will be made below.

Third, the identification of the effects of the welfare system requires the use of a welfare variable which is both exogenous and which has variance in a cross-section. In the U.S. welfare system, the major such variable is the level of the AFDC benefit for a fixed family size in the 51 states and jurisdictions (in the United States, the individual states and jurisdictions set benefit levels). In the analysis below, the state-specific medical benefit for AFDC families (Medicaid) will be considered as well. However, the use of such state-specific indicators of welfare generosity runs the risk of resulting in estimates confounded with other, unobserved but state-specific, variables. Marital status varies across the states for social and cultural reasons that are difficult to capture with observables but which may be correlated with the generosity of welfare benefits, a point stressed previously by Ellwood and Bane (1985). The preferable approach to this problem is to employ a sufficiently large number of time periods of data that state fixed effects can be permitted in the model. Unfortunately, since only three cross-sections will be used for the analysis reported here, such a model cannot be reliably estimated. Instead,

[3]An approach to this problem is to enter the mean values of the characteristics of those of the opposite sex in the local residential areas of the individuals in the sample. Severe data constraints on local-area data prevent a very detailed approach of this kind. For a recent attempt, see Fitzgerald (1988).

only fixed regional effects will be allowed. Future work with more cross-sections will address this problem.[4]

Finally, if one is interested in estimating dynamic rather than static models, the question of whether cross-sectional data can be used at all arises. This is considered in section 5 below.

3. Review of past work

There has been a considerable amount of past work on the effect of AFDC payments on marital status and female headship but much of it was conducted in the 1970s. Perhaps a dozen or so studies of this type are available and have been summarized in several other places [e.g. Groeneveld et al. (1983)]. Some studies conducted purely cross-sectional analyses using aggregate or individual data while others examined the effects of AFDC benefit levels on transition rates, primarily those for marital dissolution but also for remarriage [e.g. Hutchens (1979)]. Also, most of the studies used data from the late 1960s or from the early 1970s.

The findings of the work are, overall, extremely mixed. No effects, or effects in the opposite direction as expected, were found in at least half of the studies, if not more. In many of those where effects have been found they are small in magnitude or weak in significance. The absence of a strong effect in the expected direction is particularly surprising because the period under consideration (late 1960s, early 1970s) was the period of greatest growth in real welfare benefit levels. As will be shown below, real benefit levels have fallen since that time.

Since 1980 there have been two studies of direct relevance to the present one. First, a paper by Danziger et al. (1982) used 1975 CPS data to estimate the cross-sectional effect of the welfare system on female headship. Unfortunately, Danziger et al. did not estimate the direct effect of welfare benefits but only of a composite income variable for total net income in the non-headship state (i.e. the coefficients on all types of income in that state were constrained to be equal). In any case, they found an effect in the expected direction but one which was extremely small in magnitude, implying that the AFDC system as a whole has increased headship rates by about one percentage point (compared to means of 40 percent for non-whites and 16 percent for whites). Moreover, their estimated effects considerably underpredict the time-series increase in headship from 1968 to 1975.

Ellwood and Bane (1985) conducted a more comprehensive analysis than prior studies. Ellwood and Bane (EB) estimated a 1975 cross-section using aggregate state data and found the welfare effect to be opposite in sign to that expected and often significant, a result also opposite to that of Danziger

[4]Another issue is the effect of the presence of state-specific random effects and consequent implications for standard errors. This issue is also ignored in the analysis reported below.

et al. EB attributed this result to unobserved cross-state differences that are correlated with the AFDC benefit, and argued that a fixed-effects model is necessary. EB estimated more than one type of fixed-effects model but their preferred model used 1976 cross-sectional census data and showed small but significant effects on female headship and on divorce and separation. Their model is quite different from that which will be estimated below because the identification conditions they imposed to be able to obtain fixed-effects estimates on a single cross-section will not be imposed here.[5]

While attempting to generalize from these two studies is difficult, given the types of models estimated, they both appear to have found small but significant effects in the expected direction. While it may appear that the two studies have found effects slightly stronger than in the previous literature, this is not so clear when it is realized that some studies in the previous literature also found some effects in the expected direction. Nevertheless, they do show that there continues to be little evidence for strong effects, at least on data up to 1976.

4. Cross-sectional estimates in 1969, 1977, and 1985

Three March files of the CPS are available for this analysis, those in 1969, 1977, and 1985. These three waves are each eight years apart, which makes the dynamic analysis in the next section easier to specify. But the three years also span the 1970s and early 1980s and hence should provide an indication of the time trend in the estimated effect of AFDC. For each CPS, all individuals aged 16–55 are selected. Separate estimates are provided for four subgroups – white females, black females, white males, and black males. To keep sample sizes to manageable proportions the two white subgroups are subsampled down to approximately 5,000.

The means of the major variables used in the analysis are shown in table 1. As is well known, marital probabilities have fallen over time for both whites and blacks and female-headship rates have risen, although both trends are much greater for blacks. Table 1 also shows the variables included in the equations – age, education, metropolitan residence, the state manufacturing wage, regional dummies, and a measure of the welfare benefit ('benefit sum'), to be discussed further below. As noted previously, there are few individual characteristics that are exogenous to the marital choice process. The state manufacturing wage is included to proxy wage levels. Both it and the

[5]EB identified the AFDC effect by using within-state variation in AFDC participation as an instrument, whose coefficient was identified by the exclusion of two variables for children from the main equation. Since children will be assumed endogenous in the work reported below, this method will not be possible. For the same reasons, other estimates of EB obtained by comparing childless women to women with children will not be used here. However, they did obtain some fixed-effects estimates using data from 1960, 1970, and sometimes 1975. They regarded the results, which showed unstable and often insignificant welfare effects, as unreliable.

Table 1

Means of the variables used in the analysis.

	Females		Males	
	White	Black	White	Black
Marital status[a]				
1985	0.62	0.33	0.63	0.43
1977	0.66	0.40	0.66	0.48
1969	0.70	0.48	0.72	0.58
Female headship[b]				
1985	0.08	0.27	–	–
1977	0.07	0.22	–	–
1969	0.04	0.15	–	–
Characteristics in 1985[c]				
Age	36.85	35.36	37.07	34.99
Education	12.23	11.72	12.59	11.40
Lives in metropolitan area	0.56	0.73	0.55	0.73
State manufacturing wage/100[d]	3.45	3.41	3.47	3.42
Northeast	0.24	0.20	0.23	0.19
Midwest	0.26	0.18	0.26	0.18
South	0.28	0.54	0.23	0.55
West	0.23	0.08	0.28	0.09
Real benefit sum/100	5.64	5.31	5.63	5.32
Sample sizes				
1985	4844	5530	4792	4209
1977	4787	5020	4741	4056
1969	4775	4787	4830	3734

Notes: Samples used to construct table include individuals aged 56–64.
[a]1 = married, spouse present. 0 = other marital status.
[b]1 = female head or subhead with children less than 18, no spouse present. 0 = other.
[c]Characteristics in 1969 and 1977 shown in the appendix, table A.1.
[d]In 1982 PCE (personal consumption expenditure deflator) dollars. Weekly.

regional dummies, which will capture more general area-specific forces, are included to reduce the problem of unobserved state-specific variables correlated with the state welfare benefit.[6]

Table 2 shows the trends in the transfer benefit variables used in the analysis – the AFDC guarantee (i.e. the amount paid to a family with no other income), the Food Stamp guarantee, the insurance value of the Medicaid benefit, and the sum of the three.[7] Since AFDC and Medicaid are state programs, the means in the table represent those computed across all states in each year. However, the Food Stamp program is fixed nationwide. The table indicates that real AFDC guarantees trended upward from 1955 to 1970 but have fallen since that time. In fact, the AFDC guarantee in 1985 is less than it was in 1955. However, in-kind transfers for food and medical

[6]The state unemployment rate was also tested but was always insignificant.
[7]Family-of-four values for all are used. These are highly correlated with the values for other family sizes.

Table 2

Means of the state benefit variables.[a]

	AFDC guarantee, family of four (1)	Food stamp guarantee, family of four (2)	Insurance value of Medicaid, family of four (3)	Sum[b] (4)
1955	416	0	0	416
1960	458	0	0	458
1965	474	0	0	474
1970	498	233	102	619
1975	462	267	168	652
1980	400	241	181	588
1985	348	237	182	547

[a]All values deflated by 1982 PCE deflator.
[b]Pre-1968: (4) = (1); post-1968: (4) = 0.70 * (1) + (2) + 0.368 * (3).

expenses were introduced in the late 1960s and resulted in a net increase in the benefit sum through 1975, though the sum fell thereafter.[8] Elsewhere I have argued that the reduction in the AFDC guarantee was a result of the introduction of Food Stamps and Medicaid and that the benefit sum has grown exactly in line with income growth [Moffitt (1987b)]. But for present purposes it is necessary only to stress that it is the benefit sum that should be the measure of transfer benefits rather than AFDC alone, for Medicaid transfers to the non-elderly are almost entirely received by AFDC recipients and AFDC recipients constitute the largest category of Food Stamp recipients.

These trends show that the time-series evidence is not prima facie consistent with a disincentive effect of the welfare system on marital status or female headship. This conclusion seems incontrovertible at the present time. However, if there is evidence of cross-sectional disincentive effects, this may suggest that the time-series trends are simply the result of some other opposing force (e.g. the increase in the female wage).

Table 3 shows a series of probit estimates of marital-status equations for white females in 1985. Column (1) shows the closest estimate that is plausible of a raw correlation between marital status and the benefit sum, for in addition the equation only includes a spline in age. Interestingly, the coefficient is negative and very significant. The magnitude implies that a $100 increase in the benefit sum would lower marital probabilities by about 3.5

[8]The benefit sum in the table is calculated as the sum of 70 percent of the AFDC guarantee, the Food Stamp guarantee, and 36.8 percent of the insurance value of Medicaid. The 70 percent reduction is applied because Food Stamp benefits are reduced by 30 percent of the AFDC benefit, and the 36.8 percent reduction is applied to adjust Medicaid to a cash-equivalent value [see Smeeding (1982)].

Table 3

Probit estimates of white female marital-status equations, 1985.

	(1)	(2)	(3)	(4)	(5)
Benefit sum/100	−0.094*	–	−0.067*	−0.072*	−0.014
	(0.023)		(0.024)	(0.025)	(0.035)
AFDC benefit/100	–	−0.069*	–	–	–
		(0.016)			
Log age	5.273*	5.276*	5.496*	5.490*	5.516*
	(0.331)	(0.331)	(0.334)	(0.339)	(0.342)
Max[0, log (age − 25)]	−4.044*	−4.050*	−4.260*	−4.253*	−4.268*
	(0.505)	(0.505)	(0.412)	(0.512)	(0.515)
Max[0, log (age − 35)]	−1.494*	−1.486*	−1.618*	−1.619*	−1.646*
	(0.397)	(0.397)	(0.401)	(0.402)	(0.402)
Education	–	–	−0.032*	−0.032*	−0.032*
			(0.008)	(0.008)	(0.008)
Metropolitan residence	–	–	−0.248*	−0.256*	−0.235*
			(0.045)	(0.046)	(0.048)
Manufacturing wage	–	–	–	0.041	−0.042
				(0.047)	(0.057)
Northeast	–	–	–	–	−0.225*
					(0.084)
Midwest	–	–	–	–	0.024
					(0.079)
West	–	–	–	–	−0.101
					(0.081)
Intercept	−16.10	−16.39	−16.42	−16.51	−16.57
Log likelihood	−2247.9	−2247.4	−2225.1	−2224.8	−2218.4

Notes: Standard errors in parentheses.
*Significant at the 10 percent level.

percentage points at the mean. This is a considerably stronger estimate than has been obtained in the literature.

Column (2) shows the effect of using the more conventional AFDC benefit in place of the sum. As the table indicates, this does lower the magnitude of the estimate and hence may explain some of the difference with past work. On the other hand, the significance level is still much higher than that in the literature and is, in fact, higher than in column (1).

Columns (3)–(5) show the effect on this estimate of expanding the list of regressors. Neither the addition of individual characteristics for education and metropolitan residence nor the addition of a state-specific variable for the manufacturing wage has any effect on the general conclusions that would be drawn from the analysis, but the addition of regional dummies does. In column (5) the sum coefficient drops in magnitude and into insignificance, an indirect indication that marital-status probabilities are higher in regions where the benefit is lower (e.g. the South).

Table 4 shows estimates of the 'raw-correlation' coefficients and the fully-adjusted coefficients for other time periods for white females. A comparison of the specification (1) estimates across years shows a pattern of increasing

Table 4

Probit estimates for white females across time.

	1969 (1)	1969 (5)	1977 (1)	1977 (5)	Pooled[a]
Benefit sum/100	−0.057* (0.018)	0.017 (0.035)	−0.065* (0.018)	−0.043 (0.030)	−0.014 (0.018)
Log age	6.679* (0.330)	6.826* (0.337)	4.753* (0.269)	4.884* (0.278)	5.644* (0.178)
Max[0, log (age −25)]	−6.311* (0.543)	−6.633* (0.552)	−3.672* (0.458)	−3.834* (0.472)	−4.743* (0.289)
Max[0, log (age −35)]	−1.097* (0.453)	−1.033* (0.457)	−1.559* (0.430)	−1.543* (0.434)	−1.431* (0.245)
Education	–	−0.015* (0.008)	–	−0.005 (0.008)	−0.176* (0.005)
Metropolitan residence	–	−0.157* (0.053)	–	−0.261* (0.048)	−0.219* (0.028)
Manufacturing wage	–	−0.163 (0.100)	–	0.142* (0.060)	0.014 (0.037)
Northeast	–	−0.200 (0.123)	–	−0.105 (0.100)	−0.179* (0.055)
Midwest	–	0.097 (0.104)	–	−0.031 (0.088)	0.007 (0.049)
West	–	0.027 (0.099)	–	−0.107 (0.084)	−0.087* (0.047)
1977 dummy	–	–	–	–	−0.175* (0.034)
1985 dummy	–	–	–	–	−0.426* (0.039)
Intercept	−20.09	−20.20	−14.282	−15.045	−168.61
Log likelihood	−1784.9	−1773.6	−2108.1	−2088.5	−6108.6

Note: Standard errors in parentheses.
*Significant at the 10 percent level.
[a]Specification (5).

effects both in magnitude and in significance level. This may explain why many of the cross-sectional estimates found here appear to be stronger than what has been found in the literature. No attempt will be made to explain this trend, since no structural models are estimated, though some speculation will be provided later.

Nevertheless, table 4 also shows that the significance of the effects disappears and the coefficient magnitudes drop when regional variables are added (indeed, the 1969 coefficient is positive). The pooled estimates indirectly indicate this result, for the overall estimate of the coefficient for all cross-sections is negative but insignificant.

Table 5 shows similar estimates for the other three subgroups. Column (1) shows a rather consistent set of estimates for the simple model across groups and across years. The coefficient estimates are uniformly significant and grow monotonically over time for every group, a result unlikely to be due to

Table 5

Probit coefficients on benefit sum for other subgroups.

	Specification (1)	Specification (5)
Black females		
1969	−0.045* (0.014)	−0.043 (0.029)
1977	−0.066* (0.014)	−0.018 (0.028)
1985	−0.089* (0.021)	−0.043 (0.039)
Pooled	–	−0.027* (0.016)
White males		
1969	−0.046* (0.020)	−0.039 (0.037)
1977	−0.048* (0.020)	−0.018 (0.033)
1985	−0.093* (0.025)	−0.060 (0.038)
Pooled	–	−0.031 (0.019)
Black males		
1969	−0.018 (0.018)	−0.016 (0.036)
1977	−0.052* (0.018)	−0.073* (0.033)
1985	−0.120* (0.025)	−0.149* (0.145)
Pooled	–	−0.058* (0.020)

Note: Standard errors in parentheses.
*Significant at the 10 percent level.

chance. Once again, however, the addition of the other covariates (especially region) lowers the significance of most coefficients below conventional levels. Interestingly, the exception is that of black males, for whom the significance remains and magnitudes in fact grow. Indeed, it will be found repeatedly in the rest of the work reported below that effects are strongest for black males of all the groups, a result not found previously in the literature because males have not been studied.[9]

It should also be noted that the adjusted coefficients in column (2) remain

[9]This may be the best place to note that there must be, by definition, a relationship between any effects found for men and women. With a complete sample of the population and with equal numbers of men and women, marriage rates must be equal in both. But the estimates here do not have to be equal for the two groups not only because these are only samples and because the total population does not have exactly equal numbers, but also because the equality must hold only when the regressors are integrated out.

Table 6

Probit coefficients on benefit sum in subsamples.[a]

	Age <35	Education <12	Below-mean hourly wage[b]
White females			
1985	−0.033	0.050	0.033
	(0.047)	(0.078)	(0.052)
Pooled	−0.031	0.022	0.007
	(0.023)	(0.034)	(0.025)
Black females			
1985	−0.063	−0.045	−0.043
	(0.052)	(0.082)	(0.058)
Pooled	−0.021	−0.046*	−0.023
	(0.022)	(0.025)	(0.025)
White males			
1985	−0.066	−0.069	−0.107*
	(0.050)	(0.084)	(0.061)
Pooled	−0.029	0.004	−0.019
	(0.026)	(0.039)	(0.030)
Black males			
1985	−0.140*	−0.351*	−0.232*
	(0.060)	(0.102)	(0.074)
Pooled	−0.024	−0.118*	−0.060*
	(0.028)	(0.030)	(0.032)

Note: Standard errors in parentheses.
*Significant at the 10 percent level.
[a]Specification (5).
[b]Uses wages predicted from first-stage equation.

negative for all groups for all years, which is also unlikely to have occurred by chance if the true effect were zero. In addition, the significance levels of the 1985 estimates for white males (12 percent) and for black females (25 percent) are not far from conventional levels. The pattern shown here, and repeatedly below, is consistent with a true negative effect being estimated with low power.

To determine whether the apparent inefficiency of the estimates is a reflection of the estimation of effects only on the total population of each subgroup, some estimates were also obtained on subsamples thought on a priori grounds to be more likely to respond to the AFDC program. Unfortunately, the reduction in sample size that accompanies this subsampling works in opposition to the goal of an increase in efficiency. The results are shown in table 6. Estimates are provided for those less than 35 in age, with fewer than 12 years of education, and with an hourly wage less than the mean.[10] Only estimates for 1985 and on the pooled sample are

[10]Predicted wages from a first-stage log hourly wage regression were used. The variables included were age, age squared, education, metropolitan residence, and regional dummies.

Table 7

Probit coefficients on benefit-sum female-headship equations.[a]

	All	Age <35	Education <12	Below-mean hourly wage
White females				
1985	−0.090*	−0.066	−0.160	−0.096
	(0.046)	(0.062)	(0.101)	(0.066)
Pooled	−0.003	0.042	0.020	0.005
	(0.025)	(0.035)	(0.044)	(0.036)
Black females				
1985	−0.071*	−0.063	−0.002	−0.062
	(0.038)	(0.048)	(0.073)	(0.054)
Pooled	0.009	0.010	0.034	0.014
	(0.018)	(0.024)	(0.026)	(0.026)

Note: Standard errors in parentheses.
*Significant at the 10 percent level.
[a]Specification (5).

provided, the latter to provide an indirect indication of whether the 1985 estimates are stronger or weaker than the average effect across the three years. The most surprising aspect of table 6 is the relative weakness of the coefficients in general as against their counterparts on the total sample in the previous tables. While the effects for black males are indeed larger and more significant in the subsamples, and while the white male effect for 1985 in the low-wage sample is larger than in table 5, the results for the other groups are quite weak. For black females, the effects in the subgroups are usually weaker than in the total samples and for white females this is especially so, for the coefficients often change sign. The statistical power of the coefficients also falls greatly, as well as their magnitudes. Further work is necessary to explore these findings, for they suggest that the benefit-sum effects found in the total-sample results may be spurious. The inclusion of state fixed effects in an expanded data set may resolve this inconsistency.

One of the differences between this study and many of those in the literature is the examination of marital status here rather than female headship, the outcome variable more commonly examined. To determine whether this difference is a cause of the difference in results, several headship equations were estimated as well. The results are shown in table 7.[11] As the table indicates, 1985 estimates for headship are in fact stronger than for marital status (for example, the estimates on the fully-adjusted specification

[11]Although the CPS measured female headship correctly in 1985 it did not do so in years prior to 1982 [Ellwood and Bane (1985)]. To adjust estimates in those years (necessary for the pooled estimates), single never-married childless women over 16 were assigned a status of female headship in any household where there were children under 11 coded 'other relative of head'. The estimates in table 7 for the pooled case include this adjustment, but estimates obtained without it showed no difference of any consequence.

are significant for women on the total sample of each group). However, once again, significance is lost and coefficient magnitudes are reduced in the subsamples.

Overall, the cross-sectional estimates provided in this section provide somewhat greater evidence than have those in the prior literature of disincentive effects of the welfare system on marital status and headship, although the coefficients are often very weak in significance and the pattern of results in subsamples is inconsistent with a true effect. However, cross-sectional estimates are often subjected to the criticism that they misspecify what is clearly a dynamic process with a cross-sectional equation that treats only current values of the regressors as determinants, when in fact past values must have affected the current values of the dependent variable [see, for example, the critique of Groeneveld et al. (1983)]. Indeed, many if not most of the most recent estimates of marital-status models are obtained on panel data rather than cross-sectional data for this reason. The next section of the paper considers this criticism.

5. Estimating dynamic models with repeated cross-sections

Although it is commonly supposed that dynamic models cannot be estimated with cross-sectional data, this is mistaken, as can be demonstrated analytically.[12] To use the framework of duration analysis, we define the following three terms:

p_{it} = probability that individual i is married at age t,
μ_{it} = probability that individual i is unmarried at age $t-1$ but married at time t (i.e. hazard of moving from unmarried to married),
λ_{it} = probability that individual i is married at age $t-1$ but unmarried at time t (i.e. hazard of moving from married to unmarried).

Then we have the identity:

$$\begin{aligned} p_{it} &= \mu_{it}(1-p_{i,t-1})+(1-\lambda_{it})p_{i,t-1} \\ &= \mu_{it}+\eta_{it}p_{i,t-1}, \end{aligned} \tag{6}$$

where $\eta_{it}=1-\mu_{it}-\lambda_{it}$. Since it is surely the case that an individual who is married at $t-1$ is more likely to be married at t than an individual who is unmarried at $t-1$, we can presume that $\eta_{it}>0$, although there is no algebraic requirement that it be so. Eq. (6) is the critical equation for estimating dynamic models with cross-sectional data.

It is common in some applications to use (6) with cross-sectional data by assuming that the hazards are time invariant and the individual to be in a steady state. With such an assumption the steady-state value of $p_i=\mu_i/(1-\eta_i)=\mu_i/(\mu_i+\lambda_i)$.[13] Assuming a set of determinants of μ_i and λ_i, this will generate a cross-sectional relationship between p_i and those determinants. However, the assumption of time-invariant hazards and a steady state are generally not plausible and demonstrably false in the case of marital status.

In any case, this is not necessary because the parameters of (6) are clearly identifiable with cross-sectional data in several other ways. For example, a set of means of p_{it} for a single cohort (i) over several ages (t) could be used to estimate (6) provided some restriction on the pattern of the μ_{it} and η_{it} were imposed. Imposing $\mu_{it}=\mu_i$ and $\eta_{it}=\eta_i$ would lead to a straightforward linear regression formulation, and letting μ_{it} and η_{it} be polynomials in t or some other function of t would lead to a similar regression expression. Alternatively, if the p_{it} and η_{it} were allowed to follow an unrestricted age pattern, data on p_{it} for several different cohorts at each t would allow (6) to be estimated with regression methods if some restriction were imposed on the μ_{it} and η_{it} across i. For example, if $\mu_{it}=\mu_t$ and $\eta_{it}=\eta_t$, we would again have a constant-coefficients model across different cohorts (i) at the same t.[14] Functional form restrictions of all these types are typically imposed in studies using panel data.

Restrictions with more economic content are possible if we have available a vector of variables X_{it} which affects the hazards. This also permits an instrumental-variable interpretation of the availability of a consistent estimator and hence the identifiability of the parameters. Suppose, for example, that a model of marital status, such as one of those discussed in the previous section, generates the linear probability model:

$$\mu_{it}=X_{it}\beta, \tag{7}$$

$$\lambda_{it}=X_{it}\gamma, \tag{8}$$

where X_{it} is a row vector of regressors for individual i at age t with the first element equal to 1. Then we have:

$$p_{it}=X_{it}\beta+(X_{it}\delta)p_{i,t-1}, \tag{9}$$

where $\delta=[1\,0\ldots0]'-(\beta+\gamma)$. Eq. (9) is consistently estimable provided an instrument for $p_{i,t-1}$ is available. But (9) implies that such an instrument will

[12] A more detailed econometric exposition of this issue is provided in Moffitt (1988).

[13] This technique has been applied, for example, in the study of unemployment duration using the CPS by Topel (1983).

[14] There are obvious questions about the consistency of, say, OLS in this context but this is ignored for the moment. It is clear that there will exist a consistent method of estimating the parameters and therefore that they are identified. The identification could be shown more formally by recursively substituting in for $p_{i,t-1}$ in (6) and by deriving its reduced form in terms of the past hazards. It could then be shown directly that the age or cohort restrictions discussed would allow their identification from repeated measures of p_{it}.

always be available because the reduced form of (9) for $p_{i,t-1}$ is a polynomial in the lagged values of the X_{it}. In the case of a single non-time-varying X_i, for example, it can be shown that

$$p_{i,t-1} = \sum_{j=0}^{t-1} \alpha_{tj} X_i^j, \tag{10}$$

where each α_{tj} involves only β and γ. If some of the elements of X_{it} are time varying, this temporal variation provides additional instruments for $p_{i,t-1}$ as well.[15]

The econometric model I will estimate in the following sections is based upon these general points but modifies them to take into account the categorical nature of the dependent variable when individual data are used. The model that results when normality is assumed for the relevant error terms is a generalization of the well-known probit model. In place of (9) let us posit the model:

$$P_{it}^* = X_{it}\theta_1 + (X_{it}\theta_2)P_{i,t-1} + \varepsilon_{it}, \tag{11}$$

$$P_{it} = \begin{cases} 1, & \text{if } P_{it}^* \geqq 0, \\ 0, & \text{if } P_{it}^* < 0, \end{cases} \tag{12}$$

where upper case P replaces lower case p to distinguish individual binary choices from population probabilities. The error ε_{it} is assumed to have a unit normal distribution. Given (11)–(12), the hazards are now:

$$\mu_{it} = F[X_{it}\theta_1], \tag{13}$$

$$\lambda_{it} = 1 - F[X_{it}(\theta_1 + \theta_2)], \tag{14}$$

where F is the normal distribution function.

[15]These means of estimation and identification of models for repeated cross-sectional data are similar to those presented by Deaton (1985) and Heckman and Robb (1985) in different contexts. Deaton demonstrated that a life-cycle model could be estimated from aggregate data available by age and year by constructing cohort profiles and differencing out fixed effects. Heckman and Robb showed that the effect of training on earnings could be estimated from individual or grouped data from repeated cross-sections. Both papers make the point that identification and estimation by means of grouping and aggregation are equivalent to instrumental variables methods. The discussion I have provided here generalizes their points by considering models with categorical dependent variables and, models with a more dynamic structure. The methods I have outlined also make clear that actual grouping of the data need not be done and that the variation and information in the underlying individual data can be efficiently utilized as part of the procedure.

Although (11) could be estimated directly by probit once an instrument for $P_{i,t-1}$ is developed, I will instead estimate the model by maximum likelihood using a computationally simple and intuitively appealing procedure. Note first that the probability that an individual is married at age t is:[16]

$$\begin{aligned} q_{it} &= \mathrm{Prob}(P_{it}=1) \\ &= \mathrm{Prob}[X_{it}(\theta_1+\theta_2)+\varepsilon_{it} \geqq 0]\,\mathrm{Prob}(P_{i,t-1}=1) \\ &\quad + \mathrm{Prob}[X_{it}\theta_1+\varepsilon_{it} \geqq 0]\,\mathrm{Prob}(P_{i,t-1}=0) \\ &= F[X_{it}(\theta_1+\theta_2)]q_{i,t-1} \\ &\quad + F[X_{it}\theta_1](1-q_{i,t-1}). \end{aligned} \tag{15}$$

Furthermore, the same relationship holds for $t-1$ and all prior t:

$$q_{i,\tau} = F[X_{i\tau}(\theta_1+\theta_2)]q_{i,\tau-1} + F[X_{i\tau}\theta_1](1-q_{i,\tau-1}), \quad \tau = 1, \ldots, t-1. \tag{16}$$

where $q_{i0}=0$. If (16) were recursively solved for $q_{i,t-1}$ and inserted into (15), it would be clear that the probability that individual i is married at age t in a particular cross-section should be a function not only of the current X_{it} but also of all lagged $X_{i\tau}$, since the latter determine the path of the stocks of married and unmarried individuals over time and hence the stocks just prior to t.[17]

Rather than directly estimate such a cumbersome equation, maximum likelihood estimates of the model can easily be obtained by iterative estimation of (15) and application of (16). For a given set of starting values of θ_1 and θ_2, a set of $\hat{q}_{i,t-1}$ are calculable for all observations by recursively solving (16) from q_{i1} to $q_{i,t-1}$; using these calculated values, (15) is estimable by a weighted probit procedure using the $\hat{q}_{i,t-1}$ as weights; the resulting estimates of θ_1 and θ_2 can then be reapplied to (16), etc. until convergence.[18]

[16]Eq. (15) imposes independence of the error terms for simplicity but this is not required for the consistency of the procedure because the instruments for the q_{it} will be based upon independent samples.

[17]The simplicity of (15) – which is just a means of integrating out all possible prior sequences of marriage and non-marriage – is partly based upon the Markov assumption. In a semi-Markov model, for example, all possible durations in the two states would also have to be integrated out. This would be computationally a bit more cumbersome but would not alter the estimation method or the identifiability of the parameters.

[18]This procedure bears some resemblance to the EM estimation method. Note too that the standard errors which are generated from the probit step are incorrect since they assume the $\hat{q}_{it}$ to be non-stochastic. Correct standard errors are obtained only after the ML parameter estimates are obtained, by calculating the outer product of numerically calculated individual scores.

It should be stressed that estimation of dynamic models with cross-sectional data does not necessarily generate as efficient estimates of the parameters of the two hazard functions as would a comparable panel data set (i.e. one with the same sample size, without attrition, etc.). It is unlikely that estimates of the determinants of transition probabilities can be very efficient with data in which individual transitions cannot be observed. However, efficient estimates of the combined effect of the X_{it} on p_{it} may well be relatively efficient even though the separate estimates of effects on the two hazards are not. Given the superiority of the CPS over available panels for measuring p_{it}, cross-sectional data may well be the best data to use to measure its determinants rather than those of the hazards.

Estimates of the benefit-sum coefficients from models estimated on the 1985 cross-section alone are shown in table 8.[19] With a single cross-section, the model is implicitly just one with a high-order lag structure with non-linear restrictions across the coefficients. The X vector used in these equations is shown in table A.2 in the appendix, along with the rest of the coefficients. The vector is the same as that in the previous section except for the omission of some of the higher-order age splines, reflecting the high-order age non-linearity introduced implicitly by the lag structure.

The results indicate coefficient signs in the expected direction but which are almost always low in significance level. The coefficient θ_1 represents the effect of the benefit sum on the marital formation rate and is negative for all groups and for female headship as well as marital status, but is significant only for black males. The effect of the benefit sum on the dissolution rate is found by summing the θ_1 and θ_2 coefficients and reversing the sign. It can be seen that the effect is positive for all groups and dependent variables except for black males, where it is essentially zero. Table 8 also shows the chi-squared statistics for the joint significance of both parameters together (critical value$=4.61$ at the 90 percent level). Only the parameters for black males are jointly significant at the 90 percent level, although the estimates for female headship are significant at the .80 percent level. However, the other groups have insignificant effects. This pattern of low significance levels is the same as that obtained in the previous section.

At least two conclusions can be drawn from this section. First, the pattern of results found in the previous section is the same as that found here – namely, coefficients usually of the expected sign but usually insignificant as well – and hence the misspecification of the relationship there does not appear to have seriously affected the results. This may simply result from a

[19] The sample used to estimate this model is slightly smaller than that used in the previous section because of the requirement that data on the benefit sum be available for all periods back to age 16 for each individual. Since AFDC guarantee data are available only back to 1955, no individuals older than 46 ($1985-1955+16=46$) can be included. An age-55 cutoff was used in the previous section.

Table 8

Probit coefficients on benefit-sum dynamic model, 1985 sample.[a]

	Marital status	Female headship
White females		
θ_1	−0.094	−0.062
	(0.097)	(0.067)
θ_2	0.048	−0.245
	(0.139)	(0.551)
χ^2	1.94	3.28
Black females		
θ_1	−0.036	−0.057
	(0.049)	(0.053)
θ_2	−0.125	−0.427
	(0.229)	(0.602)
χ^2	1.98	3.52
White males		
θ_1	−0.068	
	(0.098)	
θ_2	−0.004	
	(0.139)	
χ^2	2.04	
Black males		
θ_1	−0.209*	
	(0.083)	
θ_2	0.211	
	(0.225)	
χ^2	13.92*	

Note: Standard errors in parentheses.
*Significant at the 10 percent level.
[a]Other coefficients shown in the appendix, table A.2.

high correlation of state benefit variables over time, which would imply that the benefit level at a point in time is a sufficient proxy for the lagged benefit levels as well. Second, it would appear important to expand the model developed in this section with more cross-sections and more time-varying variables, which may improve the efficiency of the estimates and generate more reliable results.

6. Summary

This paper has been concerned with the reduced-form adjusted correlations between the marital status of men and women in the United States and measures of the generosity of the welfare system. Although this issue has been examined before, several new findings have emerged. (1) The cross-sectional estimate of the effect of welfare payments on marital status and female headship appears to have grown over time for both women and men. (2) A consistent pattern of negative effects of payments on marital status and headship appears in the results, although rarely attaining conventional significance levels. (3) In 1985, the estimates for female headship are significant and those for marital status of women are negative in sign but not significant. (4) The evidence on males, who have been rarely examined in the past, shows stronger effects than for females. Marital-status effects are strong and significant for black males and significant for low-wage white males.

Why the estimate of benefit effects has grown over time and why, therefore, it appears stronger in this study than in past work based upon earlier years, has not been explained. An obvious potential explanation is that there are sufficiently long lags in the response of marital behavior to the welfare system that it takes several years for its effect to show up.

Several directions of work are suggested by these results. First, since they are sufficiently different from the literature it would seem important to confirm them with other data sets and with more cross-sections of the CPS. The latter seems particularly important because it would allow the estimation of models with fixed state effects. Second, it would seem necessary to move beyond the reduced-form models estimated here to structural models of marital status to determine more concretely whether the effects being found can sensibly be interpreted as welfare effects within a proper behavioral model. Given the complexity of the marital formation and dissolution process, this is a difficult and challenging task.

Appendix

Table A.1

Means of characteristics in 1969 and 1977.

	Female				Male			
	White		Black		White		Black	
	1969	1977	1969	1977	1969	1977	1969	1977
Age	37.57	36.44	36.27	34.99	38.06	36.51	36.51	34.38
Education	11.31	11.79	9.82	10.93	11.45	12.09	9.40	10.62
Live in metropolitan area	0.65	0.57	0.72	0.71	0.64	0.55	0.72	0.70
State manufacturing wage	3.22	3.43	3.07	3.31	3.21	3.44	3.08	3.31
Northeast	0.27	0.22	0.21	0.17	0.26	0.22	0.20	0.16
Midwest	0.30	0.28	0.20	0.20	0.30	0.28	0.21	0.19
South	0.27	0.27	0.52	0.54	0.28	0.27	0.52	0.56
West	0.16	0.24	0.07	0.08	0.16	0.24	0.07	0.10
Real benefit sum/100	6.67	6.52	6.23	6.00	6.67	6.52	6.22	5.98

Notes: See table 1.

Table A.2

1985 probit coefficients in dynamic models for marital status.

	Females		Males	
	White	Black	White	Black
θ_1				
Sum/100	−0.094	−0.036	−0.067	−0.209*
	(0.097)	(0.049)	(0.098)	(0.083)
Log age	9.781*	4.456*	7.687*	5.113*
	(0.996)	(0.425)	(0.937)	(0.739)
Max[0, log(age − 25)]	−9.030*	−1.881*	−2.307	−3.367*
	(1.280)	(0.643)	(1.918)	(1.129)
Education	−0.284*	0.104*	−0.136*	0.029
	(0.038)	(0.019)	(0.031)	(0.023)
Metropolitan residence	−0.159	−0.185	−0.348*	0.086
	(0.128)	(0.113)	(0.122)	(0.121)
Manufacturing wage	−0.310*	0.130	0.015	0.063
	(0.143)	(0.107)	(0.141)	(0.124)
Northeast	−0.283	−0.381*	−0.468*	−0.025
	(0.229)	(0.130)	(0.229)	(0.181)
Midwest	0.202	−0.616*	−0.119	−0.355*
	(0.211)	(0.133)	(0.197)	(0.176)
West	0.406*	–	−0.163	0.606*
	(0.232)		(0.230)	(0.232)
Intercept	−25.057	−15.959	−22.189	−16.707
θ_2				
Sum/100	0.048	−0.125	−0.004	0.211
	(0.139)	(0.229)	(0.139)	(0.225)
Log age	0.113	−1.534	−3.834*	−1.489
	(1.046)	(0.964)	(1.640)	(0.326)
Education	0.316*	−0.106*	0.165*	0.034
	(0.044)	(0.053)	(0.037)	(0.056)
Metropolitan residence	−0.199	−0.916	0.150	−0.669*
	(0.186)	(0.841)	(0.177)	(0.312)
Manufacturing wage	0.389*	−24.507*	−0.132	−0.159
	(0.207)	(9.339)	(0.210)	(0.328)
Northeast	0.297	11.897*	0.580*	0.456
	(0.335)	(6.230)	(0.374)	(0.449)
Midwest	−0.128	21.569*	0.444	0.776*
	(0.298)	(8.190)	(0.286)	(0.468)
West	−0.566*	–	0.288	−1.078*
	(0.319)		(0.318)	(0.051)
Intercept	−6.779	70.330	0.134	5.360

Note: Standard errors in parentheses.
*Significant at the 10 percent level.

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