Panel Data Models and Transitory Fluctuations in the Explanatory Variable

by

Terra McKinnish
Department of Economics, UCB 256
University of Colorado-Boulder
Boulder, CO 80309-0256
mckinnis@Colorado.edu
303-492-6770

Abstract

This paper demonstrates that fixed-effects and first-differences models often understate the effect of interest because of the variation used to identify the model. In particular, the within-unit time-series variation often reflects transitory fluctuations that have little effect on behavioral outcomes. The data in effect suffer from measurement error, as a portion of the variation in the independent variable has no effect on the dependent variable. Two empirical examples are presented: one on the relationship between AFDC and fertility and the other on the relationship between local economic conditions and AFDC expenditures.

Coefficient estimates from first-differences, long-differences, and fixed-effects models are compared. These estimates differ in ways that are consistent with the presence of measurement error. Results from the analysis of AFDC expenditures, a dependent variable likely to respond to long-term changes in economic conditions, are compared to an analysis of UI Expenditures, a dependent variable likely to respond to short-term changes in economic conditions. Further analysis considers instrumental variables approaches and the use of lagged effects models.

I. Introduction

This paper explores the use of panel data models in cases in which the effect of interest may not be well-estimated using the within-unit time-series variation in conditions. While it is well known that panel data models can exacerbate attenuation bias due to measurement error, it is not widely appreciated that these results apply to a broader set of circumstances. Even though the independent variable may not be measured with error, per se, it still may mismeasure the factor that truly affects the outcome of interest. For example, we may know the exact value of state welfare benefits from administrative records, but not all of the variation in these benefit levels will necessarily influence behavior. In particular, we expect many outcomes to respond differently to short-term and long-term variation in conditions. This differential effect of long-term and short-term variation can generate the same bias as "true" measurement error.

This paper investigates two empirical examples. The first studies the effect of benefit levels in the Aid to Families with Dependent Children (AFDC) program on birth rates. The second studies the effect of local economic conditions on AFDC expenditures. In both of these cases, the outcome of interest is likely more responsive to sustained changes in the explanatory variable than transitory fluctuations. For both examples, we first compare estimates from fixed-effects, first-differences and long-differences models to confirm the pattern that is consistent with measurement error and, therefore, these differential effects.

The estimates of the effect of local economic conditions on AFDC expenditures are also compared to results using Unemployment Insurance (UI) expenditures as the dependent variable. Both AFDC and UI are income maintenance programs. As a result, we expect expenditures in both programs to increase during economic downturns. Because, however, AFDC is a program with relatively long-term receipt and UI is a program with short-term receipt, we expect UI expenditures to be more sensitive to short-term variation in economic conditions than AFDC participation. In this case, analytic results show that the patterns of coefficient estimates from differences and lagged effects models should differ systematically between the AFDC regressions and the UI regressions. These differential patterns are confirmed in the data.

For both examples, we attempt to obtain consistent estimates of the effect of interest. In the birth rate example, in the absence of external instruments, lagged internal instruments are considered. We demonstrate that lagged instruments will tend to be "weak" and those that are sufficiently correlated with the independent variable will be most sensitive to correlation in the measurement error.

In the welfare expenditures example, the decline of the steel industry is used as an instrument that predicts long-term changes in local economic conditions. A comparison of OLS and IV estimates from differences models and lagged effects models illustrates the differential effect of transitory and sustained changes in economic conditions. This exercise also clarifies the differences in estimates from models with and without lagged effects that have been highlighted in many recent studies.

II. Measurement Error in Panel Data

This section reviews the basic analytical measurement error results for panel data from Griliches and Hausman (1986). Consider the following model:

(1) $$Y_{it} = \alpha_i + \beta Z_{it} + \varepsilon_{it}$$

(2) $$X_{it} = Z_{it} + \nu_{it}$$

For example, in the first empirical application, Y_{it} represents the birth rate in state i at time t, X_{it} represents the welfare benefit, Z_{it} represents the sustained component of the welfare benefit and ν_{it} the transitory component of the welfare benefit. The term "measurement error" is not being used in the conventional manner, because we know the precise value of the AFDC benefit. But it is still the case that the benefit level (*X*) may mismeasure the factor that truly affects fertility, the sustained component of AFDC benefits (*Z*). The results that follow assume classical measurement error, in which ν_{it} and ε_{it} are white noise, both uncorrelated with Z_{it}. It is also assumed that a stationary process generates Z_{it}.

If we consider a differences model regressing Y on the observed variable, X:

(3) $$Y_{it} - Y_{it-j} = \beta_d (X_{it} - X_{it-j}) + (\varepsilon'_{it} - \varepsilon'_{it-j}),$$

the asymptotic result is:

(4) $$\text{plim}\left(\hat{\beta}_d\right) = \beta\left(\frac{\sigma_Z^2 (1-\rho_j)}{\sigma_Z^2 (1-\rho_j) + \sigma_\nu^2}\right),$$

where ρ_j is the correlation between Z_{it} and Z_{it-j}. Consider the first-differences model in which $j=1$. If *Z* is highly correlated over time and two observations of *X* from adjoining time periods are differenced, most of the information about *Z* will be eliminated, leaving primarily variation due to the noise component, *v*. If Z_{it} has a declining correlogram, then $\rho_j < \rho_1$, and estimates obtained using longer differences will be less inconsistent than estimates using shorter differences. By taking differences of observations that are less correlated with each other, the variance of the signal is increased relative to the noise. If Z_{it} has a declining correlogram, $\text{plim}\left(\hat{\beta}_d\right)$ converges to the well-known cross-sectional result $\beta\sigma_\nu^2 / (\sigma_\nu^2 + \sigma_Z^2)$ as *j* becomes large.

If the fixed-effects model is used:

(5) $$Y_{it} - \overline{Y}_i = \beta_{fe}(X_{it} - \overline{X}_i) + (\varepsilon'_{it} - \overline{\varepsilon}'_i) \ .$$

then:

$$\text{(6)} \quad \operatorname{plim}\left(\hat{\beta}\right) = \beta\left(\frac{\sigma_z^2 - \frac{1}{T^2}\left[T + 2\sum_{k=1}^{t-1}(T-K-1)\rho_j\right]}{\sigma_z^2 - \frac{1}{T^2}\left[T + 2\sum_{k=1}^{t-1}(T-K-1)\rho_j\right] + \left(\frac{T-1}{T}\right)\sigma_v^2}\right),$$

Griliches and Hausman (1986) summarize a number of general results comparing the fixed-effects, first differences, and long-differences models under the assumption that Z_{it} is independent of v_{it} and stationary with a declining correlogram. They show that for T>2:

(a) The inconsistency of fixed-effects and long-differences is less than that of first-differences.
(b) The inconsistency of the (T-1) long-differences estimator is less than that of the fixed-effects estimator.
(c) The relative inconsistency of differences shorter than (T-1) and fixed effects depends on more specific characteristics of the correlation structure.

These analytical results suggest that if the dependent variable is more responsive to long-term changes in the independent variable than transitory fluctuations, we should observe a distinct pattern when we compare first-differences, fixed-effects and long-differences estimates. First, all estimates should be attenuated towards zero. The attenuation should be greatest in the first-differences estimates and reduced as longer differences are used. The fixed-effects estimates should be larger in magnitude than the first-differences estimates but there is no prediction about the relative magnitude of the fixed-effects and long-differences results. [1]

It is important to note that other forms of misspecification can also produce a similar pattern. If there is a time-varying unobserved confounder and this unobserved confounder is more highly correlated over time than the observed independent variable, this generates the same

[1] This pattern is predicted even without all of the assumptions of the classical error model. They hold even if the measurement error is correlated over time, as long as the correlation of the measurement error is smaller than the correlation of the signal. They can also hold for the case in which *Z* is not stationary, if *v* follows a stationary process. See McKinnish (1999) for details.

pattern in the fixed-effects, first-differences and long-differences models.[2] In this case, the bias will be smallest for first-differences, since the unobserved characteristic will largely be differenced out, but the bias will be larger for differences over longer periods that allow more change in the unobserved characteristic.

III. Description of Empirical Examples

A. AFDC Benefits and Birth Rates

A simple economic model suggests that any government transfer program that lowers the cost of supporting a child should increases birth rates. This section examines the effect of the generosity of the AFDC program on birth rates of young women, using a panel of state-level data on age-specific birth rates and AFDC benefit levels from 1973-92. A number of studies have used panel data models to study the relationship between welfare and fertility (Jackson and Klerman,1994; Matthews, Ribar and Wilhelm,1997; Clark and Strauss, 1998; Rosenzweig, 1999; Hoffman and Foster, 2000; Argys, Averett, and Rees, 2000). Similar studies have examined female headship, an outcome which combines fertility and marital decisions (Moffitt,1994; Hoynes, 1997). Panel data studies typically have found positive, but modest, effects of benefits on fertility. The two studies that found large effects (Strauss and Clark, 1998; Rosezweig, 1999) arguably used specifications that identify the effect from longer-term variation in benefits.[3]

As childbearing is a commitment to consumption not only in the period of birth but to future periods as well, we expect fertility decisions to be relatively non-responsive to transitory changes in welfare generosity. This raises the question of what variation will be available to

[2] Heckman and Hotz (1996) proposed a popular test of the fixed-effects assumption in differences models, that of including a lagged value of the dependent variable in the model. If unobserved confounders been differenced out, the coefficient on the lagged dependent variable should be zero. McKinnish (2005) shows that measurement error generates the same failure of the Heckman-Hotz test as omitted variables, so the Heckman-Hotz test cannot be used to distinguish between these forms of misspecification.

[3] Clark and Strauss used an IV strategy and Rosenzweig averaged benefits across years, both of which act to "smooth" out transitory changes in benefits. Hoffman and Foster (2000) show that the large effects estimated by Rosenzweig are sensitive to specification.

identify the effect of interest in panel data models. Figure 1 graphs the nominal AFDC benefit for a family of four over time in three states: Massachusetts, Montana and Mississippi. This benefit level is guaranteed to a family of four with no additional income, a legislated parameter of the program that is known with certainty. There are clearly permanent cross-sectional differences between the three states in their general benefit level. Looking within each state over time, the graph shows that that Massachusetts's time trend contains frequent small fluctuations. Mississippi's time trend, on the other hand, contains no such fluctuations. The state changes benefits only twice in the period from 1970-94. Montana's time trend shows both transitory and sustained variation. Figure 1 suggests that the cross-sectional differences in benefit level eliminated by differencing are fairly permanent, but much of the time-series variation remaining once the cross-sectional effects are eliminated reflects transitory fluctuations that likely have little bearing on fertility rates.[4]

Data used in this example are counts of births by state, year, age and race of mother for 1973-92 obtained using the Detailed Natality Files from the National Center for Health Statistics. This paper reports the results for white women ages 20-24.[5] Denominators for the birth rates are obtained from various US Census files. Births that occur in the first 9 months of a year are attributed to the previous year. The independent variable of interest is the AFDC benefit for a family of four with no additional income.[6] Measures of real earnings per capita are obtained from the Bureau of Economic Analysis' (BEA) Regional Economic Information System (REIS).[7]

[4] Moffitt (1994) and Rosenzweig (1999) have also noted that using year-to-year variation in AFDC benefits is likely to understate the effect of interest. Keane and Wolpin (2002) estimate a structural model of welfare effects that assumes women are forward-looking and therefore distinguishes between transitory and permanent benefit changes. They, however, are not able to control for state fixed-effects in the model.

[5] The results for white women ages 15-19 and ages 25-29, as well as black women ages 20-24 and ages 25-29, all display the same patterns shown here for white women ages 20-24. The sole demographic group that does not conform to the expected pattern is black teens. See McKinnish (1999) for estimates.

[6] Obtained from various years of *The Green Book* publication of the U.S. House Ways and Means Committee and provided in electronic form by Robert Moffitt.

[7] Earnings are divided by the population age 10 and older in order to avoid endogenous effects of changes in the

The differences regression equation is:

(7) $$\Delta_j BirthRate_{st} = \beta_1 \Delta_j AFDC_{st} + \beta_2 \Delta_j Earnpc_{st} + Year_t \beta_3 + \mu_{st}\,.$$

where Δ_j indicates a j-year difference, $BirthRate_{st}$ is the logged birth rate for white women ages 20-24 in state *s* at year *t*, $AFDC_{st}$ is the logged real AFDC benefit and $Earnpc_{st}$ is the logged real earnings per capita. $Year_t$ is a vector of year indicators. The fixed-effects model is therefore:

(8) $$BirthRate_{st} = \beta_0 + \beta_1 AFDC_{st} + \beta_2 Earnpc_{st} + Year_t \beta_3 + State_s \beta_4 + \varepsilon_{st}\,.$$

where $State_s$ is a vector of state indicator variables.

B. Local Economic Conditions and Welfare Expenditures

The second empirical example examines the relationship between AFDC program participation and local labor market conditions, using county-level data on earnings and AFDC expenditures from 1969-93. The earlier research on this topic (Fitzgerald, 1995; Miller and Sanders,1997; and Hoynes, 2000) estimated the relationship between contemporaneous economic conditions and AFDC participation using micro-level data. The more recent studies (Bartik and Eberts, 1999; CEA,1999; Figlio and Ziliak, 1999; Wallace and Blank, 1999; Ziliak, Figlio, Davis and Connolly, 2000; Mueser et al., 2000; and Blank, 2001) have used aggregate caseload data and include lagged values of economic conditions in their specification. The general finding in this literature is that panel data models that only use contemporaneous economic conditions find a very modest relationship, but those models that add up multiple lagged effects find much larger effects. The relationship between the lagged effects specifications and the specifications used in this paper will be pursued in a later section of the paper.

The basic regression model is:

---

birth rate. AFDC and earnings variables are deflating using the July CPIU, base year 82-84.

(9) $\Delta_j AFDC_{ist} = \beta_0 + \beta_1 \Delta_j Earnings_{ist} + \beta_2 \Delta_j Pop_{ist} + (State_s * Year_t)\beta_3 + \varepsilon_{ist}$,

where Δ_j operator again indicates a j-year difference, $AFDC_{ist}$ is logarithm of real AFDC expenditures for county *i* in state *s* and time *t*; $Earnings_{ist}$ is the logarithm of aggregate real earnings; Pop_{ist} is the logarithm of county population; $(State_s * Year_t)$ is the interaction of state and year dummy variables.

The fixed-effects model is:

(10) $AFDC_{ist} = \beta_0 + \beta_1 Earnings_{ist} + \beta_2 Pop_{ist} + (State_s * Year_t)\beta_3 + County_i \beta_4 + \varepsilon_{ist}$

Annual measures of county earnings, AFDC expenditures, and county population are obtained from the BEA REIS data from 1969-93. AFDC expenditures are a function of benefit levels and caseload. Because, however, benefit levels vary at the state level, the state-year effects net out the effect of benefits. The within-state variation in expenditures at the county level should largely reflect changes in caseloads.

IV. Fixed-Effects and Differences Results

Results from the fixed-effects and differences models specified in equations (7)-(10) are reported in Table 1. The first column reports results from the regression of birth rates on state AFDC benefit levels. The second column reports results from the regression of county AFDC expenditures on county earnings. Results are reported for both models for fixed-effects, first-differences, 3-year long-differences, 5-year long-differences and 7-year long-differences. For the AFDC expenditures regressions, the larger sample size and longer time series also allow the estimation of a 20-year long-differences model. For both empirical examples, the results display the expected pattern. Consistent with the analytical predictions from Section II, the first-differences coefficient estimates are smaller in magnitude than the fixed-effects estimates. Also consistent with the analytical predictions, the coefficient estimates increase in magnitude with the length of the differences.[8] The change in the coefficients is substantial, in both cases increasing almost an order of magnitude between the first-differences and the 7-year long-differences.[9]

In both columns of Table 1, the fixed-effects estimate is larger in magnitude than the 5-year differences estimate, and in the fertility example, is larger than even the 7-year differences estimate. These results in Table 1 provide some guidance about selection of panel data models. If the dependent variable is relatively unresponsive to transitory fluctuations in the explanatory variable, then first-differences models perform poorly. In these examples, the fixed-effects estimates and the long-differences estimates are similar in magnitude, but because much of the data is eliminated with long-differences models, their precision is considerably less than the fixed-effects estimates. This suggests that in most cases fixed-effects is the preferred estimator.

There are some important caveats to the above conclusion. First, the analytical results discussed in section II indicate that we can only be certain that the fixed-effects estimate will be less inconsistent than the first-differences model. In other applications, long-differences could be sufficiently less inconsistent than fixed-effects to warrant the loss of precision. Second, it must be remembered that all of the estimates in Table 1, including the fixed-effects estimates, suffer from attenuation bias. The bias is simply less severe than for first-differences. Finally, the performance of the fixed-effects estimator will depend in part on the length of the available time series.

V. A Comparison of AFDC and UI Results

[8] Baker, Benjamin and Stanger (19990 document a similar pattern in analysis of minimum wage laws. They also consider differences of varying length and find that the magnitude of the effect is larger for longer differences.

[9] One concern might be that the sample is shrinking as longer differences are used. These same patterns are observed even if the sample is restricted to be the same across all models. It is, however, unclear that the restricted sample estimates are preferred. To illustrate this, consider the fact that data from 1980-90 on a cross-section of N observations can be used to estimate a 10-year long-difference model with sample size N or a first-differences model with sample size 10N, but both estimates will use the variation from 1980 to 1990. If the sample is restricted to be the "same", so that both models are estimates using a sample of size N, the first-differences model will be estimated using the change from 1989-1990, and the pre-1989 variation will be removed from the model.

This paper argues that the results in Table 1 are due to the differential effect of long-term and short-term changes in conditions, rather than just omitted variable bias. A useful exercise to support this claim is to compare the results from the analysis of AFDC expenditures with results from an analysis of Unemployment Insurance (UI) expenditures. Like AFDC, UI is an income maintenance program, but one that is specifically designed to act as a buffer to business cycle fluctuations. The program typically provides qualified recipients 50-70% of their previous wages for up to 26 weeks. As the UI program is designed to sustain workers through temporary job losses, UI expenditures should be quite sensitive to transitory fluctuations in economic conditions. Because of limits on the duration of benefits, it is unlikely that UI expenditures are as responsive to long-term changes in economic conditions as AFDC expenditures.[10] As a result, there should be a very different pattern predicted for the empirical results.

A. Analytical Results

This section reports analytical results for the a model in which the dependent variable is most responsive to long-term changes in the explanatory variable and for a model in which the dependent variable is most responsive to short-term changes in the explanatory variable. Analytical results for differences models and for differences models with a lagged effect will be discussed.[11]

Consider a slight variation of the measurement error model described in equations (1) and (2):

(7) $$Y_{it} = \alpha_i + \beta Z_{it} + \varepsilon_{it} ,$$

(8) $$X_{it} = Z_{it} + v_{it} ,$$

[10] Making a similar argument, Black, Daniels and Sanders (2002) show that UI expenditures were relative non-responsive to the large shocks to the coal economy during the 1970's and 1980's.
[11] Proofs of all asymptotic results appear in Appendix A.

(9) $$Z_{it} = \rho Z_{it-1} + \theta_{it},$$

and:

(10) $$v_t = \delta v_{t-1} + \mu_t,$$

where $0 < \delta < 1$ and $0 < \rho < 1$. If $\delta < \rho$, then *Y* is a function of the more highly correlated component of the observed variable. If $\delta > \rho$, then *Y* is a function of the less correlated component of the observed variable.[12]

For the differences model described in equation (3), the asymptotic result is now:

(11) $$\text{plim}\left(\hat{\beta}_d\right) = \beta\left(\frac{\sigma_z^2(1-\rho^j)}{\sigma_z^2(1-\rho^j) + \sigma_v^2(1-\delta^j)}\right)$$

If $\delta < \rho$, the anticipated pattern in the differences estimates remains the same as described in Section II. The first differences will be the most attenuated, as much of the signal is differenced out. Longer differences will capture proportionally more signal and be less attenuated.

If, instead, $\delta > \rho$, so that the dependent variable responds to the more transitory fluctuations in *X*, the reverse is true. The first-differences estimates will be least attenuated, as the signal to noise ratio will be greatest. The estimates should decrease in magnitude as longer-differences are used and the signal to noise ratio decreases.

Now consider a differences regression model that includes a lagged effect of the independent variable:

(12) $$Y_{it} - Y_{it-j} = \beta_d(X_{it} - X_{it-j}) + \beta_l(X_{it-1} - X_{it-j-1}) + (e_{it} - e_{it-j})$$

For j=1:

[12] Likewise, we could state that $\delta < \rho$ and compared the case where Y is a function of Z to the case in which Y is a function of v. It will simply be easier to present the analytical results that follow using this representation of the model.

$$\text{(13)} \qquad \text{plim}(\hat{\beta}_l) = \frac{2\beta\sigma_v^2\sigma_z^2[(1-\rho)(1-\delta)^2-(1-\delta)(1-\rho)^2]}{4[\sigma_z^2(1-\rho)+\sigma_v^2(1-\delta)]^2-[\sigma_z^2(1-\rho)^2+\sigma_v^2(1-\delta)^2]^2}.$$

And, for j>1:

$$\text{(14)} \quad \text{plim}(\hat{\beta}_l) = \frac{2\beta\sigma_v^2\sigma_z^2[(1-\delta^j)(2\rho-\rho^{j+1}-\rho^{j-1})-(1-\rho^j)(2\delta-\delta^{j+1}-\delta^{j-1})]}{4[\sigma_z^2(1-\rho^j)+\sigma_v^2(1-\delta^j)]^2-[\sigma_z^2(2\rho-\rho^{j+1}-p^{j-1})+\sigma_v^2(2\delta-\delta^{j+1}-\delta^{j-1})]^2}.$$

It can quickly be shown that, in both cases, $\text{plim}(\hat{\beta}_l)$ has the same sign as β if $\delta < \rho$ and that $\text{plim}(\hat{\beta}_l)$ has the sign of $-\beta$ if $\delta > \rho$. Furthermore, it will generally be the case that $\left|\text{plim}(\hat{\beta}_{l,j+1})\right| > \left|\text{plim}(\hat{\beta}_{l,j})\right|$, regardless of the relative magnitude of δ and ρ.[13]

The above results make some important predictions for differentiating between a dependent variable that responds to transitory variation compared to a dependent variable that responds to sustained changes. In the case of the dependent variable that responds to sustained changes, the estimates from the differences model and the estimates of lagged effect should be of the same sign. Furthermore, both sets of estimates should increase in magnitude with longer differences. In the case of the dependent variable that responds to transitory variation in the explanatory variable, the coefficient estimates on the lagged effect should be the opposite sign of the coefficient estimates from the differences model. Furthermore, the estimates from the differences model should *decrease* in magnitude with the length of the differences, while the estimates of the lagged effect should increase in magnitude with the length of the differences.[14]

B. Empirical Results

[13] See Appendix for further details. Result concerning relative magnitudes of estimates from *j* and *j+1* models generated from grid search.

[14] Klerman and Haider (2001) demonstrate that caseload models, even those including multiple lags of economic conditions, are also likely biased because they fail to take into account the fact that caseloads are the net result of flows on and off the program. They show that a simple stock-flow model of welfare caseloads indicates that any regression analysis of the stock requires a full lag structure, and interactions of those lags, that is equal in length to the longest time people spend on the welfare program. Dealing with such a complicated lag structure is beyond the scope of this paper. One way to interpret their analysis of welfare case flows is that lagged values of economic conditions matter because they affect entry and exit to and from welfare in previous periods. Lagged economic conditions therefore affect the composition of the current welfare caseload and the extent to which the current caseload will respond to changes in economic conditions. This suggests that detailed information on caseload composition could potentially substitute for the lagged effects.

The empirical results for this exercise are reported in Table 2. The first two columns report results from the AFDC expenditures regressions. The first column reports the results from the differences model described in equation (9). As was the case in Table 1, we see that the magnitudes of the estimates increase with the length of the differences.[15] In second column reports the results from adding a single lagged effect to the differences model, as described in equation (12). We observe the same pattern seen in column 1. The estimates increase in magnitude with the length of the differences.

The results for UI expenditures are reported in columns 3 and 4. As was the case with AFDC expenditures, the differences estimates in the third column suggest that UI expenditures decrease when economic conditions improve. In this case, however, the estimates *decrease* in magnitude with the length of the differences, as implied by the analytic results in equation (11). In column 4, the coefficient on the lagged effect is now positive, as implied by the analytic results in equation (12). Both of the analytical predictions are born out in the data. When a dependent variable is responsive to shorter-term changes in the explanatory variable, the effects are largest for short rather than long-differences, and the sign of the lagged effect is the opposite of the contemporaneous effect.

The results in this section emphasize that the performance of different panel data estimators depends crucially on the type of variation in the explanatory variable that generates responses in the dependent variable.

VI. Lagged Internal Instruments

The results in Table 1 are consistent with attenuation bias, which suggests that even the largest estimates in Table 1 understate the magnitudes of the effects of interest. The most

[15] The small differences in estimates between the first column of Table 2 and the second column of Table 1 are due to small reductions in sample so that each estimate in a row of Table 2 is estimated using the same sample.

common correction for measurement error is to use IV estimation with an instrument that is correlated with the signal but independent of the measurement error. Unfortunately, it is difficult to find such an instrument for AFDC benefits. Griliches and Hausman (1986) point out that in the absence of an appropriate external instrument, it is possible to use lagged values of the independent variable as instruments to correct the measurement error bias.

Consider first the basic differences model from equation (3):

$$Y_{it} - Y_{it-j} = \beta(X_{it} - X_{it-j}) + (\varepsilon_{it} - \varepsilon_{it-j})$$

If the measurement error, *v*, is uncorrelated over time, then any value of *X* other than X_{it} and X_{it-j}, or any function of these values, is a valid instrument for X_{it} -X_{it-j}.[16]

This section provides evidence that these instruments have not worked well in practice because many of the theoretically valid instruments are in fact weak instruments. The lagged values of *X* produce consistent estimates under the appropriate assumptions about the structure of the measurement error. But if the lagged values of *X* are only mildly correlated with X_{it}-X_{it-j}, the IV estimator can have large bias in small samples and can be extremely imprecise (Bound, Jaeger and Baker, 1995). The intuition of the problem is fairly simple. If the signal is highly correlated over time, differencing two adjacent observations will leave almost no signal. Therefore, when instrumenting the first-differences, one is trying to instrument an observation that is almost entirely white noise. Instruments that are lagged several periods behind the independent variable and instruments that are differences of lagged observations will tend to be particularly weak. In order to increase the correlation between the instrument and the explanatory variable, one might instrument long-differences rather than first-differences, and use instruments that overlap the independent variable rather than ones that lag behind it.

[16] This assumes, of course, that the explanatory variables are strictly exogenous.

Table 3 reports the results obtained using lagged internal instruments to estimate the relationship between AFDC and fertility. The basic differences model from equation (7), which includes controls for per capita earnings and year effects, is used. Lagged AFDC benefits are used as instruments for first differences, 2-year differences and 3-year differences. The first column describes the instrument and reports the first-stage partial F-statistic on the instrument. There are three types of instruments used in Table 3: lagged differences, lagged levels and overlapping differences. The lagged differences are conceptually the most attractive, but, as predicted, tend to be very weak instruments. Lagged levels have less theoretical justification unless one has a conceptual model that suggests that the growth rates should depend on the previous levels. Overlapping differences are the most likely to be strong instruments, but will be most vulnerable to violations of the assumption of uncorrelated errors (or in this case, correlation in the transitory component of the benefit level).

Many of the instruments in Table 3 have an F-statistic less than one. Only 5 instruments can be classified as "strong" instruments, all of which have F-statistics larger than 40. Consistent with our expectations, none of the instruments for first-differences are strong, and all of the strong instruments overlap the explanatory variable.

The second column in Table 3 reports the IV estimate of the coefficient on AFDC benefits. There is clearly tremendous variance in the coefficient estimates. If, however, attention is focused on the results in bold-face when are obtained using the strong instruments, the results are all positive and of reasonable and similar magnitude, in the range of 0.12-0.13. The results obtained with the "strong" instruments are very similar in magnitude to those obtained with the 7-year long-differences and fixed-effects results. On one hand, this is not surprising, as the results using the overlapping instruments are identified from the same persistent year-to-year changes that identify the fixed-effects estimates. The fixed-effects and long-differences estimates, however, still suffer from attenuation bias that the IV estimates should not if the assumption of uncorrelated measurement errors holds. This suggests that either the attenuation bias in these estimates is very slight, or that there is sufficient correlation in the transitory component of benefits to generate inconsistent estimates when using the overlapping instruments.

VII. Steel Shock Instrumental Variable Results

This section uses the structural decline of the steel industry between 1970 and 1987 as an instrument that is correlated with long-term changes in local economic conditions.[17] The steel industry analysis focuses on eight states: Alabama, California, Illinois, Indiana, Michigan, New York, Ohio, and Pennsylvania. These eight states have the largest employment in primary metals in 1970, accounting for nearly 69 percent of total primary metals employment in the United States. Figure 2 graphs the fraction of total earnings attributed to primary metals manufacturing over time for both the entire U.S., and the eight steel-producing states used in this analysis. The graph shows that the fraction of total earnings attributed to primary metals manufacturing more than halved between 1979 and 1987.

Because the steel industry is geographically concentrated, the collapse of steel manufacturing was not felt equally across all counties. In the eight-state region in 1969, 60 percent of counties had less than 1 percent of total employment in primary metals, while 8 percent of counties had more than 10 percent of employment in primary metals. Those counties with little steel employment were left relatively untouched by the decline in steel manufacturing, while those with high concentrations were very hard hit by the decline.

[17] Results in Table 3 and the discussion in this section appear in similar form in Black, McKinnish and Sanders (2003)

The instrument used in this analysis is the fraction of men in the county employed in primary metals manufacturing in 1969 and the interaction of that variable with the fraction of total earnings in the United States in that year attributable to the primary metals industry. The employment concentration measure, calculated from 4th County Population File C from the 1970 Census, provides a measure of each county's vulnerability to the decline of steel. The earning share, calculated annually from BEA data, measures changes in demand for domestic steel at the national level. This instrument interacts a measure of a county's dependence on steel with a measure of aggregate demand for steel.[18] Because of the large structural decline in the steel industry during the period under study, the instrument should be highly correlated with the long-term component of earnings, but not the transitory. Coefficient estimates from the first-stage regressions are reported in Appendix B.

Table 4 reports OLS and IV results for the steel states. The first column reports OLS results for four specifications: fixed-effects, first-differences, first-differences with two lagged effects, and 5-year long-differences.[19] The first-differences coefficient is –0.131, but the sum of the contemporaneous effect and two lags is -0.441.[20] As documented by a number of recent studies, the estimated effect of local economic conditions is considerably larger if lagged effects are included. The fixed-effects and 5-year differences coefficient are even larger at -0.598 and –0.544.

The second column reports the IV results for all four models. The IV results are substantially larger in magnitude than the OLS results. For the first-differences model, the

[18] For the fixed-effects model, it was determined that the first-stage performance of the instrument was substantially better if the fraction earnings in primary metals was replaced with period indicators for 1974-76, 1977-81 and 1982-87. Results are reported using this preferred instrument.
[19] See Laporte and Windmeijer (2005) for a discussion of the difference between fixed-effects and first-differences estimators when the explanatory variable is a binary indicator and there are omitted lags and/or leads of the binary treatment.
[20] Adding additional lags does not increase the size of the estimated effect.

coefficient increases in magnitude from -0.131 to -0.696. Two lags of the instruments are used to instrument the lagged effects model. It is particularly interesting that the IV estimate obtained from the first-differences without lags is very similar in magnitude to the IV estimate of the first-differences model with lags; the estimates are –0.696 and –0.728 respectively. This suggests an explanation for the difference between the OLS first-differences estimate and the OLS lagged effects estimate. Given contemporaneous changes in economic conditions, lagged changes in economic conditions provide a measure of the duration of the change. Therefore, in OLS, the lagged model better estimates the effect of long-term changes in conditions. When the models with and without lagged effects are instrumented with a long-term local shock, the difference between the models disappears.

The IV estimates for the fixed-effects and the 5-year differences model are –0.858 and –0.982. There are two reasons that the IV results should be greater in magnitude than the OLS results, even the fixed-effects and long-difference ones. One is that all of the OLS models suffer attenuation bias due to transitory fluctuations in economic conditions. This bias is, of course, more extreme for the first-differences model, but still exists for the fixed-effects and long-differences models. Additionally, the IV estimates are identified using variation in economic conditions resulting from changes in industrial job opportunities for low-skilled workers, which presumably should be more directly related to welfare participation than changes in general economic conditions. It is not possible, therefore, to say what fraction of the difference between the OLS and IV estimates is due to the purging from the data of the transitory variation in economic conditions.

VIII. Conclusions

Fixed-effects and first-differences models are extremely popular because the relationship of interest is often confounded by unobserved heterogeneity in the cross-section. Unfortunately, if the independent variable is an imprecise measure of the relevant factor, coefficient estimates from these models can be severely attenuated towards zero. The time series variation that remains after removing fixed effects often largely reflects idiosyncratic changes in the independent variable that have little influence on the decision of interest. This is problematic anytime the outcome of interest is one that likely responds to sustained changes in conditions, such as fertility, welfare program participation, educational attainment, migration, and marital status.

These findings suggest that studies using fixed-effects or first-differences models can understate the magnitude of the effect of interest. Specifically, the results suggest that when the outcome does not respond to transitory variation in the explanatory variable, first-differences will produce particularly attenuated estimates. Attenuation still exists in the fixed-effects estimates, but should be less severe. Alternatively, if the outcome of interest primarily responds to transitory fluctuations in the explanatory variable, first-differences estimates are preferred.

For researchers, comparing fixed-effects, first-differences, and long-differences models is a simple way to check for misspecification. If these estimates differ, the research must then consider what forms of omitted variable bias and what assumptions regarding how shocks in the explanatory variable affect the outcome are consistent with the pattern of the estimates.

References

Argys, Laura, Susan Averett and Daniel Rees. 2000. "Welfare Generosity, Pregnancies and Abortions among Unmarried Recipients." *Journal of Population Economics*. 13(4):569-94.

Baker, Michael, Dwayne Benjamin, and Shuchita Stanger. 1999. "The Highs and Lows of the Minimum Wage Effect: A Time-Series Cross-Section Study of the Canadian Law," *Journal of Labor Economics*, 17(2):318-50.

Bartik, Timothy J. and Randall W. Eberts. 1999. "Examining the Effect of Industry Trends and Structure on Welfare Caseloads." In Sheldon Danziger (ed.), *Welfare Reform and the Economy: What Will Happen When a Recession Comes?* Kalamazoo, MI: Upjohn Institute for Employment Research, pp. 119-57.

Black, Dan A., Kermit Daniel, and Seth G. Sanders. 2002. "The Impact of Economic Conditions On Participation in Disability Programs: Evidence from the Coal Boom and Bust." *American Economic Review* 92(1):27-50.

Black, Dan A., Terra McKinnish, and Seth G. Sanders. 2003. "How the Availability of High-Wage Jobs to Low-skilled Men Affects AFDC Expenditures: Evidence from Shocks to the Steel and Coal Economies." *Journal of Public Economics*. 87(9):1919-40.

Blank, Rebecca M. 2001. "What Causes Public Assistance Caseloads to Grow?" *Journal of Human Resources*, 36(1): 85-118.

Bound, John, David A Jaeger and Regina M. Baker. 1995. "Problems with Instrumental Variables Estimation When the Correlation Between the Instruments and the Endogenous Explanatory Variable is Weak." *Journal of the American Statistical Association*. 90:443-50.

Clark, George and Robert Strauss. 1998. "Children as Income-Producing Assets: The Case of Teen Illegitimacy and Government Transfers." *Southern Economic Journal*. 64:827-56.

Council of Economic Advisers. 1999. "Economic Expansion, Welfare Reform, and the Decline in Welfare Caseloads: An Update. Technical Report," Washington, D.C.: Executive Office of the President.

Figlio, David and James Ziliak. 1999. "Welfare Reform, the Business Cycle and the Decline in AFDC Caseloads." In Sheldon Danziger (ed.), *Welfare Reform and the Economy: What Will Happen When a Recession Comes?* Kalamazoo, MI: Upjohn Institute for Employment Research, pp. 19-48.

Fitzgerald, John. 1995. "Local Labor Markets and Local Area Effects on Welfare Duration," *Journal of Applied Policy and Management*, 14: 43-67.

Griliches, Zvi and Jerry Hausman. 1986. "Errors in Variables in Panel Data." *Journal of Econometrics*. 31:93-118.

Heckman, James and V. Joseph Hotz. 1989. "Choosing Among Alternative Nonexperimental Methods for Estimating the Impact of Social Programs." *Journal of the American Statistical Association*. 84:862-74.

Hoffman, Saul and E. Michael Foster. 2000. "AFDC Benefits and Non-Marital Births to Young Women." *Journal of Human Resources*. 35(2):376-91.

Hoynes, Hillary. 1997. "Does Welfare Play Any Role in Female Headship Decisions?" *Journal of Public Economics*. 65:89-117.

Hoynes,Hillary. 2000. "Local Labor Markets and Welfare Spells: Do Demand Conditions Matter?" *Review of Economics and Statistics* 82(3):351-68.

Jackson, Catherine and Jacob Klerman. 1994." "Welfare, Abortion and Teenage Fertility." Unpublished manuscript, RAND Corporation.

Keane, Michael, and Kenneth Wolpin. 2002. "Estimating Welfare Effects Consistent with Forward-Looking Behavior." *Journal of Human Resource*s 37(3):570-622.

Klerman, Jacob and Steven Haider. 2004. "A Stock-Flow Analysis of the Welfare Caseload." *Journal of Human Resources*. 39(4):865-86.

LaPorte, Audrey and Frank Windmeijer. 2005. "Estimation of panel data models with binary indicators when treatment effects are not constant over time." *Economics Letters* 88(3): 389-396.

Matthews, Ribar and Wilhelm. 1997. The Effects of Economic Conditions and Reproductive Health Services on State Abortion and Birth Rates." *Family Planning Perspectives*. 29:52-60.

McKinnish, Terra. 1999. "Model Sensitivity in Research on Welfare and Fertility." unpublished PhDdissertation, Carnegie Mellon University.

McKinnish, Terra. 2005. "Lagged Dependent Variables and Specification Bias." *Economics Letters* 88(1) 55-59.

Miller, Robert and Seth Sanders. 1997. "Human Capital Development and Welfare Participation." *Carnegie-Rochester Conference Series on Public Policy* 46:1-47.

Moffitt, Robert, 1994. "Welfare Effects on Female Headship with Area Effects." *Journal of Human Resources*. 29:621:636.

Mueser, Peter, Julie Hotchkiss, Christopher King, Phillip Rokicki, and David Stevens. 2000. "The Welfare Caseload, Economic Growth and Welfare-to-Work Policies." Unpublished manuscript.

Rosenzweig. Mark. 1999. “Welfare, Marital Prospects and Non-Marital Childbearing.” *Journal of Political Economy*. 107(6):S3-S32.

Wallace, Geoffrey and Rebecca Blank. 1999. “What Goes Up Must Come Down? Explaining Recent Changes in Public Assistance Caseloads.” In Sheldon Danziger (ed.), *Welfare Reform and the Economy: What Will Happen When a Recession Comes?* Kalamazoo, MI: Upjohn Institute for Employment Research, pp. 49-90.

Ziliak, James, David Figlio, Elizabeth Davis and Laura Connolly. 2000. “Accounting for the Decline in AFDC Caseloads.” *Journal of Human Resources*. 35(3):570-86.

Appendix A

Proof of result in equation (11):

$$\text{plim}(\hat{\beta}_{\text{d}}) = \frac{Cov(Y_{it} - Y_{it-j}, X_{it} - X_{it-j})}{Var(X_{it} - X_{it-j})} = \beta \frac{Var(Z_{it} - Z_{it-j})}{Var(Z_{it} - Z_{it-j}) + Var(v_{it} - v_{it-j})} = \beta \left(\frac{\sigma_z^2 (1-\rho^j)}{\sigma_z^2 (1-\rho^j) + \sigma_v^2 (1-\delta^j)} \right)$$

$\left|\text{plim}(\hat{\beta}_{\text{j+1}})\right| > \left|\text{plim}(\hat{\beta}_{\text{j}})\right| \; \forall j$ iff: $(1-\rho^{j+1})(1-\delta^j) > (1-\rho^j)(1-\delta^{j+1})$ which is true iff $\delta < \rho$.

Proof of result in equation (13):

$$\text{plim}(\hat{\beta}_l) = \frac{Var(X_{it} - X_{it-1})Cov(Y_{it} - Y_{it-1}, X_{it-1} - X_{it-2}) - Cov(Y_{it} - Y_{it-1}, X_{it} - X_{it-1})Cov(X_{it} - X_{it-1}, X_{it-1} - X_{it-2})}{Var(X_{it} - X_{it-1})Var(X_{it-1} - X_{it-2}) - Cov(X_{it} - X_{it-1}, X_{it-1} - X_{it-2})^2}$$

$$= \frac{2\beta\sigma_v^2\sigma_z^2[(1-\rho)(1-\delta)^2 - (1-\delta)(1-\rho)^2]}{4[\sigma_z^2(1-\rho) + \sigma_v^2(1-\delta)]^2 - [\sigma_z^2(1-\rho)^2 + \sigma_v^2(1-\delta)^2]^2}$$

Proof of result in equation (14):

$$\text{plim}(\hat{\beta}_l) = \frac{Var(X_{it} - X_{it-j})Cov(Y_{it} - Y_{it-j}, X_{it-1} - X_{it-j-1}) - Cov(Y_{it} - Y_{it-j}, X_{it} - X_{it-j})Cov(X_{it} - X_{it-j}, X_{it-1} - X_{it-j-1})}{Var(X_{it} - X_{it-j})Var(X_{it-1} - X_{it-j-1}) - Cov(X_{it} - X_{it-j}, X_{it-1} - X_{it-j-1})^2}$$

$$= \frac{2\beta\sigma_v^2\sigma_z^2[(1-\delta^j)(2\rho - \rho^{j+1} - p^{j-1}) - (1-\rho^j)(2\delta - \delta^{j+1} - \delta^{j-1})]}{4[\sigma_z^2(1-\rho^j) + \sigma_v^2(1-\delta^j)]^2 - [\sigma_z^2(2\rho - \rho^{j+1} - p^{j-1}) + \sigma_v^2(2\delta - \delta^{j+1} - \delta^{j-1})]^2}.$$

For both results, $\text{plim}(\hat{\beta}_l)$ is the sign of β iff $(1-\rho)(1-\delta)^2 - (1-\delta)(1-\rho)^2 > 0$, which is true iff $\delta < \rho$. The property: $\left|\text{plim}(\hat{\beta}_{l,j+1})\right| > \left|\text{plim}(\hat{\beta}_{l,j})\right|$ was investigated by grid search. A large grid search failed to turn up any violations of the property for cases in which $j \geq 2$. Violations at *j=1* tended to occur when either ρ or $\sigma_\mu^2 / \sigma_\theta^2$ was high.

Appendix B

This appendix reports first-stage estimates for IV analysis in Table 4. For the first-differences and long-differences models, the instruments are the fraction of men in the county employed in primary metals manufacturing in 1969 and the interaction of that variable with the fraction of total earnings in the United States in that year attributable to the primary metals industry. For the fixed-effects models, it was found that the instruments performed better in the first-stage if fraction of employment in primary metals was interacted with period indicators for 1974-76, 1977-81 and 1982-87, instead of with the fraction of earnings from primary metals. The main effect of the fraction employment variable is absorbed into the county fixed-effects.

Table B1: First-Stage Estimates from Steel-State IV Analysis

	First-Differences	5-Year Differences		Fixed-Effects
Fraction Steel	-0.379 (0.049)	-1.76 (0.185)	Fraction Steel* (1974-76)	0.235 (0.055)
Fraction Steel *Fraction Earnings	19.29 (2.71)	99.89 (10.79)	Fraction Steel* (1977-81)	0.420 (4.20)
			Fraction Steel* (1982-87)	-0.175 (0.102)
Partial F-Statistic	36.6	45.0		29.9
N	10,949	8,478		11,044

Figure 1: Nominal AFDC Benefits, Family of Four, 1970-94

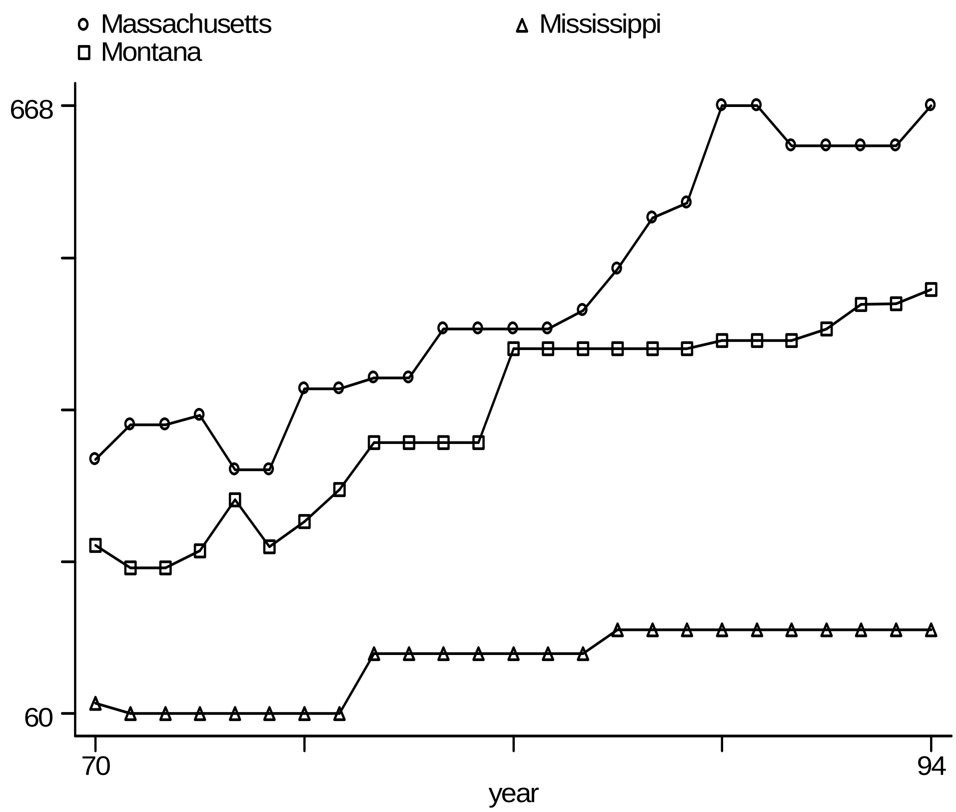

Figure 2: Fraction of Earnings from Steel

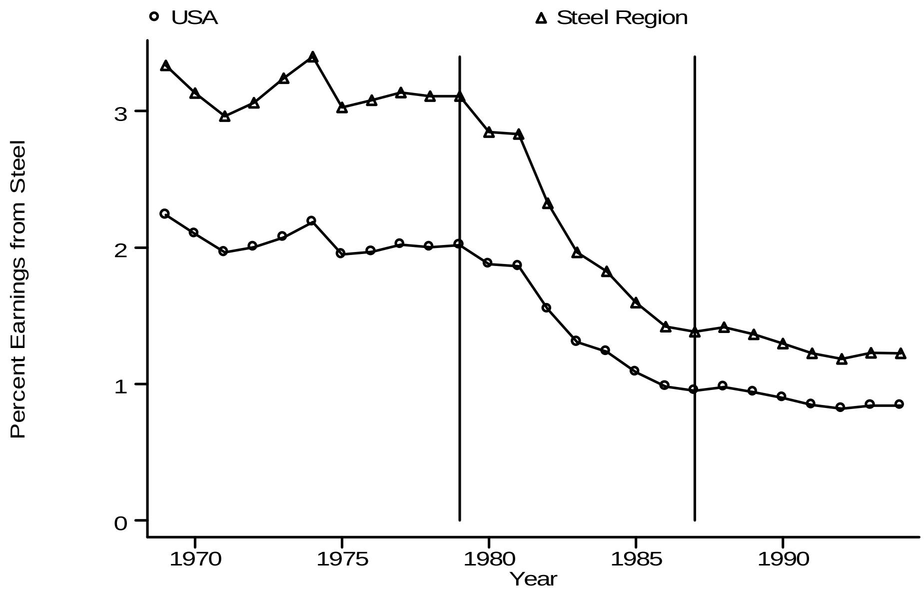

Table 1: Fixed-Effects and Differences Estimates

Estimation Method	AFDC Benefits and Fertility		County Earnings and AFDC Expenditures	
	All States 1973-92	N	All Counties 1969-93	N
Fixed Effects	0.123 (0.022)	1020	-0.244 (0.006)	70,621
First Differences	0.017 (0.023)	969	-0.038 (0.006)	67,302
3-year Differences	0.074 (0.051)	867	-0.165 (0.011)	61,279
5-year Differences	0.094 (0.059)	765	-0.219 (0.014)	55,391
7-year Differences	0.116 (0.059)	663	-0.246 (0.017)	49,518
20-year Differences			-0.268 (0.038)	12,884

Notes:
Column 1: Dependent variable is the logarithm of state birth rate for white women ages 20-24. Table reports coefficient on logarithm of state AFDC benefit level. Per capita income and year effects are included. Standard errors, in parentheses, are clustered at the state level. Data include all 50 states and the District of Columbia.
Column 2: Dependent variable is the logarithm of county AFDC expenditures. Table reports the coefficient on the logarithm of total county earnings. All regressions include controls for county population and state-year effects. Standard errors, in parentheses, are clustered at the county level. There are 3,182 counties in the U.S. Missing observations are due to suppression of the AFDC expenditures variable in small counties (suppression rate is 9.7%).

Table 2: Comparison of Estimation Results: County AFDC vs UI Expenditures, 1969-93

	AFDC Expenditures		UI Expenditures		
	Differences Estimate	Lagged Effect	Differences Estimate	Lagged Effect	N
j=1	-0.047 (0.006)	-0.074 (0.006)	-0.261 (0.018)	0.182 (0.015)	63,709
j=3	-0.175 (0.013)	-0.116 (0.009)	-0.280 (0.020)	0.353 (0.024)	57,781
j=5	-0.231 (0.015)	-0.123 (0.011)	-0.201 (0.019)	0.378 (0.025)	52,023
j=7	-0.269 (0.018)	-0.104 (0.013)	-0.160 (0.020)	0.431 (0.029)	46,193
j=20	-0.299 (0.040)	-0.132 (0.055)	-0.043 (0.037)	0.557 (0.065)	9,930

Notes: Dependent variable is the logarithm of county AFDC expenditures or UI expenditures. Table reports the coefficient on the logarithm of county earnings or one-year lag of county earnings. All regressions include controls for county population and state-year effects. Standard errors, in parentheses, are clustered at the county level. There are 3,182 counties in the U.S. Missing observations are due to suppression of the AFDC expenditures and UI expenditures variables in small counties (combined suppression rate is 11.5%).

Table 3: AFDC Benefits and Fertility: Lagged Internal Instruments

	Coefficient on AFDC Benefit
Instrument X_{it}-X_{it-1} With:	
X_{it-2} [0.70]	-0.349 (0.823)
X_{it-3} [0.95]	-0.272 (0.645)
X_{it-4} [1.25]	-0.272 (0.655)
X_{it-2}-X_{it-3} [0.41]	0.120 (0.440)
X_{it-3}-X_{it-4} [0.42]	-0.273 (0.995)
X_{it-2}-X_{it-4} [0.02]	1.48 (12.41)
Instrument X_{it}-X_{it-2}With:	
X_{it-3} [4.31]	0.775 (0.470)
X_{it-4} [4.90]	0.776 (0.445)
X_{it-3}-X_{it-4} [0.04]	0.715 (4.40)
X_{it-1}-X_{it-3} [135.0]	**0.137 (0.064)**
X_{it-1}-X_{it-4} [105.0]	**0.129 (0.060)**
Instrument X_{it}-X_{it-3}With:	
X_{it-2} [0.26]	2.49 (5.00)
X_{it-4} [3.79]	0.762 (0.498)
X_{it-1}-X_{it-4} [360.8]	**0.118 (0.056)**
X_{it-1}-X_{it-2} [106.4]	**0.116 (0.062)**
X_{it-2}-X_{it-4} [61.3]	**0.121 (0.061)**

Notes: First-stage partial F-statistic in reported in square brackets. Standard errors, in parentheses, are clustered at the state level.

Table 4: Instrumental Variables and Lagged Effects, Steel State Sample, 1970-87

	OLS	N	IV	N
Fixed-Effects	-0.598 (0.067)	11,044	-0.858 (0.277)	11,044
First-Differences	-0.131 (0.029)	10,949	-0.696 (0.201)	10,949
Sum of First-Differences and 2 Lagged Differences	-0.441 (0.047)	9,836	-0.728 (0.221)	9,836
5-Year Differences	-0.544 (0.054	8,478	-0.982 (0.233)	8,478

Notes: Dependent variable is the logarithm of county AFDC expenditures. Table reports the coefficient on the logarithm of total county earnings. All regressions include controls for county population and state-year effects. Instrument is the county's fraction of male employment in primary metals in 1969 interacted with that year's fraction of total earnings in the United States from primary metals manufacturing. For the fixed-effects model, the employment variable is interacted with period indicators for 1974-76, 1977-81 and 1982-87 rather than the fraction earnings variable. Standard errors, in parentheses, are clustered at the county level. First-stage F-statistic is 29.9 for the fixed-effects model, 36.6 for first-differences model and 45.0 for 5-year differences model. There are 619 counties in the eight-state steel region of Alabama, California, Indiana, Illinois, Michigan, New York, Ohio, Pennsylvania.