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WELFARE REFORM AND THE SPREAD OF HIV

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Abstract: Previous research has studied the effects of welfare payments under the Aid to Families with Dependent Children (AFDC) program on incentives and behavior. By lowering the cost of raising children, states with larger welfare payments have higher rates of fertility among poor women, an adverse consequence. In previous papers, we concluded that by lowering the cost of unprotected sexual activity, greater welfare payments are associated with higher rates of sexually transmitted diseases, including HIV. The restructuring of the welfare system in 1996 transformed AFDC into time limited assistance with an emphasis on work and personal responsibility. We test whether this program (Temporary Assistance for Needy Families, or TANF) has succeeded in eliminating the adverse incentive structure existing under AFDC. Using GLS and IV estimation procedures on state data from 1993 through 2002, we find that the effect of TANF payments on heterosexual HIV incidence is significantly less than under AFDC.

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1. INTRODUCTION

Economists have long studied the theoretical and actual effects of welfare on some rather personal individual decisions. For example, because the amount of financial assistance a family receives increases as the number of dependents increase, welfare lowers the cost to recipients of having children.[1] As a result, economists have hypothesized and demonstrated that states with more generous welfare systems will have higher rates of fertility among poor women. Ozawa (1989), Caudill and Mixon (1993), and Clark and Strauss (1998) have all determined that the level of financial support to unwed mothers is positively related to rates of fertility. Bearing children is, of course, the result of sexual activity. By extension, welfare lowers the cost to poor women of sexual activity. There are additional possible consequences of such activity other than bearing children. Leibowitz et al. (1986) as well as Gohmann and Ohsfeldt (1993) found an inverse relationship between welfare support and rates of abortion. The rationale being that mothers

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who reside in states that offer generous welfare are more able to afford to raise a child and therefore less likely to abort their pregnancy.

Research also exists on state-wide variations in additional provisions relating to welfare reform. Kearney (2004) and Joyce et al. (2005) examine the effect of family cap provisions on fertility behavior across states. The purpose of a family cap is to reduce or eliminate additional cash benefits to a family on welfare resulting from the birth of an additional child. The obvious assumption behind such a policy is that the availability of additional cash benefits per birth provides a woman an economic incentive at the margin to bear additional children. Kearney concludes that family cap policies do not lead to a decline in births. Joyce et al. identified women at high risk for public assistance, and compared births to high risk women with previous live births to high risk childless women. They conclude that births fell more for women with at least one previous live birth compared with childless women. However, this result was found in states without a family cap provision as well as in those with such a provision. Thus, they conclude that the family cap exerts no independent influence on the birth rate.

Another possible outcome of increased sexual activity is an increased risk of contracting an STD. Research has demonstrated that the generosity of a state's welfare program is causal to higher contraction rates of syphilis, gonorrhea, and chlamydia, Ressler et al. (2006) and higher rates of HIV, Ressler et al. (2005).

In 1996, President Clinton signed into law the "Personal Responsibility and Work Opportunity Reconciliation Act," thereby transforming the nation's welfare system. Perhaps the most significant feature of this reform was the establishment of a limit to the amount of time an individual could receive welfare. The former program, Aid to Families with Dependent Children (AFDC) was replaced with Temporary Assistance for Needy Families (TANF). Under the TANF guidelines, recipients could receive welfare for a maximum of five years - consecutive or otherwise. All of the aforementioned studies were conducted under the AFDC welfare system.[2] We argue in the present paper that because of the five year limitation on assistance under the current TANF program, the adverse incentives that were present under AFDC have been severely weakened. We therefore retest the relationship between the magnitude of the state's welfare payment and contraction rates of HIV.

In Section 2 we specify the model and define variables used in the estimations. In Section 3 we outline the empirical methodology and provide estimation results. We provide a detailed Appendix 2 in which we examine the issue of instrument strength. In the final section concluding remarks are made.

2. MODEL SPECIFICATION

We specify the following model using data on the 26[3] states for 1993-1996 (years when welfare was administered under AFDC); and for 1998-2002 (years when TANF was in place[4]):

$$\text{HET-HIV Rate} = f(\text{AFDC or TANF, COLLEGE, DOCTORS, NOINSURE, AGE, BLACK, IMMIGRANT, HEALTH, PCY, DYEARS, REGION}) \quad (1)$$

Where:

- HET-HIV Rate is the estimated proportion of a state's population newly infected with HIV through heterosexual contact per year; that is, we multiply the reported number of new HIV cases per capita by the reported proportion of new AIDS cases caused by heterosexual contact, per state, per year,[5]

- AFDC or TANF is the amount of the welfare payment, per recipient,
- COLLEGE is the percentage of population with at least four years of college,
- DOCTORS is the number of practicing doctors per 100,000 population,
- NOINSURE is the percentage of the population without health insurance,
- AGE is the percentage of the population aged 18-34,
- BLACK is the percentage of the population who are African-American,
- IMMIGRANT is the number of immigrants admitted per resident population,
- HEALTH is government spending on health care and hospitals per capita,
- PCY is personal income per capita,
- DYEARS represents dummy variables indicating years (1997 is the base year),
- REGION is a vector of regional dummy variables, with definitions in Appendix 1.

If poor women are aware of the five year limit to receiving welfare that exists under TANF, then rational behavior suggests a change in their behavior from what had been optimal under AFDC. We suggest that the absence of such a time limit under AFDC was an important factor in lowering the cost to poor women of unprotected sexual activity. The promotion of unprotected sex resulted in an increase in fertility and the contraction of HIV and other STDs. The impact of the five year time limit under TANF on contraction rates of HIV is ultimately an empirical question: if welfare reform is successful, the relationship between these two variables should be statistically insignificant.

Advocates of the poor often argue that seemingly reckless behavior is more the result of ignorance, not irresponsibility. We reason that college graduates should be well informed of the risks involved with having casual and/or unprotected sex. Accordingly, we expect the coefficient of COLLEGE to be negative.

One might expect the presence of numerous health professionals in an area would increase the overall level of health and lower the incidence of most diseases. However, because HIV has no known cure, we have no such expectation. Conversely, those infected with HIV may migrate to areas where doctors are concentrated. Additionally, a doctor's care may extend the life of HIV patients, thus increasing the stock of HIV-positive individuals. For these reasons, we expect DOCTORS to be positively related to HET-HIV.

With measures of both income and education as explanatory variables, NOINSURE isolates the financial burden placed on consumers of healthcare. Other things the same, those with no health insurance are less likely to seek medical services, and therefore are less likely to be aware of their condition if they have contracted HIV[6]. Consequently, an HIV positive individual who is ignorant of his or her condition may unknowingly infect others. We therefore expect NOINSURE to have a positive impact on HET-HIV.

AGE represents the percentage of the population 18 to 34 years of age. Rates of fertility tend to be high in this range, particularly for low income individuals. In addition, young adults are more likely to be unattached and have multiple partners. These risk-enhancing factors lead us to expect a positive coefficient on AGE.

It has been well documented that the African-American community has been particularly hard hit by the AIDS epidemic. The CDC estimates that although African-Americans account for 13 percent of the U.S. population, they account for 50 percent of new HIV/AIDS cases in 33 states. We suspect that the disproportional contraction rate experienced by blacks is due primarily to differences in socioeconomic status. Factors such as average income and education, and access to health care – variables we include as regressors – account for much of the disparity across races. To control for the possibility of an unobservable or unknown variable that differs across race, we also include BLACK as an explanatory variable.

The history of the United States contains many instances of immigrants bringing diseases into the indigenous population. HIV was brought into the United States through international travel of Americans and through immigration. Though previous research has failed to statistically link statewide HIV contraction rates to the prevalence of immigrants, we nonetheless include IMMIGRANT as a control variable and expect its coefficient to be positive.

HEALTH measures per capita government expenditures on healthcare and hospitals. These expenditures include prevention and educational efforts regarding all sexually transmitted diseases – measures we expect to lower the incidence of HIV. This expectation implies a negative coefficient on HEALTH but must be tempered by the possibility that government spending on healthcare is often reactive. If healthcare spending on the part of government is a response to higher than average rates of infection, then the coefficient of HEALTH may be positive.

Personal income per capita is included to provide some control for state-to-state cost of living variations. PCY will be higher for high cost states. The inclusion of PCY provides us with an indirect method of converting TANF figures from nominal to real. We have no a priori expectation regarding the coefficient sign of PCY.

Welfare recipients under AFDC may have been aware of the impending policy changes that TANF would bring to bear. Likewise, recipients under TANF may have required some time to adjust to the newly imposed time limit that accompanied transfer payments. Therefore, we include yearly dummy variables in both the AFDC and the TANF estimations. The base years are 1993 for the AFDC estimation and 1998 for the TANF estimation. If TANF reduced the adverse incentives present under AFDC, we would expect behavior to adjust accordingly over time. The more years TANF is in place, the more optimal a recipient's behavior will be. This effect may be accentuated by a general increase in the level of public awareness of HIV prevention, perhaps due to media exposure and word-of-mouth information. As such, we expect the yearly dummies to carry negative coefficients.

Finally, we include a vector of regional dummy variables. With the New England states as the base, we have no expectations regarding the coefficient signs of these dummies.

3. EMPIRICAL METHODOLOGY AND RESULTS

We estimate model (1) for the years prior to 1997 and for the years after 1997 and test the change in the coefficient of the key variable, AFDC-TANF. The logic of the break in the data is that the law changed in fiscal year 1997, so states had AFDC for half the calendar year and TANF for the other half. As a result 1997 was omitted from estimations. Table 1 contains the summary statistics for these variables for the two distinct periods that we consider, 1993-1996, and 1998-2002.

3.1. Fixed Effects

The data used in this analysis are a cross-section of the N = 26 states observed over the sample periods 1993-1996 and 1998-2002. It would seem appropriate to include state-level fixed effects (dummy variables) to control for social, cultural and political norms. Unfortunately with these data this is not feasible. One of our key explanatory variables, BLACK, changes little over the period, or sub-periods. Its inclusion along with state level fixed effects leads to regression failure due to extreme collinearity.[7] To control somewhat for state-level effects we have included regional dummy variables.

3.2. Heteroskedasticity

Equation (1) was initially estimated by least squares for each period. The assumption of homoskedasticity was strongly rejected in both periods. The White/Koenker test amounts to regressing the squared least squares residuals on a set of P candidate variables that might include the original variables, and their squares, cross-products.[8] Under the null hypothesis that no heteroskedasticity is present the value of NR^2 has a chi-square distribution with P–1 degrees of freedom. In the pre-1997 period the tests using variable levels (P–1= 19 degrees of freedom), levels and squares (P–1=28) had p-values 0.0001, 0.0017, respectively. In the post-1997 period the tests using variable levels (P–1=20 degrees of freedom), levels and squares (P–1=29) had p-values 0.0114, 0.0542, respectively. Inspection of the residuals showed marked state by state differences in residual magnitudes. Using the least squares residuals, we obtained an estimated error variance for each state [based on 4 observations in the pre-period and 5 observations in the post- period], and scaled all data by dividing by the respective standard deviations. The procedure is very simple and captures gross differences between states. The p-values of the NR^2 test using levels, and levels and squares, for the various models applied to weighted data are reported in Table 2 as HETTEST. All the results in Table 2 are based on these weighted data.

Applying least squares to the weighted data yields generalized least squares (GLS) estimates for the “groupwise” heteroskedastic regression (Greene, 2008, pp. 172-174). These results are reported as model “GLS” in Tables 2a and 2b. Using the transformed data we fail to reject the hypothesis that the errors are homoskedastic (based on HETTEST) at the 5% level. Our weighting of the data has been successful. Note specifically that in the pre-1997 period the coefficient of ADFC-TANF is positive and significant and in the post-1997 period it is insignificant.

3.3. Instrumental Variables Estimation Results

As noted in the variable discussion, one might argue that DOCTORS and HEALTH are potentially endogenous. To address this issue we report instrumental variables (IV) estimates in Table 2.[9] To use IV estimation we must find valid instruments. They must be (i) correlated, after accounting for the effect of all other exogenous variables, with the potentially endogenous variable. In addition, they must be (ii) uncorrelated with the regression error in (1). That is, they must not be significant omitted variables from that equation. Because we have two potentially endogenous variables we must have at least two instrumental variables. The instruments we used are JEWISH (the percent of the state population that is Jewish), CATHOLIC (the percent of the state population that is Catholic), JEWISH×METRO and METRO (the percent of the state population living in metropolitan areas). The econometric evidence for the relevance and validity of these instruments is discussed in Appendix 2. On economic grounds, we argue that these variables indirectly affect HET-HIV. Cultural norms characteristic of a particular region or state may be captured by religious participation and membership, political affiliation, and membership in social organizations, for example. While not directly influencing HIV rates in a causal sense, these variables may capture social and moral/ethical beliefs that influence behavior across groups. Similarly, METRO is not expected to affect HIV incidence directly. Merely living in a metropolitan area will not increase one's likelihood of contracting HIV per se; one must first engage in risky sexual behavior. We argue that the greater anonymity associated with life in a large urban environment lowers the cost of engaging in the risky behaviors associated with HIV contraction.

Table 2a contains the coefficient estimates for equation (1) using observations covering the years 1993-1996 from the AFDC program. The table contains generalized least squares estimates (GLS) and instrumental variables estimates (IV) on weighted data.[10] As the Table indicates, the coefficient of AFDC is positive and significant in both GLS and IV estimations. This result is consistent with previous research and confirms the hypothesis that AFDC payments lower the cost of unprotected sexual activity thus encouraging its incidence and the occurrence of one of its possible consequences.

Of the other explanatory variables specified, BLACK, HEALTH, PCY and the vector of regional dummies are all statistically significant. Additionally, COLLEGE is significant to the 0.10 level when coefficients are estimated using GLS. Of these estimates, perhaps the most intriguing is the coefficient of HEALTH. The robustness of the negative coefficient estimate may suggest that government spending on healthcare was effective in reducing HIV contraction rates under the AFDC program. The caution with which this inference is stated is necessitated due to HEALTH's broad definition. Although it includes spending on prevention efforts as noted, the variable is admittedly a rough proxy for this specific type of expenditure. Unfortunately, data on HIV/AIDS prevention expenditures are not available for our years of analysis.

The positive coefficients that persist on all of the regional dummy variables indicate that relative to the rest of the nation, the New England states have significantly lower contraction rates of heterosexually transmitted HIV. This may reflect unobservable cultural differences across regions which contribute to variations in HIV incidence, variations in HIV reporting procedures across states and regions, or simply where infected individuals choose to locate. We can only speculate that these factors, as well as the relatively well-educated populace in the largely rural region may lead to this result. Finally, the 1996 dummy variable is marginally significant indicating that during the last full calendar year in which the AFDC program was in existence, HIV contraction rates fell. This result is not unexpected given that, although TANF went into effect during 1997, welfare reform legislation was passed in 1996 to great fanfare. It appears that the legislation and/or a rising level of general public awareness may have had an impact on behavior even before the new program took effect.

Table 2b contains results using observations from the years 1998-2002. During these years the TANF program had replaced AFDC, and welfare was (and still is) capped at five total years of benefits. There was significant structural change between the pre-1997 and post-1997 periods. A first indication of this change is that in the post-1997 period the Wu-Hausman test for the exogeneity of DOCTORS and HEALTH expenditures was strongly rejected (p-value 0.0035). Thus for this period, the instrumental variables estimates (IV) are more relevant. Furthermore, some of the coefficient estimates are markedly different. The variable TANF is insignificant, and more importantly, the t-value for testing that the TANF coefficient in post-1997 is less than the ADFC coefficient in pre-1997 is –2.77. The p-value for a one-tail test is 0.0028, and we conclude that the effect of TANF is significantly less than the effect of AFDC .

This suggests that though “welfare without end” which persisted under AFDC seemed to encourage risky behavior and subsequently higher HIV contraction rates, the welfare reform legislation of 1996 which initiated TANF appears to have eliminated the previously robust relationship between welfare and HIV. We conclude, therefore, that the change in the law altered recipients’ calculus of optimization, thereby resulting in a change in behavior.

Of the remaining explanatory variables, the coefficients of COLLEGE, DOCTORS, and BLACK are statistically significant. It is interesting that the coefficient of DOCTORS is positive and significant in the post-1997 period, whereas HEALTH expenditures are not significant. Also, there are strong differences between the GLS and IV estimates, which is a further indication of the structural change between these two periods.

The regional dummy variables remained significant and positive under TANF as they were under AFDC. With 1998 as the base year, the coefficients of all yearly dummies are negative and are significant in the years 1999 and 2002.

4. CONCLUSION

It appears as though the change from AFDC to TANF has truly been a reform of some consequence. The five year limit to receiving welfare that exists under TANF seems to have lowered the adverse incentives that were present under AFDC. Previous research has demonstrated that the generosity of a state’s welfare program is positively associated with HIV contraction rates. We are able to confirm this pattern with AFDC data. However, using post welfare reform TANF data, the statistical link between welfare and HIV contraction rates no longer exists.

In light of our findings, it is perhaps appropriate to newly affirm or contradict many causal relationships involving welfare that can be found in the literature. The links between welfare and fertility rates: Ozawa (1989), Caudill and Mixon (1997), Rozenzweig (1995); welfare and abortion: Leibowitz, et al. (1986), Medoff (1988), Gohmann and Ohsfeldt (1993) and welfare and adoption, Medoff (1993), were all examined when welfare was administered under AFDC. Whether the introduction of TANF has altered any of the aforementioned links is an empirical question.

APPENDIX 1

The following are the nine regions defined in the United States Census. We used just eight regions, since our 26-state sample did not include any states in the Pacific region. The omitted category is New England.

- NEW ENGLAND: Maine, Vermont, New Hampshire, Massachusetts, Connecticut, Rhode Island.
- MIDDLE ATLANTIC: New York, Pennsylvania, New Jersey.
- SOUTH ATLANTIC: West Virginia, Maryland, Delaware, Virginia, North Carolina, South Carolina, Georgia, Florida, District of Columbia.
- EAST SOUTH CENTRAL: Kentucky, Tennessee, Mississippi, Alabama.
- WEST SOUTH CENTRAL: Louisiana, Arkansas, Oklahoma, Texas.
- EAST NORTH CENTRAL: Wisconsin, Michigan, Illinois, Indiana, Ohio.
- WEST NORTH CENTRAL: North Dakota, South Dakota, Minnesota, Nebraska, Iowa, Kansas, Missouri.
- MOUNTAIN: Montana, Idaho, Wyoming, Utah, Colorado, Arizona, New Mexico, Nevada.
- PACIFIC: Washington, Oregon, California, Alaska, Hawaii.

APPENDIX 2

Instrument Relevance

In Tables 2a and 2b we report the reduced form equations for DOCTORS and HEALTH, the weighted least squares (GLS) estimates and instrumental variables (IV) estimates. Instruments are relevant if they are strongly (or at least not weakly) related to the potentially endogenous variables after accounting for other exogenous variables in the model. One test of relevance is obtained by estimating the reduced form equation including all the exogenous explanatory variables and the four instrumental variables, and then jointly testing the significance of the instruments using an F-test. The results of this test are reported in Table 2 as "Firststage-F". The reported F-values are robust to reduced form heteroskedasticity, whose presence is indicated by the NR^2 (HETTEST) p-values. In each case the instruments are jointly significant at less than the 0.01 level. Further note that in the Pre-1997 period JEWISH and CATHOLIC are strongly significant in both reduced form equations. The interaction of JEWISH and METRO is significant in the reduced form for DOCTORS and is strongly significant in the reduced form for HEALTH expenditures. METRO is strongly significant in the DOCTORS reduced form equation. The "rule of thumb" guideline that the F-value should be greater than 10 is satisfied for both reduced forms in the Pre-1997 period, although this guideline is specifically for the case of a single endogenous variable. Nonetheless, these results show that the instruments are strongly related to the potentially endogenous variables and should be adequate.

Because we will be testing for differences between estimations based on two different sample periods, we use exactly the same model in pre- and post-1997 periods, and exactly the same instruments, as otherwise additional uncertainty is added. We will document several structural differences between the two periods. In the reduced form equations for the post-1997 period, the instruments' coefficients have the same signs as in the pre-1997 period, but they are not all individually significant. CATHOLIC remains strongly significant in both equations. METRO is significant in the HEALTH expenditures reduced form and strongly significant in the DOCTORS reduced form. However JEWISH is not significant in either post-1997 reduced form equation, and its interaction with METRO is significant only in the HEALTH equation. The F-test of joint significance for the HEALTH expenditures reduced form is 4.80, which is below the guideline value of 10. We propose that these instruments are adequately strong based on the robustness of estimation results that we will be presenting and on the basis of other indications and tests that will be discussed below.

The question of identification can be characterized as a question about the rank of a certain matrix. Following Baum et al. (2007a, 486-487) let the regression of interest be $y = X\beta + u$. Suppose that X is partitioned into $X = [X_1\ X_2]$ where X_1 contains K_1 potentially endogenous variables, and X_2 is exogenous. Let Z be the matrix of instrumental variables, which is partitioned as

$$Z = [Z_1\ Z_2] = [Z_1\ X_2]$$

where Z_1 contains L_1 "external" or "excluded" instruments, and $Z_2 = X_2$ contains the included instruments. The reduced form equation for X_1 is

$$X_1 = Z_1\Pi_{11} + Z_2\Pi_{12} + v_1$$

The matrix Π_{11}, which is of dimension $L_1 \times K_1$, must be of rank K_1 for the equation to be identified. In our model $K_1 = 2$ as we have 2 potentially endogenous variables, and $L_1 = 4$ as we have 4 external instrumental variables. A robust test for the rank of a matrix has been proposed by Kleibergen and Paap (2006). Test has been implemented in Stata 10.0. The null hypothesis is that the matrix Π_{11} has rank less than K_1, and the alternative is that Π_{11} is of full rank, and thus the equation is identified. The test statistic has a chi-square distribution with degrees of freedom $L_1 - K_1 + 1$ degrees of freedom. In the pre-1997 period the p-value of the LM version of this test is 0.0061, and in the post-1997 period the p-value of this test is 0.0015. The Wald version of the test has p-values of <0.0001. The p-values of the corresponding tests for iid errors are even smaller. Thus, based on this evidence, we can be more confident about our IV estimation results.

Unfortunately it is still possible that a "weak instrument" problem might arise. There is quite a large literature on the poor performance of IV estimation when instruments are not strong. As discussed by Baum et al. (2007a, 489-490)[11], Stock and Yogo (2005) proposed a test for weak instruments. The test is valid if the regression errors are independent and identically distributed. While we have reported IV estimates with robust standard errors as a precaution, our tests of heteroskedasticity reveal no evidence of a problem. Interestingly, the test critical values are different for IV estimation and limited information maximum likelihood (LIML) estimators, because LIML estimation is more robust to weak instruments than IV estimation. The test statistic critical values are of two types, one based on the amount of estimator bias relative to OLS and the second based on the rejection rate r of a nominal 5% test of significance. In the pre-1997 period, the weak identification test for the IV estimator rejects 10% relative bias, and just fails to reject the rejection rate r = 15%. In the post-1997 period the weak identification test for the IV estimator rejects a 30% relative bias and fails to reject a rejection rate of r = 25% for a nominal 5% test.

Limited information maximum likelihood estimation (LIML) is a k-class estimator. The LIML estimator results when k is set to λ the smallest eigenvalue of a certain matrix, and if k = 1 the result is the IV estimator. See Greene (2008, 375-377). In the pre-1997 period the estimated value of λ is 1.01, and in the post-1997 period the estimate is 1.03. The result is that the LIML estimates are very close to the IV estimates that we report. The test of weak identification rejects as much as a r = 10% in the pre-1997 period and in the post-1997 period. We conclude that our instruments are not "weak".

Instrument Validity

The instruments must also be valid, in the sense that they are uncorrelated with the error term. The outcome of Hansen's J-test, Hayashi (2000, 217-218) and Baum et al. (2003, 14-18), is reported in the Table 2 as "OVERID p-value." These are tests of the validity of two overidentifying restrictions in our IV and GMM estimations. We cannot reject the validity of the overidentifying instruments in either period.

Testing Endogeneity

The discussion of instrumental variables has been motivated by the possible endogeneity of DOCTORS and HEALTH expenditures. Given adequate instruments, we can test this endogeneity using the Hausman test, Wooldridge (2002, p. 121). In this test the reduced form residuals are added as regressors to model (1). This equation is estimated by least squares, and the joint significance of the residuals tested using a robust F-test. In Table 2 the outcome of this test is denoted "ENDOG p-value." Comparing the pre-1997 period to the post-1997 period we see another structural difference. In the post-1997 period we conclude that DOCTORS and/or HEALTH are not exogenous, and IV estimation is justified. In the pre-1997 period, we cannot reject the null hypothesis that these variables are exogenous, and thus estimation by IV is only a precaution. The concern with using such a cautious approach is that the IV estimator is far less efficient than the GLS estimator.

Heteroskedasticity

The estimation results labeled GLS are in fact weighted least squares regressions. As noted in the text the simple weighting by state-level variance substantially reduced the severity of heteroskedasticity. In the pre-1997 period, the White/Koenker test p-value = 0.0652 using levels of exogenous variables as indicators and p = 0.1252 using levels and squares. While this is not strong evidence, we use heteroskedasticity robust standard errors for the GLS estimates in Table 2a. For the pre-1997 IV estimation, and the estimations in the post-1997 period, there is really no evidence of heteroskedasticity. But again, to make the results comparable across periods, we report heteroskedasticity robust standard errors for all estimations. It makes very little difference. Because there is no heteroskedasticity evident in the IV estimations, the use of GMM is not called for.

Table 1a
Summary Statistics 1993-1996

Variable	Obs	Mean	Std. Dev.	Min	Max
hivofall	104	13.24851	14.88431	0	82.19115
afdctanf	104	104.3043	34.58682	42.15	199.4
doctors	104	1.986058	.4169292	1.3	3.37
health	104	9.04	7.21813	.84	34.06
pcy	104	20.89681	3.685227	14.745	34.174
black	104	11.59163	10.29419	.41	36.04
immigrant	104	1.410497	1.276275	.2807863	7.924762
college	104	21.175	4.777694	12.7	33.3
age	104	24.87981	1.226834	22.2	27.9
noinsure	104	14.1625	3.863526	7.3	24.1
metro	104	66.00192	19.853	29.7	100
jewish	104	.8538462	1.16926	.	1 5.6
catholic	104	15.31154	12.07161	2.3	41.8

Table 1b
Summary Statistics 1998-2002

Variable	Obs	Mean	Std. Dev.	Min	Max
hivofall	130	13.05899	13.65462	0	73.01627
afdctanf	130	152.9163	134.8207	1.671951	1360.686
doctors	130	2.205769	.4326605	1.54	3.61
health	130	10.28577	7.04639	.73	33.19
pcy	130	26.90446	4.763688	18.958	42.706
black	130	11.62952	10.38086	0	36.80362
immigrant	130	1.468462	1.207264	.2	6.7
college	130	22.79769	5.183882	12.3	35.7
age	130	23.56187	1.401236	20.48541	29.83592
noinsure	130	14.00308	3.43624	7.1	24.2
metro	130	66.76615	19.21766	29.6	100
jewish	130	.9407692	1.325171	0	5.8
catholic	130	15.7086	11.68248	2.255109	41.82228

Table 2a
Pre-1997 Estimations

	Doctor	Health	GLS	IV
jewish	0.573***	-26.63***		
	(2.83)	(-3.14)		
catholic	0.00863***	-0.322***		
	(3.82)	(-4.01)		
jewmet	-0.00477**	0.354***		
	(-2.01)	(3.60)		
metro	0.0113***	0.0922		
	(5.41)	(1.60)		
afdctanf	0.000882	0.179***	0.0523**	0.0787***
	(0.93)	(7.12)	(2.21)	(2.67)
age	-0.00864	-2.241***	0.352	-0.107
	(-0.35)	(-3.66)	(0.80)	(-0.19)
college	0.0209***	0.378***	-0.280*	-0.235
	(4.04)	(3.52)	(-1.92)	(-1.36)
immigrant	-0.161***	0.104	-0.0226	0.658
	(-3.19)	(0.10)	(-0.02)	(0.55)
noinsure	-0.0171**	0.0560	-0.135	-0.0624
	(-2.41)	(0.39)	(-1.12)	(-0.44)
black	-0.00370	0.962***	1.236***	1.436***
	(-0.81)	(7.65)	(8.51)	(7.59)
pcy	-0.0195	-0.824***	0.984***	0.788**
	(-1.30)	(-3.42)	(2.93)	(2.10)
midatlan	-0.0892	-18.33***	49.91***	49.86***
	(-0.38)	(-2.93)	(4.63)	(4.60)
souatlan	-0.323**	21.90***	17.64***	20.91***
	(-2.35)	(4.27)	(7.24)	(6.15)
escentra	-0.536***	17.37***	14.21***	17.19***
	(-3.16)	(3.20)	(3.32)	(3.44)
wscentra	-0.511***	17.68***	14.76***	17.19***
	(-3.15)	(3.24)	(4.74)	(4.54)
encentra	-0.864***	30.62***	12.52***	18.52***
	(-7.57)	(6.42)	(5.86)	(3.95)
wncentra	-0.724***	26.17***	14.80***	18.29***
	(-5.01)	(4.65)	(7.23)	(5.42)
mountain	-0.814***	16.80***	18.44***	21.23***
	(-4.93)	(3.61)	(6.55)	(5.68)
yr1994	0.0311	-0.276	-0.235	-0.147
	(0.95)	(-0.41)	(-0.32)	(-0.19)
yr1995	0.0877**	0.623	-1.081	-0.839
	(2.04)	(0.69)	(-1.27)	(-0.88)
yr1996	0.172***	0.276	-2.096**	-2.121*
	(3.68)	(0.24)	(-2.12)	(-1.93)

contd. table

	Doctor	Health	GLS	IV
const	2.106***	19.83	-37.55***	-35.01***
	(2.77)	(1.26)	(-3.08)	(-2.69)
doctors			-1.601	0.423
			(-0.85)	(0.15)
health			-0.374***	-0.626***
			(-4.85)	(-3.44)
N	104	104	104	104
Firststage-F	44.37	10.85		
overid p-value				0.7837
endog p-value				0.1727
Hettest p-values				
IV levels	0.0000	0.1407	0.0652	0.3645
IV levels & sq	0.0001	0.0120	0.1252	0.2735

Heteroskedasticity robust t statistics in parentheses
* p<0.10, ** p<0.05, *** p<0.01

Table 2b
Post-1997 Estimations

	Doctors	Health	GLS	IV
jewish	0.143	-23.86*		
	(0.59)	(-1.98)		
catholic	0.0145***	-0.194***		
	(8.32)	(-2.78)		
jewmet	-0.00168	0.282**		
	(-0.61)	(2.08)		
metro	0.00757***	0.104**		
	(5.30)	(2.08)		
afdctanf	-0.000184*	0.00679*	-0.00281*	-0.00323
	(-1.94)	(1.96)	(-1.80)	(-1.54)
age	0.0145	-0.785*	0.0194	0.0479
	(1.25)	(-1.93)	(0.10)	(0.17)
college	0.0117**	0.452**	-0.160	-0.459**
	(2.12)	(2.30)	(-1.01)	(-2.35)
immigrant	-0.157***	-0.755	1.276**	1.216*
	(-5.49)	(-0.70)	(2.29)	(1.67)
noinsure	-0.0265***	-0.221	-0.132	-0.0914
	(-4.39)	(-1.01)	(-1.06)	(-0.72)
black	0.00981***	0.719***	0.951***	0.607***
	(3.15)	(6.97)	(8.83)	(4.16)
pcy	0.0257***	-0.517*	0.239	0.291
	(2.83)	(-1.79)	(1.36)	(1.18)

contd. table

	Doctors	Health	GLS	IV
midatlan	-0.0227	-12.67*	26.30***	28.80***
	(-0.15)	(-1.91)	(7.13)	(7.39)
souatlan	-0.344**	0.178	20.02***	26.51***
	(-2.60)	(0.04)	(6.49)	(6.71)
escentra	-0.655***	-7.948*	22.29***	32.35***
	(-4.43)	(-1.70)	(4.83)	(6.67)
wscentra	-0.671***	-3.868	16.05***	25.13***
	(-4.63)	(-0.73)	(4.70)	(5.71)
encentra	-0.826***	8.188	13.57***	17.29***
	(-7.23)	(1.46)	(6.04)	(6.54)
wncentra	-0.632***	5.382	14.58***	20.27***
	(-4.61)	(0.87)	(5.82)	(6.59)
mountain	-0.634***	0.536	16.80***	24.26***
	(-4.76)	(0.10)	(5.81)	(6.32)
yr1999	0.0230	2.114	-2.362**	-3.709***
	(0.44)	(1.14)	(-2.14)	(-2.77)
yr2000	-0.101***	-0.130	-1.518**	-0.940
	(-2.63)	(-0.09)	(-2.11)	(-1.00)
yr2001	-0.0668	0.678	-1.887**	-1.477
	(-1.58)	(0.42)	(-2.52)	(-1.44)
yr2002	0.0121	1.980	-4.736***	-4.829***
	(0.23)	(1.04)	(-5.64)	(-4.30)
const	1.312***	21.99*	-19.11*	-31.27***
	(3.24)	(1.98)	(-1.97)	(-2.85)
doctors			2.739	7.376**
			(1.35)	(2.56)
health			-0.274***	0.0802
			(-4.01)	(0.48)
N	130	130	130	130
Firststage-F	34.32	4.80		
overid p-value				0.2047
endog p-value				0.0035
Hettest p-values				
IV levels	0.0000	0.0002	0.7350	0.7417
IV levels & sq	0.0002	0.0066	0.1539	0.9157

heteroskedasticity robust t statistics in parentheses

* p<0.10, ** p<0.05, *** p<0.01

Notes

1. Some may regard this logical link as rather tenuous. We agree that the average low income woman may not change her sexual behavior or even fertility decisions based upon characteristics of the welfare program. We hypothesize that changes in welfare requirements and differences in welfare payouts will influence the behavior of women at the margin. Women who are already considering having multiple partners or engaging

in risky sexual behavior will be more likely to do so if the cost of doing so is – even very slightly – diminished. Though we regard this behavior as atypical, aggregating data to the state level is likely to reveal that when the cost of any type of behavior is lowered, more of that behavior takes place.

2. For a detailed review of the pertinent literature, see Ressler et al. (2005, 2006).
3. Although all 50 states and the District of Columbia report AIDS cases to the CDC, our data set is limited to those 26 states with laws or regulations requiring confidential name-based reporting of HIV cases and which consistently reported HIV numbers in 1993. They are: Alabama, Arizona, Arkansas, Colorado, Connecticut, Idaho, Indiana, Louisiana, Michigan, Minnesota, Mississippi, Missouri, Nevada, New Jersey, North Carolina, North Dakota, Ohio, Oklahoma, South Carolina, South Dakota, Tennessee, Utah, Virginia, West Virginia, Wisconsin, and Wyoming.
4. TANF guidelines went into effect at the beginning of the 1997 fiscal year. However, our data are gathered in accordance with the calendar year. Thus in the 1997 calendar year, AFDC was in place during the first six months and TANF during the last six months. As a result, we omitted 1997 from our estimations.
5. Unfortunately, published data on state-wide HIV incidence by exposure category (e.g., through heterosexual contact, homosexual contact, receipt of blood transfusion, injecting drug use, etc.) are not available. Only a limited amount of data has been published which indicates exposure category, or transmission mechanism, by state (only national data are readily available for both AIDS and HIV cases by exposure category). These data, the proportion of a state's total AIDS cases generated from heterosexual contact, have been published for the years 1993-2002 in APIDS: AIDS Public Information Data Set. It is this limitation which dictates the size of our state-based data set. In December 1999, the CDC issued guidelines recommending that all states adopt name-based HIV surveillance. Note that as of July 2001, only 34 of the 50 states had implemented name-based HIV case reporting to the CDC. It was not until April 2008 that all states began using confidential name-based reporting to collect HIV infection data.
6. According to the CDC, "An estimated 252,000 – 312,000 persons living in the United States are unaware that they are infected with HIV…." This quote comes from June 2, 2006 CDC publication entitled Epidemiology of HIV/AIDS —-United States, 1981—2005.
7. The R-squared of the auxiliary regression of BLACK on the state-level dummies is 0.9999 for the pre-1997 years, and 0.9721 for the post-1997 years.
8. See Greene (2008, p.166) and Baum et al. (2007) for a description of the tests. They were implemented with Stata 10.0 command IVHETTEST.
9. For IV estimation HETTEST reports the p-values of the Pagan-Hall general test. There is no indication of heteroskedasticity in the IV residuals for either period. See Baum et al. (2003, 11-14) for discussion of the Pagan-Hall test. The use of robust standard errors does not change the level of significance of any key variable. A complete set of results is available upon request.
10. During this time period, we cannot reject the null hypothesis that DOCTORS and HEALTH expenditures are exogenous. The "endog p-value" from the regression based Wu-Hausman test is not significant at even the 10% level. We report the IV results for this period for completeness.
11. Many of these tests are available in Stata 10.0 using the module IVREG2, by Baum et al. (2007b).

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