The Effect of Pre-PRWORA Waivers on AFDC Caseloads and Female Earnings, Income, and Labor Force Behavior

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Abstract

A recent report of the Council of Economic Advisors (CEA) examined the effect of pre-PRWORA waiver activity in the early 1990s on the AFDC caseload, and found that waivers made a substantial contribution to the reduction in the AFDC caseload although less than that of the declining unemployment rate. The CEA study used an aggregate state-level caseload model estimated over the period 1976-1996. This paper uses the CEA methodology but applies it to microdata from the Current Population Survey (CPS), where information is available on labor force activity, earnings, and income, as well as on demographic characteristics such as age, sex, and education. The results from the CPS show that less educated women had gains in labor force attachment in the form of increased weeks worked and hours of work as a result of waivers, but no statistically significant increases in earnings or wages. The only statistically significant earnings or wage increases occurred among better-educated women, generally those with at least twelve years of education. The latter result is, however, somewhat sensitive to which historical recession is used to forecast the effect of the business cycle.

The passage of the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) in September 1996 has led to increased interest in the effects of welfare reform on caseloads and on individual and family well-being. A number of evaluation efforts of PRWORA are underway around the country but these will not be issuing findings for some time. In the meantime some research attention has been directed toward analyzing the effects of pre-PRWORA waiver activity, which by definition ended in 1996. While there have been some evaluation reports of specific waiver demonstrations during this period, another approach to evaluation is to conduct a cross-state econometric analysis, using pre-1996 variation in waiver activity across states to estimate the effect of waivers on various outcomes. That is the approach discussed in this paper.

The most widely-circulated study taking this approach is the report of the Council of Economic Advisors (1997), hereafter called the CEA Report. The CEA Report used aggregate state-level AFDC caseload data as the outcome, and the state unemployment rate and state waiver activity as independent variables. Using data from 1976 to 1996, the CEA Report found that waivers significantly reduced the caseload. However, the Report also found that the decline in the unemployment rate had a large effect, greater in magnitude than that of waivers. The findings of the Report indicated that the unemployment-rate decline explained from 31 percent to 44 percent of the caseload decline between 1993 and 1996, depending on the model, while the increase in waiver activity explained between 14 and 30 percent.

There has been considerable discussion of the CEA Report's methodology and findings (Martini and Wiseman, 1997; Ziliak et al., 1997) as well as an update and extension of the Report (Levine and Whitmore, forthcoming).[1] A number of issues have been raised concerning whether other policy developments such as those in Medicaid or the EITC could have been alternative or additional contributors to caseload decline; the coding of the waiver activity in the states and the difference between official waiver start dates and genuine implementation dates; whether waivers were endogenous and introduced with a higher probability in states whose caseloads were going to decline for other reasons; and modeling issues such as the inclusion of the lagged dependent variable, the length of lags used, whether monthly or annual data are preferred, estimating in levels vs. first-differences, and many other issues. Another issue is the question of whether the waiver activity was significant enough in the states to have plausibily generated the size of effect estimated in the CEA Report, and exactly what the waiver variables were proxying--a specific policy or a more general set of policies which, together, might have had an effect.

Most of these issues will not be joined here, with one exception. That one exception concerns the business cycle, whose contribution to caseload growth is subjected to sensitivity testing in the last section below. Aside from this one departure from the CEA specification, the bulk of the paper takes the CEA methodology as given and studies the implications of applying that methodology, with all its strengths and weaknesses and all the uncertainty surrounding it, to micro data from the CPS.

Microdata should have an important role to play in econometric evaluations of welfare reform.

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[1] See Blank (1997) and Stapleton et al. (1997) for other caseload models, but in these two cases focused more on the origins of the increase in the caseload in the late 1980s and early 1990s than on its later decline.

Aggregate data at the state level necessarily gloss over the differences within a state's population and do not permit analysis of the groups most likely to be affected by welfare reform. Not only should individual data on the most-likely-affected groups permit a more precise estimation of the effect of the welfare, they should also aid in the detection of spurious effects that might have been estimated at the aggregate level. For if effects are detected in the microdata even for groups that are credibly unaffected by the reform, this suggests that the aggregate estimate might be spuriously picking up some more general trend. In addition, while aggregate data are available on the AFDC caseload, none are available for individual and family outcomes of interest like earnings, family income, and labor force behavior. Consequently, the CPS can contribute to the study of welfare reform to outcomes other than the caseload.

The study reported on here applies the CEA methodology to CPS microdata. The major distinction made with the microdata is between women of higher and lesser education levels, for education is the best single proxy for labor market skill and hence for outside opportunities off welfare. It is the best single scale of how well-off or poorly-off a welfare recipient is, in general. The analysis here finds that the CEA methodology implies that more-educated and less-educated women are affected in different ways by welfare reform. For the least-educated women, welfare reform decreases AFDC participation and increases annual hours of work and annual weeks of work but has no statistically significant effects on earnings, wages, or family income. For somewhat more educated women, on the other hand, welfare reform increases earnings as well as hours of work. The findings of the paper demonstrate that a recognition of the diversity and heterogeneity of the population, and the differences in their response to welfare reform, is critical to understanding the effects of that reform.

The CEA methodology requires that the effect of the business cycle on caseload and other outcomes be estimated in order that the effects of welfare reform be estimated, for the latter are estimated after, implicitly, netting out the caseload trends that would have occurred even in the absence of those reforms from the unemployment rate decline. There were only two significant recessions between 1977 and 1995, and they had different effects on the caseload. Moreover, cross-state variation in the unemployment rate has had different effects over time. The analysis here shows that the caseload has become more cyclically sensitive over time. Hence forecasting what the effects of the decline in the unemployment rate would have been in the absence of the waiver activity could be argued to be best conducted with the most recent data possible, namely, data in the late 1980s. The results here show that when the most recent business cycle is used to estimate the model, all aggregate caseload effects of waivers disappear, as do the positive effects on earnings of more-educated women. More analysis is needed to resolve this issue.

I. REPLICATING THE CEA REPORT FINDINGS

The CEA Report used annual data from 1976 to 1996 on (i) average annual monthly AFDC caseloads per capita, (ii) average annual monthly unemployment rates, and (iii) waiver activity within

each year (see below), all at the state level.[2] The analysis considers the relative influences on the caseload of the unemployment rate and of waivers. The waivers were coded from DHHS information on the approval dates for AFDC waivers; dummy variables indicate whether the state had any waiver in effect in the year in question (thus the dummy variable goes from 0 to 1 and stays at 1 for as long as the waiver is in force).[3] Additional dummy variables were constructed which indicate whether waivers were in effect for particular policies--JOBS exemptions, JOBS sanctions, family caps, time limits, and other specific types of reforms. Only waivers since 1993 were coded, on the grounds that waivers before then did not affect a sufficient fraction of the caseload. Waivers were also coded only if the waiver activity was statewide; of the 43 waivers in effect in the final year, only 35 were in this category.[4]

Figure 1 shows the trends from 1976 to 1996 in the aggregate AFDC caseload, the unemployment rate, and waiver activity. The AFDC caseload, shown by the upper curve, was fairly stable until 1989, when it began to grow significantly. The growth peaked in 1993 and declined thereafter.[5] The unemployment rate, shown by the middle curve, has also gone through considerable gyrations as well, with two recessions occurring over the period. The caseload and unemployment cycles have a rough positive correlation in the late 1970s and after 1988, but not from 1979 to 1987. The CEA Report ascribes the latter to the influence of OBRA, which had a caseload-depressing effect that may have prevented the caseload from rising, although this seems an inadequate explanation for the long length of the lack of relationship. In any case, the methodology used to estimate waiver effects, as shown below, does not make use of this aggregate time-series correlation (or lack of it) because state-level data are used, not aggregate data.

The lower curve in the figure shows the fraction of states with statewide waivers and hence demonstrates the rise of waiver activity after 1991. The figure makes clear that the separation of the effects on the caseload of waivers, on the one hand, and the declining unemployment rate, on the other, must necessarily be a major challenge, given that they both changed course around the same time--1992--and given that they continued changing in the same direction thereafter.

The models estimated in the CEA Report use the log of the per capita caseload as the dependent variable, and include year dummies, state dummies, and, in most cases, state dummies multiplied by a year variable (i.e., a time trend) as independent variables. Also included in the equations

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[2] Thus, with 51 states for 21 years, a total of 1071 observations were used in the estimation. All caseload, unemployment, and waiver data are on a fiscal year basis. Thus the variables for year t are calculated as averages of the months from October of year t-1 to September of year t.

[3] In the year in which the waiver first appears, the variable is coded as the fraction of the year that the waiver was in effect.

[4] An Appendix in the CEA Report shows the exact waiver dates and provides more discussion of the nature of the different types of waivers.

[5] The caseload has continued to decline markedly since 1995.

are variables for the unemployment rate, the presence of a welfare waiver, and the state AFDC guarantee. With both year dummies and state dummies included, the coefficients on the unemployment rate and the waiver variables represent the effects of a change in their respective variables on the deviation from a state-specific trend in the per capita caseload. As noted above, therefore, the analysis does not make use of the relationship between the caseload, the unemployment rate, and waivers at the aggregate level, but rather the relationship between these variables at the state level, and in first-difference, or change, form (i.e., the relationship between year-to-year changes in waivers, changes in unemployment rates, and changes in caseloads across different states).[6,7]

The March Current Population Survey (CPS) can be brought into the analysis only for 1977 and after, for only starting in 1977 were all individual states identified (see below). Thus the year 1976 is deleted from the analysis of administrative data as well, for comparability. In addition, the analysis below deletes the year 1996 because it is close to the enactment of PRWORA.[8] Some caution is reasonable under these circumstances given that the heightened welfare reform activity at the federal level just prior to that enactment could have influenced state caseload-reducing efforts. However, as will be shown momentarily, while some of the tangential findings of the CEA Report are sensitive to the inclusion of this year, the main ones are not. Nevertheless, the 1996 year is still excluded from the main analyses to ensure that no ambiguities will arise in the interpretation of the findings in this respect.

It is worth noting that the caseload modeling strategy of the CEA report, like many caseload modeling approaches, captures both the entry and exit effects of welfare reform on the caseload. Unlike many studies of waivers which determine the rate at which families who are initially on the welfare rolls leave them following the enactment of a waiver, studying the caseload as a whole implies that the effects of a waiver on entry are also captured. A decline in entry rates can lower the caseload just as an increase in exit rates can, and hence the caseload can fall for either reason. Thus the caseload modeling approach has a distinct advantage over welfare "leaver" studies and other waiver

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[6] The CEA model does not include some of the main variables posited in the economic model of welfare participation to affect the takeup decision--for example, potential wage rates and exogenous nonlabor income. The implicit assumption is that these variables do not vary in their trends in waiver and non-waiver states.

[7] The major danger in this method is that those states who adopted waivers may have had different changes in their caseloads than non-waiver states even in the absence of adopting a waiver. For example, Appendix Table 1,which reports caseloads for waiver and non-waiver states, shows that waiver states had higher caseloads than non-waiver states. If those caseloads were higher in the pre-waiver period, then they may have declined even in the absence of waivers. We will not address these types of specification issues but will maintain the CEA model throughout.

[8] As noted in a prior footnote, all data are in fiscal year form so the 1996 data end in September 1996. Thus the amount of actual overlap is trivial.

evaluations which take as their study populations only families initially on welfare.[9]

Replication of CEA Results. Columns (1)-(4) of Table 1 estimate models whose specification--that is, dependent and independent variables-- are exactly the same as those in the CEA report.[10] As in the CEA Report, the dependent variable equals the log of a ratio whose numerator is the size of the AFDC caseload taken from administrative data and whose denominator is the state population. The independent variables are as defined previously. The major difference in the regressions reported in Table 1 and those appearing in the CEA Report is that the Table 1 regressions exclude the years 1976 and 1996.[11]

The basic specification in column (1) allows for effects of the current and lagged unemployment rate and an overall waiver dummy. The lagged unemployment rate and waiver coefficients are significant, as in the CEA Report, and similar in magnitude. The coefficients in the CEA Report for this specification imply that the unemployment rate explains 45 percent of the decline in the per capita caseload between 1993 and 1996 and that the waiver variable explains 13 percent (Council of Economic Advisors, 1997, Table 3). The coefficients in Table 1 for this specification imply contribution percents of 47 and 15, respectively.[12]

The results for the specifications in columns (2), (3), and (4) are, however, rather different than those in the CEA report, at least for the waiver variable coefficients. Column (2) in Table 1 shows a barely significant coefficient on a waiver for work requirement time limits and a more significant effect of waivers for a family cap, whereas the CEA results showed neither of these to be significant; only JOBS sanctions effects were significant in the Report. The change is entirely the result of excluding year 1996, when several states added new waivers. The sensitivity of these coefficients to the years included and excluded is unfortunate and compounds the already existing suspicion that the separate effects of the individual waivers are not being correctly picked up by these variables. The results in column (3) show significant effects neither for the any-waiver variable nor for the JOBS-sanctions variable, the latter of which was significant in the CEA report. Again, the exclusion of 1996 is responsible for this result.

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[9] See Moffitt (1996) for a discussion of entry effects in the context of education and training programs for welfare recipients.

[10] To be precise, these models appear in columns (3)-(6) of Table 2 of the CEA Report.

[11] The Table 1 regressions also do not use state population weights, unlike those in the CEA Report. However, this difference is responsible for only a small portion of the difference in results. Estimates using population weights and the years 1976-1996 exactly replicate the results in Table 2 of the CEA Report. Phillip Levine graciously provided the data used in the CEA Report.

[12] A specification was also estimated which replaced the waiver dummy by dummy variables for the number of years since the waiver was approved, to test whether the effects were growing, declining, or constant. The coefficients revealed a growing effect with each year (i.e., a more negative coefficient with each passing year).

Column (4) was tested in the CEA report to determine whether there was an advance response to waivers, by including a one-year lead variable for both the any-waiver and sanctions variables.[13] The CEA report found significantly negative effects of both, whereas neither is significant in Table 1. Again, the exclusion of 1996 is the reason for the difference.[14]

Given that the specifications in columns (2)-(4) test tangential hypotheses, and that the simple specification in column (1) is robust to inclusion of different years--and since the exclusion of 1996 is a conservative strategy, as argued above-- the rest of the analysis in this paper will use the specification in column (1).[15]

II. EXTENDING THE MODEL TO THE CPS

The CPS is a relatively large monthly household survey which is representative of the U.S. population. For most of the analysis we use only the March survey in each year, which gathers information on AFDC receipt, employment and labor force behavior, and annual earnings and income at both the family and individual level. Identifiers for all states are available beginning in 1977, and we therefore use the March CPS files from 1977 to 1995.

We begin by replicating the CEA results just reported to the closest degree they can be, using information in the CPS. Rather than administrative counts of AFDC recipients in each year, we use the number of women 16-54 reporting AFDC receipt in each state. In addition, rather than dividing by total population, we divide by the number of women 16-54. Since women in this age range are the

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[13] The coefficients on the lead variables are open to multiple interpretation. One interpretation, that preferred by the CEA Report, interprets the coefficients as representing advance, or anticipatory, behavior on the part of the states, given the publicity surrounding the submission of waiver requests to the federal government. An alternative interpretation is that the coefficients of the lead variables are measuring endogenous policy responses, with causality running from a declining caseload to a later waiver introduction. The latter interpretation is the basis, in fact, for econometric tests for unobserved heterogeneity in panel data which use the inclusion of a future value of the dependent variable as a regressor. On the assumption that future variables cannot be true causal influences on contemporaneous outcomes, the coefficients can be interpretated as measuring persistent unobserved individual effects. See Heckman (1981) and Heckman and Borjas (1980). Without further analysis, these and other interpretations cannot be distinguished.

[14] The lead variables also present a special problem if the year 1996 is included because in that case their use requires an assumption of what would have happened in 1997 had PRWORA not passed. The CEA Report coded the lead variables in 1996 as equal to their 1996 values, under the assumption that states did not expect further waiver activity in 1997. Excluding 1996 allows us to avoid having to make an assumption about expectations in that year.

[15] The CEA report (Table 2) also included two simpler specifications, one without the lagged unemployment rate and one without state*trend variables. Estimation of those models on the 1977-1995 data also show, as in the CEA report, significant effects of the any-waiver variable.

primary population group from which AFDC recipients are drawn, this should provide a more precise measure of the rate of receipt than that out of the entire population.[16] With the dependent variable defined in this way for each state in each year 1977-1995, we can use for regressors the exact same set used in the CEA analysis--state and year dummies and state-level variables for waiver activity, the unemployment rate, and the state benefit level.[17]

The last two columns in Table 1 show estimates using the specification in column (1). The dependent variable is the log of the AFDC participation rate in the state and year in question, as defined above. One column shows the results when the simple unweighted mean is used, and the second column shows the results when weighted least squares is applied using as weights the ratio of the mean administratively-defined caseload per capita in the state to the mean CPS-defined AFDC participation rate in the state (the weights are normalized to one). These weights adjust for the sampling error in the CPS which arises from small sample size by blowing up each of the CPS means to its proper population proportion as represented in the administrative data. The small samples in some states in some years is the main obstacle to using the CPS for state-specific policy analysis in general, and the use of weights in this way reduces the impact of that problem.[18]

As the results show, the CPS generates remarkably similar estimates for the effects of the state unemployment rate on the caseload as those which use administrative caseload data. The effects of waiver policy are also highly significant, and considerably larger in magnitude than in the equations using the administrative caseload data. Thus the CPS is clearly consistent with the finding of significant

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[16] It could be argued that an even more precise population on which to measure AFDC receipt is the population of female heads of family. However, there is some evidence that female headship itself responds to AFDC policy. Therefore, as a conservative strategy to avoid missing some of the policy effects, we examine only women as a whole.

[17] Appendix Table 1 shows the means of the CPS participation rates. The sample sizes in the CPS are an issue whenever the data are used for state-level analysis. The minimum number of observations for any state (averaged over the number of observations in each of the 1977-1995 years for that state) is 387. The average is 847. These samples are adequate for the state-level analysis reported in Table 1.

[18] Using WLS with weights of this kind is an application of the weighted maximum likelihood methods developed in choice-based sampling, most particularly that of Manski and Lerman (1977). The Manski-Lerman estimator was developed for a case in which a binary outcome equation is to be estimated (e.g., transportation mode choice, denoted as y) where the data are composed of two samples, one composed of individuals with y=1 and one composed of individuals with y=0, but not from a random sample and hence not in correct population proportions. Manski and Lerman showed that reweighting the sample to blow it up to population proportions results in consistent estimates of the coefficients. For studies from a related literature, that concerning the use of aggregate totals and outside data in microdata equation estimation, see Imbens and Lancaster (1994) and Imbens and Hellerstein (1999).

waiver effects on caseloads.[19]

Disaggregating and Using Other Outcomes with the CPS. As noted in the introduction, the CPS data afford the opportunity of examining effects of waivers on subcomponents of the caseload and on the within-state population, thereby permitting a more disaggregate analysis than is allowed with state-level administrative data. Only two demographic dimensions are examined here, for simplicity: education and age. Education is classified into four categories: less than 12 years of schooling, 12 exactly, 13-15, and 16 or more years. Age is classified into four categories: 16-25, 26-34, 35-44, and 45-54. Within each state and each year, these four education and age categories are used to form sixteen age-education cells, within each of which a mean AFDC participation rate is calculated.[20] The resulting sample has 15,504 observations (51 states, 19 years, 16 demographic groups per state and year). The AFDC participation-rate models already estimated can be reestimated on this larger sample, using the same state-level regressors as used in the previous analysis. However, age and education effects can now also be allowed, and waiver effects can be estimated separately for different age and education groups as well. Some of the age-education cells in some states have very few observations, so we shall conduct sensitivity analysis to the exclusion of small cells.[21]

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[19] These results are somewhat sensitive to the treatment of the retrospective nature of the CPS welfare receipt questions, which ask the respondent for such information for the prior calendar year. The CEA data pertain to the fiscal year, so it is not possible to align the CPS data to the CEA time frame. The results shown in Table 1 align the CEA fiscal year with the year of the March survey date; the results are, for reasons we cannot discern, weaker when the CEA fiscal year is aligned with the CPS year prior to the March survey date.

[20] Thus the analysis here still uses grouped data, even if using sixteen times as many groups as used in the aggregate state-level analysis. It may be reasonably asked if it would not be preferable to use the individual micro data rather than the means of the sixteen cells in each state and year. The answer is that a micro regression with sixteen dummies for the age-education cells would have no advantage over using the group means of those sixteen cells except for efficiency. Efficiency gains could be had because the standard errors of the means could be used in the estimation. Of course, use of variables in addition to age and education could result in a more meaningful extension of what we have done here, but that extension could also be conducted either with group means over more variables, or at the micro data. In the limit, if every micro observation has a unique set of regressors, a microdata regression is equivalent to a grouped regression because each individual represents a single "group".

[21] Small cell sizes are not a problem if the waiver variable were kept at the state level and not interacted with age and education. In that case, using age-education means instead of aggregate state means can only increase efficiency. However, when the waiver effect is allowed to be different for some of the sixteen cells, as it will be here, these effects will be less precisely estimated than the average effect at the state aggregate level because the effective sample size going into each waiver coefficient is

There is relatively little formal evidence upon which to generate hypotheses regarding the differential effect of waiver policies by age and education. The strongest dimension upon which generally accepted knowledge of waiver policies can be applied is education, for education is generally regarded as the best proxy for labor market skill and hence potential earnings off the welfare rolls. All available evidence, which is admittedly anecdotal, suggests that the caseload-reducing effects of waiver policy have had their greatest impact on those women on the rolls who are in a better position, i.e., the better educated. There is widespread agreement that those with greater labor market opportunities have been disproportionately represented among those leaving the rolls, thereby increasingly leaving the welfare caseload composed of the worse-off cases, with the fewest outside opportunities.

However, much of this consensus is based upon the presumed effects of the decline in the unemployment rate, which is almost surely likely to have drawn off the rolls those with higher labor market skills. The effects of welfare reform are to some extent more ambiguous, for while it should be presumed that the increased emphasis on work which is the central feature of most welfare reform programs would be most easily accommodated by those with more labor market skill, it is simultaneously the case that those with less labor market skill may be less able or willing to comply with work requirements while on the rolls and hence may be more likely to be sanctioned or otherwise leave the rolls because of an inability to cope with the requirements. Evidence on this issue is thus far lacking.

A second potential of the CPS is to permit the examination of outcomes other than AFDC participation. For this purpose, we construct for each of the age-education cells in each state in each year the means of annual weeks worked, annual hours worked, and real annual earnings of the woman in question, and also real family income of the family in which she resides.[22] Welfare reform and waiver activity may, on the one hand, improve these outcomes if such policies are successful in stimulating former welfare recipients to improve their labor market position, while they may, on the other hand, leave women worse off if they are not able to do so.

The problem of small samples in some of the age-education cells in some states will be addressed in two ways. First, some of the equations will be estimated only on state-demographic cells with more than some minimum number of observations. Second, equations will also be estimated using the outgoing rotation group (ORG) data from the CPS. Every month, about one-quarter of the CPS sample is asked questions about weekly earnings. Taking the samples of individuals asked these questions over all 12 months in a year yields a sample that is approximately three times the size of the single-month March CPS. Models identical to those estimated with the March CPS can be estimated with these data, thereby implicitly gauging the importance of sample size to the results.

Results. Table 2 presents results of models for the AFDC participation rate using the March CPS

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smaller. The average cell size taken over the 51 states at this disaggregated level is 55 and ranges from 1 to 564. We will continue to reduce these efficiency losses by weighting the observations by the state-level administrative weights, however.

[22] Zeroes are included for all the variables.

data. Because around a quarter of all the education-age cells have a zero participation rate, the absolute level of the participation rate is used rather than its log. The first column of Table 2 shows an aggregate-state level regression for comparability. The results indicate that the signs and significance levels of the coefficients are similar to those in the last column of Table 1, and imply that the waivers reduced the fraction of women 16-54 who were on AFDC by about eight-tenths of a percentage point. The second and third columns use the within-state education-age demographic cell data. The first regression simply includes education and age dummies but continues to estimate an average, state-level welfare effect. The coefficients on the education and age dummies are usually significant and in the expected direction, with higher education groups having a lower probability of being on AFDC and with participation rates highest among women 26-34. The waiver coefficient is significant and of approximately the same magnitude as that in the state-level regression, indirectly indicating that the education and age compositions of waiver and non-waiver states were not very different.

The more important results are those in the third column, which report separate waiver effects by education category. The results indicate that the largest effects of waivers on AFDC participation rates occurred among women with less than 12 years of education, for whom waivers reduced participation by 1.7 percentage points (recall that the base is all women in this education category). This is in accord with expectations, for the impact of welfare reform should be expected to occur among the most disadvantaged and most welfare-prone. It also suggests that the state-level results are not spuriously picking up effects that were occurring across the education distribution, thus providing some support for the credibility of those results.[23]

Table 3 shows the results for the other outcomes taken from the CPS. The table shows the results obtained when state aggregates are used as well as with the demographic cell data are used. All of the estimated effects at the state level are insignificant except for annual earnings, which is significantly positive and implies that waivers increased earnings by $274 per year.[24] The results are somewhat different when the results are disaggregated by education group, however. Both the weeks worked and annual hours worked of those with the least education increased from waivers, but neither their annual earnings nor weekly earnings increased. The aggregate annual earnings effects occurred instead among those with 12 years of education only. For that group, there was a significant increase in hours of work, although of somewhat smaller magnitude than that for the least educated group.

These findings, therefore, suggest that waivers had a major impact on very low-skilled women by reducing their rates of participation in AFDC (through both entry and exit, presumably) and increasing their labor force attachment and levels of connection to the workforce (again, possibly

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[23] On the other hand, the waiver coefficients for the four education categories are not statistically significant from one another because the standard errors are relatively large. It should also be noted that the coefficients on these regressions on the variables which interact education level and the unemployment rate, and education level and year dummies, are significant and are needed as controls, for the waiver coefficients change when they are not included. The unemployment effects are consistently less negative for more educated women, for example.

[24] These and all other dollar figures in the paper are in real 1992 dollars.

through non-entry as well as exit). However, there was little effect on their annual or weekly earnings, indirectly indicating that the wage rates of the jobs they obtained were very low. Somewhat more educated women--those with 12 years of education, to be exact--whose participation rates also fell, experienced both increases in workforce attachment as well as increases in earnings. This suggests that it was the better-educated, presumably more skilled, women who did better off the rolls, and were able to better replace their lost welfare benefits with earnings.

The final column in the table shows the effects on family income, where the findings indicate no significant effects for any education group. Indirectly, therefore, this implies that the least educated women must have replaced their welfare benefits from some other source, either through income from others in the family or from other government programs. This bears more investigation in the future.

The standard errors on many of the coefficients in the Table are relatively high and suggest that small sample sizes in the CPS may be weakening the results. The results for weekly earnings in Table 3, which come from the much larger ORG data set, are, however, no more significant than the March CPS coefficients and, in fact, generally less so. Of course, the less significant coefficients in the ORG data could be a reflection of a lack of increase in wage rates as a result of waivers; the positive effects on annual earnings of the twelve-years-of-education group could be entirely a result of their increases in hours worked. Table 4 reports results when small cell sizes are excluded from the regressions, as an additional check on this aspect of the findings. Virtually all results in the table are the same in general magnitude to those in Tables 2 and 3, with the exception that welfare participation and annual earnings effects are stronger for those with 13-15 years of education, a moderately implausible result at least in the magnitude implied by the coefficients. The ORG data, when restricted to even larger cell sizes than those in the CPS, however, reveals significantly positive effects on weekly earnings of those with 12 and those with 13-15 years of education. These effects corroborate the March CPS results for annual earnings to some extent and increase our confidence in those findings.

III. BUSINESS CYCLE SENSITIVITY

The separation of waiver effects from business cycle (i.e., unemployment) effects on caseloads and other outcomes implicitly requires a judgement on what the course of those outcomes would have been in the absence of the decline in the unemployment rate that accompanied the growth of waiver activity. Waiver effects are necessarily estimated from the implicit residual change in the caseload and other outcomes after the effect of the unemployment rate has been netted out. Although there is some variation in unemployment rates in each state after waivers are introduced, the unemployment rate effect for waiver states is largely determined from the data on the cyclical relationship over the years prior to 1992. For the estimates of waiver effects to be correct, therefore, it must be the case that the cyclical relationship between unemployment rates and caseload and other outcomes be the same in 1992 and after as it was before 1992. While there is a sense in which this can never be known with certainty, for we will never know what would have happened over the 1992-1995 period if waiver activity had not increased, we can determine what the historical experience of the cyclical relationship was and can make estimates of waiver effects conditional upon assumptions about whether that historical experience was maintained over the 1992-1995 period.

The regression specifications reported thus far implicitly make such an assumption, by assuming

that the average unemployment rate relationship over the 1977-1991 period, estimated in the same way that waiver effects are estimated--namely, from the relationship between year-to-year changes in caseload and other outcomes and year-to-year changes in the unemployment rate across different states--would have persisted over the 1992-1995 period. One issue is whether the 1977-1991 period is long enough to have estimated that relationship reliably, for there were only two major recessions at the national level during those years. On the other hand, there was considerable cross-state variation in the magnitude of those recessions, and this reduces this problem to some extent. Another issue is whether the cyclical relationships were stable over the 1977-1991 period itself. This issue is more easily addressed with the data at hand, for one can simply estimate different cyclical sensitivities for different periods.

Table 5 shows results of models which test whether cyclical effects have been stable and, if not, whether estimated waiver effects are sensitive to them. The regressions reported in the table are comparable to those in Table 1 and are run at the state level using the administratively-defined AFDC caseload variable used in the CEA report. Thus these regressions return to the CEA data and aggregate caseload model. Column (1) includes only pre-waiver observations and allows the unemployment-rate coefficient to vary with three distinct time periods, based on Figure 1: 1977-1980, before the early 1980s recession and recovery; 1981-1986, which includes one full cycle; and 1987-1992, which covers the slight decline and rise of unemployment in the late 1980s and early 1990s, the second cycle in the pre-waiver data. As the results show, the cyclical sensitivity of the caseload has risen drastically over time. In the late 1970s the caseload was, oddly, procyclical, and during the 1980s and through 1992 it was countercyclical but increasing in sensitivity.[25]

Given this instability, a natural hypothesis is that cyclical sensitivity during the post-1992 period, the period of interest, would have been, in the absence of PRWORA, closest to the cyclical sensitivity exhibited in the pre-1992 period closest to it in calendar time, namely, the period in the late 1980s and early 1990s. The last two columns in Table 5 show the estimates of waiver effects when, respectively, the 1977-1980 observations are deleted and the 1977-1986 observations are deleted, thus gradually deleting more of the more historically-distant observations from 1992. As the table shows, the waiver coefficients lose significance when pre-1981 observations are excluded, and even become significant and positive when the pre-1987 observations are included. This should not be surprising given the results in column (1), for since the more recent periods have greater cyclical sensitivity, excluding the more distant years results in a larger expected post-1992 caseload decline from the unemployment rate alone. Hence the role of waivers in explaining caseload decline is correspondingly reduced.

Table 6 shows the results of estimating the CPS-based models on the 1987+ period only. The results in this case are, interestingly, very similar to those reported earlier, with the notable exception of a lack of significant increase in annual earnings for those with twelve years of education. The

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[25] It is worth emphasizing that these estimates are based upon cross-state variation in unemployment changes. Since unemployment rates change quite differently from state to state, there is considerable variation in the data even in periods when the overall unemployment rate is not changing very much.

disaggregation thus appears to be important here, for the reduced impact of waivers on the caseload reported in Table 5 appear to be masking continued declines among the least-educated women. On the other hand, these results suggest that the positive earnings impacts reported earlier may be sensitive to the implicit cyclical assumption, for Table 6 has the implication that there were, in the population, no significant increases in earnings or wages for any group despite reductions in caseloads and increases in hours worked and weeks worked. This is a somewhat different conclusion than was reached previously.

It is worth emphasizing that it cannot be known with certainty that the most recent recession prior to the waiver activity is the best for forecasting cyclical effects after 1992. Indeed, the true cyclical sensitivity post-1992 could be different than that exhibited in any historical experience in the years of these data. One could argue that the best way to proceed is to average over the cycles that are available in the data, on the assumption that the post-1992 period should be expected, on average, to be like all those in the past, not the most recent one. Some other source of information needs to be brought to bear on this issue to make further progress.

IV. CONCLUSIONS

A recent report of the Council of Economic Advisors (CEA) examined the effect of pre-PRWORA waiver activity in the early 1990s on the AFDC caseload, and found that waivers made a substantial contribution to the reduction in the AFDC caseload although less than that of the declining unemployment rate. The CEA study used an aggregate state-level caseload model estimated over the period 1976-1996. This paper uses the CEA methodology but applies it to microdata from the Current Population Survey (CPS), where information is available on labor force activity, earnings, and income, as well as on demographic characteristics such as age, sex, and education. The results from the CPS data show that less educated women had gains in labor force attachment in the form of increased weeks worked and hours of work as a result of waivers, but no statistically significant increases in earnings or wages. The only statistically significant earnings or wage increases occurred among better-educated women, generally those with at least twelve years of education. The latter result is, however, somewhat sensitive to which historical recession is used to forecast the effect of the business cycle. The findings of the paper demonstrate that a recognition of the diversity and heterogeneity of the population, and the differences in their response to welfare reform, is critical to understanding the effects of that reform.

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Table 1
Aggregate State-Level Caseload Regressions, 1977-1995

	Log Caseload Per Capita				Log AFDC Participation Rate (CPS)	
	(1)	(2)	(3)	(4)	Unweighted	Weighted
Unemployment rate	0.031 (0.435)	0.010 (0.434)	0.037 (0.435)	0.062 (0.435)	-0.512 (1.127)	-0.866 (1.167)
Lagged unemployment rate	4.334* (0.424)	4.323* (0.424)	4.329* (0.424)	4.316* (0.424)	4.672* (1.098)	4.780* (1.143)
Any waiver	-5.751* (2.600)	-	-2.694 (3.475)	-0.280 (4.185)	-15.047* (6.735)	-17.68* (6.981)
JOBS sanctions	-	-2.043 (5.641)	-5.740 (4.305)	-2.705 (5.249)	-	-
JOBS exemptions	-	5.733 (4.695)	-	-	-	-
Termination time limits	-	-6.790 (7.000)	-	-	-	-
Work requirement time limits	-	-9.211* (5.600)	-	-	-	-
Family cap	-	-10.580* (4.751)	-	-	-	-
Earnings disregard	-	-4.569 (4.318)	-	-	-	-
Lead of any waiver	-	-	-	-3.203 (3.638)	-	-
Lead of JOBS sanction waiver	-	-	-	-4.083 (4.207)	-	-
Log max AFDC benefit for family of 3	14.352* (5.286)	12.831* (5.335)	14.345* (5.283)	14.123* (5.276)	4.148 (13.691)	1.948 (14.230)

Notes: Standard errors in parentheses; * indicates significance at the 10% level; all regressions include state dummies, year dummies, and interactions of state dummies with year trend variable. All

regressions are unweighted by state population. All coefficients and standard errors multiplied by 100.

Table 5

Aggregate State-Level Caseload Regressions:
Sensitivity to Nonconstant Cyclical Effects

	1977-1992 Observations	1981-1995 Observations	1987-1995 Observations
Unemployment rate	-	-0.745*	0.031
		(0.458)	(0.421)
Lagged unemployment rate	-	4.500*	2.220*
		(0.443)	(0.450)
Any waiver	-	-1.653	3.744*
		(2.498)	(1.825)
Unemployment rate 1977-1980	-1.571*	-	-
	(0.591)		
Unemployment rate 1981-1986	2.364*	-	-
	(0.297)		
Unemployment rate 1987-1992	3.885*	-	-
	(0.446)		

Notes: Dependent variable is the log AFDC per capita caseload and specification of regressions is identical to that in Table 1, column 1. Standard errors are shown in parentheses; * indicates significance at the 10 percent level. All coefficients are multiplied by 100. Population weights are not used.

Table 2

CPS AFDC Participation Rate Regressions

	State-Level Aggregates	Demographic Cell Data	
		No Interactions	Interactions
Unemployment rate	-0.002 (0.001)	-0.003* (0.001)	-0.003* (0.001)
Lagged unemployment rate	0.002* (0.001)	0.006* (0.001)	0.006* (0.001)
Any waiver	-0.008* (0.003)	-0.010* (0.004)	-
Education=12	-	-0.058* (0.007)	-0.058* (0.007)
Education 13-15	-	-0.061* (0.007)	-0.061* (0.007)
Education 16+	-	-0.065* (0.007)	-0.065* (0.007)
Age 26-34	-	0.034* (0.002)	0.034* (0.001)
Age 35-44	-	0.001 (0.001)	0.001 (0.001)
Age 45-54	-	-0.024* (0.001)	-0.023* (0.001)
Waiver* (Education<12)	-	-	-0.017* (0.006)
Waiver* (Education=12)	-	-	-0.009 (0.006)
Waiver* (Education 13-15)	-	-	-0.007 (0.006)

Waiver*	-	-	-0.008
(Education 16+)			(0.006)

Notes: Standard errors in parentheses; * denotes significance at the 10 percent level. The dependent variable is the AFDC participation rate. All regressions are estimated on years 1977-1995. The omitted education category is less than 12 years and the omitted age category is 15-25 years old. Other variables included in the column (1) regression include the AFDC maximum benefit, year dummies, state dummies, and state*trend variables. Other variables included in the column (2) and column (3) regressions are all those in column (1) plus interactions between the education dummies and year dummies, and interactions between the education dummies and the current and lagged unemployment rate. All regressions use administrative-CPS weights.

Appendix Table 1

Means of the Outcome Variables in the CPS

Variable	All States	Early Waiver States	Late Waiver States	Never Waiver States
Log Caseload per capita[a]	4.18 (1.61)	4.70 (1.71)	4.11 (1.16)	4.08 (1.86)
CPS AFDC participation rate	0.049 (0.013)	0.053 (0.015)	0.048 (0.010)	0.049 (0.015)
Real Annual Earnings (thousands)	9.907 (1.743)	9.905 (1.082)	10.168 (1.580)	9.727 (2.005)
Annual Weeks Worked	30.8 (2.5)	30.7 (1.9)	31.6 (1.9)	30.3 (2.9)
Annual Hours Worked (thousands)	1.115 (0.091)	1.088 (0.068)	1.143 (0.586)	1.102 (0.110)
Real Family Income (thousands)	30.025 (4.822)	40.671 (4.408)	40.000 (5.778)	39.907 (4.050)
Real Weekly Earnings (CPS-ORG)	201.8 (35.3)	202.8 (24.3)	206.3 (32.9)	198.3 (39.9)

Notes: Standard errors in parentheses. Sample consists of all women 16-54.

Early waiver states are those with waivers in 1992 or 1993; late waiver states are those with waivers in 1994 or 1995; and never waiver states are those which never had a waiver approved by the end of 1995.

[a] This variable is taken from the administrative data rather than the CPS. Included for comparison with CPS participation rate.

Table 3

Effect of Waiver Policies on Female Labor Force and Income

	Annual Weeks Worked		Annual Hours Worked		Annual Earnings		Weekly Earnings		Annual Family Income	
	State Aggregates	Demog Cells	State Aggregates	Demog Cells	State Aggregates	Demog Cells	State Aggregates	Demog Cells	State Aggregates	Demog Cells
Any waiver	0.303	-	15.3	-	274.3*	-	3.4	-	392.7	-
	(0.270)		(11.7)		(161.8)		(2.4)		(474.0)	
Waiver* (Educ<12)	-	1.536*	-	67.6*	-	87.8	-	1.61	-	-239.7
		(0.573)		(25.1)		(318.6)		(4.32)		(909.3)
Waiver* (Educ=12)	-	0.545	-	41.0*	-	560.0*	-	6.27	-	568.7
		(0.573)		(25.1)		(318.6)		(4.32)		(909.3)
Waiver* (Educ1315)	-	0.504	-	11.7	-	441.4	-	4.38	-	869.7
		(0.573)		(25.1)		(318.5)		(4.32)		(909.3)
Waiver* (Educ 16+)	-	0.176	-	0.6	-	154.7	-	1.29	-	-898.4
		(0.308)		(25.1)		(318.7)		(4.32)		(909.3)

Notes: Standard errors in parentheses; * denotes significance at the 10% level. All regressions are unweighted. The other variables included in the "state aggregates" regressions are those in column (1) of Table 1. Other variables in "demog cell" regressions are those in the regressions reported in the last two columns of Table 2. All data are from the March CPS (1977-1995) except for real weekly earnings, which is from the CPS ORG (1979-1995).

Table 4

Effect of Waiver Policies on Participation Rate, Labor Force, and Income: Excluding Small Cells

	Participation Rate	Weeks Worked	Hours Worked	Annual Earnings	Family Income	Weekly Earnings 20+	Weekly Earnings 50+
Waiver* (Educ<12)	-0.015*	1.269*	63.9*	-150.6	-772.3	1.87	-5.91
	(0.006)	(0.602)	(26.2)	(331.9)	(976.3)	(4.52)	(4.93)
Waiver* (Educ=12)	-0.007	0.365	36.6*	505.1*	-727.7	6.31	8.09*
	(0.005)	(0.476)	(20.7)	(262.6)	(772.4)	(4.26)	(3.90)
Waiver* (Educ1315)	-0.009*	0.377	14.5	498.6*	907.8	4.40	6.89*
	(0.005)	(0.476)	(20.7)	(262.6)	(772.5)	(4.26)	(3.90)
Waiver* (Educ 16+)	-0.009	-0.049	-10.9	-87.1	104.1	-0.54	1.08
	(0.006)	(0.532)	(23.1)	(293.1)	(862.3)	(4.30)	(4.24)

Notes: Standard errors in parentheses; * denotes significance at the 10 percent level. All other variables and years in regressions identical to those in Table 3 and last column of Table 2. All regressions impose a minimum of 20 observations per cell except last column, which imposes a minium of 50 observations per cell.

Table 6

Effect of Waiver Policies on Participation Rate, Labor Force, and Income:
1987-1995 Years

	Participation Rate	Weeks Worked	Hours Worked	Annual Earnings	Family Income	Weekly Earnings
Waiver*	-0.017*	1.437*	58.0*	-295.1	-948.7	-1.02
(Educ<12)	(0.008)	(0.632)	(27.7)	(369.9)	(1097.9)	(4.86)
Waiver*	-0.010	0.410	29.1*	153.8	-195.3	3.57
(Educ=12)	(0.008)	(0.632)	(27.7)	(369.9)	(1097.9)	(4.86)
Waiver*	-0.008*	0.441	2.3	60.4	171.2	1.90
(Educ1315)	(0.008)	(0.632)	(27.7)	(369.9)	(1097.9)	(4.86)
Waiver*	-0.009	0.107	- 9.2	-231.1	-1573.8	-0.91
(Educ 16+)	(0.008)	(0.632)	(27.7)	(369.9)	(1098.0)	(4.86)

Notes: Standard errors in parentheses; * denotes significance at the 10 percent level. All other variables and years in regressions identical to those in Table 3 and last column of Table 2. All regressions impose a minimum of 20 observations per cell except last column, which imposes a minium of 50 observations per cell.