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Does Benefit Receipt Affect Future Income? An Econometric Explanation

Dean Hyslop*

ABSTRACT

This paper provides an econometric analysis of the effects of receiving welfare benefits on individuals' future income, using longitudinal administrative data on individual incomes. After controlling for heterogeneous differences in individual incomes, spurious effects of contemporaneous benefit receipt and possible endogeneity with incomes, there is no systematic evidence of a positive or negative effect of benefit receipt on incomes. The results are generally imprecisely estimated and sensitive to the choice of specification. Also, a simple first-order specification with unobserved heterogeneity provides a reasonable characterisation of individual income dynamics, although formal statistical tests tend to reject this specification as being too parsimonious.

* Policy Coordination and Development, The Treasury and
Department of Economics, UCLA
405 Hilgard Ave
Los Angeles, CA 90095
USA
dhyslop@paua.sscnet.ucla.edu

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I: Introduction

Economists have long observed that recipients of welfare benefits in one period are substantially more likely than others to receive benefits in subsequent periods. In their pioneering work on dynamic patterns of welfare participation in the U.S., Bane and Elwood (1983) presented two facts which have been confirmed by many authors since. First, the longer a family is in receipt of welfare benefits the less likely they are to leave welfare. Second, the longer a previous recipient remains off welfare, the less likely they are to return to welfare.

There are two competing interpretations often put forward for the observed persistence in welfare receipt. The first is that there is a "welfare trap", in that the receipt of welfare *per se* acts to change recipients behaviour and hence their propensity to experience benefit spells in the future. There are several explanations for this welfare dependence. Participation in welfare programmes may lead to a reduction in labour supply, via a fall in the distribution of wage offers as a result of a deterioration in human capital and/or adverse signaling to employers. Similarly, the opportunity cost of welfare participation may increase as the time off welfare increases, again through human capital effects. Alternatively, welfare participation may increase the costs of working if participation directly affects family structure decisions such as marriage and childbearing. Finally, a "welfare trap" may be explained by fixed costs of either exiting welfare, due to job search, and/or entering welfare, such as establishing eligibility.

The second explanation for the observed persistence in benefit receipt is that it reflects heterogeneity in the population, with some individuals and their families continuously in need of assistance. Under this theory, the declining exit and reentry rates may simply be due to compositional changes in the respective populations. For example, if the population of recipients consists of two groups, distinguished by their propensity to be eligible for welfare, then the observed likelihood of leaving welfare would decline irrespective of any behavioural change due to welfare participation. This is because, over time, the low propensity group is more likely to leave welfare, so that the remaining population will consist disproportionately of the high propensity group. A similar compositional effect would also explain the observed declining likelihood of return to welfare as time off welfare increases.

Both of these theories can explain persistence in benefit receipt, however the public policy implications of each are very different. If the welfare system acts as a trap which creates a dependence among recipients, then public policy should give emphasis to reducing the duration of benefit receipt by, for instance, increasing the opportunity costs of benefit receipt. On the other hand, if persistence in benefit receipt is primarily due to population heterogeneity, then suitable public policy should emphasise alleviating the effects of low income or improving the skills and opportunities of individuals on welfare, rather than the (dis)incentives associated with welfare benefits. It is therefore important to understand the extent of persistence due to each of these competing theories.

This paper provides an econometric evaluation of these two competing hypotheses of welfare persistence using longitudinal data on benefit receipt and market outcomes of individuals over a four year period. Rather than focusing on benefit participation as the dependent outcome of interest, we instead examine whether current receipt of welfare benefits has an effect on individuals' future market outcomes, such as income or employment propensities. In particular, we focus on the question of whether benefit receipt has an effect on future outcomes of individuals after controlling for confounding factors which might reasonably be expected to affect those outcomes. Out primary focus in the empirical analysis is on whether receiving a benefit influences the future income of individuals after controlling for other factors which affect individuals' incomes and may be correlated with their benefit receipt. In addition, and in order to test the robustness of the results, we examine alternative empirical specifications of market outcomes and also welfare receipt.

The paper is organised as follows. The next section describes the framework used to analyse the effects of benefit receipt on individuals income, or some other market outcome. Section 3 discusses the data used in the analysis. We present the results in section 4, and section 5 concludes.

II: Analytical Framework

There are two factors which complicate the identification of the effects of benefit receipt on market outcomes. The first is that individuals and families earnings and labour market status are correlated over time, independent of welfare benefits. The second is that eligibility for welfare benefits depends directly on either individual or family income and/or their labour market status. A consequence of these factors is that benefit recipiency can be expected to be serial correlated, irrespective of any behavioural effect associated with program participation. In addition to these factors which affect eligibility, individuals may also respond to incentives provided by welfare benefits to become eligible, when they wouldn't otherwise be.[1] This implies that to credibly identify the structural effects of benefit receipt on market outcomes of interest requires that a suitable specification for the outcome in the absence of any benefit program be adopted.

Several different approaches to assess the contribution of state dependence to the persistence in welfare participation have been adopted in the literature. Two approaches that have been used are, first, direct econometric modeling of the welfare participation process to identify the effect of state dependence in participation (e.g., Chay and Hyslop, 1998; Chay, Hoynes and Hyslop, 1999); and, second, using social experimental data to measure the impact of state dependence on welfare participation by comparing the participation patterns of "treatment" and "control" groups (e.g. Plant, 1984).

The framework developed here is a special case of a more general framework used to analyse the effects of training programs on earnings (e.g., see Ashenfelter, 1978). It is also similar to the empirical approach used by Beaudry and DiNardo (1991) to examine whether past labour market conditions have an effect on individuals' current wages. In the following discussion, we assume that the outcome of interest is individual's log(income), and the measure of benefit receipt is a dummy variable for whether the individual received any benefit income; however, we will also consider alternative measures in the empirical analysis. We adopt a parsimonious dynamic specification for the outcome of interest: in the absence of benefit receipt, the outcome variable of interest (e.g. income) is assumed to follow a first-order dynamic process. In particular, we assume the following specification:

$$y_{it} = X_{it}'\beta + \gamma y_{it-1} + \varepsilon_{it} \tag{1}$$

where y_{it} is individual i's log(income) in period t, X_{it} is a vector of variables which affect income, and ε_{it} is a residual term. The dataset which is used for the empirical analysis includes very little of demographic information, and contains only information on the individual's sex and age. For this reason the models allow for unrestricted age effects and are estimated separately for males and females. We return to this and related issues later. Although more general income dynamics could be modeled by allowing additional lags of income, such generalisations quickly impose strong demands on the number of time periods of data required to estimate the model. For this reason, we concentrate on the simplest dynamic specification, although this can and will be tested empirically.

We begin our analysis of whether benefit receipt has a (persistent) effect on income, by allowing for lagged benefit receipt to affect current income:

$$y_{it} = X_{it}'\beta + \gamma y_{it-1} + \delta B_{it-1} + \varepsilon_{it} \tag{2}$$

where B_{it-1} is a dummy variable for whether individual i received any benefit income in period (t-1). An obvious method to test the hypothesis that benefit receipt has no persistent effect on income in this framework is to test whether $\delta=0$ while, under the alternative hypothesis that there is welfare dependence, we would expect $\delta<0$.[2] The basic idea behind this is to test whether lagged benefit receipt has an effect on current income after controlling for lagged income.

There are several potentially important misspecifications associated with equation (2). First, equation (2) assumes that current benefit receipt does not affect income, which is obviously a strong restriction. If benefit receipt and income are negatively correlated contemporaneously, and benefit receipt is positively correlated over time, the estimate of δ will be biased due to omitted variable bias. Therefore, a more suitable specification would also control for current benefit receipt:

$$(3) \qquad y_{it} = X_{it}'\beta + \gamma y_{it-1} + \delta_0 B_{it} + \delta B_{it-1} + \varepsilon_{it}$$

Again, the hypothesis that there are no persistent benefit effects on income implies that $\delta=0$.

The second problem associated with equation (2) (and (3)) is that the effect of lagged benefit receipt on current income may capture transitory effects of benefit receipt on income. For example, assuming B and y are negatively correlated contemporaneously then, after controlling for B_{it} and y_{it-1}, we might expect to find a spurious positive correlation between y_{it} and B_{it-1}. In order to control for such effects, we model the dynamic specification of the model as being in terms of "benefit-adjusted" income:

$$(4) \qquad y_{it} - \delta_0 B_{it} = X_{it}'\beta + \gamma(y_{it-1} - \delta_0 B_{it-1}) + \delta B_{it-1} + \varepsilon_{it}$$

or

$$(4') \qquad y_{it} = X_{it}'\beta + \gamma y_{it-1} + \delta_0 B_{it} + \delta_1 B_{it-1} + \varepsilon_{it}$$

where $\delta_1 = \delta - \gamma\delta_0$. One interpretation for equation (4) is that the appropriate measure of income is that which has been adjusted for the contemporaneous effects of benefits – i.e., (4) is equivalent to equation (2) with the income measure redefined as $(y - \delta_0 B)$. In this specification, the hypothesis that there are no persistent benefit effects on income implies that $\delta=0$ in (4), or $\delta_1=-\gamma\delta_0$ in (4′).[3]

Econometric Estimation Issues

First, if ε_i is a random residual term which is uncorrelated over time, then equation (2) can be estimated consistently in levels using ordinary least squares (OLS) estimation. For this specification, at least two periods of data are required. However, the assumption that ε_{it} is purely random is strong and likely to be unrealistic in this context. In particular, given that the only socio-demographic factors observed in the data is the person's age and sex, unobservable factors are likely to be important in explaining individual incomes. Such unobservable factors may include a person's ethnicity, level of education, qualifications and skills, and also regional location.[4] Also, such unobservable factors can be expected to be correlated with benefit receipt. Assuming that the effects of these unobservable factors are approximately constant over time, then a reasonable specification to adopt for the residual is $\varepsilon_{it} = \alpha_i + u_{it}$, where α_i is a person-specific term that is constant over time and u_{it} is a purely random error component. In this case, we can allow for arbitrary individual-effects in equation (2) by adopting a "fixed-effects" specification for α_i, and α_i can then be eliminated by first-differencing the data:

$$(5) \qquad \Delta y_{it} = \Delta X_{it}'\beta + \gamma \Delta y_{it-1} + \delta \Delta B_{it-1} + \Delta u_{it}$$

In equation (5) Δy_{it-1} will be negatively correlated with Δu_{it} (since y_{it-1} is positively correlated with u_{it-}

$_{1}$), so that Δy_{it-1} needs to be instrumented. If the first-order dynamic specification is valid then y_{it-2} will be a suitable instrument for Δy_{it-1} – i.e., y_{it-2} will be uncorrelated with Δu_{it-1}, and is correlated with Δy_{it-1}. For this specification at least three periods of data are required.

A similar approach can be adopted to estimate specifications (3) and (4). In addition to the need to instrument Δy_{it-1}, if benefit receipt is (contemporaneously) correlated with the transitory errors, u, then ΔB_{it} and ΔB_{it-1} will also be correlated with Δu_{it}, and instruments for these variables will be required: suitable instruments may be B_{it-2} and B_{it-3}. Given this choice of instruments, at least four periods of data are required to estimate these specifications.

Finally, with four periods of data it is possible to provide a partial test of the validity of the specifications by including y_{it-3} as an additional instrument, and testing the overidentification restriction implied in this specification. Rejection of the overidentifying restriction will imply rejection of the adopted specification, which may occur as a result of either misspecified income dynamics or the relationship between benefits and income.

III: Data

The data we use is from an Inland Revenue Department (IRD) administrative database of individual income tax returns over the four year period corresponding to the 1994 – 1997 tax years. We have constructed a two-percent longitudinal random sample of individual income tax filers over this period, using information from two possible sources of individuals' incomes.[5] The first source of information is the individual's filed tax return (IR3 or IR5). This source provides information on their income from National Superannuation, other combined PAYE earnings (wages and salaries, and welfare benefits), and other income types (interest, dividends, self-employment income, etc.); tax rebates; family support tax credits; and assessed tax. Although the filed returns provide quite a detailed breakdown on income components, they do not enable wage and salary income to be distinguished from (non-Superannuation) welfare benefit income. In addition, low income individuals with only PAYE income are not required to file tax returns, so there is likely to be significant non-random selection of higher income earners in the sample of IR returns.[6] However, in principle, the IR returns provide consistent income information on individuals who either have high wage and salary income, or receive income that is not subject to PAYE withholding tax, such as self-employment income.

The second source of information comes from Tax Deduction Certificates (TDC), which are filed by payers who withhold PAYE tax from individuals' earnings. These payers are primarily employers and the Department of Social Welfare.[7] From the TDC source it is possible to separately identify wage and salary earnings; National Superannuation; other welfare benefits; earnings related ACC payments; family support paid during the year by NZISS; and tax deductions. There are two advantages of having the TDC information: first, it enables wage and salary income to be distinguished from welfare benefit income; and, second, it provides information on the wage and salary, and benefit income of individuals who do not file IR returns.[8]

Thus, the database includes relatively extensive information on individuals' income receipt and their spells of benefit receipt in each year, although it does not provide detailed information about the timing of various benefit and other income spells <u>during</u> the year. It is partly for this reason that we adopt a panel data modeling approach, which focusses on benefit receipt and income on a period-by-period basis, rather than a duration approach, which analyses <u>how long</u> individuals receive benefits, to the issues.[9]

The demographic information on individuals in the IRD database is limited to their age and sex. For the analysis here we exclude individuals in or near retirement age, and restrict the sample to individuals aged 20-55 (as at March 1996). In addition, as much of the analysis requires information on individuals in each of the four years, we focus on the (balanced) sample of individuals who have non-negative market income and positive gross income in each year. Table 1 contains a description of the sample characteristics, and also compares the selected sample with the sample of 20-55 year old individuals who do not satisfy the selection criteria. The selected sample makes up about three-quarters of the overall sample of 20-55 year olds. Individuals in this sample are on average slightly older (about 1.5 years), have considerably higher market and benefit income, and have a higher fraction of gross income from benefits than those who do not satisfy the selection criteria. Thus, although the selection criteria excludes a significant fraction of the sample, the selected sample is arguably more representative of the population which has contact with the benefit system.

Table 1: Sample Characteristics

	Males		**Females**	
	Balanced Sample	**Non-selected Sample**	**Balanced Sample**	**Non-selected Sample**
Age	36.08	34.54	35.88	34.27
	(.08)	(.13)	(.08)	(.12)
Sample average:				
Market Income	33.87	15.14	19.53	8.61
	(.33)	(.36)	(.16)	(.18)
Benefit Income	1.38	0.96	2.60	1.28
	(.02)	(.03)	(.04)	(.04)
Gross Income	35.25	16.10	22.13	9.89
	(.32)	(.36)	(.14)	(.18)
Disposable Income	26.71	13.37	18.20	8.32
	(.25)	(.80)	(.12)	(.19)
Benefit/Gross Income	0.14	0.08	0.22	0.10
	(.002)	(.003)	(.003)	(.003)
Number of Years with:				
Benefit Income	0.95	0.48	1.27	0.55
	(.01)	(.01)	(.01)	(.01)
Market Income	3.69	1.60	3.49	1.55
	(.01)	(.02)	(.01)	(.02)
Number of Observations	15,310	5,095	14,324	5,830

Notes: Estimated standard errors in parentheses. Incomes have been adjusted using the CPI and are expressed in constant (1998 $1,000) values. The sample is restricted to individuals aged 20 – 55 (in 1996). Other sample selection criteria require that individuals not have negative market income in any year, and have positive gross (market plus benefit) income in each year.

As there is no theoretically implied definition for either y_{it} or B_{it} in the framework above, the empirical analysis will examine the robustness of the results to alternative definitions. Suitable measures of y_{it} may be either market or gross income. Gross or disposable income would be more suitable if log(income) were to be adopted, due to the prevalence of zero market incomes. An alternative definition for y_{it} could be as a binary variable for whether or not individual i received any market income in year t. In this case the focus would be on whether benefit receipt affects the (labour) market participation behaviour of individuals. The simplest definition for B_{it} is a dummy variable for whether individual i received any welfare benefit in year t. However, alternative definitions could also include some measure of the *intensity* of benefit receipt. The analysis will focus primarily on the dummy variable definition of B, but will also consider the fraction of total income received in the form of welfare benefits as an alternative measure.

IV: Empirical Results

Before discussing the results from the econometric analysis, we first present a simple description of the differences in various outcomes. Tables 2 and 3 presents means of annual market and gross income, and the fraction of gross income from benefits, for each of the four sample years, for males and females respectively. Each sample is stratified by individuals benefit receipt status in the final 2 years (1996 and 1997): the first column pertains to individuals who received no benefit income in either 1996 or 1997, which accounts for about three-quarters of the overall sample of males, and two-thirds of the sample of females; the second column pertains to individuals who received benefit income in 1997 but not in 1996 (about 5 percent of the samples); the third column pertains to those who received benefit income in 1996 but not in 1997 (about 5 percent of the samples); and column four pertains to those who received benefit income in both 1996 and 1997 (one-sixth of males, and one-quarter of females).

The time series patterns in income across the subsamples reveal several differences of note. First, there is substantial heterogeneity in incomes across the groups. For example, the incomes of individuals who do not receive any benefit in either 1996 or 1997 (the (0,0) group), is substantially higher in all years than that of other individuals. Column 1 of table 2 shows that, for males, annual income is about $40,000 during the period for this group, compared to about $6,000 (market) or $12,000 (gross) for those who receive benefits in 1996 and 1997 (column 4). Also a slight increase in incomes over time is apparent in column 1, particularly between 1994 and 1995. Second, there is relatively little year-to-year variation in average incomes within these groups, except for when there is a change in benefit receipt status. For example, in column 2 there is a substantial drop in market and gross incomes in 1997, while in column 3 there is a (roughly) equal and opposite increase in average incomes in 1997, corresponding to changes in receipt of benefit income. Similar patterns are apparent in table 3 for females, although the average market incomes are markedly lower for women than men.

The information in tables 2 and 3 allow several simple estimates of the (lagged) effect of benefit receipt on current income which, to some extent, parallel the econometric estimates to follow. Table 4 contains several different estimates for males and females. The most naive estimate, presented in rows 1, is obtained by comparing the average 1997 incomes of individuals who did and did not receive a benefit in 1996. Using market incomes (column 1), this estimate is -$32,829 (with a standard error of $481) for males, and -$20,986 ($251) for females, and represents 97 and 109 percent of sample average market incomes respectively. The estimates using gross income, although smaller, remain substantial (-$27,600 for males, and -$13,200 for females). However, these estimates take no account of heterogeneity in individual incomes, or differences due to current benefit receipt of individuals, that are apparent in tables 2 and 3.

Table 2: Description of Income Patterns – Males

	Benefit Receipt Sequence (1996,1997)			
	(0,0)	**(0,1)**	**(1,0)**	**(1,1)**
Age	37.20	32.62	30.12	33.54
	(.09)	(.44)	(.33)	(.19)
Market Income in				
1994	38.78	18.66	12.15	6.51
	(.41)	(.64)	(.57)	(.21)
1995	42.15	22.28	12.22	6.54
	(.43)	(.62)	(.51)	(.20)
1996	43.47	23.58	16.11	5.60
	(.46)	(.58)	(.38)	(.17)
1997	44.06	15.39	24.20	6.23
	(.47)	(.70)	(.47)	(.18)
Gross Income in				
1994	39.15	20.28	15.47	12.63
	(.40)	(.59)	(.51)	(.17)
1995	42.30	23.12	15.77	12.85
	(.43)	(.59)	(.45)	(.16)
1996	43.47	23.58	18.58	12.34
	(.46)	(.58)	(.36)	(.15)
1997	44.06	17.92	24.20	12.95
	(.47)	(.67)	(.47)	(.15)
Benefit/Gross Income in				
1994	0.04	0.17	0.38	0.64
	(.001)	(.01)	(.02)	(.01)
1995	0.01	0.08	0.37	0.64
	(.001)	(.01)	(.01)	(.01)
1996	0	0	0.20	0.67
			(.01)	(.01)
1997	0	0.26	0	0.66
		(.01)		(.01)
Number of Observations	11,392	556	686	2,676

Notes: Estimated standard errors are in parentheses. In the benefit sequences, a “1” represents receipt of benefit, and a “0” represents no benefit received. Incomes are expressed in terms of 1998 ($1,000), adjusted by the CPI.

Table 3: Description of Income Patterns – Females

	Benefit Receipt Sequence (1996,1997)			
	(0,0)	**(0,1)**	**(1,0)**	**(1,1)**
Age	37.30	32.04	30.50	33.72
	(.10)	(.43)	(.37)	(.16)
Market Income in				
1994	24.38	14.23	8.87	4.10
	(.26)	(.52)	(.37)	(.13)
1995	26.11	16.88	9.03	3.99
	(.21)	(.55)	(.41)	(.12)
1996	27.31	17.92	12.69	3.43
	(.21)	(.51)	(.41)	(.10)
1997	27.99	11.28	19.48	3.93
	(.23)	(.41)	(.47)	(.12)
Gross Income in				
1994	24.82	15.93	13.00	12.22
	(.26)	(.48)	(.37)	(.11)
1995	26.11	17.80	13.50	12.69
	(.21)	(.53)	(.37)	(.11)
1996	27.31	17.92	15.81	12.74
	(.21)	(.51)	(.39)	(.10)
1997	27.99	14.63	19.48	13.26
	(.23)	(.38)	(.47)	(.11)
Benefit/Gross Income in				
1994	0.04	0.17	0.40	0.72
	(.002)	(.01)	(.02)	(.01)
1995	0.02	0.09	0.40	0.74
	(.001)	(.01)	(.02)	(.01)
1996	0	0	0.28	0.78
			(.01)	(.01)
1997	0	0.32	0	0.77
		(.01)		(.01)
Number of Observations	9,445	512	622	3,745

Notes: Estimated standard errors are in parentheses. In the benefit sequences, a "1" represents receipt of benefit, and a "0" represents no benefit received. Incomes are expressed in terms of 1998 ($1,000), adjusted by the CPI.

Table 4: Non-Econometric Estimates of the Effect of Benefit Receipt on Future Income

	Estimate	Market Income	Gross Income
		Males	
1.	$y_{1997}(1.) - y_{1997}(0.)$	-32,829	-27,598
		(481)	(475)
2.	$y_{1997}(10) - y_{1997}(00)$	-19,860	-19,860
		(665)	(665)
	$y_{1997}(11) - y_{1997}(01)$	-9,160	-4,970
		(723)	(687)
3.	$y_{1997}(10) - y_{1996}(01)$	-100	-40
	$- (y_{1996}(10) - y_{1997}(01))$	(1,092)	(1066)
		Females	
1.	$y_{1997}(1.) - y_{1997}(0.)$	-20,986	-13,157
		(251)	(248)
2.	$y_{1997}(10) - y_{1997}(00)$	-8,510	-8,510
		(523)	(523)
	$y_{1997}(11) - y_{1997}(01)$	-7,350	-1,370
		(427)	(396)
3.	$y_{1997}(10) - y_{1996}(01)$	150	380
	$- (y_{1996}(10) - y_{1997}(01))$	(904)	(882)

Notes: Standard errors in parentheses. Estimates are based on average incomes presented in table 2. In the notation used, a "1" in the first position indicates benefit receipt, while a "0" indicates no benefit receipt, in 1996; similarly, the second position "0" or "1" indicates benefit (non-)receipt in 1997. Thus, for example, $y_{1997}(10)$ is the average income in 1997 of individuals who received benefits in 1996 and no benefits in 1997; $y_{1997}(1.)$ is the average income in 1997 of individuals who received benefits in 1996, etc.

The second estimate we consider controls for the effect of current benefit receipt and measures the difference between 1997 incomes of individuals who did and did not receive a benefit in 1996, but have the same 1997 benefit status. This provides two estimates, according to 1997 benefit status. The estimate for males is -$19,860 ($665) conditional on 1997 non-benefit receipt, and -$9,160 ($723) conditional on 1997 benefit receipt. For females, the corresponding estimates using market incomes are -$8,510 ($523) and -$7,350 ($427). Again the estimates using gross income tend to be smaller but still quite large. Comparing these estimates with the naïve estimates in row 1 suggests the importance of current benefit status in estimating the effect of past benefit receipt on current income levels. Also, comparing the two sets of estimates presented in row 2 for either measure of income suggests the importance of heterogeneity in individual incomes across benefit sequences: this is particularly so for males, where the second estimate using market income is less than half the first estimate.

Finally, we consider a third estimate of the effect of lagged benefit status, which controls for heterogeneous incomes across benefit sequences under the assumption that income process is stationary. Under this assumption, if benefit receipt only affects contemporaneous income, we would expect the income gains, when moving off benefits (column 3 in tables 2 and 3) should be the same as the income losses when moving onto benefits (column 2 in tables 2 and 3). In contrast, if benefit receipt adversely affects (future) income, we would expect the income gain for individuals moving off benefits to be lower than the income loss of individuals moving onto benefits. Thus, the third estimator we consider is a "difference-in-differences" estimator of the 1996 and 1997 incomes of individuals who experience a change in benefit-receipt status between 1996 and 1997 (i.e., columns 2 and 3 in tables 2 and 3). In particular, we compare the income growth of individuals who move out of benefit status in 1997 with the income loss of individuals who move into benefit status in 1997: $\delta = (y_{1997}(1,0) - y_{1996}(1,0)) - (y_{1996}(0,1) - y_{1997}(0,1))$, where (e.g.) $y_{1996}(1,0)$ is the average 1996 income of individuals who received benefit in 1996 but not in 1997. For males, the estimated effect of lagged benefit on market income is -$100 ($1,092), and for females $150 ($904). The estimates are small and suggest there is little effect of lagged benefit receipt on individuals' current income. It is also worth noting that the level of precision of these estimates is such that an estimate on the order of $2,000 would be needed before it was considered statistically significantly different from zero. This represents about 6 percent of average income for males and nearly 10 percent for females.

Econometric Analysis

We now turn our attention to the econometric analysis which controls for spurious effects more systematically. Table 5 contains the results of the effects of benefits on log(gross income) for several econometric specifications. In addition to the variables listed in the table, unrestricted age effects are included in all specifications. Column (1) presents Ordinary Least Squares (OLS) estimates for the simplest dynamic specification, equation (2), estimated in levels and ignoring possible unobserved heterogeneity.[10] These results imply that lagged benefit receipt has a strong negative effect on current gross income, reducing male and female gross incomes by 23 and 9 percent respectively. In addition there is a first-order income dynamic effect of about 0.7 for both males and females: assuming this specification is valid, this implies that about 50 percent of contemporaneous income differences dissipate within two years.[11]

Table 5: Econometric Estimates of the Effect of Benefit Receipt on Log(Gross Income)

Coefficient on	(1)	(2)	(3)	(4)	(5)	(6)
			Males			
y_{it-1} (γ)	0.685	0.409	0.388	0.362	0.288	0.390
	(.007)	(.032)	(.033)	(.029)	(.109)	(.078)
B_{it} (δ_0)	---	---	-0.177	-0.182	-1.384	0.128
			(.023)	(.023)	(1.38)	(.860)
B_{it-1} (δ_1)	-0.231	0.093	0.045	0.037	0.468	-0.130
	(.014)	(.022)	(.023)	(.023)	(.549)	(.342)
Estimated $\delta^{(a)}$	-0.231	0.093	-0.024	-0.029	0.070	-0.080
	(.014)	(.022)	(.026)	(.025)	(.301)	(.048)
Over-identification	---	---	---	3.011	---	2.487
				(.08)		(0.11)
			Females			
y_{it-1} (γ)	0.734	0.304	0.302	0.291	0.387	0.399
	(.007)	(.023)	(.022)	(.022)	(.065)	(.069)
B_{it} (δ_0)	---	---	-0.040	-0.041	2.434	2.946
			(.025)	(.025)	(1.74)	(1.76)
B_{it-1} (δ_1)	-0.092	0.034	0.024	0.023	-1.134	-1.332
	(.013)	(.023)	(.024)	(.024)	(.672)	(.680)
Estimated $\delta^{(a)}$	-0.092	0.034	0.012	0.011	-0.193	-0.157
	(.013)	(.023)	(.027)	(.027)	(.179)	(.241)
Over-identification	---	---	---	5.027	---	0.436
				(.03)		(0.51)

Notes: In all specifications, y = log(gross income), and B = 1(benefit income>0), is a dummy variable for whether the individual has any benefit income. All regressions include unrestricted age effects. Estimated standard errors in parentheses, except p-value for the Over-identification statistic.

[a] In columns (1) and (2), δ=coefficient on lagged Benefit receipt; in columns (3) – (5), the implied estimate of $\delta=\delta_1 + \gamma\delta_0$.

Model (1) is equation (2) in text, estimated in levels.
Model (2) is equation (3) in text, estimated in first-differences using y_{it-2} as an instrument for Δy_{it-1}.
Model (3) is equation (4′) in text, estimated in first-differences using y_{it-2} as an instrument for Δy_{it-1}.
Model (4) is equation (4′) in text, estimated in first-differences using y_{it-2} and y_{it-3} as instruments for Δy_{it-1}.
Model (5) is equation (4′) in text, estimated in first-differences using y_{it-2}, B_{it-2} and B_{it-3} as instruments for Δy_{it-1}, ΔB_{it} and ΔB_{it-1}.
Model (6) is equation (4′) in text, estimated in first-differences using y_{it-2}, y_{it-3}, B_{it-2} and B_{it-3} as instruments for Δy_{it-1}, ΔB_{it} and ΔB_{it-1}.

If there is unobserved heterogeneity, the OLS estimate of γ in column (1) will be biased upwards. Also, if the unobserved heterogeneity is negatively correlated with benefit receipt (e.g., individuals with higher income due to unobservable factors will be less likely to receive benefit), the OLS estimate of δ will be biased downwards. To examine the effects of unobserved heterogeneity, arbitrary individual-specific fixed effects are allowed.[12] This specification is estimated in first-differences using y_{it-2} as an instrument for Δy_{it-1}, and the results are presented in column (2) of table 5.[13] There are two changes of note from the results in column (1), which confirm the presence of substantial unobserved heterogeneity in incomes over time. First, the estimated first-order income dynamic effects are substantially smaller: 0.4 for males, and 0.3 for females.[14] These estimates of the dynamics imply that the *regression towards individual means* is relatively quick, while the unobserved heterogeneity accounts for about 45 percent of the total variation in individual incomes. Second, the estimated effect of lagged benefit receipt on current income is now positive for both males and females. The estimate for males implies that lagged benefit receipt increases male income by 9 percent; for females, the estimated effect is 3 percent but not statistically different from zero.

As discussed above, the estimated positive effect of lagged benefit receipt on current income by be the result of omitted variable bias and a spurious correlation. To examine these issues, we next include contemporaneous benefit receipt in the regression, and impose the restrictions implied in equation (4′) in determining the benefit dependence effect of interest (δ). Estimates from these specifications are presented in the remaining columns of table 5. In column (3), we maintain the assumption that ΔB_{it} and ΔB_{it-1} are exogenous with respect to Δu_{it} and instrument Δy_{it-1} using y_{it-2}. In this specification, the estimated first-order dynamics are little changed from column (2). As expected, the unrestricted coefficients on current benefit receipt are negative, although not statistically so for females, while the coefficients on lagged benefit are insignificantly positive for both males and females. The implied estimate of δ in this specification is negative for males and positive for females, although both estimates are not statistically different from zero. It is worth noting here that the coefficient standard errors are little changed from column (2), which implies that collinearity is not an issue in this specification. Next, in column (4), we also include y_{it-3} as an instrument for Δy_{it-1}, and test whether the implied over-identification restrictions are valid. The p-values associated with the over-identification test reject the implied restrictions, particularly for females, and suggests that a first-order dynamic specification is too parsimonious. However, despite this result, the coefficient estimates are almost identical to those in column (3).[15]

We now consider the possibility that transitory income shocks will be correlated with benefit receipt, in which case ΔB_{it} and ΔB_{it-1} will be correlated with Δu_{it}. The final two columns in table 3 contain estimates in this situation. In column (5), y_{it-2}, B_{it-2} and B_{it-3} are used as instruments for Δy_{it-1}, ΔB_{it} and ΔB_{it-1}. Compared to the results in column (3), there is a substantial loss in precision in the estimates in this specification, which implies that B_{it-2} and B_{it-3} are relatively weak instruments. Consequently, although the point the estimates of δ are quite large, they are insignificantly different from zero. In column (6), we present the results when y_{it-3} is added to the instrument set. This tends to improve the precision for males, but not for females. The estimate of δ for males from this specification implies that lagged benefit receipt tends to reduce gross income by 8 percent, and is statistically significant at the 10 percent level. The over-identification test statistics for this specification provide no evidence against the validity of the instrument set, although this may reflect the weakness in the instrument set rather than any power to detect inconsistent instruments. In order to try to improve the quality of the instruments for this specification, we included the two interaction variables $y_{it-2}*B_{it-2}$ and $y_{it-3}*B_{it-3}$ in the set of instruments. The results, presented in column (4) of appendix table A1, indicate that these additions improve the fit quite a bit – e.g., the standard errors of the unrestricted coefficient estimates are approximately halved. For males, there is little effect on estimate of the parameter of interest, δ, or its standard error; for females, the estimate is large and statistically significant, and implies lagged benefit receipt reduces gross income by about 25 percent.

Table 6: Econometric Estimates of the Effect of Benefits on Alternative Market Outcomes

y =	**Log(Gross Income)**		**Market Income**		**1(Market Income>0)**	
B =	**Benefit/Gross Income**		**1(Benefit>0)**		**1(Benefit>0)**	
Coefficient on	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
			Males			
y_{it-1} (γ)	0.371	0.342	0.009	-0.001	0.234	0.243
	(.028)	(.028)	(.016)	(.019)	(.018)	(.108)
B_{it} (δ_0)	-0.029	-0.231	-7.943	-15.552	-0.035	-0.008
	(.008)	(.070)	(.691)	(8.43)	(.006)	(.501)
B_{it-1} (δ_1)	0.125	-0.442	-0.981	3.919	-0.010	-0.022
	(.016)	(.185)	(.681)	(3.22)	(.006)	(.203)
Estimated $\delta^{(a)}$	0.114	-0.521	-1.054	3.931	-0.018	-0.024
	(.018)	(.208)	(.670)	(3.39)	(.007)	(.083)
Over-identification	2.678	1.124	21.426	7.526	3.354	1.118
	(0.10)	(0.34)	(<0.01)	(<0.01)	(0.07)	(0.34)
			Females			
y_{it-1} (γ)	0.332	0.336	0.151	0.119	0.297	0.239
	(.023)	(.028)	(.030)	(.029)	(.020)	(.052)
B_{it} (δ_0)	-0.003	-0.006	-6.248	-16.197	-0.041	-0.679
	(.0002)	(.003)	(.437)	(4.44)	(.009)	(.442)
B_{it-1} (δ_1)	0.002	0.001	0.011	3.131	-0.013	0.245
	(.0004)	(.0004)	(.459)	(1.38)	(.008)	(.163)
Estimated $\delta^{(a)}$	0.001	-0.002	-0.933	1.198	0.026	0.083
	(.0004)	(.001)	(.429)	(1.21)	(.009)	(.092)
Over-identification	5.392	4.178	3.779	3.073	7.106	2.274
	(0.02)	(0.01)	(0.05)	(0.03)	(0.01)	(0.08)

Notes: Estimated standard errors in parentheses, except p-value for the Over-identification statistic. All regressions include unrestricted age effects.

(a) Estimate of $\delta = \delta_1 + \gamma\delta_0$.

Models (1), (3) and (5) is equation (4′) in text, estimated in first-differences using y_{it-2}, and y_{it-3} as instruments for Δy_{it-1}.

Models (2), (4) and (6) is equation (4′) in text, estimated in first-differences using y_{it-2}, y_{it-3}, B_{it-2} and B_{it-3} as instruments for Δy_{it-1}, ΔB_{it}, ΔB_{it-1}, and interactions y_{it-2}*B_{it-2} & y_{it-3}*B_{it-3}.

Alternative Specifications

In order to examine the robustness of the results above, we next discuss results using various alternative specifications for the dependent outcome of interest and/or the benefit variable. Table 6 contains the results for this exercise for specifications corresponding to those in table 5, columns (4) and (6) – i.e. first, Δy_{it-1} is instrumented and, second, Δy_{it-1}, ΔB_{it} and ΔB_{it-1} are instrumented.[16] First, in order to examine whether a binary variable for benefit receipt is a suitable measure, we consider the effect on log(gross income) of benefit income *intensity*, as defined by the fraction of gross income that is from benefits. In column (1), when only Δy_{it-1} is instrumented, the over-identification statistic provides support for the instrument set for males, while it rejects the consistency of the instrument set at the 2 percent level for females. In this specification the effects of lagged benefit receipt on current income are positive and statistically significant for both sexes. However, the magnitudes of the effects are small: a 10 percentage point increase in the fraction of gross income from benefits increases gross income by 1 percent for males, and 0.01 percent for females. Column (2) contains the results when ΔB_{it} and ΔB_{it-1} are also instrumented. In this case, the results for males imply a 10 percentage point increase in the fraction of gross income from benefits reduces income by about 5 percent, while for females a 10 percent increase in the benefit/gross income ratio reduces female gross income by 0.02 percent.

Next, we consider the effect of benefit receipt on market income, measured in levels rather than logs to allow for the presence of zero market incomes. The results for this exercise are presented in columns (3) and (4). For each case, the over-identification statistic strongly rejects the consistency of the instrument sets. The standard errors on the benefit variables when these are instrumented again suggest that the instruments used are weak. For females, the estimates in column (3) imply statistically significant negative benefit dependence effects on market income of $900 when only Δy_{it-1} is instrumented.

The final outcome variable we consider is a binary variable for whether the individual has any market income, which is used as a measure of market participation. For ease of manipulation we use a linear probability specification for this analysis.[17] The results for this specification imply negative benefit dependence for males and positive for females, although not statistically different from zero when the benefit variables are instrumented for. Again the over-identification statistics reject the consistency of the instrument sets – strongly for females, and weakly for males.

V: Concluding Discussion

This paper provides an investigation of the persistence effects of benefit receipt on various measures of future income. The analysis used a parsimonious dynamic econometric specification to control for spurious factors which may bias the dependence effects of interest. As expected, ignoring unobserved heterogeneity and other confounding factors, the results show that benefit receipt has a large and negative effect on individuals' future income. Controlling for unobserved differences between individuals, the results indicate that lagged benefit receipt has a <u>positive</u> effect on income. However, once suitable controls for other confounding effects have been made, there is little robust evidence of either a positive or negative effect of benefit persistence on individuals' income or other outcomes. What is clear from this study is that differences between individuals, as opposed to benefit receipt per se, account for much of the observed correlation between benefit receipt and income. How much of these differences between individuals are due to ability, skills, location, discrimination, or preferences for work is unclear and cannot be revealed with this data.

However, there are possible caveats to the analysis and conclusions. The most important is that, in the preferred specifications which allow benefit receipt to be correlated with time-varying errors, the effects are generally imprecisely estimated. This measure cannot reject a wider range of effects, including the possibility that welfare does act to some extent as a trap. To some extent this reflects an inherent difficulty faced in identifying the effects of interest in the presence of several confounding factors.

In addition to the results concerning the persistent effects of benefit receipt, the analysis also sheds some light on individual income dynamics. First, the results illustrate that it is important to control for unobservable factors in modeling individual income dynamics. Allowing for an individual-specific fixed effect in income, the results show that this explains nearly half of the total variation in individuals incomes, while the estimated dynamics parameter falls rom about 0.7 to between 0.3 and 0.4. Second, although the over-identification tests and more general dynamic specifications provide some evidence against a first-order income dynamics specification, which could bias the results, this specification appears to capture most of the income dynamics. For example, allowing for second-order dynamics results in a very small effect of y_{it-2} on y_{it}, and little change in the estimated effect of y_{it-1}. However, there is no evidence that relaxing this specification has any noticeable effect on the results.

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Table A1: Econometric Estimates of the Effect of Benefits on Log(Gross Income)

Coefficient on	(1)	(2)	(3)	(4)
		Males		
y_{it-1} (γ)	0.561	-0.335	0.414	0.363
	(.009)	(.009)	(.042)	(.042)
y_{it-2}	0.196	---	0.023	---
	(.009)		(.014)	
B_{it} (δ_0)	---	---	-0.172	-0.167
			(.024)	(.336)
B_{it-1} (δ_1)	-0.165	-0.072	0.055	-0.009
	(.014)	(.017)	(.026)	(.131)
Estimated $\delta^{(a)}$	-0.165	-0.072	-0.016	-0.070
	(.014)	(.017)	(.028)	(.048)
Over-identification	---	---	---	1.093
				(0.35)
		Females		
y_{it-1} (γ)	0.632	-0.243	0.322	0.345
	(.009)	(.008)	(.026)	(.039)
y_{it-2}	0.144	---	0.023	---
	(.009)		(.011)	
B_{it} (δ_0)	---	---	-0.038	1.296
			(.026)	(.806)
B_{it-1} (δ_1)	-0.073	-0.003	0.028	-0.701
	(.013)	(.020)	(.024)	(.322)
Estimated $\delta^{(a)}$	-0.073	-0.003	0.016	-0.253
	(.013)	(.020)	(.017)	(.074)
Over-identification	---	---	---	2.130
				(0.09)

Notes: In all specifications, y = log(gross income), and B = 1(benefit income>0), is a dummy variable for whether the individual has any benefit income. All regressions include unrestricted age effects. Estimated standard errors in parentheses, except p-value for the Over-identification statistic.

[a] In columns (1) and (2), δ=coefficient on lagged Benefit receipt; in columns (3) – (5), the implied estimate of $\delta=\delta_1 + \gamma\delta_0$.

Model (1) is estimated in levels by OLS.

Model (2) is estimated in first-differences by OLS.

Model (3) is estimated in first-differences, using y_{it-3} as an instrument for Δy_{it-1}.

Model (4) is estimated in first-differences, using y_{it-2}, y_{it-3}, B_{it-2}, B_{it-3}, and interactions $y_{it-2}*B_{it-2}$ & $y_{it-3}*B_{it-3}$ as instruments for Δy_{it-1}, ΔB_{it} and ΔB_{it-1}.

[1] Ashenfelter (1983) discusses a variety of issues associated with participation in welfare programs.

[2] A possible, although less expected, alternative is that benefit receipt actually has a positive effect on future income – e.g., if benefit receipt enables individuals to maintain or improve their health or human capital capabilities, then their income opportunities may improve as a result.

[3] Although our preferred specification is equation (4), there are alternative restrictions. Perhaps the most obvious alternative is to allow lagged benefits to have a spurious effect on y_{it} which is equal and opposite to the contemporaneous effect:

(3′) $$y_{it} = X_{it}'\beta + \gamma y_{it-1} + \delta_0(B_{it} - B_{it-1}) + \delta B_{it-1} + \alpha_i + u_{it}.$$

[4] It is worth noting that even with quite rich survey data unobservable factors tend to remain important in -- e.g. typical wage or income regressions estimated using US survey data can explain up to 30 percent of the observed variation, while about one-half of the unexplained variation persists over time and so can be attributed to systematic unobserved factors. In addition, age or experience is generally the observable factor which has the highest explanatory power.

[5] A more detailed discussion of the database is provided in Hyslop and Smith (1999).

[6] The low-income limits were \$20,000 in the 1994-96 tax years, and \$34,200 in 1997.

[7] There is no information on the database concerning interest income and withholding tax.

[8] It is not possible to identify the individual for approximately 5 percent of TDCs; however, this figure is lower for employer earnings and DSW benefit TDCs which forms the population of primary focus in this paper. It is possible to examine the accuracy of the IR information provided by individuals by comparing the TDC and IR reported PAYE incomes of individuals for which both is available. Compared to most survey data, we believe the reported income information is quite accurate. For example, we find that the PAYE incomes are (exactly) equal in 80 percent of matches, and lie within 10 percent of each other in about half of the remaining matches.

[9] Even in the presence of “perfect data”, it is not clear that either analytical approach dominates, as the panel modeling approach is more flexible in handling (unobserved) heterogeneity, while the duration analysis allows more flexible forms of state dependence.

[10] This specification is the closest to those estimated by Creedy (1997) using data from the same database for an earlier period and with only individuals IR tax-return information. Although Creedy does not allow for permanent unobserved effects, his specifications do allow for the error term to follow a stationary first-order autoregressive process, which translates into a second-order dynamic income process. For these reasons, our results are not directly comparable to Creedy's, however the results here are broadly similar. Coleman (1996) provides a useful discussion and critique of Creedy's specifications.

[11] Column (1) in appendix table A1 contains the results when the second lag of log(gross income), y_{it-2}, is included in the model. For both males and females, the coefficient on y_{it-1} falls by 15-20 percent, and the coefficient on y_{it-2} is statistically significantly positive, but substantially smaller than the coefficient on y_{it-1}.

[12] Given the absence of any demographic information, other than age and sex, on the IRD database any unobserved heterogeneity will include the effects of commonly observed characteristics such as education and ethnicity, as well as other unobserved factors which have a persistent effect on individuals’ incomes.

[13] If benefit receipt is correlated only with the permanent component of error, then a consistent estimate of δ can be obtained without instrumenting ΔB_{it-1}. We return to this issue later.

[14] Column (2) of appendix table A1 contains the OLS results for this specification. The OLS estimate of the lagged dependent variable is significantly negative due to the strong negative correlation between Δy_{it-1} and Δu_{it-1} as a result of first-differencing.

[15] We have also reestimated the model with second-order dynamics (i.e. including the y_{it-2}), and using just y_{it-3} as an instrument for Δy_{it-1}. The results from this specification, presented in column (3) of appendix table A1, support the view that while there is a statistically significant second-order dynamic effect, it has a marginal impact on the results.

[16] In the latter cases, we also include interactions between y_{it-2} and B_{it-2}, and between y_{it-3} and B_{it-3} as instruments in order to improve the precision of the estimates.

[17] The choice of linear specification for binary outcome should have little effect on estimating mean-effects – see Hyslop (1999) for results in a similar context.