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The Impact of Recent Welfare Reforms on Labor Supply Behavior in New Zealand

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Abstract

New Zealand recently initiated sweeping reforms to its social welfare program by cutting benefits and tightening eligibility criteria. One of the objectives of these reforms was to provide incentives for people to enter or re-enter the labor force. Econometric analysis is used in this paper to isolate the actual effects of these benefit reforms on labor supply. Previous research has often failed to accurately measure the extent of these work disincentives, or to observe variation in these programs that would allow this empirical analysis to take place. The structure of the benefit programs and the nature of the reforms in New Zealand offer a unique opportunity to identify these behavioral responses. Quarterly random samples of individuals over the period 1987–1995 are used to isolate the effects of the reforms. This study finds compelling evidence that the recent benefit reforms in New Zealand increased both labor force participation and hours of labor supplied at the aggregate level.

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I. INTRODUCTION

In December 1990 the New Zealand government announced extensive changes to its social welfare programs. One of the motivations behind these reforms was the belief that lower benefit levels and tighter eligibility criteria would increase the labor supply of existing and potential beneficiaries. This study empirically isolates the general effects of income transfer programs, and the specific effects of these benefit reforms, on aggregate labor supply behavior in this country.[1]

The work disincentive effects of social welfare programs is an issue that continues to confront researchers and policymakers in many countries. Although fairly consistent evidence from the United States has shown that the Unemployment Insurance (UI) and Aid to Families with Dependent Children (AFDC) programs influence such measures as unemployment duration, labor force participation, and hours of work (e.g., Burtless 1990; Moffitt 1992), considerable uncertainty remains over the magnitudes of these effects. Evidence on the impact of benefit reforms in the United Kingdom has been mixed (e.g., Minford 1983; Micklewright 1986). Furthermore, Bean (1994) has found relatively weak evidence of any link between the generosity of benefits and unemployment rates across Europe.

At least two major problems confront this empirical literature. First, most countries have a myriad of social welfare programs with rather complicated and interrelated structures. It is often difficult to condense the features of these programs into a manageable and valid set of regressors that adequately capture such complex work disincentives. Even a single welfare program can create budget constraints with various effective wage rates, notches, and discontinuities that impose a considerable burden on the

[1] New Zealand has undergone numerous economic reforms since 1984. See recent reviews of these policy changes and their possible economic effects in Silverstone, Bollard, and Lattimore (1996) and Evans, Grimes, and Wilkinson (1996).

analyst who wants to accurately represent the resulting labor supply choices facing an individual or family (e.g., Fraker and Moffitt 1988).

Second, regression analysis requires exogenous variation in the explanatory variables in order to isolate the work disincentives associated with these programs. This variation has three possible sources. The first involves the rare opportunity to observe outcomes from a controlled experiment, where program parameters can be exogenously altered among groups of randomly assigned individuals.[2] The second focuses on the aggregate variation in the generosity or structure of these programs over time. This has been used in assessing the effects of welfare reforms in the United Kingdom. Some of the problems with this research have been small sample sizes with aggregate data, the difficulty of boiling complex reforms down to a few proxy variables (i.e, measurement error), and the concern that these regressors may be capturing other aspects of general economic reforms or macroeconomic conditions (i.e., omitted-variable bias). The third source relies on cross-sectional variation in the independent variables. Benefits and other factors vary across individuals and households because of age, sex, marital status, children in the household, and residence in a particular state or region. Moffitt (1989) cautions that any reliance on these cross-sectional data may be inappropriate because of a lack of true exogenous variation in these program parameters. For example, the causality in the regression could be reversed. Benefit levels could be set by policymakers on the basis of a demographic group's past labor market performance. Instead of picking up a behavioral response, these regressions may be capturing the decision-making process in establishing these benefits.

This study offers a unique opportunity to mitigate the two general methodological problems outlined above by using data from New Zealand both before and after our recent welfare reforms to estimate the possible work disincentives. First, the nature of the social welfare system makes it relatively

[2]For example, see Killingsworth's discussion (1983: 392–408) of the labor supply findings from negative income tax experiments.

easy to identify the key components of the welfare programs that would affect labor supply behavior. As we will see, personal and family characteristics map directly into the matrix of both programs and benefits that face individuals at a point in time. Benefit amounts, for example, do not depend on factors such as pre-unemployment earnings as they do under the UI system in the U.S. Many of the structural changes in the benefit system that took place over our sample period targeted specific demographic groups. Thus, we can measure the effects of some key qualitative changes in these programs as they were phased in for specific subpopulations.

Second, this study merges both time-series and cross-sectional variation in the relevant data. The reductions in nominal benefits were not uniform across demographic groups or programs. For example, the maximum unemployment benefit available to single adults between the ages of 20 and 24 without dependent children fell by 25 percent in April 1991. The benefit available to married couples without children declined by 3 percent. Other benefits were not reduced during the same period. It is this variation in both magnitude and timing of the benefit changes that provides us with something closer to the exogenous variation that we want in isolating the associated behavioral responses.

Section II outlines the structure of New Zealand’s main social welfare programs and the nature of the reforms that have occurred over the last decade. Section III develops a theoretical and empirical framework for this analysis. Section IV briefly examines the data used in this study and presents some descriptive statistics. Section V describes and analyzes the regression results. Finally, Section VI draws some conclusions from this study.

II. RECENT REFORMS TO NEW ZEALAND'S SOCIAL WELFARE SYSTEM

This study concentrates on the three main social welfare programs in New Zealand for the able-bodied or nondisabled population. All three are essentially negative income tax programs that target specific subpopulations. All benefits are subject to income tax.

The Domestic Purposes Benefit (DPB) provides income support to single-parent families and to women aged 50 and over who live alone. In terms of categorical eligibility and structure, it is similar to the AFDC program in the U.S. The basic benefit guaranteed to a single parent depends on the number of children in family. Once earned income exceeds some threshold, this nominal benefit abates at reduction rates that rise from 30 percent to 70 percent.

Superannuation benefits are available to people above "retirement age," which changes over our survey period. The benefit amount depends on marital status and has traditionally been set at around two-thirds of the level of average earnings in the country. Superannuation also has income exemptions and abatement rates (known as Tax Surcharges) that have both changed repeatedly over our sample period.

By default, nearly everyone else is potentially eligible for income support through the Unemployment Benefit (UB) program. Basic benefits under the UB program vary by age, marital status, and number and ages of children in the family. Again, certain income exemptions and abatement rates apply. Unlike the U.S., there is no time limit on the receipt of unemployment benefits, and workers are not categorically ineligible for these benefits if they work part-time.

In addition to these three main programs, the Family Support and Family Benefit programs supplement these basic benefits for families with children.[3] These payments are included in all potential benefits in this study. The only major welfare program in New Zealand for the able-bodied population

[3]The Family Benefit was abolished in April 1991, and these benefits were essentially incorporated into Family Support payments at that time.

ignored in this study is public housing and Accommodation Supplements. During our sample period, New Zealand moved away from the traditional public housing scheme, in which individuals queue for the available stock of public housing and pay below-market rents once these accommodations become available. Individuals now receive an Accommodation Supplement which varies by, among other things, housing costs in their region. Since this policy shift went into effect, public housing has been sold and the rents on remaining public housing have increased to market levels. It would be inappropriate to include the cash payments under the Accommodation Supplement in our measures of basic benefits unless we did the same for the monetary equivalent of the earlier in-kind benefits. Since we have no information about who resided in public housing before this policy change, or the value of these in-kind transfers, it was decided that this benefit would be treated “symmetrically” over the period of the study and simply ignored in all calculations.

As mentioned earlier, the government announced in December 1990 a series of changes to New Zealand’s social welfare programs. On average, nominal benefits were reduced by approximately 10 percent in April 1991. The benefit cuts, however, were not uniform across demographic groups or programs. Moreover, welfare reforms were not restricted to either this period or to mere benefit changes. The unemployment benefit for those aged 16 or 17 was reduced by 26 percent in April 1989. The “stand-down” period was increased from 6 weeks to 26 weeks in March 1991. This meant that individuals could be denied unemployment benefits for this period if they left employment voluntarily or were dismissed for misconduct. Other work-test provisions were tightened at this time. The minimum age of eligibility for both UB and DPB benefits was raised in 1991 from 16 to 18.

Perhaps the most important qualitative change in these programs occurred with Superannuation. In the 1970s, the minimum age of eligibility for basic retirement benefits, funded out of general revenue, was lowered to 60. By the late 1980s, it was clear that this scheme was becoming fiscally unsustainable. In April 1992, the minimum age of eligibility for Superannuation was raised from 60 to 61. Since that time, it has increased in scheduled increments of 3 months in age for every 6 months in time. By the end of 1995 the minimum age of eligibility for Superannuation had risen to 62.75 years. These scheduled increases will stop when the minimum age of eligibility reaches 65 in 2001.

III. A THEORETICAL AND ECONOMETRIC FRAMEWORK

We begin with a simple algebraic representation of the labor supply behavior of a representative individual. Let L_{it} be the proportion of available time that individual i devotes to market work in period t (i.e., $0 \leq L_{it} \leq 1$). An exogenous, real after-tax wage W_{it} is received for each hour worked. A real after-tax benefit B_{it} is received for each hour of nonwork ($1-L_{it}$). The person optimally allocates his or her time across these two labor force states by maximizing the following utility function:

$$U = \alpha Y_{it} - \frac{\gamma}{2} L_{it}^2 + \delta_{it} L_{it} \tag{1}$$

subject to the following income constraint:

$$Y_{it} = W_{it} L_{it} + B_{it}(1 - L_{it}) \tag{2}$$

where Y_{it} is total disposable income. Utility is written as a quadratic function in L_{it} to allow for an increasing marginal disutility of work.

The first-order condition for utility maximization is the following:

$$L_{it} = \frac{\alpha}{\gamma}(W_{it} - B_{it}) + \frac{\delta_{it}}{\gamma}. \tag{3}$$

If α and γ are positive as expected, then labor supply depends positively on the wage and negatively on the benefit. The variable δ_{it} captures all other personal characteristics and time-specific factors that influence the individual's labor supply behavior.

We can transform expression (3) into a regression equation with a few modifications. First, this simple theoretical model assumes that a dollar in net earnings and a dollar in benefits have exactly opposite impacts on labor supply. Because of the potential stigma effects of being a beneficiary (e.g., Moffitt 1983) and other theoretical considerations (e.g., Hillier 1985), we allow for differences in their respective effects on this behavior and test for the equality between these coefficients.

Second, we need to make δ_{it} operational. Some differences in labor supply may be related to observable factors like gender, ethnicity, age, marital status, and the presence of children in the family. These variables are included in the vector X_i below. Some differences may be related to measurable characteristics of the relevant social welfare program (other than the available benefit) that vary across both individuals and time (due to welfare reforms). These covariates are included in the vector Q_{it}. Other differences may be related to factors that cannot be easily quantified. For this reason, we include a dummy variable η_t for each time period to control for factors that are time-specific but invariant across individuals (e.g., general economic conditions and other reforms), and add a disturbance term u_{it}.

$$L_{it} = \beta_0 + \beta_1 W_{it} + \beta_2 B_{it} + \beta_3 X_i + \beta_4 Q_{it} + \eta_t + u_{it} \qquad \textbf{(4)}$$

More discussion is needed on both the specification of this regression function and the appropriate estimation technique. Some of this will be delayed until Section IV, where we present the data that will be used for this purpose. For now, we consider the general nature of the disturbance term and the measurement of the relevant "wage" and "benefit" variables. This error term can be decomposed into something that varies solely across individuals (v_i) and something that varies across both individuals and time (ε_{it}). The latter component is assumed to be independently and identically distributed across both dimensions.

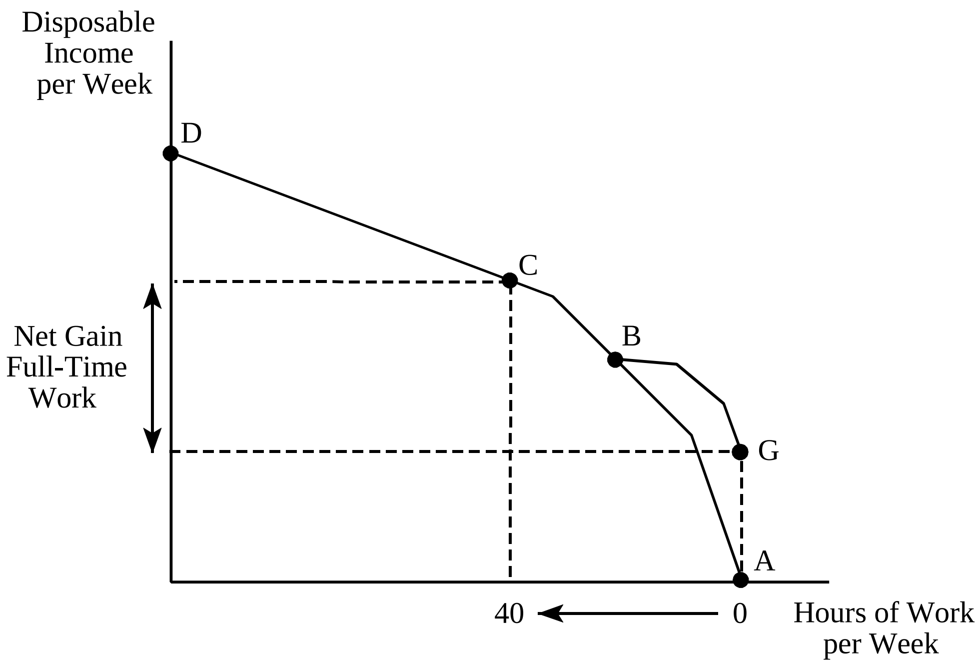
$$u_{it} = v_i + \varepsilon_{it} \tag{5}$$

The above theoretical model oversimplifies the nature of the tax and transfer system and its influence on labor supply behavior. Individuals are not paid for every hour they do not work. Instead, they are guaranteed some level of income which abates as they work more hours until they reach some break-even point where they are no longer eligible for benefits. The "effective" wage rate depends not only on this abatement regime but also on the various marginal tax rates under the income tax system.

A more accurate representation of this tax-transfer system is depicted in Figure 1. The vertical axis measures total weekly after-tax income. The horizontal axis measures hours of work per week from right to left. From a labor supply perspective, the relevant budget constraint under this negative income tax program is GBCD, which contains a number of kinks and effective wage rates. Moreover, the position of these kinks and the slopes of the intervening line segments can vary across individuals, programs, and time. Even more troubling, tax and various transfers programs can interact with each other to produce an even more complex incentive structure with notches and discontinuities.

One possibility would be to choose several discrete points along the budget constraint and include these potential income amounts in the labor supply regression. The problem with such an approach is the extensive multicollinearity among the resulting regressors. The simplification adopted in this study, consistent with the simple labor supply model above, is to construct a "replacement rate" for full-time work (40 hours of work per week) that incorporates all aspects of the relevant budget constraint for a given individual in a given time period. In this case, the replacement rate would be the after-tax income at G divided by the after-tax income at C. This is the net gain in income from full-time work.

FIGURE 1


Disposable
Income
per Week
D
C
B
G
A
Net Gain
Full-Time
Work
40
0
Hours of Work
per Week

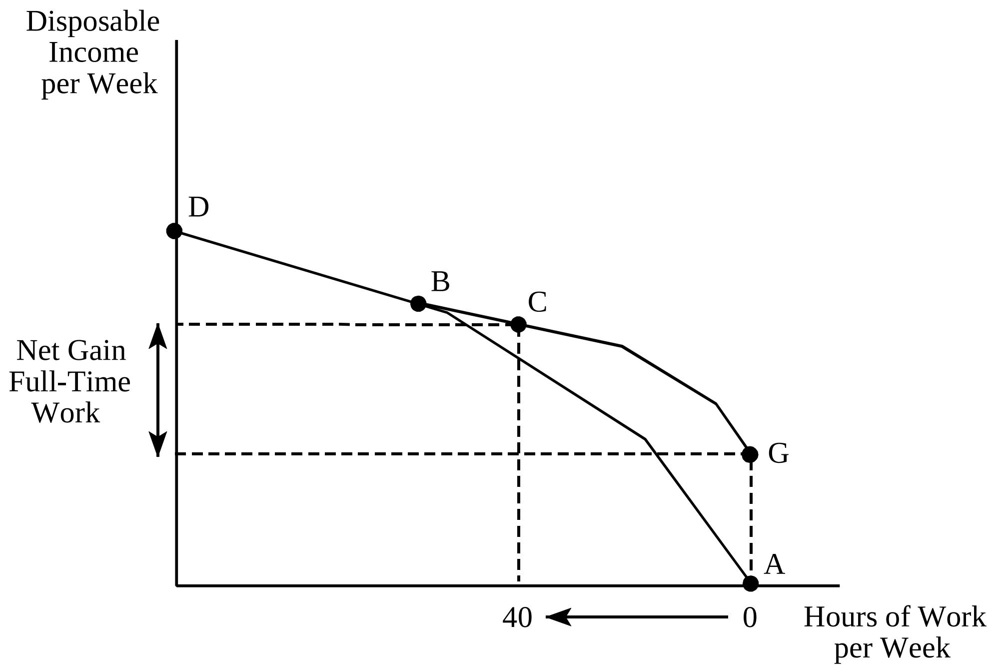
The principal problem with this approach is one of “errors-in-variables.” In this example, the full-time replacement rate would not incorporate the income exemptions and benefit reduction rates. This is because this individual’s wage rate is sufficiently high (and benefits sufficiently low) so that her full-time earnings exceed the break-even point. But this potential problem is mitigated by extensive cross-sectional and time-series variation in replacement rates in this study. In other situations, wage rates are sufficiently low (or benefits sufficiently high) so that full-time earnings would not exceed the break-even point. Individuals would be eligible for reduced benefits at 40 hours of work. Such a situation is depicted in Figure 2.

In Figure 2, the relevant replacement rate for the individual is still the ratio of G to C, but after-tax income at C includes both net earnings and partial benefits. The measured full-time replacement rates used in this study capture the variation in gross wages, income taxes, income guarantees, income exemptions, and benefit reduction rates across both individuals and time. However, since the specification of the earlier labor supply regression showed the numerator and denominator of this replacement rate entering as separate regressors, we will follow this convention in our empirical analysis. The relevant wage rate W_{it} is the potential after-tax income from full-time work. The relevant benefit B_{it} is the potential after-tax income from no work.

IV. DATA AND DESCRIPTIVE STATISTICS

The principal data source used in this study is the Household Labour Force Survey (HLFS). The HLFS is a random sample of households drawn each quarter from the population and retained in a “rotation group” for eight consecutive quarters. This survey began in December 1985, and the last survey available for this analysis is December 1995. Sample size varies from 16,000 to 32,000 households over the life of the HLFS. The chief advantages of HLFS data are the frequency of the survey, the large

FIGURE 2


Disposable
Income
per Week
D
B
C
Net Gain
Full-Time
Work
G
A
40
0
Hours of Work
per Week

sample sizes, and the consistency of labor force definitions over time and relative to the standards laid down by the International Labour Office.

Unit record data from the HLFS is unavailable due to confidentiality concerns. However, specific aggregated data were obtained for the purposes of this study. In each quarter, nondisabled individuals were grouped according to various personal characteristics and family circumstances.[4] Attributes were chosen partly to match the criteria used by the Department of Social Welfare to determine the income transfer programs for which individuals are potentially eligible. Unlike other countries where potential benefits might be determined in other ways (e.g., as a proportion of pre-unemployment earnings), eligibility for a specific benefit in New Zealand depends entirely on a person's sex, age, marital status, number and ages of dependent children, and the time period in which they are surveyed. A simple algorithm was constructed for mapping these personal characteristics into the existing social welfare programs and benefits amounts.

The major drawback of the HLFS is the lack of any earnings or income-related data. Nonlabor income of the family and the earnings of the spouse would reduce the potential benefit for which an individual would be entitled. In the absence of this family-specific income information, we simply assign the *maximum potential* benefit to the individual. In other words, it is assumed that if the individual does not work, the only income available is through the social welfare programs for which he or she is eligible. This is one source of potential measurement error in our computation of the relevant benefits

[4]Six characteristics define the cells into which individuals fall in each quarter: sex (2 categories: male and female), age (9 categories: 16–17, 18–19, 20–21, 22–24, 25–34, 35–44, 45–54, 55–59, and 60–64), marital status (2 categories: married and not married), ethnic origin (3 categories: Maori, Pacific Islander, and European/other), education (5 categories: no qualifications, school but no postschool qualification, postschool but no school qualification, school and postschool qualification, and university degree), and number of children in the family (3 categories: no children, one child, and two or more children). This gives a maximum of 1,620 cells in each quarter. We also know the proportion of individuals in each cell living in families with an infant (aged 0 or 1) or a preschool child (aged 2 to 4), the average number of children in the cells with two or more children, the sum of the "weights" used by Statistics New Zealand to extrapolate the samples to the general population, and various labor market outcomes (e.g., proportion in the labor force and average hours worked per week for those employed).

that individuals face in their labor supply decisions. However, the availability of family income data would raise the issue of how this information should be incorporated into the analysis. At one extreme, it is often assumed that nonlabor family income and, more important, the earnings of the spouse are exogenous. This other family income simply reduces the potential benefit for the individual. Since the labor supply of the spouse may be determined simultaneously, it may be inappropriate to use the current earnings of the spouse to compute this effective benefit. The opposite extreme is used in this paper. All family income is treated as endogenous.

To test whether the estimates of this economic behavior hinge on the treatment of other family income, an alternative data source was used to re-estimate these labor supply regressions. This data set, the Household Economic Survey (HES), is described in more detail below. Although it contains a number of important shortcomings as the primary data set for this project, it does provide a full range of information on family income. The relevant benefit facing an individual was computed net of current nonlabor income of the family and the earnings of the spouse. The results of this analysis are reported in an earlier monograph on this project (Maloney 1997: chapter 7). However, we found that the estimated effects of welfare reforms on labor supply did *not* vary substantially between the two extreme assumptions regarding inclusion or exclusion of other family income in the calculation of the relevant benefits from nonwork.

A second problem associated with the lack of earnings-related information in the HLFS is the lack of some measure of the potential wage rates facing the individuals in our data set. For this information, we again turn to the HES. The HES is a random sample of between 3,000 and 5,200 households interviewed from April of one year to March of the following year. Unit record data from HES are available for the period 1987–88 through 1995–96. This overlaps only partially with the HLFS data running from December 1985 through December 1995. For this reason, we restrict our use of the HLFS data to the period 1987:1 to 1995:4. Separate wage regressions were estimated with the HES data in each of these calendar years. Self-employed and disabled individuals were eliminated from these samples.

The dependent variable in these auxiliary regressions is the natural logarithm of nominal hourly earnings of an individual in his or her primary job at the time of the survey. Usual earnings, including commissions and bonuses, are divided by usual hours of work in that job. This log wage is regressed on dummy variables for being female, Maori, or a Pacific Islander, for educational attainment broken down by the categories matching the HLFS data (i.e, school qualification, postschool qualification, and university degree), and for age and age squared (where age is divided by 10 for ease in displaying parameter estimates). Dummy variables for the first three quarters of the year are included to capture the effects of inflation, labor market conditions, and any other factors specific to each time period. Finally, the female dummy variable was interacted with age to allow for different shapes in wage profiles that may be related to differences in accumulated labor market experience by gender.

Results from these nine separate wage regressions are displayed in Table 1. Sample sizes vary between 2,655 and 3,934 workers. The independent variables explain somewhere between 24 percent and 33 percent of the variation in these log wages. Most of the estimated coefficients have the expected signs and are significantly different from zero at conventional test levels. For example, there is little statistical evidence that Maori face lower wages than Europeans when other measured factors are held constant. On the other hand, consistent evidence is found of systematically lower wages for Pacific Islanders with similar observed characteristics. The overall wage profile is clearly concave with age, but significantly flatter for women than for men. Positive and significant returns are found for educational attainment.

The purpose of these wage regressions is to predict the net weekly full-time earnings for all individuals in our HLFS sample. Even if we had earnings information for workers in the HLFS, we would still need to estimate the potential earnings of nonworkers. The problem is that simple regressions may not accurately predict the wages facing nonworkers. The issue is one of sample-selection bias.

TABLE 1
Estimated Determinants of HES Hourly Earnings: 1987–1995

Independent Variables	1987	1988	1989	1990	1991	1992	1993	1994	1995
Constant	1.000**	.957**	1.218**	1.125**	.812**	.968**	.946**	.948**	1.158**
	(.076)	(.089)	(.101)	(.116)	(.118)	(.103)	(.121)	(.114)	(.112)
Maori	-.021	-.031	-.009	-.008	-.009	-.026	-.073*	.002	-.065*
	(.021)	(.025)	(.025)	(.029)	(.029)	(.026)	(.031)	(.028)	(.031)
Pacific Islander	-.115**	-.048	-.075*	-.063*	-.070*	-.114**	-.179**	-.048	-.115**
	(.028)	(.032)	(.033)	(.032)	(.033)	(.033)	(.036)	(.044)	(.039)
Female	.047	.044	-.030	.074	.066	.044	.126**	.039	-.002
	(.036)	(.040)	(.042)	(.047)	(.046)	(.040)	(.047)	(.047)	(.049)
Age/10	.620**	.662**	.601**	.622**	.762**	.693**	.705**	.698**	.629**
	(.036)	(.040)	(.047)	(.052)	(.052)	(.046)	(.053)	(.050)	(.049)
$(Age/10)^2$	-.067**	-.073**	-.065**	-.066**	-.085**	-.077**	-.077**	-.076**	-.066**
	(.005)	(.005)	(.006)	(.007)	(.007)	(.006)	(.007)	(.007)	(.006)
School qualification	.138**	.153**	.152**	.147**	.164**	.167**	.126**	.196**	.172**
	(.016)	(.020)	(.019)	(.021)	(.023)	(.019)	(.024)	(.023)	(.024)
Postschool qualification	.254**	.274**	.246**	.246**	.236**	.247**	.198**	.268**	.242**
	(.017)	(.021)	(.021)	(.024)	(.024)	(.020)	(.025)	(.024)	(.025)
University degree	.464**	.532**	.534**	.490**	.506**	.485**	.413**	.507**	.435**
	(.023)	(.027)	(.027)	(.030)	(.032)	(.025)	(.030)	(.029)	(.030)
First quarter	-.059**	-.038*	-.077**	-.024	.014	-.029	-.034*	-.062**	-.041*
	(.021)	(.017)	(.019)	(.020)	(.020)	(.019)	(.020)	(.021)	(.021)

(table continues)

TABLE 1, continued

Independent Variables	1987	1988	1989	1990	1991	1992	1993	1994	1995
Second quarter	-.037**	-.032*	-.064**	-.017	-.006	-.009	-.029	-.033*	-.006
	(.014)	(.018)	(.019)	(.021)	(.021)	(.017)	(.021)	(.020)	(.021)
Third quarter	-.035*	-.006	-.044*	-.019	-.019	.016	-.038*	-.021	-.037*
	(.015)	(.018)	(.019)	(.021)	(.020)	(.017)	(.021)	(.020)	(.021)
Age/10 • Female	-.068**	-.056**	-.039**	-.063**	-.057**	-.052**	-.067**	-.040**	-.043**
	(.009)	(.010)	(.011)	(.013)	(.012)	(.011)	(.013)	(.012)	(.012)
Inverse Mills ratio	.011	-.003	-.104*	-.030	.061	.012	.033	.031	-.035
	(.037)	(.041)	(.042)	(.046)	(.047)	(.040)	(.046)	(.047)	(.048)
N	3,747	3,538	3,284	2,687	2,655	3,934	3,105	2,852	2,866
R^2	.330	.320	.323	.289	.299	.270	.239	.273	.278

*Significant at .10 level, two-tailed test.
**Significant at .01 level, two-tailed test.

Notes: Standard errors in parentheses. Dependent variable is the natural log of nominal hourly earnings of workers in their current, primary jobs in the Household Economic Survey. The inverse Mills ratio is computed from separate probit equations on the probability of being employed and reporting hourly earnings at the time of the survey, and controls for possible sample-selection bias.

Unobserved variables that influence the likelihood that individuals are working at the time of the survey may also affect the wages they face in the labor market. For example, one might anticipate that nonworkers face systematically lower wages than workers with similar observed characteristics.

To control for possible sample-selection bias, we estimate the probability that individuals in the HES were working and reporting hourly earnings at the time of the survey. The regressors in these annual maximum likelihood probit regressions include the independent variables in the wage regressions and other variables believed to influence labor supply behavior (e.g., marital status and number and ages of children in the family). These regression results are not reported, but inverse Mills ratios computed from these probit equations are included in the wage regressions to control for possible sample-selection bias. The estimated coefficients on these lambda terms are weakly significant in only one of the nine regressions. The statistical evidence does not support the conclusion that nonworkers face lower wages than workers once observed characteristics are held constant.

Hourly earnings are first predicted for all individuals in our HLFS sample by multiplying their personal characteristics by the estimated coefficients from the relevant HES wage regressions.[5] This estimated wage rate is then multiplied by 40 to predict potential full-time weekly earnings. These gross earnings are then reduced by the income taxes that these individuals would have faced in the relevant fiscal year. A computer subroutine constructed for this purpose generates after-tax earnings from data on personal and family characteristics and from information on the income tax code in each tax year. A second computer program estimates any benefits that these individuals would have been entitled to if they worked full-time or not at all. Finally, the replacement rate can be constructed as a descriptive statistic. It is the ratio of the maximum weekly after-tax benefit divided by the sum of after-tax, full-time

[5]To reduce the bias in predicting wages from log-wage regressions, half of the estimated variance of the disturbance term is added to the predicted log wages before taking antilogs of these fitted value (see Kmenta 1986: 511–512).

benefits and earnings. It is the proportion of net income from full-time work that can be “replaced” by net benefits if the individual does not work.[6]

Figure 3 plots the mean quarterly replacement rates for our sample of individuals aged 16 to 64 over the period 1987:1–1995:4. The average replacement rate during this sample period was 55.8 percent. The lowest individual replacement rate in our sample was 20.0 percent, and the highest was 120.8 percent.[7] These and other descriptive statistics are reported in Table 2. The mean replacement rate in our sample increased from approximately 58 percent to 60 percent by mid-1988. It then fell to around 56.5 percent prior to the main benefit reforms. The reforms announced in December 1990 took full effect by June 1991. The result was an immediate drop in the replacement rate from over 56.5 to under 53 percent. This rate fluctuated between 53 percent and 55 percent from mid-1991 to the end of 1995.

Although real benefit levels were relatively constant after 1991 under all programs, the “effective” benefits for those aged 60–64 declined as the age of eligibility for Superannuation was gradually raised from 60 to 65 after April 1992. The effective benefit for those in this age category is the proportion of those still eligible for Superannuation multiplied by this benefit, plus the proportion now eligible for UB multiplied by this relatively lower benefit. For example, when the age of eligibility was raised from 60 to 61 in April 1992, it was assumed that 20 percent of the individuals in the cells containing 60–64 year-olds were no longer eligible for Superannuation. Thus, the effective benefit for this older age group fell steadily after April 1992.

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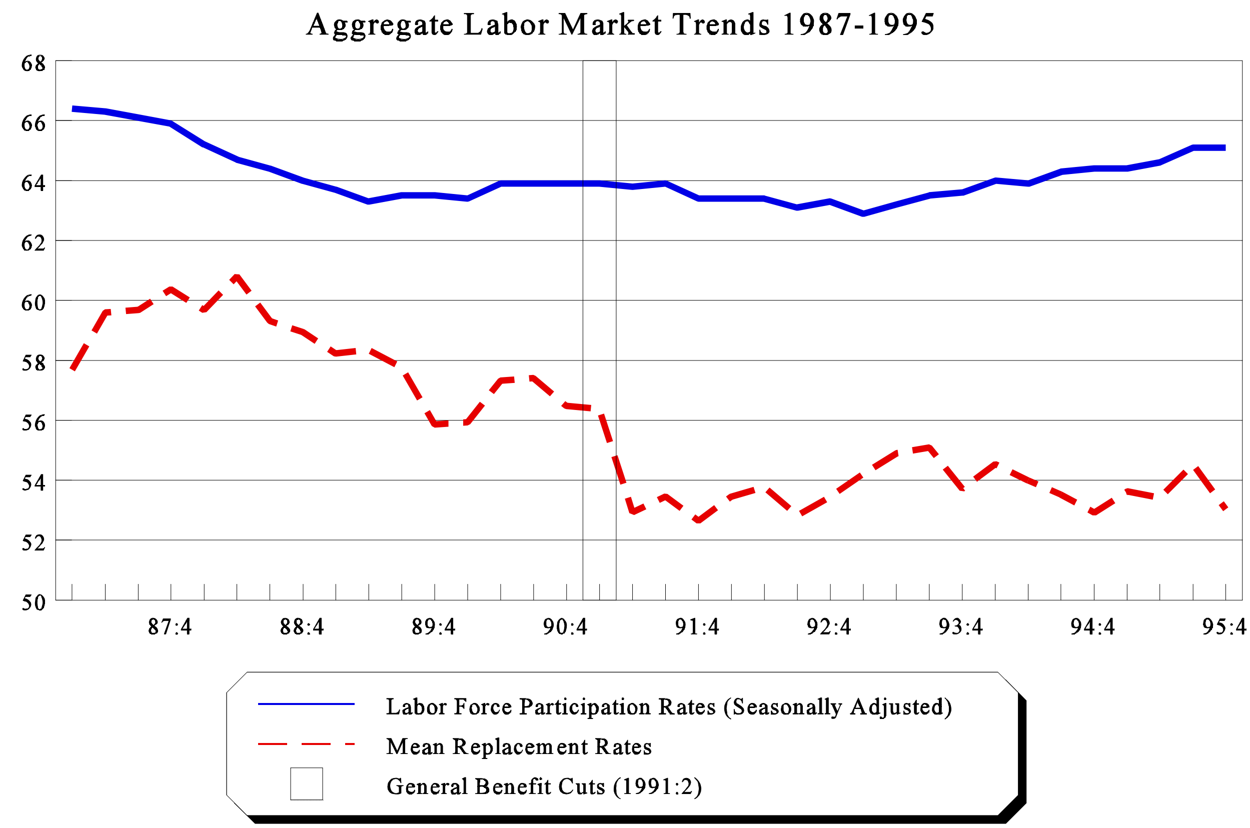
[6]The replacement rate for 16- and 17-year-olds is *not* recorded as zero after the age of eligibility was raised to 18 for both UB and DPB in 1991. Two new programs came into existence at this time. The Independent Youth Benefit (IYB) provides emergency assistance for those who cannot be supported by their parents, and the Job Search Allowance provides benefits to youth who have lost their jobs or have completed training programs. Since the vast majority of 16- and 17-year-old beneficiaries have ended up on IYB, which offers more generous assistance, this benefit level replaces the basic unemployment benefit after 1991. A dummy variable is included in the regression analysis to capture any effects of a tightening in this eligibility criterion.

[7]Replacement rates can exceed 100 percent because individuals are ineligible for benefits under the UB and DPB programs if they work more than 30 hours per week (although they may still be entitled to some benefits under the Family Benefit or Family Support programs). This creates a discontinuity in the relevant budget constraint.

Figure 3



Notes: The aggregate, seasonally adjusted labor force participation rate is taken from Statistics New Zealand for all individuals 16 years old and over. The mean replacement rates are based on the author's calculations using HLFS and HES data for those aged between 16 and 64.

TABLE 2
Descriptive Statistics 1987–1995

Variables	Mean	Standard Deviation	Minimum	Maximum
Maori	0.099	0.299	0.000	1.000
Pacific Islander	0.039	0.194	0.000	1.000
Female	0.504	0.500	0.000	1.000
Married	0.629	0.483	0.000	1.000
Child aged < 2 in family	0.092	0.181	0.000	1.000
Child aged 2 to 4 in family	0.125	0.217	0.000	1.000
Average number of children	0.786	1.091	0.000	7.500
Age	36.677	13.287	16.500	62.000
No qualifications	0.330	0.470	0.000	1.000
School qualification only	0.265	0.441	0.000	1.000
Postschool qualification only	0.335	0.472	0.000	1.000
University degree	0.071	0.256	0.000	1.000
Labor force participation rate	0.744	0.234	0.000	1.000
Proportion “studying”	0.045	0.107	0.000	1.000
Predicted real hourly earnings	$13.871	$3.435	$6.268	$27.020
Replacement rate	0.558	0.167	0.200	1.208
Rise in eligibility age for UB & DPB	0.029	0.166	0.000	1.000
Rise in eligibility age for Superannuation	0.010	0.061	0.000	0.550
Number of individuals in cells	28.435	60.089	1.000	889.000

Notes: Number of cells = 32,663. All observations are weighted by population weights constructed by Statistics New Zealand to extrapolate the HLFS to the general population. The only exception is the raw count of individuals within the cells.

Since we ultimately want to know the extent to which these benefit changes influenced labor supply, we plot the aggregate, seasonally adjusted labor force participation rate using HLFS data over the same period in Figure 3. Between early 1987 and the time of the general benefit cuts, participation declined steadily from just over 66.4 percent to just under 64 percent. It continued to decline until it reached 63 percent by the end of 1992, but rebounded to 65.1 percent by the end of 1995. Inspection of the relationship between these aggregate series does not provide clear evidence of the hypothesized negative impact of replacement rates on labor supply. These descriptive statistics, of course, may not be indicative of the true relationship between the variables. We are not holding constant the variety of other factors that may be independently influencing labor supply, and we are not taking advantage of the available disaggregated data. One concern here is the cyclical effects of a deep recession experienced in New Zealand in the period before and during these benefit reforms, followed by a vigorous recovery in the post-reform period. For this reason, we use regression analysis to isolate the effects of the *specific* benefit changes that are most relevant to the individuals in our sample and to control for other appropriate factors.

Figure 3 raises the issue of why replacement rates changed over our sample period. They could be attributed to changes in benefit programs, potential gross earnings in the labor market, and the income tax system. Table 3 decomposes the aggregate change in the mean replacement rate over the 5 years since the 1991 benefit reforms. Average values of the statistics in 1990 are compared to the average values in 1995. Overall, replacement rates for our sample fell from 56.79 percent to 53.65 percent between these years. This represents a 5.5 percent decline from the 1990 level. In real terms, maximum weekly benefits fell by 9.5 percent over this period. The smaller relative decline in replacement rates can be attributed to three sources. First, potential *gross* weekly earnings fell by 2.2 percent. Second, an increase in the effective income tax rate meant that *net* weekly earnings fell by a larger 3.6 percent. This income tax effect came from tax brackets that were fixed in nominal terms (i.e., “bracket creep”) during

TABLE 3

Decomposition of Changes in Mean Replacement Rates: 1990–1995

	1990	1995	% Change 1990–1995
Replacement rates	56.79%	53.65%	-5.5%
Maximum real weekly benefits	$255.56	$231.29	-9.5%
Gross real weekly full-time earnings	$563.07	$550.70	-2.2%
Net real weekly full-time earnings	$430.79	$415.40	-3.6%
Net real weekly full-time income	$452.17	$431.61	-4.6%

a period of low but steady inflation and from the introduction of payroll taxes in later fiscal years. Finally, some individuals qualify for partial benefits with full-time work through the Family Benefit, Family Support, and Superannuation programs. Because of various changes in these programs since 1990, potential net real weekly *income* declined by 4.6 percent while potential net real weekly *earnings* fell by only 3.6 percent. Thus, the mean replacement rate declined by less than the cut in benefits, because gross wages were falling, effective income tax rates were rising, and the potential benefits associated with full-time work were dropping.

V. REGRESSION RESULTS

Column 1 of Table 4 reports the parameter estimates for labor force participation decision among individuals aged between 16 and 64 in the HLFS over our sample period. Observations are "cell means" where individuals are grouped by the personal and family characteristics defined earlier. The dependent variable is the proportion of those within the cell who are either officially employed or unemployed at the time of the survey. Thus, it is a continuous variable within the 0–1 interval.[8] Weighted, generalized least-squares (GLS) estimation is used in all regression results reported in this table. Observations are weighted by the sample weights constructed by Statistics New Zealand to extrapolate these random samples to the general population.

The disturbance term in the generic labor supply regression in Section III contained a latent term that varied across individuals but not time. However, it would be inappropriate to use fixed-effects techniques to eliminate the cross-sectional variation in these data. The reason is that they do not constitute a true panel data set; they could be referred to as a "synthetic" panel. The same individuals are

[8]This does raise the issue of the "censoring" of the dependent variable at zero and one. The failure to incorporate this censoring into the estimation procedure could produce coefficient estimates that are both biased and inconsistent. However, this censoring problem is unlikely to be appreciable in this study. More than 97 percent of the individuals in our sample are located in cells with participation rates between the extremes of zero and one.

TABLE 4
Estimated Determinants of Labor Supply: Random Effects Estimation

Independent Variables	Labor Force Participation	Hours of Labor Supplied	Labor Force Participation + Studying
	Dependent Variables		
Maori	-.059**	-3.134**	-.063**
	(.004)	(.219)	(.003)
Pacific Islander	-.044**	-2.997**	-.015**
	(.005)	(.272)	(.004)
Married	.225**	12.550**	.241**
	(.007)	(.364)	(.005)
Child aged < 2 in family	.043**	.742	.062**
	(.012)	(.591)	(.009)
Child aged 2 to 4 in family	-.018	-.668	-.011
	(.012)	(.627)	(.011)
Average number of children in family	.018**	.770**	.051**
	(.004)	(.200)	(.003)
Female	-.047**	-4.443**	-.036**
	(.006)	(.312)	(.003)
Female • Married	-.050**	-5.992**	-.051**
	(.008)	(.400)	(.004)
Female • Child aged < 2 in family	-.371**	-12.475**	-.460**
	(.015)	(.739)	(.012)
Female • Child aged 2 to 4 in family	-.147**	-5.787**	-.158**
	(.016)	(.812)	(.014)
Female • Average number of children	-.023**	-2.017**	-.017**
	(.004)	(.223)	(.003)
Aged 16 or 17	-.148**	-11.266**	.105**
	(.012)	(.596)	(.007)
Aged 18 or 19	-.015*	-4.302**	.102**
	(.009)	(.463)	(.005)

(table continues)

TABLE 4, continued

Independent Variables	*Dependent Variables* Labor Force Participation	 Hours of Labor Supplied	 Labor Force Participation + Studying
Aged 20 to 24	.038**	-.532*	.098**
	(.006)	(.301)	(.003)
Aged 55 to 59	-.198**	-9.139**	-.212**
	(.007)	(.351)	(.004)
Aged 60 to 64	-.629**	-26.358**	-.635**
	(.008)	(.428)	(.005)
Potential income from full-time work	.064**	2.315**	.073**
	(.003)	(.132)	(.002)
Potential benefits from no work	-.113**	-5.010**	-.179**
	(.005)	(.243)	(.004)
Rise in eligibility age for UB and DPB	-.051**	-3.175**	.042**
	(.005)	(.242)	(.005)
Rise in eligibility age for Superannuation	.305**	12.158**	.312**
	(.012)	(.588)	(.011)
R^2	0.810	0.848	0.830

*Significant at .10 level, two-tailed test.
**Significant at .01 level, two-tailed test.

Notes: Number of cells = 32,663. Standard errors are in parentheses. Observations are weighted by population weights constructed by Statistics New Zealand. “Potential income from full-time work” and “Potential benefits from no work” are measured in hundreds of 1995:4 dollars per week. Dummy variables for the 36 quarters are included in these regressions, but these results are not reported. Random effects are based on the 1,371 cells of individuals observed over the sample period.

not followed over the entire sample period. Although rotation groups keep the same households in the HLFS for 2 years, the individuals within these households can migrate across cells due to changes in age, marital status, number of children in the family, etc. These groups of individuals are essentially random samples of all individuals in the population at a point in time who share the same characteristics. This means that the observed labor market outcomes within the cells are sample statistics. For this reason, random effects estimation is used. Disturbance terms are allowed to be correlated across time for the demographic groups demarcated by the cells in our data set.[9]

There are 32,663 cells in the labor supply regressions, containing information on over 928,000 individual observations. Column 1 of Table 4 shows the basic findings from this study in terms of labor force participation. Most of the control variables have the expected signs and are significantly different from zero at conventional test levels. Maori and Pacific Islanders are less likely to participate in the labor force. Marital status and the number and ages of children in the family have substantially different effects on participation between men and women. Individuals outside the prime working age of 20 to 54 are less likely to be in the labor force. Although dummy variables for all 36 quarters are included in the regressions, these results are not reported. They were designed to capture all time-specific factors that did not vary across individuals (e.g., general economic conditions).

Two variables are used to measure the net gain from full-time work. The first is the estimated potential after-tax weekly income from full-time work. The second is the estimated potential after-tax weekly income from no work. The Consumer Price Index is used to express these variables in constant 1995:4 dollars, and both are expressed in hundreds of dollars for ease in displaying the resulting coefficient estimates. Our null hypotheses are that the first variable will have a positive and the second variable a negative impact on labor force participation.

[9]See Verbeek and Nijman (1992) for a discussion of estimation techniques using synthetic panel data.

The estimated coefficient on potential income from full-time work is 0.064. This says that an increase in weekly earnings of $100, other things held constant, increases labor force participation by 6.4 percentage points. With a standard error of 0.003, this effect is significant at better than a 1 percent level.[10] This is consistent with our prior expectation that greater returns from work increase labor supply (i.e., labor supply functions slope upward). This estimated coefficient can be converted into an elasticity by dividing by the mean of the dependent variable (0.744) and multiplying by the mean of the independent variable (4.437). The estimated labor supply elasticity is 0.382.

The estimated coefficient on potential income from no work is -0.113. With a standard error of 0.005, this estimated coefficient is statistically significant at better than a 1 percent level. This is consistent with our null hypothesis that higher benefits decrease labor supply (i.e., the labor supply function shifts inward to the left). An increase in weekly benefits of $100, other things held constant, decreases labor force participation by 11.3 percentage points. This labor supply effect from a change in benefits is 77 percent larger in absolute magnitude than the effect of the same change in full-time earnings. The null hypothesis that the coefficients on the wage and benefit variables are identical can be rejected at a 1 percent level. This supports the inclusion of these variables as separate regressors, rather than using their ratio as a single regressor (i.e., the replacement rate). Again, this estimated coefficient can be converted into an elasticity by dividing by the mean of the dependent variable and multiplying by the mean of the independent variable (2.471). This estimated elasticity of 0.375 is remarkably close to the labor supply elasticity of 0.382 found above for earned income.

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[10]The standard error on this estimated coefficient may be biased, because the regressor is based on a predicted value from an earlier regression. For example, it is well known that the true standard error on a fitted value in the second stage of a two-stage least squares procedure may need to be increased. However, the procedure for making this adjustment is not possible in this case, because actual earnings are never observed among HLFS workers and the resulting regressor is not simply the fitted value from an earlier gross wage regression. With a t-statistic of 24.9, however, it is unlikely that any adjustments to the standard error would jeopardize the statistical significance of this variable.

Two dummy variables capture the increases in age of eligibility for the three principal social welfare programs during our sample period. The first is associated with increases in the age of eligibility from 16 to 18 for basic benefits under UB and DPB. This variable takes on a value of one for unmarried 16- and 17-year-olds with children after 1991:2 (when the age of eligibility for DPB was raised), and a value of one for other 16- and 17-year-olds after 1990:4 (when the age of eligibility for UB was raised). This variable is set equal to zero for all other age groups and other time periods.

The second variable rises slowly from zero to one as the age of eligibility for Superannuation is gradually raised from 60 to 65. Our HLFS data are grouped into nine age categories, including one for those between 60 and 64. In 1992:2, the age of eligibility for Superannuation was raised from 60 to 61. Since this policy directly affected approximately 20 percent of the 60- to 64-year-old individuals in our sample, this variable jumped from zero to 0.2 for this age group. The minimum age of eligibility has since been raised in 3-month increments every 6 months and will reach 65 in 2001. Our policy variable mimics this steady rise in the age of eligibility for Superannuation, reaching 0.55 in 1995:4. Our null hypotheses are that increases in the age of eligibility under all three programs will increase the labor supply among the relevant age groups.[11]

The impacts of these qualitative program changes on labor force participation are mixed. The increase in the age of eligibility for Superannuation has a positive effect on participation. The estimated coefficient of 0.305 is significant at better than a 1 percent level. This says that when the full impact of this rise in the age of eligibility from 60 to 65 has occurred in 2001 (when the dummy variable has reached a value of one), labor force participation among those aged 60 to 64 is predicted to increase by 30.5 percentage points (other things held constant). This may seem like an unusually large behavioral

[11]The rise in the age of eligibility for Superannuation is also reducing the potential benefits for this age group. Individuals no longer eligible for Superannuation become eligible for lower benefits under the UB program. However, we expect this rise in the age of eligibility for Superannuation will have a larger impact than just the fall in potential benefits. The primary reason is that individuals are expected to seek employment under the other social welfare programs, while no such obligation is inherent under Superannuation.

response. However, this policy now directly affects just over half of those in this age group. Multiplying the value of this variable in 1995:4 (0.55) by the estimated coefficient (0.305) gives us a predicted increase in labor force participation of 16.8 percentage points. This is not far from the actual increase in labor force participation of those aged 60 to 64 of 12.8 percentage points between December 1991 and December 1995. Independent of any associated effects of the *level* of benefits available, raising the age of eligibility for publicly funded retirement has had a considerable impact on labor supply.

Contrary to our original hypothesis, the increase in the age of eligibility from 16 to 18 for both UB and DPB programs appears to have *reduced* labor supply. The estimated coefficient of -0.51 is significant at better than a 1 percent level. Since these youth are potentially eligible for benefits under other programs with much tighter eligibility criteria but similar benefit amounts (included under "Potential income from no work"), this dummy variable would capture any increase in participation among 16- and 17-year-olds. Instead, it says that labor force participation among 16- and 17-year-olds declined by 5.1 percentage points as a result of this tightening in eligibility criteria. We return to this issue later in this section.

The labor force participation rate may not adequately capture the labor supply of the individuals in our sample. It ignores any variation in how much time individuals are willing to work once they are in the labor force. As an alternative, the labor force participation rate was multiplied by the number of hours worked per week by those employed within each cell. This assumes implicitly that those unemployed in a given cell would want to work the same workweek as those employed within the same cell. Without having access to a measure of the desired hours of work among all participants, this seemed like the best possible comprehensive measure of labor supply.

The regression model was re-estimated with this new dependent variable on hours of labor supplied. The results are reported in column 2 of Table 4. Note that the estimated coefficients increase substantially as we move from column 1 to column 2. The reason for this is that the unit of analysis has changed. Rather than talking about changes in the propensity to participate, we are now looking at changes in hours of labor supplied per week.

Again, most of the covariates have the expected signs and are significantly different from zero. The estimated coefficient on potential income from full-time work is 2.315 and is significant at better than a 1 percent level. This says that an increase in weekly earnings of $100, other things held constant, increases the desired workweek by over 2.3 hours. This reinforces our earlier finding that greater returns from work increase labor supply. We can convert this estimated coefficient into an elasticity by dividing by the mean of the dependent variable (28.263) and multiplying by the mean of the independent variable (4.437). The estimated labor supply elasticity is 0.363. This result is not substantially different from the elasticity computed earlier for labor force participation (0.382).

The estimated coefficient on potential income from not working is -5.01, and significant at better than a 1 percent level. This says that an increase in weekly benefits of $100, other things held constant, decreases the desired workweek by 5 hours. This labor supply effect of a change in benefits is, in absolute value, more than twice as large as the effect of a similar change in full-time earnings. Again, the null hypothesis that these coefficients are equal to one another can be rejected at a 1 percent level. The estimated labor supply elasticity with respect to benefit income (0.438) is now larger than the estimated labor supply elasticity with respect to work income (0.363).

Note that the estimated coefficient on the rise in the age of eligibility for UB and DPB is still negative and significant in this second regression. Counter to our original hypothesis, this tightening in eligibility criteria again reduced the labor supply of youth. One reason for these findings is that the definitions of labor supply used thus far may be too narrow for the affected group of 16- and 17-year-olds. A broader definition of “economic activity” might include current participation in either the labor force or educational attainment. It would be easy to show in a simple model of human capital investment that reductions in future welfare benefits could increase optimal schooling levels.

Column 3 of Table 4 reports the results from a re-estimation of the original regression, where the new dependent variable is the proportion of individuals within a cell who are either in the labor force or report "studying" as their main activity during the survey week.[12] It is useful to compare the parameter estimates from this regression to the original regression on labor force participation in column 1. An increase in potential weekly earnings of $100 increases participation in the labor force or education by 7.3 percentage points. An increase in potential weekly benefits of $100 decreases participation in the labor force or education by 17.9 percentage points. Since these effects are *larger* in absolute magnitude than the estimated effects in the earlier labor force participation regression, we must conclude that these factors do influence educational enrollments.

The most important finding from this last regression relates to the rise in the age of eligibility for UB and DPB. We saw earlier that this policy reduced labor force participation of youth by 5.1 percentage points, other things held constant. Yet we now find that this policy *increased* the participation of 16- and 17-year-olds in *both* the labor force and education by 4.2 percentage points. This estimated coefficient is significant at better than a 1 percent level. The increase in educational participation associated with this tightening in eligibility criteria appears to have more than offset the reduction in labor force participation for this affected age group.

These regression results provide empirical estimates of labor supply responses to both quantitative and qualitative changes in New Zealand's social welfare programs. To put these responses in perspective, we multiply these estimated coefficients by the actual changes in the magnitudes of these policy variables between 1990 and 1995. The resulting calculations are displayed in Table 5. For example, average real benefit guarantees fell by 9.5 percent between 1990 and 1995. This decline of $24.27 per week was multiplied by the estimated coefficient in column 1 of Table 4 on potential benefits

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[12]Unfortunately, no information is available in the HLFS on educational participation among the employed or unemployed. Nor do we have any information on the job training of any respondents in this data set.

TABLE 5

Estimated Effects of Benefit Reforms on Labor Supply: 1990–1995

	Percentage Point Change in Labor Force Participation	Change in Hours of Labor Supplied Per Week	Percentage Point Change in Labor Force Participation + Studying
9.5% decline in maximum benefits	2.74	1.22	4.34
1.0% decline in partial benefits from full-time work	-0.33	-0.12	-0.38
Rise in eligibility age for UB and DPB	-0.25	-0.16	0.21
Rise in eligibility age for Superannuation	0.99	0.39	1.01
Net effects of reforms	3.15	1.33	5.18

Notes: Each individual effect in this table is the product of the relevant estimated coefficient in Table 4 multiplied by the actual change in the mean independent variable between 1990 and 1995. The "net effects" at the bottom of each column are simple summations of these individual effects.

from no work (-0.113). The result is the estimated increase of 2.74 percentage points in the aggregate labor force participation rate from this average benefit cut. These same reforms reduced the potential benefits from full-time work. This $5.16 per week decline (a 1.0 percent fall in overall work income) multiplied by the estimated coefficient on potential income from full-time work (0.064) gives a 0.33 percentage point decrease in labor force participation. The same was done for the respective effects associated with the actual changes in the variables for the increases in ages of eligibility.

The net effects of all welfare reforms between 1990 and 1995 on aggregate labor supply are shown in the bottom row of Table 5. We estimate increases of 3.15 percentage points in labor force participation, 1.33 hours in labor supplied per week, and 5.18 percentage points in participation in either the labor force or education. Since the responses are not directly comparable, these effects are divided by the mean of the respective dependent variables over the sample period to convert them to elasticities. Larger estimated responses from these reforms are found for combined labor force and educational participation (6.56 percent) and hours of labor supplied per week (4.71 percent) than for labor force participation (4.23 percent).

One final cautionary note about these results should be mentioned. It has been assumed implicitly throughout this analysis that these benefit reforms had no impact on the wages that workers receive in the labor market. It is possible that at least some of the estimated decline in gross full-time earnings between 1990 and 1995 (see Table 3) resulted from the increase in labor supply accompanying these reforms. Although no attempt is made in this study to isolate the possible indirect effect of these reforms, some quick calculations can be made to show how it would alter the above findings. Suppose that the 2.2 percent decline in real gross labor market earnings between 1990 and 1995 can be attributed to these benefit reforms. These lower returns from full-time work would reduce labor supply. It this case, it is estimated that the overall effects of these welfare reforms on labor force participation would decline from 3.15 to 2.36 percentage points. Similar reductions in the other measures of labor supply could be computed due to these indirect effects on wages.

VI. CONCLUSIONS

The findings of this study support the conclusions that, in general, social welfare programs influence labor supply behavior, and that, specifically, recent benefit reforms increased labor supply in New Zealand. These results were generated through regression analysis on disaggregated data between 1987 and 1995. Both quantitative and qualitative changes to the structure of New Zealand's main social welfare programs over this period were incorporated in this estimation. Synthetic panel data were used to allow for both time-series and cross-sectional variation in the relevant variables. This is important because benefit changes over the sample period were not uniform across the population.

In accordance with economic theory, reductions in benefits increased labor force participation and hours of labor supplied. These effects were reinforced by the rise in the retirement age. No single demographic group experienced a larger impact from the benefit reforms than 60- to 64-year-olds as the age of eligibility for Superannuation was gradually raised from 60 to 65.

Contrary to our original hypothesis, the rise in the age of eligibility for basic benefits under the UB and DPB programs *decreased* labor force participation and hours of labor supplied by the affected group of 16- and 17-year-olds. However, subsequent regression analysis suggests that these results were due to an excessively narrow definition of "economic activity." The reforms in New Zealand appear to have increased the educational enrollment of youth by more than enough to offset any negative effects on labor force participation.

Overall, the benefit reforms implemented in New Zealand since 1990 are estimated to have increased aggregate labor force participation by more than 3 percentage points. Slightly larger *relative* effects are estimated for both hours of labor supplied and participation in either the labor force or education. Yet, these positive labor supply effects could be reduced if these reforms indirectly lowered the returns from employment. This issue, and many others involving the potential effects of these reforms on the labor market, must await future research efforts.

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