**Impact of Social Security Reform on Labor Force Participation Rates of Pensioners and Non-pensioners: Evidence from Chile**

by

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## Abstract

Recent theoretical and empirical research has argued that incentives stemming from social security systems influence the worker's decisions about retirement, investment in human capital and even family formation. The experience of Chile, which radically changed its national system in 1981, offers an opportunity to test this hypothesis. Chile shifted from a defined benefit system to an actuarially fair defined contribution plan, exempted pensioners from the pension payroll tax and tightened early retirement restrictions. Using probit analysis and simulations of the behavior of 50 cohorts followed through 47 household surveys, we estimate the impact of the 1981 pension reform on the probability of dropping out of the labor force for older workers. We also try to disentangle the effects of each reform component. We find large effects: Labor force participation rose dramatically among male cohorts that approached retirement age after 1981 in Chile, in contrast to the decline observed before and the decline taking place in other countries at the same time. The rise was particularly great for pensioners, suggesting that the payroll tax exemption played a large role. Longer working lives and lower implicit and explicit taxes on labor should also lead individuals to invest more in education, job training and other types of human capital that yield a labor market return.

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**Impact of Social Security Reform on Labor Force Participation Rates of Pensioners and Non-pensioners: Evidence from Chile**

Labor force participation of older men has declined in many countries during recent decades. This has decreased the available supply of labor, increased the fiscal problems of pension programs and diminished the incentive to invest in human capital whose returns accrue over the working life. A considerable literature has developed on the extent to which incentives stemming from social security systems have contributed toward this decline or can help reverse it. The experience of Chile, which radically changed its system in 1981, offers an opportunity to test whether large system changes produce large changes in the supply of labor and the human capital it embodies.

Prior to 1981 Chile had a traditional pay-as-you defined benefit (DB) system that included disincentives to work among older individuals, similar to those found in many European countries today. Contribution rates were high and allowable pension ages low. Benefits didn't change commensurately with incremental contributions, postponed pensions or increased longevity. Workers had strong incentives to start their pensions as soon as possible because this maximized their lifetime net benefits, and labor force participation dropped dramatically when pensions started. This changed with the 1981 reform, which replaced the DB system with a defined contribution (DC) system, exempted pensioners from the payroll tax and tightened early pension constraints. Previous work on Chile's reform has emphasized the positive growth impact of the increased saving and financial market development stemming from the shift from pay-as-you-go to full funding (Schmidt-Hebbel 1999, Acuna and Iglesias 2001). This paper shows that the reform also increased growth through another channel—the increase in labor supply and human capital—due to the new incentives and constraints. We are not aware of any previous study addressing this impact of the Chilean pension reform or, more generally, the labor supply impact of a shift from a national DB to DC plan.[1]

We seek to measure the over-all impact of the reform on labor force participation rates (LFP) and pension age, and to disentangle the effects that are due to tighter early pension conditions, which could be imposed in a DB system, versus those that are due to incentives stemming from the shift to a DC system and the reduction in implicit and explicit pension payroll taxes. To separate these effects, we exploit differences in the rules that apply to pensioners versus non-pensioners and between individuals over and under age 65. We expect:

(1) The exemption of pensioners from the pension payroll tax and the elimination of restrictions on their continued work should increase incentives for pensioners to work; this is the most clear-cut prediction.

(2) Giving market returns to incremental contributions by non-pensioners via the shift from DB to DC should increase work incentives for non-pensioners; the effect should be larger for non-pensioners over 65, who are totally exempt from the pension payroll tax.

(3) The DC accumulation is eventually turned into a pension on actuarially fair terms and postponed pensioning produces an equivalent increase in monthly payout. This reduces incentives for individuals to start their pension early. In addition, tighter early pension pre-conditions increase the number of older individuals who are required to postpone pensioning. Both these effects should increase over time as life expectancy rises, depressing the monthly pension amount. This should raise aggregate participation rates further, since non-pensioners are more likely to work than pensioners.

To carry out this analysis we draw on 47 years of household surveys conducted by the Universidad de Chile, which give us employment and retirement data for members of 50 cohorts in the age range 50-70 before and after the reform. We use a probit model with year of birth as a proxy for probability of membership in the new system. Our main findings: Age-specific labor force participation rates (*LFP*) rose steadily among "post-reform" cohorts that reached old age after 1981, in contrast to the declining *LFP* observed before. The increase was particularly dramatic among pensioners, whose work propensities more than doubled—consistent with hypothesis #1. We also observe a smaller increase in *LFP* among non-pensioners, especially for those over age 65—consistent with hypothesis #2. Initially pensioners worked much less than non-pensioners, but the gap has narrowed over the last two decades. At the same time, the probability of early pensioning fell steadily. Postponed pensioning could be due to voluntary choice under actuarial fairness or to tighter pre-conditions on early pensions. The sharp drop-off in postponement at age 65 suggests that tighter pre-conditions dominate. These behavioral impacts of the reform remain after controlling for many individual and time-specific co-variates, including macro-economic conditions.

Part 1 reviews the literature on the impact of social security system rules on retirement age and develops our hypotheses about work and pension incentives in the new versus old Chilean schemes. Part II describes our data set, probit model, reform indicators and other variables, and presents descriptive data. Parts III and IV estimate the impact of the reform on the probability of becoming a pensioner and of dropping out of the labor force for older men. Longer working lives and lower implicit and explicit taxes should also increase individuals' incentive to invest in education, on-the-job training and other components of human capital that earn labor market returns. Part V pulls together the likely effects on human capital, family formation and economic growth. The Conclusion highlights lessons for other countries.

## I. What do Theory and Empirical Evidence Suggest about Retirement Choice?

**Retirement behavior and pension systems: theoretical framework**

An adaptation of the options value method of measuring marginal retirement incentives used by Gruber and Wise (1999 and 2004) is the starting point for our analysis of the impact of the Chilean reform. According to this model, workers calculate the expected present value (*EPV*) of incomes for all future potential retirement dates. They postpone retirement beyond the earliest eligible age (t) to some future date (r), if the utility from the incremental *EPV* of net wages between t and r plus incremental lifetime benefit growth due to postponing retirement (date at which pension starts and work stops) from t to r exceeds the utility from incremental leisure between t and r.

$$U\ (EPV \textstyle\sum_t^r (W - C) +\ EPV \sum_t^D (B_r - B_t)) > U(L_t^r) \qquad (1)$$

where: $EPV \sum_t^r (W - C)$ = individual's gross wage minus contributions from t to r

$EPV \sum_t^D (B_r - B_t)$=expected present value of the difference in benefit streams due to postponing retirement from period t to r, summed over period t to death (D)

$U(L_t^r)$ = discounted utility of incremental leisure from t to r

In mandatory systems, social security wealth ($EPV \sum B_t$) exceeds the amount many individuals would have saved voluntarily. This increases the relative value placed on leisure and the desire to stop work. This wealth effect combined with liquidity constraints is one of the reasons workers start the pension at the earliest eligible age and stop work at the same time.

If the mandatory system is DB, an additional incentive to retire as soon as possible stems from the absence of actuarial fairness. In a system that is actuarially fair at the margin, 1) incremental contributions yield incremental monthly benefits with an equivalent EPV and 2) postponing the pension age incrementally raises monthly benefits enough to hold the total EPV constant. In addition, in a system that is actuarially fair over the lifetime, 3) the EPV of total contributions equals the EPV of total benefits (see Disney, Queisser and Whitehouse 2006). If mortality is given, incremental actuarial fairness implies that:

$$EPV\left[\sum_t^D (B_r - B_t) - \sum_t^r C\right] = 0 \qquad (2)$$

Then, equation (1) collapses to equation (3), and work continues so long as:

$$U\left(EPV \sum_t^r (W)\right) > U(L_t^r) \qquad (3)$$

In contrast, in an actuarially unfair system equation (2) does not hold. Around the world, DB systems are often actuarially unfair (empirical evidence discussed below). Specifically, $EPV\left[\sum_t^D (B_r - B_t) - \sum_t^r C\right] < 0$, creating a tax wedge between gross and net remuneration, leading the pension to start and contributory work to stop in accordance with equation (1), sooner than it would under equation (3). As a result of these system rules, two potentially separable decisions--starting the pension and stopping work— are de facto linked, a single term—retirement--is commonly used to denote them, and workers retire at the earliest eligible age. As government policies have reduced the earliest eligible pension age, increased the tax wedge and expanded pension wealth, retirement age has dropped in many countries.

When evaluating systems over long periods of time, during which life expectancy may change, lifetime actuarial fairness is relevant. This requires that the EPV of all contributions from start of work (s) to final contribution (F) equals the EPV of all benefits from pension age (p) to expected age of death (D)--for any pension age and final contribution date that might be chosen, as mortality rates change.[2]

$$EPV \sum_s^F C = EPV \sum_p^D B \qquad (4)$$

Since the EPV of lifetime benefits is determined by the level of accumulated contributions, the monthly benefit must fall as life expectancy rises, in systems with lifetime actuarial fairness. This increases the marginal utility of retirement income relative to leisure. Individuals are likely to respond by voluntarily saving more and working longer, to restore their monthly benefits. If early pension eligibility is conditioned on benefit size, the constraint becomes more binding and postponed pensioning may be required.

In contrast, in actuarially unfair DB systems equation (4) does not hold and monthly pensions do not automatically fall when longevity increases. Instead, the EPV of lifetime benefits rises, leaving the relative utility of incremental income and leisure unchanged. Workers get the unchanged monthly benefit with no need to trade off current consumption or leisure, therefore no increase in retirement age. Chile fit this picture before 1981.

**Retirement rule changes in the Chilean reform and how they affect behavior**

The Chilean pension reform was a complex package, with many rule changes. Starting with a system that was decidedly unfair actuarially, that tied the work and pension decisions together and made early retirement easy, the reform moved toward actuarial fairness in all dimensions, eliminated pension payroll tax and work restrictions for pensioners and tightened early retirement pre-conditions. The new system was basically a mandatory saving scheme with constraints on pension age and amount.

*Moving toward actuarial fairness by replacing DB with DC.* The old Chilean system consisted of several sub-systems, with different details but similar general outlines. In the largest sub-system, Servicio Seguro Social (SSS), the payroll tax for pensions was 23%, with another 10% for other social insurance, bringing the total to 33%.[3] The monthly defined benefit was 50% of the base wage for the first ten years of service but only 1% additional for each year thereafter, until a 70% ceiling was reached, at which point the incremental benefit became 0. While some groups received positive cross-subsidies as a result of this formula, in general increasing contributions or postponing the pension did not raise the benefit commensurately nor did greater life expectancy decrease it. As in many other countries with traditional systems, workers had an incentive to start the pension and stop work as soon as they became eligible, and rising longevity did not lead them to postpone pensioning or work longer.

In the new system, each worker is required to contribute 10% of his or her wages to an individual DC account, plus another 2-2.5% for administrative fees and disability and survivors insurance, totaling about half the old rate.[4] Contributions are invested in a pension fund (AFP) of the worker's choice and accumulate a market rate of return. Payouts must take the form of price-indexed annuities purchased from insurance companies or gradual withdrawals over the person's lifetime, managed by the AFP. Most retirees purchase annuities, delayed annuitization increases the monthly pension by an equivalent amount, and annuities have had a money's worth ratio of 100% or more (James, Martinez and Iglesias 2006). Thus, the new system is actuarially fair, in the sense that incremental and total contributions as well as postponed pensioning are rewarded with equivalent expected real benefits. This increase in actuarial fairness might lead individuals to postpone pension age (hence remain non-pensioners longer) and non-pensioners to work and contribute longer, on a voluntary basis.

At the same time, the incentive toward postponed pensioning and longer work is limited by the fact that many individuals may not regard the new system as completely fair, because their own subjective discount rates for time or risk do not coincide with market rates. For example: a worker may be forced to save at a higher rate than he would prefer; he wishes to start his pension sooner than the retirement age rules permit; he wishes to use his saving to cover emergency or other consumption in the early years of retirement; he prefers to invest in different ways from those permitted by regulations;[5] or he may be in ill-health, hence does not expect to get back his full premium through annuities or gradual withdrawals. In general, restrictions on saving rate, investment choice and payouts in mandatory DC systems, combined with heterogeneous savings-consumption and risk-return preferences among workers, create a tax wedge even if they are supposedly actuarially fair. It has been estimated that, on average, a 50% implicit pension tax remains in Chile, continuing to depress work incentives.[6]

*Eliminating work restrictions and payroll tax for pensioners and for non-pensioners over 65.* In the old system, continued work in the public sector was not allowed once the worker started the pension. Private sector work was allowed, but often workers had to change jobs upon pensioning. Moreover, contributions had to continue so long as the individual worked, even if he had already pensioned and got little or no additional benefit. In contrast, in the new system pensioners can continue working, with no restrictions. They are totally exempt from the pension payroll tax after pensioning. Any contributions after that point are purely voluntary. Their nominal net wage rises by 14% (12.5/87.5) after pensioning (or by 7% if the implicit tax component was 50%). Non-pensioners are also exempt after the normal retirement age of 65 for men (60 for women). This makes the system totally actuarially fair to them, at the margin.

*Raising the allowable retirement age.* In the old system, normal pension age was 65 for men, 55-60 for women, but earlier retirement based on length of service, disability or for other reasons was common.[7] In the new system, normal pension age is 65 for men, 60 for women, and early pensioning was not permitted before 1988. Starting in 1988 it was allowed, but subject to tight restrictions. Until 2004 the retirement accumulation had to provide a benefit that was at least 50% of the worker's own average wage and 110% of the minimum pension guarantee (MPG), for pensioning prior to 65/60. Recently these were raised to 70% and 150%, respectively (Edwards and James 2006; James, Martinez and Iglesias, 2006). At the same time, the disability pathway to early retirement was tightened (James, Edwards and Iglesias 2009). [8]

*Automatic market-based adjustments to longevity.* In the old system, the monthly benefit was fixed over time, regardless of longevity increases. In the new system, the conversion of retirement savings accumulations into monthly annuities depends on competitive pricing by insurance companies, who take life expectancy into account. Studies show that their payments are approximately actuarially fair, in the sense that the EPV of lifetime benefits equals the EPV of lifetime contributions, when discounted at the risk-free market rate (James, Martinez and Iglesias 2006). This implies that monthly benefits are depressed as longevity increases.

**How these rule changes are expected to change behavior**

In the new system the decisions to start the pension and stop work were separated, so we must distinguish between these two dimensions of retirement. We expect to find the following effects on participation rates and pension age of older workers:

1. *LFP—Tax-exemption of pensioners.* The removal of restrictions on their right to work and their exemption from the pension payroll tax should increase the age-specific work propensity for pensioners (and for non-pensioners over 65). Equation (3) applies to these groups.

2. *LFP—partial actuarial fairness for non-pensioners.* The shift to DC may lead non-pensioners under age 65 to work longer, on a voluntary basis, because the implicit tax wedge has decreased so their net wage has increased. That is, we may observe a behavioral shift from equation (1) toward equation (3) for non-pensioners. However, this impact is likely to be smaller than that for pensioners, because any mandatory system

remains incompletely fair to many workers and because the *LFP* of non-pensioners is high to begin with, probably due to liquidity constraints. The work response to the reform should be larger for non-pensioners over 65, at which point they become completely exempt from the pension payroll tax.

*3. Postponed pensioning--voluntary vs. involuntary.* Actuarial fairness might lead individuals voluntarily to delay pensioning beyond the earliest eligible age, because they will get a return from postponement that was absent in the pre-reform system. But if a tax wedge (albeit smaller) continues to exist, many individuals will prefer to start their pension as soon as possible, as they did before.[9] At the same time, tighter early retirement pre-conditions may lead to involuntary postponed pensioning, even for those who want to start early. These pre-conditions do not apply after age 65. Thus, the pension postponement effect should be larger for those under 65 and any postponement after 65 must be voluntary.

*4. LFP impact of pension postponement.* Since non-pensioners have higher participation rates than pensioners, the rise in proportion of non-pensioners will further increase aggregate labor force participation rates.[10]

*5. Longevity growth—impact on LFP and pension age.* When we examine changes over time, we must also take account of falling replacement rates due to longevity growth in the context of lifetime actuarial fairness (equation 4). This raises the marginal value of income relative to leisure, makes it harder for workers to satisfy the early pension requirements, and should lead individuals increasingly to postpone labor force exit on a voluntary or involuntary basis.

**Previous empirical studies on the U.S. and other countries**

During the past decade a large empirical literature has developed to investigate the falling labor force participation rate among older men and the impact of pension system wealth and rules. The benchmark studies by Gruber, Wise and their colleagues (1999 and 2004) measure retirement incentives facing older men across eleven industrialized countries and through time. They show large work disincentives stemming from DB social security systems, and the greater these disincentives the less likely individuals are to postpone retirement. Borsch-Supan (1998) finds similar effects for Germany and other European countries. A recent study by Lluberas (2007) finds that workers in employer-sponsored DC plans in the UK expect to retire later than those in DB plans. Baker and Benjamin (1999) and Disney and Smith (2002) find that when private earnings diminish size of public benefits, the labor supply of older workers falls, in Canada and the UK, respectively. Butler, Huguenin and Teppa (2004) attribute the falling labor supply of older men in Switzerland to the maturing of generous mandatory pension plans, which increased their pension wealth. Workers spend this wealth on longer periods of retirement leisure.

Several studies use U.S. data to examine the impact of social security wealth, implicit tax incentives and early retirement rules. The early studies find small to modest effects that vary by income and marital status, and a discontinuous drop in labor force participation at the earliest and normal age of eligibility for pensioning (Hurd 1990, Fields and Mitchell 1984, Burtless and Moffitt 1986, Stock and Wise 1990, Krueger and Pischke 1992, Blau 1994, Gustman and Steinmeyer 1985, Anderson, Gustman and Steinmeyer 1999, Coile and Gruber 2001). Gruber and Orszag (2003) find negligible labor supply effects of the retirement earnings test (which reduced benefits for pensioners who earned wages). However, more recent studies, such as Song and Manchester (2007), Haider and Loughran (2008) and Engelhardt and Kumar (2009) find that the 2000 removal of the test for workers over age 65 increased work propensities substantially, especially among individuals in the higher income percentiles who were most affected.

Partly as a result, over the last decade participation rates of older workers have been increasing in the U.S, albeit to a much lesser extent and for a much shorter period than in Chile. Friedberg (2007) summarizes various explanations for this trend, including the elimination of the penalty for working while collecting social security benefits, the increased increment from delaying the start of benefits and the gradually rising normal retirement age. Maestas and Zissimopoulos (2010) suggest the changing educational, health and skill composition of the labor force as the primary reason why older individuals have begun to work longer, and notes changes in social security rules and the shift from employer-sponsored DB to DC plans as secondary explanations. Pang, Warshawsky and Weitzer (2008) also find that the shift from employer-sponsored DB to DC plans delays retirement in the U.S.

Most of these studies have had the advantage of longitudinal data, which we do not have. But the system incentives and responses they studied have not changed as dramatically as in Chile nor do they have such a long period of reaction time to observe the consequences.

## II. Data, Probit Model and Reform Indicators

To carry out this analysis, we draw on 47 years of annual household surveys, the *Greater Santiago Area Encuesta de Ocupación,* covering 2500-3000 households in the greater Santiago area, collected by the University of Chile since 1957, with our latest data from 2004. For all members of the household, we know their gender, schooling, marital status and number children, as well as their current wage, pension and other income. This is not a longitudinal or retrospective data set. Different individuals were observed in each survey. Our basic strategy is to construct the life employment and retirement histories of synthetic cohort members, assuming that the behavior of a given cohort, observed at different ages in successive cross-sections, proxies a panel, if we control for individual and time-specific characteristics. Our analysis focuses on the 31,547 men in 50 cohorts born between 1900 and 1950 and observed at ages 50 to 70 at some point between 1960 and 2004. The first 21 years observed were pre- and the second 24 years post-reform. We link these data to variables that measure macroeconomic conditions for each year of the survey.

This data set, however, has several shortcomings. First, we are unable to separate those eligible for pension benefits from those ineligible. We know that social security coverage is incomplete because only 70-75% of men in the 70-74 age range collect pensions.[11] Since individuals who don't belong to any system or have only marginal affiliation are unlikely to be affected by the reform, our inability to remove them from the data set leads to an underestimate of the impact of the reform among those who do belong, particularly among non-pensioners.

We also do not know explicitly whether covered individuals are in the new versus old systems. Our basic strategy is to use birth cohort as a proxy for probability of new system membership, and to analyze the change in work propensities among successive cohorts, who have increasing probabilities of new-system affiliation (Tables 1 and 2).

The 1981 reform placed new entrants to the labor force in the new system and gave existing workers the option to stay in the old system or switch. The propensity to switch to the new system was inversely correlated with birth age. By 1985 77% of all contributing workers were in the new system, including most workers under age 50, while the majority of older members were in the old system (Palacios and Whitehouse 1998; Acuna and Iglesias 2001; SAFP 2003; see Table 1). Starting with birth-year 1931, more than half of each cohort ended up in the new system. Among those born 1926-31, 40% of contributors were in the new system. Therefore, we define cohorts born before 1926 as "pre-reform" and those born after 1931 as "post-reform," with 1926-30 as the "bridge" generation, with a small expected reform effect in their later years. We expect the probability of new-system affiliation and new-system effects to rise with each successive cohort. We are modeling a transition from one system's steady state to another, looking for sequential differences in the behavior of younger cohorts until the probability of new-system membership approaches 100%. At this point, a new equilibrium will be reached.

We focus on behavior between ages 50 and 70. Individuals under age 50 are unlikely to pension and withdraw from the labor force. If they do so, the pensions are probably disability pensions, which have different behavioral models from old age pensions—but our data do not distinguish among benefit types. We stop at age 70 because we have few observations over 70 from new-system cohorts. We do not know the age at which individuals started their pension or initially withdrew from the labor force; we only know their status in the survey year. We also don't know their voluntary saving, so we can't distinguish those who could easily offset the new mandatory saving.[12] We confine the analysis to men, because many women receive survivors' or social assistance pensions, which face different incentive structures. Also, women are more likely to be subject to the minimum pension guarantee, which increases the incentive to work until they qualify and decreases it thereafter—but we do not have information about their eligibility.[13] We examine changes over time, for which the University of Chile data are ideally suited because they have been collected in similar form for half a century.

**Summary of aggregate data**

As a first descriptive step, we look at the aggregate data without controlling for individual or macroeconomic effects. Figures 1-5 summarize the aggregate picture and Figures 6 and7 compare this with other countries for the same period.

(1) *Labor force participation by age and cohort.* In prime age, all cohorts born 1916-1950 had very high labor force participation rates--exceeding 95%--that remain high after the pension reform. Participation rates fall after age 50 for all cohorts. But they fall much more slowly after the reform for each successive post-reform cohort (Figure 1).

(2) *Aggregate participation by age over time.* Consequently, aggregate labor force participation of older men gradually rises through time after the reform, contrasting with the decline before. This effect is absent for prime-age men (Figure 2).

(3) *Participation rates of pensioners versus non-pensioners.* Participation rates are much lower among pensioners than non-pensioners. However, the increase after 1981 is concentrated in pensioners, so the gap in work propensities of these two groups narrows over time (Figure 3). (For example, among pensioners age 55 to 59, participation rates trebled between 1982 and 2004, from 14% to 42%, while work propensities among non-pensioners were fairly stable, at 90-94%).

(4) *Participation rates of pensioners versus non-pensioners for pre- versus post-reform cohorts.* For similar reasons, age-specific participation rates are higher for post-reform than pre-reform cohorts, especially among pensioners (Figure 4).

(5) *Pension probabilities by age over time*. The probability of being a pensioner before age 65 falls dramatically after the reform. Pension probabilities for men age 50-64 were at a 40-year low in 1997-2004—ranging from 10% in their early 50's to 30% in their early 60's--while pension probabilities for men over 65 were close to their historic highs—over 60%--evidence of postponement, rather than reduced coverage (Figure 5).

(6) *Participation rates in Chile compared with other countries.* For comparison, between 1970 and 2000 labor force participation rates of men aged 55-64 declined in practically every high-income OECD country, with average declines of 22% (Figure 6, see Casey et al 2003; also see OECD 1998 which gives similar results for 1980-96). This trend has been mainly attributed to social security rules that allowed them to retire increasingly early with generous pensions (see previous discussion of literature). Pension coverage is much lower in Latin America than Europe because of the prevalence of rural and informal sector workers, so we would expect aggregate participation rates to be less responsive to pension system rules, and indeed they are roughly stable over this period. In contrast to both sets of countries, participation rates in Chile for ages 55-64 rose steadily post-reform—by 17% for the nationwide data and 22% for the data set used in this analysis, which is based on urban workers (Figure 7).[14]

Thus, prior to 1981 participation rates of older men in Chile were dropping, but starting in 1981 they have been rising. This effect is concentrated in pensioners. Also since 1981 the probability of becoming a pensioner before age 65 has declined. At the same time, participation rates were falling in countries with high coverage in DB plans that encouraged early retirement. This paper analyzes the connection between the 1981 pension reform in Chile and these empirical observations.

**Probit model of retirement and pension probabilities**

*LFP.* We use probit analysis and simulations to estimate retirement probabilities for older individuals. Our main dependent variable is the individual's labor force participation rate ($LFP_i$), which we model as a function of reform indicators based on cohorts ($R_c$), controlling for time-specific macro-economic characteristics ($E_t$), personal and family characteristics ($X_i$), pension status ($P_i$) and benefit size ($B_i$)—variables that influence the person's potential wage and willingness to trade-off wage for leisure.

Reform indicators are the main variables in our empirical analysis. Since we do not explicitly know the individual's system affiliation, our basic strategy is to proxy this probability according to his birth cohort. Earlier cohorts arrive at the age range we are examining before the reform, while later cohorts have an increasing probability of being in the new system. The age composition of our sample varies by cohort and the cohort mix varies by age (Appendix Tables A1 and A2). We therefore expect different age groups to be at different points along the transition path by the last year in our sample, 2004. Moreover, we do not expect uniform reform effects for all cohorts at a given age, since later cohorts will have had more time to adapt to the reform in their life cycle planning. For these reasons, we measure discrete LFP shifts for cohort groups, and these shifts are allowed to vary by age-range.[15]

To capture the effect of reform on the behavioral relationship described above, we construct a set of dummies that identify each individual observation according to one of seven birth-cohorts $Cohort_k$ and one of three age-groups ($G_j$). To compare reform effects on pensioners and non-pensioners, we interact the $(Cohort_{ki})(G_{ji})$ and $P_i$ terms. In our reduced form probit model predicting LFP:

$$Pr(LFP_i=1 | R_{ci}, G_{ji}, P_i, B_i, E_{ti}, X_i) =$$

$$\Phi\left(\sum\left(Coh_{ki}G_{ji}\ \delta_{kj}\right) + \sum\left(Coh_{ki}G_{ji}\ P_{kj}\lambda_{kj}\right) + P_iB_i\omega + E_{ti}\gamma + X_i\ \beta + \xi_i\right) \quad (5)$$

We are particularly interested in $\delta_{kj}$ and $\lambda_{kj}$, which measure the effect of the pension reform on pensioners and non-pensioners of different cohorts (k) and age groups (j).

*Pension probabilities.* As a subsidiary point, to see whether pensioning has been postponed by the reform, we also estimate the probability of being a pensioner:

$$Pr(PEN_i=1 | R_{ci}, G_{ji}, E_{ti}, X_i) = \Phi(\sum Coh_{ki}G_{ji}\tau_{kj} + E_{ti}\pi + X_i\alpha + \varepsilon_i) \quad (6)$$

There is a basic difference between the LFP and PEN equations. Pension describes an on-going absorbing status. Once a person pensions, he stays pensioned; it is a once-and-for-all decision. Our data do not tell us when this decision was made or the characteristics of the worker at that point. We simply observe the current pension status, which is mainly determined by past decisions or constraints, and the PEN equation informs us of the current characteristics that are correlated with this status.

In contrast, the labor force participation decision is remade each month or year. Being out of the labor force in period 1 increases the likelihood of being out in period 2, and this likelihood and inter-period correlation increase with age, but nevertheless, individuals move in and out of spells of employment and non-employment past age 65 (See Meghir and Whitehouse 1997 for an analysis of detailed British data on this subject). Consequently, the LFP equation on which this paper focuses models current LFP decisions, with current pension status and amount taken as pre-determined variables.

*Simulating reform effects for different sub-groups.* We are particularly interested in measuring differences in reform effects across sub-groups, that is, in the interaction between the cohort, age and pension status of the individuals observed. Non-linearities make it inappropriate to use dprobits for this purpose. Therefore, we interpret our results using simulation of representative people in each sub-group (see Allison 1999; Berry, Esarey and Rubin 2007; Norton, Wang and Ai 2004; Tomz, Wittenberg and King 2003). We first estimate our probit models and use these coefficients to simulate the pension and labor force participation probabilities for individuals in different cohort-age-pension status groups, using a STATA routine.[16] We report coefficients for a complete set of variables in the estimated probit equations in the Appendix and the simulated reform effects—our focus--in the text.

**Reform indicators—cohort-age groups ($Cohort_k G_j$)**

Specifically: we collapse the 50 separate cohort birth years into seven broad cohort groups and represent each group by a dummy variable: ($Coh_0$ *to* $Coh_6$). Variations by age are captured by 3 age groups: 50-59, 60-64, 65-70. Table 2 defines each age-cohort group and their likely reform impacts.

$Cohort_0$, born 1900-1915, is the omitted control group. $Cohort_1$, born 1916-25, is the pre-reform cohort that was mainly affected by the old system. $Cohort_2$, born 1926-30, is the bridge generation, which was exclusively in the old system at age 50 but had a noticeable minority of new system members by age 60; we expect a small reform effect in their 60's. The next two cohort groups, ($Cohort_3$ and $Cohort_4$), born 1931-35 and 1936-40, were closely divided between the two systems and should exhibit significant reform effects, starting in their 50's and growing stronger with age as they learn and adapt. The final two cohort groups, ($Cohort_5$ and $Cohort_6$), born 1941-44 and 1946-50, were mainly in the new system so should display stronger and younger reform effects—but we observe them mainly in their 50's. Since our data end in 2004, only $Cohort_0$-$Cohort_4$ are fully observed in their 60's (see Tables A1 and 2). When a new equilibrium is reached, with full new-system membership, marginal effects will cease for successive cohorts, but the cumulative effect on behavior should remain.

**Impact of Pension status and amounts on LFP and reform effects ($P_i$ and $B_i$)**

*Pension status.* For reasons given above, we expect that pensioners will have 1) a lower LFP rate but 2) a more positive reform effect on LFP than non-pensioners. In some LFP specifications we interact the age-cohort groups with pension status, and we also run separate regressions for pensioners and non-pensioners, expecting to find the largest reform coefficients for pensioners. The estimated reform impact in specifications with no pension controls combine two effects: the increasing proportion of non-pensioners, who have relatively high participation rates, and the rising participation rates of pensioners in post-reform cohorts. The specifications with pension controls capture only the latter.

*Benefit amounts.* Rising benefits and social security wealth have been given as one explanation for falling participation rates of older workers during the 1970's and 1980's in the U.S. and Switzerland. Real pension amounts received by Chileans plummeted during the early 1970's due to hyper-inflation without indexation, but they recovered steadily from the late 1970's on, in part due to wage growth. As a result, inflation-adjusted pensions of individuals in post-reform cohorts are higher than those for pre-reform cohorts. We use the actual benefit amount for each individual (pre-determined when the person pensioned) in our pensioner LFP equations and we expect a positive coefficient. This would have decreased pensioner LFP during the 1980's and 1990's in Chile--leaving a larger explanatory gap for the rising participation rates observed.

*Wage replacement rates.* But wages increased even more rapidly than pensions, therefore replacement rates moved downward. This was due to many factors, including longevity improvement combined with actuarially fair conversion to annuities. Then, if individuals want to smooth their consumption, they might choose to save more and work longer. Additionally, falling replacement rates may mean that some individuals do not meet the threshold for early pensioning, must remain as non-pensioners, and continue working because of liquidity constraints. Our sample does not include data on prior wages of individuals, so we could not include the individual's own replacement rate as a variable. However, we created a pseudo-replacement rate for pensioners by taking the ratio of individual i's pension to mean wage for i's cohort and schooling group in the age range 40-49. For reasons given above, we expect a negative coefficient, so the fall in pseudo replacement rates after the reform may help explain the observed increase in participation rates.

**Macro-economic and individual characteristics**

Many other changes were taking place in the Chilean economy and society over the period studied, including severe macro-economic cycles, labor market development and dramatic increases in education and household income. To make sure we are not confusing the reform effect with other changes that were occurring over the period studied, we control for a number of time-specific macro-economic variables and individual characteristics whose average values changed over time. The former include unemployment rate and deviations from Hodrick-Prescott trend (which measures cyclical variations). The latter include individual's education level, household income per capita minus individual's own wage and pension, marital status, labor market status of wife, spousal age differential and number of children.

*Macro-economic variables.* During the post-reform period, unemployment went through an entire cycle, starting with 23% in 1982, falling to 6% by 1995 and then rising to 14% by 2002. The existence of a complete business cycle from the mid-1980's through 2004 is important as it reduces the correlation between the phase-in of the reform and economic growth, and allows us to separate out these effects. We expect pension probabilities to rise and LFP to fall during cyclical downturns in the economy, due to the discouraged worker effect, the difficulty older workers may experience in obtaining new jobs when laid off from their existing jobs, and the greater ease of meeting early retirement pre-conditions.

*Individual characteristics.* The individual-level variables help explain differences in pension and work propensities and also control for socio-economic changes that occurred over the post-reform period. Major educational gains took place in Chile between $Coh_0$ and $Coh_6$.[17] On the one hand, higher education exerts a lifetime wealth effect that might lead to earlier withdrawal from the labor force; but on the other hand it also has a positive substitution effect on work propensities, by raising the pecuniary reward for work and experience, as well as access to jobs that are more interesting and less physically demanding. More educated individuals are more likely to satisfy the early pension eligibility constraints, which enables them to stop work sooner with a pension if they want to do so. Partly as a result of higher education levels, r*eal per capita household income* rose substantially—trebling from $\text{Cohort}_0$ to $\text{Cohort}_6$. It has been argued that long-term income growth is a major reason behind the decline in pension age and labor supply and the increase in retirement leisure of older male workers in other countries. *Female labor force participation* increased even more sharply than that of men over this period (in part due to the higher educational levels of women), and this may influence the pension and work choices of husbands, who wish to coordinate retirement plans with their wives. The direct impact of these macro-economic conditions and individual characteristics on pension probabilities and LFP are important per se. But most important for our purposes, by controlling for them we are left with the pension reform as the major explanation for any remaining changes in retirement behavior observed across cohorts during this period. (See Table 3 for definition and means of covariates).

## III. Reform Impact on Age-Specific Pension Probabilities

Table 4 presents our simulations of marginal changes in pension probabilities for each cohort and the cumulative post-reform effect for each age group. Tables 5 and 6 present our simulations of reform effects on *LFP* rates, with and without controlling for pension status. We expect all these reform effects to grow for successive post-reform cohorts with a larger proportion of new-system members. In each case we test whether the reform effects are greater or smaller for individuals over age 65 (see Appendix Tables A3-5 for underlying probits).

**Pension postponement for successive cohorts**

As expected, we find that post-reform cohorts postpone pensioning. For ages 50-64, the probability of being a pensioner declines by 2 to 6 percentage points per successive cohort-group, starting with $Cohort_3$ (Table 4). By $Cohort_6$, the last cohort observed in age group 50-59, the cumulative post-reform reduction in pension probabilities totals 10 percentage points. By the last cohort observed in ages 60-64, it totals 8 percentage points—but for this age range we don't yet have full data for $Cohort_5$ and $Cohort_6$. These reductions are large, compared with the estimated pre-reform ($Cohort_1$) pension probabilities of 21% and 37% for these respective age groups. An annual reduction of 10 percentage points for 15 age-years adds up to about 1.5 years of postponement, for the latest cohort.

**Incentives vs. constraints: significant but smaller changes after age 65**

Postponed pensioning could be caused by tighter early retirement constraints or by voluntary choice under greater actuarial fairness, which made workers more willing to save through the pension system. We cannot directly separate these two sources for individuals under age 65. However, since everyone is eligible to start their pension at age 65, we expect to find a smaller reform impact after 65 and any further decline would be attributable to incentives, not constraints. In fact, for ages 65-70, our estimations show no significant marginal or cumulative reductions in pension probabilities for post-reform cohorts. This suggests that the reduction at earlier ages is a temporary postponement rather than a permanent decline and tighter early retirement constraints are probably the dominant reason.

**Understated effect of actuarial fairness**

However, we may be underestimating the incentive impact of actuarial fairness on pension age, for several reasons: First, many individuals who were not pensioned by age 65 are not in any pension system and therefore are not subject to its incentives and constraints. If these non-affiliates were excluded from the sample, we might find a larger change in behavior for those who remained—the system members. Second, for the over-65 age group we do not yet have evidence from $COH_5$ and $COH_6$. Additionally, older people are likely to have a stronger preference to dissave and consume even in an actuarially fair system, because their remaining lifetime is scarcer and their accumulated savings are larger. Thus, even if postponement stopped at age 65, this does not mean that actuarial fairness played no role for those under 65.

Also, actuarial fairness with respect to longevity has probably made the early pension constraint more binding over time. According to Chilean mortality tables constructed in 1985 and 2004, life expectancy for males in their 60's increased by 2 years over this period. Our annuity calculations indicate that this would have decreased replacement rates by 4.3% in an actuarially fair system. Eligibility for early retirement depends on replacement rates. By 2004, ceteris paribus, men close to the early retirement threshold would have had to postpone pensioning and work for another year—over half the average postponement—just to make up this amount and pass the threshold.[18] This should show up even more in future surveys.

**How do macro-economic and individual co-variates affect these results?**

Could the decreased pension probabilities be caused, in part, by changing macro-economic or demographic conditions? A higher *unemployment rate* significantly increases pension probabilities (derived from Table A3). Thus, the 15 percentage point drop in unemployment rates during the mid-to-late 1980's might account for part of the decrease in pension probabilities—but the 8 percentage point rise during the late 1990's would have had the opposite effect. Having *6 or less years of schooling* or having many *children* have highly significant negative coefficients, so the sharp decline in the values of these variables would have increased pensioning—but instead a decrease occurred. *Having a spouse who worked* also has a negative impact on pension probabilities; thus the growth in female labor force participation may have contributed to the observed pension postponement. Nevertheless, the strong negative impact of the reform on pension probabilities (i.e. the decline across cohorts) remains, after controlling for these macro-economic and time-sensitive individual-specific co-variates.

## IV. Reform impact on LFP rates

### Pensioners and non-pensioners pooled

Table 5 presents our simulations of reform effects on *LFP* rates, without controlling for pension status. It shows the marginal reform impact for each successive cohort relative to the preceding cohort and the cumulative post-reform effect, stemming from postponed pensioning plus increases in work propensities among pensioners and non-pensioners, pooled. This captures all sources of increased participation—exemption of pensioners from the pension payroll tax, changing behaviors due to actuarial fairness, and increased proportion of non-pensioners as early retirement rules tighten.

The negative coefficient on $Coh_1$ indicates that participation rates of older workers were declining in pre-reform years, as elsewhere in the world. But starting with $Cohort_2$ in their late 60's, $Cohort_3$ and $Cohort_4$ in their 50's, this decline stops and we observe significant marginal increases in LFP of 2 to 7 percentage points per successive cohort group in each age range. Moreover, once a cohort starts working more it continues working more, by even larger amounts, as it ages. Cumulative gains total 12-19 percentage points by the last observations in ages 50-64 and 13 percentage points for ages 65-70. Since marginal coefficients for the last cohort observed continue to be positive, a steady state may not yet have been reached and the totals may increase further.

The strong post-65 *LFP* effect contrasts with the weak post-65 effect on pension probabilities. Despite the likelihood that willingness to trade off leisure for income will diminish with age and despite the fact that pensioning is no longer postponed, older workers apparently respond to work incentives in the new system--the exemption of pensioners and of non-pensioners over 65 from the payroll tax.

**Reform impact on participation rates of pensioners vs. non-pensioners**

Table 6 controls for pension status and amount and allows cohort effects to differ between pensioners and non-pensioners, while constraining other co-variates to be the same. This approach has the advantage that it allows us to test whether the reform had different effects on these two groups, but the disadvantage that it introduces the possibility of selection bias between pensioners and non-pensioners and eliminates the participation effect that operates through the increased proportion of non-pensioners. (We also ran separate regressions for pensioners and non-pensioners and the marginal reform effects by cohort were essentially the same as in Table 6; we present the cumulative effects for each age group for both approaches). Since pension status is given, this table does not include the impact of changing proportions of pensioners and non-pensioners. Therefore the weighted average of effects for pensioners and non-pensioners in Table 6 should be less than the pooled effects in Table 5.

It is most useful to look at the changing behavior of pensioners and non-pensioners before and after age 65. Prior to 65 we expect the reform impact to be stronger for pensioners, because of the complete elimination of the pension payroll tax and the removal of work restrictions, while any impact on non-pensioners would stem from the increased actuarial fairness of the system, which is incomplete and less transparent. After age 65 non-pensioners too are exempt from the pension payroll tax. Consequently, they should behave more like pensioners at older ages.

Starting first with pensioners: their participation rates rise substantially, by 5-16 percentage points for each successive cohort, in each age range. This process starts with $Cohort_2$ in their late 60's, $Cohort_3$ in their early 60's and $Cohort_4$ in their 50's, as later cohorts had an opportunity to affiliate with and adapt to the new system at earlier ages. By the last cohort observed in ages 50-64, LFP rates for pensioners had risen 33 percentage points—more than doubling their pre-reform work propensities.[19] Since marginal gains are still large for $Cohort_5$ and $Cohort_6$, a new steady state has probably not yet been reached and the total increase may eventually be greater. By comparison, participation rates were falling strongly for pre-reform $Cohort_1$. A sharp turn-around in participation rates of pensioners obviously occurred after the reform.

Work propensities also rise, but by a smaller amount—a cumulative 17 percentage points-- for pensioners over age 65. The smaller increase may be due to the stronger preference for leisure time among older individuals, or to the fact that $Cohort_5$ and $Cohort_6$ haven't yet reached these ages. In contrast, the marginal impact of the reform on the LFP of non-pensioners shows up only after age 65. The cumulative post-reform impact on non-pensioners is 1 percentage point in their 50's and 5 percentage points in their early 60's, but reaches 11 percentage points after 65. Only the latter is significant when we keep pensioners and non-pensioner in the same regression but when we run them separately all these effects are significant, albeit small before age 65.

Thus, the evidence points toward a much stronger reform impact on pensioners than non-pensioners, and a larger impact on non-pensioners after age 65 than before. How can we explain these results? The incentive from the total elimination of the pension payroll tax is easy to evaluate, while the incentive from the DB-DC shift for non-pensioners under 65 is smaller and more difficult for workers to figure out. Non-pensioners are already working at very high rates to meet their consumption needs so there is not much space for further increases. (The 5 percentage point increase in work propensities in their early 60's actually amounts to a 42% decrease in labor force non-participation). It is also likely that we have underestimated the reform impact on non-pensioners who are affiliated to the system, because these data do not separate them out from individuals who are not affiliated.

A DB-DC shift alone might have elicited a greater response from pensioners than non-pensioners because their initial work propensities are smaller. Unfortunately, Chile's policy design does not allow us to test this effect since it is superseded by pensioners' exemption from the payroll tax. However, their dramatic response to the tax exemption (total actuarial fairness) suggests that they would likely have responded, by a lesser amount, to the DB-DC shift (partial actuarial fairness), if that were the only change. In sum, we have evidence that exemption from the pension payroll tax has a strong positive impact on work and the shift from DB to DC has a weaker positive impact. To pin this down further would require more information about the system affiliation of non-pensioners and the behavior of pensioners when exposed to the DB-DC shift alone.

**Direct impact on LFP of pensioner status, benefit amount and replacement rate**

We also examine the direct impact on LFP of pensioner status and benefit amount. We expect that pensioners would be less likely to work than non-pensioners in both systems because of access to pension wealth, hence fewer liquidity constraints. Indeed, pensioners work 66 percentage points less than non-pensioners in $\text{Cohort}_0$, but this gap is cut in half for ages 50-64 by behavioral change after the reform (Table 6).

To assess the impact of benefit size we used, alternatively, pension amount and pseudo replacement rate in our specifications; results were similar. As expected, the larger the pension amount, the lower is the work propensity, due to an income or liquid wealth effect. But if workers seek consumption-smoothing, the replacement rate is more relevant. A higher pseudo replacement rate reduces the *LFP* rate strongly and significantly (Tables A4 and A5). This is indirect evidence that, if replacement rates fall over time due to longevity growth post-reform, the *LFP* of pensioners will rise.

**Could the large reform effect on pensioners be due to selection/composition bias?**

Could the large response by pensioners be due to selection bias—a change in composition among pensioners toward those with higher work propensities and vice versa for non-pensioners after the reform? The old system granted early retirement status liberally, including to many with low work propensities. In contrast, the new system conditions access to early pension on meeting a fund accumulation-replacement rate threshold, which is more likely to be achieved by those who contributed persistently—because they have higher work propensities. In the old system some individuals with a high propensity to work had to remain non-pensioners in order to keep their job, while in the new system these individuals can become pensioners and continue to work. All these effects could raise average participation rates among post-reform pensioners compared with pre-reform pensioners due to selection bias into pensioner status.

However, our findings are not consistent with selection or composition as the major explanation. If the shift of individuals with high work propensities into pensioner status were the main reason for the rising work proclivities of pensioners, it follows that LFP would have fallen for non-pensioners, it would not have continued increasing after age 65 when everyone can become a pensioner, and the reform would not have had a positive impact over-all. On the contrary, we find that non-pensioners remain more likely to work than pensioners and they work somewhat more than they did before the reform. Labor force participation also increased both for pensioners and non-pensioners over 65. Finally, the increase in post-reform LFP rates remains (in fact, is larger) in the pooled equation when pension status is not controlled. This is consistent with the view that we are observing behavioral change due to new incentives and constraints, rather than simply a rearrangement of people with unchanged behaviors.

**Does the reform effect hold with individual and macro-economic controls?**

We include a long list of macro-economic and individual-specific co-variates, whose average values change over time, in these models. *Unemployment rate* has a significant negative effect when pension status is not controlled, which becomes insignificant and less negative when pension is in the equation. This implies that the negative unemployment impact on work propensities is largely due to its positive impact on pension probabilities. *Deviations from H-P trend* have a significant positive effect on LFP rates; work propensities increase during cyclical upswings. Since both upswings and downswings occurred after the reform, neither of these variables account for the monotonic change in LFP rates over time and by cohorts that we observe.

In almost all cases, the coefficients of the individual-level co-variates (education, household income, marital status, spousal age differential, number children, in Tables A3-5) are significant and consistent with our expectations; but they do not erase the reform impact, either through their direct effect or through the changes in their average values over time. The higher education growth and rising female labor force participation rate in Chile over the past two decades probably account for part of the observed rise in aggregate LFP rates of older male workers. But other co-variates, such as higher household income, had counteracting effects. Most important for this analysis, the reform impact remains large and significant when these co-variates are in the equation.

## V. Reform Impact on Labor Supply, Human Capital and Growth

### Impact of higher LFP and pension postponement on labor supply and income

By the last cohort-group in our sample, $Cohort_6$, the total increase in labor force participation rate relative to pre-reform $Cohort_1$ for ages 50-64 is estimated to be 13.8 percentage points (based on Table 5).[20] A 13.8 percentage point increase in annual *LFP* for 15 years between ages 50-64 would raise expected lifetime work by 2.1 years. Given their average pre-reform 73% participation rate, this constitutes a 19% increase in the labor force of this age group (or 4% of the labor force as a whole if this age group is about 20% of the entire labor force). Among pensioners, *LFP* for ages 50-64 has gone up by 33 percentage points, raising expected lifetime work by 5 years—more than doubling their pre-reform expected work time (Table 6). We don't yet know the full effects for those over 65, but it appears that positive increments will continue into this age range. These extra years of work, of course, raise lifetime income and consumption commensurately. For example, if the incremental 2.1 years of wages earned between ages 50-64 were paid out gradually between ages 50-83, this would raise the person's annual income over this age range by 11% of his wage level.[21]

In addition to the decision to work longer is the decision to start the pension later. In age group 50-59, the probability of becoming a pensioner has fallen by 10 percentage points for $Cohort_6$ relative to pre-reform $Cohort_1$. The reduction is somewhat less at 60-64, but this figure will probably grow as later cohorts enter this age group. A 10 percentage point annual reduction in pension probability for 15 years implies, on average, a 1.5 year postponement in start of pension. Abstracting from the higher *LFP* (already measured above), postponing the start of pension constitutes a decision (or requirement) to consume the same present value of retirement savings over a shorter time period. This keeps the stock of saving higher than it would be otherwise and increases the average monthly pension by about 10%.

If retirement saving would have replaced 50% of the person's wage (the most common replacement rate in Chile) without these changes in *LFP* and pension age, it replaces 50% + 11% + 10%/2 = 66% afterwards. To achieve this same increase in monthly benefits without changing participation rates or pension age would have required a 3 percentage point rise in the pension payroll tax. To the degree that the incremental work and saving is a matter of voluntary choice under the new incentives rather than an involuntary adjustment to the new early pension rules, the increased retirement income exceeds the value of the leisure and earlier consumption foregone. And the treasury's balance improves if some of the incremental income is captured in taxes. All these effects will grow larger as the proportion of older workers in the population rises and as a new steady state is reached for later cohorts who are fully in the new system.

**Decomposition of higher LFP: increased work propensities vs. more non-pensioners**

To approximate how much of the total increase in *LFP* is due to increased work propensities of pensioners and how much to the increased proportion of non-pensioners, we compare the reform impact of the models with and without pension controls. For example, in age group 50-59 pooled *LFP* rose by 11.7 percentage points; this captures both forces on *LFP* (Table 5). At the same time, the corresponding increase was 33.6 percentage points for pensioners alone and 1 percentage point for non-pensioners (Table 6). Since 19% of our pre-reform sample in this age group were pensioners, if this proportion had remained constant the total post-reform increase in participation rates would have been 33.6*.19+1*.81 = 7.2 percentage points. Apparently 7.2/11.7=61% of the total estimated pooled increase is due to the payroll tax exemption and removal of work restrictions on pensioners. The remaining 39% is attributable to pension postponement and the increasing proportion of non-pensioners that occurred post-reform. A similar calculation shows that 78% of the total *LFP* increment at 60-64 and virtually the entire increment at 65-70 were due to increased work propensities, mainly of pensioners. Thus, increased work by pensioners is the primary force for higher *LFP*, particularly among the old-old, but pension postponement also plays an important role among the young-old.

**Impact on human capital**

While this paper has quantified the impact of the new system on the quantity of labor, one can identify several channels through which the quality of labor and human capital have likely been increased by the pension reform. Older workers carry with them more experience, hence higher productivity. The expectation of longer working lives and the reduction in implicit payroll tax should lead middle-aged workers and their employers to invest more in on-the-job training. Recent research suggests that delayed retirement may slow down cognitive decline among older individuals (Rohwedder and Willis 2010).

Previous theoretical research, using a human capital-based endogenous growth model, demonstrates that the move from a DB to DC system should stimulate growth through its positive impact on incentives for fertility and education (Ehrlich and Kim 2005, Ehrlich and Kim 2007, Ehrlich and Liu 1998). Reducing the (implicit plus explicit) pension payroll tax and raising *LFP* increases the family's incentive to invest in schooling. From this human capital perspective, while past education of current workers helps determine *LFP* today, education of their children, the future workers, is influenced by parental expectations about *LFP* and labor market returns tomorrow. This would be a very long run inter-generational effect of the reform. In the case of Chile, education levels rose dramatically over this period. Virtually all primary school-age but only 57%

of secondary school-age children attended school in 1980. Secondary education coverage rose to 77% in 1990 and 95% in 2009. Even more, higher education coverage trebled, from 13% in 1980 to 40% in 2009 (Ministerio de Planification, CASEN). These young individuals, with their incremental education, are now in the labor force or will be shortly. While many demand and supply side forces, including income growth and education sector reform, helped account for this steady increase, the pension reform added substantially to the labor market rewards for more education. All these factors contributed toward Chile's sustained growth rate of over 4.8% per year (compared to just 1% for the Latin American and Caribbean region during the same period).[22]

## VI. Conclusion

Many countries with aging populations are seeking ways to increase work propensities and delay pension probabilities of their older workers. Chile is an unusual case in that the labor force participation rates among older individuals have been increasing and pension probabilities decreasing for almost 30 years. This increases the labor supply, the stock of human capital embodied in experienced workers and the incentive to acquire more education and training. We argue that Chile's unusual *LFP* trend is mainly due to incentives and constraints stemming from its new social security system. The comprehensiveness of these system changes and their long duration afford us a unique opportunity to investigate the labor market impact of pension reform.

Pre-reform Chile looked like many European countries today—with early and declining age of pension and withdrawal from the labor force. These trends were sharply reversed and the labor supply of older workers, especially pensioners, began a sustained rise in 1981, while participation rates of prime age workers and non-pensioners under age

65 changed relatively little. The pension reform was the big event at that time that could have affected the labor supply of older more than prime-age workers and of pensioners more than non-pensioners. Two competing explanations--macro-economic variations over time or selection between pensioners and non-pensioners—fail to yield results that are consistent with this pattern.

The expected impact is very sensitive to details of the reform:

1) Pensioners had greater incentives to continue working because the new system exempts them from the pension payroll tax and eliminates work restrictions that existed previously. These results should be strongest because tax exemption is immediate, transparent and easy to calculate.

2) The shift from DB to DC decreased the implicit tax for non-pensioners, hence increased their net wage. Therefore, non-pensioners should also work longer, although this effect should be weaker than that for pensioners. Incentives for non-pensioners over 65 are similar to those of pensioners, since they are also exempt from the payroll tax.

3) Actuarial fairness may lead individuals to postpone pension age, on a voluntary basis. Furthermore, tighter early pension pre-conditions constrain more individuals to remain non-pensioners. These early pension pre-conditions will become more binding over time as longevity growth dampens monthly benefits. This constraint ends at age 65. Postponed pensioning is likely to involve longer work because of liquidity constraints.

Repeated household surveys collected by the University of Chile for over half a century enable us to study long term trends in participation rates and how they changed after the pension reform, using the person's birth cohort as a proxy for the probability of new-system affiliation. In particular, we exploit the fact that different subgroups face different incentives and constraints that should lead to different behaviors. Our results underscore that the labor supply of older workers responds strongly to tax incentives.

This is consistent with other recent work on older workers, and contrasts with the inelasticity of labor supply of younger workers found previously. Younger workers must work to consume and the labor market does not make it easy for them to marginally adjust their hours of work. Older workers have access to an alternative income source—a pension—and can withdraw from the labor force completely. We estimate a cumulative increase of 11.7 percentage points in participation rates of cohorts that recently entered the 50-59 age range and 18.8 points for ages 60-64, relative to pre-reform rates.

Our results imply that the complete payroll tax exemption for pensioners has a stronger impact than the shift from DB to DC had for non-pensioners. This is not surprising given that non-pensioners start out with much higher participation rates than pensioners and the DB-DC shift implies partial actuarial fairness while the tax exemption implies complete actuarial fairness for marginal work. Postponed pensioning also raises aggregate *LFP*, as more individuals remain in the non-pensioner group.

We have probably understated the impact of the DB-DC. Non-pensioners in the system might be shown to have a larger reaction if they could be distinguished from those who are not in any formal system. The potential effect on pensioners is superseded by their exemption from the payroll tax; their strong response to complete fairness suggests there would probably have been some response to partial fairness if that were the only policy change. The system may be more actuarially fair to certain sub-groups, such as individuals who have their own voluntary saving that can offset the mandated saving, than to others who are at a corner solution with no saving because their discount rate exceeds the market's—but we cannot identify these sub-groups with these data.

In later work we plan to utilize a different data set that provides more individual-level information on system affiliation, type of pension and voluntary saving, to explore these differences among sub-groups. By contrast, the current data set provides us with a long time perspective and avoids issues of selection between systems and between the formal and informal sectors. Also remaining for future work is an analysis of the impact of the pension reform on improved quality of labor and human capital in Chile—longer working lives and lower implicit and explicit taxes on labor should lead to greater willingness to invest in growth-enhancing education, on-the- job training and other forms of human capital, for oneself and ones' children.

These results have important policy implications. If payroll taxes for prime-age men are considered relatively non-distortionary because their labor supply is inelastic, this does not hold for older workers. Actuarial fairness for incremental contributions by older non-pensioners, by definition, implies a DC-type scheme.[23] But the other policies that we have focused on could be adopted whether the underlying social security scheme is DB or DC. Indeed, countries with DB, notional DC, flat or minimum pension schemes have recently adopted some of these policies, as noted below. The response to Chile's new system suggests that the labor supply of older workers and their incentives to invest in themselves at earlier ages can be increased substantially by:

1) Raising the net reward that older individuals receive for working and making these rewards easy to perceive--for example, by exempting workers from the pension payroll tax after a specified age or pension threshold (as in Chile) or removing earnings tests for benefits (as in the U.S.) or increasing benefits at a faster rate for years worked after a threshold retirement age (as Norway is considering for its minimum pension);

2) Raising the allowable age for pensioning (as in New Zealand);[24] and/or

3) Tying pension age or benefit amount to life expectancy, either directly (as done recently in Germany, Finland and Japan) or indirectly via the actuarially fair conversion of accumulated contributions to benefits (as in Sweden), while conditioning early pension age on benefit size or replacement rates (as in Chile).[25]

**Table 1: Percentage of contributors and pensioners in new system**

| Cohort number | Cohorts year of birth | % contributors in new system, 1985[i] | % contributors in new system, 2000[ii] | % pensioners, age 50-70, in new system, 2002[iii] | % male pensioners, age 50-70, in new system, 2002[iii] |
|---|---|---|---|---|---|
| **$Cohort_0$** | 1916-25 | 28 | n.a. | n.a. (>70) | n.a. (>70) |
| **$Cohort_1$** | 1926-30 | 40 | n.a. | n.a. (>70) | n.a. (>70) |
| **$Cohort_2$** | 1931-35 | 48 | 68 | 38 | 45 |
| **$Cohort_3$** | 1936-40 | 58 | 79 | 50 | 59 |
| **$Cohort_4$** | 1941-45 | 72 | 85 | 66 | 67 |
| **$Cohort_5$** | 1946-50 | 82 | 94 | 77 | 73 |

[i] Source: Calculations by authors based on Palacios and Whitehouse 1998. Because many old-system members in cohorts born 1916-25 had already started their pensions and dropped out of the labor force by 1985, these numbers overstate new-system affiliation among all individuals covered by social security in these cohorts.

[ii] Source: EPS 2002. Because new system affiliates were less likely to be pensioners than old-system affiliates, and therefore more likely to contribute as workers, these numbers slightly overstate new-system affiliation among all individuals covered by social security in these cohorts. There were very few contributors in 2000 for $Cohort_1$ or 2, because they were >70.

[iii] Source: EPS 2002. Because new system affiliates were less likely to become pensioners than old-system affiliates, these numbers understate new-system affiliation among all individuals covered by social security in these cohorts.

**Table 2: Age of different cohorts at time of reform and end of our sample**

| Cohort number | Born in: | Age in 1981 | Age in 2004 | Expected impact on pension prob. | Expected impact on pensioner lfpr | Last age observed in sample |
|---|---|---|---|---|---|---|
| **$Cohort_0$** | 1900-15 | 66-81 | 89-104 | Control-old system | Control-old system | 70 |
| **$Cohort_1$** | 1916-25 | 56-65 | 79-88 | Control-old system | Control-old system | 70 |
| **$Cohort_2$** | 1926-30 | 51-55 | 74-78 | Negligible | Small, in 60's | 70 |
| **$Cohort_3$** | 1931-35 | 46-50 | 69-73 | Moderate | Moderate, in 60s | 70 |
| **$Cohort_4$** | 1936-40 | 41-45 | 64-68 | Large | Large | 68 |
| **$Cohort_5$** | 1941-45 | 36-40 | 59-63 | Larger , but<65 | Larger | 63 |
| **$Cohort_6$** | 1946-50 | 31-35 | 54-58 | Larger, but <60 | Larger, in 50s | 58 |

**Table 3: Variable Definitions and Sample Means (Men 50-70)**

**Variables to capture the age profile of participation and pension probabilities**

| | | |
|---|---|---|
| **Age-50** | Age minus 50 | Varies between 0 and 20 |
| **Age-60** | Age minus 60 if >60 | Varies between 0 and 10 |
| **Age-64** | Age minus 64 if age>64 | Varies between 1 and 6 |

**Variables to capture the effect of individual and pension characteristics ($X_i$ and $B_i$)**

| | | |
|---|---|---|
| **Ed6 or less** | Dummy=1 if schooling<=6 | 0.40 |
| **Ed12 or more** | Dummy =1 if schooling>=12 | 0.35 |
| **Real Hh income per capita** | Real household monthly income per capita (minus own wage + pension) 1977 pesos | 876 |
| **Benefit amount** | Real monthly pension income in 1977 pesos (for pensioners) | 3,350 |
| **Pseudo replacement rate** | Pension income for indiv i/ mean wage of 40-49 year olds in i's cohort and ed group (for pensioners) | 0.74 |
| **#children** | # of children younger than 18 | 0.59 |
| **Spouse present** | Dummy=1 if the individual has a spouse living in hh | 0.81 |
| **Spouse in LF** | Dummy=1 if the individual's spouse works (if spouse present) | 0.23 |
| **Spousal age diff** | Own age-spouse age (if spouse present) | 5.1 |

**Variables to capture macroeconomic effects $E_t$**

| | | |
|---|---|---|
| **UnE rate** | Unemployment rate in % | 10.5 |
| **Deviations from H-P trend** | Real lnGDP growth rate minus Hodrick–Prescott filter in % | 0.19 |

**Variables to capture total change for $Cohort_k$ relative to $Cohort_0$, within specified age group, in probits**

| | | |
|---|---|---|
| **$Cohort_0$** | Dummy=1 if born 1900-15 | 1 or 0 |
| **$Cohort_1$** | Dummy=1 if born 1916-25 | 1 or 0 |
| **$Cohort_2$** | Dummy=1 if born 1926-30 | 1 or 0 |
| **$Cohort_3$** | Dummy=1 if born 1931-35 | 1 or 0 |
| **$Cohort_4$** | Dummy=1 if born 1936-40 | 1 or 0 |
| **$Cohort_5$** | Dummy=1 if born 1941-45 | 1 or 0 |
| **$Cohort_6$** | Dummy=1 if born 1946-50 | 1 or 0 |
| **Total post-reform change** | $Cohort_k$ coefficient-$Cohort_1$ coefficient | |
| **Marginal changes by successive cohorts** | $Cohort_k$ = $Cohort_k$ coefficient-$Cohort_{k-1}$ coefficient | |

**Variables to capture differential impact of reform on pensioners and non-pensioners**

| | | |
|---|---|---|
| Pen*$Cohort_k$ | Dummy = 1 if in $Cohort_k$ & pensioner, else 0 | 1 or 0 |

**Table 4 :** ***Reform effects*** as captured by the impact of successive cohorts on **pension rates** (calculations shown in percentage points) [a]

| | AGE GROUP | | |
|---|---|---|---|
| | **Age 50 - 59** | **Age 60 – 64** | **Age 65 – 70** |
| | **Average pension rates at start** | | |
| **Estimated pension rates for Cohort$_0$** | 20.5* | 34.91* | 57.72** |
| | **Marginal effects of successive cohorts[b]** | | |
| **Cohort$_1$** | 0.18 | 2.18 | 7.64* |
| **Cohort$_2$** | -1.39 | 0.58 | -0.53 |
| **Cohort$_3$** | -2.56** | -4.00*** | -1.31 |
| **Cohort$_4$** | -3.82** | -6.63* | 0.45 |
| **Cohort$_5$** | -0.89 | 1.92 | |
| **Cohort$_6$** | -1.38 | | |
| | **Total post-reform effect** | | |
| **Last Cohort observed relative to Cohort$_1$** | -10.04** | -8.10** | -1.4 |

[a] Results based on model (1) reported in Table A3.
[b] Marginal effects for Cohort$_k$ are the difference in total pension probability from Cohort$_{k-1}$ to Cohort$_k$. They are simulated at the mean age of the corresponding age group, setting all other covariates at their sample mean. (The marginal effects of other covariates can be approximated by dividing model (1) coefficients reported in Table A3 by 3.1).

**Table 5:** ***Reform effects*** as captured by the impact of successive cohorts on **labor force participation (LFP) rates**. Full sample, no pension controls (shown in percentage points) [a]

| | AGE GROUP | | |
|---|---|---|---|
| | **Age 50 - 59** | **Age 60 – 64** | **Age 65 – 70** |
| | **Average LFP rates at start** | | |
| **Estimated LFP rates for COH$_0$** | 80.38* | 62.89* | 38.32* |
| | **Marginal effects of successive cohorts[b]** | | |
| **Cohort$_1$** | -1.40 | -3.93** | -5.49* |
| **Cohort$_2$** | 0.73 | 1.73 | 5.83* |
| **Cohort$_3$** | 2.47** | 7.31* | 4.93*** |
| **Cohort$_4$** | 4.46* | 7.47* | 2.09 |
| **Cohort$_5$** | 2.27** | 2.30 | |
| **Cohort$_6$** | 1.80*** | | |
| | **Total post-reform effect** | | |
| **Last Cohort obs. relative to Cohort$_1$** | 11.73* | 18.81* | 12.85** |

[a] Results for Table 5 are based on model (2) reported in TABLE A3. .
[b] Marginal effects for Cohort$_k$ are the difference in labor force participation from ohort$_{k-1}$ to Cohort$_k$. They are estimated at the mean age of the corresponding age group, setting all other covariates at their sample mean. (The marginal effects of other covariates can be approximated by dividing model (2) coefficients reported in Table A3 by 3.12).
*    significant at 1%
**  significant at 5%
***significant at 10%

**Table 6 :** ***Reform effects*** as captured by the impact of successive cohorts on **labor force participation (LFP) rates**. Full sample with pension controls and interactions. (Shown in percentage points) [a]

| | AGE GROUP | | |
|---|---|---|---|
| | **Age 50 - 59** | **Age 60 – 64** | **Age 65 – 70** |
| **NON-PENSIONERS** | | | |
| | **Average LFP rates at start—non-pensioners** | | |
| **Estimated LFP rates of *non-pensioners* in Cohort$_0$** | 94.63* | 87.36* | 74.33* |
| | **Marginal effects of successive cohorts--non-pensioners**[b] | | |
| **Cohort$_1$** | 0.31 | 0.16 | -2.39 |
| **Cohort$_2$** | -0.45 | 2.43 | 8.61* |
| **Cohort$_3$** | 0.69 | 1.79 | -2.30 |
| **Cohort$_4$** | 0.85 | -0.03 | 4.25 |
| **Cohort$_5$** | 0.13 | 0.75 | |
| **Cohort$_6$** | -0.22 | | |
| | **Total post-reform effect--non-pensioners** | | |
| **Last Cohort obs. relative to Cohort$_1$** | 0.99 | 4.94 | 10.56** |
| **Last Cohort obs. relative to Cohort1 in separate nonpensioner regression** | 1.26** | 5.25** | 11.66* |
| **PENSIONERS** | | | |
| | **Effect on LFP at start--pensioners** | | |
| **Change in LFP due to *pensioning,* Cohort$_0$** | -66.44* | -68.30* | -61.63* |
| | **Marginal effects of successive cohorts--pensioners**[b] | | |
| **Cohort$_1$** | -8.34* | -6.64** | 0.23 |
| **Cohort$_2$** | 1.28 | 1.94 | 4.96** |
| **Cohort$_3$** | -0.38 | 10.78* | 10.67* |
| **Cohort$_4$** | 6.83** | 11.17* | 1.25 |
| **Cohort$_5$** | 16.65** | 8.81 | |
| **Cohort$_6$** | 9.20*** | | |
| | **Total post-reform effect--pensioners** | | |
| **Last Cohort obs. relative to Cohort$_1$** | 33.59** | 32.71** | 16.89* |
| **Last Cohort obs. relative to Cohort1 in separate pensioner regression** | 28.32** | 29.37** | 14.67** |

[a] Results for Table 6 are based on model reported in Table A4.
[b] Marginal effects of successive cohorts interacted with pension status are simulated using CLARIFY (Tomz, Wittenberg, and King (2003). (The marginal effects of other non-interacted covariates can be approximated by dividing the coefficients reported in Table A4 by 3.67)
* significant at 1%
** significant at 5%
***significant at 10%

**Table A1: Number of Observations by Cohort and Age**

| Cohort | Year of birth | Number observations, by age range 50 to 54 | 55 to 59 | 60 to 64 | 65 to 70 |
|---|---|---|---|---|---|
| $Cohort_0$ | 1900-1915 | 1,450 | 2,052 | 2,371 | 2,485 |
| $Cohort_1$ | 1916-1925 | 2,660 | 1,859 | 1,596 | 1,642 |
| $Cohort_2$ | 1926-1930 | 1,342 | 938 | 874 | 802 |
| $Cohort_3$ | 1931-1935 | 1,260 | 915 | 836 | 805 |
| $Cohort_4$ | 1936-1940 | 1,236 | 880 | 748 | 322 |
| $Cohort_5$ | 1941-1945 | 1,270 | 939 | 366 | |
| $Cohort_6$ | 1946-1950 | 1,410 | 490 | | |

**Table A2: Number and % of Pensioners in Sample, by Cohort and Age**

| | | Number pensioners, by age | | | | Pensioners as % of total obs. | | |
|---|---|---|---|---|---|---|---|---|
| **Cohort** | **Birth year** | **50 to 54** | **55 to 59** | **60 to 64** | **65 to 70** | **50-59** | **60-64** | **65-70** |
| $Cohort_0$ | 1900-1915 | 221 | 449 | 771 | 1,345 | 19 | 33 | 54 |
| $Cohort_1$ | 1916-1925 | 348 | 499 | 613 | 1,059 | 19 | 38 | 64 |
| $Cohort_2$ | 1926-1930 | 204 | 229 | 330 | 486 | 19 | 38 | 61 |
| $Cohort_3$ | 1931-1935 | 193 | 165 | 275 | 500 | 16 | 33 | 62 |
| $Cohort_4$ | 1936-1940 | 105 | 149 | 212 | 183 | 12 | 28 | 57 |
| $Cohort_5$ | 1941-1945 | 101 | 149 | 100 | | 11 | 27 | |
| $Cohort_6$ | 1946-1950 | 98 | 72 | | | 9 | | |

**Table A3: Probit Models: Probability of Pension and Labor Force Participation (LFP) full sample, no pension controls**

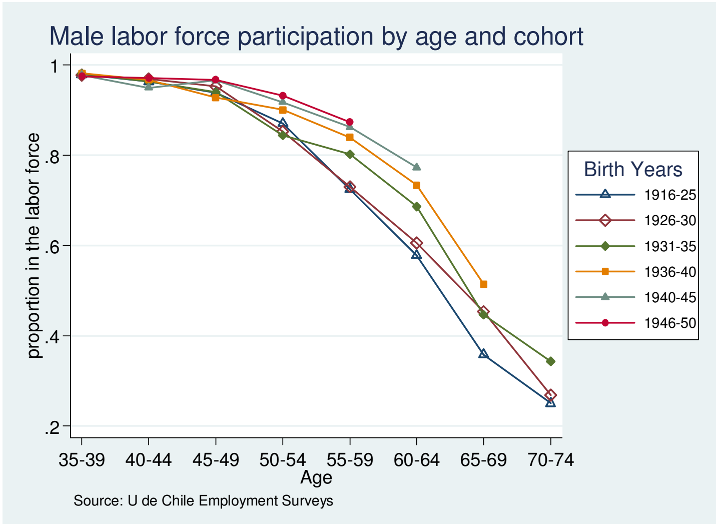
| | (1) | | (2) | |
|---|---|---|---|---|
| | **Pension probability** | | **LFP rate, no pension controls** | |
| | Coefficient | P value | Coefficient | P value |
| **Age Group 50-59** | | | | |
| $Cohort_0$ | -1.205 | 0 | 0.958 | 0 |
| $Cohort_1$ | 0.005 | 0.884 | -0.051 | 0.145 |
| $Cohort_2$ | -0.043 | 0.334 | -0.023 | 0.604 |
| $Cohort_3$ | -0.143 | 0.001 | 0.066 | 0.135 |
| $Cohort_4$ | -0.307 | 0 | 0.252 | 0 |
| $Cohort_5$ | -0.351 | 0 | 0.366 | 0 |
| $Cohort_6$ | -0.424 | 0 | 0.467 | 0 |
| **Age Group 60-64** | | | | |
| $Cohort_0$ | -0.006 | 0.907 | -0.113 | 0.021 |
| $Cohort_1$ | 0.053 | 0.329 | -0.214 | 0 |
| $Cohort_2$ | 0.068 | 0.253 | -0.173 | 0.004 |
| $Cohort_3$ | -0.039 | 0.516 | 0.029 | 0.63 |
| $Cohort_4$ | -0.230 | 0 | 0.249 | 0 |
| $Cohort_5$ | -0.179 | 0.024 | 0.321 | 0 |
| **Age Group 65-70** | | | | |
| $Cohort_0$ | 0.030 | 0.674 | -0.206 | 0.004 |
| $Cohort_1$ | 0.232 | 0.002 | -0.352 | 0 |
| $Cohort_2$ | 0.218 | 0.007 | -0.198 | 0.014 |
| $Cohort_3$ | 0.182 | 0.026 | -0.074 | 0.366 |
| $Cohort_4$ | 0.193 | 0.043 | -0.020 | 0.834 |
| **Other Covariates** | | | | |
| Age – 50 | 0.065 | 0 | -0.062 | 0 |
| Age - 60 if age>60 | -0.007 | 0.528 | 0.008 | 0.457 |
| Age - 64 if age>64 | 0.049 | 0 | -0.053 | 0 |
| 6 or less years of schooling | -0.203 | 0 | 0.099 | 0 |
| 12 or more years of schooling | 0.008 | 0.701 | 0.101 | 0 |
| Real household income/capita | 0.000 | 0.011 | -0.0002 | 0 |
| Number of children younger than 18 | -0.050 | 0 | 0.059 | 0 |
| Spouse present | 0.223 | 0 | 0.122 | 0 |
| Spouse in the labor force | -0.340 | 0 | 0.275 | 0 |
| Spouse age differential (Husband-Wife) | -0.010 | 0 | 0.013 | 0 |
| Unemployment Rate | 0.946 | 0 | -0.826 | 0 |
| | | | | |
| **Log likelihood** | | -15,984 | -15,902 | |
| **# of observations** | | 31,579 | 31,579 | |
| **Pseudo $R^2$** | | 0.1475 | 0.1573 | |

**Table A4: Probit Model for the Probability of Labor Force participation --full sample, controlling for pension effects**

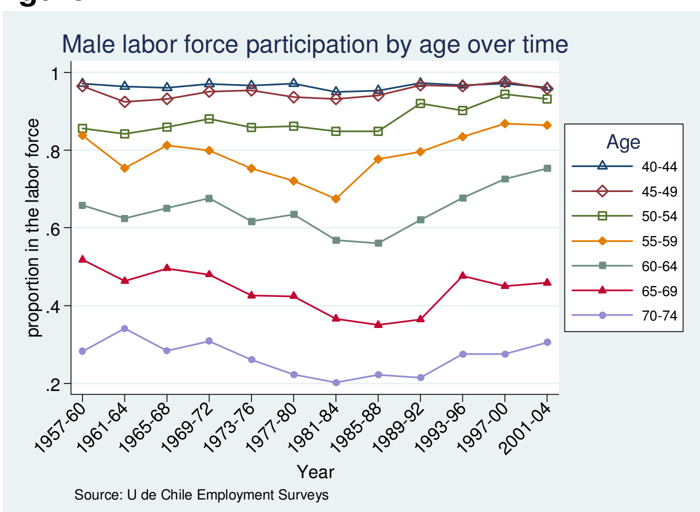
| | **Cohort*Age effects** | | **Cohort*Age*Pensioner effects** | |
|---|---|---|---|---|
| | Coefficient | P value | Coefficient | P value |
| $Cohort_0$ | 1.461 | 0 | -2.371 | 0 |
| $Cohort_1$ | 0.027 | 0.626 | -2.669 | 0 |
| $Cohort_2$ | -0.013 | 0.851 | -2.585 | 0 |
| $Cohort_3$ | 0.054 | 0.43 | -2.669 | 0 |
| $Cohort_4$ | 0.142 | 0.04 | -2.531 | 0 |
| $Cohort_5$ | 0.160 | 0.019 | -2.098 | 0 |
| $Cohort_6$ | 0.132 | 0.075 | -1.837 | 0 |
| **Age Group 60-64** | | | | |
| $Cohort_0$ | -0.105 | 0.17 | -2.568 | 0 |
| $Cohort_1$ | -0.102 | 0.225 | -2.856 | 0 |
| $Cohort_2$ | 0.034 | 0.731 | -2.903 | 0 |
| $Cohort_3$ | 0.139 | 0.161 | -2.613 | 0 |
| $Cohort_4$ | 0.136 | 0.188 | -2.284 | 0 |
| $Cohort_5$ | 0.201 | 0.117 | -2.122 | 0 |
| **Age Group 65-70** | | | | |
| $Cohort_0$ | -0.072 | 0.527 | -2.855 | 0 |
| $Cohort_1$ | -0.142 | 0.246 | -2.775 | 0 |
| $Cohort_2$ | 0.138 | 0.304 | -2.846 | 0 |
| $Cohort_3$ | 0.062 | 0.649 | -2.419 | 0 |
| $Cohort_4$ | 0.214 | 0.195 | -2.535 | 0 |

| **Other Covariates** | Coefficient | P value |
|---|---|---|
| Age – 50 | -0.049 | 0 |
| Age - 60 if age>60 | -0.010 | 0.567 |
| Age - 64 if age>64 | -0.043 | 0.058 |
| pensioner*(Age - 50) | 0.039 | 0 |
| pensioner*(Age - 60 if age>60) | 0.046 | 0.112 |
| pensioner*(Age - 64 if age>64) | 0.000 | 0.993 |
| 6 or less years of schooling | -0.068 | 0.008 |
| 12 or more years of schooling | 0.197 | 0 |
| Pseudo replacement rate | -0.092 | 0 |
| Real household income/capita | 0.000 | 0 |
| Number of children younger than 18 | 0.046 | 0 |
| Spouse present | 0.436 | 0 |
| Spouse in the labor force | 0.035 | 0.236 |
| Spouse age differential (Husband-Wife) | 0.011 | 0 |
| Unemployment Rate | -0.335 | 0.179 |
| **Log likelihood** | | -9,715 |
| **Number of observations** | | 31,376 |
| **Pseudo $R^2$** | | 0.4783 |

## Figure 1




## Figure 2




## Figure 3

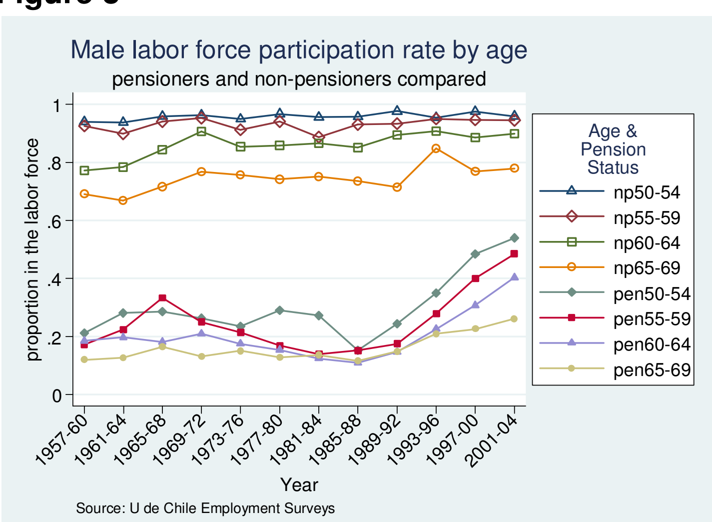


**Figure 4**

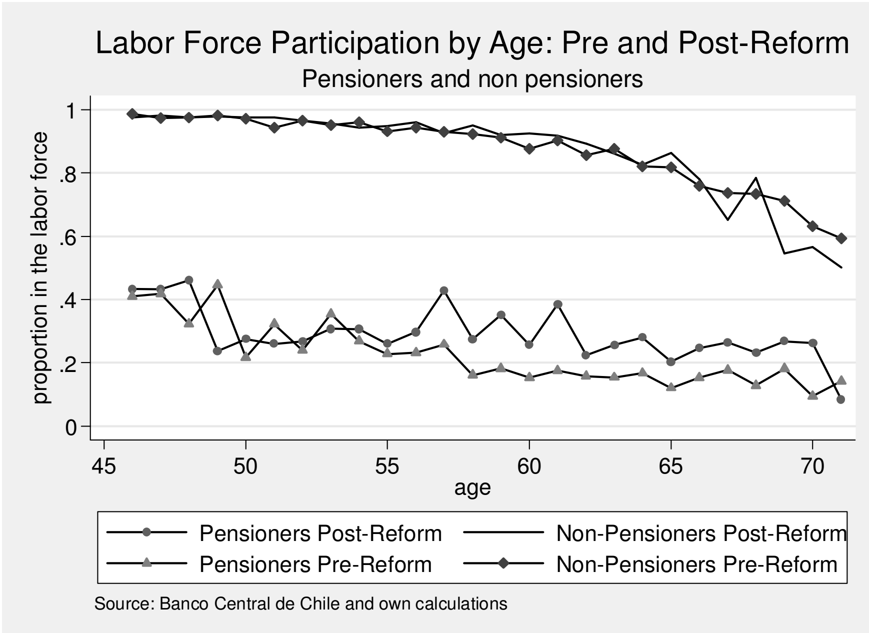



**Figure 5**

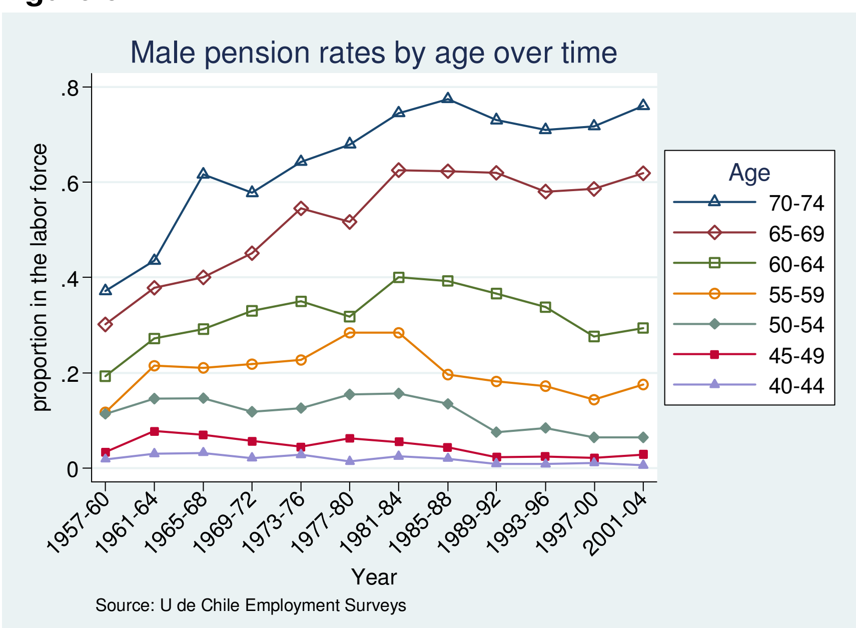


**Figure 6: Labor Force Participation Rates of Males, Age 55-64, 1970-2000, Selected OECD countries**

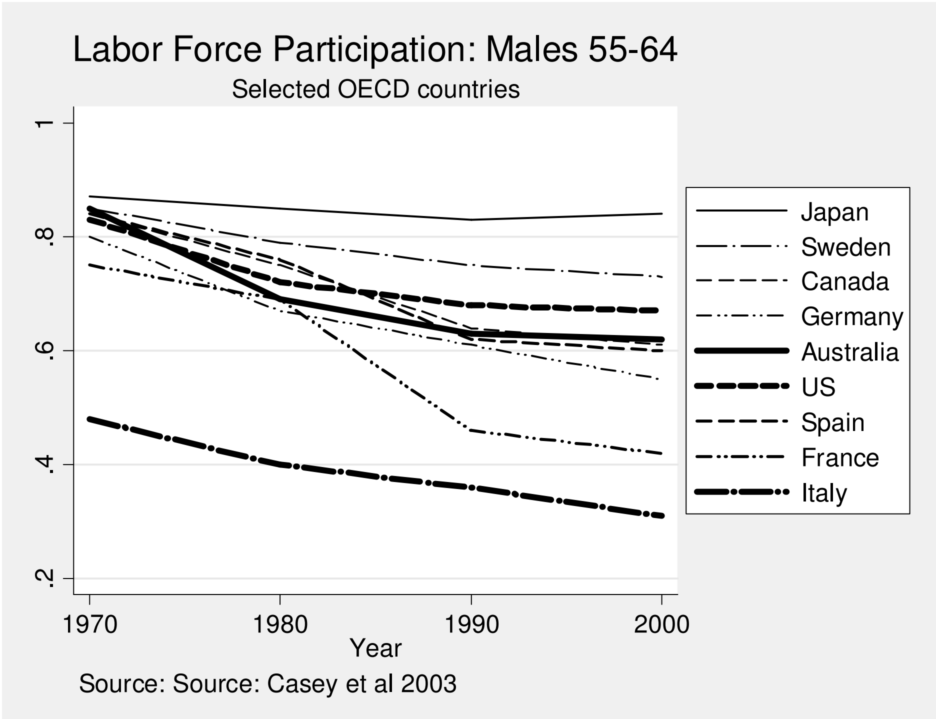
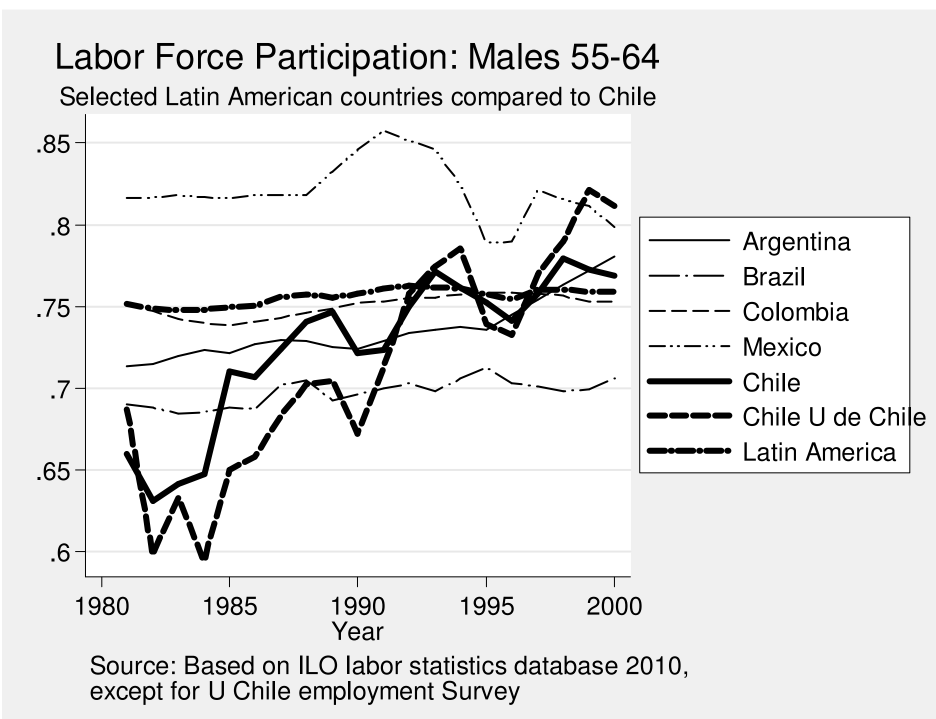



**Figure 7: Labor Force Participation Rates of Males, Age 55-64, 1981-2000, Selected Latin American Countries**



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## Endnotes

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[1] For preliminary descriptive statistics on labor supply impact see Edwards and Edwards (2002). Gruber (1997) studied the incidence of the payroll tax in Chile.

[2] If equation (4) holds, then, by definition, equation (2) also holds. However, it is possible for equation (2) to hold for incremental contributions and postponed pension age, with given mortality rates, even if equation (4) does not hold when mortality rates change. For example, the U.S. attempts to adjust monthly benefits for postponed pensioning in an actuarially fair manner, but does not adjust for changing expected lifetimes. This is part of the reason for the large unfunded social security debt in the U.S.

[3] These descriptions are based on SAFP (2003), Cheyre (1991) and personal communications with Augusto Iglesias, Primamerica Consultores.

[4] While employers paid 2/3 of the 23% tax in the old system, workers pay the entire tax in the new system, an increase from 7.25% to 12.5% of wages. In the short run, wage increases were legally required for workers who switched, to compensate for the tax saving to employers and new tax paid by workers. In that case, gross and net wages would have increased. We don't know how effectively that requirement was implemented or how markets responded in the long run to the changed tax rate. Real wages have risen, on average, about 2% per year for almost 30 years. In 2008 the responsibility for covering disability and survivors' insurance was shifted to the employer.

[5] Until 2002 workers had a choice of investment manager but virtually no choice of investment strategy. Since 2002 greater choice of investment options has been allowed but this is still limited to 4 or 5 portfolios. Workers who wish to put all their funds into stocks or foreign investments are not allowed to do so. Investment options during the payout phase are even more tightly constrained. Until 2004 only fixed annuities or gradual withdrawals based on conservative investments were allowed.

[6] The estimate of a 50% implicit tax is based on wage differentials between workers who pay the payroll tax and get benefits versus those who don't (Edwards and Edwards 2002). For a related UK analysis see Disney 2004. The negative effects of these constraints in Chile were mitigated during the first 20 years of the new system by a high average annual real rate of return that averaged over 10%.

[7] There were some variations by sub-system. Retirement age was even lower before the old system was reformed, for fiscal reasons, in the late 1970's. Early retirement and evasion resulted in a contributor/ pensioner ratio of 2.2 in 1980, lower than in the U.S. today, despite a much younger population in Chile.

[8] Although these restrictions appear to be high, the majority of pensioners since 1988 have been early pensioners, consistent with our expectation that most individuals will start their pension as soon as possible. The implied age of eligibility varies widely across individuals, so we do not expect bumps in retirement at any particular point until age 65.

[9] Several other forces in the new system may lead to some postponement of pensioning. Non-pensioners who contribute get access to disability and survivors insurance at a community rate that is advantageous (below true risk cost) for older workers; they lose this access once they start their pension. Some low-pension retirees will be entitled to a

higher minimum pension guarantee if they start their pension later. Those with less than 20 years of contributions may delay pensioning until they meet this eligibility condition for the MPG. Finally, pensions are taxable so postponing pensions also postpones taxes.

[10] This applies directly to involuntary postponement due to tighter early retirement pre-conditions. Individuals who have voluntarily postponed pensioning will take liquidity constraints into account, and will make a joint decision about pension age and work. Actuarial fairness could then lead them to postpone pension age and continue working longer as non-pensioners, simultaneously.

[11] At any given point in time, about 60-70% of the labor force contributes. Most men have contributed at some point during their working lives, but some of them only sporadically (Berstein, Larrain and Pino 2005).

[12] The new system should be more actuarially fair and the positive incentive effects stronger for workers with voluntary savings, who could offset any negative effects of high mandatory saving rates, thereby staying within their preferred gross saving rates. Ideally, we would examine the behavioral response of workers with and without voluntary saving, but our data do not give us information about individual-level saving. We do know that aggregate household saving increased during the post-reform period, but voluntary saving was negative every year (ranging between -2 to -8% of GDP)—suggesting that the mandatory rate exceeded the preferred rate for most people (Bennett, H., K. Schmidt-Hebbel, and C. Soto 1999).

[13] The post-1981 system contains a minimum pension guarantee (MPG), whose implications for work incentives are complex, partially conflicting and apply to only a small subset of individuals—those with low earnings and a partial history of contributions. Twenty years of contributions are required for eligibility. Low earners have a large incentive to work long enough to become eligible, but to stop work thereafter. To ascertain the impact of the MPG on work and pension behavior requires detailed individual-level information on employment histories and accumulations that our data set does not provide. The government has recently started replacing the MPG with a means-tested basic benefit that will be targeted toward the 60% poorest households in the economy, including non-contributors, creating yet another set of incentives. Women are major recipients of the MPG and the basic benefit. We reserve this topic for further investigation using a different data set and focus on the broader incentives outlined above, in this paper.

[14] Several of these countries—for example Sweden, Colombia and Mexico--reformed their systems in the late 1990's and these labor market effects might show up after 2000. Because a variety of reforms have been adopted since 2000 in many countries, we stop our country comparisons in 2000. Argentina adopted a Chilean-type reform in 1994, which may help explain the small upturn in its LFP before 2000.

[15] Initially we measured single-year cohort trend lines without distinguishing among age groups—one underlying trend starting with the cohort born in 1916 and a drift from this trend starting with the cohort born in 1931—the post-reform effect. This reform effect on LFP had a significant main coefficient of 1.35 percentage points per cohort year and a negative quadratic term that would eventually bring the transition to a close. However,

for reasons given in the text, we believe that allowing the marginal reform effect to vary by cohort and age group is more justifiable.

[16] This routine is part of a suite of Stata programs for interpreting statistical results. See Michael Tomz, Jason Wittenberg and Gary King, 2003. CLARIFY: Software for Interpreting and Presenting Statistical Results. Version 2.1. Stanford University, University of Wisconsin, and Harvard University. Jan.5. See http://gking.harvard.edu/.

[17] The proportion of individuals with only primary education was cut by more than half over this period—from 51% for $Cohort_0$ to 23% for $Cohort_6$. At the same time, the proportion of individuals with post-secondary education doubled—from 26% for $Cohort_0$ to 50% for $Cohort_6$. We alternatively measured education as a continuous variable—years of schooling. The reform impact was very similar in both cases.

[18] These calculations assume a 4% real interest and discount rate and 2% real wage growth. The actual real rate of return to pension funds in Chile during the post-reform period was over 10%. The interest payable on government bonds was lower. Of course, everything else did not remain constant over this period. For example, accumulations grew and interest rates fell (implying temporary capital gains), which would have changed the monthly benefit, even if longevity were unchanged.

[19] While these effects may seem surprisingly large, Laitner and Silverman (2008) also find surprisingly large effects from a hypothetical payroll tax exemption for older workers in the U.S., in simulations.

[20] Based on cumulative effects for full sample, no pension controls (Table 5), using 2.5:1 weighted average of ages 50-59 and 60-64 in Table 5.

[21] The calculations in this and the following section assume a 4% real rate of return.

[22] For the period 1981-2008, based on World Bank data in 2000 U.S. dollars.

[23] The DC scheme could be funded, as in Chile, or PAYG, as in Sweden. Some countries have adopted notional (PAYG) DC schemes because this enables them to get the labor market incentives of DC without incurring the transition costs inherent in funding.

[24] When New Zealand phased in an increase in the age at which individuals could begin receiving its flat old age pension from 60 to 65 during the 1990's, participation rates of men age 60-64 more than doubled--from 32% in 1992 to 67% in 2004, and participation rates of women rose from 15% to 45% (Baily and Kirkegaard 2009).

[25] Sweden's notional DC plan, Norway's planned notional DC plan, Finland's DB scheme and the sustainability factor in Germany's point system have also tied monthly benefit amount to life expectancy, as calculated by a public agency. These adjustments may affect the individual's choice of retirement age, but they do not directly alter the early retirement constraint; that depends on the ability of workers to make an accurate calculation. Denmark is planning to increase retirement age on par with increases in life expectancy. Chile's retirement age adjustment operates via a market response to greater longevity combined with a replacement rate requirement for early pensioning.