Incentive Effects and Unemployment Accounts: Two Natural Experiments

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Abstract

It has been traditionally argued that unemployment insurance programs are responsible for higher unemployment rates, due to the moral hazard of individuals reducing their search efforts when receiving unemployment benefits that are conditional on remaining unemployed. More recently, Chetty (2006) has argued that the duration effect of traditional UI benefits may be partly due to credit constraints that are relaxed by the system. In addition, a number of authors, most notably Stiglitz and Yun (2005) and Feldstein and Altman (2007), have argued that a system of individual accounts can help overcome the moral hazard problem because of the individual ownership of the account. However, liquidity might still be relevant under these types of programs. In this paper we take advantage of two natural experiments within the Chilean UI system, a system mainly based on individual accounts. Both natural experiments provide extra liquidity to the unemployed, by front loading benefits that are financed by the individual account, without changing the present value of the stream of benefits. Based on the predictions of a stylized search model, these experiments allow us to test both whether there are incentive effects in an individual account system and whether these effects are driven by liquidity. Our findings are mixed. Our preferred results, those that take advantage of a 2009 legislation reform, show that front loading the benefits is correlated with a less intense search effort, consistent with the hypothesis of liquidity effects. However, these estimated effects are neither economically nor statistically significant. On the other hand, our results based on a regression discontinuity approach exhibit a positive effect of significantly frontloading benefits (going from two payments to only one) on search effort during the first two months. These differences could be explained by the different margins being identified by the two strategies.

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1.- Introduction

The design of optimal unemployment insurance policy has been a topic of vigorous debate among academics and policy makers for a long time. Theoretical models have stressed the trade-off between the consumption-smoothing effects and the moral hazard cost of reduced search associated to UI benefit provision (Baily (1978), Hopenhayn and Nicolini (1997), Acemoglu and Shimer (1999)). A wealth of empirical research has consistently found that the generosity of a UI system measured by the level of the benefits and the potential duration of eligibility have positive effects on the average length of unemployment spells. Moreover, the reemployment probability spikes as benefits exhaustion approaches (Gruber (1997), Katz and Meyer (1990), Meyer (1990)). Chetty (2006) has more recently argued, however, that these effects may confound moral hazard and liquidity; i.e., UI benefits may loosen liquidity constraints, allowing workers to search more efficiently. Based on the eligibility conditions for perceiving severance pay in Austria, Card et al. (2006) provide empirical results consistent with the liquidity effect of social insurance.

In addition, a number of papers have stressed that shifting to a system of individual investments-based savings accounts may help overcome the negative incentive effects of traditional systems (Stiglitz and Yun (2005), Feldstein and Altman (2007)). In the case of UI, a system of mandatory individual accounts complemented by a government credit fund could provide insurance without distortions in search behavior.

In 2002, Chile introduced a new unemployment insurance system, combining self-insurance and social insurance. Under the system, workers and employers contribute to an investment-based individual savings account (UA), whereas the government and employers make contributions to a common pool called the Solidarity Fund (SF). The unemployed first draw benefits from their individual account; i.e., they support consumption during spells of unemployment drawing on savings accumulated while working. Upon depletion and under certain conditions on the type of contract, the number of months of contributions and the cause of termination, workers may choose to draw from the Solidarity Fund. All unused UA funds at retirement are available for withdrawal or for deposit into the workers' pension system individual account. The balance is bequeathed if the worker dies before retirement.

The Chilean UI scheme provides an opportunity to test some of the underlying assumptions implicit in this literature. In this context, the goal of this paper is twofold. First, we intend to evaluate whether there are incentive effects in a UA based scheme. Secondly, we intend to assess whether the Chilean experience can shed light on the role of liquidity effects in job search decisions.

In order to do so, we take advantage of two natural experiments that have taken place within the Chilean system. Under both experiments, the total potential benefit an unemployed worker expects to perceive is unchanged, but the flow of payments becomes more front-loaded. Intuitively, in a pure UA system and without liquidity constraints, individuals should be indifferent between receiving their benefits in one, two or any number of payments. No adverse effects on incentives should arise as the balance of the account is fully owned by the worker. However, if cash on hand is restricted, then front loading payments will generate liquidity effects as those described by Chetty (2006).

Using administrative data from the UI system for the period 2005-2010, we explore whether there are reemployment effects of front loading the benefits of the UA. Until 2009, a worker with an open-ended contract who lost his job was entitled to withdraw funds from his UA, no matter the cause for separation, if he had contributed for at least 12 months since enrollment or since the last benefit payout. The total balance of the account was paid out in at most 5 months, depending on the number of monthly contributions. For instance, workers who had contributed for at least 12 months but for no more than 18 months could cash their balance in full in the first month of unemployment. Workers who had contributed between 19 and 30 months could take their balance in two payments. Sharp discontinuities also occur at 42 and 54 months of work. Our first experiment thus compares the reemployment behavior of similar unemployed workers around these cut-off numbers of months. In other words, we compare the outcomes of workers who had very similar balances in their UA and who faced similar total replacement rates, but that had worked for 18 or 19 months, 31 or 32 months, respectively, and so on. If losing one's job at 18 or 19 months is an exogenous event, then any effect we find on their reemployment behavior should be due, in principle, to a liquidity effect.

Our second experiment exploits the variation in the flow of payments that resulted from a reform implemented in May 2009. As explained before, workers cashing their UI account prior to the reform received their benefit flow in a number of payments that depended on the number of months contributed. The reform changed the basis for benefit payment to a replacement rate schedule. That is, with enough balance in the account, the first payment equals 50% of lost labor income; the second equals 45%, the third 40%, the fourth 35%, the fifth 30%, the sixth 25% and the remainder payments equal 20% each until balance exhaustion. So depending on the balance in the account and the number of months with contributions, workers cashing benefits right before and right after the reform may have expected to receive the same total amount of funds, but with a different time distribution.

Our main findings are mixed. The estimated results for the experiment on the reform show, as expected, that increasing liquidity in the first month of unemployment increases the likelihood of not finding a covered job right away. This result, in turn, is consistent with the hypothesis that workers search less intensively because of a cash-on-hand effect. However, these coefficients are almost always not statistically significant.

On the contrary, the results for the experiment based on the pre reform discontinuities lead to the opposite conclusions. This time our estimates indicate that front loading payments (going from two payments to one) is related to shorter unemployment spells consistent with higher search intensity. Our preferred estimates, based on an instrumental variable strategy, show statistically significant effects during the first two months, the actual length of benefit receipt.

One potential explanation for the differences between the results from the two different strategies is that they identify different objects; the reform strategy identifies the average effect of front loading at the intensive margin (increasing the importance of the first payment but essentially keeping the number of payments unchanged) for a broad group of individuals (those with contributions between 12 and 31 months). The regression discontinuity strategy identifies the average effect of front loading at the extensive margin (changing the number of payments, from two to one, therefore greatly increasing the importance of the first payment) for a narrower group of individuals: the compliers with the instrument at the 18-19 contributions margin, i.e. the individuals for whom the 19th contributed month actually makes a difference between receiving one or two months.

The remainder of the paper is organized as follows. Section 2 describes the workings of the Chilean UI system and provides more detail on our identification strategy. Section 3 develops a very simple search theoretic model in order to shed light on the predicted effects of the natural experiments. Section 4 describes the data used for the empirical estimation and presents descriptive statistics relevant for the two identification strategies. In Section 5 we describe our methodological approach for estimating the experimental effects, whereas Section 6 presents our results. We conclude in Section 7.

2.- The Chilean UI system and the identification strategy

In October of 2002, Chile launched a new UI scheme[2]. The system replaced a program called *Subsidio de Cesantía* -- Unemployment Subsidy-- characterized by low coverage and low benefits, and financed by general taxes (Coloma, 1996). The new system is mainly based on individual savings accounts administered by a private firm, similarly to the individual capitalization pension system set up in Chile in the early 1980s[3]. It is also backed by a contingency fund, called the Solidarity Fund, from which workers can draw under certain conditions if the individual accumulation is insufficient.

The Chilean UI system covers salaried employees in the private sector, excluding apprentices, workers under 18 years of age, domestic workers and retirees. The system is mandatory for all workers signing contracts after September of 2002 and voluntary for those starting employment relationships prior to the implementation of the new legislation.

The Chilean UI system provides different coverage to permanent (or indefinite) employees than to temporary workers. For workers hired under indefinite contracts, the object of our analysis, the UI

[2] In this section we focus on the requirements and benefits for workers under open ended contracts, who are the subjects of our analysis. See Acevedo, Eskenazi and Pages (2006), Senbruch (2004) and more recently Superintendencia de Pensiones (2010, in spanish) or Berstein, Fajnzylber and Gana (2011) for broader descriptions of the workings of the system.

[3] The management of the UI resources is auctioned to a single firm for a 10 year period on the basis of the administrative fee offered. A fee between 0.5 and 0.7% of the balance is charged to contributing workers. The commission is also applied to the Solidarity Fund. The investment managing firm not only administers the accumulated resources, but also collects contributions, keeps information records, verifies eligibility criteria and pays benefits.

system is based on two components: an individual account and the contingency fund. A fraction equal to 2.2% of the wage is deposited in the account every month --1.6% paid by the employer and 0.6% by the worker—up to a wage cap of about 4400 dollars[4]. The accumulation plus its return can be withdrawn at the end of the employment relationship even if the worker has not been dismissed. The only requirement is to have contributed for at least 12 months since affiliation or since the last benefit was paid. The contributions do not need to be continuous for entitlement. Workers may withdraw all their savings in one unemployment spell.

Prior to the 2009 reform, the number of withdrawals and the amount of each depended on the number of months of contributions. After the reform, the payments depend on replacement rates. The rules are described in Table 2.1.

Table 2.1. UA Withdrawal Rules

Rules between October 2002 and April 2009

Number of months of contributions	Number of payments	Factor to compute first payment	Payment as fraction of first's month benefit
18 or less	1	1.0	1.0
19-30	2	1.9	0.9
31-42	3	2.7	0.8
43-54	4	3.4	0.7
55 or more	5	4.0	

Rules starting in May 2009

Benefit	Replacement Rate (%)
First	50
Second	45
Third	40
Fourth	35
Fifth	30 or remainder

Prior to the reform, workers with fewer than 18 months of contributions could withdraw all their funds at once. Workers with more than 18 months of contributions could also take their entire balance in fractions according to the schedule in Table 2.1. If workers were entitled to more than one benefit, the first payment was calculated by dividing the balance in the account by a factor;

[4] We assume an exchange rate of 500 Chilean pesos per dollar.

then the following payments were computed as a fraction of the first. The fifth payment was equal to all the remaining funds. Finally, if the worker found a job, he was entitled to the benefit of the current month and was allowed, upon request, to withdraw the following payment.

In January 2009, as a response to an over accumulation of resources in the Solidarity Fund, a UI system reform passed into law that became effective in May 2009. Since then, the requirements on UA withdrawals are unchanged, but the benefit payout formula switched from a function of the number of contributed months to a function of the replacement rate (Table 2.1).

Contributions to the individual account cease after 11 years of relationship with the same employer. At the time of retirement, the worker can either withdraw the unused resources in one payment or transfer them to the individual account of the pension system. The balance in an account is bequeathed if the worker dies before retirement. Workers who have contributed with an open ended contract for at least 12 continuous months, who have been dismissed for no fault of their own, and who have accumulated less than two months' wages in their account are entitled to top up their benefits with resources from the Solidarity Fund.[5] When the worker takes up the SF, benefits are first deducted from the individual account balance. Once the account is exhausted, the benefits are paid out of the Solidarity Fund. This common pool is financed by the contributions of employers (0.8% of the wage) and by the State. The payments replace pre tax wages at the same rates as the UA after the reform (Table 2.1).[6] There are minimum and maximum values for the benefits, which are updated once a year according to changes in the consumer price index. Beneficiaries from the Solidarity Fund must be unemployed when asking for benefits and must register monthly at a municipal employment agency[7]. Workers can use this option at most twice every 5 years.

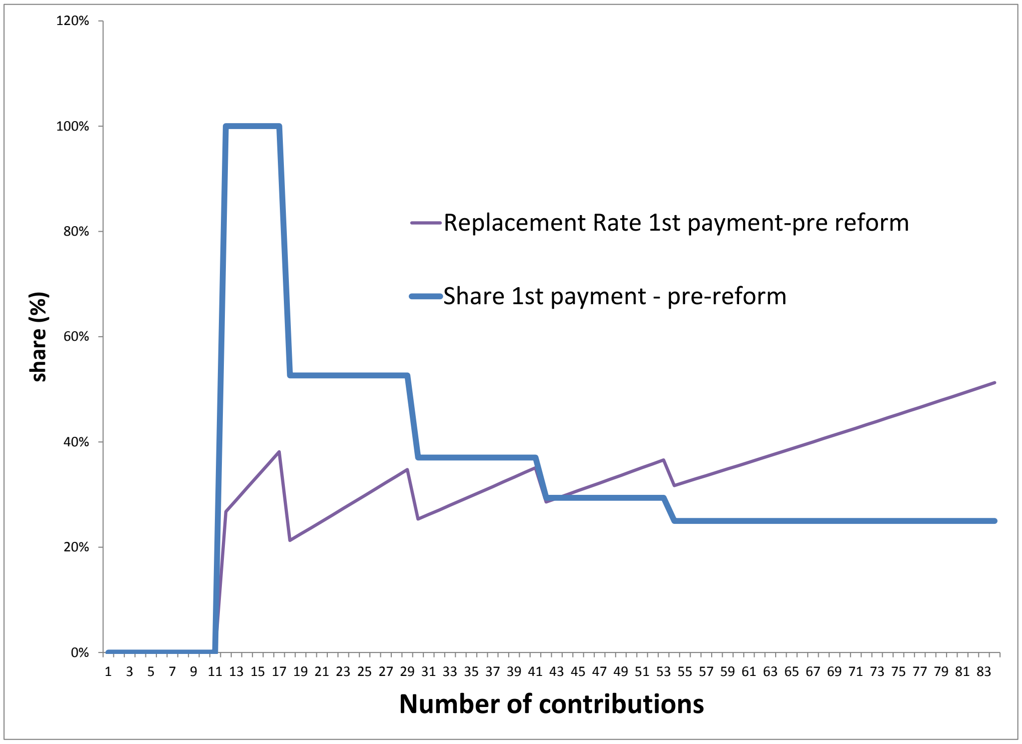
The new legislation passed into law in 2009 requires, instead of the 12 *continuous* contributions, 12 contributions in the past 24 months for Solidarity Fund eligibility. In addition, benefits are extended for two additional months whenever the national unemployment rate surpasses a threshold. The two extra payments replace wages at rates 25% and 20%.

In this paper we use the properties of the UA payout schemes to identify the incentive effects of the system. In the first experiment, we take advantage of the sharp discontinuities in payments depending upon the number of months contributed. More specifically, at months 18, 30, 42 and 54, the payment flow changes from one to two payments, two to three, three to four and four to five, respectively. Figure 2.1 depicts these sharp discontinuities by displaying the implicit replacement rate of the first payment, and the share of the benefit represented by the first payment as a function of the number of months of contributions. Two otherwise identical workers

[5] To be eligible, workers must have lost their jobs either due to "unavoidable events" or "business necessity", i.e. articles 159 n.6 and 161 of the Labor Code.
[6] Benefits from the UI scheme are tax exempt.
[7] It is also expected that any job offer that pays 50% or more of the previous earnings must be accepted, unless there is a justified reason for not doing so.

with identical balances in their UA will face different payout streams if they lose their job or quit right before and right after these cutoff points.

Figure 2.1. Natural Experiment Pre Reform – replacement rate on first payment and share of first payment on total benefits, as a function of the number of contributions



Source: Simulations assuming a constant wage, a 2.2% contribution rate and 3% annual interest rate.

Our second experiment takes advantage of the 2009 modifications of the legislation. The change in the payout formula implied that, given the actual balances in their accounts and the number of contributed months, many workers saw reductions in the number of benefits they expected to perceive and thus a front loading of the benefits. Figure 2.2 displays the stylized effect of the reform on the number of payments (figure 2.2a), the replacement rate corresponding to the first payment (figure 2.2b), and the share of the total payment given to the first payment (figure 2.2c), as a function of the number of contributed months.[8]

According to these stylized simulations, workers with less than 71 contributions should receive fewer payments after the reform, implying a higher replacement rate and a higher share to be paid at the beginning (more front-loaded benefits). On the other extreme, individuals who, after May 2009, contribute more than 82 months will receive more than the maximum 5 payments of

[8] Benefits in figure 2.2 were simulated assuming a constant wage, a 2.2% contribution rate and 3% annual interest rate.

the pre-reform environment, therefore receiving a smaller first payment (in terms of replacement rate and share of total payment) in the post reform scenario.

Figure 2.2a. Natural Experiment Around the Reform – Number of benefits

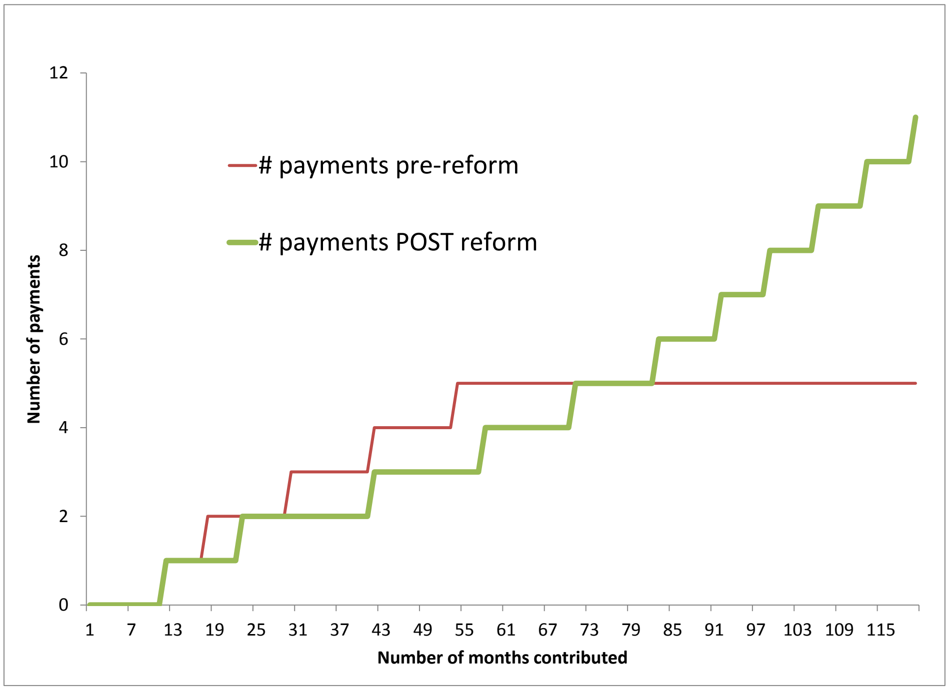


Figure 2.2b. Natural Experiment Around the Reform – Replacement rate first payment

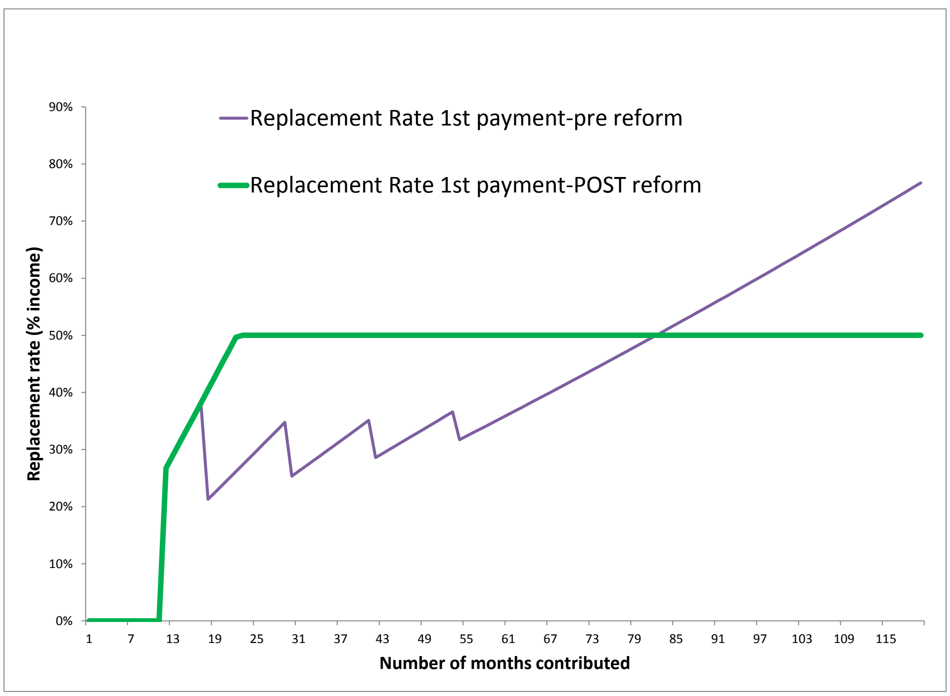
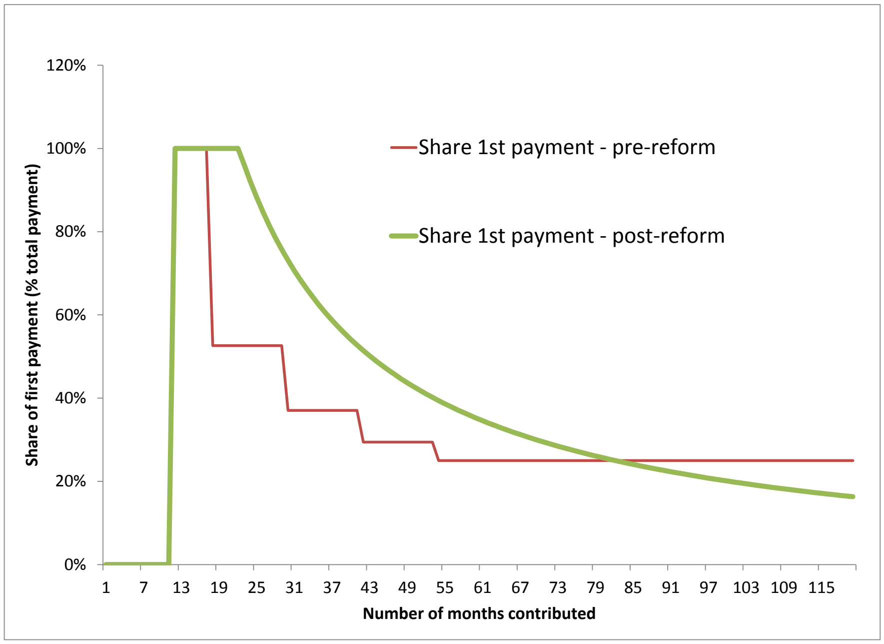


Figure 2.2c. Natural Experiment Around the Reform –share of first payment on total payment



Source: Simulations assuming a constant wage, a 2.2% contribution rate and 3% annual interest rate.

The previous figures were constructed from simulations from a representative worker. Figures 2.3 and 2.4 display the actual evolution of the number of scheduled benefits and the average share of the first payment, for individuals with different numbers of contributions. The particular focal points used the figures were chosen to represent the areas where, according to figure 2.2a, we would have expected the largest changes in the number of payments. Notice that we did not include a group with more than 82 months of contributions. The reason is that, by construction, these individuals reached that level after the 2009 reform (76 months after the creation of the program). For that reason, the reform should have an unambiguous effect on the number of payments for the individuals to be subject to our analysis.

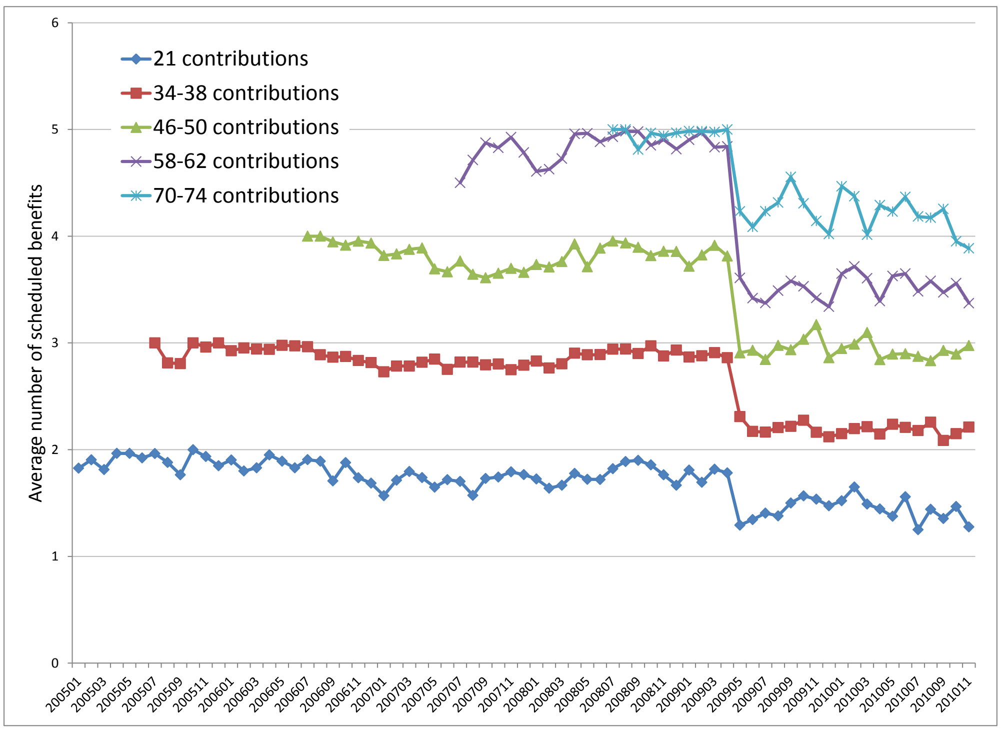
The results are exactly as predicted by the simulations: in all groups, the average number of payments went down after May 2009. At the same time, the share of the total payment represented by the first payment increased significantly, not only because the number of payments decreased but also because the benefits were front-loaded.

Our second natural experiment thus compares the reemployment behavior of similar workers who cashed their UA right before and right after the changes in the legislation.

It is worth noticing that the decrease was, in all cases, less than one. On one hand the pre-reform level is slightly lower than the predicted one: the group with 21 contributions has slightly less than 2 payments, the second group is below the 3 payments, etc. The reason for this is that, from the administrative data, we measure the total number of monthly contributions since the last benefit. However, the actual number of contributions used in the calculation of benefits is, in some cases, lower than this figure because of two particular regulations: one establishes that the contributions made to a previous employer for which the labor relationship was not officially ceased should not be counted towards the eligibility requirement or the calculation of benefits.[9] The second regulation establishes that only the contributions that had been officially credited in the account at the time of application can be considered in the calculation of benefits. This implies that a worker with 19 previous contributions could appear in the system with only 18 contributions at the time of application, therefore receiving only 1 payment. This could even apply to workers with 20 contributions if the employer had not yet paid the last two contributions.

Detailed information about the exact time when a contribution is credited is not currently available in the administrative data made available to researchers, making it difficult to know the exact number of contributions considered by the system when calculating benefits. This will be important in the empirical strategy presented later in this article.

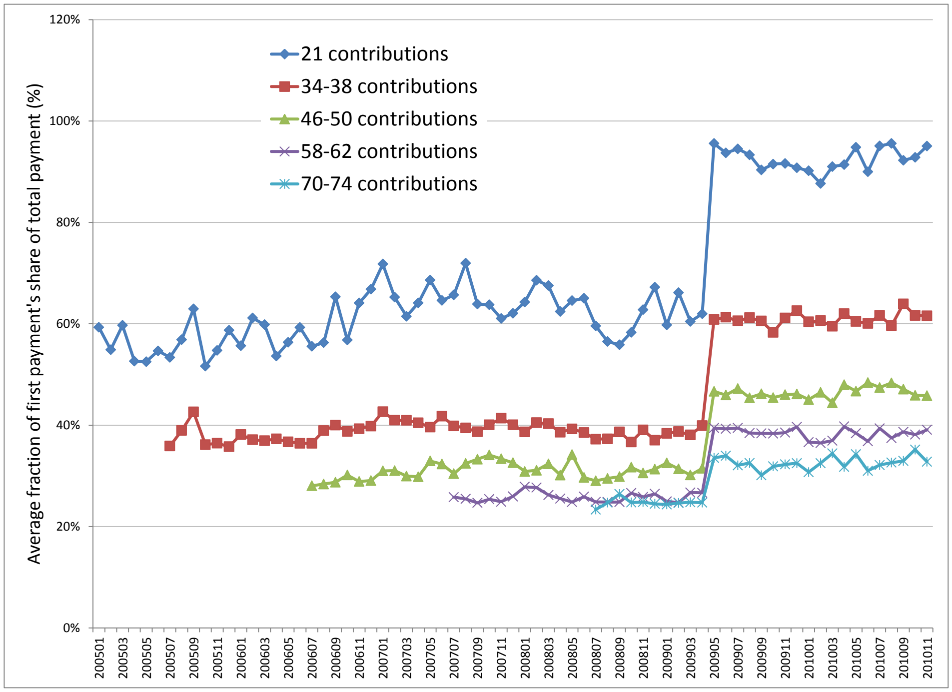
Figure 2.3. Average number of scheduled payments, individuals with different number of contributions



Source: authors' calculation based on administrative data.

[9] This regulation officially applied between July 2007 and April 2008. Its effect can be partially seen in figure 2.3, particularly for the third group.

Figure 2.4. Average first payment's share, individuals with different number of contributions



Source: authors' calculation based on administrative data.

In the following section we introduce a very simple model based on Card et al. (2006) to illustrate the predicted effects of these natural experiments on search behavior. Then we estimate the difference in reemployment behavior, taking advantage of the described natural experiments in the Chilean UI system.

3.- A simple model

Consider a worker who becomes unemployed at the beginning of period i=0 and is eligible for UI benefits. Time is discrete and lifetime lasts 3 periods. The worker spends periods i=0,1 in the labor market and is retired in period i=2. The length of unemployment depends upon search effort. That is, when unemployed in i=0,1 he chooses effort S_i, such that $S_i \in [0;1]$. For simplicity, we normalize search effort so the probability of finding a job in i is equal to S_i. There is no on the job search. Once a job is found, the worker earns an exogenous wage w and does not lose again his job until retirement.

Instantaneous utility in period i is given by $u(c_i) - \psi(S_i)$ with $u(\ )$ strictly concave and $\psi(\ )$ strictly convex. There is a unique fixed interest rate in the economy r. The worker discounts the future at factor β.

When employed, the worker contributes at a rate t to his mandatory UI savings account with balance B_i that accrues interest at the market rate. The first period benefit equals αB_i. If still unemployed, the second period benefit equals $(1-\alpha)B_{i+1}$. If he finds a job, the unused portion of the UI account can be cashed upon retirement.

The worker may also borrow and save at the market rate accumulating liquid assets A_i. If the worker faces liquidity constraints, then $A_i > L$. In general, the asset limit L may or may not be binding.
Therefore, when employed, the worker consumes $w(1-t)+A_i-A_{i+1}/(1+r)$. If unemployed, he consumes $\alpha B_i + A_i-A_{i+1}/(1+r)$ in the first unemployment period and $(1-\alpha)(1+r)B_i + A_{i+1}-A_{i+2}/(1+r)$ in the next. When retired, the worker consumes all remaining assets in the UI account B_2 (the unspent portion plus interest), all funds in his voluntary savings account A_2 and any exogenous pension *K*.

The worker must then choose voluntary savings and search effort, conditionally on being unemployed in i=0, that maximize his lifetime utility. Let c_i^E denote consumption in period i if employed, and let c_i^U denote consumption in period i if unemployed. Similarly, let $c_2^{a,b}$ denote consumption at retirement after experiencing state a (employed of unemployed) in period 0 and state b in period 1. Thus the worker chooses consumption and search effort in order to maximize

$$V(A_0, B_0) = S_0 u(c_0^E) + (1-S_0)u(c_0^U) - \psi(S_0) +$$

$$\beta[S_0 u(c_1^E) + (1-S_0)S_1 u(c_1^E) + (1-S_0)(1-S_1)u(c_1^U) - (1-S_0)\psi(S_1)] +$$

$$\beta^2\left[S_0 u\left(c_2^{E,E}\right) + (1-S_0)S_1 u\left(c_2^{U,E}\right) + (1-S_0)(1-S_1)u\left(c_2^{U,U}\right)\right]$$

subject to the budget constraints and the liquid asset limit.

Optimal search effort in period 0 follows the first order condition

$$\psi'(S_0^*) = [u(c_0^E) - u(c_0^U)] +$$

$$\beta\left[u(c_1^E) - [S_1 u(c_1^E) + (1-S_1)u(c_1^U) - \psi(S_1)]\right] +$$

$$\beta^2\left[u\left(c_2^{E,E}\right) - \left[S_1 u\left(c_2^{U,E}\right) + (1-S_1)u\left(c_2^{U,U}\right)\right]\right]$$

That is, the optimal search effort level in i=0 balances the marginal cost of increased effort and the additional utility due to a higher probability of being employed in the first period. A similar first order condition describes optimal effort in the second period if still unemployed.

We are interested in the effects on optimal search effort of front-loading unemployment benefits; i.e., the effects of increasing α at the optimum. It is easy to show that

$$\frac{dS_0^*}{d\alpha} = \frac{\left[-u'(c_0^U) + (1-S_1)\beta(1+r)u'(c_1^U) + S_1\beta^2(1+r)^2u'\left(c_2^{U,E}\right)\right]B_0}{\psi''(S_0)}$$

The first term of the equation shows that a rise in the fraction of the UA that is paid out in the first month of unemployment reduces the gain in finding employment right away according to the marginal utility of consumption in i=0 if unemployed. This effect reduces optimal search effort and is equivalent to the effect of raising current benefit generosity in a traditional UI system.

However, under a UA system, the reduced balance in the unemployment account implies lower future consumption. The remainder terms of the equation capture this counteracting effect. If still unemployed in period 1, an event with likelihood 1-S_1, the second period benefit is smaller with marginal cost $\beta(1+r)u'(c_1^U)$. In contrast, if he does find employment in period 2, there are lower resources left for consumption at retirement, at marginal cost of $\beta^2(1+r)^2u'\left(c_2^{U,E}\right)$. This latter event occurs with probability S_1. These two possibilities tend to raise first period effort.

If consumers are fully forward looking and do not face binding liquidity constraints, moving resources from the future to the present as α does, should not affect optimal effort. That is, these effects exactly cancel out as the numerator of the equation represents the Euler Equation of moving one dollar today when unemployed to the future. In this case, we should expect that otherwise identical workers choose the same search effort level no matter how much of the B_0 balance is paid out in the first period. In our data set, we should find no differences in reemployment probabilities.

However, if workers do face binding liquidity constraints, the search behavior will change as predicted by Chetty (2006). As a matter of fact, if the worker is currently liquidity constrained then

$$u'(c_0^U) > (1-S_1)\beta(1+r)u'(c_1^U) + S_1\beta^2(1+r)^2u'\left(c_2^{U,E}\right)$$

and $dS_0^*/d\alpha < 0$. That is, the marginal value of one extra dollar of consumption is much greater in the present than in the future, so loosening the liquidity restriction allows the worker to search with less intensity. If this is the case, in our data set we should find a lower likelihood of finding employment immediately.

It is interesting to note also that if the workers are either myopic (β tends to zero) or they do not internalize that the funds in the account are their own and do not expect to be able to spend the remaining balance in the future, then we should also expect a negative effect of front loading UI payments on initial search effort. This is equivalent to the negative incentive effects of UI studied in the traditional optimal UI literature.

4.- Data and descriptive statistics

4.1.- Data

For the empirical analysis, we use administrative data for a representative sample of participants in the Chilean Unemployment Insurance system.[10] The sample includes the entire history of contributions and benefits for a sample of 537,684 individuals, equivalent to approximately 7.8% of the entire population of contributors.

Our analysis is restricted to individuals who applied for benefits financed out of their individual accounts between January 2005 and December 2010 (approximately 69 thousand observations). In the case of the identification strategy based on the 2009 reform, the sample is further restricted to the period around the reform (January-August 2009). For the identification based on the cutoff points, the sample is restricted to the pre-reform period (January 2005-April 2009).

4.2.- The 2009 reform

The May 2009 reform implied a sudden change along two main directions: a change in eligibility conditions to access the Solidarity Fund and a change in the payment schedule for benefits financed out of the individual accounts.

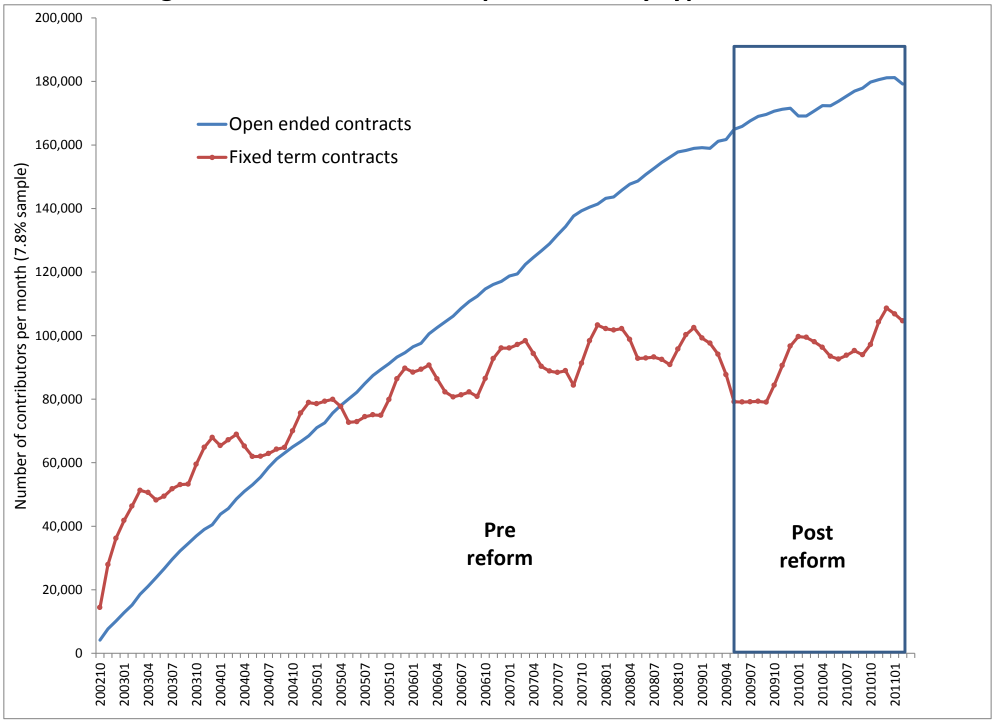
For the reform to provide a credible identification strategy, it is important to look at potential selection effects and, if present, provide a strategy to account for it.

The following figure shows the evolution of the number of contributors to the UI scheme per month, by type of contract (open ended or fixed term). To understand the evolution of contributors during this early period of the program, it is important to mention that workers are affiliated to the UI scheme only if they establish a new employment relationship after October 2002. This implies that workers with fixed term contracts were more likely to be affiliated in the early period (given their higher turnover), whereas workers with open ended contracts are gradually drawn into the program as they switch jobs. The continuous increase in the number of open ended contributors mostly reflects this gradual affiliation into the program.

We can see that the May 2009 reform coincides with a period of slowdown in the number of workers with a fixed term contract. In contrast, open ended contract workers (who are the focus of the analysis in this paper) do not experience a sharp change around the reform.

[10] The data base "Muestra De La Base De Datos De Afiliados Al Seguro De Cesantia" was constructed by the Chilean Pension Supervisor (www.spensiones.cl).

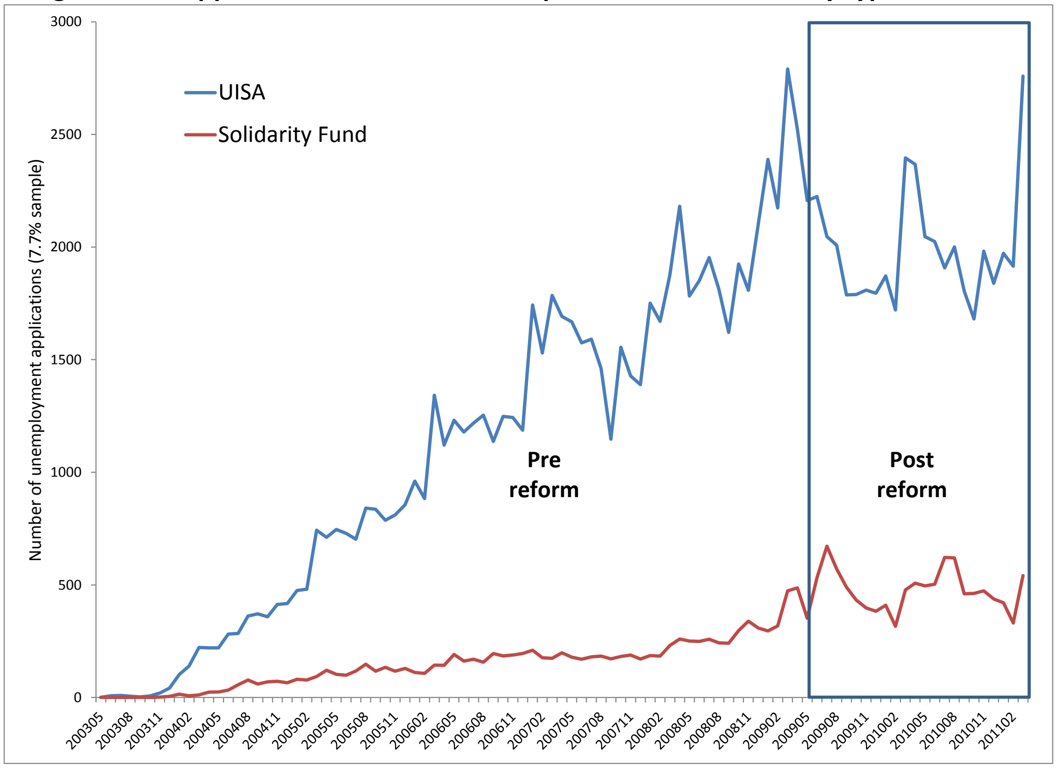
Figure 4.1 – UI Contributors per month, by type of contract



Source: authors' calculations based on administrative data.

Figure 4.2 shows the number of applications for unemployment benefits per month presented by workers with open ended contracts, according to the type of benefit; i.e., either financed entirely from the individual account, UISA, or following the Solidarity Fund option.

Figure 4.2 – Applications for UI benefits, open-ended contracts, by type of benefit



Source: authors' calculations based on administrative data.

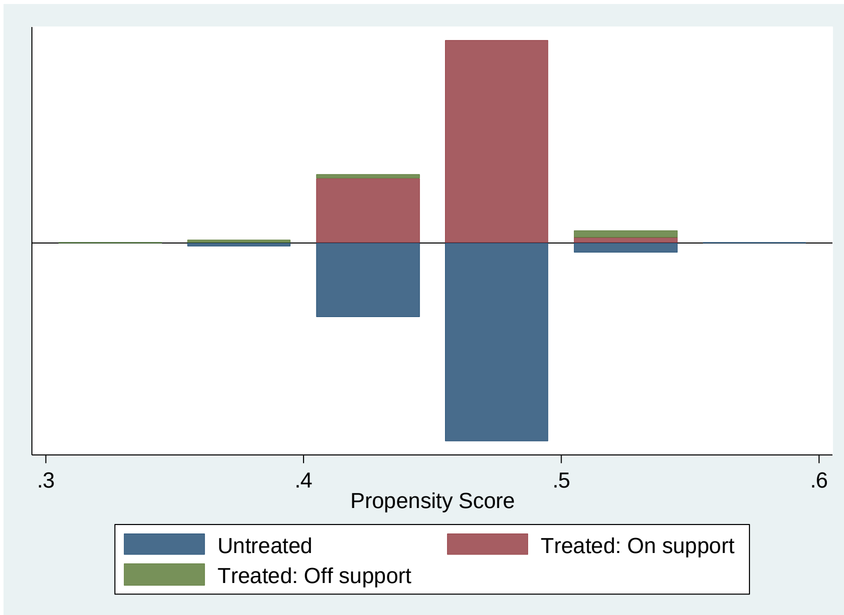
The empirical analysis presented here is based on the individuals with open ended contracts who applied for UISA benefits around the reform (4 months).[11] To analyze the effect of the reform on individuals with only 1 or two payments, the sample was restricted to individuals with at most 31 contributions.

The key identifying assumption is that the introduction of the reform is orthogonal to the characteristics of job losers. If the assumption is appropriate, individuals who become unemployed a few months before or a few months after the reform should be similar at least in terms of their observable characteristics. To make sure that the individuals are comparable, it is useful to first estimate and analyze the distribution of propensity scores for both groups of individuals.

[11] The original sample was constructed with careful consideration to eliminate applications that were denied or withdrawn, applications with an abnormal number of payments, etc. A few additional conditions were applied to eliminate outliers: individuals with average covered wage between US$100 and US$5000, a balance between US$80 and US$5000, and a total replacement rate greater than 25%. On the aggregate, these conditions implied losing 1% of the sample.

The following figure shows the propensity score distribution of individuals before and after the reform, including those who were considered to be off-support because of being in regions where the density of controls was below 5%.[12]

Figure 4.3 – Distribution of propensity score for individuals before (untreated) and after the reform (treated)



Source: authors' calculations based on administrative data on a 7.8% random sample of participants.

A first look at this graph shows that both distributions are well balanced.[13] To see this, the following table presents the normalized differences and t-statistics for the tests of difference in means for some of the key variables, for a sample restricted to the area of common support (2.26% of the sample was dropped).[14]

The first variable (duration>0) represents the basic outcome of interest, the probability that the individual has an unemployment spell longer than 0 (at least 1 month without contributions). A simple mean comparison would suggest that the reform did not change the average probability of

[12] The propensity score was estimated as a logit model with "reform" as the dependent variable and the following independent variables: age, gender, balance, income, total replacement rate, and number of contributions. The estimation results are presented in the appendix.

[13] A formal balancing test, including equality of means of all covariates within 7 blocks defined by the propensity score, was satisfied.

[14] The normalized difference corresponds to the difference in means divided by the squared root of the average sample variance between the two groups. Following the rule of thumb suggested by Imbens and Wooldridge (2009), a large normalized difference (above 0.25) implies that linear regression methods tend to be sensitive to the specification.

delaying employment in at least one month. The following 7 variables show a similar pattern for the probabilities of unemployment spells larger than 1, 2, etc.

The second and third variables (the number of payments and the share of the total potential payment to be paid during the first month) are related to the treatment implicit in the reform: as the benefit calculation rule is changed, the number of payments presents a statistically significant but small decrease, while the importance of the first payment increases sharply, from 76% to 91%. The other variables represent the pre-treatment covariates we use in the propensity score specification and to be used in the treatment effect estimations below. All the variables present a small normalized difference, suggesting that a simple OLS estimator should not be sensitive to the selected specification. The equality in means test is rejected for individuals before and after the reform in some of the variables: post reform individuals are slightly younger and with lower balance and wages than pre-reform individuals.

Table 4.1 – Mean comparison between individuals before (untreated) and after the reform (treated)

				Test for difference in means	
Variable	Mean (reform=0) N=4527	Mean (reform=1) N=3572	Normalized difference	T-stat	P-value
Duration >0	0.734	0.733	-0.004	-0.158	0.875
Duration >1	0.616	0.627	0.022	0.969	0.332
Duration >2	0.529	0.538	0.019	0.848	0.396
Duration >3	0.459	0.475	0.030	1.349	0.178
Duration >4	0.415	0.419	0.008	0.345	0.730
Number of payments	1.486	1.447	-0.077	-3.421	0.001
First payment share of total benefits	0.763	0.911	0.765	35.468	0.000
Age	34.386	33.715	-0.064	-2.881	0.004
1 if woman	0.329	0.323	-0.013	-0.59	0.555
UISA Balance (US$1000)	0.369	0.333	-0.112	-5.068	0.000
Covered wage (US$1000)	0.719	0.636	-0.151	-6.872	0.000
Total replacement rate	0.518	0.519	0.002	0.108	0.914
Contributions since last benefit	20.524	20.558	0.006	0.28	0.779

Source: authors' calculations based on administrative data on a 7.8% random sample of participants.

To ensure a balanced sample of pre and post reform individuals, a propensity score matching without replacement procedure was implemented to identify for every post reform individual the pre-reform worker with the closest characteristics (as summarized by the estimated propensity score). The resulting balanced sample presents the following normalized differences and t-statistics. The general idea is the same as before but in this case, there is no statistically significant difference in means in covariates (age, gender, etc.) between pre and post reform individuals. The results presented in the following section will be obtained from this balanced sample.

Table 4.2 – Mean comparison between individuals before (untreated) and after the reform (treated) – Balanced sample from PS matching without replacement

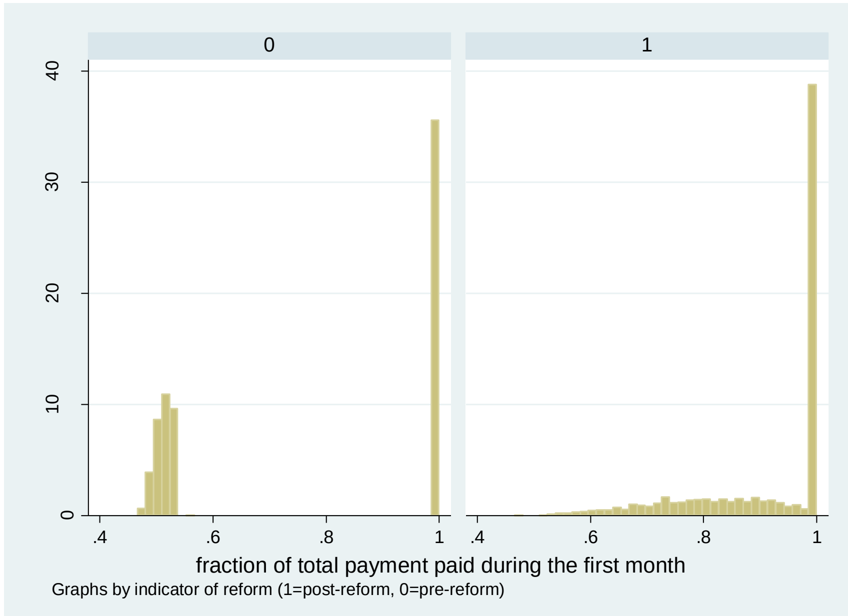
				Test for difference in means	
Variable	Mean (reform=0) N=3572	Mean (reform=1) N=3572	Normalized difference	T-stat	P-value
Duration >0	0.737	0.733	-0.009	-0.375	0.708
Duration >1	0.618	0.627	0.018	0.757	0.449
Duration >2	0.529	0.538	0.018	0.759	0.448
Duration >3	0.460	0.475	0.030	1.257	0.209
Duration >4	0.415	0.419	0.009	0.384	0.701
Duration >5	0.375	0.373	-0.004	-0.147	0.883
Duration >6	0.341	0.337	-0.009	-0.375	0.708
Duration >7	0.317	0.300	-0.038	-1.589	0.112
Number of payments	1.482	1.447	-0.070	-2.967	0.003
First payment share of total benefits	0.765	0.911	0.758	32.023	0.000
Age	33.865	33.715	-0.015	-0.614	0.539
1 if woman	0.325	0.323	-0.005	-0.202	0.840
UISA Balance (US$1000)	0.334	0.333	-0.002	-0.09	0.928
Covered wage (US$1000)	0.637	0.636	-0.002	-0.065	0.948
Total replacement rate	0.517	0.519	0.009	0.393	0.694
Contributions since last benefit	20.516	20.558	0.008	0.326	0.744

Source: authors' calculations based on administrative data on a 7.8% random sample of participants.

The previous table shows a significant difference in the share of the total payments represented by the first payment. This change is, however, heterogeneous within the population, as it is a function of the relationship between the balance and the average covered wage of the individual. To see this, the following figure presents the distribution of the "First payment share of total benefits" for the individuals before and after the reform included in the previous sample.

Before the reform, individuals are either paid their entire balance in one payment (share=100%) or in two payments, where the first one corresponds to the total balance divided by 1.9 (hence the two modes in 0.52 and 1). After the reform, the first payment is calculated as 50% of the average covered income of the individual and the second payment (if the balance in the account is greater than the previous amount) is equal to what remains in the account. We can see that in most cases, this calculation implies that the first payment is higher than the pre-reform rules and the second payment smaller.

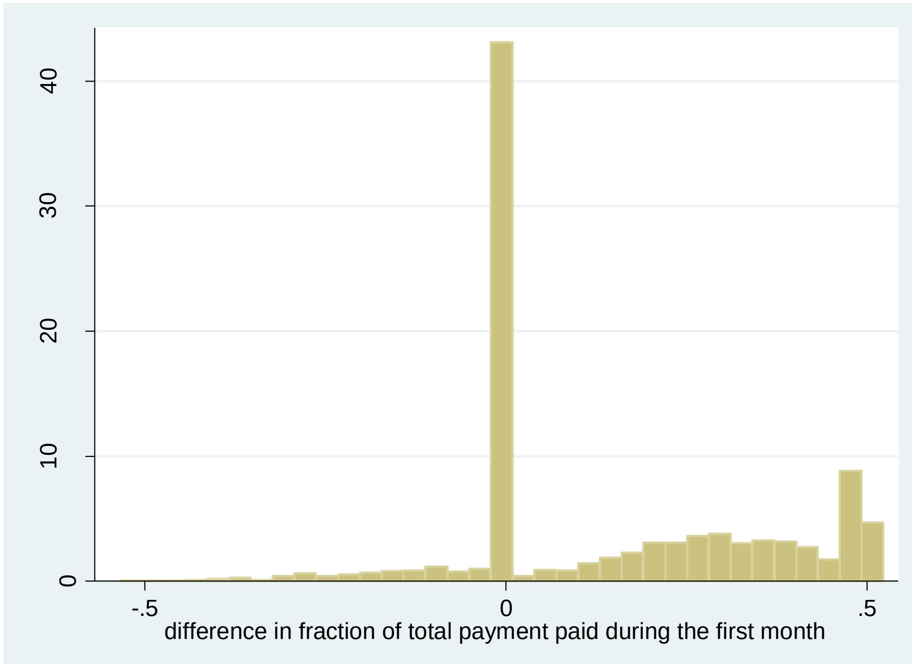
Figure 4.4 – Distribution of the "First payment share of total benefits", pre and post reform



Source: authors' calculations based on administrative data on a 7.8% random sample of participants.

A second observation for the post reform period is that, as before the reform, a large share of individuals are given 100% of their balance in one payment (as their balance is smaller than 50% of their average wage). This means that a fraction of the individuals in this group might have not been affected by the reform (if they receive 1 payment in both cases). Using matched pairs based on covariate-matching, we can construct the pseudo difference in the share of the first payment between the post-reform individuals and their pre-reform counterparts. The following figure presents the distribution of this difference. According to this calculation, approximately 43% of the individuals in this group were not affected by the reform. There is also a small fraction (8%) of individuals for which the reform may have reduced the relative importance of the first payment.

Figure 4.5 – Distribution of the difference in the "First payment share of total benefits", between the pairs identified by the matching procedure



Source: authors' calculations based on administrative data.

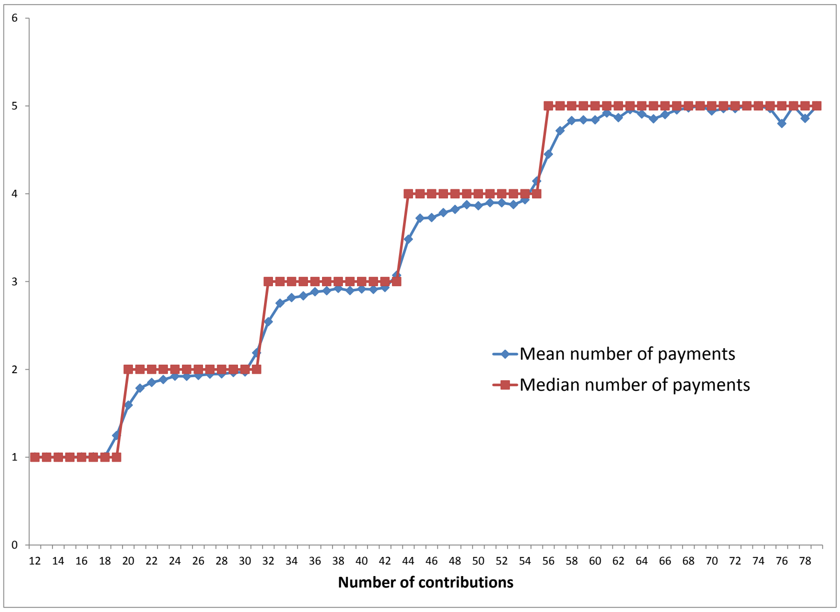
4.3.- The regression discontinuity approach

As described earlier, our second identification strategy exploits the discontinuous formula used before the 2009 reform to calculate the number and amount of UI benefits to be paid from the individual account.

As the following figures and the figures in section 2 show, the number of payments presents sharp discontinuities at different points of the distribution of the number of contributions. Consequently, the share of the total payment corresponding to the first payment presents discontinuities at the same points. As an example, the median share of the first monthly payment goes from 100% with 19 months to 52.5% with 20 months of contributions (from 1 to two payments).

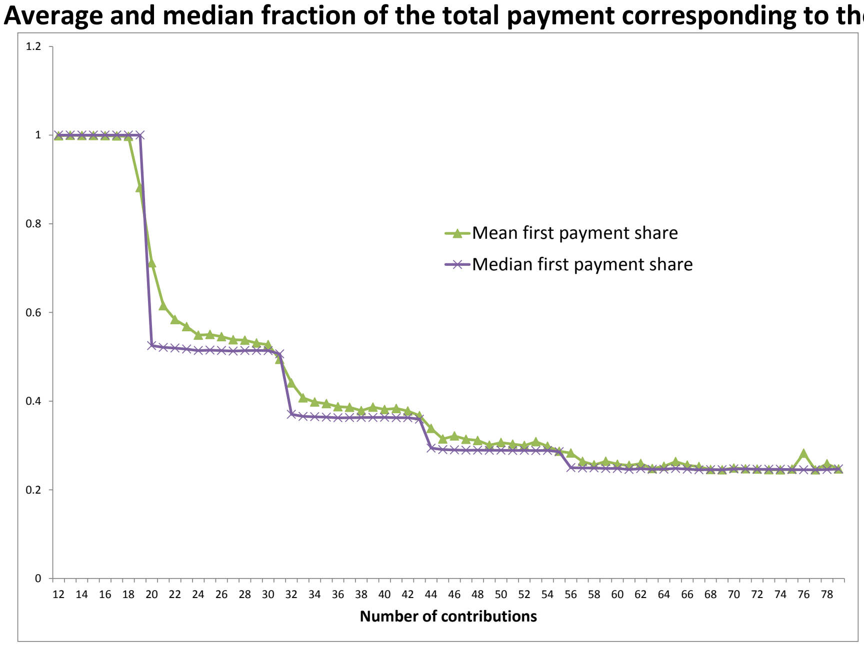
Two points are worth emphasizing from these figures: First, the sharp discontinuities marked by the median statistics occur with one month of delay with respect to the regulation. Second, individuals well above the cutoff points are receiving fewer payments than expected. In principle, with 19 months of contributions, all individuals should receive two payments. In the sample, however, only 25% of the individuals with 19 months are eligible for two payments. As mentioned earlier, there are two reasons for this phenomenon: one is that the 19th contribution might not yet be credited in the account when the person applied for benefits. The other is that during most of the period, a regulation established that employers must officially report ceased relationships to the Supervisor, in order for contributions to be included in the eligibility calculation. This helps to understand why there are individuals with far more than 19 contributions who are still receiving 1 payment. Unfortunately, the official status of previous employment relationships –whether it was formally ceased or not-- is not available on the administrative data to be able to perfectly predict the number of payments.

Figure 4.6 – Average and median number of payments as a function of the number of contributions



Source: authors' calculations.

Figure 4.7 – Average and median fraction of the total payment corresponding to the first month



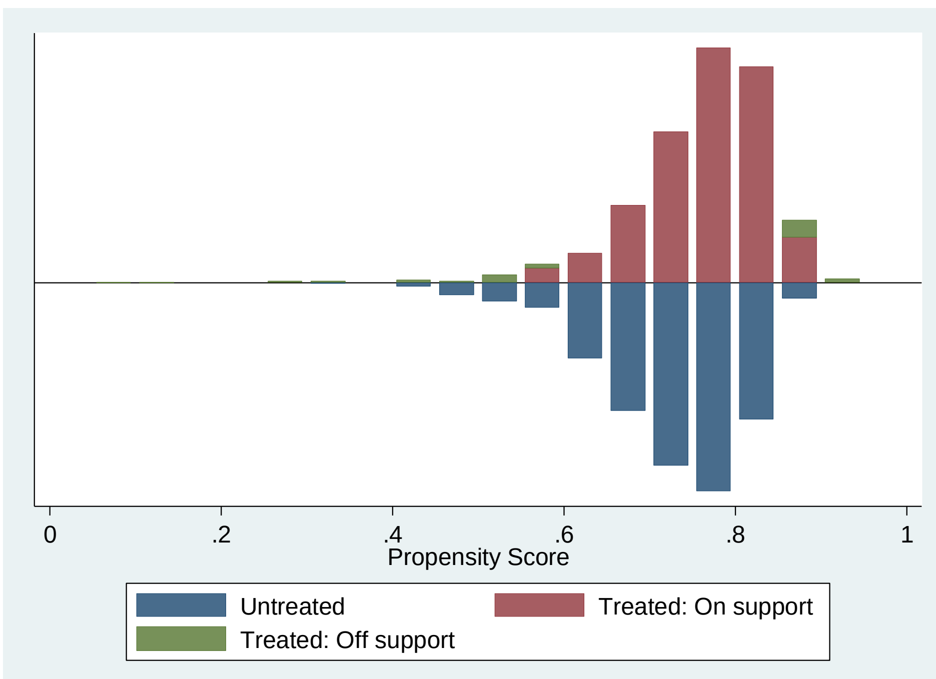
Source: authors' calculations.

As the largest discontinuity occurs around the first cutoff (at 19 months of contributions), our analysis focuses in this region. For consistency with the reform strategy, we define the treatment group as the workers who receive their entire balance in just one payment whereas the control group includes the individuals who are entitled to receive two payments.

To assess how similar are the individuals around this cutoff point, we first look at the distributions of the propensity score, defined as the predicted probabilities of receiving one payment. The following figure presents the distribution of predicted propensity scores for treated (one payment) and control individuals (two payments) within the group of individuals with exactly 19 months of contributions. The covariates used in the estimation of the propensity score were age, gender, balance, covered wage, total replacement rate, the month of application and the number of severance payments for which the person might be eligible.[15]

The graph shows that the sample is relatively well balanced between treated and control individuals, at least in terms of the distribution of propensity scores. The common support requirement tends to drop treated individuals on the two extremes of the distribution.

Figure 4.8 – Distribution of propensity score for individuals with two (untreated) or one payments (treated)



Source: authors' calculations.

[15] In general terms, a worker who is fired from his job for no cause of his own after at least 12 months of work under an open ended contract is entitled to one month of salary in severance pay. This payment goes up to two monthly salaries after 19 months of employment relationship. The basis for calculation is slightly different from the UISA payment, as this is a function of the number of contributions, either continuous or not. In addition, UISA benefits are payable to workers with 12 months of contributions, irrespective of the cause for separation.

Table 4.3 presents the normalized differences and test statistics for the equality of means for the characteristics used in the propensity score estimation. All but one of the covariates exhibits a normalized difference below 0.2. The exception is the total replacement rate, which tends to be smaller among the individuals with only 1 payment. The test statistics show that other variables are not necessarily well balanced between treated and controls.

Table 4.3 – Mean comparison between individuals with two (untreated) or one payments (treated)

				Test for difference in means	
Variable	Mean (reform=0) N=575	Mean (reform=1) N=1746	Normalized difference	T-stat	P-value
Duration >0	0.753	0.650	-0.226	-4.832	0.000
Duration >1	0.675	0.515	-0.329	-6.95	0.000
Duration >2	0.584	0.420	-0.333	-6.936	0.000
Duration >3	0.522	0.358	-0.334	-6.882	0.000
Duration >4	0.487	0.308	-0.373	-7.599	0.000
Age	32.965	33.543	0.057	1.189	0.235
1 if woman	0.369	0.292	-0.165	-3.372	0.001
UISA Balance (US$1000)	0.310	0.281	-0.112	-2.22	0.027
Covered wage (US$1000)	0.671	0.649	-0.039	-0.774	0.439
Total replacement rate	0.473	0.444	-0.362	-7.715	0.000
Month of application	65.226	66.662	0.096	1.967	0.049
Contributions with last employer	14.543	14.317	-0.046	-0.953	0.341
Severance payments	0.181	0.227	0.096	2.04	0.041

Source: authors' calculations.

Unlike the case of the reform strategy, restricting the sample to the common support region defined by the propensity score does not significantly improve the balancing between the two groups.

Table 4.4 – Mean comparison between individuals with two (untreated) or one payments (treated) – Balanced sample from PS matching without replacement

				Test for difference in means	
Variable	Mean (two payments) N=575	Mean (one payment) N=1659	Normalized difference	T-stat	P-value
Duration >0	0.753	0.646	-0.235	-4.973	0.000
Duration >1	0.675	0.510	-0.340	-7.14	0.000
Duration >2	0.584	0.417	-0.340	-7.031	0.000
Duration >3	0.522	0.356	-0.339	-6.939	0.000
Duration >4	0.487	0.304	-0.380	-7.694	0.000
Age	32.965	33.300	0.033	0.685	0.493
1 if woman	0.369	0.295	-0.156	-3.182	0.001
UISA Balance (US$1000)	0.310	0.274	-0.146	-2.851	0.004
Covered wage (US$1000)	0.671	0.634	-0.065	-1.284	0.199
Total replacement rate	0.473	0.440	-0.451	-9.046	0.000
Month of application	65.226	66.471	0.083	1.696	0.090
Contributions with last employer	14.543	14.335	-0.043	-0.871	0.384
Severance payments	0.181	0.215	0.072	1.509	0.131

Source: authors' calculations.

5.- Methodology

5.1.- Econometric models for the 2009 reform

As seen in the previous section, the 2009 reform implied a heterogeneous treatment in the sense that the change in the benefit calculation rule implied a different shift in the importance of the first payment, depending on the relationship between the balance and the average wage of the individual. In fact, the reform did not change the benefit structure for a large portion of the individuals who would have received 1 or 2 payments without the reform.

As justified by our conceptual framework, we will focus initially on the probability of leaving unemployment during the first few months, as these probabilities are associated with the level of effort exerted by the individual to find a new job.

Under the simplest specification, we estimate a series of linear probability models where the dependent variable is equal to 1 if the individual does not leave unemployment during the first month of her unemployment spell. These models take the form

$$\{U.Duration_{it} > 0\} = \alpha + \beta \cdot PostReform_{it} + X_i'\gamma + \delta \cdot t + \varepsilon_i$$

where the variable $PostReform_i$ is a dummy variable equal to 1 if the individual applied for benefits in the post-reform period. The vector of individual characteristics (X_i') includes gender, age, balance, wage, total replacement rate and the observed number of contributions. The variable t corresponds to a linear trend. The sample is restricted to individuals who applied for benefits within a 4 months window around the beginning of the reform (January-August 2009) who are included in the common support of the propensity score distribution. In some of the models, we will further restrict the sample to individuals who, according to the matching procedure described in the previous section, would have been affected by the reform (individuals whose initial payment would have been different if they applied before or after the reform).

The same model is estimated using as dependent variable a binary indicator of whether the unemployment spell was longer than 1, 2, 3 and 4 months.[16]

A second set of estimates for the average effect of the reform can be obtained through a matching procedure. Post reform individuals are matched with pre-reform individuals based on the same set of covariates (X_i'). This procedure provides a new estimate, constructed as the average difference in the probabilities of leaving unemployment between pre and post reform individuals.

$$\widehat{\beta_M} = \frac{1}{N_1} \sum_{i \in PostReform} \left(Y_i - Y_{nn(i)}\right)$$

where Y_i is the same dependent variable as in the previous models and $nn(i)$ represents the nearest neighbor of the individual *i* among the pre-reform individuals, based on the minimum distance between their vector of covariates:

$$nn(i) = arg \min_{j \in PreReform} \left\|X_i - X_j\right\| = \min_{j \in PreReform} \left(X_i - X_j\right)' A \left(X_i - X_j\right)$$

where the matrix A corresponds to the Mahalanobis metric, constructed as the inverse of the sample variance-covariance matrix of the vector of covariates.[17]

The previous models provide an estimate for the average effect of the reform, associated with a change in the benefit calculation. A more precise estimate of the elasticity of search effort with respect to the fraction of the total payment associated with the first month. The matching procedure provides the opportunity to look directly at the effect of the change in the first payment share on the duration probabilities. This is estimated as a simple linear model expressed in differences between the post reform individuals and their pre reform neighbors.

$$\left(Y_i - Y_{nn(i)}\right) = \alpha + \beta \cdot \left(W_i - W_{nn(i)}\right) + \left(X_i - X_{nn(i)}\right)' \gamma + \varepsilon_i$$

Finally, to exploit the entire distribution of unemployment durations, some of which are censored due to the restricted scope of the observation window, we estimate a parametric duration model

[16] One concern with exploring outcomes further down the spell distribution is related to the February 2010 earthquake which may have changed significantly the labor prospects of the unemployed.

[17] Following Abadie and Imbens (2011), we use a regression based bias correction to the simple matching estimator presented here.

for the unemployment spell, including a reform dummy variable to test whether the hazard rate from unemployment is different between pre and post reform individuals.

More specifically, we estimate a parametric duration model assuming that the unemployment duration follows a Weibull distribution function, a common choice for unemployment duration models.

<u>5.2.- The regression discontinuity approach</u>

Most of the empirical strategies described for the reform analysis are valid in the context of the discontinuity created by the number of contributions. We estimate linear probability models for the probability that an unemployment spell is longer than 0 months, as a function of the treatment dummy (1 if individual receives one payment and 0 otherwise) and covariates. The initial models are estimated on a sample of individuals with exactly 19 contributions, some of which received one payment while the others, two payments.

$$\{U.Duration_{it} > 0\} = \alpha + \beta \cdot OnePayment_{it} + X_i'\gamma + \delta \cdot t + \varepsilon_i$$

We also estimate the bias corrected matching estimator of Abadie and Imbens (2011).

$$\widehat{\beta_M} = \frac{1}{N_1} \sum_{i \in OnePayment} \left(Y_i - Y_{nn(i)}\right)$$

The main difference with the previous identification strategy is that, there is no heterogeneity in the treatment intensity as the distinction between 1 and 2 payments implies a constant difference in the share of the total payment to be received during the first month (from 52% to 100%). For this reason, it will not be necessary to estimate the differenced model described in the previous section.

A traditional regression discontinuity approach would use the cut-off point of 18 contributions as measure of the treatment effect. In this case, we observed that some of the individuals with more than 18 contributions still received only one payment, due to some normative regulations regarding previous employment relationships and the formal crediting of the last contribution. In other words, this is a case of a regression discontinuity problem with a fuzzy design (the probability of being treated jumps at the threshold).

Consistently, we implement a two stage least squares approach with observations around the cutoff point (18-19 contributions), using an indicator of having contributed more than 18 months as an instrument for the receipt of only payment (instead of two). This can be represented by the following two equations, where n_i represents the observed number of contributions.

$$Second\ stage: \quad \{U.Duration_{it} > 0\} = \alpha + \beta \cdot OnePayment_{it} + X_i'\gamma + \delta \cdot t + \theta n_i + \varepsilon_i$$

$$First\ stage: \quad OnePayment_{it} = \alpha_2 + \beta_2 \cdot (n_i > 18) + X_i'\gamma_2 + \delta_2 \cdot t + \theta_2 n_i + \vartheta_i$$

6.- Results

6.1.- Reform strategy

The following table presents the results from the linear probability models for the probability that the unemployment spell is greater than 0. The models differ in the particular estimation sample: In the first model, the sample is restricted to the region of common support (based on the propensity score). In the second model, post reform individuals are matched on the propensity score without replacement to pre-reform individuals, and the model is estimated with the resulting balanced sample. In the third model, matching is done with replacement, based on the whole vector of covariates. The fourth model is a restricted version of the third, in which only individuals for which the reform should have an effect are included.

In all the cases, the estimated effect is small and not statistically significant. In fact, only the age is a consistently significant predictor of the first month unemployment hazard; older individuals are more likely to exit unemployment during the first month.

Table 6.1 – OLS results based on linear probability models – DV=duration greater than 0

	Dependent Variable: 1{Duration >0}			
VARIABLES	(1)	(2)	(3)	(4)
indicator of reform (1=post-reform, 0=pre-reform)	0.0139	0.00403	0.0212	0.0142
	(0.0198)	(0.0210)	(0.0233)	(0.0335)
Age	-0.000987**	-0.00122**	-0.00172***	-0.00224***
	(0.000474)	(0.000509)	(0.000569)	(0.000861)
1 if woman	0.0121	0.00791	-0.00313	-0.0246
	(0.0105)	(0.0112)	(0.0124)	(0.0181)
UISA Balance (US$1000)	-0.0917	0.0678	-0.0172	-0.267*
	(0.0605)	(0.0797)	(0.0891)	(0.144)
Covered wage (US$1000)	0.0126	-0.0955**	-0.0590	0.100
	(0.0326)	(0.0470)	(0.0514)	(0.0904)
Total replacement rate	-0.00261	-0.131*	0.0113	0.242*
	(0.0610)	(0.0743)	(0.0867)	(0.129)
Number of contributions	-0.000320	0.000935	-0.00235	-0.00165
	(0.00142)	(0.00154)	(0.00179)	(0.00307)
Monthly trend	-0.00473	-0.00207	-0.00609	-0.00554
	(0.00450)	(0.00480)	(0.00531)	(0.00756)
Constant	0.785***	0.857***	0.865***	0.730***
	(0.0334)	(0.0387)	(0.0430)	(0.0887)
Sample	Common support	PS matching without replacement	Covariate matching with replacement	Covariate matching with replacement, Delta W>0
Observations	8,099	7,144	5,847	2,851
R-squared	0.004	0.006	0.009	0.011
Robust standard errors in parentheses				
*** p<0.01, ** p<0.05, * p<0.1				

Source: authors' calculations.

Table 6.2 presents the estimated reform coefficients and standard errors for the same types of models but applied to different parts of the duration distribution. The fifth model corresponds to the bias corrected matching estimator.

The general conclusion is the same: the reform is correlated with a higher probability of a longer duration spell. This result is consistent with the predictions of our stylized models: as payments become front loaded, individuals put less effort into searching for a new job. Alternatively, individuals do not foresee that the unused portion of their UA can be used in the future. However, except for one case, all estimators turn out to be not statistically significant. This result is consistent with the hypothesis that the UA has no adverse incentive effects and at the same time, Chilean workers contributing in the UI system face no liquidity constraints.

Table 6.2 – OLS results based on linear probability models – DV=multiple cutoff points

	Estimating sample				
	Common support	PS matching without replacement	Covariate matching with replacement	Covariate matching with replacement, Delta W>0	Covariate matching estimator
DV=1{Duration >0}	0.0139	0.00403	0.0212	0.0142	-0.0107
	(0.0198)	(0.0210)	(0.0233)	(0.0335)	(0.0115)
DV=1{Duration >1}	0.0317	0.0262	0.0524**	0.0353	0.0052
	(0.0217)	(0.0230)	(0.0255)	(0.0366)	(0.0126)
DV=1{Duration >2}	0.0210	0.0264	0.0387	0.0317	-0.0025
	(0.0223)	(0.0237)	(0.0262)	(0.0375)	(0.0129)
DV=1{Duration >3}	0.0187	0.0255	0.0310	0.0388	0.0051
	(0.0223)	(0.0237)	(0.0262)	(0.0376)	(0.0127)
DV=1{Duration >4}	0.00834	0.0147	0.0190	0.0304	-0.0030
	(0.0220)	(0.0234)	(0.0260)	(0.0371)	(0.0125)

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Source: authors' calculations.

The following table presents the results from the covariate-matching approach for the model in differences between the post-reform individuals and their pre-reform counterparts. The first model, with the binary variable "duration greater than zero" is estimated using the matched sample. The second model is restricted to cases where the difference in the first period payment is greater than 0. The structure is repeated for the other models.

The results in Table 6.3 are consistent with those of Tables 6.1 and 6.2. That is, if benefits are front loaded (a negative difference in the fraction of total payment received during the first month of unemployment), the probability of a longer unemployment spell rises. However, once again, the results show no evidence of a statistically significant difference in the search behavior of pre and post reform individuals.

Finally, Table 6.4 presents the results from two duration models based on a parametric specification assuming a Weibull distribution function for the unemployment duration process.

The first model uses all the individuals in the sample with the actual unemployment durations, only censored in those cases where the individual had not presented contributions by the end of the observation window. To address the concerns of February 2010 earthquake which might have affected the unemployment prospects of individuals, the second model censors all durations at a maximum of 6 months. In both cases, the reform dummy is not statistically significant. This time, however, the estimated coefficients are negative.

Table 6.3 – OLS models in difference between post and pre reform matched individuals

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Duration > 0		Duration > 1		Duration > 2		Duration > 3		Duration > 4	
VARIABLES	All	Delta W >0	All	Delta W >0	All	Delta W >0	All	Delta W >0	All	Delta W >0
difference in fraction of total payment paid during the first month	-0.0358	-0.128	-0.0385	-0.157	-0.0585	-0.179	-0.0193	-0.154	-0.0339	-0.0766
	(0.0488)	(0.116)	(0.0540)	(0.128)	(0.0559)	(0.131)	(0.0553)	(0.132)	(0.0542)	(0.132)
Age difference	-0.00453	-0.00225	-0.00545	-0.00906	-0.00596	-0.00606	-0.000207	-0.00184	-0.000120	0.00214
	(0.00549)	(0.00726)	(0.00588)	(0.00781)	(0.00571)	(0.00763)	(0.00578)	(0.00775)	(0.00562)	(0.00744)
Balance difference	-0.215	-1.689	-1.064	-2.498**	-1.359	-2.356*	-1.503*	-1.660	-1.442*	-1.837
	(0.801)	(1.138)	(0.886)	(1.239)	(0.873)	(1.214)	(0.881)	(1.238)	(0.869)	(1.227)
Income difference	0.137	0.998	0.617	1.595**	0.610	1.202	0.821	0.917	0.697	0.905
	(0.449)	(0.688)	(0.497)	(0.741)	(0.501)	(0.741)	(0.501)	(0.750)	(0.494)	(0.741)
total replacement rate difference	0.237	0.826	1.019	1.622	1.396	1.797	1.318	1.328	1.663*	1.678
	(0.817)	(1.134)	(0.902)	(1.226)	(0.900)	(1.230)	(0.913)	(1.262)	(0.895)	(1.241)
Difference in number of contributions	0.00658	0.00488	0.00618	-0.00534	-0.0116	-0.00756	-0.0105	-0.00664	-0.0143	-0.0137
	(0.0159)	(0.0206)	(0.0176)	(0.0227)	(0.0174)	(0.0228)	(0.0176)	(0.0230)	(0.0173)	(0.0225)
Constant	0.000505	0.0380	0.0161	0.0661	0.0163	0.0674	0.0173	0.0771	0.0141	0.0399
	(0.0125)	(0.0418)	(0.0138)	(0.0458)	(0.0142)	(0.0467)	(0.0143)	(0.0475)	(0.0141)	(0.0473)
Observations	3,572	1,744	3,572	1,744	3,572	1,744	3,572	1,744	3,572	1,744
R-squared	0.001	0.002	0.001	0.004	0.002	0.004	0.001	0.002	0.001	0.002

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Source: authors' calculations.

Table 6.4 – Weibull model for unemployment duration

VARIABLES	(1) Censored in december 2010	(2) Censored at 6 months
indicator of reform (1=post-reform, 0=pre-reform)	-0.0166	-0.0131
	(0.0581)	(0.0712)
Age	-0.00924***	-0.00161
	(0.00141)	(0.00172)
1 if woman	-0.522***	-0.449***
	(0.0322)	(0.0405)
UISA Balance (US$1000)	0.00470	0.0137
	(0.166)	(0.198)
Covered wage (US$1000)	0.0458	0.0756
	(0.0901)	(0.108)
Total replacement rate	0.0849	0.238
	(0.174)	(0.214)
Number of contributions	0.00142	-0.00464
	(0.00407)	(0.00502)
Monthly trend	0.0163	0.00545
	(0.0132)	(0.0163)
Constant	-1.694***	-2.414***
	(0.0995)	(0.124)
Observations	5,943	5,943

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Source: authors' calculations.

6.2.- Regression discontinuity approach

The following table summarizes the results from the linear probability models estimated on a sample of individuals with exactly 19 contributions, some of which receive 1 payment while others receive two payments.

In all the specifications, the results suggest that receiving one payment reduces the probability of extending the unemployment spell. In other words, it increases the speed at which individuals find jobs. The effect seems to be present in at least the first 4 months of the unemployment spell. The results from the bias corrected matching estimator (available upon request) were essentially the same as in table 6.5.

These results are inconsistent with those of the pre and post reform experiment and with the predictions of our simplified model. It is possible that having contributed for 19 months but being eligible for different number of payments could be related to characteristics of previous employment relationships that were not officially reported as finished by the employer.

To check this, table 6.6 summarizes the results of the instrumental variable approach where a discrete variable capturing whether an individual is above or below the 18 months cutoff point is used as an instrument for the treatment variable (1 payment instead of two payments). Three sets of results are presented, using in each case windows of different size around the cutoff point (1 month before and after, 2 months before and after and 3 months before and after).

The results on the probability of not leaving unemployment during the first month are even stronger than the previous specification; in the base case, individual with only one payment showed 15.5 percentage points lower probability of not leaving unemployment (they find jobs at a higher rate) with respect to individuals with two payments.

It is interesting to note that in the tighter specification – only one month around the cutoff point- the effects are only statistically significant during the first two months, which is consistent with an incentive effect that occurs only while the individual is receiving benefits. The wider window specification – three months around the cutoff point – shows a persistent effect, possibly reflecting unobserved differences between individuals with histories that are further apart.

Table 6.5 – Linear probability models for duration probabilities – regression discontinuity design

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Duration > 0		Duration > 1		Duration > 2		Duration > 3		Duration > 4	
VARIABLES	Common support	Covariate matching with replacement	Common support	Covariate matching with replaceme nt	Common support	Covariate matching with replacemen t	Common support	Covariate matching with replaceme nt	Common support	Covariate matching with replacemen t
1 if one payment	-0.112***	-0.112***	-0.160***	-0.160***	-0.156***	-0.153***	-0.155***	-0.150***	-0.171***	-0.166***
(0 if two payments)	(0.0219)	(0.0231)	(0.0237)	(0.0250)	(0.0246)	(0.0260)	(0.0245)	(0.0259)	(0.0243)	(0.0257)
Age	-0.000496	-0.000455	0.000381	0.000254	0.000116	-5.84e-05	0.00111	0.000993	0.000744	0.000713
	(0.00101)	(0.00104)	(0.00108)	(0.00110)	(0.00108)	(0.00110)	(0.00106)	(0.00109)	(0.00103)	(0.00106)
1 if woman	0.0359*	0.0423*	0.0752***	0.0851***	0.111***	0.123***	0.117***	0.126***	0.111***	0.118***
	(0.0211)	(0.0219)	(0.0225)	(0.0233)	(0.0228)	(0.0235)	(0.0226)	(0.0234)	(0.0222)	(0.0229)
UISA Balance (US$1000)	-0.789**	-0.869**	-0.362	-0.353	0.0777	0.139	-0.159	-0.158	-0.387	-0.385
	(0.383)	(0.442)	(0.393)	(0.458)	(0.379)	(0.439)	(0.379)	(0.431)	(0.373)	(0.415)
Covered wage (US$1000)	0.333**	0.365*	0.173	0.173	-0.0344	-0.0547	0.0728	0.0738	0.184	0.185
	(0.165)	(0.187)	(0.170)	(0.195)	(0.163)	(0.185)	(0.163)	(0.182)	(0.161)	(0.176)
Total replacement rate	0.299	0.316	0.217	0.225	0.0928	0.0345	0.228	0.192	0.337	0.262
	(0.238)	(0.267)	(0.252)	(0.283)	(0.250)	(0.283)	(0.248)	(0.278)	(0.244)	(0.270)
Monthly trend	0.000657	0.000667	0.000585	0.000571	0.000251	0.000341	0.000592	0.000827	0.000466	0.000605
	(0.000664)	(0.000694)	(0.000710)	(0.000742)	(0.000723)	(0.000753)	(0.000716)	(0.000744)	(0.000706)	(0.000732)
Number of severance payments	0.0188	0.00666	0.00401	-0.00487	0.0119	0.00491	0.0112	0.00498	-0.00309	-0.0110
	(0.0215)	(0.0225)	(0.0223)	(0.0233)	(0.0222)	(0.0229)	(0.0219)	(0.0225)	(0.0212)	(0.0217)
Constant	0.590***	0.583***	0.489***	0.487***	0.476***	0.493***	0.294**	0.291**	0.229*	0.247*
	(0.118)	(0.129)	(0.125)	(0.137)	(0.125)	(0.136)	(0.123)	(0.134)	(0.121)	(0.130)
Observations	2,234	2,133	2,234	2,133	2,234	2,133	2,234	2,133	2,234	2,133
R-squared	0.014	0.014	0.027	0.026	0.033	0.031	0.036	0.033	0.041	0.036

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Source: authors' calculations.

Table 6.6 – Two stage least squares models for duration probabilities – regression discontinuity design
Summary of treatment effect estimates for different windows

	Duration>0	Duration>1	Duration>2	Duration>3	Duration>4	Observations	Partial R2 First Stage
Window of 18-19 contributions	-0.153***	-0.129**	-0.0864	-0.0989	-0.0719	4,748	0.152
	(0.0593)	(0.0620)	(0.0618)	(0.0603)	(0.0587)		
Window of 17-20 contributions	-0.468*	-0.277	-0.180	-0.259	-0.149	9,384	0.318
	(0.265)	(0.270)	(0.268)	(0.262)	(0.254)		
Window of 16-21 contributions	-0.221**	-0.237**	-0.199*	-0.236**	-0.191*	13,968	0.439
	(0.110)	(0.115)	(0.114)	(0.112)	(0.109)		

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Instrument for treatment effects variable = Indicator equal to 1 if the individual had more than 18 contributions at the moment of application.
Other covariates: Same as in table 6.5 plus a variable measuring the number of contributions (except for the 18-19 window results).

7.- Concluding remarks

Do savings accounts based UI systems provide better reemployment incentives than traditional UI systems? Is observed search behavior a result of substitution or liquidity effects? The answer to these and other related questions are relevant in designing optimal unemployment insurance and more generally, in designing optimal social insurance systems.

The Chilean UI system, based on a mix of self and social insurance, provides an opportunity to analyze these questions. In this paper, we take advantage of two natural experiments that front loaded the benefits that unemployed workers perceive, without changing the expected present value of total benefits. According to a stylized search theoretic model, these experiments should have no effects on the reemployment behavior of workers unless they are liquidity constrained. If so, front loaded payments provide valuable cash-on-hand that allows the worker to search less intensively. This is an efficient response, different from the negative incentive effects of providing benefits only as the worker remains unemployed (Chetty, 2006). Still, in the context of a UA based system, this liquidity effect may be confused with the traditional negative incentive effects whenever workers are either myopic or do not fully understand that the unused funds in the account will be cashed in the future.

Our estimated results are mixed. Our preferred experiment, the one that compares reemployment outcomes before and after the 2009 reform, shows that front loading the benefits is correlated with a less intense search effort. However, most of these results are not statistically significant, possibly implying both, that UA based systems are better shielded from negative incentive effects and that at the same time, workers under analysis are not restricted by liquidity constraints.

The alternative experiment provides the opposite results, however. One potential explanation for this differences could be that the two different strategies identify different objects; the reform strategy identifies the average effect of front loading at the intensive margin (increasing the importance of the first payment but essentially keeping the number of payments unchanged) for a broad group of individuals (with contributions between 12 and 31 months). The regression discontinuity strategy identifies the average effect of front loading at the extensive margin (changing the number of payments, from two to one, and greatly increasing the importance of the first payment) for a narrower group of individuals: the compliers with the instrument at the 18-19 contributions margin, i.e. the individuals for whom the 19^{th} contributed month actually makes a difference between receiving one or two months. Future drafts will inquire into the effect at different points of the distribution of months contributed, given the cutoff points at 30, 42 and 54 months.

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Appendix 1 – Propensity Score specification – Logit models for the two identification strategies: post reform v/s pre reform, 1 payment v/s two payments (19 months)

	Reform strategy	Cutoff strategy
VARIABLES	DV=reform	DV=1 payment (instead of two)
Age	-0.00433**	0.0102**
	(0.00217)	(0.00516)
1 of woman	-0.0506	-0.377***
	(0.0477)	(0.104)
UISA Balance (US$1000)	0.0258	-2.226
	(0.251)	(1.423)
Covered wage (US$1000)	-0.0820	0.677
	(0.135)	(0.631)
Total replacement rate	0.425*	-3.333***
	(0.249)	(0.940)
Contributions since last benefit	-0.00533	
	(0.00607)	
Month of application		0.0107***
		(0.00336)
Number of severance payments		0.187*
		(0.111)
Constant	-0.0856	1.886***
	(0.143)	(0.496)
Observations	8,286	2,321

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1