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Rafael Sánchez

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Do reductions of standard hours affect employment transitions?: Evidence from Chile*

Rafael Sánchez
University of Warwick

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Abstract

This study exploits the reduction of weekly working hours from 48 to 45 occured in Chile in January 2005. We use this pure and exogenous policy change to identify the employment effects of such a policy. Our main contribution is that we overcome the problems of previous studies such as: selection between hours and employment, lack of identification strategy due to the joint implementation of policies and lack of crucial variables (like hourly wages and usual hours). Our results suggest no significant effects of a reduction of standard hours on employment transitions and a significant effect on hourly wages (i.e. wage compensation). These results are robust to several specifications.

1 Introduction

Several countries like France, Germany, Portugal, Netherlands, Belgium, Spain, Italy, United Kingdom among others have implemented and (or) discussed policies to reduce (maximum) standard working hours.[1] Some of them due to the European Working Time Directive (1993)

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[1]The maximum standard working hours are define as the maximum number of hours above which employers have to pay overtime. For simplicity, we follow the convention in the literature and we will refer to the maximum standard working hours as "standard hours", "basic hours" or "normal hours". This may be misleading since it could be the case that, because of contractual characteristics, some employees are paid overtime even when they work less than the maximum number of standard hours. We will return to this point later, but for the moment we rule out this case.

but others have made earlier reductions mainly to tackle high rates of unemployment. The rationale for this policy is that, for a given output, a reduction of standard hours will decrease the total usual (average) hours worked by employees and therefore it will be necessary to hire new workers. However, when firms use overtime, a reduction of standard hours will increase the relative marginal cost of employment relative to the marginal cost of hours leading to a negative effect on employment and an increase in overtime hours. Therefore the demand for hours by firms, before the reduction of standard hours, will be crucial for the final effect on employment (Calmfors and Hoel 1988 and Hamermesh 1993, among others).

Furthermore, the overall effect of the reduction of standard hours on employment will also hinge on the reaction of monthly earnings. If monthly earnings remain constant and usual hours decrease due to the reduction of standard hours then hourly wages will increase. Firms will then substitute capital for labour with a negative effect on employment and usual hours (Hunt 1999). Moreover, if firms adjust the level of output, there will be an additional negative scale effect to be added to the previous effects. Given this ambiguity in the theoretical predictions, the effect of a reduction of working hours on employment remains an empirical question.

The literature has pointed out that a change in standard hours may also affect the composition (mix) of employment.[2] In particular, Hart (2004) shows that a reduction of standard hours increases the relative cost of full-time workers, inducing firms to increase the share of part-time workers which might attenuate the substitution towards hours.

Unfortunately there are few studies with micro-econometric evidence about the effect of working hours on employment, almost all of them in Europe (see Hunt 1999, Steiner and Peters 2000 among others) and none for developing countries. Furthermore, the empirical evidence presents some caveats. The first one is that most of the studies analize work sharing policies derived from collective bargains, which induce them to use instruments to control for endogeneity of standard hours and therefore rely on the validity of the instrument. Fewer studies have used exogenous changes in regulation to asses the impact of reductions of standard hours on employment (Crepón and Kramarz 2002 and Chemin and Wasmer 2009); The problem is that even those studies are unable to identify the "net" effect[3] of the policy due to lack of data or negligible magnitude of the policy change. A third problem of previous literature is that, in general, reductions of standard hours are jointly implemented with other policies like higher flexibility (e.g. Portugal) and\or financial incentives (e.g. France) and therefore it is difficult to isolate the pure effect of the reduction of standard hours on employment. Also, there is no

[2] Share of part-time workers on total employment of the firm.

[3] "Net" refers to the direct effect of the reduction of working hours once controlled for the indirect effect of wages.

direct micro-econometric evidence about the effects of reductions on standard working hours on mix of employment, which can affect the substitution between working hours and employment.

This study exploits an exogenous variation of standard hours given by a change in Chilean regulation about maximum standard hours per week to study the effect on employment transitions for those people affected by the policy. Specifically, in September of 2001 the Chilean Parliament approved a labour reform which included a compulsory reduction of the maximum standard hours from 48 to 45 hours per week.[4] The whole reform took place in December 2001 except the reduction of hours which (compulsorily) began on the first of January of 2005. This separation of the reduction of standard hours from the rest of the reform gives us a pure policy change on working hours not present in other studies.[5]

This study uses the *EPS Panel (Encuesta de Proteccion Social)* , which includes information about weekly usual (average) hours, monthly earnings, employment and type of contract (among others) before and after the change in policy. Hence, we extend the analysis of Crepón and Kramarz (2002), since they do not have data on usual hours and hourly wages before the policy change. Limitations that we do not have.[6] Similarly to the French case, this study does not analyze the overall effect on employment, instead it focuses on those employees affected by the reduction of standard hours (i.e. study the excess of employment destruction and not net job destruction).[7] Specifically, this study uses a difference-in-difference approach (DID) to study whether workers affected by the reduction of working hours (i.e. employees who worked 46-48 hours before the policy change) lost their jobs more often than those not affected by the policy.[8] Also we extend the analysis to those who were working overtime (i.e. those who worked 49-60 hours).

This study is organized as follows: Section 2 explains the implications derived from theoretical literature. Section 3 presents empirical evidence at the macro and micro level. Section 4 summarizes the institutional framework of the policy change in Chile. Section 5, presents

[4]One of the arguments stated for the reduction of standard hours was the high level of unemployment in Chile due to the effects of the Asian Crisis (1998-2000) and the low productivity of Chilean workers due to long working hours. Other reasons were the negative effects on health and family (social) life of long working hours. Dirección del Trabajo (Undersecretaryship of labour), Temas Laborales N°11 (2002).

[5]Although it introduces some complications. We will refer extensively to this in section 5.

[6]These variables are crucial since as Kramarz et al (2008) pointed out *"....the impact of a compulsory reduction in working hours on employment hinges on the reaction of wages"*.

[7]Although, Crepón and Kramarz show that the almost 8% excess of destruction found for low wage workers in their study is consistent with a net job destruction of around 2% which gives an elasticity of employment to labour costs just below -1, which is in line with french empirical evidence. Therefore, the excess job destruction seems to have some impact on net job destruction.

[8]Because they were working below the new standard (i.e. those who worked 44-45 hours before the change in policy).

the description of the dataset and the key variables. Section 6, introduces the identification strategy, the methodology we use to evaluate the effect of the policy and the results. Section 7, presents the sensitivity analysis and section 8 conludes.

2 Theoretical Evidence: labour demand and working hours

Theoretical literature on labour demand recognizes the distinction between hours of work and number of workers (employment). The main reason for this is that if hours and workers were perfectly substitutable then companies would choose the amount of hours without taking into account the manner in which those hours were divided up among its workers. This scenario is only true if the productivity of an hour of work and the rate of utilization of capital do not depend on the average individual length of time worked. On the one hand, the former case is unlikely due to the existence of setup costs which makes that worker' productivity has increasing returns for small values of hours worked and beyond a threshold fatigue will start affecting workers' productivity and then productivity will have decreasing returns. On the other hand, the latter case is also unlikely since in order to keep the rate of utilization of capital independent of the hours of work we need to assume that companies adjust their production process with every change in working hours.

Furthermore, the distinction between hours of work and employment becomes more important since the cost of labour is not a linear function of its duration. There are at least two reasons for this: firstly, there are some costs (like cost of hiring and firing, training cost and certain social security contributions) that do not depend on how many hours people actually work. Secondly, it is common to find regulations in different countries that specify the maximum standard hours of work and the overtime hours. Overtime hours are usually remunerated at a higher rate than the standard hour (the cost of overtime hours could also be non linear with respect to its duration like in France and Portugal). Therefore, there will be a difference between the costs arising from an increase in the number of workers from the one that arise from a change in working hours per worker.

The theoretical literature which analyze the effect of reducing working hours on employment started with Rosen (1968) and then it were extended by Ehrenberg (1971) and Calmfors and Hoel (1988) among others. They focus on the demand side of the labour market and implicitly assume that wages are given and that labour is homogenous. The implications of these earlier

literature can be easily derived from Calmfors and Hoel ' (1988) version of the labour demand model which assumes that we have a firm that minimize costs given a particular level of output:

$$Min\ CN\ subject\ to\ F\left[G\left(h\right)N, lk\right] \geq Y \tag{1}$$

$$C = \left\{ \begin{array}{c} \left[f + w_0 T + w_1\left(h - T\right)\right]\ if\ h > T \\ \left[f + w_0 h\right]\ \ if\ h \leq T \end{array} \right\} \tag{2}$$

Where: C is the cost per worker, w_0 is the wage for a basic (standard) hour of work, N represents the number of workers (employment), h is the average (usual) individual hours worked, Y = output, T = standard workweek[9], w_1 is the overtime wage = w_0 (1+θ) where θ is the overtime premium (assumed to be constant), f = positive scalar that represents those costs which do not depend on hours of work (i.e. cost of hire and fire, training costs , among others)[10], $K = l\ k$ = capital services where l = operating time (*i.e.* the number of hours the plants is in operation) and k = capital stock (assumed to be given)[11]. It is assumed that F is a well behaved concave production function.

Calmfors and Hoel (1988) also work with labour services (L=labour services=$G(h)N$=$g(h)hN$) instead of working hours, hence we define $G(h)$ as a function which describes the relation between productivity of each worker and his working time in the production of labour services. Without this function G (i.e. L=hN), workers' productivity would be independent of hours. By the same argument, $g(h) = G(h)/h$ = is the average productivity per hour of each worker.

It is also assumed that due to setup costs the marginal productivity of an hour of work is positive, g_h>0 (i.e. increasing returns), for small values of h and because beyond certain threshold (h^*) fatigue will set in, it is assumed that g_h<0 (i.e. decreasing returns) for large values of h. We also assume that we are in this latter region, which implies that the elasticity

[9]By assumption it will be imposed by law or by central bargaining, which means that it will be exogenous.

[10]These fixed costs depends on the institutional environment and on the unemployment rate. For example: fixed costs should be higher in those countries with higher job protection and when the rate of unemployment is low since it takes more time to find unemployed workers.

[11]The productivity of capital is thus assumed to be independent of the operating time. Calforms and Hoel (1988) relax this assumption at the end of their paper by assuming that operating time of capital is proportional to hours. The main change under this setting is that working time will not be scale independent anymore which generates more ambiguous cases.

of labour services with respect to working time is smaller than one. Finally it is assumed that marginal productivity is declining (i.e. $G_{hh}<0$ for $h>h^*$).[12]

Equation (2) represents labour costs and implies that labour demand (i.e. number of workers employed and hours worked) should depend on the relative magnitudes of the variable costs (w_0, T and θ) versus fixed costs (f). Hence, a relative increase in variable costs (or a relative fall of fixed costs) should increase employment and decrease hours. This implies that the demand for hours and the demand for workers may vary in opposite directions.This Keynesian framework was usually in mind when cuttings in working hours were first proposed, since under this setting it might be possible, under certain conditions, to have an increment on employment by cutting working hours. Nevertheless, with this framework we can have different equilibriums depending on the situation of the firm before the reduction in working hours. To see this we minimize (1) keeping l, k and Y fixed while C is giving by (2) and w_1 is a constant. Thus, the first order conditions are:

$$\frac{w_0}{C} = \frac{G_h}{G} \; for \; h < T \tag{3}$$

$$\frac{w_1}{C} = \frac{G_h}{G} \; for \; h > T \tag{4}$$

$$\frac{w_0}{C} = \frac{G_h}{G} = \frac{w_1}{C} \; for \; h = T \tag{5}$$

Where (3) and (4) are interior solutions and (5) is a corner solution. Hence the consequence of the reduction in working time will depend on the starting situation of the firm:

Case A: $\frac{Nw_0}{C} = \frac{NG_h}{G}$, where Nw_0 is the marginal factor cost for hours when actual time (h) is below the standard working time (T). NG_h is the marginal product of hours and G is the marginal product of employment in the production of labour services. In this case a small reduction in T will not have effect. This is the case when standard hours (maximum hours) are higher than optimal hours (hours chosen optimally by the firm). This is the less interesting case, since a small reduction in standard hours will not affect the actual hours (optimal hours) or employment.

Case B (corner solution): If the firm chooses working time exactly equal to standard working hours, then a small reduction in T will unambiguously decrease usual hours and will have an

[12]These assumptions are necessary for the existence of interior solutions with overtime.

a priory ambiguous effect on employment depending on the nature of the new equilibrium. If after the reduction of standard hours it is still optimal to equalize usual hours to standard ones then employment will increase. This is the model that supporters of work sharing have in mind. Nevertheless, if the new equilibrium is not the one that equalizes standard and usual hours the effect on employment will be ambiguous due to a trade off between the wage (w_0T) and the fixed costs (f). It will only increase employment if fixed costs (f) are sufficiently low (see Calmfors and Hoel 1988 and Chemin and Wasmwe 2009) which, in general, is more likely to be the case of a developing country than of a developed one.

Case C: $\frac{Nw_1}{C} = \frac{NG_h}{G}$, where Nw_1 is the marginal cost of hours when usual time (h) is above the standard working time (T), C is the marginal cost of employment and $\frac{NG_h}{G}$ is the marginal rate of substitution between hours and employment which has to equal the ratio of the marginal costs ($\frac{Nw_1}{C}$). In this case a reduction in T will increase h and will decrease N. Hence, in this case, a reduction of the standard workweek is counterproductive as an instrument of policy. We can check this with equation (6):

$$\frac{G_h}{G} = \frac{w_1}{C} = \frac{w_1}{f - (w_1 - w_0)\,T + w_1 h} \tag{6}$$

A reduction in T will, ceteris paribus, increase the denominator which means that $\frac{w_1}{C}$ will decline (*i.e.* the marginal cost of hours decreases relative to the marginal cost of employment). The intuition for this is that a reduction of standard hours act *as* an increase in the fixed cost (f) per worker while maintain the cost of overtime unchanged; hence firms are induced to substitute employment for longer hours.

To summarize, under cost minimization the reduction of standard hours has an a priori ambiguous effect on employment, since it will depend on the original situation of the firm.

Calmfors and Hoel (1988) extended the model by assuming that firms maximize profits instead of minimize costs, which means that output is not fixed anymore. The authors conclude that the probability of having an increase in employment under profit maximization is even lower than before (with cost minimization) since to the previous substitution effect we have to add a non positive scale effect due to the negative effect of higher labour costs on output prices and output demand.[13]

[13] In case (C), where firms use overtime, the substitution effect decrease employment and increase working hours when the standard work week is reduced since the relative marginal costs of employment and hours changed. With profit maximization, there is a scale effect that gives an additional reduction on employment due to the reduction in output and the use of labour services induced by an increase in C (labour costs), which

Hitherto we have considered a reduction in working hours under a competitive model taking the hourly wage as given which is the so-called "*direct effect on employment*". If there is an increment in hourly wages[14] (i.e. *wage compensation*), labour demand theory suggests that, ceteris paribus, employment should decrease due to the substitution towards capital which is the so-called "*indirect effect on employment*". This is clear if we write the reduced form of the labour demand as $N\ [w_0(T),\ T]$. Then the employment elasticity with respect to standard hours is:

$$\eta_{NT} = \ \eta_{NT|_{w_0=const.}} + \eta_{Nw_0}\eta_{w_0T} \tag{7}$$

Hence, the more negative the standard working time elasticity of the hourly wage (η_{w_0T}) the less likely will be to have a positive effect on employment[15] . Then it will be crucial to consider the effect of wages since a reduction in standard hours with constant labour costs (i.e. with full wage compensation) will make impossible to increase employment. This has consequences for those workers who are paid the minimum wage, since it means that for them it is not possible to increase employment with a reduction in standard hours[16] .

Hart (2004) highlighted that it may exist also another indirect effect on employment, specifically an effect on the mix of employment. Hart shows that, in a case with two groups of individuals, part-time workers and full-time workers where the former ones work less than standard hours while the latter ones work overtime (*i.e.* similar to case C), a cut of standard hours has two effects: firstly, it leads to overtime-employment substitution among full time workers (i.e. same conclusion as in case C). Secondly, leads the firm to increase the ratio of part-time to full-time employees due to a higher relative cost of the latter ones. Furthermore, he concludes that "*. . . the ability of firms to vary the mix of part time and full time workers may serve to mitigate a tendency to increase average overtime hours given the shortened standard work week*".

follows from the reduction in the normal working time. Also, Calmfors and Hoel (1988) showed that in case B the result is still ambiguous under profit maximization but the probability of getting an increase in employment will be smaller than in the cost minimization case.

[14]This could take place since wage-earners should resist a cut on income and, therefore, demand higher hourly wages.

[15]Ceteris paribus. Although, if there is substitution from hours to employment when there is an increment in wages the sign of the wage elasticity of employment (η_{Nw_0}) will be positive. Nevertheless, we implicitly assumed that it is negative. This is supported by empirical evidence (Hamermesh 1993).

[16]Kramarz et al. (2008) pointed out that standard hour reduction can also have two further beneficial effects on employment. The first one is that average labour productivity is larger when the duration of work is shorter. The second one refers to the reorganization of the production process. This is because a cut in standard hours may induce significant reorganization in the production process leading to a more intensive utilization of capital and thus higher employment.

Recent developments suggest that by relaxing the perfect competitive market assumption by allowing for monopsony power, a reduction of standard working hours might improve employment (Marimon and Zilibotti 2000) depending on the magnitude of the reduction of hours. Without an upper hourly limit a monopsony that maximizes its profits subject to a labour supply constraint will choose a work duration larger than the competitive work duration and a smaller wage than the competitive one. This can be seen in figure 2 taken from Kramarz et al. (2008). If an upper hourly limit is introduced the effect on employment will be ambiguous depending on the level of the upper limit relative to the monopsony use of hours. With an upper hourly limit above the monopsony hours there will be no effect on employment. If the upper hourly limit decreases slightly (to a level that lies between the competitive and monopsony usual hours) then employment levels will increase.

Therefore, and to summarize, the theoretical effect of a reduction of standard hours on employment is ambiguous and remains an empirical question.

3 Empirical Evidence

The empirical evidence on the employment effect of reductions of standard hours is scarce and it is concentrated on European experiences. The leading examples are Germany and France which represent examples of the two main approches to reduce standard hours: by bargaining agreements (Germany) and by legislation (France).

In the case of (West) Germany, Hunt (1999) uses industry level variation in reductions of standard hours. She uses a fixed effect estimation approach to study the effect of the reduction of standard hours on employment. In her preferred specification she does not include wages as a covariate since it might be jointly determined with employment in the collective bargain. Therefore, she obtains the gross effect of the cut of standard hours. Her findings are that a reduction of 1 standard hour decreases employment by 3.8%, but this is not significant. Once she includes a proxy for wages[17] the point estimate becomes smaller and remains insignificant.

Since the employment effects of a reduction in standard hours are likely to differ by skills[18] Steiner and Peters (2000) use industry-level data and separate by unskilled, skilled and high

[17] She uses the index of bargained monthly wages rather than actual monthly wages.

[18] This is mainly for two reasons. Firstly, the direct effect (holding hourly wage constant) may differ because the possibility of substitution differs by skill groups. Secondly, the indirect effect of the reduction of standard hours, due to wage compensation, may also differ by skill groups since the wage elasticity of labour demand is likely to differ by skill groups as well.

skilled workers. They show that, given wages, the direct employment effect of a reduction in standard working hours is negligible for all three groups. Nevertheless, once the adjustment on wages is taken into account, the net employment effect becomes negative on average, but especially strong for unskilled workers.

Due to different theoretical predictions on the employment effect generated by a reduction of standard hours for firms with and without overtime workers, Andrews et al. (2005) specified a model with different types of firms. Different types of firms are defined by the proportion of workers who work overtime in each firm[19].They use plant-level data (IAB Establishment Panel) in a first differences estimation approach to study the effect of the reduction of standard working hours on employment (without controlling for wages due to a potential endogeneity problem). Also, they instrumented standard hours by Industry level standard hours to deal with potential endogeneity problems.They could not find a significant effect on employment, except (a positive one of 2%) in small plants in the East German non-service sector. They argue that one of the possible explanations might be a lower increment in hourly wages in East Germany.

Usually all the studies above face the difficulty of endogeneity of standard hours due to the process of bargain with unions. That is why they instrumented it. An alternative way would be to exploit the change in legislation as an exogenous variation in standard hours. This is interesting since many countries have used legislation to vary standard hours (example: Japan, Canada, Portugal, Italy, France among others), nevertheless microeconometric evidence is scarse.

The oldest evidence from countries where a change in legislation was applied comes from time series studies. For Japan, Brunello (1989) uses Monthly Labour Survey to estimate a system of equations of demand and supply of hours and employment. He finds that a reduction of standard hours has a negative and significant impact on employment and a positive effect on overtime. There is a problem with this kind of approach since the reduction of standard hours might be confounded with the effect of another variable trending down. Also, there is a problem of endogeneity between hours and employment since it does not utilize the exogenous shock given by Japanese law.

The most studied country in reference to reductions on standard hours is France, which has experienced two rounds of work week reductions, in 1982 and in 1998 (*Aubry I*)-2000 (*Aubry II*) respectively. Each of them with different characteristics (designs) and, most interestingly,

[19]Therefore there are three types of firms. Those firms where every worker works zero overtime. Those firms where every worker works positive overtime and those firms where a proportion of workers work overtime.

with different consequences due to these different designs of the policy. The main difference between the reform of 1982 and 1998-2000 is that in the latter one monthly labour cost remain relatively constant whereas in the former one increased.

The only empirical evidence studying the employment effects of the reduction of hours from 40 to 39 per week in 1982 has been done by Crepón and Kramarz (2002). They use an unexpected change in policy as a quasi-experiment. They used a difference-in-differences approach which compares the difference in the behaviour of the treatment group (those who work 40 hours before 1982, then extended to include those who work overtime up to 48 hours) with the difference of the behaviour of the control group (those who work 36-39 hours before 1982).They conclude that those individuals in the treatment group in March 1981 have 3-4% (depending on the especification) higher probability of being not employed in 1982 than observationally identical workers who, in 1981, were working 36-39 weekly hours. Their analysis neither includes hourly wages, which are crucial to understand the effect of the reduction of working hours, nor usual hours, which are more appropriate than actual hours in order to avoid noise from short run shocks. Due to lack of information on hourly wages and usual hours before 1982 they decided to use a second identification strategy which uses information post policy change including both variables since there are no data limitations after the implementation of the policy. They conclude that those workers employed 40 hours in 1982 have at least (i.e. a lower bound) a 4% higher probability of losing their jobs more often than those already employed under the new standard workweek. Finally, Crepón and Kramarz (2002) show that employees who work 40 hours and earn the minimum wage have a 7.7% higher probability of losing their jobs than identical employees who work 39 hours.

With respect to the 1998-2000 reforms in France (*Aubry I* and *II*), Askenazy (2008) points out that the problem of *Aubry I* and *II* is that "*statistical ex-post evaluation of the impact of the reduction of working hours is extremely difficult*" which is mainly due to selection problems. Selection comes from the fact that in 1998 was announced a reduction of hours to begin in 2000 but also were introduced financial incentives (payroll tax subsidies) and flexibility to incentivate the adjustment of hours to a maximum of 35 per week. Among those studies which try to encompass these selection problems are Crepón et al. (2005), Bunel (2004) and Gubian (2000). They conclude that there is a positive net effect on employment (around 6% - 9%) of *Aubry I* and also positive but weaker effect (around 3%) of *Aubry II*.

Kramarz et al. (2008) use several panel datasets to study the effect of the reduction of hours due to the Aubry laws on economic outcomes (i.e. employment, labour productivity, and capital productivity among others). They found a positive effect on employment, although they

point out that their analysis is not fully causal. Specifically, they recognize that they do not have instruments that affect the decision to reduce working hours without having an impact on economic outcomes.

A different conclusion is reached by Estevão and Sa (2008). They use the *French Labour Force Survey* and exploit the time difference of the application of *Aubry I* (by firm size) to set up a quasi-experiment which study the effect of the law on workers welfare. Specifically, they use a difference in difference approach, where large firms (i.e. with more than 20 employees) are used to construct the treatment group and small firms to construct the control group. They find that the *Aubry I* law had no aggregate effect on net job creation. There are some caveats in this study: firstly, they focus on the effects of the *Aubry I* law and since this law includes financial incentives they do not identify the effect of the reduction in hours. Secondly, the control group and the treatment group are affected in different magnitudes by the policy, which violates one of the assumptions of the difference in differences approach. This later caveat is also pointed out by Askenazy (2008).

Chemin and Wasmer (2009) obtain similar results. They use a triple difference (DDD) approach based on a particular characteristic of the French' Alsace-Moselle "droit local"[20]. These regions have a slightly different labour law than the rest of France. Specifically, they have 2 extra public holidays, which were included in the non worked time once the reduction to 35 hours took place in France. Hence, the reduction on hours was less stringent than in the rest of France. The authors find no significant difference on employment between this region and the rest of France. However, the authors pointed out that their result might be due to the fact that 2 days maybe too small to find a significant difference.

Askenazy (2008) points out that the general consensus about *Aubry I* and *Aubry II* laws is that they have generated a positive net effect on employment. Nevertheless, there is no agreement on what happened. This is, whether the cause of the positive net effect on employment was the reduction of standard hours, the reduction of labour costs or the increment in flexibility. Specifically, Askenazy (2008) argues that "*...selection bias may be too important to allow conclusions to be reached*".

A recent study by Raposo and Van Ours (2008) analyzes the case of Portugal where in December of 1996 was introduced a new law on working hours which gradually reduced the standard workweek from 44 hours to 40 hours and also increased flexibility for firms.[21] They

[20] Local Legislation.

[21] This flexibility implied that the reduction was implemented taking into account that the normal workweek could be defined on a 4 months average. It was also allowed to increase the maximum number of hours with 2 hours per day if the total did not exceed 10 hours per day and 50 hours per week.

use a longitudinal data set (*Quadros de Pessoal*) which matchs firm and employee data to study how this mandatory working hours reduction affected employment destruction and earnings of workers involved. They find that hours reduction did not lead to an increased job loss of workers directly affected (although they do not separate the reduction of working hours from the increase in flexibility). They also find that hourly wages increased, keeping monthly earnings approximately constant for workers who were affected by the new law. On a very recent companion study Raposo and Van Ours (2009) extend their previous analysis to the study of the reduction of standard hours on overall employment. Here, they find a possitive effect of the reduction of working hours on overall employment through a fall in job destruction and no effect on job creation. They argue that their results might be explained by the increased flexibility in the use of the standard workweek that made it easier for firms to cope with the reduced standard hours.[22]

To the best of our knowledge the only non European microeconometric study carried out so far has been done for Canada by Skuterud (2007). He uses the *Canadian Labour Force Survey* data and a triple difference aproach to analyze the reduction of the standard workweek from 44 to 40 hours in the Canadian province of Quebec during the period 1997-2000. The peculiarity of the Canadian case is that, unlike the European worksharing experiences presented above, the Quebec policy contained no suggestion or requirement that firms provide wage increases to compensate workers for lower hours. One important characteristic of this study is that it includes actual hours instead of usual (average) hours. As several authors point out, this may generate that what is being capture is the effect of the economic conditions or irregular (or unusual) overtime during that period of time instead (or in addition) of the effect of the reduction of standard hours. Despite this, the author finds that the reduction of weekly hours worked failed to raise employment at either the provincial level or within industries where hours of work were affected relatively more.

Finally, Kapteyn et al. (2004) use aggregate panel data for 16 OECD countries. They find that the direct effect of a reduction in standard hours has a positive effect on employment but that the upward indirect effect of wages makes the final effect on employment insignificant. The problem is that by aggregating data of countries with very different implementation process of work sharing policies it is difficult to disentangle the pure effect of the reduction of standard hours.

[22]Notice that in the case of Portugal and France, overtime rate are not constant. This makes more likely that the reduction of standard hours generate a positive effect on employment than in the traditional case presented in section 2 with constant overtime rate (see Calmfors and Hoel 1988).

4 Institutional framework of the Chilean labour reform

The new socialist government, which started in March of 2000, sent a new labour reform to the Parliament during its first year. A year later, in 2001, during the discussion of the reform the possibility of a reduction in standard hours was introduced. Finally, the 5th of October of 2001, the labour reform was published on the Official newspaper. This reform started to operate "almost" fully the 1st of December 2001. The main adjustments and/or changes on the "Labour Code" can be classificated in 6 categories. Those are:

1. End of contracts (separation of workers) and layoff costs.
2. Exceptional distribution of the working time in some industries.
3. Over time hours.
4. Collective Bargains.
5. Working privileges, Management limiting fines, higher penalties and more supervision from the Ministry of Labour.
6. Duration of the working week.

The reduction of the number of weekly working hours (last category) was the only part of the reform which did not apply inmediately.[23] The reduction of hours was announced to be implemented by the 1st of January of 2005 (as explained below). This (three years) window was established to give time for companies to adjust, although as we will show below, most of the adjustment happened during the last year. This three years gap between the first five changes and the sixth one is important since it allows us to isolate the reduction of standard hours from the rest of the reform, which means that the reduction of standard hours is not contaminated with the jointly implementation of other policies like in the case of Portugal and France (*Aubry I* and *II*).[24]

In reference to the duration of the maximum standard working week, the reform preanounced a reduction in the upper limit from 48 to 45 hours per week. This reduction will be compulsory in the whole country from the 01/01/2005; hence it gave to companies around 3 years to adjust. (From the 01/12/2001 until 31/12/2004 was optional).

[23] To see the description of the other elements of the labour reform see the appendix.

[24] Although this three years gap may generate anticipation effects. As we will see below, the data suggest that this effect is not significant. We will discuss extensively this point in the next section.

This constraint will not apply to: independent workers (self-employed), workers who have more than one employer, CEO, managers and all those people who work without direct superior supervision; Also, it will not apply to those who work from their homes or in a place chosen freely by their selves, insurer sellers, traveller sellers, and all those that do not work on their job' premises. All those that work on fishing boats, those that work mainly outside the firm dependencies and those that work by using long distance technologies are also excluded. Finally, for people who work in hotels, restaurants or clubs (except the administrative, laundry and kitchen staff) will not apply when the daily flow of customers is low and when they have to be available for the customers.[25]

As part time job is defined as the ones that can not work more than 2/3 of the hours of a full time job, the reduction from 48 to 45 means that from the 01/01/2005 part time workers can not work more than 30 hours (before was 32 hours). The reform was explicit in the sense that it pointed out that this reduction must not affect workers who work intramarginal hours before the reform (i.e. for example 40 or 45 hours).[26]

The reform stated that adjustments of working hours have to be done by agreement between employer and employee. If there is no agreement, then employers can unilaterally modify working hours without affecting the weekly distribution of them.[27] Despite the labour reform clearly specifies the reduction of working hours it was ambiguous about the adjustment of wages. Specifically says:

> *"Employee and employer have to make an agreement about monthly earning in order to adjust it to the reduction on working hours, nevertheless the legislator believes that it is not desirable a proportional reduction on monthly earnings as a consequence of less working hours".* It also states that*: "if there is no agreement between employer and employee, then the employer has to maintain employee' s monthly earning irrespective if it is a fix, variable or a mixed one".*[28]

To clarify this issue, in September 2004 (*i.e.* before the mandatory reduction of working hours) the Undersecretaryship of Labour pointed out that "*the reduction of monthly earnings it is against the law given that the objective of the long adjustment window (3 years) has been*

[25]Notice that these exemptions also applied when the maximum were 48 hours per week, hence it is not altered by the reform.

[26]Res. de la Dirección del Trabajo 4338/168

[27]It cannot alter lunch time or the maximum hours of work per day (10 hours).

[28]Res. de la Dirección del Trabajo 4338/168. Own translation.

to allow companies to reduce the impact on their costs (due to the maintenance of earnings) by increments in productivity and readjustment of the production process"[29]. This rigidity in earnings could be important since, as we saw in the theoretical model, it might increases the likelihood of a negative effect on employment levels even with high increments on productivity.[30] This is especially important for those workers paid at the minimum wage.

As it was explained above the reform was not clear about earnings until september 2004, but as we will see below firms seemd to follow the advice of the Undersecretaryship of Labour.

5 Description of the Data

5.1 General Description of the EPS Panel

To study the effect of the reduction of working hours on employment transitions we use the *EPS* Panel[31], which has 3 waves so far (2002, 2004, 2006). When individuals are interviewed for the first time, they are asked about their labour market activities since January 1981 or since they were 15 years old, whichever ocurred last (i.e. this includes employed, unemployed, looking for a job for the first time or inactive). They also reported the initial and final month and year for every spell.

The population of reference for the survey is all those who were affiliated[32] since January 1981 until august 2001 (registered in the administrative files of each *AFP*[33] and the *INP*[34] and who were also available in the system in august of 2001). Then, the Microdata center of the economics department of the University of Chile selected an historic sample of all the individuals affiliated to the system in 1981 or after, giving a sample size of 17,246 people for the first wave. This wave was carried out between June and December 2002 and included economic, socio-demographic information of individuals like usual working hours (but not monthly earnings). Subsequent waves updated the population of reference to include new affiliated and non affiliated (who were not included in wave one). The second wave was carried out between November 2004

[29] Report 4338/168, September 2004, Ministry of Labour. Own translation.

[30] As Kramarz et al. (2008) pointed out: "....reduction in standard hours with full wage compensation appear to be detrimental to employment even if the productivity gains are huge"

[31] Panel de Encuesta de Proteccion Social.

[32] Affiliated is defined as: all the people with at least one contribution to the pension system. 94% of dependent workers are affiliated to the pension system irrespective of the type of contract (i.e. part time or full time) (*Superintendence of the Pension System*).

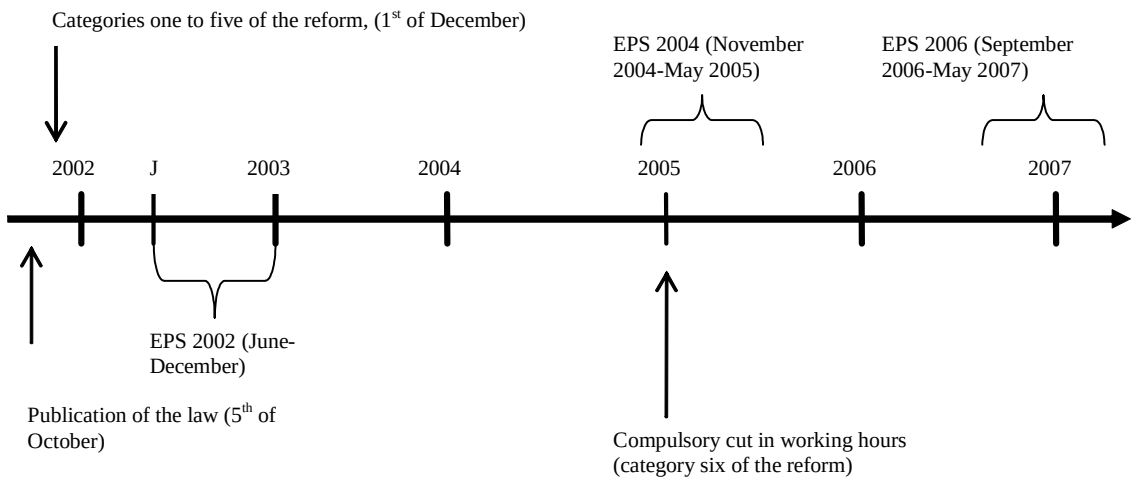
[33] Administradora de Fondo de Pensiones. (Private pensions management funds).

[34] Instituto Nacional de Prevision. (i.e. National Institute of Pensions)

- May 2005 and included information on monthly earnings and usual hours since January 2002. Similarly, wave three included information on monthly earnings and usual hours since January 2004.

As a summary, the time line of the data and the problem under study are shown in figure 1.

Figure 1: Time line of the reform and waves of data



5.2 Evolution of usual hours, hourly wages and employment

An advantage of the EPS Panel relative to the Crepón and Kramarz' (2002) study is that we do have data on monthly earnings and usual hours since January 2002 (i.e. before the policy was implemented in January 2005). Our setting would be ideal if the policy was announced sometime after our first period with earnings data. This is clearly not our case since there is a pre-anouncement of the reduction of standard hours in december 2001 when the labour reform was introduced.[35] The fact that we only have full data from January 2002 onwards may imply that we can have some anticipation to the policy in the sense that firms might reduced hours during 2001-2002. Although this is not likely, due to the long period of adjustment (3 years)

[35] Also, it would be possible to have some change in firms' behaviour due to the discussion of the reduction of standard hours previous to its implementation in december 2001.

introduced during the discussion of the labour reform, it is important to check the behaviour of the distribution of hours to be confident that there were no significant variation on it.

5.2.1 Anticipation via usual hours

We check the behaviour of usual weekly hours before the reduction of standard hours was introduced (or even discussed) in the labour reform (e.g. January 2000), during the period of optional adjustment (e.g. January 2003) and after the compulsory date (e.g. January 2005). Figure 3 suggests that the discussion and publication of the labour reform which included the announcement of reductions of weekly hours, did not have a significant effect on working hours between January 2000 - January 2003 since the distribution of usual weekly hours remains the same. The variation on the mean of usual weekly hours between January 2000 - January 2003 is negligible (around -0.02%). Similarly, the evolution of the standard deviation of usual weekly hours decreases marginaly (-0.22%). On the other hand, the comparision between January 2003 and January 2005 shows an important variation on the distribution of hours, thus the adjustment seems to start at some point in between. To check this, we analyze the information on usual hours given in the second and third waves of the *EPS* panel (Figure 4) we observe that there is a declining trend of the mean of usual weekly hours for dependent workers who work 44-60 hours per week which started very slowly in mid 2003 until late 2004 when there was a sharp drop due to the mandatory application of the reduction of weekly working hours in January 2005. The standard deviation maintains a fairly constant behaviour except in January 2005 where it suffered a sharp drop. Nevertheless, a few month after the compulsory reduction of hours the standard deviation follows again a fairly constant behaviour although at a lower level than the one before January 2005. These suggest that there might be a change of behaviour of firms during 2003 and early 2004 but most of the adjustment seems to be held during late 2004 and begining of 2005.

5.2.2 Anticipation via earnings

It might be the case that firms did not anticipate the change of hours between January 2000 - January 2003, but instead modified monthly earnings or the rate of growth of monthly earnings. If these were the cases then it would imply an anticipation via monthly earnings. Furthermore, the analysis of monthly earnings is important because the reform of December 2001 was not clear about it adjustment. Originally the law suggested that it was "not desirable" to reduce monthly earnings, although it was not explicitly forbidden in the reform. Only in September

of 2004 the Undersecretaryship of Labour was precise about the illegality of the reduction of monthly earnings (see the end of section 4). Therefore, if monthly earnings were unaltered and individuals decreased the number of hours, then this implies that firms faced higher hourly wages which might have consequences on employment (if they followed the "advice"). Also, it could be the case that firms adjusted the nominal hourly wage (or monthly earnings) at a lower rate than inflation in order to crowd-out the increment of costs.[36]

Unfortunately, the first wave of the *EPS* panel does not include information on monthly earnings, hence we can not use the first wave to analyze the behaviour of earnings before the application of the labour reform in december 2001. An alternative is to use two indexes from the National Institute of Statistics of Chile (figure 5) that we can trace back to January 2000 to help us analyze if there were anticipation effects via hourly wages. These indexes are: (a) the nominal index of wages and (b) the nominal labour costs index. The difference between the two is that the former one does not include overtime pay while the latter one does.[37] Both indexes show that companies neither reduced the increment in nominal hourly wages nor reduced the nominal hourly wages during the discussion of the labour reform in 2001. The same argument holds after the labour reform began to be operative in december 2001 (and therefore the pre announcement of the future reduction of weekly hours). Interestingly, we observe that there is a higher increment in the hourly wage and hourly cost of labour indexes since the date of the mandatory reduction of weekly hours (in late 2004-early 2005). Therefore, these indexes suggest no anticipation effects via hourly wages, and since there were no variation on the distribution of hours, it is reasonable to assume no anticipation via monthly earnings.

When we analyze the data on hourly wages from waves two and three we observed an increament on the nominal hourly wage in January 2005, as can be seen in figure 6 which give us a general picture that is consistent with the information obtained from the nominal labour cost indexes displayed in figure 5 in the sense of no major changes on hourly wages during the three years gap, but wage compensation in January 2005 when they reduce hours.[38]

[36] Assuming that inflation represents the increment in the price of the firm´s output.

[37] These indexes are used in the national account statistics by the Central Bank of Chile and also are the reference for transport, electricity and telecommunication tariffs.

[38] This may suggest that firms which adjusted hours between mid 2003 - late 2004 did not give wage compensation to their employees. This may be due to the early ambiguity of the Undersecretary of Labour about earnings. Issue solved in late 2004 when the illegality of the reduction of earnings was explicitly announced.

5.2.3 Anticipation via employment

Hitherto it seems that firms did not anticipate neither the change of hours nor the changes in hourly wages. Nevertheless, it may be the case that firms changed their behaviour affecting employment as the result of the pre announcement of reduction of standard hours. We observe employment behaviour in figure 7, where it is possible to observe that there is variation of the employment rate within each year.[39] It seems that employment rate behaviour does not suffer any significant alteration due to the pre announcement of reduction of standard hours in december 2001. Interestingly, there is a small decline of the employment rate in January 2005 which may imply a negative effect on employment coming from the reduction of standard hours. We will come back to this point below.

Based on these arguments, it seems to be no anticipation at least up to January 2003. Furthermore, data suggest that there are some change in behaviour starting in mid 2003 until late 2004 when a sharp adjustment occurs in hourly wages and standard and usual hours. Therefore, if we use a difference-in-differences approach, as we will explain in the next section, we can consider January 2002 - January 2003 as if it were a pre-policy period and given that employment, usual hours and hourly wages were not affected until January 2003 we can consider the change in policy, ocurred later on, as if it were a quasi-experiment (i.e. exogenous shock).

5.3 Methodology, Time Periods and Control Group

5.3.1 Definition of pre and post-treatment period:

It could be the case that individuals employed between 46-48 hours have higher (lower) transition rates from employment to non-employment relative to similar groups (i.e. even in the absence of the reduction in working hours those workers who work 46-48 hours might have higher or lower subsequent unemployment probabilities). This makes the difference-in-differences approach a natural one to take, since the difference between the control and treatment group before the treatment is compared with the difference between these groups after the treatment. Therefore, the challenge with difference-in-differences is to: define a suitable control group in order to be able to identify the effect of the policy and also to specify a pre-treatment and post-treatment period. This latter point ideally refers to: firstly, a period where there are no policy

[39] This rate is: the number of people (per month) who declare to be employed over the total amount of people who declare to be employed, unemployed and inactives. It is possible to observe that January and February of each year are months with high employment rates (due to seasonal activities like agriculture among others). Therefore, it is important for our analysis to use the same month each year.

change (not even pre-announcement) which is the so-called pre-treatment period and secondly, a period that includes the policy change which is the so-called post-treatment period. Based on firms' non anticipation behaviour presented in the section above and also on the significant difference on the distributions of hours between January 2003 and January 2005 (see figure 3), we consider January 2002 - January 2003 as the period before the policy change.

To define the period which covers the application of the reduction of standard hours is more complicated. This is because there are alternatives:

(a) On the one hand we can choose the start of the post-treatment period to be very close to the treatment date (e.g. January 2004 - January 2005) but at the potential cost of being affected by anticipation effects during the gap period between the pre and post-treatment period (i.e. January 2003-January 2004). This is clear from the results presented above (see figure 4) where it is possible to appreciate some degree of variation on hours during January 2003 - January 2004 which might affect our estimation. A possible solution to this problem would be the use of a one year moving window.[40]

(b) On the other hand, if we define the post-policy period to start very close to the end of our pre-policy period (i.e. January 2003-January 2005), then we overcome the potential anticipation problem, avoiding the underestimation of the effect of the policy change presented in option (a) and making more likely the fact that covariates were not affected by the pre-announcement of the reduction of standard hours. Nevertheless, if we choose this latter option, two issues will appear. Firstly, due to the two years transition window (instead of one year transition in option a), it is more likely to have cases of people who loose their jobs and find new ones within this wider window of time. To avoid the complication of including people who loose their job and find a new one during this period it is possible to construct a proxy for tenure. This proxy will allow us to refine the question of interest to: which of the individuals who were employed in January 2003 are still employed at the "*same job*" in January 2005. In this way we would recover those individuals affected during January 2003- January 2005 who find a new job before January 2005. The *EPS* Panel does not have a direct measure of tenure, therefore we construct one (see appendix).[41] Secondly, if we want to define the post-treatment period

[40] This is, fix the pre-policy period as January 2002 - January 2003 and then define the post-policy period as January 2003 - January 2004 and estimate the difference-in-difference. If this estimation gives us a nonsignificant effect of the reduction of standard hours on employment transitions then it would impliy that there were non-significant anticipation effects. If this is not the case, then anticipation effects will be important and this method would underestimate the effect of the policy change. In case of non significance, we repeat the procedure but now replacing the post-treatment period for January 2004 - January 2005.

[41] Results does not change with respect to the specification without "tenure".

as January 2003 - January 2005, and because we are using a difference-in-difference approach, we need to assume that the determinants of employment transitions over two years period and its effects are the same than the determinantes (and their effects) of employment transitions over a one year period or that these differences are similar between the treated and controls in a way that they will be eliminated in the differentiation between groups (i.e. absorbed by the common trend). If the assumption is not valid then our estimation would be contaminated with the differences on transitions of two years versus one year, thus they would not be directly comparable.

We decided to use both definitions (a and b) to estimate the potential effect of the reduction of standard hours on employment transitions. Results are similar under both alternatives, hence, due to the more restrictive nature of option (b), we choose this one as a matter of presentation of the model. Then, we will check the results contrasting it with the less restrictive option (a). In the final section we will also check how robust are our results to alternatives definitions of time periods.

5.3.2 Definition of control group:

Apart from the definition of time periods, differences-in-differences methodology requires the definition of a suitable control group in order to be able to identify the effect of the policy. In our case we take those individuals who are as close as possible to the treatment group in terms of hours to make their behaviour as similar as possible. Thus, we choose those individuals who work between 44-45 hours per week as control group. As can be seen from the summary statistics presented in Table 1, both groups seems to be very similar in terms of observables (see definition of each covariate in the appendix).

Table 1: Summary Statistics in January 2002

	Control Group		Treatment Group	
	Mean	Std. Dev.	Mean	Std. Dev.
Gender (Female=1)	0.26	0.43	0.24	0.43
Age (years)	41.63	9.83	41.69	9.77
Schooling (7 categories)	3.72	1.73	3.72	1.73
Region (13 categories)	9.39	4.14	9.38	4.13
Occupation (7 categories)	5.52	2.14	6.13	2.05
Industry (9 categories)	5.80	2.49	5.18	2.48
Union (unionized=1)	0.17	0.37	0.14	0.35
Firm' size (7 categories)	5.13	2.18	5.12	2.17
Observations	970		2666	

We observe that most of the variables are well balanced between control and treatment groups. For example, the control group has 26% of females (i.e. when the dummy of gender =1) while the treatment group has 24%. The average age of the control group is 41.6 years, while the average in the treatment group is almost the same 41.7 years. The main differences between the control and treatment group come from occupation and industry. Furthermore, if we observe the distribution of industry classification among the treatment and control (figure 8)[42] we notice that individuals in the control group tend to work relatively more on those industries classified in categories 7, 8 and 9 which are: transport, communications, financial intermedation and public, social and personal services respectively and relatively less on categories 1, 3 and 6 which are: agriculture, hunting, forestry, fishing, manufacturing and commerce respectively. By analizing the distribution of occupation categories among treatment and control groups (figure 9) we find that controls are more likely to work in categories 3 and 4 which are: clerical support workers and services and sales workers respectively and work relatively less in categories 7 and 9 which are: craft and related trades workers and elementary occupations respectively.[43]

[42] Where the line with triangular markers represent the treatment group (g=1) and the smooth one the control group (g=0).

[43] This information is consistent with the one in ENCLA 2003, which is a cross sectional labour survey for 2003.

6 Empirical Strategy and Estimation

6.1 Identification Strategy

In contrast with most European cases, where the national adjustment of working hours was not binding[44], the reduction in the Chilean case was binding, since more than 65% of non-self employed individuals with employment in the private sector were working 48 hours or more. Hence, in this sense our case is similar to the French reduction of working hours of 1982 studied by Crepón and Kramarz (2002).

It is true that in January 2002 the policy change was already known, but given that the data suggest no anticipation, it can be considered as if it were unexpected. Given the "unexpected shock" and that we do have workers already employed 45 or less hours (i.e. below the new standard) in January 2002, this reduction in working hours can be seen as a "quasi-experiment". This quasi-experiment will allow us to study the employment transitions of those in the treatment group during January 2002 - January 2003 and compare it to transitions of treated individuals during January 2003 - January 2005. Then, we add a control group which represents what would have happened with the treated if had they not been treated. If the control group is a valid one, this procedure should retrive the effect of the reduction of standard hours on employment transitions for those affected and it is called difference-in-differences approach. In our case, this is likely since treatment and control group have almost full common support[45] (see figures 8-10) and also are very similar in observables.[46] Therefore, is reasonable to assume that they are also similar in unobservables.[47]

It is also possible to have more than one treatment group, therefore we can extend our analysis by including those individuals who work 49-60 hours a week.[48] These are individuals who are treated since they were working overtime hours before the policy change was implemented. The upper limit of 60 hours per week is due to the maximum legal hours including overtime established by Chilean Law.[49]

[44] Due to lower hours agreed by collective barging (see the cases of Germany, Sweden, among others).

[45] For the ATT, full common support means that given X the probability of being treated is less than one, i.e. $P(D = 1 \mid X) < 1$. This implies that for a covariate X there are treated and control individuals, not only treated individuals.

[46] See Table 1.

[47] Difference-in-differences does not required that treated and control have equal unobservables (i.e. similar unobservables is sufficient but not necessary). It is only necessary that unobservables change similarly over time.

[48] See for example Lechner (1999).

[49] Which has not been modified by the change in labour regulation under study.

6.2 Econometric Model and Estimation

If the control group is a valid counterfactual (i.e. that the control group identify what would have been the behaviour of the treatment group in the absence of the policy change) and workers in the control group (i.e. 44-45 hrs.) have not been affected by the reduction of the working hours, then we can follow Crepón and Kramarz (2002) and represent our problem in terms of potential outcomes:

$$NE_{i,t+p} = NE^0_{i,t+p} + D_{it}\left[NE^1_{i,t+p} - NE^0_{i,t+p}\right] \tag{8}$$

Where: $NE_{i,t+p}$ is the non employment status[50] of individual i at period "t+p", especificaly: $NE_{i,t+p}$ =1 if, conditional on being employed at period "t", individual i is not employed (or employed at a "different job") in period "t+p", where p is the lenght of the transition period (in years) and $NE_{i,t+p}$ =0 otherwise.[51] $D_{i,t}$=1 if the individual i is employed more than 45 hours a week before the policy change (i.e. in the treatment group), and the superscripts represent 0 = control group and 1 = treatment group. Equation (8) shows the decomposition of the non employment status of individual i as the sum of the non employment status of individual i in the control group plus the extra effect due to the treatment (i.e. terms in bracket). The problem with the bracket is that only one of these two variables is observable. This problem makes the individual identification of the treatment impossible; nevertheless we can identify the expectation of the effect given that maximum weekly working hours changed, which is the so-called "*Average Treatment on the Treated*" (ATT):

$$E\left[NE^1_{i,t+p} - NE^0_{i,t+p} \mid D_{it} = 1\right] \tag{9}$$

Where to capture (9) we need to assume:

$$E\left[NE^0_{i,t+p} \mid NE_{it} = 0, x_{it},\ D_{it} = 1\right] = E\left[NE^0_{i,t+p} \mid NE_{it} = 0, x_{it},\ D_{it} = 0\right] \tag{10}$$

This is, that conditional on observable variables at period "t" (i.e. x_{it}) the counterfactual in which workers are not affected by the reduction (i.e. $NE^0_{i,t+p}$) is independent of being affected

[50] This includes unemployed and inactive status

[51] The proxy for tenure enters here when "different job" is defined. See the appendix for more details about the definition of tenure.

by the reduction of hours to 45 per week (i.e. the so-called *Conditional (mean) Independance Assumption (CIA)*).[52] When (10) holds, the expectation of (8) given the treatment can be represented as:

$$E\left[NE_{i,t+p}|\ NE_{it}=0,x_{it},\ D_{it}\right]=$$

$$E\left[NE^{0}_{i,t+p}|NE_{it}=0,x_{it}\right]+D_{i}E\left[NE^{1}_{i,t+p}-NE^{0}_{i,t+p}|\ NE_{it}=0,x_{it}\ ,D_{it}=1\right] \tag{11}$$

If we use a cross-section model to analyze employment losses during the period between January 2003 - January 2005 (i.e. p=2) of individuals employed in January 2003, and for simplicity we assume homogeneous effects, then (11) becomes:

$$E\left(NE_{i,05}|\ NE_{i,03}=0,x_{i,03},D_{i,03}\right)=x^{'}_{i,03}\beta+\alpha g_{1i,03} \tag{12}$$

where: $NE_{i,05}\ =1$ if, conditional on being employed in January 2003 (i.e. $NE_{i,03}=0$), individual i is not employed or employed at a different job at the end of the transition period (i.e. January 2005) and zero otherwise. $g_{1i,03}$=1 for individuals who are in the treatment group (i.e. working 46-48 hours in January 2003). $x^{'}_{i,03}$ is a vector of covariates measured in January 2003. α is the average impact of the reduction in working hours on employment to non employment transition for those individuals who were working between 46-48 hours per week in January 2003.[53] To include overtime (12) can be extended to:

[52] In the case of balance panel data, *CIA* is sufficient but not necessary since it is too strong. The reason is that *CIA* imposes that conditional on x, the treatment does not affect the untreated potential outcome. In the linear model this is equivalent to $E\left(u_{0,i}|x,D\right)=E\left(u_{0,i}|x\right)$ .Nevertheless, in difference in differences we only need that $E\left(u_{0,a}-\ u_{0,b}|x,D=1\right)=E\left(u_{0,a}-\ u_{0,b}|x,D=0\right)$, which is the so-called *common macro trend*. In case of repeated cross-section or unbalance panels we need to strenghten the common macro trend by adding the assumption of no systematical change in composition of the groups in terms of the untreated potential outcomes . This new assumption is redundant with balance panels since it will always be true. These two assumptions together are equivalent to the *CIA*, since as Lee (2005) points out *CIA* rules out systematic moves accross groups.

[53] As mentioned above, we assume homogeneous effects since α represents the change in the intercept between the treated and control groups. By neglecting heterogeneity and estimating the average effect of the reduction of standard hours as in (12), the expected value of the unexplained component of employment transitions,$E(\epsilon_i)$, also includes the term $E(\alpha_{i,03}$ - $\alpha)g_{1i,03}$, where $\alpha_{i,03}$ is the individual-specific effect of treatment in year 2003. Thus, for identification of α under the homogeneity assumption, we now require that: (a) homogeneity really holds (i.e. no heterogeneity), or (b) if homogeneity does not hold, then we have to assume that the heterogeneity in the effect of treatment has to be uncorrelated with their occurrence. This latter assumption is likely to hold

$$E\left(NE_{i,05}|\ NE_{i,03}=0, x_{i,03}, D_{i,03}\right) = x_{i,03}^{'}\beta + \alpha_s g_{1i,03} + \alpha_{ov} g_{2i,03} \tag{13}$$

Where the vector of covariates $x_{i,03}$ includes variables like: age, gender (female=1, male=0), education (6 categories), ln (hourly wage), region (12 dummies), size of the firm (6 categories), dummies for occupation (6 categories), industry (8 categories), unionization status (unionized=1, 0 otherwise) and the 1 year lagged weekly hours[54]. Again α_s is the average impact of the reduction in working hours on employment to non employment transition for those individuals who were working between 46-48 hours per week in January 2003 and α_{ov} measures the same but for people who were working overtime at that time (i.e. 49-60 hours). We estimate (13) for January 2002 -January 2003 and for January 2003-January 2005 obtaining the results displayed in Table 2:

Table 2: OLS estimation

	Employment Status in January 2003 given employment in January 2002			Employment Status in January 2005 given employment in January 2003		
	All	Men	Women	All	Men	Women
	(1)	(2)	(3)	(4)	(5)	(6)
Workers with 46-48 hours	-0.001	0.008	-0.022	0.020	0.013	0.028
	(0.014)	(0.015)	(0.032)	(0.015)	(0.017)	(0.038)
Workers with 49-60 hours	0.015	0.021	0.008	0.006	0.022	-0.041
	(0.018)	(0.019)	(0.045)	(0.019)	(0.020)	(0.045)
Observations	3535	2663	872	3028	2268	760

Note: control variables included are age, dummies for gender, educational level, occupation, industry, unionization status, region, size of the firm and two group dummies (i.e. standard and overtime groups). Covariates also include: logarithm of hourly wage and 1 year lagged weekly hours. The population includes all full-time dependent workers in the private sector who were working in January 2002 (for columns 1-3) and in January 2003 (for columns 4-6). Robust standard errors are given in parenthesis.

The estimates of the first column suggest that workers in both treatment groups (i.e. those who were working 46-48 hours and those who were working 49-60 hours) in January 2002 have

in our case since, due to the exogeneity of the policy change, cross-sectional units do not self-select into lower hours (i.e. treatment) based on their expected gains. Therefore, $E(\alpha_{i,03}$ - $\alpha)g_{1i,03} = 0$. If we think that the homogeneity assumption is less likely to hold, then heterogeneous effects can be introduced. To do this $g_{1i,03}$ could be interacted with the x variables. Thus, these interactive terms should capture the difference in the slopes between the treated and control groups. The cost of the heterogeneity is the curse of dimensionality problem, due to the potential too many interactions and covariates.

[54]This is to make the conditional mean independence assumption more plausible (Heckman, Ichimura, Smith and Todd (1998)). Despite the significance of this variable in the estimation our results of the effect of the reduction of standard hours does not change if it is excluded.

no different transitions to non employment when compared with those workers in the control group (44-45 hours) category. When we observe the fourth column there is a change on signs on the point estimates but they are still not significant. When we decompose the sample into men and women it is possible to observe that in both cases they are statisticaly insignificant. In the rest of the estimation we will focus on male results mainly for two reasons. Firstly, the female group is small and therefore it will give us very unprecise estimates (e.g. the female' control group has 218 and 190 women in each cross-section respectively). Secondly, and most important, this group seems to violate one of the assumptions as we will explain below. In the case of men, point estimates transitions for the 46-48 hours group (standard group hereafter) and for the 49-60 hours group (overtime hereafter) are not significantly higher than the transition of the control group in both periods (2002-2003 and 2003-2005).

6.3 Difference-in-Differences

We use a *difference-in-difference* (*DID*) approach to study if there is a specific effect during the January 2003- January 2005 period that was not present during the January 2002- January 2003 period. We define the pre-treatment employment transition to be the one between January 2002- January 2003 and the post-treatment employment transition to be the one between January 2003 - January 2005. In order to obtain the effect of the policy change on the employment transitions we estimate equation (14) for all full-time dependent workers (i.e. not self-employed) in the private sector who work between 44-60 hours per week.

$$E\left(NE_{i,t+p}|\ NE_{it}=0,x_{it},D_{it}\right)=x_{it}^{'}\beta+\alpha_s g_{1it}+\alpha_{ov}g_{2it}+\gamma_1 d_{t+p}+\gamma_2 g_{1it}d_{t+p}+\gamma_3 g_{2it}d_{t+p} \qquad (14)$$

Where d_{t+p} =1 if the new maximum standard hours were in place at "$t+p$" and zero otherwise, and $g_{1it}d_{t+p}$ is an interaction term composed by the time dummy (i.e. d_{t+p}) and the group dummy for those working 46-48 hours (i.e. g_{1it}). Similarly, $g_{2it}d_{t+p}$ is the interactive term for those working overtime (49-60 hours). Therefore, the parameters of interest will be γ_2 *and* γ_3. The estimates of the coefficients of the interaction terms are presented on panel A of Table 3.[55]

[55]For our estimations we are using those individuals who are employed at the begining of each transition period, which makes the adoption of a repeated cross section a better option than the use of the data as if it were a panel. This implies that some individuals will appear twice in the sample used in the analysis.

Table 3: Difference-in-Differences and GAP estimation

	January 2002 - January 2003 - January 2005			
A	High and Low skill workers			
	Difference-in-Differences		Gap (per overtime hour effect)	
	Dependent variable: NE_{t+p}		Dependent variable: NE_{t+p}	
	covariates with log(hourly wage)	covariates without log(hourly wage)	covariates with log(hourly wage)	covariates without log(hourly wage)
	(1)	(2)	(3)	(4)
Standard group	0.005	0.010	0.002	0.006
	(0.019)	(0.019)	(0.018)	(0.018)
Overtime group	0.008	0.013	0.002	0.003
	(0.025)	(0.025)	(0.004)	(0.004)
Observations	5084	5084	5084	5084
B	Low skill workers			
Standard group	0.008	0.013	0.002	0.006
	(0.025)	(0.025)	(0.025)	(0.025)
Overtime group	0.010	0.015	0.002	0.004
	(0.032)	(0.032)	(0.032)	(0.032)
Observations	3698	3698	3698	3698

Note: panel A displays estimates for all dependent workers in the private sector included in those occupations affected by the reduction of standard hours. Panel B, displays the estimates for all those in panel A but who have up to complete high school level of education only. Control variables include: age, dummies for educational level, occupation, industry, unionization status, size of the firm, region and two group dummies (i.e. standard and overtime). It also includes the logarithm of hourly wage, a time dummy and one year lagged weekly hours. Clustered standard errors (at the individual level) are given in parenthesis.

From the first column of panel A of Table 3, we observe that point estimates of the effect of the reduction of standard working hours increase the transitions from employment to non-

This may generate non independent observations for those individuals. A solution to this would be to use clustered standard errors (at the individual level). Also, in DID framework, the use of group*time dummies, when individual data is used, poses the same problem encountered when using macro data in microeconometric regressions, known in the literature as the Moulton problem. Moulton problem often generates downward-biased standard errors. In our case, the concern is that observations for individuals on the same group of hours in a given point in time might be correlated. Bertrand et al. (2004) have noted that matters can be further complicated if there is correlation over time within groups. In our case, this would occur if the idiosyncratic error of individuals on the same group of hours were correlated over time. As noted by Angrist and Pischke (2009) the debate on how to deal with these issues (when there is a small number of groups) has not reach a consensus. This is because, the best practice in cases where the presence of only a small number of groups or time periods advices against the adoption of standard errors clustered at the group*time level (to address the Moulton problem) or at the group level (to tackle serial correlation within groups). Given such uncertainty, prominence is given to the concern that the use of repeated observations on individuals very likely to generate correlation over time and therefore standard errors are clustered at the individual level.

employment in 0.5 percentage points for those who were working between 46-48 hours per week and in 0.8 percentage points for those who were working between 49-60 hours per week. In both cases the point estimates are insignificantly different from zero. Nevertheless, it is interesting that by comparing the point estimates of the first two columns of panel A we observe that the effect of the hourly wage is as predicted. When hourly wage is excluded, and therefore we allow for an increment in hourly wage, the point estimate of the effect of a reduction of standard hours on transitions to non-employment almost doubled (1.3 percentage points). It is important to notice that even though the effect of hourly wage are as predicted they are not strong enough to make the point estimates significant.

The overtime group has a wide range of hours since it includes individuals with 49 hours to individuals with 60 hours per week. This broad range might give unprecise results because the analysis above uses the interaction between the post-treatment indicator (i.e. d) with a binary indicator of the treatment group and therefore it assumes that, within groups (standard and overtime), the effect of the reduction of hours would be the same irrespective of the number of hours worked. This means that the effect would be the same if individual i works 49 hours or 60 hours per week. Therefore, following Stewart (2004) we can apply an alternative estimator, the so-called "*gap*" that in our case becomes "*hour gap*". This gap should capture the difference between the hours of individual i in period t with respect to the new standard in period $t+1$. Formally:

$$GAP = \left\{ \begin{array}{ccc} h_{it} - S_{t+p} & if & S_{t+p} < h_{it} \\ 0 & & else \end{array} \right\} \tag{15}$$

Where h_{it} is individual's i usual hours at time t, S_{t+p} is the relevant new standard at time $t+p$. The idea is to replace the overtime group dummy (g_{2it}) for the gap in equation (14). Results are presented in the third and fourth columns of Table 3. The estimated effects are smaller but with the same sign than those analized before. It is especially interesting the reduction of the point estimate in the overtime group (from 0.8 to 0.2 percentage points with hourly wages and from 1.3 to 0.3 percentage points without hourly wages). These are consistent with the interpretation of the coeficients when the gap is used. In these cases the interpretation is by "*unit gap*" which in our case is by "*overtime hour*". Hence, the fact of being in the overtime group and being treated increases transitions to non-employment in 0.2 percentage points per hour of overtime when hourly wages are included and in 0.3 percentage points per hour of overtime when hourly wages are excluded. In both cases, results are very small and not significantly different from zero.

Furthermore, because the employment effect may differ by skill levels we re estimate equation (14) and (15) but excluding high skilled workers. This means that we restrict the sample to those workers with less or equal studies than full high school. Results are presented in panel B of Table 3. They suggest that the reduction of hours does not affect employment transitions (i.e. there is an insignificant positive effect of between 0.8 - 1.3 percentage points depending if we are or we are not keeping hourly wages fixed). Similarly, for overtime workers, estimates suggest an insignificant effect on employment transitions (i.e. an insignificant positive effect between 1-1.5 percentage points depending on the inclusion or exclusion of hourly wages).

6.4 Hourly wages and monthly earnings

As pointed out throughout this study, the effects of hourly wages as a source of the so-called *indirect effect* is important in order to estimate the impact of a reduction of standard hours on employment transitions. This is because when weekly hours are reduced and monthly earnings are held constant, hourly wage increases inducing a substitution effect towards capital. This means that because of the imposibility of downward adjustment of the monthly earnings, the treatment affects indirectly employment transitions via hourly wages. Nevertheless, it could be the case that the mechanism of transmission of the indirect effect is not hourly wages, or at least not hourly wages alone. It could be the case that changes on monthly earnings are affecting employment transitions. To study this we re-estimate equation (14) but now replacing the logarithm of hourly wage for the logarithm of monthly earnings.

Table 4: estimation with monthly earnings instead of hourly wages

	Pooled OLS estimation January 2002 - January 2003 - January 2005	
	Dependent variable: NE_{t+p}	
	covariates with log(monthly earnings) (1)	covariates without log(monthly earnings) (2)
Standard group	0.009	0.010
	(0.019)	(0.019)
Overtime group	0.012	0.013
	(0.025)	(0.025)
Observations	5084	5084

Note: the sample includes all full-time dependent workers in the private sector. Control variables include: age, dummies for educational level, occupation, industry, unionization status, size of the firm, region and two

group dummies (i.e. standard and overtime), it also includes the logarithm of monthly earnings, a time dummy and one year lagged weekly hours. Clustered standard errors (at the individual level) are given in parenthesis.

By comparing the estimates of the first and second column of Table 4 we observe that the point estimates are almost the same in the case when we control for monthly earnings with respect to the case when we do not control for it. This suggest that there are no effects on employment transitions coming through monthly earnings.

One further complication may be the potential endogeneity of wages and employment transitions. If this is the case, then our estimates will be biased. A solution would be the use of an instrument which has to be correlated with hourly wage but not with employment transitions. To find such an instrument is not easy and the consequences of a bad instrument can be worse than the solution. The endogeneity of wages seems less likely due to our choice of post-treatment period. This is, given the non-anticipation behaviour on hours, employment and hourly wages analized above and by taking January 2003 (instead of January 2004) as the period when covariates are measured for the transition January 2003 - January 2005 (instead of January 2004 - January 2005), we are minimizing the possibility that hourly wages were jointly determined with employment. Also, in our favor is the evidence of strong wage rigidity in Chile (Cobb and Opazo 2008) which makes less likely the existance of endogeneity between wages and employment transitions.[56] Furthermore, in our favor is the fact that the expected point estimates coefficients behave as expected when hourly wages are included in regression (14) and also that monthly wages seem not to have an impact on points estimates of employment transitions.

6.5 Part-Time workers

A very tempting alternative to potentially explain the low magnitudes of the point estimates above would be Hart's proposition about change on the mix of employment. Recalling that in a simple model Hart (2004) shows that with two types of workers (full time and part time) where the latter ones work less than standard hours while the former ones work overtime, a reduction of standard hours will have two effects: firstly, it leads to overtime-employment substitution among full time workers. Secondly, leads the firm to increase the ratio of part time to full time employees due to a higher relative cost of the latter ones (i.e. a change on the mix of employment). Therefore, a change on the mix of employment (by vary the mix of part

[56] Cobb and Opazo (2008) point out that "t*he average length of time that it would take for the whole economy to adjust its wages is just over nine quarters, with some differences between economic sectors*".

time and full time workers) may serve to mitigate a tendency to reduce employment given the shortened standard work week.

The inconvenience comes from the labour reform itself. This is because part-time employment is defined in the Chilean Labour Code as up to a proportion of the full time standard employment workweek. Specificaly, says that part-time employment is defined as up to $\frac{2}{3}$ of the maximum standard workweek, which was equivalent to 32 hours before the reduction of hours and 30 hours after it. Hence, Hart's point is weakened here since there is a reduction for both groups, full-time with overtime and part-time.[57] Therefore, we should not expect a big effect of the mix of employment on overtime transitions to non employment. Despite the small number of people working in the 30 - 32 hours category it is possible to argue that the proportion of individuals working 30 and 32 hours does not change between 2002 and 2005. For example, 1.42% and 0.17% of the individuals working in the private sector were working 30 and 32 hours respectively in January 2002. In January 2005 the proportions were 1.38% and 0.18% respectively. This suggest no important movements between full-time and part-time workers.

6.6 Checking the Assumptions

In order to identify the effect of the policy change on employment transitions we introduced two assumptions. The first one is that workers in the control group must not have been affected by the reduction of the working hours. The second assumption is that there are no interactions between the group dummies and the time effects in the absence of the policy change. We check both assumptions here.

6.6.1 Checking the validity of the control group:

In order to test the first assumption we follow Crepón and Kramarz' (2002) idea but extend it by the use of usual hours (instead of actual hours). We estimate the change in usual hours between t and $t+p$ for those workers in the control group who were still employed at $t+p$. Then, if the assumption holds we should not expect to find significant differences in changes of usual hours during the period January 2003 - January 2005 with respect to the period January 2002 - January 2003. This should not be true for workers in the treatment groups. This

[57] It is also true that while full-time faced a 3 hours cut, part-time faced a 2 hours cut, hence we could exploit that variation on the magnitud of the policy, but unfortunately there is few people working in that range of hours.

test assumes no employment effects since it uses individuals still employed after the transition, which is consistent with what we have found on our estimations above.Therefore, we estimate the following cross-sectional models for January 2002- January 2003 and for January 2003- January 2005:

$$E\left(\Delta Hours_{i,t}^{t+p} |\ NE_{it} = 0, NE_{i,t+p} = 0, x_{i,t}, D_{i,t}\right) = x_{it}^{'}\beta + \alpha_c g_{0it} + \ \alpha_s g_{1it} + \alpha_{ov} g_{2it} \tag{16}$$

Where $\Delta Hours_t^{t+p}$ is the change of usual hours between t and t+p, and $x_{it}^{'}$ is a vector of controls which includes the same variables than in equation (14) except for the intercept which is excluded here.[58] The other three variables ($g_{kit}, where\ k = 0, 1, 2$) are dummies equal to one for 44-45, 46-48 and 49-60 hours respectively and zero otherwise. The estimates are presented in the first two columns of panel A of Table 5.

[58]Otherwise the three group dummies will sum up to one which generates multicollinearity if an intercept is included.

Table 5: Variation in usual hours and log(hourly wage) during transition periods given employment at the end of the period

A	Var. in usual hours between January 2002 - January 2003 given employment in January 2003	Var. in usual hours between January 2003 - January 2005 given employment in January 2005	Pooled OLS
	(1)	(2)	(3)
Control group	-0.19	-0.25	-0.09
	(0.51)	(0.52)	(0.39)
Standard group	-0.23	-2.92***	-2.86***
	(0.50)	(0.51)	(0.38)
Overtime group	-0.54	-0.89*	-0.26
	(0.50)	(0.50)	(0.39)
B	Var. in ln(hourly wages) between January 2002 - January 2003 given employment in January 2003	Var. in ln(hourly wages) between January 2003 - January 2005 given employment in January 2005	Pooled OLS
Control group	0.021	0.034	0.012
	(0.028)	(0.028)	(0.014)
Standard group	0.031	0.066***	0.031**
	(0.028)	(0.028)	(0.013)
Overtime group	0.033	0.056**	0.020
	(0.029)	(0.029)	(0.016)
Observations	2080	1824	3904

Note: for the first two columns of panel A control variables include: age, dummies for educational levels, occupation, industry, size of the firm, region, unionization status, three group dummies (control, standard and overtime, thus we drop the intercept) and the logarithm of hourly wage. For the third column of panel A control variables are the same than for the first two columns plus the inclusion of a time dummy and the interactions of all the group dummies with the time dummy. Robust standard errors are reported in parenthesis for the first two columns and clustered standard errors (at the individual level) are given in parenthesis for the last column. For panel B, the covariates are the same except the logarithm of hourly wage which is excluded since in panel B the dependent variable is the variation on the logarithm of hourly wage. *p<0.10, **p<0.05 and ***p<0.01.

Estimates suggest that in the first period there are no significant variation in usual hours, which seems to support our ex-ante exploration of the distribution of usual hours in section 5. Nevertheless, estimates for the second period seems to suggest that there are significant effects

in the standard and overtime groups at 1% and 10% of significance respectively. This negative variations on usual hours might happened even in the abcense of the reduction of standard hours, hence we extended the model in (16) to try to estimate if there was a significant effect in January 2003- January 2005 that can not be found in January 2002- January 2003. In order to do this we estimate (17):

$$E\left(\Delta Hours_{it}^{t+p} \mid NE_{it}=0, NE_{i,t+p}=0, x_{it}, D_{it}\right) = x_{it}^{'}\beta + \alpha_s g_{1it} + \alpha_{ov} g_{2it} + \gamma_1 d_{t+p}$$

$$+\gamma_2 g_{1it} d_{t+p} + \gamma_3 g_{2it} d_{t+p} + \gamma_4 g_{0it} d_{t+p} \tag{17}$$

Where $g_{kit}d_{t+p}$ $(k = 0, 1, 2)$ represents the interaction variable between the post treatment dummy (d_{t+p}) and the respective group dummy (g_{kit}). $x_{it}^{'}$ does not include a constant and the base category are those workers in the control group in January 2002 - January 2003. Estimates are presented in the last column of Table 5 and suggest that the change of usual hours in period January 2003- January 2005 is not significantly different from the change of usual hours during the period January 2002- January 2003 for the control and overtime group whereas for the standard group the estimates suggest that the change of usual hours it is different and significant at 1% respectively. This suggest that our control group is a valid one.[59] This was not the case for females and that is why we decided to exclude them from the analysis. For the standard group, usual hours decrease by almost the same amount of the statutory reduction of 3 hours (i.e. 2.86 - (-0.09) = 2.77). For the overtime group the results were unexpected since we obtained no significant variation of usual hours instead of expected (theoretical) increment. This results may be explained because for workers in the overtime group, the reduction of the standard workweek produces only an income effect, which will tend to reduce desired hours of work, potentially offsetting the increase in hours coming from the labour-demand side.

Panel B of Table 5 presents the results of the estimation of equations 16 and 17 when the dependent variable is the variation (change) of ln(hourly wages) for the control, standard and overtime groups. Results suggest no significant changes on ln(hourly wages) for any of the three groups during January 2002 - January 2003, but results suggest a significant increment on ln(hourly wage) for the standard and overtime groups for January 2003 - January 2005. Once POLS is estimated in column 3, only the effect for the standard group remains significant.

[59] In our favor is one of the specifications of the labour reform. It states that "....all individuals whose contracts specify less or equal hours than 45 per week should not be affected by the reduction of hours" (own translation). Law 19758. 27-09-2001. Library of the Congress.

Specificaly, for the standard group, hourly wage increase by 1.9% (i.e. 0.031 - (0.012) = 0.019). Thus, a decrease of one hour per week, from the original 48 hours (i.e. a 2% reduction) generated a 1.9% increment in hourly wage, which is in line with our findings in figure 6 suggesting wage compensation. This result imply that monthly earnings for individuals in the standard group remained close to the same after the reduction in hours. We can also observed from panel B of Table 5 that the result also suggest no significant spillover effects on hourly wages of the control group.

The fact that results for the standard group are as expected with respect to reduction of usual hours and increments on hourly wages but not on employment seems peculiar. One possible explanation for the non significant effect on employment might be the long period of adjustment given by the government to firms (almost 3 years), which might allowed firms to adjust their production process or the productivity of workers. Another reason might be that the difference in skills between employed and unemployed makes substituting the lost hours of the former with jobs for the latter more difficult (this can be reinforced by high severance payments as in the Chilean case). A further potential reason comes from the labour supply side. On the one hand, for workers in the overtimegroup, the reduction of the standard workweek produces only an income effect, which will tend to reduce desired hours of work, potentially offsetting the increase in hours coming from the labour-demand side. On the other hand, for workers in the standard group, the policy also produces a substitution effect, which tends to raise desired hours of work and reduces the likelihood that work-sharing leads to employment growth. These labor supply adjustments are likely to have significant dampening effects on any potential employment-hours tradeoffs (as Skuterud 2007 points out).

6.6.2 Checking the common macro trend:

The second assumption is that there are no interactions between the group dummies and the time effects in the absence of the policy change (i.e. common macro tren). The usual method to test this is to use a pre-treatment period. In our case, we can do that but at the cost of sacrificying the model with hourly wages. We can use equation (14) in a period where there is no pre-announcement of reductions of standard hours. A candidate would be a pre-treatment period like January 1999 - January 2000 and January 2000 - January 2001. Since we do not have hourly wages for this period we estimate equation (14) but excluding the logarithm of hourly wages from vector x. Estimates are presented in Table 6.

Table 6: Falsification test for January 1999 - January 2001

	Pooled OLS estimation January 1999 - January 2000 - January 2001
	Dependent variable: NE_{t+p}
Standard group	0.001
	(0.017)
Overtime group	-0.003
	(0.020)
Observations	5568

Note: control variables include: age, dummies for educational levels, occupation, industry, firm size, unionization status, region, two group dummies (standard and overtime group), 1 year lagged weekly hours, a time dummy and interaction of all the group dummies with the time dummy. Clustered standard errors at the individual level are given in parenthesis.

Results from the first column of Table 6 show that both interactive terms are not significantly different from zero, therefore the assumption of zero interaction terms in the absence of the reduction of standard hours is well supported by the data.

7 Robustness

It is important that our results do not depend on any specifical construction. In order to analize the sensitivity of our estimations we modify the underlying especificantion in several ways. Special attention is given to the definition of time periods, control group and model of estimation. Also, potential problems with measurement error are analized.

7.1 Definition of Control Group

The analysis of employment transitions has been carried out by comparing a treatment and a control group, where this latter one has been defined to be those individuals who work between 44-45 hours per week. The advantage of this narrow range is that we make the control group as similar as possible to those in the treated group (in term of unobervables) and therefore it is expected that individuals in the control group respond to shocks in similar ways than individuals in the treatment group. Nevertheless, there is a trade off, since widening the control group range has also some advantages: firstly, increases the number of obervations and

therefore increase the precision of the estimation (*ceteris paribus*). Secondly, it diminish the problem created by potential missclasification of hours. Thirdly, reduces the impact of the threats to the identification strategy like spillovers effects and substitution between groups.

We re-estimate equation (14) with diferents definitions of control group. Results are presented in Table 7 in the appendix. In all the cases, as before, the point estimates are not significant. Furthermore, point estimates are robust to the definition of control group, all of them in the range (0.4 - 1.3 percentage points) for the standard group and in the range (0.7 - 1.9 percentage points) for the overtime group. We also observe that the effect of hourly wages are as expected in all the cases, this is increasing the magnitudes of point estimates when they are positive, although they remain insignificant.

7.2 Definition of Time Periods

It has been acknowledge that one problem is the potential anticipation effects due to the pre-announcement of the policy. The data analized in section 5 above seems to suggest that the anticipation effect was not significant and due to this result we defined January 2002 - January 2003 as the "pre policy" period and January 2003-January 2005 as post policy period. This conclusion was supported by the results displyed in figure 4. However, because this is an important issue to take into account when we further check the robustness of our results with respect to the definition of periods, we investigate the sensitivity of our results when the definition of pre and post policy period are modified.

Results are presented in two panels. Panel A presents estimates when time period is modified, for example: instead of being January 2002 - January 2003 - January 2005, it becomes February 2003 - February 2003 - February 2005 for the first two columns pf panel A and March 2002 - March 2003 - March 2005 for the last two columns of panel A. Panel B present the result for the alternative especification of post-treatment period (option a) presented in subsection 5.3.. Especification which uses a moving window from January 2003 - January 2004 in the first two columns of panel B and January 2004 - January 2005 in columns 3 and 4 of panel B. In either case, results are not significant. Therefore, our estimates seems to be robust to the definition of time period (panel A) and to the definition of post-treatment period (panel B).

7.3 Model specification

All estimations presented so far have used *Ordinary Least Squares* (*OLS*) mainly due to its simplicity (and the potential need for instrument). Furthermore, the model is saturated and as Wooldridge (2002) points out, in saturated models *OLS* is a good aproximation when most of the covariates are discrete, which is our case. Nevertheless, it is well known that *OLS* has some problems when the dependant variable is a dummy[60] and binary models have been proposed for these cases. Therefore, we re estimate equation (14) but now using a probit model. Results for the marginal effects are presented in Table 9 in the appendix and suggest that OLS results are similar to those obtained with probit. Nevertheless, this kind of non linear transformation of single index models (i.e. assumes linearity in the group and time effects) like probit has been criticized of being restrictive (Heckman 1996). Due to this, Athey and Imbens (2006) developed a non-parametric generalization of differences-in-differences called *Changes-in-Changes* (*CIC*). The costs of this generalization are twofold: firstly, requires more data due to its non-parametric nature. Secondly, when the outcome is discrete it is only possible to estimate bounds. Thus, I left this as a potential extention of the robustness checks.

7.4 Measurement error

The *EPS* panel used in our study, as every survey data, might be subject of measurement error. Furthermore, the fact of using self reported employment histories may aggravate the problem. This is especialy important for hours of work, since on the one hand, measurement error in this variable could lead to misclassification of individuals into hours groups and thereby to a dilution of the estimated effect on employment transitions. On the other hand, it will affect hourly wages, since they are constructed as a combination of monthly wage, weeks and hours. Finaly, the *EPS* panel does not have a direct question on overtime which may lead to misclassification of individuals into hours groups.[61] All these measurement error effects will bias our estimates. Therefore, this section attempts to measure the magnitude of this potential bias by using sensitivity analysis.

Our first analysis deals with misclassification coming from the fact that the *EPS* panel does not have a direct question of overtime. This would generate a downward bias on our results,

[60] These problems are: a) the predicted probabilities may lie outside the range $[0, 1]$ .b) non normality of the error term and c) heteroskedasticity.

[61] For example, someone who report 48 hours a week of usual hours might imply: (a) 48 normal hours and zero overtime or (b) 45 hours plus 3 of overtime. This is important since in the first case the individual will be classified to the treatment group and in the latter case to the control group.

giving us a lower bound of the effect of the policy change on employment transitions. To test how important is this effect and based on *CASEN* 2000[62], we observe that the probability of working overtime is affected mainly by industry category. In particular, workers on minning and transport sectors have higher probability of working overtime hours. This is in line with *ENCLA* 2002[63] which shows that minning and transport category have the longest workweek. Therefore, if we do have an important missclasification due to the lack of a direct question of overtime, we should expect our results to change by excluding workers in the minning and transport categories, since they should have higher probabilities of misclassification. Results are presented in Table 10 in the appendix and suggest that estimates do not change when we exclude workers in the mentioned categories, hence seems to be that missclasification is not a significant problem in our case. Furthermore, distributions of usual hours in January 2003 and January 2005 in figure 3 show high peaks at the legal maximum of standard hours (48 and 45 respectively). Hence, it is likely that responses do not include overtime.

Our second analysis exploits the accuracy of the measure of hours by region. This is because some regions in Chile have a very high proportion of workers with special distribution of hours, which are not only concentrated in the minning and transport sectors but also on services related to them. *Antofagasta* and *Atacama* regions concentrate a high proportion of the minning industry (which has a special distribution of hours), and therefore most of the services there are related to the minning sector. For example, they can concentrate weekly hours in 4 days of 12 hours each and then 3 day of holidays, or in a more extreme case, employees can work 20 days in a row and then have the proportional resting days, but the average has to be 48 hours per week (before January 2005, or 45 hours after January 2005).[64] This variation of hours may introduce noise in the measure of weekly hours. Therefore, we exclude these two regions to obtain better measures of hours. Results seems to be robust to this specification since they suggest that by excluding noisier regions, in terms of measure of weekly hours, results do not significantly vary.

7.5 General equilibrium effects

The effect of a reduction of standard hours on employment transitions has been analized from a partial equlibrium perspective which basically assumes that individuals not directly affected

[62] *CASEN* 2000 (Encuesta de caracterizacion social) is a cross-section data carried out at the end of 2000. It includes one question about normal (standard) weekly hours agreed with the employer or defined in the contract and another question on actual weekly hours. Therefore, it is possible to know who is working overtime.

[63] Encuesta Laboral.

[64] Observatorio Laboral 7. Septiembre 2002

by the policy are indeed not affected at all, or in Lee's words...."*we do not allow for externalities of the treatment*". This is in line with most of the policy evaluation literature, since it is a reasonable assumption when policy interventions have small scale (e.g. small training programmes) and allow researchers to avoid time consuming especification of general equilibrium models which sometimes require many more assumptions. Nevertheless, when the intervention has a bigger scale, like changes in regulation (e.g. changes of the minimum wage, cut of standard hours or massive training programmes among others.) which affect a broader range of the population, then the support for a partial equilibrium approach weakens.

In our case, a cut of standard hours to 45 per week affected (treated) all workers above that threshold. If there are externalities to this policy, then it might affect those below the threshold or those above the threshold but in not affected jobs or those individuals who were not employed at that time. In the first case, we check potential effects on individuals below the threshold when we check the assumption that the policy change do not affect the control group, and we coud not found any significant effect. This is supported by the policy change that explicitly mention that individuals with less (or equal) hours than the new threshold should not be affected. In the second and third cases, if there are externalities then they should be reflected somehow on earnings (or hourly wages) and therefore should be internalized by the inclusion of it. Furthermore, if spillovers and substitution effects were important in our case they should affect our results when the control group range was broadened, which did not happen. Hence, we do not claim that there are no general equilibrium effects but seems to be that if they exist it effects should be not highly significant.

8 Conclusion

Many countries around the world have implemented or discussed worksharing policies like reductions of the maximum standard weekly hours, usually as a way of decreasing high rates of unemployment. Despite its popularity, theoretical evidence concludes that the effect of a reduction of standard hours on employment is ambiguous and therefore remains an empirical question.

The scarse micro-econometric evidence can be grouped in those studies which analize reduction of standard hours derived from collective bargains and those derived from change in regulation. The first group usually exploit panel data methods where the dependent variable is employment and one of the covariates is standard hours. Since standard hours usually are jointly determined with employment, instruments for standard hours are needed. Therefore,

these studies relies on the exogeneity of the instruments. An alternative approach has been the use of an exogenous changes of legislation. Most of the evidence of this approach suffers from simultaneity problems since in general a reduction fo standard hours has been jointly implemented with higher flexibility and/or financial incentives which do not allow to differentiate the effect of each policy. The only exemptions have been the reduction of 40 to 39 hours in 1982 in France studied by Crepón and Kramarz (2002) and the reduction of 44 to 40 hours in the Canadian region of Quebec. The problem with both of them is the lack of crucial variables like hourly wages and/or usual hours.

We exploit a variation of the labour regulation in Chile which includes a reduction of the maximum standard hours from 48 to 45 hours per week to study the effect of the reduction of hours on employment transitions. The characteristics of the labour reform allow us to have a pure reduction of standard hours (i.e. it is not jointly implemented with other policies). Also, relative to Crepón and Kramarz (2002), the advantage of our data (*EPS* Panel) is that *EPS* Panel includes information related to the employment history of individuals, which includes hourly wages and usual hours before the implementation of the reduction of hours. A major issue is the potential anticipation effects due to the fact that there were a pre-announcement just before the initial period considered in our study. Nevertheless, given the long period of adjustment established in Chilean legislation this is unlikely. We check the behaviour of crucial variables to support this hypothesis and all of them suggest no anticipation effects.

Therefore, our contribution is that we tackle the main drawbacks of previous empirical evidence on the employment effects of a reduction of working hours. This means firstly that we use a dataset which includes crucial variables like hourly wages and usual hours before the implementation of the policy change. Secondly, we use a pure reduction of working hours, i.e. there are no joint policies implemented so we can isolate the pure effect of the reduction of hours. Thirdly, given the nonanticipation effects we take the policy change as exogenous eliminating the endogeneity between hours and employment.

Our results suggest no significant direct effects of the reduction of standard hours on employment transitions (i.e. no effect on excess job destruction). These effects remain insignificant when the indirect effect from hourly wages is allowed. These findings are robust to several specifications. We also find that individuals affected by the reduction of standard hours work less hours and get higher hourly wages (i.e. wage compensation). Therefore, the results for Chile are in line with most of the evidence of European countries and Canada. Our results of non significant effect of excess of job destruction and the negligible variation on overall employment suggest that there is little support for work-sharing policies as a job-creation strategy.

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9 Appendix

Description of the Chilean Labour Reform of December 2001

1. In reference to the separation of workers and layoff costs, before the reform there were 3 ways of finishing the contract between a company and an employee:

a) Due to "business (or economic) reasons": those include the modernization or rationalization of the company, lower productivity, change in the economy or in the market and the lack of the employee' appropriate skills. By using this way of finishing the contract the employer has to pay severance payments of 1 month (30 days) per worked year with a maximum of 11 years.

b) Due to causes that can not be attributable to the employee: among these are the employee's death, agreement between employer and employee, employee's resignation, end of the job or service that originated the contract.

There is no severance payments in this case.

c) Due to causes that can be attributable to the employee: these include damage to the company's property, violence against a peer and/or superior, skipped some of his/her contractual duties and so on.

There is no severance payments in this case either.

The reform basically incremented fines when firms invoke the wrong cause for separating an employee.[65]The reform also modified the procedures by which severance payments were paid. This is, before the reform, there were no specifications on how indemnizations had to be paid, so worker and employer could negotiate how to do it. The reform stated that indemnizations in all the above cases have to be paid at once, when the contract ends or it can be paid in instalments (including readjustments due to interest). It also states that if severance payments are not paid as stipulated above, the court can increase them in 150%.

[65]If the employer can not accredited the causes that originated (a) (in case the employer states (a)) the court could increment severance payments in 20%. The reform increased this fine to 30%. It also eliminated the lack of the employee' appropriate skills as a cause of invoking (a), hence if the employer fires the employee anyway, the reform states that the employer has to increase the payment in 50%. Before the reform, in case of wrongly invoking (b) the court could increment the indemnization in 20%. The reform increased it to 50%. The same will happens if the company does not specify any of the above alternatives of terminating the contract.

Finally, the reform incorporated that if the employer fires a worker due to practices against unionization, then this layoff will not be effective. Furthermore, the company will have two options: firstly, reincorporate the fired worker or secondly, if the worker does not want to come back to the company then the employer has to pay the severance payments per year to the worker plus an additional severance payment equivalent to 3 to 11 months per year worked depending on the decision by the court.

2. In reference to the exceptional distribution of the working time in some industries, before the reform of 2001, the Ministry of Labour (by the direct authorization of its Undersecretaryship of Labour) allowed the possibility of establishing exceptional distribution of the working time and leisure different from the ones allowed by law given the particular characteristics of the job (this is very important in the mining industry and salmon fisheries since those activities are usually located far away from urban centres or have some peculiarities).

The modification established by the reform is that the authorization given by the Undersecretaryship of Labour will last only 4 years (so now it has a limit), the same will occur with the renewals and it also has to be authorized by the employees. If all that happens, then the Undersecretaryship of Labour "might" do the renewal (i.e. completely discretional).

3. With respect to over time hours, before the reform, overtime hours have to be agreed between employer and employee with the only requisite that it had to be explicitly specified in the contract or in a posterior document. The overtime premium was 50% of the hourly wage. The reform did not change the direct cost of the overtime. The premium remained at 50% of the hourly wage. Nevertheless, the reform stated that over time hours can only be agreed for a particular or temporal necessity of the company and that they have to be specified in a document and the maximum period of the agreement can not be superior to three months, although it is possible to renew it.

4. In reference to changes on collective barging relationship, the reform made more expensive the replacement of workers on strike. This is because before the reform employers could replace workers on strike from the first day only if the employer' last offer ensured the existing benefits adjusted by the inflation of the period of the duration of the contract. If not, then the employer could only replace workers after 15 days of strike. Also, employees that have to go to strike when the majority of workers decided it could choose to return to

work after 15 days since the beginning of the strike. The reform added that the employer can replace workers on strike (as above) but only if it pays them (workers on strike) a bond of 4 UF[66] per replaced worker.

5. In this category we include modifications on working privileges, fines that limit the management of the firm, higher fines and better supervision from the Undersecretaryship of Labour (with 300 extra labour agents).

a) In reference to working privileges, the reform creates a privilege of 40 days for those workers who participated on the assembly that generated the union.[67]

b) With respect to the fines that limit the management of the firm, the reform modifies the law that regulate business management in the sense that it makes it more rigid. This is because it was added that "any alteration made to the legal identity, division of the company or loosening of individual and collective labour rights (i.e. wage and indemnization per year of service among the former ones and the right to unionize and collective barging among the latter ones) will constitute a subterfuge to avoid labour and pensions obligations" (and then a fine like the ones presented in (c) have to be applied).

c) With respect to the increment of fines, before the reform there were fines between 1-10 UTM[68] depending on how big the fault was plus 0.15 UTM per worker affected for the fault (this applies to any fault which does not have a specified fine in any other part of the law). The reform increased these fines to 1-20 UTM, but if the employer has more than 50 workers then the fine increases to 2-40 UTM, and if he has more than 200 workers it will increase to 3-60 UTM.

[66] UF means "Unidades de Fomento". The UF was determined by law in it origins and indexed to the monthly inflation (currently has a value around 21 pounds).

[67] 10 days before the assembly and 30 days after the assembly.

[68] UTM means Unidades Tributaries Mensuales. Similar to the UF but its value is around 37 pounds (also indexed by inflation).

Description of Variables

Dependent Variable In our first especification, the dependent variable (NE) was defined as a dummy:

$$\left\{ \begin{array}{ll} = 1 \;\; if & individuals\ are\ not\ employed\ at\ the\ end\ of\ the\ transition\ period \\ = 0 \;\; if & individuals\ are\ employed\ at\ the\ end\ of\ the\ transition\ period \end{array} \right\}$$

for example for the cross section January 2002 - January 2003: NE=1 if, conditional of being employed in January 2002, individual i is not employed in January 2003 and NE=0 if, conditional of being employed in January 2002, individual i is employed in January 2003. The same definition applies for period January 2003 - January 2005.

The definition presented above implies that NE=0 if individual i is still employed in the next period. This will include all those individuals not affected by non-employment spells plus those individuals affected by non-employment spells but who got a different job and are employed at the the end of the transition period. For example: lets take the period January 2003 - January 2005. All individuals are employed in January 2003, then NE=0 if individuals are still employed in January 2005. This includes all those who remain in their jobs for the whole period (January 2003-January 2005) plus those who loose their job at some point and obtain a new job before January 2005. This latter group should not be consider as not affected since they indeed loose their job. Due to this, the results presented in section 6 come from the following especification of the dependent variable:

(NE) is a dummy variable:

$$\left\{ \begin{array}{ll} = 1\ if & individuals\ are\ not\ employed\ or \\ & employed\ in\ a\ different\ job\ at\ the\ end\ of\ the\ transition\ period \\ = 0\ if & individuals\ are\ employed\ in\ the\ same\ job\ \ at\ the\ end\ of\ the\ transition\ period \end{array} \right\}$$

Since we do not have a direct question on tenure, to define "same job", we generate a variable tenure=1 if individual i is employed in January 2005 in the same occupation, in the same industry and in the same region than in January 2003. Results does not change much

with respect to the previous specification of the dependent variable. Results presented come from this latter especification.

Covariates

Gender:

dummy variable which is: $\left\{ \begin{array}{l} = 1 \;\; if \quad Female \\ = 0 \;\; if \quad Male \end{array} \right\}$

Age:

Age of individual i.

Schooling:

We construct seven categories (s) based on years of education. These categories are:

s=1 if individual i has no education, pre-school education or kidergarten.
s=2 if individual i has 1-8 years of education (first level).
s=3 if individual i has special education (handicap).
s=4 if individual i has 9-12 years of education (Scientific-Humanist -second level).
s=5 if individual i has 9-12 years of education (Technical-Professional -second level).
s=6 if individual i has a degree from a Technical-Professional Institute (third level).
s=7 if individual i has a degree from a University (or M.A., Ph.D.) (third level).

then we create one dummy per category.

Region:

We include dummy variables per region. Chile had 13 regions until 2006. Currently there are 15 regions due to a sub-division of two of the former ones (Region de los Lagos and Region de Tarapacá). Given that during the period of analysis Chile was experienced the transition from having 13 regions to 15, we reclassified those who reported any of the two new regions as part of the respective older regions.

Occupation:

We construct dummy variables per occupation category. We follow the International Standard Classification of Occupations ($ISCO$) of the International Labour Organization (ILO).

These are the major groups:

1. Managers.
2. Professionals.
3. Technicians and associate professionals.
4. Clerical support workers.
5. Service and sales workers.
6. Skilled agricultural, forestry and fishery workers.
7. Craft and related trades workers.
8. Plant and machine operators, and assemblers.
9. Elementary occupations.
10. Armed forces occupations

For the purposes of our analysis, we drop the first two categories and the last one, since they were not affected by the labour reform, That is why in table 1, occupation only has 7 categories (from 3 until 9).

Industry:

We construct dummy variables per industry category. We follow the International Standard Classification of Industry.

The major categories are:

1. Agriculture, Hunting, Forestry and Fishing.
2. Minning and Quarring.
3. Manufacturing.
4. Electricity, Gas and Water supply.
5. Construction.
6. Commerce, Hotels and Restaurants.
7. Transport, Communications
8. Financial Intermediation.
9. Public, Social and Personal Services.

Union:

Dummy variable which is: $\left\{ \begin{array}{ll} = 1 & if \quad individual\ i\ is\ unionize \\ = 0 & if \quad individual\ i\ is\ not\ unionize \end{array} \right\}$

Size of the firm:

Dummy variables per size category. These categories are:

Size=1 if individual i works in a firm with 1 employee
Size=2 if individual i works in a firm with 2-9 employees
Size=3 if individual i works in a firm with 10-19 employees
Size=4 if individual i works in a firm with 20-49 employees
Size=5 if individual i works in a firm with 50-99 employees
Size=6 if individual i works in a firm with 100-199 employees
Size=7 if individual i works in a firm with 200-499 employees

Size=8 if individual i works in a firm with more than 500 employees

Table 7: Different definitions of control group

	control group defined as (43-45 hours)		control group defined as (42-45 hours)	
	Dependent variable: NE_{t+p}		Dependent variable: NE_{t+p}	
	covariates with Log(hourly wage)	covariates without Log(hourly wage)	covariates with Log(hourly wage)	covariates without Log(hourly wage)
	(1)	(2)	(3)	(4)
Standard group	0.005	0.007	0.004	0.006
	(0.019)	(0.019)	(0.019)	(0.019)
Overtime goup	0.009	0.012	0.007	0.011
	(0.025)	(0.025)	(0.025)	(0.025)
Observations	5100	5100	5193	5193
	control group defined as (41-45 hours)		control group defined as (40-45 hours)	
	Dependent variable: NE_{t+p}		Dependent variable: NE_{t+p}	
	covariates with Log(hourly wage)	covariates without Log(hourly wage)	covariates with Log(hourly wage)	covariates without Log(hourly wage)
Standard group	0.007	0.010	0.009	0.013
	(0.020)	(0.020)	(0.018)	(0.018)
Overtime goup	0.009	0.014	0.011	0.019
	(0.026)	(0.026)	(0.024)	(0.024)
Observations	5249	5249	5319	5319

Note: control variables include age, dummies for education, occupation, industry, size of the firm, unionization status, region, logarithm of hourly wage, one year lagged weekly hours, 2 group dummies (standard and overtime groups) and the interaction of all the variables with the time dummy. Clustered standard errors at the individual level are given in parenthesis.

Table 8: Different definitions of time periods

A	February 2002 - Feb. 2003 - February 2005		March 2002 - March 2003 - March 2005	
	Dependent variable: NE_{t+p}		Dependent variable: NE_{t+p}	
	covariates with Log(hourly wage)	covariates without Log(hourly wage)	covariates with Log(hourly wage)	covariates without Log(hourly wage)
	(1)	(2)	(3)	(4)
Standard group	0.004	0.008	0.006	0.010
	(0.021)	(0.021)	(0.020)	(0.020)
Overtime goup	0.008	0.011	0.007	0.009
	(0.024)	(0.024)	(0.023)	(0.023)
Observations	5073	5073	5080	5080
B	Second period: January 2003 - January 2004		Second period: January 2004 - January 2005	
	Dependent variable: NE_{t+p}		Dependent variable: NE_{t+p}	
	covariates with Log(hourly wage)	covariates without Log(hourly wage)	covariates with Log(hourly wage)	covariates without Log(hourly wage)
Standard group	0.002	0.005	0.009	0.0014
	(0.020)	(0.020)	(0.019)	(0.019)
Overtime goup	0.004	0.007	0.0012	0.019
	(0.025)	(0.025)	(0.023)	(0.023)
Observations	5085	5085	5082	5082

Note: control varaibles include age, dummies for school, occupation, industry, size of the firm, region and unionization status, one year lagged weekly hours, group dummies (standard and overtime group) and interactions of all the previous variables with the time dummy. Cluestered standard errors at the individual level are given in parenthesis.

Table 9: Marginal effects of the Probit estimation

	Probit	
	Dependent variable: NE_{t+p}	
	covariates include Log(hourly wage)	covariates do not include Log(hourly wage)
	(1)	(2)
Standard group	0.005	0.011
	(0.020)	(0.020)
Overtime group	0.009	0.015
	(0.023)	(0.023)
Observations	5084	5084

Note: control variables include age, dummies for education, occupation, industry, size of the firm, unionization status, region, group dummies, logarithm of hourly wage, a one year lagged weekly hours and the interactions between all previous variables and the time dummy. Clustered standard errors at the individual level are given in parenthesis.

Table 10: Exclusion of some Industries and Regions

	No minning nor Transport sector		No Antofagasta nor Atacama regions	
	Dependent variable: NE_{t+p}		Dependent variable: NE_{t+p}	
	covariates with Log(hourly wage)	covariates without Log(hourly wage)	covariates with Log(hourly wage)	covariates without Log(hourly wage)
	(1)	(2)	(3)	(4)
Standard group	0.007	0.012	0.006	0.010
	(0.020)	(0.020)	(0.019)	(0.019)
Overtime group	0.009	0.014	0.011	0.016
	(0.026)	(0.026)	(0.025)	(0.025)
observations	4607	4607	4724	4724

Note: covariates include age, dummies for education level (and type), occupation, industry, size of the firm region, logarithm of hourly wage, group dummies, a one year lagged weekly hours and the interactions of the previous variables with a time dummy. Clustered standard errors at the individual level are given in parenthesis.

Figure 2: Employment in a monopsony

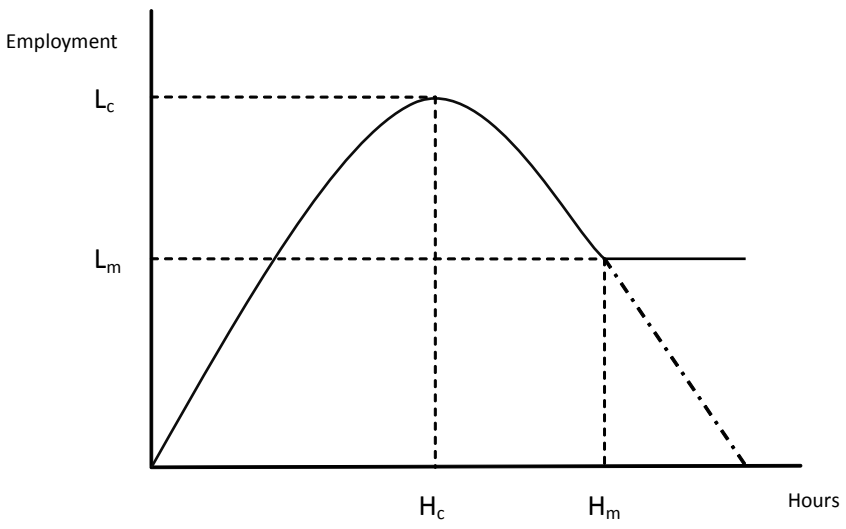

Figure 3: Distribution of usual hours by Gender

Figure 4: Evolution of usual hours 2002 2006

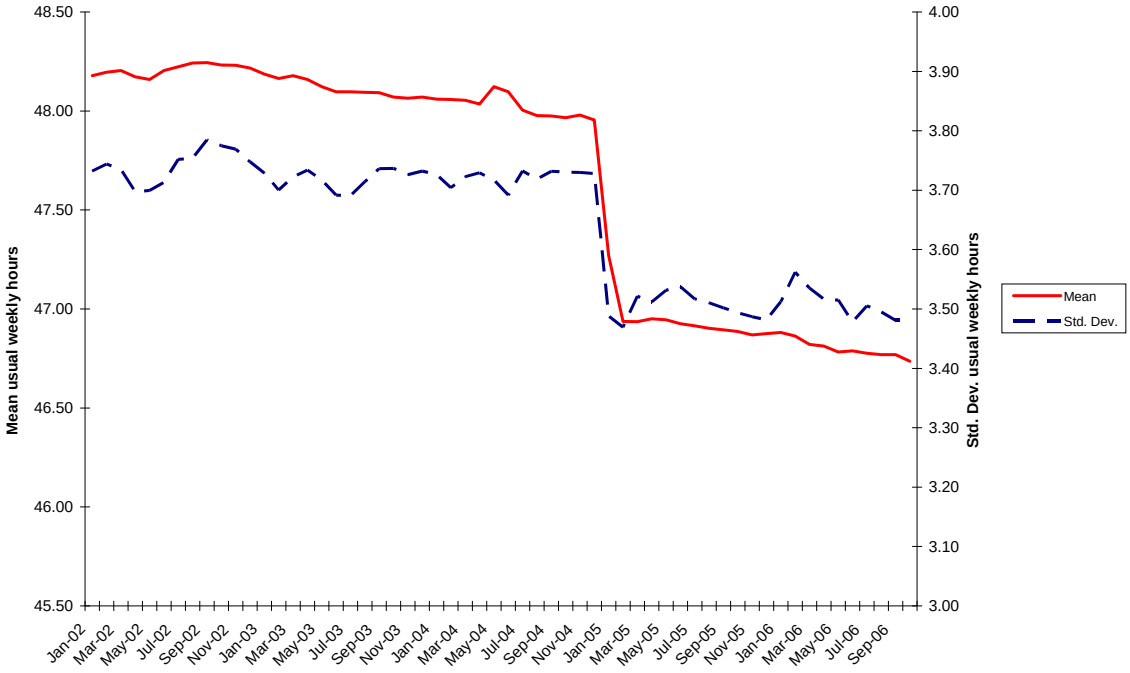

Figure 5: Nominal Wage Index and Nominal Labour Cost index (April 2003=100)

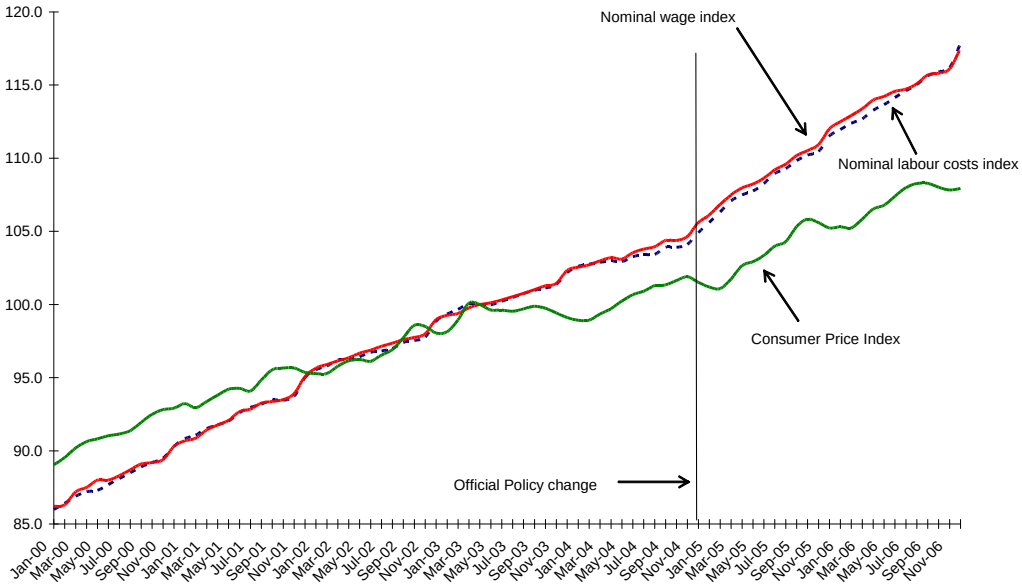

Figure 6: Evolution nominal hourly wage 2002-2006 (Chilean Pesos)

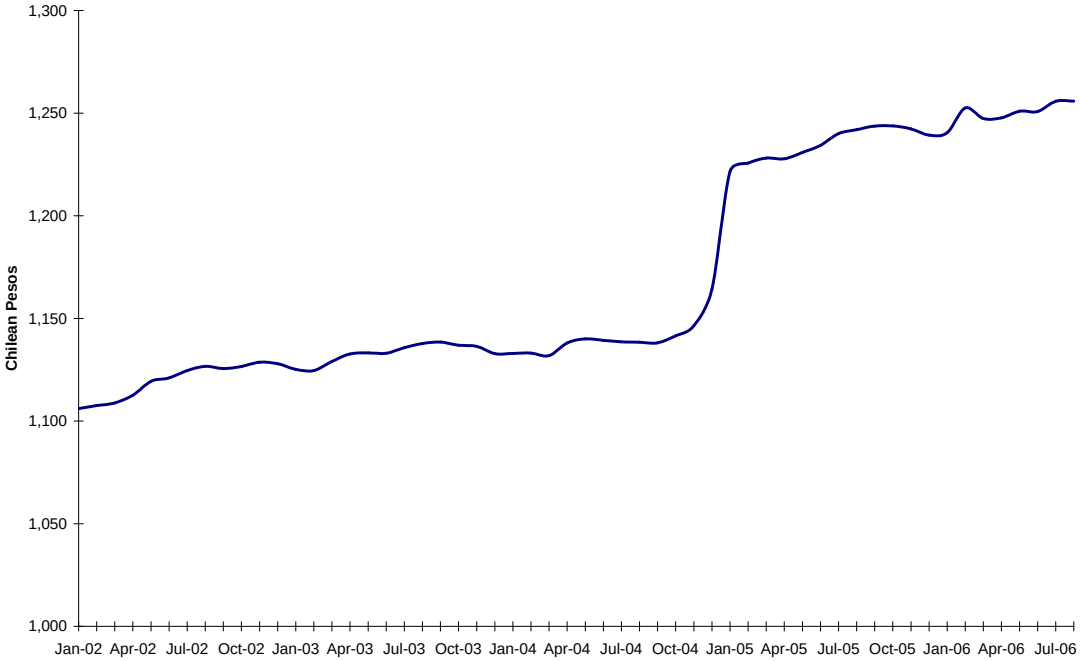

Figure 7: Evolution of employment rate

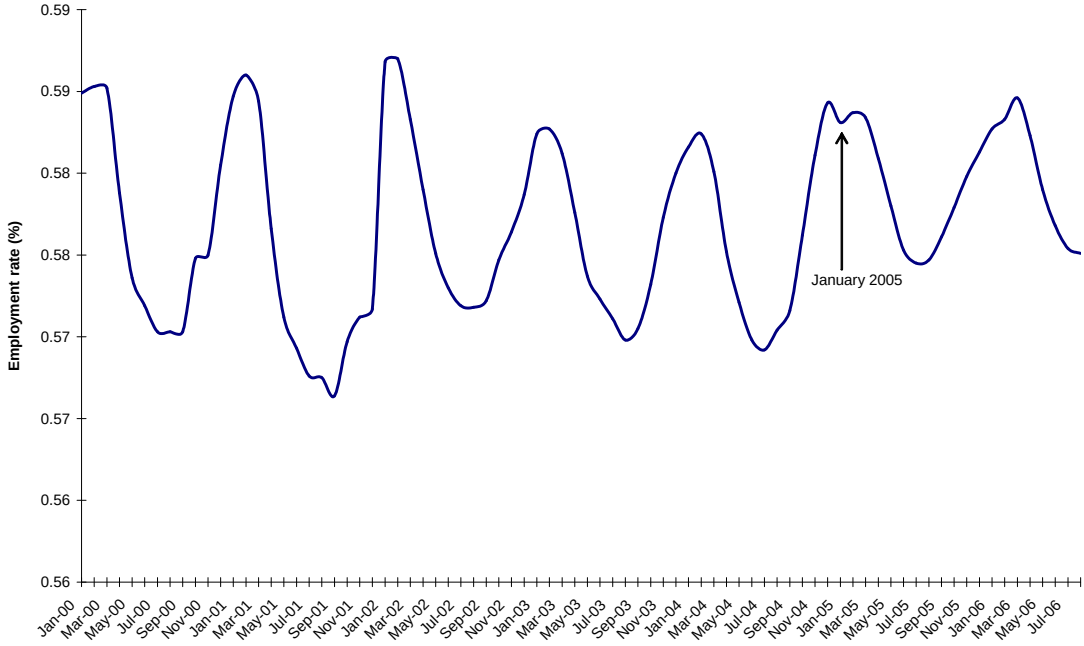

Figure 8: Distribution of treatment and control group individuals by industry category.

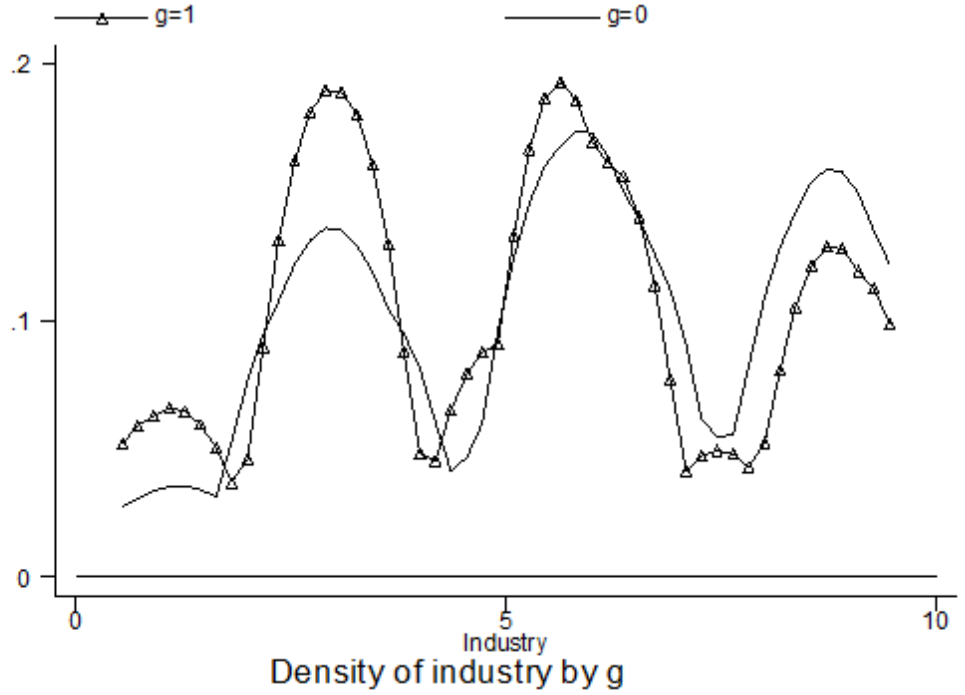

Figure 9: Distribution of treatment and control group individuals by occupation category

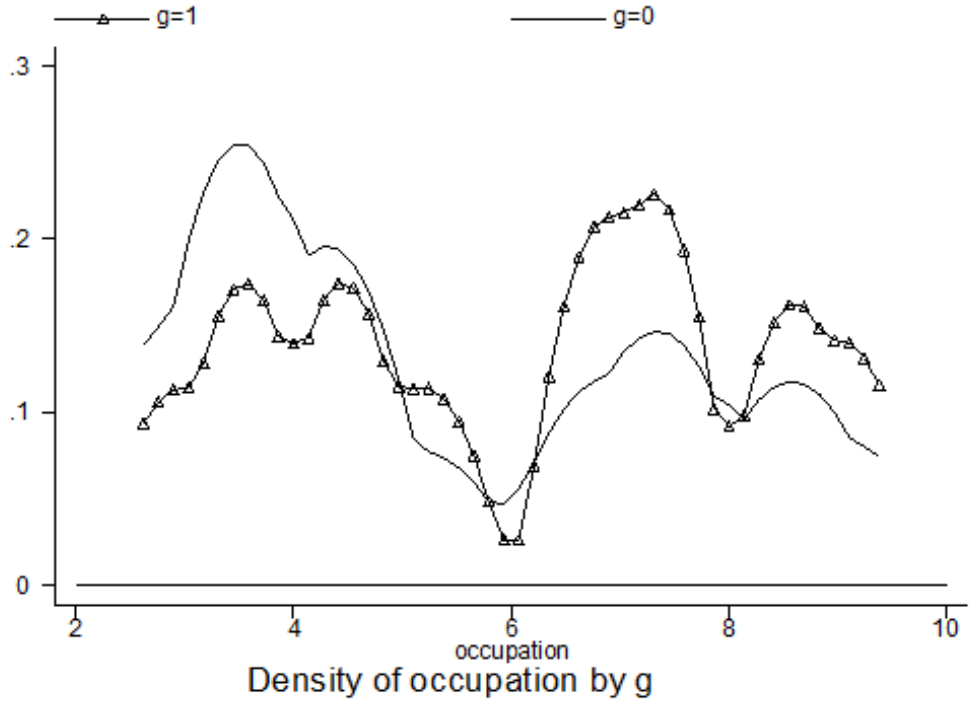

Figure 10: Distribution of treatment and control group individuals by size of the firm