Essays in Labour Regulation

by

Rafael Sanchez

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Signature of Author ....
Department of Economics
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Accepted by ....
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University of Warwick

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Abstract

This thesis consists of three empirical essays within the field of labour economics. As a whole, it explores the (un)intended consequences of labour regulation, with each chapter providing an independent analytical contribution to a specific aspect of the field.

Chapter 1 analyzes the effect of a reduction in standard working hours on employment transitions. In this chapter, I study Chile's reduction of weekly working hours from 48 to 45, which was announced in 2001 but implemented in 2005. This policy was innovative, compared with those in other countries, because it isolated the reduction in working hours from other policy changes, such as working time flexibility and financial incentives to firms. Thus, this policy is an interesting example for other countries to study, especially those without the fiscal capacity to provide such incentives, as it allows them to identify its effects on employment. Our results, which are confirmed by several robustness checks, suggest that despite the pre-announcement of the policy, firms displayed non-anticipatory behaviour on key variables. Furthermore, we find that firms waited to implement the reduction in working hours until just before the deadline. Overall, we find that a reduction in standard hours had no significant effects on employment transitions, although we do find a significant effect on hourly wages (i.e., wage compensation).

Chapter 2 extends the analysis of Chapter 1 to health outcomes. This is important, as the health effects of reductions in working hours have not been addressed by the existing literature; instead, most of the empirical evidence concerns employment outcomes, family life balance, and social networks. Using panel data from France and Portugal, this chapter exploits the exogenous variation of working hours coming from labour regulation and estimates its impact on health outcomes. In this way, our contribution to the existing literature is threefold: first, this is the first evaluation of health outcomes of policies that reduce working hours. Second, we avoid the problem of endogeneity with health outcomes by using exogenous reductions of working hours. Third, as the effects on health might depend on the level of working hours, our analysis is performed for two different countries with differing weekly hour thresholds (France, 35 hours; Portugal, 40 hours). Our results suggest a non-monotonic relationship between weekly working hours and health outcomes. In particular, a negative (positive) effect is found for young men (women) in France, and no effect is found in Portugal.

Chapter 3 (coauthored by Eugenio Rojas and Mauricio Villena) examines childcare policies and analyzes who effectively pays for childcare when it is not publicly funded. This is interest-

ing, since in several countries governments provide and fund childcare, but in many others it is privately funded, as labour regulation mandates that firms have to provide childcare services. For this latter case, there is no empirical evidence on the effects generated by the financial burden of childcare provision. In particular, there is no evidence about who effectively pays for childcare (i.e., firms or employees) and how it is paid for (i.e., via wages and/or employment). Our study is the first one to provide empirical evidence on the effects generated by the financial burden of childcare provision. For this, we exploit a Chilean labour regulation requiring that firms with 20 or more female workers provide and fund childcare for their workers. Our hypothesis is that, in imperfect labour markets (e.g., oligopsonistic), firms will pass childcare costs on to their workers. To analyze this, we exploit a discontinuity in the childcare provision mandated by the Chilean Labour Code. Our results suggest that firms pass almost the entire childcare cost (nearly 90%) on to their workers via lower wages (not only to female but also to male workers) and not by altering the share of male workers within the firm.

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Introduction

There is no formal definition of a labour market institution; however, a commonly used one is a system of laws, norms, or conventions resulting from a collective choice and providing constraints and incentives that alter individual choices over labour and pay (Boeri and Van Ours 2008). In this way, single individuals and firms consider the institutions as given when making their own decisions. Because they are based on collective choice, institutions are the by-product of a political process. Thus, they are often (but not always) established by laws (Boeri and Van Ours 2008), which is why labour market institutions are also called labour market regulations. Examples of labour market institutions (regulations) are minimum wages, unions, regulation of working hours, family policies, employment protection legislation, and unemployment benefits, among others.

Labour market institutions interfere with wages and labour allocation, as they affect labour demand and/or labour supply. The effects of the interference may be positive, neutral, or negative depending, among other things, on the underlying labour market structure. In particular, if a competitive labour market is assumed, then labour market institutions reduce the total surplus to be shared between workers and firms because they introduce a wedge between labour demand and supply by affecting either prices (e.g., minimum wages) or quantities (e.g., working hours regulations).

If this is the case, why should labour market institutions exist? Boeri and Van Ours (2008) offer three arguments for their existence: efficiency, equity, and policy failures.

Regarding efficiency, Boeri and Van Ours (2008) point out that since the first-best competitive labour market equilibrium is unattainable because of imperfect information, externalities, and labour market frictions, a second-best argument would justify the presence of these institutions. They suggest that "well-designed labour market institutions, in this context, may remedy these failures of markets and increase the size of the pie compared with the laissez-faire outcome".

Regarding equity, the authors suggest that labour market institutions are best suited to achieve some redistribution that is supported by voters by changing the allocation of the surplus between employers and employees.

Finally, when the benefits of an institution are concentrated in powerful minorities and the costs are spread over a very large crowd of individuals, institutions are created as some small and powerful segment of the population succeeds in imposing its will.

A. Theories

There are two broad views or strands of analysis about labour market regulations: the distortionist and the institutionalist (see Freeman 1993). The distortionist view argues that the existence of labour market regulations produces several undesired outcomes, such as a) a reduced rate of job creation and higher unemployment (Blanchard and Wolfers 2000) and b) an adverse impact on economic growth (Besley and Burgess 2004). Because of these effects, this branch of the literature supports the elimination, or at least the reduction, of labour market regulations. These authors mainly point out that the success of economic reforms depends, in general, on whether labour costs can vary freely in response to changes in labour demand. They argue that labour market regulations make the labour market more rigid, which makes any adjustment due to economic reform more difficult, which in turn makes the reallocation process slower and the outcome more inefficient.

Furthermore, this strand of the literature points out that labour market regulations lead to an increase in the cost of production, thus discouraging investments, increasing unemployment, and creating inequity. This latter effect comes from the fact that labour market regulations protect the interest of insiders and create obstacles to outsiders, who remain unemployed since they cannot enter the labour market, which contributes to the perpetuation of inequality (Boeri, Helppie, and Macis 2008).

On the other side, the institutionalist strand of the literature basically argues that the neoclassical theory is flawed.[1] Institutionalists argue that there are asymmetries in economic power and information between workers and firms and suggest that workers' weaker position

[1] The neoclassical approach is based on perfect competition and Pareto optimality.

often leads to unfair outcomes (e.g., unemployment, hazardous working conditions). In this sense, labour market regulations would help to balance the economic power between workers and firms. Supporters of this branch of the literature argue that the enforcement of labour regulation "forces" employers to shift their attention from cost cutting to productivity enhancement (Boeri, Helppie, and Macis 2008).

In particular, Wilkinson (1992) points out that firms may compete either by (a) reducing their unit costs by lowering their wages and labour standards or (b) increasing productivity with innovation and technology, improved product design, and so on. These strategies are the "low road" or the "high road" to growth, respectively. Under the former, there would be little motivation to undertake innovations to improve productivity. Furthermore, as Wilkinson suggests, in the absence of minimum labour standards, an economy may inevitably end up being stuck in a vicious cycle of low wages and low productivity.

Unfortunately, and as can be inferred from the discussion above, economic theory does not conclusively predict the effect of labour market regulation, as the predicted outcome of any particular labour market institution will depend crucially on the underlying labour market structure that one believes best describes the real world. The classic example is minimum wage regulations, which will always have a negative or neutral effect in a competitive framework (i.e., depending on the price elasticity). However, minimum wage regulation may have positive effects if the model that one believes represents the real world situation is a monopsonistic labour market. In this framework, small increases in the minimum wage will have positive effects on employment.

Because of these inconclusive predictions of economic theory, any assessment of the effects of labour market regulations rests ultimately on empirical studies (Boeri, Helppie, and Macis 2008).

B. Developed versus Developing Countries and Globalization

Although labour market regulation has existed for many centuries, only since the mid-1990s has the empirical literature started to grow. The reason for this, as Boeri and Van Ours (2008) point out, was the 1994 release of the OECD Jobs Study, a very influential policy report that attempted to explain the different employment/unemployment performance of Europe versus the United States, which was experiencing a "job miracle". The key message of this report was that European institutional rigidities prevented the European labour market from creating as many jobs in the private sector, compared with the United States. Since then, several cross-sectional studies have been performed to analyze the relationship between institutions and labour markets.

Regarding empirical evidence, there is considerable variation across countries in terms of how labour markets are regulated (see Betcherman, Luinstra, and Ogawa 2001). For example, Anglo-Saxon countries typically have regulated labour markets less heavily than have countries with civil law principles (e.g., France, Portugal, and Spain). Because of this, it is crucial to gather vast empirical evidence from different countries and different time periods.

The necessity for empirical evidence is more urgent in the case of developing countries since, until recently, the debate on the effects of labour market regulations was confined to the context of developed economies (see, e.g., Lazear 1990 and Blanchard and Wolfens 2000). However, recently there has been an increase in data availability from developing countries, which allows for empirical studies of the effects of labour market regulation on several outcomes.

Furthermore, two additional events make the realization of empirical studies in developing countries even more interesting: transition and globalization. Regarding transition, the fall of the Soviet Union and the transition of countries in central and eastern Europe, as well as the changes in the economic environments in China and India and the economic boom in Latin American countries, increased interest in these regions and their experiences. For these middle-income countries, transition processes implied a great reallocation of resources across sectors of the economy, which led to an increase in labour market risk, challenges, and opportunities for workers. Which labour market institutions these nations adopted have affected the dynamics of the transition and economic growth. Similarly, the progressive integration of all economies into the international market has generated an ongoing debate surrounding the effects of globalization on poverty, inequality, and employment, in both developed and developing countries (Boeri, Helppie, and Macis 2008). This, again, makes empirical evidence from developing countries crucial.

The focus on developing countries is even more interesting because of the characteristics of the labour market in these countries versus those in developed ones. In particular, the economies of developing countries are often characterized by weak law enforcement, a large informal sector, underdeveloped capital markets, and informal credit and insurance networks. These characteristics have at least two implications: first, results from developed countries should not directly extend to developing countries' settings without a serious reflection on these differences; second, within studies on developing countries, neglecting these features can lead to incorrect predictions and misguided interpretation of the empirical findings (Boeri, Helppie, and Macis 2008).

C. My Contribution

As explained above, three main conclusions can be obtained when labour market regulations (institutions) are analyzed. First, economic theory is not always helpful in predicting the effect of labour market regulations on economic outcomes, as they depend crucially on which labour market model one believes best describes the real world. This implies that any assessment of the effect of labour market regulations rests on empirical evidence. Second, when empirical work is performed, it is important to acknowledge that institutions do not operate in isolation. Third, as there are important differences between developed and developing labour markets, and most of the empirical evidence so far has been confined to developed economies, empirical evidence is needed for developing countries.

This PhD thesis includes three chapters that focus on two labour market regulations. Chapters 1 and 2 focus on working hours regulation, and Chapter 3 (coauthored with Eugenio Rojas and Mauricio Villena) focuses on childcare regulation. Chapter 1 analyzes the employment effect of a reduction in working hours from 48 to 45 hours a week in a developing country (i.e., Chile), which occurred in 2005. Chapter 2 extended the analysis for developed countries (France and Portugal) but focuses on health outcomes. To the best of my knowledge, this is the first evaluation of the effect of a reduction in working hours on health outcomes. Chapter 3 analyzes the Chilean labour regulation of childcare provision, where firms are mandated by law to provide childcare. In particular, we present the first empirical evidence on who bears the financial cost of childcare provision. To identify the causal effects in the first two chapters, I use changes in labour regulation that allow me to use a difference-in-differences approach, whereas in Chapter 3 we use a regression discontinuity approach that exploits a discontinuity in childcare regulation.

In this latter chapter my contribution was the proposition of the idea, the development of the econometric approach, the literature review, the parametric estimation and the obtention of the access to the data used.

Chapter 1

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

1.1 Introduction

Several countries such as France, Germany, Portugal, Belgium, Spain, Italy and the United Kingdom among others have implemented and/or discussed policies to reduce the (maximum) number of standard working hours.[1] Some of them are due to the European Working Time Directive (1993) but others have made earlier reductions mainly to tackle high rates of unemployment. The rationale for this policy is that a reduction of standard hours will decrease the total usual (average) hours worked by employees for a given output and so it will therefore be necessary to hire new workers. However, when a firm uses overtime, a reduction of standard hours increases the marginal cost of employment relative to the marginal cost of hours leading

[1] The maximum number of standard working hours are defined as the maximum number of hours above which employers have to pay overtime. For simplicity, we follow the convention in this literature and we will therefore 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. For example, in a country with a maximum standard working hours of 48, individuals might work: (a) 48 normal hours and zero overtime or (b) 45 hours plus 3 hours of overtime.

to a negative effect on employment and an increment in overtime hours. Therefore the demand of 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 a reduction of standard hours has 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 non-positive scale effect to be added to the previous effects. Given this ambiguity in the theoretical predictions, the effect a reduction of working hours has on employment remains an empirical question.

Previous 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, causing 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 which are for Europe (see Hunt 1999, Steiner and Peters 2000 among others) and none are for developing countries. Furthermore, the empirical evidence presents some caveats. The first being that most of the studies analyze work sharing policies derived from collective bargains, which cause them to use instruments to control for endogeneity of standard hours and therefore rely on the validity of the instrument. Fewer studies have used changes in regulation to assess the impact of reductions of standard working hours on employment (Crepón and Kramarz 2002 and Chemin and Wasmer 2009); The problem is that even these studies are unable to identify the "net" effect of the policy due to a lack of data or the negligible magnitude of the policy change.[3] A third problem of previous literature is that, in general, reductions of standard working hours are jointly implemented with other

[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.

policies such as higher flexibility (e.g. Portugal) and/or financial incentives (e.g. France) and therefore it is difficult to isolate the pure effect the reduction of standard working hours has on employment.

This study exploits a variation of standard hours given by a change in Chilean regulation regarding the maximum number of 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 number of standard hours from 48 to 45 hours per week.[4] The whole reform took place in December 2001 excluding the reduction of hours which became compulsory on the first of January of 2005. The pre-anouncement of the reduction of working hours clearly complicates our identification strategy, however, as we show below, there is no significant anticipation from firms. Furthermore, we apply several robustness checks and results do not significantly vary.

The separation of the reduction of standard hours from the rest of the reform gives us a policy change on working hours not present in other studies. This policy was innovative as it isolates the reduction of working hours from other policy changes and also gave time for firms to adjust. These characteristics, makes this policy interesting for other countries as it would allow them to see its effects on employment.[5]

Additionally, this kind of policy should be of particular interest for less developed countries, as in general they do not have the fiscal capacity to fund policies such as those implemented in rich countries (where all of the previous literature focuses). Furthermore, this policy change is also interesting for Latin American countries due to their labour market similarities, which are less heavily regulated relative to European ones. In particular this policy change was followed closely as most of Latin American countries suffered from persistent high unemployment after the Asian crisis in the late 90´s.

This study uses the *EPS Panel (Encuesta de Proteccion Social)* , which includes information

[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^{o}11 (2002).

[5] See the debates in Argentina 2006, Brazil 2010, Colombia 2009, Mexico 2004, Peru 2002 and Venezuela 2007.

on weekly usual (average) hours, monthly earnings, employment and type of contract (among others) before and after the change in policy. Hence, we have extended the analysis of Crepón and Kramarz (2002), since they do not have data on usual hours and hourly wages before the policy change which are limitations we do not have.[6] Similar to the French case, this study does not analyze the overall effect on employment but instead it focuses on those employees affected by the reduction of standard working hours (i.e. study the excess of employment destruction and not net job destruction). Specifically, this study uses a difference-in-difference approach (DD) to study whether workers are affected by the reduction of working hours (i.e. employees who worked 46-48 hours before the policy change) and lost their jobs more often than those not affected by the policy.[7] We also extended the analysis to those who were working overtime (i.e. those who worked 49-60 hours).

This study is organized as follows: Section 1.2 explains the implications derived from theoretical literature. Section 1.3 presents empirical evidence at the macro and micro level. Section 1.4 summarizes the institutional framework of the policy change in Chile. Section 1.5, presents the description of the dataset and the key variables. Section 1.6, introduces the identification strategy, the methodology we use to evaluate the policy's effect and the results. Section 1.7, presents a sensitivity analysis and section 1.8 concludes.

1.2 Theoretical Evidence: Labour Demand and Working Hours

Theoretical literature on labour demand recognizes the distinction between hours of work and number of workers (employment) since they are not in general perfect substitutes. The main reason for this is due to the existence of setup costs which means that workers' productivity has increasing returns for small values of hours worked and beyond a certain threshold fatigue will start affecting workers' productivity and then productivity will have decreasing returns. Furthermore, the distinction between hours of work and employment becomes more important

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

since the cost of labour is not a linear function of its duration due to, for example, costs of hiring and firing, existence of overtime premium, etc.

The theoretical literature which analyzes the effect of reducing working hours on employment started with Rosen (1968) and was further 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. These studies show that, in a setup where firms minimize costs given a particular level of output, the effect of a reduction in working time will depend on the starting situation of the firm. In general, Calmfors and Hoel (1988) show that the case where employment increases requires very specific circumstances. It will only happen if the firm was already at the standard level of hours and if after the reduction of standard hours, it is still optimal to equalize usual hours to the new standard ones. This is the model that supporters of work sharing have in mind. But the contrary would happen if firms were already using overtime, since in this case a reduction of standard hours would act *as* an increase in the fixed cost per worker while maintain the marginal cost of overtime unchanged; hence firms are induced to substitute employment for longer hours.

Calmfors and Hoel (1988) then extended the model by assuming that firms maximize profits instead of minimizing costs, which means that output is no longer fixed. The authors conclude that the probability of having an increase in employment under profit maximization is even lower than before (with cost minimization). This is because to the previous substitution effect we have to add a non positive scale effect as a result of the non positive effect of higher labour costs on output prices and output demand (known as scale effect).

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[8] (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*". Hence, the more negative the standard working time elasticity of the hourly wage ($\eta_{w_0 T}$) the less likely it will be to have a positive effect

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

on employment[9] . Thus 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 it impossible to increase employment.[10]

Hart (2004) highlighted that another indirect effect on employment may also exist, 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, a reduction of standard hours has two effects: firstly, it leads to overtime-employment substitution among full time workers but secondly, it leads the firm to increase the ratio of part-time to full-time employees due to a higher relative cost of the latter ones. This effect may serve to mitigate a tendency to increase average overtime hours given the shortened standard work week.

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 longer work duration as well as lower wages than the competitive case (see Kramarz et al. (2008) for more details). 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 point that lies between the competitive and monopsony level) then employment levels will increase.

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

[9] 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).

[10] 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.

1.3 Empirical Evidence

Empirical evidence regarding the effects of reductions of standard hours on employment is scarce and it concentrates on European experiences. The leading examples are Germany and France which represent examples of the two main approaches 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 reduction 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[11] the point estimate becomes smaller and remains insignificant. Since the effects a reduction in standard hours has on employment are likely to differ by skills[12] Steiner and Peters (2000) use industry-level data and separate by unskilled, skilled and highly 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 wage adjustment is taken into account, the net effect on employment on average becomes negative and especially strong for unskilled workers.

Due to different theoretical predictions on the employment effect generated by a reduction of standard working hours for firms with and without overtime workers, Andrews et al. (2005) specified a model with different types of firms. These different types of firms are defined by the proportion of workers who work overtime in each firm[13].They use plant-level data (IAB Establishment Panel) in a first differences estimation approach to study the effect the reduction

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

[12] 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.

[13] 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.

of standard working hours has 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 in small plants in the East German non-service sector (a positive effect of 2%). They argue that one of the possible explanations might be a lower increment in hourly wages in East Germany.

Usually all the aforementioned studies face the difficulty of standard hours endogeneity due to the bargaining process with unions which is in turn the reason for the use of instruments. 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. The oldest evidence from countries where a change in legislation was applied comes from time series studies. For Japan, Brunello (1989) uses a Monthly Labour Survey to estimate an equation system for demand and supply of hours and employment. He finds that a reduction of standard hours has a significant negative 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. Additionally, there is a problem of endogeneity between hours and employment since the author 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, different consequences due to these being different policy designs of the policy. The main difference between the reform of 1982 and 1998-2000 is that in the latter monthly labour costs remain relatively constant whereas an increase can be seen in the former.

The only empirical evidence studying the employment effects of the reduction of hours from 40 to 39 per week in 1982 has been carried out 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% higher probability (depending on the specification) of not being 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 the 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 it was announced that a reduction of hours would begin in 2000 and also that financial incentives were introduced (payroll tax subsidies) and flexibility to motivate the adjustment of hours to a maximum of 35 per week. Askenazy (2008) points out that the general consensus regarding the *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. That 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*".

Despite the mentioned complications, 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 Sá (2008) who 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 studies 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.

Chemin and Wasmer (2009) obtain similar results. They use a triple difference (DDD) approach based on a particular characteristic of the French' Alsace-Moselle local legislation. 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 is a too short time frame to find significant differences.

A recent study by Raposo and Van Ours (2010) analyzes the case of Portugal where in December of 1996 a new law on working hours was introduced which gradually reduced the standard workweek from 44 hours to 40 hours and also increased flexibility for firms.[14] They use a longitudinal data set (*Quadros de Pessoal*) which matches firm and employee data to study how this mandatory working hours reduction affected employment destruction and earnings of

[14] 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.

workers involved. They find that a reduction in working hours did not lead to increased job loss for 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 the reduction of working hours on overall employment has a positive effect through a fall in job destruction and no effect on job creation. They argue that their results might be explained by increased flexibility in the use of the standard workweek which made it easier for firms to cope with the reduced standard hours.[15]

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 approach to analyze the reduction of the standard workweek from 44 to 40 hours in the Canadian province of Quebec during the period of 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 captured is the effect of the economic conditions or irregular (or unusual) overtime during that time period instead of (or in addition to) the effect of the reduction of standard hours. Despite this, the author finds that the reduction of weekly hours worked had failed to raise employment at either the provincial level or within industries where hours of work were affected relatively more often.

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

[15] Notice that in the case of Portugal and France, overtime rates are not constant. This makes it more likely that the reduction of standard hours generates a positive effect on employment than in the constant overtime cost case.

processes of work sharing policies it is difficult to disentangle the pure effect of the reduction of standard hours.

1.4 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, on the 5^{th} of October of 2001, the labour reform was published in the Official newspaper. The main adjustments and/or changes in the "Labour Code" can be classified into 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.

This reform began operating "almost" fully on the 1^{st} of December 2001. The reduction of the number of weekly working hours (last category) was the only part of the reform which did not immediately apply.[16] It was announced that the reduction of hours was to be implemented by the 1^{st} of January of 2005 (as explained below). This (three year) window was established to give time for companies to adjust, although as we will show below, most of the adjustments took place during the last year. This three year 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

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

the reform, which means that the reduction of standard hours is not contaminated by the joint implementation of other policies such as in the case of Portugal and France (*Aubry I* and *II*).[17]

In reference to the duration of the maximum standard working week, the reform pre-anounced a reduction in the upper limit from 48 to 45 hours per week. This reduction will be compulsory throughout the entire country from the 1^{st} of January of 2005.

This constraint will not apply to: independent workers (self-employed), workers who have more than one employer, CEOs, 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 themselves, insurance salespersons, travelling salespersons and all those that do not work on their job's premises. All those who 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 customers.[18]

As part time work is defined as jobs that can not be more than 2/3 of the hours of a full time job, the reduction from 48 to 45 hours means that from the 1^{st} of January 2005 part time workers can not work more than 30 hours (the limit was 32 hours before this date). 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).[19]

The reform stated that working hour adjustments have to be made under agreement between employer and employee. If there is no agreement, then employers can unilaterally modify working hours without affecting their weekly distribution.[20] Despite the fact that labour reform clearly specifies the reduction of working hours it was ambiguous about the adjustment of wages. It specifically says:

[17] 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 this point extensively in the next section.

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

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

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

> *"Employee and employer have to make an agreement about monthly earning in order to adjust it to the reduction in working hours, nevertheless the legislator believes that a proportional reduction on monthly earnings as a consequence of less working hours it is not desirable".* It also states that*: "if there is no agreement between employer and employee, then the employer has to maintain the employee' s monthly earning irrespective of whether it is fixed, variable or a mixed".*[21]

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 is against the law given that the objective of the long adjustment window (3 years) has been 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*"[22]. This rigidity in earnings could be important since, as we saw in the theoretical model, it might increase the likelihood of a negative effect on employment levels even with high increments in productivity.[23]

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

1.5 Description of the Data

1.5.1 General Description of the EPS Panel

To study the effect of the reduction of working hours on employment transitions we use the *EPS* Panel[24], 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 occurred last (i.e. this includes employed, unemployed,

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

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

[23] 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"

[24] Panel de Encuesta de Proteccion Social.

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 are all those who were affiliated since January 1981 until august 2001 (registered in the administrative files of each *AFP* and the *INP* and who were also available in the system in august of 2001).[25] 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 such as usual working hours (but not monthly earnings). Subsequent waves updated the population of reference to include new affiliated and non affiliated members of the population (who were not included in wave one). The second wave was carried out between November 2004 - 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.

1.5.2 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). Unfortunately there was a pre-announcement of the reduction of standard hours in December 2001 when the labour reform was introduced.[26] The fact that we only have full data from January 2002 onwards implies that we may have some anticipation for the policy in the sense that firms might have reduced hours or changed their policy on wages or employment during 2001-2002. Although this is not likely, due to the long period of

[25] Where: AFP is Administradora de Fondo de Pensiones (Private pensions management funds) and INP is Instituto Nacional de Prevision (i.e. National Institute of Pensions). Affiliated is defined as: all those 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*).

[26] 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.

adjustment (3 years) introduced during the discussion of the labour reform, it is important to check the behaviour of hours, wages and employment in order to be confident that there were no significant variation in their behaviour.

Anticipation via usual hours

We checked 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 2 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 in the mean of usual weekly hours between January 2000 and January 2003 is negligible (around -0.02%). Similarly, the evolution of the standard deviation of usual weekly hours decreases marginally (-0.22%). On the other hand, the comparison between January 2003 and January 2005 shows an important variation in the distribution of hours, thus the adjustment seems to start at some point in between. To check this, we analyzed the information on usual hours given in the second and third waves of the *EPS* panel (Figure 3) and we observe that there is a declining trend in 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 months after the compulsory reduction of hours the standard deviation again follows 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 in firms during 2003 and early 2004 but most of the adjustment seems to be held during late 2004 and the beginning of 2005.

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 growth rate of monthly earnings. If this was the case, then it would imply anticipation via monthly earnings. Furthermore, the analysis of monthly earnings is important because the reform of December 2001 was not clear about its 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 costs increments.[27]

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 Chilean National Institute of Statistics (Figure 4) 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 does not include overtime pay while the latter does.[28] 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 became operative in December 2001 (and therefore the pre announcement of the future reduction of weekly hours). Interestingly, we observed 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 variations in the distribution of hours, it is reasonable to assume no anticipation via monthly earnings.

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

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

When we analyzed the data on hourly wages from waves two and three we observed an increase in the nominal hourly wage in January 2005, as can be seen in Figure 5 which gives us a general picture that is consistent with the information obtained from the nominal labour cost indexes displayed in Figure 4 in the sense of no major changes in hourly wages during the three year gap, but of wage compensation in January 2005 when firms reduce hours.[29]

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 a reduction of standard hours. We observed employment behaviour in Figure 6, where it is possible to observe that there is a variation in the employment rate within each year.[30] It seems that employment rate behaviour does not suffer any significant alteration due to the pre-announcement of a reduction of standard hours in December 2001. Interestingly, there is a small decline in 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 that there is no anticipation at least up to January 2003. Furthermore, data suggests that there are some changes in behaviour starting in mid 2003 until late 2004 when a sharp adjustment occurs in hourly wages as well as 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 occurred later on as if it were a quasi-experiment (i.e. exogenous shock).

[29] 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. This issue was solved in late 2004 when the illegality of the reduction of earnings was explicitly announced.

[30] This rate is: the number of people (per month) who declare being employed over the total amount of people who declare being employed, unemployed and inactive. 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. We do not use monthly data as it is not available for all the covariates.

1.5.3 Methodology, Time Periods and Control Group

Definition of the 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. Based on firms' non anticipation behaviour presented in the section above and also on the significant difference in the distribution of hours between January 2003 and January 2005 (see Figure 2), we consider January 2002 - January 2003 as the period before the policy change.

Defining the period which covers the application of the reduction of standard hours is more complicated. This is because, 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 2) where it is possible to appreciate some degree of variation on hours during January 2003 - January 2004 which might affect our estimation. In order to solve this difficulty we use a one year moving window. That is, we fix the pre-policy period as January 2002 - January 2003 and then we 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 imply 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 and reestimate the difference-in-differences. In the final section we will also present how robust are our results to alternatives definitions of time periods.

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 took 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 chose those individuals who worked between 44-45 hours per week as a control group and those who worked 46-48 hours a week as a treatment group, both measured at the beginning of the pre-policy period (January 2002). For this procedure to be valid we required that individuals classified as controls remained as such until just before the treatment kicked in (the same applies to treated individuals). As an example, imagine an individual classified as treated because he worked 48 hours in 2002, but then for whatever reason he works 45 hours in 2003 and the treatment occurs in 2004 and he remains working 45 hours. The individual would be miss-classified affecting our results. To tackle this difficulty we checked if workers who were employed a certain number of hours which are just above the new standard hour threshold (i.e. treated) in one year were still employed the same amount of time in the next year. We do the same for the control group. To do this we calculated, for those who remained employed in January 2003 (2004), what was the probability of reporting at that time the same number of hours than that reported in January 2002 (2003). The results are 98% (in January 2003) and 90% (in January 2004) respectively (they are very similar for both treated and controls). While there are not significant differences for controls, results change for treated individuals when we compare January 2002 with January 2005. For this latter group we found that the probability of reporting, in 2002, the same number of hours of 2005 is 37%. Thus it seems reasonable to classify individuals as treated or controls by their weekly number of hours in January 2002.[31]

As can be seen from the summary statistics presented in Table 1, treated and control groups

[31] We also try an alternative by only keeping individuals who do not change hours between 2002-2003-2004 (i.e. excluding the 2% and 10% who report a change in hours) and results do not differ.

seem to be very similar in terms of observables in January 2002 (see definition of each covariate in the appendix). 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. In particular, regarding the type of industry there are significant differences in Manufacture, Construction, Financial Services and Social Services. Regarding type of occupation the differences are statistically different from zero in technicians and associate professionals, Clerical support workers as well as craft and related trade workers. These differences, suggest the importance of controlling for the type of industry and occupation of workers.

1.6 Empirical Strategy and Estimation

1.6.1 Identification Strategy

In contrast with most European cases, where the national adjustment of working hours was not binding[32], the reduction in the Chilean case was binding, since more than 65% of non-self employed individuals employed 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 suggests no anticipation, it can be considered as if it were unexpected. Given the "unexpected shock" and that we have workers already employed 45 hours or less (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 the moving window, firstly defined as January 2003 - January 2004 and secondly as

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

January 2004 - January 2005. Then, we added a control group which represents what would have happened with the treated had they not been treated. If the control group is a valid one, this procedure should retrieve 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 and are also very similar in observables.[33],[34]

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.[35] 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 number of hours including overtime established by Chilean Law.[36]

1.6.2 Econometric Model and Estimation

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

$$NE_{i,t+p} = NE^0_{i,t+p} + D_{it}\left[NE^1_{i,t+p} - NE^0_{i,t+p}\right] \tag{1.1}$$

Where: $NE_{i,t+p}$ is the non employment status[37] of individual i at period "$t+p$", specifically: $NE_{i,t+p}$ =1 if, conditional on being employed at the beginning of period "$t+p$", individual i

[33] 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. Graphs which show the support for different variables are not reported but are available on request.

[34] For summary statistics see Table 1. 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. If treatment and controls are very similar in observable it seems reasonable to think that unobservables may behave similarly (although this can not be concluded from Table 1).

[35] See for example Lechner (1999).

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

[37] This includes unemployed and inactive status

is not employed at the end of period "$t+p$", where p is equal to 0 for window January 2002 - January 2003, 1 for January 2003 - January 2004 and 2 for January 2004 - January 2005 and $NE_{i,t+p}$ =0 otherwise. $D_{i,t}$=1 if the individual i is employed 46-48 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 (1.1) 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):

$$E\left[NE^1_{i,t+p} - NE^0_{i,t+p} |\ D_{it} = 1\right] \tag{1.2}$$

Where to capture (1.2) we need to assume:

$$E\left[NE^0_{i,t+p} |\ x_{it},\ D_{it} = 1\right] = E\left[NE^0_{i,t+p} |\ x_{it},\ D_{it} = 0\right] \tag{1.3}$$

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 by the reduction of hours to 45 per week (i.e. the so-called *Conditional (mean) Independence Assumption (CIA)*).[38] When (1.3) holds, the expectation of (1.1) given the treatment can be represented as:

[38] In the case of balanced 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 $E\left(u_{0,i}|x, D\right) = E\left(u_{0,i}|x\right)$,where $u_{0,i}$ is a idiosyncratic error term. 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 unbalanced panels we need to strengthen 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.

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

$$E\left[NE^{0}_{i,t+p}|\ x_{it}\right]+D_{i}E\left[NE^{1}_{i,t+p}-NE^{0}_{i,t+p}|\ x_{it}\ ,D_{it}=1\right] \tag{1.4}$$

Given this framework, we use a *difference-in-difference* (DD) approach to study if there is a specific effect during the January 2003- January 2004 (and January 2004 - 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 a moving window (January 2003 - January 2004 and January 2004 - January 2005). In order to obtain the effect of the policy change on the employment transitions we estimate equation (1.5) for all full-time dependent workers (i.e. not self-employed) in the private sector who work between 44-60 hours per week in January 2002.

$$E\left(NE_{i,t+p}|\ x_{it},D_{it}\right)=x^{'}_{it}\beta+\alpha_{s}g_{1i}+\alpha_{ov}g_{2i}+\gamma_{1}d_{t+p}+\gamma_{2}g_{1i}d_{t+p}+\gamma_{3}g_{2i}d_{t+p} \tag{1.5}$$

Where: g_{1i}=1 for individuals who are in the treatment group, g_{2i}=1 for individuals who are in the overtime group, $x^{'}_{it}$ is a vector of covariates which includes variables such as: 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[39], α_s is the average impact of the reduction in working hours on employment to non employment transition and α_{ov} measures the same but for people who were working overtime (i.e. 49-60 hours).[40]

[39] 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.

[40] We assume homogeneous effects since α represents the change in the intercept between the treated and control groups.

d_{t+p} =1 if the new maximum standard hours were in place at "$t+p$" and zero otherwise, and $g_{1i}d_{t+p}$ 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_{1i}). Similarly, $g_{2i}d_{t+p}$ 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 Tables 2a and 2b in the appendix.[41]

From the results of Table 2a, we observe that there are no significant effects when the "post-policy" period is defined as January 2003 - January 2004, meaning that the change of behaviour occurring during this period has no significant effects on employment transitions. When we move the post-policy period window to 2004 - 2005, we observed from the first column of panel A of Table 2b, that the point estimates are positive but insignificantly different from zero. Nevertheless, it is interesting that when the hourly wage is excluded, and we therefore 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 (1.0 and 1.3 percentage points for the standard and overtime group respectively). Thus, even though the effects of hourly wages 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 imprecise 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

[41] For our estimations we are using those individuals who are employed at the begining of each transition period, which means that we are using the data as repeated cross sections. This implies that some individuals will appear twice in the sample used in the analysis. 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 DD 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 the 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 reached a consensus. This is because, the best practice in cases where the presence of only a small number of groups or time periods advises 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 are very likely to generate correlation over time and therefore standard errors are clustered at the individual level.

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:

$$GAP = \left\{ \begin{array}{ccc} h_{it} - S_{t+p} & if & S_{t+p} < h_{it} \\ 0 & & else \end{array} \right\} \tag{1.6}$$

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 (1.5). Results are presented in the third and fourth columns of Table 2a and 2b. The estimated effects for Table 2a are not significant and the point estimates are almost zero. In Table 2b results are not significant (although point estimates are larger than those presented in Table 2a) but with the same sign than those analyzed before. It is especially interesting that 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 coefficients when the gap is used. In these cases the interpretation is by "unit gap" which in our case is by "overtime hour". In any case, results are very small and not significantly different from zero.

Furthermore, because the employment effect may differ by skill levels we re estimate equation (1.5) and (1.6) but excluding highly skilled workers. This means that we restricted the sample to those workers with less or equal studies than full high school. Results are presented in panel B of Tables 2a and 2b. They suggest that the reduction of hours does not affect employment transitions (i.e. for Table 2b there is an insignificant positive effect of between 0.8 - 1.3 percentage points depending on whether we are or are not maintaining fixed hourly wages). 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).

Table 2b displays the results obtained when we move the window to January 2004 - January 2005 and reestimate equation (1.5) and (1.6). Results from Table 2b are very similar to those discussed in Table 2a and basically suggest no significant effects. They are also not significant when we exclude highly skilled workers. Given these results and for brevity, hereafter we will only report results based on the periods January 2002 - January 2003 and January 2004 - January 2005.[42]

1.6.3 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 impossibility of a downward adjustment of the monthly earnings, the treatment indirectly affects employment transitions via hourly wages. Nevertheless, it could be the case that the transmission mechanism 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 (1.5) but now replacing the logarithm of hourly wage for the logarithm of monthly earnings. Results are presented in Table 3 in the appendix.

By comparing the estimates of the first and second column of Table 3 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 also 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. Finding such an instrument is not easy and the consequences of a bad instrument can be

[42] Results for periods January 2002 - January 2003 and January 2003 - January 2004 are available upon request.

worse than the solution. The endogeneity of wages seems less likely due to the non-anticipation behaviour on hours, employment and hourly wages analyzed above. Also, in our favor is the evidence of strong wage rigidity in Chile (Cobb and Opazo 2008) which makes the existence of endogeneity less likely between wages and employment transitions.[43] Furthermore, in our favor is the fact that the expected point estimates coefficients behave as expected when hourly wages are included in regression (1.5) and also that monthly wages seem not to have an impact on points estimates of employment transitions.

1.6.4 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 work less than standard hours while the former works 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 in the mix of employment (by varying the mix of part 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. Specifically, it 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 afterwards. Hence, Hart's point is weakened here since there is a reduction for both groups, full-time with overtime as well as part-time.[44] Therefore, we should not expect

[43] 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*".

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

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 hour 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 suggests no important movements between full-time and part-time workers.

Therefore, all our results suggest that there are no effect of the policy change on employment destruction. These results combined with employment creation and overall employment data suggest that the policy change did not affect employment transitions. These can be concluded as from the data we observed that job creation did not change significantly. In particular, for unemployed individuals in year t, the probability of being employed in year $t+1$ is 19.6% in 2003, 18.9% in 2004 and 20.1% in 2005. A similar pattern is present for the overall employment level.

1.6.5 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 checked both assumptions here.

Checking the validity of the control group:

In order to test the first assumption we follow Crepón and Kramarz' (2002) idea but extended 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 2004 - January 2005 with respect to the period January 2002 - January 2003. This should not be true for workers in the treatment groups. This test assumes no employment effects since it uses individuals still employed after the transition, which is consistent with what we have found in our estimations above. Therefore, we estimate the following cross-sectional models for January 2002- January 2003 and for January 2004 - January 2005:

$$E\left(\Delta Hours_{i,t}^{t+p}|\ x_{i,t}, D_{i,t}\right) = x_{it}^{'}\beta + \alpha_c g_{0i} + \ \alpha_s g_{1i} + \alpha_{ov} g_{2i} \tag{1.7}$$

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 (1.5) except for the intercept which is excluded here.[45] The other three variables ($g_{ki}, where\ k = 0, 1, 2$) 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 4a in the appendix.

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 described above. Nevertheless, estimates for the second period seem to suggest that there are significant effects in the standard and overtime groups at 1% and 10% of significance respectively. These negative variations on usual hours might have happened even in the absence of the reduction of standard hours, hence we extended the model in (1.7) to try to estimate if there was a significant effect in January 2004 - January 2005 that can not be found in January 2002- January 2003. In order to do this we estimate (1.8):

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

$$+\gamma_2 g_{1i} d_{t+p} + \gamma_3 g_{2i} d_{t+p} + \gamma_4 g_{0i} d_{t+p} \tag{1.8}$$

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

Where $g_{ki}d_{t+p}$ $(k = 0, 1, 2)$ represents the interaction variable between the post treatment dummy (d_{t+p}) and the respective group dummy (g_{ki}). $x_{it}^{'}$ 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 4a and suggest that the change of usual hours in the period January 2004 - 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 suggests that our control group was not affected while the opposite happened with our standard group. For the standard group, usual hours decreased by almost the same amount as the statutory reduction of 3 hours (i.e. 2.86 - (-0.09) = 2.77).

Panel B of Table 4a presents the results of the estimation of equations 1.7 and 1.8 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 2004 - January 2005. Once POLS is estimated in column 3, only the effect for the standard group remains significant. Specifically, for the standard group, hourly wage increases 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 5 suggesting wage compensation. This result implies that monthly earnings for individuals in the standard group remained close to the same after the reduction in hours. We can also observe from panel B of Table 4a that the results 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 the reduction of usual hours and increments in 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 have 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).

To further investigate the validity of our identification strategy, we run a falsification test. We exploit the fact that independent workers were explicitly excluded from the policy change. In particular, we reestimate equation (1.8) but instead of restricting the sample to affected occupations we have now restricted it to unaffected occupations. Our falsification test' results suggest no significant effects (as expected).[46]

Furthermore, we estimate a triple difference DDD in order to analyze the robustness of our findings. To carry out this, we built an "affected occupation" (AO_{it}) dummy, which equals 1 if the individual belongs to any of the occupations included in our baseline model (i.e. occupations affected by the policy change) and 0 if independent workers. The affected occupation dummy was also interacted with group dummies, year dummies and ($g_i d_t$). The coefficient of interest will be the one in front of ($g_i d_t AO_{it}$). This coefficient measures the relative decrease in working hours for affected versus unaffected occupations for workers above 45 hours in the moving window (i.e. years (2003-2004) and (2004-2005) respectively). The relevant coefficients are statistically insignificant for 2003-2004 but significant and very similar to those found in our baseline model for 2004-2005. In particular, the coefficient for the DD standard working group in the period 2004-2005 relative to 2002-2003 was -2.92 hours (see panel A, column 2 of Table 4a or column 4 of Table 4b) and in the DDD model the coefficient in front of the triple interactive term is -2.51 hours (see column 6 of Table 4b). These same pattern holds for changes in hourly wages as suggested in Table 4c.

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 trend). 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 sacrificing the model with hourly wages. We can use equation (1.5) in a period where there is

[46] See results in columns (2) and (5) in Table 4b and Table 4c for hours and hourly wages respectively.

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 (1.5) but excluding the logarithm of hourly wages from vector x. Estimates are presented in Table 5a in the appendix. Results from the first column of Table 5a 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.

To further investigate if workers above 45 hours evolve on systematically different paths or not from workers below 45 hours, we carried out a falsification exercise, where we reestimate our baseline model of equation (1.5) but instead of restricting the sample to affected occupations we have now restricted it to unaffected occupations (baseline and falsification test results for the period 2002 - 2003 and 2003 - 2004 are presented in column (1) and (2) of Table 5b respectively and similarly in columns (4) and (5) are presented the results for period 2002 - 2003 and 2004 - 2005). Our falsification test' results suggest no significant effects (as expected as no differences were expected between individuals in unaffected occupations).

Finally, and similarly to the previous section, we use a triple difference approach based on affected versus non-affected occupations, where independent workers are considered as non-affected. In the same way as before, we built an "affected occupation" (AO_{it}) dummy, which equals 1 if the individual belongs to any of the occupations included in our baseline model (i.e. occupations affected by the policy change) and 0 if independent workers. The affected occupation dummy was also interacted with group dummies, year dummies and ($g_i d_t$). The coefficient of interest will be the one in front of ($g_i d_t AO_{it}$). In columns (3) and (6) of Table 5b we present the results of the triple differences-in-difference analysis. As can be seen the relevant coefficients are statistically insignificant.

1.7 Robustness

It is important that our results do not depend on any specific construction. In order to analyze the sensitivity of our estimations we modify the underlying specification 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 analyzed.

1.7.1 Definition of Control Group

The analysis of employment transitions has been carried out by comparing a treatment and a control group, where the latter has been defined as those individuals who work between 44-45 hours per week in January 2002. The advantage of this narrow range is that we make the control group as similar as possible to those in the treated group (in terms of unobervables) and therefore it is expected that individuals in the control group respond to shocks in similar ways as individuals in the treatment group. Nevertheless, there is a trade off, since widening the control group range also has some advantages: firstly, it increases the number of observations and therefore increases the precision of the estimation (*ceteris paribus*). Secondly, it diminishes the problem created by potential missclasification of hours. Thirdly, it reduces the impact of the threats to the identification strategy such as spillover effects and substitution between groups.

We re-estimate equation (1.5) with different definitions of the control group. Results are presented in Table 6 in the appendix. In all the cases, as before, the point estimates are not significant. We also observe that the effect of hourly wages are as expected in all cases, this is increasing the magnitude of point estimates when they are positive, although they remain insignificant.

1.7.2 Definition of Time Periods

To analyze the robustness of our results with respect to the definition of time periods, we investigate the sensitivity of our results when the definition of pre and post policy periods are modified from January to February and March of the respective years. Results are presented in Table 7. In particular, in the first two columns we present results when January is replaced by February and in the last two January is replaced by March. In either case, results are not significant. Therefore, our estimate seems to be robust to the definition of the time period.

1.7.3 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 approximation when most of the covariates are discrete, which in our case are. Nevertheless, it is well known that OLS has some problems when the dependent variable is a dummy[47] and binary models have been proposed for these cases. Therefore, we re estimate equation (1.5) but now using a probit model. Results for the marginal effects are presented in Table 8 in the appendix and suggest that OLS results are similar to those obtained with probit.

1.7.4 Measurement error

The *EPS* panel used in our study, as with every survey data, might be subject to measurement error. Furthermore, using self reported employment histories may aggravate the problem. This is especially 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. Finally, the *EPS* panel does not have a direct question on overtime which may lead to misclassification of individuals into hours groups.[48] 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 the 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, giving us a lower bound of the effect of the policy change on employment transitions. To test

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

[48] 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.

how important this effect is and based on *CASEN* 2000[49], we observe that the probability of working overtime is affected mainly by industry category. In particular, workers in mining and transport sectors have a higher probability of working overtime hours. This is in line with *ENCLA* 2002[50] which shows that the mining and transport categories have the longest workweek. Therefore, if we do have an important misclassification due to the lack of a direct question of overtime, we should expect our results to change by excluding workers in the mining and transport categories, since they should have higher probabilities of misclassification. Results are presented in Table 9 in the appendix and suggest that estimates do not change when we exclude workers in the mentioned categories, hence it seems to be that misclassification is not a significant problem in our case. Furthermore, distributions of usual hours in January 2003 and January 2005 in Figure 2 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 mining and transport sectors but also in services related to them. *Antofagasta* and *Atacama* regions concentrate a high proportion of the mining industry (which has a special distribution of hours), and therefore most of the services there are related to the mining 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 rest days, but the average has to be 48 hours per week (before January 2005, or 45 hours after January 2005).[51] 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 seem to be robust to this specification since they suggest that by excluding regions that have more noise, in terms of measure of weekly hours, results do not significantly vary.

[49] *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.

[50] Encuesta Laboral.

[51] Observatorio Laboral 7. Septiembre 2002

1.7.5 General equilibrium effects

The effect of a reduction of standard hours on employment transitions has been analyzed from a partial equilibrium perspective which basically assumes that individuals that are not directly affected by the policy are indeed not affected at all. 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 programs) and allow researchers to avoid time consuming specification of general equilibrium models which sometimes require many more assumptions. Nevertheless, when the intervention has a larger scale, like changes in regulation (e.g. changes of the minimum wage, reduction of standard hours or massive training programs among others.) which affect a broader range of the population, then the support for a partial equilibrium approach weakens.

In our case, a reduction 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 jobs not affected or those individuals who were not employed at that time. In the first case, we checked potential effects on individuals below the threshold when we check the assumption that the policy change do not affect the control group, and we could not find 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 have affected 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 it seems to be that if they do exist then the effects should not be highly significant.

1.8 Conclusion

Many countries around the world have implemented or discussed worksharing policies like reductions of the maximum number of 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 in standard hours on employment is ambiguous and therefore remains an empirical question.

The scarce micro-econometric evidence can be grouped in studies which analyze reductions of standard hours derived from collective bargains and those derived from changes in regulation. The first group usually exploits panel data methods where the dependent variable is employment and one of the covariates is standard hours. Since standard hours are usually jointly determined with employment, instruments for standard hours are needed. An alternative approach has been the use of changes in legislation. Most of the evidence of this approach suffers from simultaneity problems since in general a reduction of standard hours has been jointly implemented with higher flexibility and/or financial incentives which do not allow for differentiating 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 reduction of standard hours that is not jointly implemented with other policies. Also, relative to Crepón and Kramarz (2002), the advantage of our data (*EPS* Panel) is that the *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 was a pre-announcement just before the initial period considered in our study. We checked the behaviour of crucial variables to support this hypothesis and all of them suggest no anticipation effects. This is supported by the results obtained when robustness checks were carried out.

The Chilean case is interesting for other countries as it would allow them to see the effects a reduction of working hours has on employment in a period of high persistence of unemployment.

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 and 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). These results combined with no significant variation on job creation and overall employment suggest that there is little support for work-sharing policies as a job-creation strategy. Results for Chile are in line with most of the evidence from European countries and Canada.

1.9 Appendix

Table 1: Summary Statistics in January 2002

	Control	**Treatment**	**Difference**	
	Mean	Mean	Mean	Std. Dev.
Gender (Female=1)	0.26	0.24	0.02	(0.011)
Age (years)	41.63	41.69	-0.03	(0.017)
Union (unionized=1)	0.17	0.14	0.03	(0.018)
Schooling 1 (No education)	0.02	0.02	0.00	(0.001)
Schooling 2 (pre school)	0.36	0.35	0.01	(0.007)
Schooling 3 (first level)	0.01	0.02	-0.01	(0.006)
Schooling 4 (handicapped)	0.31	0.30	0.01	(0.008)
Schooling 5 (second level A)	0.13	0.14	-0.01	(0.007)
Schooling 6 (second level B)	0.07	0.09	-0.02	(0.011)
Schooling 7 (college and Postgrad.)	0.10	0.09	0.01	(0.007)
Firm' size 1 (1-2)	0.03	0.04	-0.01	(0.006)
Firm' size 2 (3-9)	0.17	0.16	0.01	(0.015)
Firm' size 3 (10-19)	0.09	0.10	-0.01	(0.012)
Firm' size 4 (20-49)	0.17	0.13	0.04	(0.021)
Firm' size 5 (50-99)	0.11	0.13	-0.02	(0.013)
Firm' size 6 (100-199)	0.10	0.10	0.00	(0.012)
Firm' size 7 (200-499)	0.10	0.12	-0.02	(0.013)
Firm' size 8 (500+)	0.23	0.22	0.01	(0.016)
Occup. 1 (Technicians & associate prof.)	0.21	0.15	0.06	(0.015)
Occup. 2 (Clerical support)	0.24	0.17	0.07	(0.014)
Occup. 3 (Service & sales)	0.11	0.12	-0.01	(0.012)
Occup. 4 (Skilled agricultural & related)	0.02	0.03	-0.01	(0.007)
Occup. 5 (Craft & related)	0.18	0.24	-0.06	(0.016)
Occup. 6 (Plant & machine operators)	0.09	0.11	-0.02	(0.012)
Occup. 7 (Elementary occupations)	0.14	0.18	-0.04	(0.015)
Observations	**970**	**2,666**		

Table 1 (cont.): Summary Statistics in January 2002

	Control	Treatment	Difference	
Industry 1 (Agriculture, Hunting & related)	0.06	0.10	-0.04	(0.110)
Industry 2 (Mining & Quarrying)	0.01	0.02	-0.01	(0.006)
Industry 3 (Manufacturing)	0.21	0.26	-0.05	(0.017)
Industry 4 (Electricity, Gas & Water)	0.01	0.01	0.00	(0.003)
Industry 5 (Construction)	0.09	0.12	-0.03	(0.012)
Industry 6 (Commerce, Hotels & Restaurants)	0.25	0.22	0.03	(0.017)
Industry 7 (Transport & Communications)	0.05	0.05	0.00	(0.008)
Industry 8 (Financial Intermediation)	0.08	0.05	0.03	(0.001)
Industry 9 (Public, Social and Personal Services)	0.24	0.17	0.07	(0.015)
Region 1 (Arica and Tarapaca)	0.04	0.02	0.02	(0.011)
Region 2 (Antofagasta)	0.05	0.03	0.02	(0.011)
Region 3 (Atacama)	0.03	0.02	0.01	(0.006)
Region 4 (Coquimbo)	0.03	0.05	-0.02	(0.011)
Region 5 (Valparaíso)	0.11	0.09	0.02	(0.012)
Region 6 (O'Higgins)	0.03	0.05	-0.02	(0.011)
Region 7 (Maule)	0.03	0.05	-0.02	(0.011)
Region 8 (Bio-Bio)	0.09	0.11	-0.02	(0.012)
Region 9 (Araucanía)	0.03	0.04	-0.01	(0.008)
Region 10 (Los Lagos)	0.05	0.07	-0.02	(0.011)
Region 11 (Aysén)	0.01	0.01	0.00	(0.002)
Region 12 (Magallanes)	0.01	0.01	0.00	(0.003)
Region 13 (Metropolitana)	0.49	0.43	0.06	(0.039)

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 terminating the contract between a company and an employee:

a) Due to "business (or economic) reasons": these include the modernization or rationalization of the company, lower productivity, change in the economy or in the market and the lack of appropriate employee skills. By terminating the contract in this way the employer has to pay severance payments of 1 month (30 days) per year worked 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 payment 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 payment in this case either.

The reform basically incremented fines when firms invoke a wrong cause for terminating an employee.[52]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

[52] If the employer could not accredit the causes that originated (a) (in case the employer states (a)) the court could increment severance payments by 20%. The reform increased this fine by another 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 by 50%. Before the reform, in case of wrongly invoking (b) the court could increment the indemnization by 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.

installments (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%.

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 return 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 court decision.

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 an exceptional distribution of the work and leisure time different from those 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 from urban centers 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" carry out the renewal (i.e. completely discretional).

3. With respect to over time hours, before the reform, overtime hours had to be agreed on 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 renewal is possible.

4. In reference to changes in the collective bargaining relationship, the reform made the replacement of workers on strike more expensive. This is because before the reform employers could replace workers on strike from the first day only if the employer's last offer ensured the existing benefits adjusted by the inflation of the contract duration. 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[53] 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 work privileges, the reform creates a privilege of 40 days for those workers who participated on the assembly that generated the union.[54]

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 bargaining among the latter) will constitute a subterfuge to avoid labour and pension obligations" (and then a fine like that presented in (c) have to be applied).

c) With respect to the increment of fines, before the reform there were fines of between 1-10 UTM[55] depending on how big the fault was plus 0.15 UTM per worker affected by the

[53] UF means "Unidades de Fomento". The UF was determined by law in its origin and is indexed to the monthly rate of inflation (the UF currently has a value of around 21 pounds).

[54] 10 days before the assembly and 30 days after the assembly.

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

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.

Description of Variables

Dependent Variable In our specification, the dependent variable (NE) is a dummy:

$$\begin{Bmatrix} =1 \ \ if \quad individuals\ are\ not\ employed\ at\ the\ end\ of\ the\ transition\ period \\ =0 \ \ if \quad individuals\ are\ employed\ at\ the\ end\ of\ the\ transition\ period \end{Bmatrix}$$

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 2004 - January 2005.

Covariates

Gender: dummy variable which is: $\begin{Bmatrix} =1 \ \ if \quad Female \\ =0 \ \ if \quad Male \end{Bmatrix}$

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 kindergarten, 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 (Region de Los Lagos and Region de

Tarapacá). Given that during the analysis period Chile 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 and 10. Armed forces. For the purposes of our analysis, we drop the first two categories as well as 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 to 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. Mining and Quarrying, 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: $\left\{ \begin{array}{ll} = 1 \;\; if & individual\ i\ is\ unionize \\ = 0 \;\; if & individual\ i\ is\ not\ unionize \end{array} \right\}$

Size of the firm: Dummy variables per size category. These categories are: Size=1 if individual i works in a firm with 1-2 employee, Size=2 if individual i works in a firm with 3-9, 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 2a: Difference-in-Differences and GAP estimation

	January 2002 - January 2003 and January 2003 - January 2004			
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	0.007	0.009	0.001	0.002
	(0.018)	(0.018)	(0.018)	(0.018)
Overtime group	0.009	0.011	0.002	0.002
	(0.023)	(0.023)	(0.004)	(0.004)
Observations	5194	5194	5194	5194
B	Low skill workers			
Standard group	0.009	0.010	0.002	0.003
	(0.023)	(0.023)	(0.023)	(0.023)
Overtime group	0.008	0.010	0.002	0.003
	(0.030)	(0.030)	(0.031)	(0.031)
Observations	3738	3738	3738	3738

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 a complete high school level of education only. Control variables include: age, female dummy, 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.

Table 2b: Difference-in-Differences and GAP estimation

	January 2002 - January 2003 and January 2004 - 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, female dummy, 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.

Table 3: Estimation with Monthly Earnings instead of Hourly Wages

	Pooled OLS estimation January 2002 - 2003 and January 2004 - 2005	
	Dependent variable: NE_{t+p}	
	covariates with log(monthly earnings)	covariates without log(monthly earnings)
	(1)	(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, female dummy, 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.

Table 4a: Variation in UsualHours 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 2004 - 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 2004 - 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, female dummy, 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.

Table 4b: Falsification Test and Triple Difference by Moving Window for Hours

Dependent Variable: Change in usual hours ($\Delta Hours_{i,t}^{t+p}$)						
	Jan. 2002-Jan.2003 and Jan. 2003-Jan. 2004			Jan. 2002-Jan.2003 and Jan. 2004-Jan. 2005		
	Occupations	Occupations	DDD	Occupations	Occupations	DDD
Coefficient	Affected	Unaffected		Affected	Unaffected	
of:	(1)	(2)	(3)	(4)	(5)	(6)
Standard Group						
$g_{i1}d_t$	-0.23	-0.21	-0.16	-2.92***	-0.41	-0.77
$g_{i1}d_tAO_{it}$	-	-	-0.03	-	-	-2.51***
Overtime Group						
$g_{i2}d_t$	-0.54	-0.44	-0.38	-0.89*	-0.55	-0.70
$g_{i2}d_tAO_{it}$	-	-	-0.10	-	-	-0.34*
Obs.	2,080	1,902	3,982	1,824	1,713	3,537

Table 4c: Falsification Test and Triple Difference by Moving Window for Wages

Dependent Variable: Variation in ln(hourly wages) ($\Delta \ln(Hourly\ wages)_{i,t}^{t+p}$)						
	Jan. 2002-Jan.2003 and Jan. 2003-Jan. 2004			Jan. 2002-Jan.2003 and Jan. 2004-Jan. 2005		
	Occupations	Occupations	DDD	Occupations	Occupations	DDD
Coefficient	Affected	Unaffected		Affected	Unaffected	
of:	(1)	(2)	(3)	(4)	(5)	(6)
Standard Group						
$g_{i1}d_t$	0.031	0.020	0.024	0.066***	0.012	0.017
$g_{i1}d_tAO_{it}$	-	-	0.011	-	-	0.054***
Overtime Group						
$g_{i2}d_t$	0.033	0.022	0.013	0.056**	0.016	0.021
$g_{i2}d_tAO_{it}$	-	-	0.011	-	-	0.040**
Obs.	2,080	1,902	3,982	1,824	1,713	3,537

Note for Table 4b and 4c: control variables include: age, female dummy, dummies for educational levels, occupation, industry, firm size, unionization status, region, three group dummies (control, standard and overtime group, but no constant term), 1 year lagged weekly hours, a time dummy and interaction of all the group dummies with the time dummy. Furthermore, for the appropriate model it also includes the affected occupation dummy plus its interaction with group and time dummies and the triple interactive terms. Control group results are not reported as they were all insignificant. Clustered standard errors at the individual level are given in parenthesis.*p<10%, **p<5% and ***p<1%.

Table 5a: 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, female dummy, 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.

Table 5b: Falsification Test and Triple Differences-in-Difference by Moving Window

Dependent Variable: Non Employment in period $t+p$ (NE_{t+p})						
	Jan. 2002-Jan.2003 and Jan. 2003-Jan. 2004			Jan. 2002-Jan.2003 and Jan. 2004-Jan. 2005		
	Occupations	Occupations	DDD	Occupations	Occupations	DDD
Coefficient	Affected	Unaffected		Affected	Unaffected	
of:	(1)	(2)	(3)	(4)	(5)	(6)
Standard						
Group						
$g_{i1}d_t$	0.007	0.019	0.009	0.005	0.017	0.009
$g_{i1}d_tAO_{it}$	-	-	-0.012	-	-	-0.013
Overtime						
Group						
$g_{i2}d_t$	0.009	0.026	0.012	0.008	0.022	0.011
$g_{i2}d_tAO_{it}$	-	-	-0.017	-	-	-0.014
Obs.	5,194	4,509	9,703	5,084	4,442	9,526

Note: control variables include: age, female dummy, 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. Furthermore, for the appropriate model it also includes the affected occupation dummy plus its interaction with group and time dummies and the triple interactive terms. Clustered standard errors at the individual level are given in parenthesis.*p<10%, **p<5% and ***p<1%.

Table 6: Different Definitions of Control Group

	Control group defined as (43-45 hours) Dependent variable: NE_{t+p}		Control group defined as (42-45 hours) 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 group	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) Dependent variable: NE_{t+p}		Control group defined as (40-45 hours) 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 group	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 7: Different Definitions of Time Periods

	February 2002 - 2003 and February 2004 - 2005		March 2002 - 2003 and March 2004- 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 group	0.008	0.011	0.007	0.009
	(0.024)	(0.024)	(0.023)	(0.023)
Observations	5073	5073	5080	5080

Note: control variables include age, female dummy, 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. Clustered standard errors at the individual level are given in parenthesis.

Table 8: 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, female dummy, 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 9: Exclusion of Some Industries and Regions

	No mining 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, female dummy, 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.

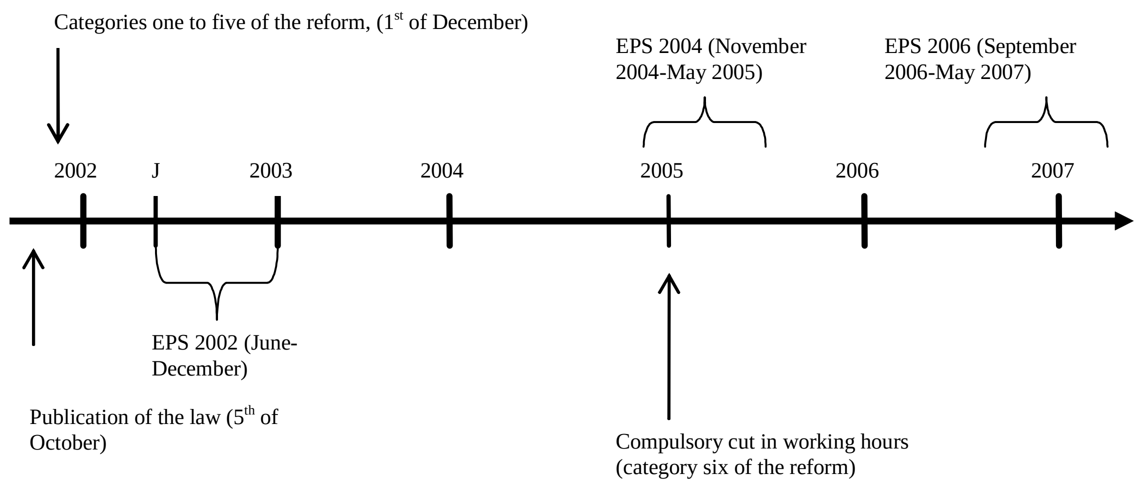
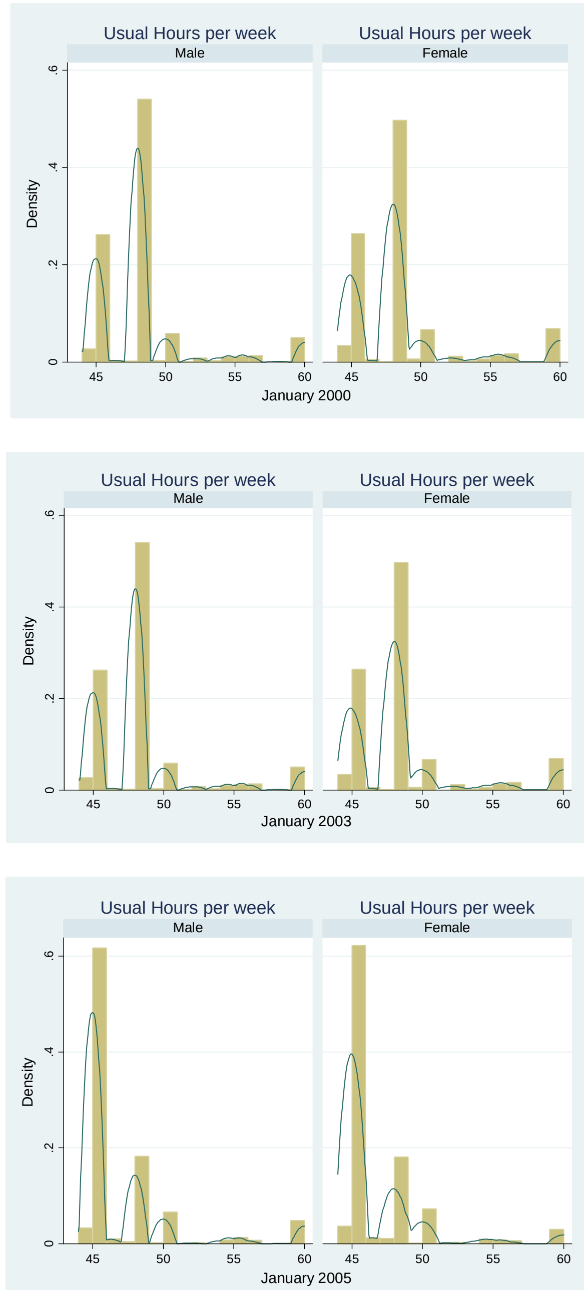


Figure 1 Time Line of the Reform and Data Waves



Figures 2 Distribution of Usual Hours by Gender

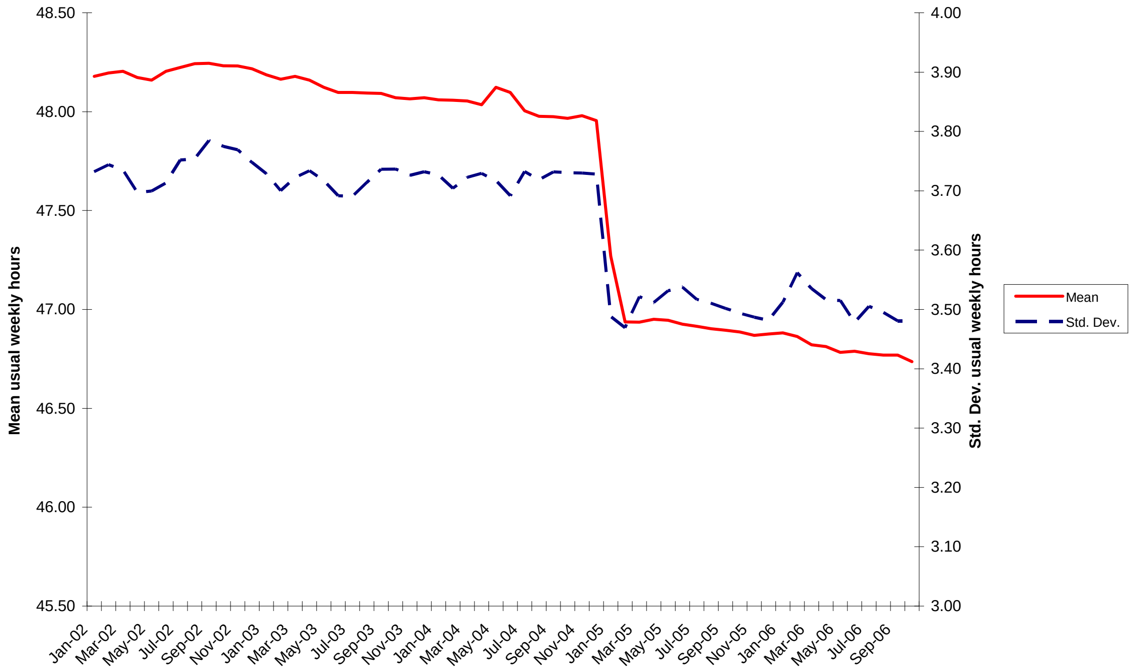


Figure 3 Evolution of Usual Hours 2002 2006

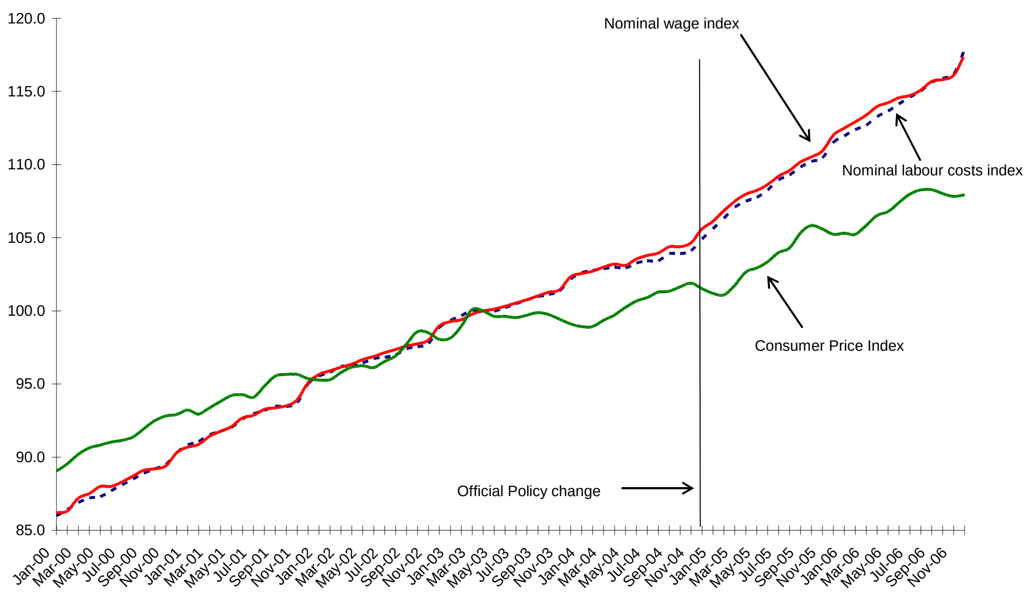


Figure 4 Nominal Wage Index and Nominal Labour Cost index (April 2003=100)

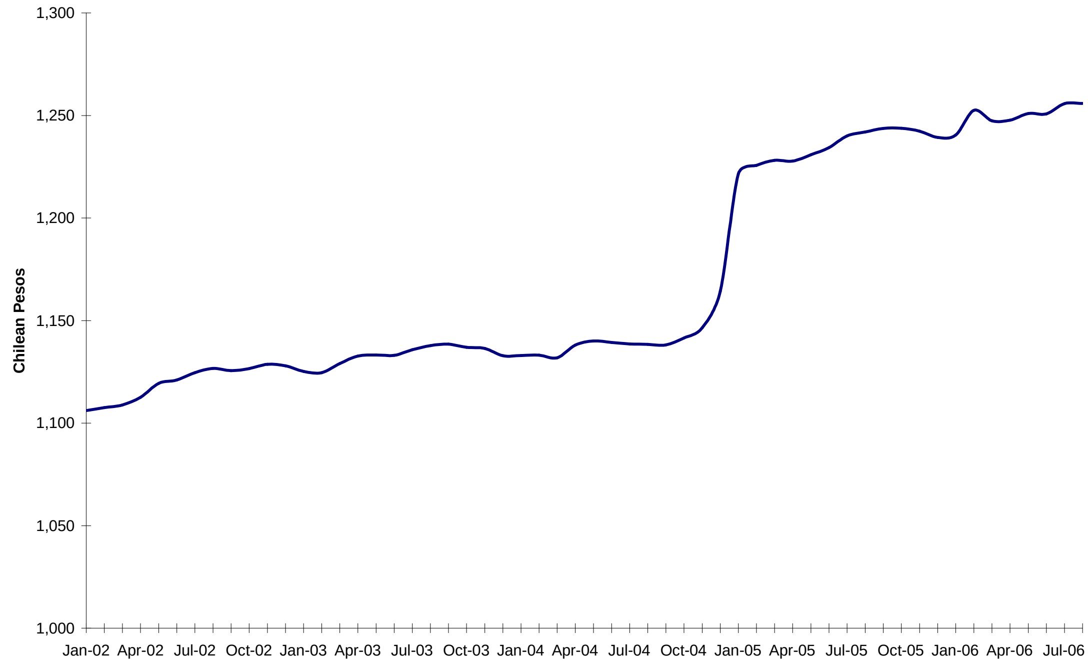


Figure 5 Evolution Nominal Hourly Wage 2002-2006 (Chilean Pesos)

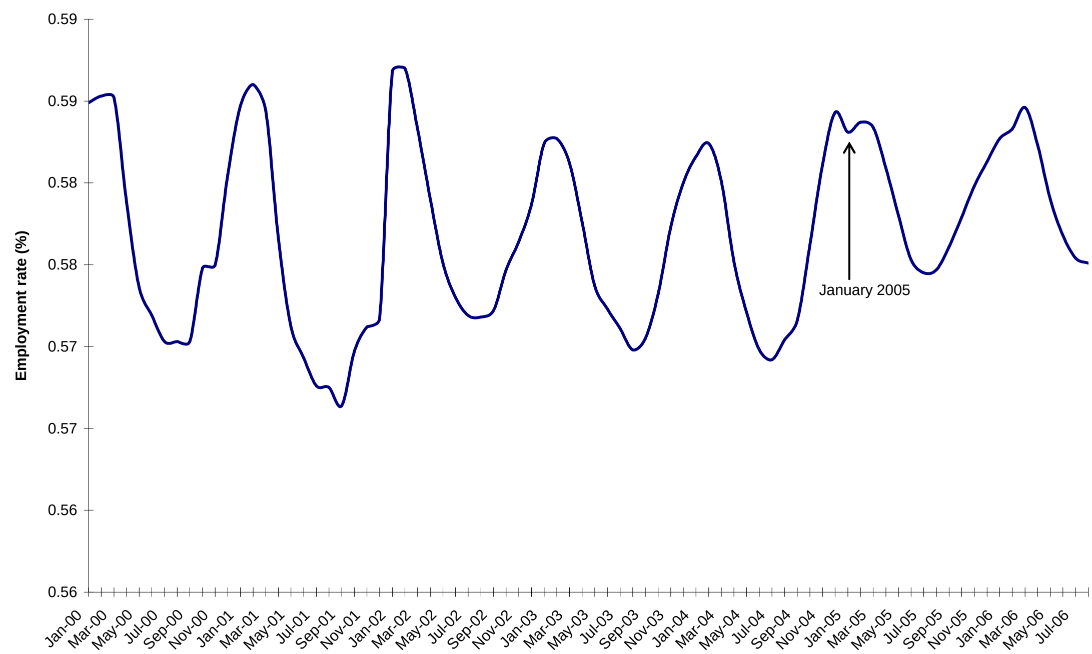


Figure 6 Evolution of Employment Rate

Chapter 2

Does decreasing working hours improve employees' health status?

2.1 Introduction

It is important to understand how reductions in working hours affect workers' health, as several institutions have suggested this kind of policy. In particular, throughout the 20^{th} century, the International Labour Organization (ILO) strongly supported the reduction of working hours specifically because of its potential benefits to workers' health (International Labour Organization 1990). Similarly, in 1993 the European Union implemented the European Time Directive, which explicitly recommended that member countries reduce their weekly working hours to potentially improve their citizens' health.

The rationale behind these recommendations is that longer working hours may be detrimental to workers' health because they disrupt workers' internal and external recovery (especially the latter).[1] In reference to internal recovery, Spurgeon et al. (1997) suggest that longer working hours negatively affect workers' health both directly and indirectly. Longer hours are directly harmful to workers' health because they cause stress as workers try to maintain performance levels while facing increasing fatigue, and they are indirectly harmful because they increase

[1] Internal recovery is the worker's capacity to recover during working hours, and external recovery is the worker's capacity to recover outside office hours (Taris et al. 2006).

the length of time that a worker is exposed to other sources of workplace stress. Taris et al. (2006) suggest that internal recovery will depend mainly on the characteristics of each job and that longer working hours will affect external recovery mainly by shortening the periods when individuals rest.[2] Working longer hours will generate a spiral, since those workers who do not fully recover from a work day will have to invest additional effort to perform adequately during the following day, resulting in an increased intensity of negative load reactions that appeal even more strongly to the recovery process. These effects will accumulate over time, affecting health outcomes (Sluiter et al. 2003).

However, apart from these negative effects that support international organizations' claims, longer working hours may also have some positive effects, as they are positively associated with current and future earnings and with faster rates of career progression (Francesconi 2001); since health improves with earnings (Deaton 2003), higher earnings should increase individuals' health status. Furthermore, the literature on promotions supports these ideas. In particular, Lazear and Rosen (1981) and Rosen (1986) view promotion as a tournament in which promotions are allocated to those workers who rank higher than all other workers in a group in a given period. The probability of getting promoted provides an incentive to exert effort, and, as this effort or propensity to work hard is not directly observable, firms will use indicators, such as hours of work or overtime hours, to select workers for promotion. Thus, a mandatory reduction in working hours for treated individuals (relative to controls) will limit the scope for competition via hours for this group of workers.[3] This negative effect on the probability of promotions (which affects the future income pattern) may have a negative impact on health, as individuals may become concerned and stressed about their future career and income. This effect is in line

[2]This points to another open debate in organizational psychology: it is not even clear whether what causes negative health effects is the length or the organization of the working hours. The only consensus here is that something should be done, since countries like the United Kingdom face costs of around £1.24 billion a year in stress-related illnesses (Beswick and White 2003).

[3]Firms have two alternatives when a law that reduces standard working hours is imposed (if the employee is not fired). On the one hand, firms can reduce the treated workers' total number of hours. On the other hand, firms can maintain the total number of hours and pay overtime. In the former case, the probability of promotion is negatively affected by the reasons explained above, and this may negatively affect health. For the latter case, and with a heterogeneous pool of workers, firms will be more willing to pay overtime for the most productive workers, putting extra pressure on workers to show that they belong to this group, and in this way affecting their health status. Additionally, if workers foresee that they are likely to lose their jobs because of an increase in the marginal cost of employment relative to the marginal costs of hours, they will experience additional negative pressure on their health status.

with the implications derived from the effort-reward imbalance (ERI) model (Siegrist 1996), explained in more detail below. In general, the ERI model acknowledges the link between high-cost/low-gain conditions, which are considered particularly stressful. One example of this situation, given by Siegrist (1996), is a high-effort situation associated with no prospects for promotion.

Therefore, reducing working hours may produce a trade-off between these two effects, thus having a theoretically ambiguous effect. For this reason, empirical evidence is needed, which is what I provide in this study.

To identify how reducing working hours affects health is complicated, as the number of working hours might be endogenous due to the so-called healthy worker effect (Frijters et al. 2009).[4] This is the main caveat of previous studies that analyze the link between health and working hours (see Beswick and White [2003] and van der Hulst [2003] for surveys). Because of this, we propose the first study that analyzes the effects of a reduction in working hours on health outcomes. To overcome potential endogeneity, we use an exogenous reduction in working hours caused by a change in regulation. Moreover, as the effect of reduced working hours may depend on the level of working hours, we analyze two different countries, each with a different threshold of working hours: France, which reduced its standard weekly working hours from 39 to 35 in 1998, and Portugal, which reduced its hours from 44 to 40 in 1996. Studying two different countries with different thresholds will be useful since, in practice, the impact of reduced working hours on health may be non-monotonic, which would be in line with a branch of the literature that proposes the existence of potential "optimal hours".

It is important to acknowledge that we are studying only the short-term effects of a reduction in working hours on health outcomes. We do not analyze the long-term effects, as we face some data constraints. For our analysis we use the eight waves of the European Community Household Panel (ECHP) for France and Portugal, which, despite the countries' institutional differences, enhances comparability since the countries use a common questionnaire. The empirical framework used is a difference-in-differences approach in a random-effects ordered-probit

[4] The healthy worker effect states that individuals with better health will tend to work longer hours than those with worse health.

setup, which will allow us to control for individual heterogeneity as well as initial health status.

The structure of this study is as follows. Section 2.2 briefly describes the theoretical background of the health effects of working hours. Section 2.3 presents the existing empirical evidence. Section 2.4 describes the institutional background of labour regulation in France and Portugal. Section 2.5 presents the identification strategy, while section 2.6 presents the data and the summary statistics. Finally, section 2.7 presents the results and the sensitivity analysis, and section 2.8 concludes.

2.2 Background

In this section we explain the theories that support the claims of international institutions that reducing working hours produces positive health effects. Most of the theoretical background comes from psychology (in particular, psychological epidemiology) and sociology, as these fields use specific models that link workplace stressors (such as longer working hours) to psychosocial effects.[5] These models, which we discuss below, are the demand-control-support (DCS) model (Karasek 1979), the effort-reward imbalance (ERI) model (Siegrist 1996), and the effort-recovery (ER) model (Meijman and Mulder 1998).

Demand-control-support (DCS) model (Karasek 1979) states that mental strain results from the interaction of job demands and job decision latitude.[6] Karasek (1979) pointed out that the combination of low decision latitude and heavy job demands (such as longer working hours) is associated with mental strain and other illnesses.[7]

[5] The term "psychosocial" refers to psychological developments during interaction with a social environment. An important characteristic of the psychosocial effects is that the individual is not necessarily fully aware of this relationship with his or her environment.

[6] Decision latitude refers to decision authority or skill level. Job demands will depend on the characteristics of the firm, while job decision latitude is probably closely related to the firm's authority structure and technology. So, this model restricts the notion of controlling objective task characteristics in terms of decision authority and skill discretion.

[7] This theory suggests that failure to distinguish between job demands and job decision latitude (i.e., adding these measures together) may generate relationships in which strain symptoms are cancelled out if, as Karasek proposed, the opportunity to use skills and make decisions (i.e., job decision latitude) reduces the undesirable effects of job demands.

Siegrist (1996) decides to shift the focus of analysis from control to reward by developing the effort-reward imbalance (ERI) model, which acknowledges the link between high-cost/low-gain conditions, which are considered particularly stressful. Siegrist (1996) points out that variables measuring low reward, in terms of low status control (e.g., lack of promotion prospects, job insecurity) in association with high extrinsic (e.g., work pressure) or intrinsic (e.g., high need for control) effort, independently predict new cardiovascular events in some groups of the population, such as blue-collar men. Siegrist (1996) also discusses the fact that high-cost/low-gain conditions are likely to be avoided or dismissed to maximize one's profit, although that does not take into account the social constraints under which individuals must make their decisions, especially the constraints associated with low occupational status control.[8] Therefore, high-cost/low-gain conditions at work are likely to occur in those groups of the workforce that exhibit a low level of occupational status control. However, among higher-status groups, these conditions may also be prevalent.

Sonnentag (2001) suggests that the effort-recovery (ER) model may provide a better explanation than previous theories. The ER model was developed by Meijman and Mulder (1998) and states that exerting effort during work leads to specific load reactions in the individual. These load reactions include physiological, behavioural, and subjective responses. Under normal conditions, these reactions are reversible; that is, when an individual is no longer confronted with work demands, his psychobiological systems previously affected by those demands return to their pre-demand levels, and recovery occurs. As a result of the recovery process, fatigue and other effects of stressful situations are reduced. However, when demands do not cease, but are continuously put on the individual, no recovery can occur. As a consequence, load reactions accumulate and result in negative effects, such as impaired well-being and health problems. Thus, for recovery to occur, it is necessary that demands previously put on the individual's psychobiological systems are removed and that the individual engages in a below-baseline activity.

[8] For example, blue-collar workers with reduced opportunities to change jobs will not minimize their effort at work even if their gain is low. The reason for this behaviour is obvious, as the possible costs produced by disengagement (e.g., the risks of being laid off or facing downward mobility) by far outweigh the costs of accepting inadequate benefits. Thus, Siegrist (1996) points out that under defined conditions of low occupational status control, effort-reward imbalance is maintained contrary to the prediction derived from the expectancy value theory of motivation.

Although the effects of longer (shorter) working hours may fit under the general setting of these three models (DCS, ERI, and ER), they do not explicitly link health and increases (reductions) in working hours. In line with the predictions of the ER model, Spurgeon et al. (1997) are more specific about the health effects of longer (shorter) working hours since they suggest that longer working hours may be detrimental to workers' health by affecting their internal and external recovery. In reference to internal recovery, Spurgeon et al. (1997) suggest that longer working hours negatively affect workers' health both directly and indirectly. Longer hours are directly harmful to workers' health because they cause stress as workers try to maintain performance levels while facing increasing fatigue, and they are indirectly harmful because they increase the length of time that a worker is exposed to other sources of workplace stress. Despite the effect of longer working hours on internal recovery, Taris et al. (2006) suggest that internal recovery will depend mainly on the characteristics of each job and that longer working hours will affect external recovery mainly by shortening individuals' rest periods. Working longer hours will generate a spiral, since those workers who do not fully recover from a work day will have to invest additional effort in order to perform adequately during the next day, resulting in an increased intensity of negative load reactions that require even more recovery time (Sluiter et al. 2003).

In economics there are no formal models that explain the link between longer (shorter) working hours and health. The closest one refers to the effect of work-sharing on employment transitions and assumes some links between hours and fatigue (see, e.g., Calmfors and Hoel 1988). Specifically, it assumes that the link between hours and productivity is inversely U-shaped. This is because individuals are productive with few hours and productivity will increase up to a certain point, but after that point fatigue will start to kick in and productivity will decrease. This suggests that the level of working hours would be important when someone evaluates the health effect of reducing working hours.

Additionally, there are some studies coming from the literature about promotions that suggest that longer (shorter) working hours may also have some positive (negative) effects as they are positively (negatively) associated with current and future earnings, and with faster (slower) rates of career progression (Francesconi 2001); since health improves with earnings (Deaton

2003), higher (lower) earnings should increase (decrease) the health status of individuals. In particular, Lazear and Rosen (1981) and Rosen (1986) view promotion as a tournament, which means that promotions are allocated to those workers who rank higher than all others in a group in a given period. The probability of getting promoted provides an incentive for a worker to exert effort without needing any formal contract with the firm (Francesconi 2001). As this effort or propensity to work hard is not directly observable, firms will use indicators such as the number of hours worked or overtime hours, for the purpose of selecting workers for promotion. Thus, a mandatory reduction in working hours for treated individuals (relative to controls) will limit the scope for competition via hours for this group of workers. This negative effect on the probability of promotions (which affects the future income pattern) may have a negative impact on health, as individuals may become concerned and stressed about their future career and income. This effect is in line with the implications derived from the ERI model (Siegrist 1996) explained above.

Therefore, all of the above evidence suggests that a reduction in working hours will have an ambiguous effect on health outcomes (a negative effect through the promotions channel and a positive effect through the psychological channel); hence, empirical evidence is needed.

2.3 Empirical Evidence on Reductions of Working Hours

Despite the existence of studies that analyze the effect of reductions in working hours on labour market outcomes, welfare, family balance, and social networks, there is no evidence on the effect of this kind of policy on health outcomes. The only related evidence available is that which focuses on analyzing the relationship between health and working hours (see Beswick and White [2003] and van der Hulst [2003] for surveys and Yang et al. [2006] and Artazcoz et al. [2007] for some newer evidence). These studies have the caveat that working hours and health may be simultaneously determined because of the so-called healthy worker effect (see Frijters et al. [2009]). In a regression framework, with health as a dependent variable and working hours as a covariate, this implies that if working hours decrease, then that reduction in working hours may be endogenous; hence, some methods need to be applied in order to overcome this potential bias on the coefficient of working hours.

Among those studies that try to analyze the link between working hours and health are Bardasi and Francesconi (2000), Ulker (2006), and Llena-Nozal (2009). Bardasi and Francesconi (2000) use longitudinal data on male and female workers drawn from the first seven waves of the British Household Panel Survey, 1991-1997, to study how nonstandard employment affects mental health.[9] The authors use a mental health indicator as the dependent variable (derived from the General Health Questionnaire [GHQ]) and a two-period lagged first-difference model that yields estimates of the effect of nonstandard employment on psychological well-being under some strong orthogonality conditions on the process governing the dynamic path of unobservable inputs. They find that working long hours in Britain (>48 hours per week) has no impact on GHQ scores; nevertheless, and as they recognize, even these estimates must be taken with some caution because the imposed orthogonality conditions are strong.[10]

Further examples are Ulker (2006) and Llena-Nozal (2009), who use longitudinal data to empirically assess how changes in labour market status and working conditions affect health (measured as SF36 scores and GHQ scores, respectively) in Australia (Ulker) and in several countries (Llena-Nozal). The within-group estimators used in both studies eliminate the bias from the time-invariant individual unobserved heterogeneity, but that does not solve the healthy worker effect that biases their results. Llena-Nozal's results suggest that negative mental health effects result from working overtime hours for Australian, Canadian, and British men; no effects exist for Canadian women, Swiss men and women, or British women; and positive effects exist for Australian women. Ulker's results show a lower general health index for those men who work long hours.

Therefore, given that there is no empirical evidence for the effects of a reduction in working hours on health outcomes, we claim to be the first to present such evidence. In particular, and given data limitations, we propose that our study is the first to analyze the short-term effect of a reduction in weekly working hours on health outcomes. For this, we use an exogenous reduction

[9] Notice that they analyze the effect of several types of nonstandard employment on health outcomes, including long working hours.

[10] Apart from the strong restrictions on the process governing the temporal path of the unobserved variables that affect health outcomes, one further caveat is that their methodology assumes that changes in working hours occur two periods before the change in mental health, which in their own words is "arguably a long period of time".

in working hours coming from a change in regulation. Furthermore, as the short-term effect of a reduction in working hours may depend on the level of working hours, we analyze two different countries, each with different hour thresholds. In this way, we are able to identify the effect of an exogenous reduction in working hours on health outcomes.

2.4 Institutional Background: Portugal and France

In Portugal, a law was introduced on December 1, 1996, to gradually reduce the maximum number of weekly working hours from 44 to 40. The law was passed because the newly elected government wanted to speed up convergence of the "traditionally long hours of work" in Portugal to the European average (Varejao 2005). This was done in two rounds and only for private-sector workers. The first one applied immediately (i.e., from December, 1, 1996) and mandated a reduction of two hours for all workers who were currently working 42 hours a week or more and a reduction for all employees who were working 40 to 42 hours per week. The second round started on December 1, 1997, and mandated that all workweeks should meet the new standard of 40 hours. With respect to overtime pay, the first hour had a premium of 50%, and the premium increased to 75% for additional overtime hours. This was not changed by the new law (although the activation point for overtime premiums was changed to 40 hours); nevertheless, some flexibility was introduced with the new law. The reduction took into account that the normal workweek could be defined on a four-month average. The maximum number of hours was allowed to increase by two hours per day if the total did not exceed 10 hours per day and 50 hours per week (Raposo and van Ours 2010). The law explicitly stated that the monthly wage could not decrease.

In France, in June 1998 the government passed the Aubry I law. This law had two parts. First, it established a weekly 35-hour limit in the private sector (from a previous limit of 39 hours), to begin January 1, 2000, for firms with more than 20 employees. For firms with fewer than 20 employees, the deadline was January 1, 2002. Besides excluding workers in the public sector, it also excluded independent workers. Before and after the reform, overtime was paid at a higher rate—25% for the first eight hours above the limit of 39 hours, and 50% for any additional overtime. The law did not change these rates, but it did shift the activation point for the overtime premium to 35 hours. Second, the Aubry I law also established financial incentives for firms (payroll tax subsidies).[11] Then, in January 2000, a second Aubry law was passed (called Aubry II) in order to introduce more detailed legal provisions regarding overtime (e.g., it introduced flexibility to the adjustment to the 35-hour limit) and in order to confirm the limit of 35 hours per week established in the Aubry I law. As in Portugal, the law explicitly forbade a decrease in the monthly wage.

2.5 Empirical Strategy and Estimation

To study the short-term effect of a reduction in working hours on health outcomes, we use a difference-in-differences approach in a random-effects ordered probit, which allows us to control for individual heterogeneity as well as for the initial health status of individuals. The treated group is defined as those individuals whose hours are just below the old threshold and above the new one, and the control group is defined as those individuals whose hours are just below the new threshold.

It is crucial to include individual unobservable heterogeneity in these kind of studies since, as Adams et al. (2003) suggest, the apparent significant causation of some covariates on health outcomes may be due to an unobservable persistence that is correlated with covariates and health outcomes. Also, a sequence of repeated observations on the same individuals makes it possible to allow for unobservable but persistent differences in the way that individuals translate their perceptions of health into survey responses. This is important, since people may differ in their psychological outlook and their interpretation of survey questions (Pudney 2008). Additionally, as health studies acknowledge, controlling for previous health is important since results show that a large part of the effects of work on mental health disappears or is reduced after including lagged mental health, confirming the hypothesis that some of the effects of work changes are driven by some preexisting condition (Llena-Nozal 2009). Unfortunately, it will not be correct to include a lagged dependent variable as a covariate when a difference-in-differences

[11] By granting financial incentives to alleviate labour costs, the law encouraged firms to reduce hours by 10% and increase the number of employees by 6% before legal deadlines were set (Askenazy 2008).

approach is used, as we would be analyzing the change of the probability of reporting a given level of health in period t conditional on the previous health status for the pre- and post-treatment periods for the control and treatment groups. That would force the previous health status to be the same in post-treatment periods between the treatment and control groups.[12] Thus we estimate:

$$h_{it}^{*}= \eta' x_{it}+\lambda' z_{it_1}+\delta' g_i + \zeta' r_t + \beta'(g_i * r_t)+\psi'(z_{it_1} * r_t) + \alpha_i+\varepsilon_{it} \tag{2.1}$$

$$(i = 1, ..., N; t = 2, ..., T_i)$$

where h_{it}^{*} is the latent health status of individual i at time t, x_{it} is a set of observed variables for individual i at period t which may be associated with health and z_{it_1} is a vector of dummies for the individual's health status in their first year t_1 (i.e. 1994), $\eta, \lambda, \delta, \zeta, \psi$ and β are parameters to be estimated, α_i is an individual-specific and time-invariant random component which is assumed to be distributed as $N(0, \sigma_\alpha^2)$. As is typical in a difference-in-difference approach, we include a group effect (g_i) equal to one for those individuals treated, time effects (r_t) and interactions between these two $(g_i * r_t)$ and whose parameter reflects the difference-in-difference effect. Our approach would imply that the health status of the first period would have the same impact on the health status of the second, third, and so on periods. To allow for different impacts of the initial health status by year, we add interactive terms $(z_{it_1} * r_t)$ between the time effect (r_t) and the health status of the initial period (z_{it_1}).Thus, our interest lies in the coefficient β.[13] x_{it} and z_{it_1} are assumed uncorrelated with ε_{it} for all t. The error term ε_{it} is assumed to be normally distributed with mean 0 and variance σ^2 and uncorrelated across

[12] In particular, we would be doing a ceteris paribus analysis on the change of P($y_{it} = j| \ y_{it-1}, x_{it,....}$), which conditions on $y_{i,t-1}$for the pre- and post-treatment periods for the control and the treatment group. Therefore, if the policy change had any effect, this latter approach would be incorrect. In the "Robustness of the Results" section, we also present the coefficients obtained when a lagged dependent variable is included as a control instead of the initial health status. The results do not change significantly for Portugal, but there are some differences for France (shown below).

[13] This assumes homogeneous effects of the treatment, which represents the change in the intercept between the treated and control groups. To allow for heterogeneous effects, one could include interactions between the group dummy, the time effect, and x_{it}, although that would have a degrees-of-freedom cost. Therefore, we maintain the assumption of homogeneous effects. This seems reasonable, as the interactive terms added to capture the heterogeneous effects are not significant in the cases of France and Portugal.

individuals and waves and uncorrelated with α. We set the variance of the idiosyncratic error equal to one and as h^*_{it} is not observed, we use an indicator of the category in which the latent indicator falls (h_{it}). The observation mechanism can be expressed as:

$$h_{it} = j \;\; if \qquad \mu_{j-1} < h^*_{it} \leq \mu_j \tag{2.2}$$

$$j = 1, ..., m$$

where $\mu_0 = -\infty, \mu_j \leq \mu_{j+1}, \mu_m = \infty$. Given that the error is normally distributed, the probability of observing a particular category of health status (i.e. Self Assessed Health) reported by individual i at time t, conditional on the covariates, initial health status and the individual effect is:

$$P_{it,j} = P(h_{it} = j) = \Phi\left\{\mu_j - \eta' x_{it} - \lambda' z_{it_1} - \delta' g_i - \zeta' r_t - \beta'(g_i * r_t) - \psi'(z_{it_1} * r_t) - \alpha_i\right\} \tag{2.3}$$

$$-\Phi\left\{\mu_{j-1} - \eta' x_{it} - \lambda' z_{it_1} - \delta' g_i - \zeta' r_t - \beta'(g_i * r_t) - \psi'(z_{it_1} * r_t) - \alpha_i\right\}$$

where $\Phi\{.\}$ is the standard normal distribution function. Before the actual estimation, we need to deal with two challenges. Firstly, the random-effects ordered probit, assumes that there is no correlation between the individual effect (α_i) and the covariates. This seems to be very restrictive in our setting since individual unobserved heterogeneity is likely to be correlated with the covariates (e.g. unobservable psychological characteristics that make individuals respond in a particular way to the health survey might be correlated with covariates as age or gender). Since the coefficients estimated by the random-effects estimator are in general inconsistent under this setting (especially when T is not very large) we can use Mundlak's (1978) parameterization. This captures the correlation between the individual effect (α_i) and the average of the regressors. Thus, we use:

$$\alpha_i = \alpha_0 + \alpha_1' \overline{x_i} + u_i \tag{2.4}$$

where $\overline{x_i}$ is the average over the sample period of the observations on the exogenous time variant variables. By construction u_i is distributed $N(0; \sigma_\alpha^2)$ and independent of the x_{it} variables and the idiosyncratic error term (ε_{it}).[14] Thus, α_i in equation (2.1) is replaced by equation (2.4).[15]

Before the estimation it is important to mention that there are four further issues with the random effects ordered probit estimates. The first refers to the usefulness of the estimated coefficients, the second refers to the difference between marginal effects versus average partial effects, the third to the cut-shifts points problem and the fourth and final complication refers to inference issues. The first aforementioned issue refers to the fact that most of the studies which use this approach report the coefficients, which in general are not very informative. This is true in nonlinear models as the marginal effects are a function of the density of the model. In our case, the marginal effects of changes in continuous regressors are:

$$\frac{\partial P(h=0 \mid W)}{\partial W} = -\phi\left(W'\gamma\right)\gamma$$

$$\frac{\partial P(h=1 \mid W)}{\partial W} = \left[\phi\left(-W'\gamma\right) - \phi\left(\mu_1 - W'\gamma\right)\right]\gamma \tag{2.5}$$

$$\frac{\partial P(h=2 \mid W)}{\partial W} = \left[\phi\left(\mu_1 - W'\gamma\right) - \phi\left(\mu_2 - W'\gamma\right)\right]\gamma$$

$$\frac{\partial P(h=3 \mid W)}{\partial W} = \phi\left(\mu_2 - W'\gamma\right)\gamma$$

where W is the deterministic part of equation (2.1). This makes the report of coefficients even less informative since only in the case of the higher category does the sign of the coefficient

[14] By construction refers to the fact that once the distribution of α_i is defined, u_i has the same distribution.
[15] This results in a likelihood that can be easily maximized using common software (e.g. Gllamm in STATA).

always match the sign of the marginal effect and in the lower category it is equal to the opposite of the sign of the marginal effect. But what happens in the middle is ambiguous since it will depend on the signs of the squared brackets.

The second issue refers to the fact that marginal effects are calculated at the sample mean of the covariates, but in our case, it is unlikely that the mean of the individual unobservable heterogeneity corresponds to an actual observation. For this reason, it has been argued that it is better to report the average partial effects (APEs, Wooldridge 2002), which is the expected value of the partial effect over the distribution of α. The difference between the two measures is that the marginal effect is calculated at the sample mean of each regressor while the average partial effect is calculated for each observation and then is averaged over the population distribution of heterogeneity and computed using the population averaged parameters β_p which in the random effect specification are given by $\beta_p = \frac{\beta^{RE}}{\sqrt{(1+\sigma_\alpha^2)}}$ (see Wooldridge (2002) and Greene (2008)).[16] Hence, we present below the average partial effect of the estimation of relevant covariates (i.e. those related to the treatment) for each of the probabilities (i.e. the discrete version of equation 2.5).

The third refers to a potential heterogeneity with respect to cut off points. This is due to the fact that population subgroups may use systematically different cut point levels when reporting their SAH, despite having the same level of "true health". As Contoyannis et al. (2004b) suggest, in ordered probit models like the one used in our study, the symptoms of cut-point shift can be captured by making the cut-points dependent on some or all of the exogenous covariates and estimating a generalized ordered probit. The problem with this approach is that in order to separately identify the influence of variables on latent health and measurement error, it would be necessary to use strong a priory restrictions on which variables affect health and which affect reporting behaviour. Some attempts to deal with this issue have used vignettes (e.g. Murray et al. 2001). Lindeboom and van Doorslaer (2004) have also used a "more objective" measure of health to study which variables may generate cut-point shifts.[17] They found significant effects of age and gender but not for other socioeconomic variables like income, education, etc.

[16] where β_p are the population averaged parameters and β^{RE} are the random effect parameters.

[17] They use SAH from the Canadian National Population Health Survey and the McMaster Health Utility Index (HUI-3) as their objective measure.

As we do not have objective measures of health or vignettes, we follow the approach adopted by Contoyannis et al. (2004b) which is to split the sample by gender and age groups before estimating our models to see if there are heterogeneous effects of the covariates by subgroups. Evidence of very different results could indicate heterogeneity with respect to cut off points (van Doorslaer and Jones 2003).

The fourth refers to inference issues. In particular, the question here is where the appropriate test of significance should be carried out, on the APE or on the coefficient. As Greene (2008) suggests, there is not a single answer and opinions differ. Greene (2008) points out that "It might logically be argued that the overall purpose of the regression analysis is to compute the partial effects, so that is where the tests should be carried out. On the other hand, the meaning of the test with respect to the partial effects is ambiguous, since they are functions of all the parameters as well as the data.....our preference on the methodological basis is for the structural coefficients, not the partial effects". Nevertheless, Greene (2008) also points out that "Patterns of statistical significance for the partial effects will usually echo those for the coefficients themselves". Hence, we follow Greene (2008) and thus, in order to be able to make some inference, we will present below not only the APEs but also the coefficients.

2.6 Data

2.6.1 Data Description

We use the European Community Household Panel (ECHP), which is a standardized panel survey used to interview a sample of households and persons every year in the European Union. These interviews cover a wide range of topics concerning living conditions, including the interviewees' income information, financial situation in a wider sense, working life, housing situation, and health, among other things. The sample size is 170,000 individuals in the initial wave for the 12 countries included. The ECHP had a total duration of eight years, running from 1994 to 2001. The main advantage is that information is homogeneous among countries since the questionnaire is similar in each case. This source of data is coordinated by the European Commission's Statistical Office (EUROSTAT).

2.6.2 Measures of Health: SAH

One of the first concerns in studies that analyze health is how to measure it. In our case, given that we are using regulation of working hours as one of the covariates, we needed data that contains both the labour and health information of individuals. Because of its wide use in health economics models, SAH is a natural choice. Several socioeconomic surveys measure SAH and, despite some differences, it has a common frame. In the ECHP, it is generally defined by a response to the following question: would you say that your health has on the whole been very good/good/fair/bad/very bad? SAH is measured as a categorical variable indicator from 1 (higher category) to 5 (lower category). Since SAH is a subjective measure of health, it is subject to criticism, as it may be affected by measurement error. Furthermore, it has been argued that the mapping of "true health" into SAH categories may vary with respondent characteristics. This happens when population subgroups use systematically different cutoff-points levels when reporting their SAH, despite having the same level of "true health".[18] Despite these caveats, SAH has been used widely in previous studies of the relationships between health and socioeconomic status (e.g., Adams et al. 2003) and between health and lifestyle (e.g., Contoyannis et al. 2004a). Moreover, SAH has been shown to be a powerful predictor of subsequent mortality (e.g., Idler and Benyamini 1997) and a good predictor of subsequent use of medical care.[19] It has also been shown that inequalities in SAH predict inequalities in mortality (e.g., van Doorslaer and Gerdtham 2003). Furthermore, an appealing characteristic of general health measures such as SAH is their ability to encapsulate and summarize a multitude of health conditions. This latter point is important since, in general, objective measures of health status are rare in survey data, and where they do exist they are often too specific to particular health conditions (Hernandez-Quevedo et al. 2005). Therefore, in our study we use SAH as a measure of health for Portugal and France for waves 1 through 8.

It is important to mention that because of the econometric method described above, which

[18] This source of heterogeneity with respect to cutoff points has been termed "state dependent reporting bias" (Kerkhofs and Lindeboom 1995), "scale of reference bias" (Groot 2000), and "response category cut-point shift" (Murray et al. 2001).

[19] Its predictive power does not appear to vary across socioeconomic groups (see, e.g., Burström and Fredlund 2001).

assumes that the top category is the one with better health, we invert SAH. In this way, SAH goes from 1 (worse health [i.e. bad]) to 4 (better health [very good]).[20]

2.6.3 Covariates Used

The included covariates can be divided into three groups: socioeconomic variables, occupational and firm-related variables, and other variables. For the first group we include dummy variables representing marital status (married, widowed, or divorced/separated), with single as the reference category; size of the household (the number of people living in the same household); and two dummies to account for the number of children at different ages (<12 and <16). The income variable is the logarithm of equivalised annual household income, equivalised by the OECD-modified scale to adjust for household size and composition.[21] We include two dummies for the highest level of educational qualification completed (second stage [ISCED 3] and third level [ISCED 5-7]); less than second stage (ISCED 0-2) is the base group.[22]

For the occupational and firm-related variables, we use three dummies for the type of work contracts (fixed-term or short-term, casual work with no contract, and some other working arrangements), where permanent employment is the base group; occupational dummies; and industry dummies (following the ILO categories). We include dummies to control for the level of job satisfaction, which is in line with Datta Gupta and Kristensen (2008), who use this variable as a proxy for job stressors.[23] For the other variables, we include age as a second-order polynomial (i.e., age and $\frac{age^2}{100}$).

[20] We merged "very bad" and "bad" categories due to small sample size of the "very bad" category.

[21] The OEDC-modified scale gives a weight of 1 to the first adult, 0.5 to other persons age 14 or over, and 0.3 to each child younger than 14. For each person, the "equivalised total net income" is calculated as its household total net income divided by equivalised household size. In this case, we use the logarithm of household income (OEDC-modified scale), taking into account the concavity in the health-income relationship.

[22] The ISCED classification comes from the International Standard Classification of Education from UNESCO. It is a seven-level scale that allows for the comparison of educational levels in different countries.

[23] Dummies for job satisfaction levels might be endogenous. Nevertheless, we include them in the model since in some cases they are significant and also because their exclusion does not affect the coefficient of interest.

2.6.4 Summary Statistics and the Evolution of Health Outcomes and Weekly Working Hours

For the French case, the sample excludes those individuals who are employed in paid apprenticeships or training schemes, are self-employed, or are classified as unpaid family workers. We also exclude those who do not work in the private sector, those who work in firms that have fewer than 20 employees, or those younger than 20 or older than 60. In the French case, we observed that until 1998 the average of weekly working hours stays right around 42 hours (see Figure 1); nevertheless, from 1998, when the policy was implemented, weekly working hours decreased significantly to almost 39 hours in 2000. The declining trend increased in 1999 probably in order to meet the January 2000 deadline that the French government set when they announced the policy. This decline in weekly working hours coincides with an overall decline in the SAH measure (see Figure 2), which may be due to the effect of age on the sampled individuals (as found by Contoyannis et al. 2004a). However, a break in the declining trend can be found, with a positive and not significant effect from 1998 (i.e., when the policy was implemented).

For the Portuguese case, the estimation excludes those individuals who are employed in paid apprenticeships or training schemes, are self-employed, or are classified as unpaid family workers. We also exclude those individuals who work in the public administration and defense sectors and those who are younger than 20 or older than 60. As shown in Figure 1, we observe a drop in weekly working hours from 1996 to 1997 and from 1997 to 1998, periods when reductions in working hours took place. Furthermore, we observe a declining trend in the SAH measure across time until 1997, then a constant level in 1998, and a slight increase in 1999. Afterwards, it started to decrease marginally. This behaviour might suggest a lagged effect on health (examined below).

The difference-in-differences approach described above requires the definition of a control group and a treatment group and a pre- and post-treatment period. For the Portuguese case, we defined our pre-treatment period as 1994-1996 and our post-treatment period as 1997 onwards.[24]

[24] We used these groupings because all the interviews were carried out before November 1996 and the policy change was in place from December 1, 1996.

For the definition of the treatment and control groups, it is important to choose very similar groups in terms of observable characteristics, to make it more likely that the unobservable characteristics are similar as well, since the control group should reflect how the treatment group would behave in the absence of the treatment; otherwise, we would capture the joint effect of the treatment and the difference in behaviour between the two groups, making it impossible to identify the effect of the treatment. For the Portuguese case, we define our control and treatment group as those workers who were working 37-40 hours a week and those who were working 41-44 hours a week, respectively, just before the policy change occurred.

For the French case, we define the control group as those individuals who were working 30-35 hours a week and the treatment group as those who were working 36-39 hours per week before the policy change was in place. Accordingly, the difference-in-differences method requires the definition of pre- and post-treatment periods. In the French case, the interviews were conducted almost entirely in October of each year; hence, the pre-treatment period will be 1994-1997 and the post-treatment period will be 1998 onwards.

We present the summary statistics of the dependent variable and the covariates separately for each country, in Table 1.[25] This table suggests that most of the covariate averages are very similar when the control and treatment groups are compared in each country. This suggests that most variables are well balanced between the control and treatment group. This is also true when we compare the distribution of each covariate (see, e.g., the age situation in Portugal, presented in Figure 3).[26] The main differences between the control and treatment groups are in occupation and industry. These can be observed, for example, by looking at Figure 4 and Figure 5 for the Portuguese case.[27] These results were expected, as the number of job hours differs across occupations and industries, which suggests that controlling for occupation and industry is important for our purposes.

[25] In each case, we discuss the final sample data.

[26] Because of the large number of figures, the rest of the distributions are not presented but are available on request.

[27] Similar situations occur for France. These figures are not included but they are available on request.

2.7 Results

2.7.1 Portugal

We compute the average partial effects (APEs) for each of the four categories of SAH, but since the total changes of these probabilities should add up to zero (and to save space), we present the results for the best two probabilities (very good health and good health) in Table 2 for men and women. In each case, we also separate the results by age range.[28]

From columns (1) and (2) of Table 2, we observed that for men younger than the average age (37 years), the top two health categories (very good and good health, respectively) present negative APEs for the treated group (41-44 hours) in 1997 and 1998. This would mean that in 1997, after controlling for several covariates, those treated individuals have a lower probability of being in the top two categories (i.e., having very good or good health), but as the β coefficient in column (3) is not significant, these variations are not statistically different from zero. The same results are obtained for men above the average age (columns [4] and [5]). These results imply that the policy did not affect the probability of reporting very good or good health for this group.

These previous analyses assume that the impact of the reduction in hours is contemporaneous, but it could be the case that the impact of the policy takes time to show up in terms of health, as might be implied by Figure 2. For this reason we repeated the previous estimation but replaced the group dummy in year t by its one-year lagged value. In terms of equation (2.1). This implies the replacement of g_i defined at year t by g_i defined at year $t-1$. By doing this, we expect to capture any effect due to the implementation of the policy one year later. The results can be seen in Table 3 for men and women, each separated by age range. In all these cases, the effects are not statistically significant and are similar to the contemporaneous case.

[28] To define the age ranges, we used the average age of the Portuguese sample, 37 years.

2.7.2 France

The results for men below the average age of 39 years (columns [1] and [2] of Table 4) show negative APEs for the top two health categories for the treated group (e.g., 36-39 hours) in 1998 and 1999. The β coefficients are significant at 1% and 5%, respectively. In particular, the treatment reduces the probability of reporting very good or good health by 23 percentage points (13 and 10 percentage points, respectively) in 1998 and by 17.5 percentage points (11 and 6.5 percentage points, respectively) in 1999. The estimated APE across all periods results in a detriment in the probability of reporting very good health and good health by 6.0 and 4.2 percentage points, respectively.[29] As the probability of reporting very good or good health in 1997 is around 67%, the effect of the policy reduces the probability of reporting very good or good health by 14.9% on average. The results for men above the average age (columns [4] and [5] of Table 4) show no significant effects due to the policy change.

For women, results are exactly the opposite of those obtained for men. In particular, for women below the average age, the coefficient is significant at 5% and positive in 1998 (column [9] of Table 4). This result implies that the policy change increased the probability of reporting very good health by 15 percentage points and caused a combined increase of 13 percentage points in the probability of reporting very good or good health. The estimated APE across all periods results in an average increase of 3.8 percentage points in the probability of reporting very good health. As the pre-treatment probability of reporting very good or good health for this group is 64% in 1997, our result suggests that the effect of the policy change generated an average increase of around 5.9% in the probability of reporting very good health.[30]

2.7.3 Discussion of the Results

The discussion of the results obtained above is complicated, as there are several dimensions to consider—in particular, comparisons between the treatment and control group by country, age

[29] This average is calculated by adding 13.1 percentage points (in 1998) plus 11 (in 1999) plus zero (in 2000 and 2001) and dividing by 4, which equals 6.0 percentage points. A similar calculation is used to get 4.2 percentage points.

[30] This includes coefficients that are significant at 5% at least.

range, and gender. Here we present some hypotheses that might explain our results. But as we will see, more research on this topic should be done in the future.

The Trade-off

The nonzero effect of the policy change in France and the zero effect in Portugal may be explained by drawing on both the literature on promotions and the literature on psychological health effects. On the one hand, the psychological literature suggests that a reduction in working hours might have positive effects on health since individuals will have more time to recover (the so-called external recovery; Taris et al. 2006). On the other hand, the probability of getting promoted provides an incentive for the worker to exert effort without the need for any formal contract with the firm (Francesconi 2001). As this effort or propensity to work hard is not directly observable, firms will use indicators, such as hours of work or overtime hours, in selecting workers for promotion. Thus, a mandatory reduction in working hours for treated individuals (relative to controls) will limit the scope for competition via hours for this group of workers. This negative effect on the probability of promotions (which affects the future income pattern) may have a negative impact on health, as individuals may become concerned and stressed about their future career and income. This effect is in line with the implications derived from the effort-reward imbalance (ERI) model (Siegrist 1996) explained in section 2.[31] Therefore, we may have a trade-off between two effects.

The differences for Men across Age Ranges

As individuals in Portugal already work longer hours than those in France, it would be more difficult for them to use overtime work as a way of increasing their chances of promotion. In other words, the scope for competition through hours is lower in Portugal than in France. Therefore, a mandatory reduction in working hours might have more negative effects on treated relative to control men in France than in Portugal.

[31] The ERI model acknowledges the link between high-cost/low-gain conditions, which are considered particularly stressful—for example, high-effort situations associated with the lack of promotion prospects (Siegrist 1996).

Furthermore, the promotions explanation also seems to be useful to help us explain the difference between the effects found for men at different age ranges—in particular, for those below versus those above the average age (i.e., those 20-38 years old versus those 39-60 years old). This is because, if the hypothesis about the effect on promotions is true, we should expect a more negative effect on those individuals who are in the beginning or early stages of their careers relative to those who are more settled in their jobs.

The differences for Women across Age Ranges

What is more difficult to explain with the promotions hypothesis are the effects of the reduction in working hours on the health status of women in France. This is because, as it was stated, our hypothesis would suggest a negative effect; however, we find a positive one. A potential extension to our hypothesis would be that women, and especially those below the average age (i.e., those younger than 38 years), have already internalized that the probability of promotions in the future might be undermined by the loss of human capital due to pregnancy. Because of that, the negative effect on the probability of promotion for those treated women relative to the control group might be smaller than the potential positive effect on health, which may give an overall positive effect.

The above hypothesis is in line with what Booth and Francesconi (2000) found. They analyzed the difference of promotion predictors by gender and found that, by comparing the effects by gender, there are "striking similarities and important differences". In particular, they found that working longer overtime hours is positively associated with the probability of promotion. As an example, they point out that the probability of promotion for men working part-time decreases by 6 percentage points as compared to men working full-time, while the effect for women working part-time is much smaller relative to those women who work full-time. This implies that fewer hours of work for women have a weaker negative effect on the probability of promotions, which is in line with our hypothesis above.

2.7.4 Robustness of the Results

Attrition

Attrition, which occurs when individuals drop out, is a typical problem to deal with when using panel data. If these individuals are missing at random, then there is no problem since the remaining individuals will have characteristics similar to those in the initial waves. The problem with attrition appears when attrition is endogenous (i.e., due to variables correlated with the dependent variable).

In our case, attrition is likely to be endogenous since healthier individuals should last longer in our panel (i.e., individuals in worse health should drop out from the panel more often than those individuals in better health). Due to this, Jones et al. (2006) studied the health-related non-response in the first 11 waves of the British Household Panel Survey and the full eight waves of the European Community Household Panel (ECHP) and explored its consequences for dynamic models of the association between socioeconomic status and self-assessed health. They use the Verbeek and Nijman (1992) test and correct for non-response (with the inverse probability weights method) in empirical models of the effect of socioeconomic status on self-assessed health. Jones et al. (2006) found that there is health-related non-response in the data, with those in very poor initial health more likely to drop out; nevertheless, as they point out, "a comparison of estimates—based on the balanced sample, the unbalanced sample and corrected for non-response by using inverse probability weights—shows that, on the whole, there are not substantive differences in the average partial effects of the variables of interest".

To test for the possibility of endogenous attrition, we follow the same approach. That is, we use a variable addition test as proposed by Verbeek and Nijman (1992), which tests the significance of an indicator that counts the number of waves observed for each individual. The reasoning behind this test is that if non-response is random, indicators of an individual's pattern of survey responses (e.g., number of waves [nw]) should not be associated with the outcome of interest (health) after controlling for the observed covariates x.[32] The results of this test for the ECHP suggest that attrition is not endogenous for France, although for Portugal it is significant

[32] This means that it tests a conditional independence condition $E\ (health \mid x,\ nw) = E(health \mid x)$.

for men and women at 10% (see Table 5). These differ from the results of Jones et al. (2006), who find that the indicator is significant at 1%. An explanation for this difference might be that Jones et al. (2006) use the entire sample of individuals (i.e., those older than 16 who are employed, those who are unemployed, etc.), while in our case, and since we are interested in the effect of a reduction in hours on health outcomes, we use only those who are employed and age 20-60. This subgroup is healthier on average than, for example, unemployed adults; therefore, the ECHP is expected to have a weaker dropout rate due to health-related reasons.[33]

Since the test used above may have low power (Verbeek 2000),[34] we investigate this issue further. We compare the coefficients of the model with the indicator relative to the coefficients of the model without the indicator. We find that the estimates are similar, which is the same result obtained by Contoyannis et al. (2004a) and Jones et al. (2006) (see Table 6 for the Portuguese case, where columns [1] and [2] show the results for men and columns [3] and [4] for women, and similarly Table 7 for the French case). This supports the idea that, even with endogenous attrition, its existence does not affect the estimates of the variables of interests. These results are corroborated when we compare the coefficients of the balanced and unbalanced panels for men and women in Portugal (see Table 8) and France (see Table 9). Therefore, and similarly to previous studies, we do find some support for no significant effects of attrition bias on the estimates of the variables of interest.[35]

Alternative Definition of Control Groups

To check the robustness of our results in relation to the specification of treatment and control groups, we present the results when slight modifications are introduced to the definition of the control group range, which can be increased or decreased and where either option has benefits and costs. On the one hand, the advantage of a narrow range is that we make the control group

[33] Another reason for the difference between our result and the one presented by Jones et al. (2006) could be that our model includes the initial health status and its interaction with the time effect as covariates, while Jones et al. (2006) use instead a lagged dependent variable as a covariate.

[34] The test may have low power because it relies on the sample of observed outcomes for health and will not capture non-response associated with idiosyncratic shocks that are not reflected in observed past health.

[35] This can also be done by comparing the coefficients with a Hausman test. We did not, however, make such a comparison, since the coefficients are almost the same with or without the indicator and the Hausman test also has low power (see Jones et al. 2006, 14).

as similar as possible to those in the treated group (in terms of unobservable characteristics), and therefore it is expected that individuals in the control group respond to shocks in similar ways as do individuals in the treatment group. On the other hand, widening the control group range also has some advantages: first, it increases the number of observations and, therefore, the precision of the estimation (ceteris paribus). Second, it diminishes the problem created by the potential misclassification of hours. Third, it reduces the impact of threats to the identification strategy, such as spillover effects and substitution between groups.

In our case, the control group definition is modified by increasing and reducing its range for each country. Results are presented in Table 10 and Table 11 for Portugal and France, respectively, for some of the verified cases. For Portugal we present the case when the control group is defined as age 36-40 instead of the 37-40 age group used above. For the French case we present the results when the control group is defined as age 31-35 instead of the 30-35 age group used above. As can be seen, results are very similar to those presented above in each case; therefore, conclusions do not change with changes in the definition of the control group.

Flexibility

In Portugal, greater flexibility was introduced along with the reduction in hours. Greater flexibility may involve the reorganization of work; this reorganization, rather than the reduction in hours, may affect the health of individuals. This generally depends on the type of industry and occupation, and given that we control for both, it is less likely to be the case. In France, flexibility was introduced only in 2000; hence, it should not affect our results for 1998 and 1999.

Common Macro Trend and Common Support

Results, when the difference-in-differences approach is used, rely on the crucial assumption of a common macro trend. In practice, this means that interactions between the group dummies and the time effects in the absence of the policy change should be insignificant, because if they were significant it would imply that control and treatment groups behave differently when there is no policy in place. The usual method to test this when several time periods are available is to check the significance of the interactions between the group dummy and the time effect before the policy takes place.

For the Portuguese case we can observe the interaction in 1996, which is not significant (see Table 2). For the French case we observe the interactions for 1996 and 1997. As can be seen for both men and women, they are not significant (see Table 4). All these results suggest that there are no significant differences in trajectories between the control and treatment groups before the policy takes place.

As the difference-in-differences approach compares the treatment group with a control group, the latter should represent what would have happened with those treated if they had been not been treated. In order for this to be true, one should contrast comparable individuals. This condition is also known as "full common support". In our case, this is likely since both the treatment and control group have full common support (see, e.g., Figures 3-5) and also are very similar in observables.[36] Therefore, it is reasonable to assume that they are also similar in unobservable characteristics.

Alternative Specification

In models where health is the dependent variable, it is typical to include a lagged dependent variable in order to try to capture health state dependence. Unfortunately, because we are exploiting a policy change, the difference-in-differences coefficient would not capture the effect of the policy change on health outcomes of period t. This is because we would be doing a ceteris paribus analysis on the change of P($y_{it} = j|\ y_{it-1}, x_{it,....}$), which conditions on $y_{i,t-1}$for the pre- and post-treatment periods for the control as well as for the treatment group. That would not be correct since, for post-treatment periods, the lagged dependent variable would be forced to be the same between the treatment and control groups, which may not be the case. Therefore, if the policy change had any effect, this latter approach would be incorrect. In any case, since in the health economics literature it is common to include a lagged dependent variable as a

[36] See summary statistics above. For the Average Treatment on the Treated (ATT), full common support means that given X, the probability of being treated is less than one, that is, $P(D = 1|\ X) < 1$. This implies that for a covariate X there are treated and control individuals, not only treated individuals.

covariate, we present in Table 12 the coefficients for France obtained when a lagged dependent variable is included as a control instead of the initial health status. As can be seen, results are similar with respect to those in Table 4, which uses the initial health condition as well as its interactions with the time effect, although there are some differences in the significance level for France. As expected, results are not significantly different for Portugal.[37]

Measurement Error

The ECHP data used in our study, as all survey data, might be subject to measurement error. This is especially important for hours of work, since measurement error in this variable could lead to misclassification of individuals into hours groups and thereby to a dilution of the estimated effect on health outcomes. As the ECHP does not include a direct question about overtime, we might have misclassified individuals into hours groups.[38] This would generate a downward bias on our results, giving us a lower bound of the effect of the policy change on health outcomes. To test how important this effect is, we exclude those sectors with higher probability of working overtime in France and Portugal just before the policy was in place (from the LABORSTA database).[39] This probability is mainly affected by occupation (economic activity). In particular, workers in wholesale retail trade, restaurants, and hotels and in community, social, and personal services sectors have a higher probability of working overtime in Portugal in 1996. For France, sectors with a higher probability of working overtime in 1998 are wholesale retail trade, restaurants, and hotels, as well as real estate, renting, and business activities. Therefore, if we do have an important misclassification due to the lack of a direct question about overtime, we should expect our results to change by excluding workers in these categories, since they should have higher probabilities of misclassification. Results suggest that estimates do not change when we exclude workers in the mentioned categories; hence, it seems that misclassification is not a significant problem in our case.

[37] These results are not included but are available upon request.

[38] For example, someone in Portugal who reports 44 hours a week of usual hours might imply (a) 44 normal hours and zero overtime or (b) 40 hours plus 4 hours of overtime. This is important since, without further information, in the first case the individual will be categorized in the treatment group and in the latter case in the control group. The same kind of problem might arise in the French case.

[39] See http://laborsta.ilo.org/.

2.8 Conclusion

The overall theoretical effect of reductions in working hours on health outcomes is ambiguous, and therefore empirical evidence is needed. Until now there has been no such empirical evidence. To our knowledge, we provide the first of such evidence. The identification of the health effects of reductions in working hours in a regression framework becomes complicated, as working hours may be endogenous due to the so-called healthy worker effect. This refers to the fact that workers with good mental and physical health are generally more likely to work longer hours than those with fair or bad health. Furthermore, as the relationship between reductions in working hours and health may not be monotonic, it is important to take into account the pre-treatment level of working hours.

To overcome these caveats, we exploit exogenous reductions in working hours coming from labour regulation for two countries with two different levels of working hours (France and Portugal, with 35 and 40 weekly hours, respectively). To enhance comparability between these two countries, we use the European Community Household Panel dataset (ECHP) between 1994 and 2001. One of the advantages of the ECHP is that it is a homogeneous questionnaire that includes health as well as labour information for individuals. We find that reductions in working hours have different effects by country, gender, and age range. In particular, we find no significant effects for Portugal and a significant one for France. In France, the effect is negative for men and positive for women, and in both cases the effect is significant only for younger individuals. Furthermore, for men (women), the results by country may imply that the relationship between hours and health may not be monotonic, as the country with a lower threshold of working hours (i.e., France) presents negative (positive) effects of the reduction in weekly hours, while the country with the higher threshold (i.e., Portugal) presents no significant effects.

These results may be explained by the trade-off between the psychological and promotions hypotheses, where the latter seems to have stronger effects than the former for young men in France, while the reverse is true for young women. This opposite result may be found because the promotion channel has weaker effects on young women, who already internalize the effect of pregnancy on their promotion pattern. From our results, we conclude that reductions in working hours need to be more carefully applied, as there are some levels of working hours that do not justify a legally imposed further reduction, since some groups may be worse off. Finally, it could be the case that the relationship between the psychological and promotions hypotheses might behave differently when higher thresholds of working hours are investigated. That may be the case represented by Portugal, where no effects were found. However, as an extension to further test this latter hypothesis, researchers could analyze reductions in working hours in countries with higher thresholds of working hours.

2.9 Appendix

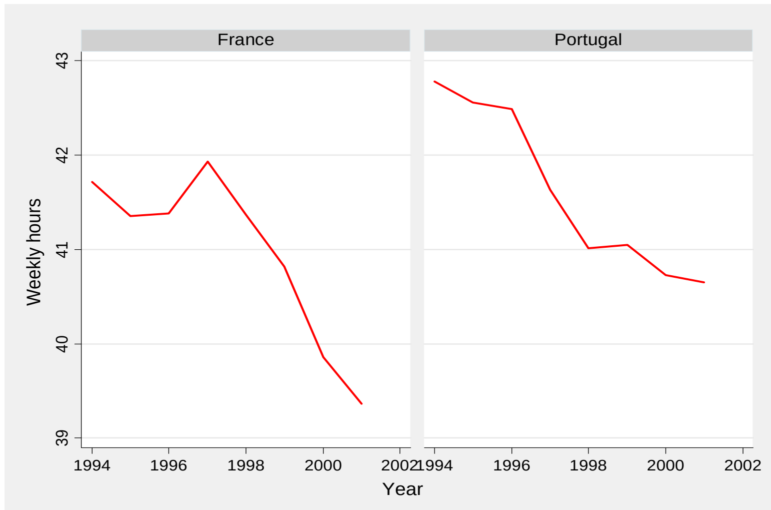


Figure 1: Weekly Hours in France and Portugal 1994-2001

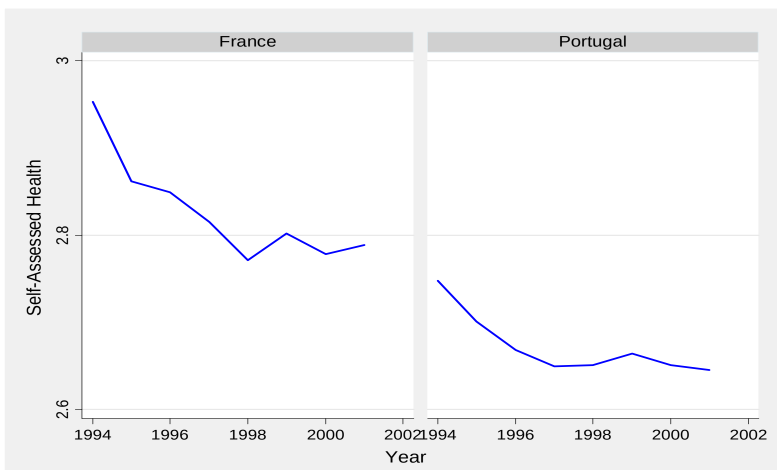


Figure 2: Self-Assessed Health in France and Portugal 1994-2001

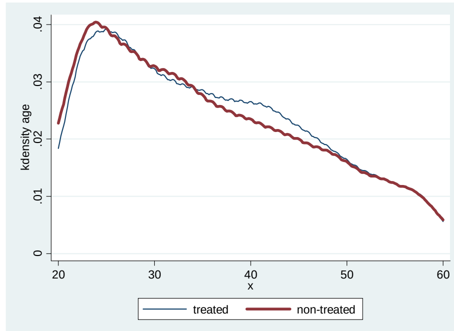


Figure 3: Distribution of Age (Portugal)

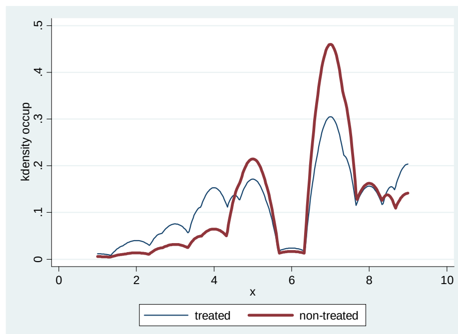


Figure 4: Distribution of Occupation (Portugal)

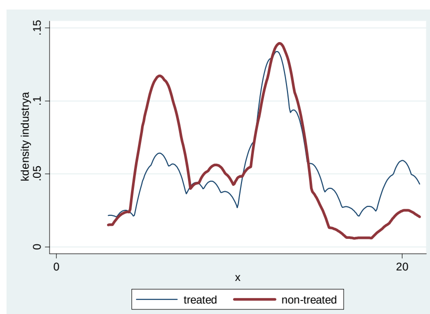


Figure 5: Distribution of Industry (Portugal)

Table N^{o}1

Summary Statistics for Portugal and France

	ECHP					
	Portugal			France		
	Control	Treated	Difference	Control	Treated	Difference
	(1)	(2)	(3)	(4)	(5)	(6)
SAH 1-4 (Bad & Very Bad-Very Good)	2.33	2.33	0.00	2.71	2.81	-0.10
Weekly Hours	39.8	43.2	-3.40***	33.1	38.7	-5.60***
ln(Household income)	14.6	14.5	0.01	12.5	12.5	0.00
Age (between 20-60 years old)	36.9	34.7	2.20	39.2	38.6	0.65
Female (male=0, female=1)	0.42	0.44	-0.02	0.79	0.38	0.40***
Children <12	1.64	1.60	0.04	1.53	1.58	-0.05
Children <16	1.80	1.79	0.01	1.83	1.83	0.00
Job satisfaction 1 (Not at all satisfied)	0.01	0.01	0.00	0.03	0.02	0.01
Job satisfaction 2 (Largely unsatisfied)	0.04	0.05	-0.01	0.03	0.03	0.00
Job satisfaction 3 (Mildly unsatisfied)	0.20	0.24	-0.04	0.10	0.14	-0.04
Job satisfaction 4 (Mildly satisfied)	0.51	0.53	-0.03	0.36	0.33	0.03
Job satisfaction 5 (Largely satisfied)	0.20	0.15	0.05	0.41	0.43	-0.02
Job satisfaction 6 (Fully satisfied)	0.04	0.01	0.03	0.04	0.01	0.03
Educ 1 (3rd level = ISCED 5-7)	0.05	0.02	0.03	0.17	0.18	-0.01
Educ 2 (2nd stage = ISCED 3)	0.18	0.07	0.11***	0.41	0.45	-0.04
Educ 3 (Less than 2nd stage = ISCED 0-2)	0.77	0.91	-0.14***	0.42	0.37	0.05
Household size	3.81	3.89	-0.08	3.37	3.21	0.16

***p<1%,**p<5% and *p<10%

Table 1(cont)

Summary Statistics for Portugal and France

	ECHP					
	Portugal			France		
	Control	Treated	Difference	Control	Treated	Difference
	(1)	(2)	(3)	(4)	(5)	(6)
M. Status 1 (Married)	0.68	0.65	0.03	0.70	0.60	0.10***
M. Status 2 (separated/divorced & widowed)	0.06	0.05	0.01	0.07	0.08	-0.01
M. Status 3 (single)	0.26	0.30	-0.04	0.23	0.32	-0.09***
Type of Contract 1 (permanent)	0.78	0.85	-0.07***	0.80	0.86	-0.06
Type of Contract 2 (fixed/short-term)	0.13	0.11	0.02	0.14	0.09	0.05
Type of Contract 3 (work with no contract)	0.04	0.02	0.02	0.03	0.02	0.01
Type of Contract 4 (other)	0.05	0.02	0.03	0.03	0.02	0.01
Occup. 1 (Legislators/Senior Officers/Managers)	0.02	0.01	0.01	0.02	0.02	0.00
Occup. 2 (Professionals)	0.04	0.02	0.02***	0.05	0.02	0.03***
Occup. 3 (Technical and associate professionals)	0.09	0.03	0.06***	0.11	0.18	-0.07***
Occup. 4 (Clerks)	0.17	0.05	0.12***	0.30	0.18	0.12***
Occup. 5 (Service and shopping)	0.15	0.15	0.00	0.21	0.09	0.12***
Occup. 6 (Skilled agriculture and fisherman)	0.03	0.02	0.01	0.01	0.02	-0.01
Occup. 7 (Craft and related)	0.21	0.44	-0.23***	0.06	0.22	-0.16***
Occup. 8 (Operators and assemblers)	0.12	0.14	-0.02	0.09	0.21	-0.12***
Occup. 9 (Elementary occupations)	0.17	0.14	0.03	0.15	0.07	0.08***

***p<1%,**p<5% and *p<10%

Table 1(cont)

Summary Statistics for Portugal and France

	ECHP					
	Portugal			France		
	Control	Treated	Difference	Control	Treated	Difference
	(1)	(2)	(3)	(4)	(5)	(6)
Ind. 1 (Agric., hunting, forestry and fishing)	0.05	0.04	0.01	0.02	0.02	0.00
Ind. 2 (Mining and quarrying)	0.01	0.01	0.00	0.02	0.02	0.00
Ind. 3 (Electricity, gas and water)	0.03	0.01	0.02***	0.02	0.02	0.00
Ind. 4 (Manufacturing)	0.23	0.45	-0.22***	0.21	0.39	-0.18***
Ind. 5 (Construction)	0.14	0.11	0.03	0.02	0.10	-0.08***
Ind. 6 (wholesale and retail trade, etc)	0.12	0.18	-0.06***	0.20	0.19	0.01
Ind. 7 (Hotels and restaurants)	0.04	0.08	-0.04***	0.04	0.01	0.03**
Ind. 8 (Transport, storage and communication)	0.08	0.03	0.05***	0.03	0.02	0.01
Ind. 9 (Financial Intermediation)	0.02	0.01	0.01	0.04	0.04	0.00
Ind. 10 (R. state renting and business activities)	0.05	0.01	0.04***	0.09	0.09	0.00
Ind. 11 (Public admin. and defense)	0.00	0.00	0.00	0.00	0.00	0.00
Ind. 12 (Education)	0.10	0.01	0.09***	0.04	0.01	0.03***
Ind. 13 (Health and social work)	0.06	0.03	0.03***	0.13	0.06	0.07***
Ind. 14 (Other community and social activities)	0.07	0.02	0.05***	0.14	0.03	0.11***

***p<1%,**p<5% and *p<10%

Table 2

Average Partial Effects in Portugal, by Gender and Age Range, of the Hours Reduction in Period t in Individuals' Self-Assessed Health in Period t

	Men						Women					
	Age 20-36			Age 37-60			Age 20-36			Age 37-60		
Health	Very Good	Good	β^{RE}	Very Good	Good	β^{RE}	Very Good	Good	β^{RE}	Very Good	Good	β^{RE}
Year	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
1996	0.017	0.014	0.18	0.004	0.023	0.11	0.007	0.021	0.13	0.005	0.042	0.19
1997	-0.008	-0.010	-0.10	0.002	0.014	0.07	0.001	0.001	0.01	-0.004	-0.048	-0.21
1998	-0.021	-0.016	-0.21	0.001	0.007	0.03	-0.005	-0.019	-0.11	-0.003	-0.026	-0.11
1999	-0.008	-0.011	-0.10	0.005	0.031	0.15	-0.002	-0.007	-0.04	-0.001	-0.009	-0.04
2000	-0.016	-0.023	-0.20	-0.005	-0.042	-0.19	-0.001	-0.001	-0.01	-0.002	-0.025	-0.11
2001	-0.001	-0.002	-0.02	-0.006	-0.049	-0.22	-0.001	-0.005	-0.03	-0.004	-0.037	-0.16
Obs.	4,206			3,414			2,586			1,906		
ICC	0.72			0.88			0.83			0.85		

Cutoff points as well as the estimated coefficients of the rest of the covariates are not reported, but are available on request. ICC is the intraclass correlation which is equal to $\frac{\sigma_\alpha^2}{1+\sigma_\alpha^2}$. The estimation is carried out with clustered standard errors at the individual level. *p<10%, **p<5%, ***p<1%.

Table 3

Average Partial Effects in Portugal, by Gender and Age Range, of the Hours Reduction in Period t in Individuals' Self-Assessed Health in Period t+1

	Men						Women					
	Age 20-36			Age 37-60			Age 20-36			Age 37-60		
Health	Very Good	Good	β^{RE}	Very Good	Good	β^{RE}	Very Good	Good	β^{RE}	Very Good	Good	β^{RE}
Year	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
1996	0.017	0.016	0.19	0.001	0.001	0.01	0.011	0.033	0.20	0.010	0.072	0.34
1997	-0.008	-0.013	-0.11	0.002	0.010	0.05	0.003	0.012	0.07	-0.002	-0.020	-0.09
1998	-0.023	-0.019	-0.24	0.001	0.004	0.02	-0.040	-0.015	-0.08	-0.001	-0.004	-0.02
1999	-0.009	-0.013	-0.11	0.003	0.018	0.09	-0.001	-0.001	-0.01	-0.001	-0.001	-0.01
2000	-0.013	-0.021	-0.17	-0.008	-0.065	-0.30	-0.004	-0.017	-0.10	-0.003	-0.032	-0.15
2001	-0.008	-0.012	-0.10	-0.010	-0.078	-0.36	-0.002	-0.007	-0.04	-0.003	-0.030	-0.14
Obs.		3,639			3,159			2,098			1,733	
ICC		0.79			0.90			0.82			0.87	

Cutoff points as well as the estimated coefficients of the rest of the covariates are not reported, but are available on request. ICC is the intraclass correlation which is equal to $\frac{\sigma_\alpha^2}{1+\sigma_\alpha^2}$. The estimation is carried out with clustered standard errors at the individual level. *p<10%, **p<5%, ***p<1%.

Table 4

Average Partial Effects in France, by Gender and Age Range, of the Hours Reduction in Period t in Individuals' Self-Assessed Health in Period t

	Men						Women					
	Age 20-38			Age 39-60			Age 20-38			Age 39-60		
Health	Very Good	Good	β^{RE}	Very Good	Good	β^{RE}	Very Good	Good	β^{RE}	Very Good	Good	β^{RE}
Year	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
1996	0.047	-0.003	0.22	0.093	0.052	0.61	0.043	0.005	0.24	0.014	0.019	0.13
1997	-0.030	-0.005	-0.16	0.022	0.023	0.18	0.073	0.001	0.39	0.011	0.012	0.10
1998	-0.131	-0.101	-0.93***	-0.009	-0.012	-0.08	0.153	-0.025	0.74**	0.016	0.017	0.14
1999	-0.110	-0.065	-0.72**	0.100	0.052	0.65	0.054	0.004	0.29	0.003	0.004	0.03
2000	-0.033	-0.060	-0.18	0.047	0.039	0.36	0.044	0.005	0.24	0.043	0.035	0.34
2001	-0.064	-0.020	-0.37	0.006	0.008	0.06	0.121	-0.012	0.61*	0.003	0.004	0.03
Obs.		3,876			4,585			2,214			2,204	
ICC		0.49			0.56			0.56			0.66	

Cutoff points as well as the estimated coefficients of the rest of the covariates are not reported, but are available on request. ICC is the intraclass correlation which is equal to $\frac{\sigma_\alpha^2}{1+\sigma_\alpha^2}$. The estimation is carried out with clustered standard errors at the individual level. *p<10%, **p<5%, ***p<1%.

Table 5

Test for Endogenous Attrition for Portugal

	Portugal		France	
	Men	Women	Men	Women
	(1)	(2)	(3)	(4)
Numwaves	0.020*	0.028*	-0.012	0.018
	(0.012)	(0.016)	(0.010)	(0.014)

Clustered standard errors are at the individual level. *p<10%, **p<5%, ***p<1%.

Table 6

Coefficients of the Random-Effect Ordered Probit for Portugal in the Unbalanced Case with and without the Attrition Indicators

	Men		Women	
	No Indicator (1)	With Indicator (2)	No Indicator (3)	With Indicator (4)
1996	0.13	0.13	0.15	0.16
1997	0.01	0.01	-0.08	-0.08
1998	0.18	0.17	-0.11	-0.11
1999	0.04	0.04	0.04	0.05
2000	-0.14	-0.14	-0.05	-0.05
2001	-0.05	-0.05	-0.09	-0.09
ICC	0.77	0.78	0.84	0.86
Log Likelihood	-5,263	-5,263	-3,261	-3,261
Obs.	7,620		4,492	

ICC is the intraclass correlation which is equal to $\frac{\sigma_\alpha^2}{1+\sigma_\alpha^2}$. Clustered standard errors are at the individual level. *p<10%, **p<5%, ***p<1%.

Table 7

Coefficients of the Random-Effect Ordered Probit for France in the Unbalanced Case with and without the Attrition Indicators

	Men		Women	
	No Indicator (1)	With Indicator (2)	No Indicator (3)	With Indicator (4)
1996	0.43	0.43	0.02	0.03
1997	0.01	0.01	0.14	0.14
1998	-0.48**	-0.48**	0.33	0.33
1999	0.03	0.03	0.03	0.03
2000	0.14	0.15	0.19	0.19
2001	-0.15	-0.15	0.12	0.12
ICC	0.54	0.54	0.58	0.58
Log Likelihood	-7,283	-7,283	-3,707	-3,707
Obs.	8,461		4,418	

ICC is the intraclass correlation which is equal to $\frac{\sigma_\alpha^2}{1+\sigma_\alpha^2}$. Clustered standard errors are at the individual level. *p<10%, **p<5%, ***p<1%.

Table 8

Comparison of Coefficients Between the Balanced and Unbalanced Panel for Portugal

	Balanced		Unbalanced	
	Men	Women	Men	Women
	(1)	(2)	(3)	(4)
1996	0.19	0.24	0.13	0.15
1997	0.08	-0.19	0.01	-0.08
1998	0.27	-0.06	0.18	-0.11
1999	0.11	0.08	0.04	0.04
2000	-0.26	-0.17	-0.14	-0.05
2001	-0.13	-0.04	-0.05	-0.09
ICC	0.70	0.78	0.77	0.84
Log Likelihood	-2,726	-1,403	-5,263	-3,261
Obs.	2,989	2,069	7,620	4,492

ICC is the intraclass correlation which is equal to $\frac{\sigma_\alpha^2}{1+\sigma_\alpha^2}$. Clustered standard errors are at the individual level. *p<10%, **p<5%, ***p<1%.

Table 9

Comparison of Coefficients Between the Balanced and Unbalanced Panel for France

	Group 1			
	Balanced		Unbalanced	
	Men	Women	Men	Women
	(1)	(2)	(3)	(4)
1996	0.32	0.08	0.43	0.02
1997	-0.03	0.18	0.01	0.14
1998	-0.39**	0.36	-0.48**	0.33
1999	0.07	0.15	0.03	0.03
2000	0.19	0.18	0.15	0.19
2001	-0.18	0.21	-0.15	0.12
ICC	0.52	0.54	0.54	0.58
Log Likelihood	-2,711	-1,316	-7,283	-3,707
Obs.	3,289	1,645	8,461	4,418

ICC is the intraclass correlation, which is equal to $\frac{\sigma_\alpha^2}{1+\sigma_\alpha^2}$. Clustered standard errors are at the individual level. *p<10%, **p<5%, ***p<1%.

Table 10

Average Partial Effects in Portugal, by Age Group and Gender, of the Hours Reduction in Period t in Individuals' Self-Assessed Health in Period t (Control Group Redefined as 36-40).

	Men						Women					
	Age 20-36			Age 37-60			Age 20-36			Age 37-60		
Health	Very Good	Good	β^{RE}	Very Good	Good	β^{RE}	Very Good	Good	β^{RE}	Very Good	Good	β^{RE}
Year	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
1996	0.013	0.011	0.13	0.002	0.014	0.07	-0.001	-0.001	-0.01	0.002	0.021	0.09
1997	-0.014	-0.019	-0.17	0.001	0.010	0.05	0.014	0.019	0.19	-0.007	-0.076	-0.34
1998	-0.023	-0.021	-0.24	0.002	0.007	0.04	-0.013	-0.031	-0.23	-0.005	-0.053	-0.24
1999	-0.016	-0.024	-0.20	0.006	0.032	0.16	-0.003	-0.004	-0.05	-0.002	-0.020	-0.09
2000	-0.016	-0.024	-0.21	-0.006	-0.044	-0.20	-0.002	-0.004	-0.04	-0.005	-0.054	-0.24
2001	-0.002	-0.002	-0.02	-0.007	-0.055	-0.26	-0.002	-0.006	-0.03	-0.005	-0.054	-0.24
Obs.		4,244			3,491			2,631			2,010	
ICC		0.70			0.87			0.87			0.89	

Cutoff points, as well as the estimated coefficients of the rest of the covariates, are not reported but are available on request. ICC is the intraclass correlation which is equal to $\frac{\sigma_\alpha^2}{1+\sigma_\alpha^2}$. The estimation is carried out with clustered standard errors at the individual level. *p<10%, **p<5%, ***p<1%.

Table 11

Average Partial Effects in France, by Age Group and Gender, of the Hours Reduction in Period t in Individuals' Self-Assessed Health in Period t (Control Group Redefined as age 31-35)

	Men						Women					
	Age 20-38			Age 39-60			Age 20-38			Age 39-60		
Health	Very Good	Good	β^{RE}	Very Good	Good	β^{RE}	Very Good	Good	β^{RE}	Very Good	Good	β^{RE}
Year	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
1996	0.019	-0.006	0.79	0.113	0.054	0.72	-0.009	-0.003	-0.06	0.033	0.056	0.35
1997	0.044	-0.002	0.21	0.003	0.004	0.03	0.087	-0.001	0.45	0.006	0.008	0.06
1998	-0.091	-0.043	-0.56**	-0.002	-0.010	-0.07	0.130	-0.015	0.64**	0.029	0.027	0.24
1999	-0.085	-0.037	-0.51**	0.091	0.050	0.64	0.046	0.005	0.25	0.015	0.016	0.13
2000	-0.016	-0.002	-0.09	-0.015	-0.022	-0.14	0.008	0.002	0.05	0.031	0.028	0.25
2001	-0.037	-0.007	-0.20	0.061	0.163	0.81	0.160	-0.027	0.77*	0.012	0.013	0.10
Obs.		3,842			4,559			2,118			2,085	
ICC		0.49			0.56			0.52			0.67	

Cutoff points, as well as the estimated coefficients of the rest of the covariates, are not reported but are available on request. ICC is the intraclass correlation which is equal to $\frac{\sigma_\alpha^2}{1+\sigma_\alpha^2}$. The estimation is carried out with clustered standard errors at the individual level. *p<10%, **p<5%, ***p<1%.

Table 12

Average Partial Effects in France, by Gender and Age Range, of the Hours Reduction in Period t in Individuals' Self-Assessed Health in Period t. Model with a Lagged Dependent Variable as a Covariate.

	Men						Women					
	Age 20-38			Age 39-60			Age 20-38			Age 39-60		
Health	Very Good	Good	β^{RE}	Very Good	Good	β^{RE}	Very Good	Good	β^{RE}	Very Good	Good	β^{RE}
Year	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
1996	0.035	-0.001	0.16	0.096	0.058	0.62	0.048	0.006	0.26	-0.002	-0.003	-0.02
1997	-0.012	-0.002	-0.06	0.034	0.035	0.26	0.066	0.004	0.34	0.017	0.018	0.14
1998	-0.127	-0.113	-0.88***	-0.010	-0.016	-0.10	0.147	-0.019	0.69**	0.025	0.025	0.20
1999	-0.081	-0.039	-0.48	0.110	0.059	0.69	0.024	0.005	0.13	0.013	0.015	0.11
2000	-0.033	-0.008	-0.17	0.032	0.034	0.24	0.036	0.006	0.19	0.050	0.039	0.37
2001	-0.077	-0.036	-0.45	-0.009	-0.014	-0.08	0.112	-0.006	0.54	0.001	0.002	0.01
Obs.		3,419			4,357			1,931			2,071	
ICC		0.32			0.33			0.45			0.38	

Cutoff points, as well as the estimated coefficients of the rest of the covariates, are not reported but are available on request. ICC is the intraclass correlation which is equal to $\frac{\sigma_\alpha^2}{1+\sigma_\alpha^2}$. The estimation is carried out with clustered standard errors at the individual level. *p<10%, **p<5%, ***p<1%.

Chapter 3

The unintended consequences of Childcare Regulation: A Regression Discontinuity Approach

3.1 Introduction

Childcare policies have been publicly debated since at least the 19th century, as the Industrial Revolution was fueled in part by the economic necessity of many women, single and married, to find wage work outside their homes.[1] Childcare policies have been designed mainly to strengthen the parent-child link without negatively affecting parents' labour market situation. This is particularly important in the Chilean case, as female participation in the labour market is low (47%; INE 2011) relative to other OECD countries (57%; OECD 2010).[2]

[1] See, for example, "The History of Child Care in the U.S.," which points out that "To draw attention to the need for child care and to demonstrate 'approved methods of rearing children from infancy on,' . . . a group of prominent New York philanthropists led by Josephine Jewell Dodge set up a Model Day Nursery in the Children's Building at the 1893 World's Columbian Exhibition in Chicago and then went on to found the National Federation of Day Nurseries (NFDN), the first nationwide organization devoted to this issue, in 1898" (Sonya Michel, "The History of Child Care in the U.S.," The Social Welfare History Project, http://www.socialwelfarehistory.com/programs/child-care-the-american-history/).

[2] Actually, Chile has one of the lowest rates of female labour market participation (above only Mexico, Turkey, and Italy) among OECD countries (OECD 2010).

Previous empirical literature on childcare policies can be classified into two main strands. On the one hand, there are studies that analyze the effects of childcare policies on the development of children's cognitive abilities (see Baker, Gruber, and Milligan 2005; Berlinski, Galiani, and Gertler 2009; Berlinski, Galiani, and Manacorda 2008; Bernal 2008; Carneiro, Löken, and Salvanes 2009; Herbst and Tekin 2010; and Urzúa and Veramendi 2011). On the other hand, there are studies that analyze the effects of childcare policies on women's labour market participation and employment (see Baker, Gruber, and Milligan 2005; Berlinsky and Galiani 2007; Betancor 2011; Blau and Tekin 2003; Cascio 2006; Encina and Martínez 2009; Gelbach 2002; Guzmán 2009; Jaumotte 2003; Schlosser 2011; and UNDP 2008).

There is no empirical evidence on who bears (i.e., pays) the financial burden of childcare regulation when it is not publicly funded. This is important since, if childcare is indeed paid for by firms, legislation that mandates that firms provide childcare is a tax on female workers in the sense that it creates a disincentive to hire female workers. However, if firms are not paying, someone else must. Thus, the objective of this study is to present, for the first time, evidence about who bears the financial burden of childcare (i.e., firms or employees). In order to do this, we study Chilean childcare regulation in which the Labour Code establishes that firms must bear the financial responsibility for childcare. In particular, Article 203 states that "every firm with more than 20 female workers, regardless of their age and marital status, has to provide childcare facilities within firm premises so that mothers can feed their children and leave them there while working". It also states that "it will be understood that firms fulfill this obligation if they pay the cost of a private childcare facility". This article also establishes that the employer will have to pay for the female workers' transportation costs, in case the childcare facility is located outside of the firm. Additionally, Article 206 states that female workers are granted up to one hour within the day to feed their children (if the childcare facility is located outside of the firm, there is a time extension to account for the time spent travelling from the firm to the facility and back), which is considered a worked hour. Currently, these regulations apply to female workers with children ages 6-24 months only (as women are granted sixth month maternity leaves).

Therefore, Chilean regulation theoretically imposes an additional cost to firms since, after a certain number of female workers, firms have to bear costs such as childcare provision, potential productivity losses for the firm due to the time spent by the female worker feeding her child, and, on occasion, the transportation costs to the childcare facility. In order to explore whether firms are indeed bearing these costs, we exploit the discontinuity given by Chilean regulation to compare wages of workers just above and just below the threshold specified in the regulation, using a regression discontinuity (RD) design (Hahn, Todd, and van der Klaauw 2001; Imbens and Lemieux 2008; Lee 2008; Lee and Lemieux 2010). If wages are lower for workers just above the threshold, it could imply that firms are transferring the costs on to their workers. If firms do not transfer all the cost, and if men and women are substitutes, there should be an employment composition effect, as it would be more convenient to hire relatively more men. Thus, we extend the analysis to the employment composition by analyzing it for those firms just below and just above the threshold.

For our study we use administrative data from the unemployment insurance system, provided by the Chilean Ministry of Labour. We show that even if the firm theoretically (legally) bears the financial cost of childcare, workers pay most of the "childcare bill" (nearly 90% of it) in the end, through lower wages.

Providing this empirical evidence is important, as there are several countries with systems in which the employer is responsible for childcare provision (such as Argentina, Bolivia, Brazil, Chile, Costa Rica, Ecuador, Guatemala, Honduras, Nicaragua, Paraguay, and Venezuela). Furthermore, learning from this evidence is also important; should those countries with mixed systems (such as Denmark, France, and Panama) or with no legally mandated private childcare (such as Cuba, El Salvador, and the United States) wish to modify their childcare policies, they may learn the effects of changing their systems to one with privately funded childcare, as in Chile.

This study is organized as follows: section 3.2 describes the institutional background, its evolution, and the economic incentives generated by it. In section 3.3 we present our empirical strategy, and in section 3.4 we present the data and the summary statistics. Finally, section 3.5 displays our results and presents robustness checks for our estimates, and section 3.6 concludes.

3.2 Institutional Background

Article 203 of the Chilean Labour Code has a long history. In 1917, it was established for the first time a law (No. 3,185) focused on childcare. This law established the employer's obligation to provide childcare within the firm if the firm had more than 50 female workers.

In 1931, the law was modified to lower the threshold of female workers from 50 to 20. Later, in 1981, the law was further modified to allow firms to provide childcare by paying an external private childcare provider (authorized by JUNJI).[3]

Since 1981, Article 203 has established that:

- Every firm with 20 or more female workers, regardless of their age or marital status, must provide childcare facilities within the firm premises so that mothers can feed their children and leave them there while working.
- Firms may fulfill this obligation by paying the cost of a private childcare facility.

This article also states that if the childcare facility provided by the employer is outside of the firm, the employer must pay the transportation costs that the female worker incurs. Additionally, Article 206 establishes that female workers are granted up to 1 hour within the day to feed their children (if the childcare facility is located outside of the firm, there is a time extension to account for the time spent travelling from the firm to the facility and back), which is considered a worked hour. Hence, all of the firms that are affected by Article 203 must also fulfill the obligations established by Article 206.

Currently, Article 203 of the Labour Code affects relatively few firms. However, it affects a large proportion of female dependent workers. Given the data supplied by the Chilean Ministry of Labour, as of October 2010, only 3% of firms in Chile (around 9,300) have 20 or more female workers. Nevertheless, these few firms employ more than 71% of dependent female workers,

[3] JUNJI stands for Junta Nacional de Jardines Infantiles, Chile's national organization of public childcare centres.

which makes the childcare costs faced by these firms quite high. Some descriptive statistics are shown in Table 1.

Finally, it is important to mention that if firms do not fulfill their obligations under Article 203, the penalty reaches 70 UTM per employee.[4] Given this, the number of firms that do not fulfill their obligations is very low. For example, in 2011, only 118 firms were found to have neglected their responsibilities, according to information provided by the Chilean Ministry of Labour.

3.3 Empirical Strategy

The way Article 203 operates allows us to use the discontinuity generated when a firm moves from 19 to 20 female workers since, from that point, it is mandatory for the firm to provide childcare services (inside or outside the firm's premises). This rule makes it possible to identify the impact of this regulation on the desired outcomes.

From now on, we will refer to "treatment" when Article 203 is activated (i.e., the firm has 20 or more female workers). In this way, let us call y_{i1} the variable of interest (e.g., wages) for individual i if she receives the treatment (i.e., works in a treated firm) and y_{i0} otherwise. Thus, an individual will be treated if she works in a firm with 20 or more female workers.

Let us call d_i the treatment variable for worker i, defined as follows:

$$d_i = \begin{cases} 1 & \text{if} \quad N_i \geq 20 \\ 0 & \text{if} \quad N_i < 20 \end{cases}$$

where N_i is the number of female workers in the firm of worker i. Let the outcome of untreated individuals be defined by the following linear model:

[4] UTM stands for Unidad Tributaria Mensual and is a monthly inflation-indexed measure. As of early May 2012, 1 UTM is equal to nearly US$81.

$$y_{0i} = \alpha + \beta \cdot N_i + u_i \tag{3.1}$$

and let the outcome of the treated individuals be defined by the homogeneous treatment effect model:

$$y_{1i} = y_{0i} + \varphi \tag{3.2}$$

Hence, the regression model will be:

$$y_i = \alpha + \beta \cdot N_i + \varphi \cdot d_i + u_i \tag{3.3}$$

where parameter φ is the one of interest as it captures the effect of the treatment. Following Hahn, Todd and van der Klaauw (2001), the parameter of interest can be expressed as:

$$\varphi = \frac{\lim_{N\to 20^+} E(y_i|N_i = N) - \lim_{N\to 20^-} E(y_i|N_i = N)}{\lim_{N\to 20^+} E(d_i|N_i = N) - \lim_{N\to 20^-} E(d_i|N_i = N)} \tag{3.4}$$

where $\lim_{N\to 20^+}$is the limit from the right of the threshold and $\lim_{N\to 20^-}$from the left side. We consider a sharp regression discontinuity design, that is, only individuals that are in firms with 20 or more female workers are treated. Thus, we can rewrite equation (3.4) as follows:

$$\varphi = \lim_{N\to 20^+} E(y_i|N_i = N) - \lim_{N\to 20^-} E(y_i|N_i = N) \tag{3.5}$$

Because $\lim_{N\to 20^+} E(d_i|N_i = N) - \lim_{N\to 20^-} E(d_i|N_i = N) = 1$ by definition. In order to allow for the existence of nonlinearities between the forcing variable (number of female workers within the firm) and the outcomes, we consider a smooth function $f(N_i)$ of the number of female workers in the firm in our regression model. Additionally, we include variables that may affect the dependent variable, denoted by vector z_i. Given these, we estimate the following equation:

$$y_i = f(N_i) + \varphi \cdot d_i + z_i'\gamma + u_i \tag{3.6}$$

where u is an error term such that $E(u|d, z) = 0$.

3.3.1 Parametric versus Non-Parametric

For a model such as the one presented above, the existing literature has used two approaches for the estimation: the parametric and the nonparametric (see Hahn, Todd, and van der Klaauw [2001], Imbens and Lemieux [2008], and Lee and Lemieux [2010] for a detailed discussion). In the parametric case, the simplest model is just to assume that $f(N_i)$ is linear. However, that would be too restrictive. A more general case would be to allow for the existence of nonlinearities (i.e. $f(N_i)$ may be not linear). To allow for this, we include polynomial functions of N_i in the model. One of the advantages of the parametric approach is that it is more efficient when the functional form is correct. However, if the functional form is incorrect, our results will be biased.[5] Another disadvantage of the parametric approach is that it provides estimates of the regression function over all values of N_i, while the RD design focuses on local estimates of the regression function at the cutoff point (Lee and Lemieux 2010).

In the nonparametric case, kernel regressions or local linear regressions can be used. Both are local methods, as they use data around the cutoff point to estimate the effect of the policy change on the desired outcome. However, kernel regression presents a boundary problem when applied in an RD design. This is because we are estimating a point effect at a boundary, which implies that kernel regression will be a weighted average of one-sided data points that will generate a systematic bias in the estimates (see Hahn, Todd, and van der Klaauw [2001] for a formal derivation of the bias). A solution to this problem has been suggested by Hahn, Todd, and van der Klaauw (2001), who propose using local linear regression to reduce the importance of the bias.[6]

As Lee and Lemieux (2010) point out, it is advisable to use both approaches (parametric and nonparametric) when estimating the smooth function, as neither alone presents the supreme solution regarding functional form problems. Therefore, the econometrician should see them more as complements than substitutes.

[5] For example, if the data suggest a nonlinear model when we estimate a linear one, the results might suggest a discontinuity when, in reality, it is just a nonlinear movement of the data.

[6] Hahn, Todd, and van der Klaauw (2001) show that the remaining bias is of an order of magnitude lower and is comparable to the usual bias in kernel regression at interior points.

The discrete nature of our assignment variable (number of female workers) has implications on the specification choice. Lee and Card (2008) state that, in this case, the conditions of the nonparametric estimation methods are not met, which implies that the model is not nonparametrically identified. The reason for this, given by the authors, is that even with an infinite amount of data, there would be no data in a region in an "arbitrarily" small neighborhood around the cutoff point. Consequently, they suggest that "one must use regressions to estimate the conditional expectation of the outcome variable at the cutoff point by extrapolation". Thus, the parametric approach should be used for estimation.

In a more recent article, Lee and Lemieux (2010) point out that the discreteness of the assignment variable does not introduce important econometric complications for the parametric estimation, provided that this variable is not too coarsely distributed (as in our case).[7] As suggested by Lee and Card (2008), if the polynomial function is correct, then least-squares inference is appropriate. However, as the true functional form is always unknown, the authors propose a procedure for inference that explicitly acknowledges errors in whatever parametric functional form is chosen. In this way, the authors propose at least the use of clustered standard errors.[8]

Given this, we use the parametric approach as our baseline case. However, since the distinction between when a running variable is discrete and when it is continuous in practical terms is somehow always arbitrary (since, strictly speaking, the running variable is always discrete), we also estimate the model using the nonparametric approach.

[7] Additionally, Lee and Lemieux (2010) point out that the discreteness of the assignment variable simplifies the problem of bandwidth choice when graphing the data, as "one can simply compute and graph the mean of the outcome variable for each value of the discrete assignment variable".

[8] Lee and Card (2008) suggest that one should not assume that the functional form "correctly" describes the underlying regression function, since there may be a deviation between them. The authors model any deviation of the true conditional means from the parametric function as a random specification error with an unknown variance, which induces a within-group correlation in the error. This implies that conventional standard error formulas understate the variablility of the least-squares estimate of the discontinuity gap. In this way, the authors propose the use of clustered standard errors, which lead to wider confidence intervals that reflect the imperfect fit of the parametric function away from the discontinuity point.

3.3.2 The Model

Our parametric specification is presented in the following equation:

$$y_i = \delta + \sum_{j=1}^{p} \kappa_j (N_i - 20)^j + \varphi \cdot d_i + z_i' \gamma + u_i$$

And the estimated parameters are given by:

$$(\hat{\delta}, \hat{\varphi}, \hat{\gamma}, \hat{\kappa}) = \text{argmin}_{(\delta,\varphi,\gamma,\kappa)} \sum_{i=1}^{n} \left(y_i - \delta - \sum_{j=1}^{p} \kappa_j (N_i - 20)^j - \varphi \cdot d_i - z_i' \gamma \right)^2$$

where p is the maximum degree of the polynomial introduced in the specification, $f(N_i)$ is $\sum_{j=1}^{p} \kappa_j (N_i - 20)^j$, where κ_j is a parameter that quantifies the effect on the outcome of the j^{th} power of the deviation $(N_i - 20)$. In this case the treatment is captured by the parameter $\hat{\varphi}$.[9]

On the other hand, our nonparametric specification is estimated using local linear regressions (see Fan 1992; Hahn, Todd, and van der Klaauw 2001; and Imbens and Lemieux 2008) on both sides of the discontinuity point. Thus, the estimated parameters of this specification are:

$$(\hat{\delta}^+, \widehat{\mu}^+, \hat{\gamma}^+) = \text{argmin}_{(\delta^+,\varphi^+,\gamma^+)} \quad \sum_{i=1}^{n} (y_i - \delta^+ - \mu^+(N_i - 20) - z_i'^+ \gamma^+)^2 K\left(\frac{N_i - 20}{h}\right) I(N_i \geq 20) \tag{3.7}$$

$$(\hat{\delta}^-, \widehat{\mu}^-, \hat{\gamma}^-) = \text{argmin}_{(\delta^-,\varphi^-,\gamma^-)} \quad \sum_{i=1}^{n} (y_i - \delta^- - \mu^-(N_i - 20) - z_i'^- \gamma^-)^2 K\left(\frac{N_i - 20}{h}\right) I(N_i < 20) \tag{3.8}$$

where μ is a parameter that quantifies the effect on the outcome of the deviation $(N_i - 20)$, K is a kernel function and h is the bandwidth. The variable $I(\cdot)$ is an index function which takes

[9] Where $\hat{\varphi}$ is the degree to which the firm passes on childcare costs to its workers.

the value 1 when the condition in the brackets takes place and 0 otherwise. The treatment effect is the difference of the linear predictions at the discontinuity point of the right and left local linear regressions. Hence, the treatment effect for the nonparametric specification will be given by the parameter $\hat{\varphi} = \hat{\delta}^{+} - \hat{\delta}^{-}$.

The kernel function used is the triangular kernel.[10] This is because, as Cheng, Fan, and Marron (1997) demonstrate, the triangular kernel has asymptotic mean squared error minimizing properties for boundary estimation problems.[11] For the selection of the bandwidth, there are two traditional methods: (1) ad hoc methods and (2) data-driven methods such as cross-validation (Ludwig and Miller 2007).[12] We use the data-driven approach, in particular Ludwig and Miller's method (LM), for our baseline estimation.

3.4 Data and Summary Statistics

We use cross-sectional data from the Chilean unemployment insurance system from October 2010, provided by the Ministry of Labour. This database contains information about individuals who are affiliated with this system, either since its origins in October 2002 or since they found a dependent job in the private sector after that date.[13]

Table 1 presents the distribution of female and male workers and firms by their numbers of female workers (fewer than 20 and 20 or more). As outlined above, we see that female workers tend to be concentrated in firms with 20 or more female workers (almost 72% are working in such firms), while the distribution of male workers is relatively homogeneous among these categories. When analyzing the number of firms in both groups, we see that nearly 97% of the firms have fewer than 20 female workers. However, this distribution of firms tends to be inherent to the Chilean economy, in which approximately 90% of the firms have fewer than

[10] Where the triangular kernel is: $K(u) = (1 - |u|)\mathbf{1}_{\{|u| \leq 1\}}$.

[11] Other kernels could also be used; however, the choice of kernel typically has little impact in practice (Lee and Lemieux 2010).

[12] More details are given in the appendix.

[13] This insurance system started in October 2002; currently, more than 94% of dependent workers are affiliated with the system. The unemployment insurance system excludes independent and public sector workers.

20 workers (men and women) according to information provided by the Chilean Ministry of Labour.

Since our main focus is on fertile female workers, we examine the economic sectors in which such women are more concentrated. Table 2 presents the distribution of fertile female workers across different types of industries.[14] As can be seen, nearly 80% of the fertile female workers are concentrated in three types of industries—commerce, financial services, and social services.[15] Hence, we focus on these industries. Given the high dispersion observed in the data, we deleted those individuals earning the highest and lowest 5% of the wages within the sample.

In this section we present the summary statistics of the data set used. Also, in order to give support to the validity of our estimation procedure, we present a graphic analysis of the used variables (as suggested by Imbens and Lemieux [2008]). Table 3 presents the summary statistics for fertile female workers (between 18 and 49 years old), separated by the size of the firm used in our data set.[16] We see that, on average, fertile female workers who are in firms with 20 or more women earn more than their peers who work in firms with fewer than 20 women, and the latter group is older than the former. We also see that fertile women who work in firms with 20 or more female workers tend to be concentrated more in firms that belong to the communal, personal, and social services industry, compared with those who work in firms with fewer than 20 female workers, where a large concentration is observed in the commerce industry.

Tables 4 and 5 present the summary statistics for nonfertile female workers (between 50 and 60 years old) and for male workers, respectively, separated by the size of the firm. We observe that the trend for nonfertile female workers is similar to that for fertile female workers, which also coincides with the trend for men.

Finally, Table 6 presents the summary statistics of the database used in the analysis of employment composition (share of male employment within the firm), which is carried out

[14] Women who work and are between 18 and 49 years old are considered fertile female workers. This definition follows the one provided by the National Institute of Statistics (INE).

[15] According to our data, there are 244,585 nonfertile female workers, 81% of whom are also concentrated in these industries (19% in commerce, 17% in financial services, and 45% in social services).

[16] This separation was based only on the number of female workers; thus, no constraint was imposed on the number of male workers.

by firm and not by worker as before. Following Lemieux and Milligan (2008), we restrict our sample to firms with more than five and fewer than 35 female workers, since there are systematic differences between firms (and their workers) with six to 34 female workers and those with either up to five or more than 35 female workers. We see that firms with 20 or more female workers have a slightly greater proportion of male workers within their labour force and that these firms are more concentrated (relative to the ones with fewer than 20 female workers) in the communal, personal, and social services industry.

3.4.1 Graphical Analysis

When an RD design is used as a method of estimation, previous literature (Imbens and Lemieux 2008; Lee and Lemieux 2010) suggests a series of tests on the variables used. The idea is that these tests allow us to see how robust the internal validity of our design is, and thus how credible our results could be. These tests check:

A. Whether there is a discontinuity on the dependent variables (in our case, wages and share of male workers).

B. Whether there are discontinuities on the control variables (in our case, age and type of industry).

C. Whether there is a discontinuity on the density of the running variable (in our case, the number of female workers in the firm).

Test A should suggest a discontinuity on the variable of interest; otherwise our estimation may conclude that there are no significant effects. Test B is important, as it checks whether covariates present discontinuities. If they do, the causality claimed from the policy change is unclear, since the discontinuity found on the dependent variable may be due to a discontinuity on the covariates and not to the policy change. Finally, test C allows us to check whether agents (in our case, firms and workers) manipulate the running variable. This is important because if there were manipulation (i.e., a discontinuity in the density at the threshold), it would imply that agents just above the threshold are not necessarily similar to those just below the threshold, and this, as Lee and Lemieux (2010) pointed out, would mean that a treatment being a discontinuous function of an assignment variable would not be sufficient to justify the validity of an RD design. Furthermore, discontinuous rules may generate incentives, causing behaviour that would invalidate the RD approach. We check for discontinuities through graphical inspection and formally test for the existence of a discontinuity of the assignment variable by using the test proposed in McCrary (2008).[17]

Test A

A.1. Firms with fertile and nonfertile female workers and male workers:

In Figure 1 we observe that there is discontinuity on wages of female workers in firms with 19 female workers, relative to firms with 20 female workers. Discontinuities on wages are also observed in nonfertile women and in men, as Figure 2 and Figure 3 show. These results suggest that firms transfer the cost of childcare not only to fertile female workers in the form of lower wages, but also to nonfertile female workers and male workers as well. We will explore the magnitude of this transfer below.

Although firms tend to transfer childcare costs to their workers, the data in Figure 4 suggest that firms with 19 female workers have a lower share of male workers than firms with 20 female workers. This result tells us that firms are not fully transferring the childcare cost to their employees, suggesting that there is a change in the relative cost between female and male workers for these firms just above the threshold.

A.2. Firms with male workers only:

To further support our previous results, we apply again test A, but now only to firms with male workers. Since Article 203 of the Labour Code applies only to firms that have female workers, we should expect no discontinuity on those firms with only male workers. Results are presented in Figure 5, and we observe exactly what we were expecting: there are no effects on wages when we move from firms with only 19 male workers to firms with only 20 male workers.

[17] For more information on McCrary's (2008) test, see the appendix.

A.3. Firms with non fertile females only:

To further study our hypothesis, we analyze the behaviour of firms with only nonfertile female workers (ages 50-60). If our hypothesis is true, the firm should not expect any childcare expenditure, and so there should be no discontinuity on wages. Our results are presented in Figure 6 and suggest that, as expected, there is no significant discontinuity at the threshold.

Test B

Our next step is to apply test B on the covariates: age and type of industry dummies. Figure 7 presents the result for age for fertile female workers, and we find that there are no significant differences between both sides of the threshold. Next, in Figures 8, 9, and 10, we present for the same group the result for percentages of fertile female workers working in commerce, financial services, and social services, respectively. We find that there is no significant discontinuity at the threshold.

We repeat the same exercise, but now for nonfertile female workers (Figures 11, 12, 13, and 14) and for men (Figures 15, 16, 17, and 18). Results again suggest no significant discontinuities at the threshold.

We repeat test B for the case where the dependent variable is the share of male workers. Results are presented in Figures 19, 20, and 21 for each type of industry dummy, and suggest no significant discontinuities at the threshold.

Test C

Finally, in Figure 22 we present the result for test C. We observe that there are no significant discontinuities on the density of the running variable at the threshold. This suggests that there is no evidence of manipulation from the agents' point of view. This is crucial, as Lee (2008) formally showed that one need not assume that the RD design isolates treatment variation that is "as good as randomized"; instead, such randomized variation is a consequence of agents' inability to precisely control the assignment variable near the known cutoff.

To further investigate the presence of manipulation of the assignment variable, we follow McCrary (2008), who developed a density test.[18] In particular, he suggests testing the null hypothesis of continuity of the density of the forcing variable at the discontinuity point. Unfortunately, his test was developed for continuous assignment variables. However, as Lemieux and Milligan (2008) pointed out, the discrete nature of the assignment variable does not further complicate the analysis, as it is straightforward to implement this test by separately estimating two local linear regressions (where we consider the fraction and log fraction of women below and above the threshold to be dependent variables) and checking whether there is statistical difference between the predicted outcomes at the discontinuity point.[19] Our results suggest that there is no evidence of manipulation of the assignment variable, which supports our previous graphical analysis. In particular, the p-value is 0.93 for the fraction of women and 0.90 for the log fraction of women.

As mentioned before, the discrete nature of our data can introduce complications in the RD analysis (Lee and Card 2008). However, Lee and Lemieux (2010) point out that the discreteness of the running variable (number of female workers in the firm) does not introduce important complications if this variable is not too coarsely distributed. As Figure 22 and the McCrary test show, this seems to be the case.

Overall, tests A, B, and C support the internal validity of our identification strategy.[20]

3.5 Results

In this section we present the results of our estimation on wages of fertile and nonfertile women and men working for the firm and on the share of male workers. Additionally, we present a sensitivity analysis of our parametric and nonparametric estimates, in order to check their

[18] For more details on McCrary's test, see the appendix.

[19] We use the triangular kernel, as suggested by McCrary (2008). Following Lemieux and Milligan (2008), we use a window of 10 female workers (i.e., from 15 to 25 female workers per firm). The weight of the observations linearly decreases from 1 at the threshold to 0 at 15 or 25 female workers.

[20] Additionally, in line with Lee and Lemieux (2010), we carried out nonparametric discontinuous regressions on the covariates. We did not find any significant discontinuity on the covariates, which supports our previous results.

robustness. In particular, we consider different kernel functions and bandwidths and falsification tests.

3.5.1 Wages

Table 7 presents the results regarding the impact of Article 203 on fertile female workers' wages. In the table, it is possible to observe that wages, on average, decrease because of the policy. The magnitude depends on the specification used (parametric or nonparametric). For the parametric case, we see that the effect varies depending on the degree of the polynomial considered.[21] For the linear polynomial, the effect is an average reduction of nearly -3.9% on monthly wages, whereas the effect is lower for the quadratic and cubic polynomial, -3.4% and -3.8%, respectively. For the quartic polynomial, the reduction is slightly larger than the linear case, -4.2%. We also see that all these estimates are statistically significant at 1%. For the nonparametric case we see that the estimation yields -4.0% (LM), which is also statistically significant at 1%. It is important to mention that even after we consider different polynomial degrees and different approaches (parametric and nonparametric), the results appear to be quite robust.

Tables 8 and 9 present the estimates, through parametric and nonparametric specifications, of the effects of Article 203 on wages for nonfertile female workers and male workers who are in firms along with fertile women, respectively. For nonfertile female workers, we see negative effects ranging from -3.9% to -2.3% for the parametric specification, and a negative effect of -3.8% for the nonparametric specification (LM), but these effects seem to be less robust than those for fertile female workers since the estimates are only statistically significant at 10%. For male workers we also observe negative impacts on wages, where the effect varies from -3.9% to -2.6% in the parametric case and is 4.0% (LM) in the nonparametric one. These results are statistically significant at 1%.

If we consider an average firm with 20 female workers, we see that the reduction of wages due to Article 203 (along with Article 206) is nearly equivalent to the expected childcare cost. Hence,

[21] As Lee and Card (2008) recommend, we use clustered standard errors at the group level in our parametric estimations.

firms pass nearly 90% of the total childcare cost on to their workers. For more details about this calculation, see the appendix. These results suggest that the firm does not discriminate between fertile and nonfertile women and men in order to charge the childcare bill to their workers.

In a competitive labour market, female and male workers who do not have children would be penalized in the above setting; therefore, they would move to firms unaffected by the policy (i.e., those with fewer than 20 female workers) until wages equalize the gains. By observing the data, we find that there are relatively few firms with 20 or more female workers and that nearly 72% of female workers work in them (see Table 1). This seems to suggest that there is an oligopsonistic labour market because, in effect, we observe lower wages for female workers in those firms affected by the policy at the margin (see Manning 2003). This market imperfection may be one explanation for workers' stickiness (immobility).

3.5.2 Employment Composition

Table 10 presents the results of the effect of Article 203 on the share of male workers in the firm. We observe no clear effect on this variable, although its magnitude and significance depends on the specification used. In particular, when we use a quadratic or quartic polynomial, we reach statistical significance at 10%, and the effect on the share of male workers is between 1.6 and 2.2 percentage points. For the nonparametric case, the point estimate is 2.7 (LM) percentage points but is not statistically significant.

These results are along the same lines as those related to wages, where the firm tends to pass almost the entire childcare costs on to its workers, something that does not significantly modify the relative prices between male and female workers. This is because when firms cannot fully transfer the childcare cost to each of their female workers, female workers become a more expensive input relative to male workers. If some degree of substitution exists between them, we should observe an increase in the relative share of male workers (relative to female workers) in the firm. This latter effect would not necessarily be true if firms were also transferring the childcare cost among male workers, which is the case.

3.5.3 Sensitivity Analysis

As Imbens and Lemieux (2008) point out, estimates that are sensitive to the order of the polynomial (in the parametric case) and the kernel or bandwidth specification (in the nonparametric case) are not very credible. In this section, we perform several estimations using different kernel functions, bandwidths, and slopes of the regression functions on both sides of the discontinuity of our parametric specifications, in order to check the robustness of our parametric and nonparametric specification (the sensitivity to a different polynomial order was shown above). Additionally, we perform falsification tests in order to validate our RD design.[22]

Alternative Kernels

The estimates of our nonparametric specifications presented in Tables 7, 8, 9, and 10 consider the triangular kernel. This kernel function has special properties, as shown in Cheng, Fan, and Marron (1997). In particular, this kernel has asymptotic mean squared error minimizing properties for boundary estimation problems. In this section we use other kernel functions, such as the Epanechnikov and Biweight kernels, in order to test the robustness of our nonparametric specification.[23] The results of our estimations using these two kernel functions for the wages of fertile female workers, nonfertile female workers, and male workers and the share of male workers are presented in Tables 11, 12, 13, and 14, respectively. We see that using a different kernel function specification does not affect the estimates to an important magnitude; the estimates using the triangular kernel barely differ from these ones.

We can conclude that the kernel specification chosen does not have an important effect on the estimates of our model. This result is aligned with what the related literature (Imbens and Lemieux [2008] and Lee and Lemieux [2010], for instance) says about conditions of consistent RD estimations.

[22] We performed a sensitivity analysis for the size of the window considered (e.g., firms with more than three and fewer than 37 female workers, or firms with more than seven and fewer than 33 female workers). Our estimates do not vary in a significant way. The results can be obtained upon request from the authors.

[23] The Epanechnikov kernel is $K(u) = \frac{3}{4}(1-u^2)\mathbf{1}_{\{|u|\leq 1\}}$ and the Biweight kernel is $K(u) = \frac{15}{16}(1-u^2)^2\mathbf{1}_{\{|u|\leq 1\}}$.

Alternative Bandwidths

We present estimates using different kernel bandwidths. In particular, we consider a difference of +2, +1, -1, and -2 of the optimal bandwidth calculated according to Ludwig and Miller (2007). The results of our estimations are presented in Table 15.[24] We find that for all outcomes, even after modifying the bandwidths, the estimates appear to be consistent. We do not find important differences in our estimations, which suggests that our RD design is well specified.

Different Slopes on Both Sides of the Discontinuity

The model defined in section 3.2 assumed that the slopes of the regression functions (of our parametric specifications) on each side of the discontinuity were the same, which can be a strong assumption in the case of RD designs. We present a sensitivity analysis for our estimations, considering that these slopes may be different. The parametric model can be redefined as follows:

$$y_i = \delta + \sum_{j=1}^{p} \kappa_j (N_i - 20)^j + \varphi \cdot d_i + \sum_{j=1}^{p} \varsigma_j (N_i - 20)^j \cdot d_i + z_i' \gamma + u_i$$

where the main difference with the specification defined in section 3.2 is the interaction terms $\varsigma_j (N_i - 20)^j \cdot d_i$, which allows for different slopes on both sides of the discontinuity. Some of the results are shown in Table 16. We see, for example, that results for fertile women suggest that considering different slopes does not introduce major alterations of our estimates in comparison with the original ones. Similar results hold for other groups.

Falsification Tests

In this section we present a falsification test. In particular, we estimate our baseline model considering a different threshold, 30 female workers. If the RD design is well specified, then

[24] We consider the triangular kernel for these estimations.

we would expect a lack of statistical significance by the RD estimators. Before estimating, in order to make a valid RD analysis, we perform McCrary's (2008) test for the density of the assignment variable for the new threshold. Results indicate that there is no discontinuity on this variable.

Table 17 presents the results of our falsification test. We see that the estimates are not statistically significant for female (fertile and nonfertile) and male workers. These results show that our RD design performs well, as changing the threshold does not yield statistically significant estimates.

3.6 Conclusion

Previous literature on childcare has focused on two main strands: (i) the effect of childcare policies on cognitive development of the child and (ii) the effects of these type of policies on female labour supply. Until now, there has been no empirical evidence on who bears the financial burden of providing childcare when childcare regulation mandates that firms must provide that service. Thus, we present the first empirical study that analyzes who bears the financial burden of providing childcare. We exploit the discontinuity generated in the Chilean Labour Code by its Article 203, which mandates that firms with 20 or more female workers must provide childcare. We explore its effects on wages and on the share of male workers in the firm using an RD design.

Article 203 theoretically imposes an additional cost on firms, which may result in different outcomes depending on who actually bears the cost (i.e., firms or employees). If firms do not transfer the cost to their workers, we should observe a disincentive to hire female workers for treated firms, through a substitution of women for men, observing a change in employment composition between treated versus untreated firms. If firms can transfer the full cost to their workers, then we should observe lower wages for those affected (in noncompetitive labour markets) or in equilibrium (in competitive labour markets).

Our findings suggest that most of the childcare cost (nearly 90%) is transferred to female workers (fertile and nonfertile) and male workers in the form of lower wages. We also observe that there is no significant change in the employment composition (relative prices between men and women remain unaltered once the threshold of 20 female workers is reached), which is consistent with the fact that firms transfer almost all the cost to their employees.

Overall, although the financial burden of Article 203 is legally imposed on firms, the final agents who carry the burden are the workers of affected firms. This result calls, then, for consideration of the potential unintended consequences of childcare regulations.

3.7 Appendix

Leave-One-Out Cross-Validation Bandwidth (Ludwig and Miller 2007)

The method for choosing the optimal bandwidth within the RD framework is not indisputable. Ludwig and Miller (2007) present an alternative method for choosing the optimal bandwidth, which consists of a "leave-one-out" cross-validation (CV) procedure. Traditional CV procedures may provide misleading results since they do not account for the discontinuity at the threshold and estimate a function in the interior of the support. Ludwig and Miller's (2007) alternative considers two estimations at each side of the threshold, which centers on boundary predictions.

The procedure is the following:

(1) Given a bandwidth h, we run separate regressions on both sides of the threshold, leaving one observation out of the sample, considering only observations that are within this bandwidth (i.e., the threshold minus the value of the running variable is, in absolute value, less than or equal to the bandwidth).

(2) Using the estimates from both regressions, we compute predictions of the dependent variable (at each side of the threshold) for the observation that was left out of the sample.

(3) We compute the difference between the predicted and observed dependent variable.

(4) Repeating this exercise for each observation yields a complete set of differences between the predicted and observed dependent variable. The optimal bandwidth is the one that minimizes the mean square of this difference.

McCrary's (2008) Discontinuity Test

The use of RD designs has become more popular in the last decade. Relatively low complex estimation techniques and relaxed identifying assumptions have made this possible. As Lee (2008) and McCrary (2008) point out, a core assumption of the RD design is the inability to alter the treatment assignment rule by individuals. A clear example of a violation of this assumption is the one presented in McCrary (2008). Suppose a doctor wishes to randomly assign patients a certain drug. To do so, the doctor assigns patients into two waiting rooms, A and B, where those in the first one will receive the drug and the others will receive a placebo. If the treatment assignment rule is known by individuals and they may undo the doctor's assignment, then we would expect room A to be crowded. In this case, because of discontinuities of the assignment variable, the treatment effect estimated by the RD design will probably be far from a precise estimation, as Lee (2008) formally shows that if there were manipulation of this variable, then there could be identification problems of the treatment effect.

McCrary (2008) proposes a formal test in order to analyze whether there are discontinuities, at the cutoff, in the assignment variable. This test consists of two steps. First, construct a detailed histogram grid of the assignment variable. Second, using local linear regressions, smooth the histogram on both sides of the cutoff of the assignment variable and test whether there is a difference in the density on both sides (of the cutoff). This applies for the case of a continuous assignment variable.

In the case of a discrete assignment variable, like the one in this article (number of female workers), McCrary's (2008) test can also be applied. As Lemieux and Milligan (2008) show, it is necessary to run local linear regressions on both sides of the cutoff and test whether the predicted outcome (fraction or log fraction of the assignment variable in the bins) of both sides is the same.

Calculations of Childcare Cost pass-through on to Workers

In this section we present the calculations of the childcare cost transferred to workers by the firm, based on our RD estimates. According to our database, in firms that belong to the commerce, financial services, or social services industries and that employ 19 female workers, the average monthly wage for fertile female workers is $378,047 CLP (Chilean pesos), $415,575 CLP for nonfertile female workers, and $443,476 CLP for male workers.[25] The average firm with 19 female workers has 17 fertile female workers, 2 nonfertile female workers, and 25 male workers. Considering a simple average of the parametric effects of Article 203 on wages and that the next female worker that the firm will hire is fertile, we find that the total monthly penalization on wages is $628,044 CLP. The following table summarizes these calculations:

Table: Cost Transfer Calculations

Type of Worker	Average Wage (CLP)	Number in a Firm With 20 Females[26]	RD Effect	Cost Transfer (CLP)
Fertile Female	$378,047	18*	-3.8%	$258,584
Non Fertile Female	$415,575	2	-3.1%	$25,766
Male	$443,476	25	-3.1%	$343,694
Total				$628,044

Note: We consider the average number of workers in a firm with 19 female workers and that the next female hired is fertile. *We assume that the 20^{th} female worker hired is a fertile one and hence the original number of fertile female workers is 17. The Regression Discontinuity (RD) effect considers a simple average of the estimated parametric effects.

According to the CASEN 2009 Survey, 13.9% of working fertile women have a child between 6 and 24 months old and hence are eligible for childcare provided by the employer.[27] Thus, nearly 2.5 fertile female workers of the firm will require the childcare service.[28] The monthly

[25] As of early May 2012, US$1 is nearly $500 CLP.

[26] We consider the average number of workers in a firm with 19 female workers and that the next female hired is fertile.

[27] This data includes fertile female dependent workers from the private sector.

[28] This result is obtained by multiplying 0.139 (13.9% is the probability of having a child between 6 and 24 months old) by 18 (the number of fertile female workers).

cost of childcare is variable. Public childcare (through JUNJI) costs nearly $165,000 CLP and private childcare costs $100,000 to $300,000, with an average that is near the public cost. Hence, the expected childcare cost for the employer is $412,500 (CLP).[29]

However, as stated in Articles 203 and 206, other type of expenditures must be paid by the employer. In particular, travel costs to the childcare facility, time spent travelling back and forth from the firm to the childcare facility, and time granted to the female worker for feeding her child are indirect costs. When this database was created (October 2010), one trip on public transportation cost $500 CLP.[30] Hence, the monthly cost of transportation that the employer has to pay for each mother is $20,000 CLP.[31] The cost associated with productivity losses for the firm due to the time the mother spends feeding her child (1 hour) can be calculated as a fraction of monthly wages. This cost is approximately $47,256 CLP, one-eighth of the daily wage.[32] In the case of the time travelled, we assume that it takes the mother 1 hour a day to get from the firm to the childcare facility and back. Thus, the cost is nearly $47,256 CLP. All the indirect costs (considering the fertile women who will require childcare) add up to $286,280 CLP.[33]

A summary of the total costs for the employer due to Articles 203 and 206 is presented in the following table:

Table: Total Costs due to Articles 203 and 206

Item	**Cost (CLP)**
Childcare	$412,500
Transport	$50,000
Productivity Loss	$236,280
Total	$698,760

[29] This result is obtained by multiplying 2.5 (the number of female workers who will require childcare) by $165,000 CLP (the average childcare cost).

[30] This is the cost of one ticket on Santiago's public transportation system, Transantiago.

[31] Assuming that women must travel to the childcare facility twice a day, we multiply $500 (the cost of one trip on public transportation) by 2 (number of trips per day) by 20 (the average number of working days in the month), which equals $20,000.

[32] Eight hours are the normal working daily hours.

[33] Our analysis is incomplete, in this point, since we do not know the number of firms that have childcare facilities within them or the exact travel times.

Note: The average number of female workers that will require childcare is considered.

We see that on average, the employer transfers to her workers approximately 90% of the total childcare costs.

Tables

Table 1: Distribution of Workers and Firms by Number of Female Workers

Type of Firm	Female Workers	Male Workers	Firms
	(Number)	(Number)	(Number)
Less than 20 Female workers	475,234 (28.1%)	1,430,388 (50.6%)	287,136 (96.8%)
20 or More Female workers	1,217,994 (71.9%)	1,391,281 (49.4%)	9,358 (3.2%)
Total	**1,693,228**	**2,821,669**	**296,494**

Note: Percentages are presented in parenthesis.

Table 2: Distribution of Fertile Female Workers by Type of Industry

Type of Industry	Female Workers	% of the Total
Agriculture, Hunting, Forestry and Fishery	61,333	4.8%
Mines and Quarry	9,592	0.8%
Industry (Manufacturing)	95,801	7.5%
Electricity, Gas and Water	3,585	0.3%
Construction	34,884	2.7%
Commerce	288,208	22.6%
Transport, Storage and Communications	53,960	4.2%
Financial and Business Services	268,824	21.0%
Communal, Personal and Social Services	461,526	36.1%
Total	**1,277,713**	**100%**

Note: Not all female workers in the database present type of industry.

Table 3: Descriptive Statistics for Fertile Female Workers

Variable	Less than 20 Female Workers	More than 20 Female Workers
Log Wage	12.4	12.5
	(0.57)	(0.65)
Age	34.1	33.3
	(8.3)	(8.2)
Commerce	0.43	0.24
	(0.49)	(0.43)
Financial and Business Services	0.25	0.25
	(0.43)	(0.43)
Communal, Personal and	0.30	0.50
Social Services	(0.46)	(0.50)
Number of Observations	**226,258**	**690,308**

Note: Mean of the variables is presented. Standard deviations in parentheses.

Table 4: Descriptive Statistics for Non Fertile Female Workers

Variable	Less than 20 Female Workers	More than 20 Female Workers
Log Wage	12.3	12.6
	(0.55)	(0.67)
Age	54.8	54.7
	(3.77)	(3.70)
Commerce	0.41	0.15
	(0.49)	(0.35)
Financial and Business Services	0.24	0.18
	(0.43)	(0.38)
Communal, Personal and	0.33	0.66
Social Services	(0.47)	(0.47)
Number of Observations	**44,195**	**113,145**

Note: Mean of the variables is presented. Standard deviations in parentheses.

Table 5: Descriptive Statistics for Male Workers

Variable	Less than 20 Female Workers	More than 20 Female Workers
Log Wage	12.6	12.8
	(0.57)	(0.64)
Age	39.3	37.2
	(11.7)	(11.7)
Commerce	0.41	0.28
	(0.49)	(0.45)
Financial and Business Services	0.35	0.31
	(0.47)	(0.46)
Communal, Personal and	0.23	0.40
Social Services	(0.42)	(0.49)
Number of Observations	**527,968**	**672,157**

Note: Mean of the variables is presented. Standard deviations in parentheses.

Table 6: Descriptive Statistics for Firms

Variables	**Less than 20 Female Workers**	**More than 20 Female Workers**
Share of Male Workers (%)	0.40	0.41
	(0.27)	(0.23)
Commerce	0.42	0.30
	(0.49)	(0.46)
Financial and Business Services	0.25	0.21
	(0.43)	(0.41)
Communal, Personal and	0.33	0.49
Social Services	(0.47)	(0.50)
Number of Observations	**14,349**	**2,637**

Note: Mean of the variables is presented. Standard deviations in parentheses.

Table 7: Impact of Article 203 on Fertile Females' Log Wages

Specification	Estimate
Parametric	
Linear	-0.039^{***}
Quadratic	-0.034^{***}
Cubic	-0.038^{***}
Quartic	-0.042^{***}
Nonparametric LM	-0.040^{***}

Note: ***, ** and * represent statistical significance at 1%, 5% and 10%, respectively. In case of the parametric specification clustered standard errors at the group level were used. For the nonparametric case the triangular kernel is used and the optimal bandwidth, chosen following Ludwig and Miller (2007), is $h^{*}= 14$.

Table 8: Impact of Article 203 on Non Fertile Females' Log Wages

Specification	Estimate
Parametric	
Linear	-0.027^{*}
Quadratic	-0.023^{*}
Cubic	-0.035^{*}
Quartic	-0.039^{*}
Nonparametric LM	-0.038^{**}

Note: ***, ** and * represent statistical significance at 1%, 5% and 10%, respectively. In case of the parametric specification clustered standard errors at the group level were used. For the nonparametric case the triangular kernel is used and the optimal bandwidth, chosen following Ludwig and Miller (2007), is $h^{*}= 14$.

Table 9: Impact of Article 203 on Males' Log Wages

Specification	Estimate
Parametric	
Linear	-0.039^{***}
Quadratic	-0.028^{***}
Cubic	-0.029^{***}
Quartic	-0.026^{***}
Nonparametric LM	-0.040^{***}

Note: ***, ** and * represent statistical significance at 1%, 5% and 10%, respectively. In case of the parametric specification clustered standard errors at the group level were used. For the nonparametric case the triangular kernel is used and the optimal bandwidth, chosen following Ludwig and Miller (2007), is $h^{*}= 14$.

Table 10: Impact of Article 203 on the Share of Male Workers in the Firm

Specification	Estimate
Parametric	
Linear	0.006
Quadratic	0.016^{*}
Cubic	0.014
Quartic	0.022^{*}
Nonparametric LM	0.027

Note: ***, ** and * represent statistical significance at 1%, 5% and 10%, respectively. In case of the parametric specification clustered standard errors at the group level were used. For the nonparametric case the triangular kernel is used and the optimal bandwidth, chosen following Ludwig and Miller (2007), is $h^{*}= 5$.

Table 11: Impact of Article 203 on Fertile Females' Log Wages (Alternative Kernels)

Kernel	Bandwidth Method	Effect
Epanechnikov	LM	-0.039^{***}
Biweight	LM	-0.040^{***}

Note: ***, ** and * represent statistical significance at 1%, 5% and 10%, respectively. The optimal bandwidth, for the Epanechnikov kernel and for the Biweight kernel is $h^{*}= 14$ (using Ludwig and Miller (2007) approach).

Table 12: Impact of Article 203 on Non Fertile Females' Log Wages (Alternative Kernels)

Kernel	Bandwidth Method	Effect
Epanechnikov	LM	-0.039^{**}
Biweight	LM	-0.036^{**}

Note: ***, ** and * represent statistical significance at 1%, 5% and 10%, respectively. The optimal bandwidth, for the Epanechnikov kernel and for the Biweight kernel is $h^{*}= 14$ (using Ludwig and Miller (2007) approach).

Table 13: Impact of Article 203 on Males' Log Wages (Alternative Kernels)

Kernel	Bandwidth Method	Effect
Epanechnikov	LM	-0.031***
Biweight	LM	-0.038***

Note: ***, ** and * represent statistical significance at 1%, 5% and 10%, respectively. The optimal bandwidth, for the Epanechnikov kernel and for the Biweight kernel is $h^{*}= 14$ (using Ludwig and Miller (2007) approach).

Table 14: Impact of Article 203 on the Share of Male Workers in the Firm (Alternative Kernels)

Kernel	Bandwidth Method	Effect
Epanechnikov	LM	0.027
Biweight	LM	0.027

Note: ***, ** and * represent statistical significance at 1%, 5% and 10%, respectively. The optimal bandwidth, for the Epanechnikov kernel and for the Biweight kernel is $h^{*}= 5$ (using Ludwig and Miller (2007) approach).

Table 15: Impact of Article 203 on Different Outcomes (Alternative Bandwidths)

	Difference with Optimal Bandwidth			
Outcome	+2	+1	-1	-2
Fertile Females (Wages)	-0.042***	-0.040***	-0.040***	-0.041***
Non Fertile Females (Wages)	-0.040***	-0.039***	-0.036**	-0.035**
Males (Wages)	-0.038***	-0.039***	-0.042***	-0.046***
Share of Male Workers	0.022	0.024	0.026	0.037

Note: ***, ** and * represent statistical significance at 1%, 5% and 10%, respectively. The optimal bandwidth was chosen following Ludwig and Miller (2007).

Table 16: Impact of Article 203 on Different Outcomes (Different Slopes on Both Sides of the Discontinuity Allowed)

Polynomial	F. Females (Wages)	Share of Male Workers
Linear	-0.037***	0.014
Quadratic	-0.039***	0.019
Cubic	-0.041***	0.026

Note: ***, ** and * represent statistical significance at 1%, 5% and 10%, respectively.

Table 17: Falsification Test: Threshold at 30 Female Workers

Outcome	Estimate
Fertile Females (Wages)	-0.012
Non Fertile Females (Wages)	-0.007
Males (Wages)	-0.001
Share of Male Workers	-0.005

Note: ***, ** and * represent statistical significance at 1%, 5% and 10%, respectively.

Figure 1: Log Wages (Mean) of Fertile Female Workers by Number of Female Workers.

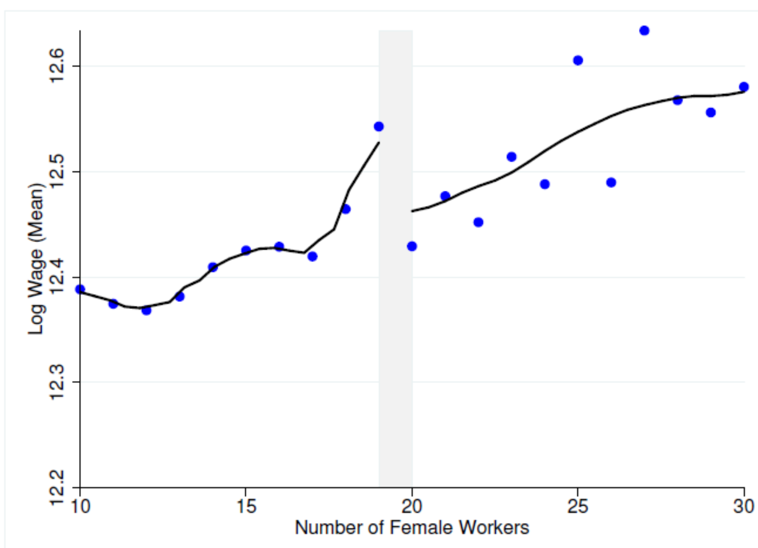


Figure 2: Log Wages (Mean) of Non Fertile Female Workers by Number of Female Workers.

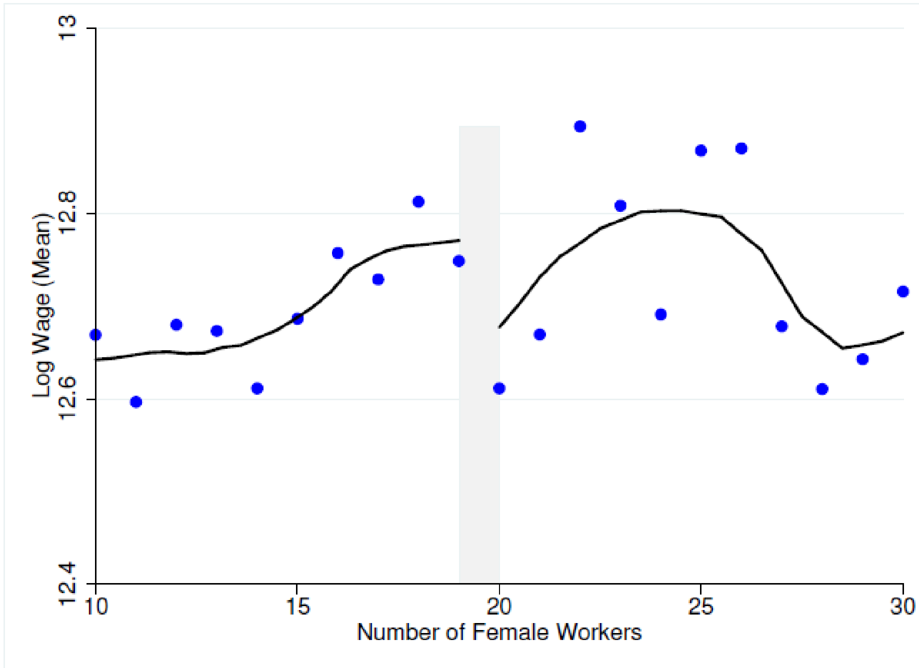


Figure 3: Log Wages (Mean) of Male Workers by Number of Female Workers.

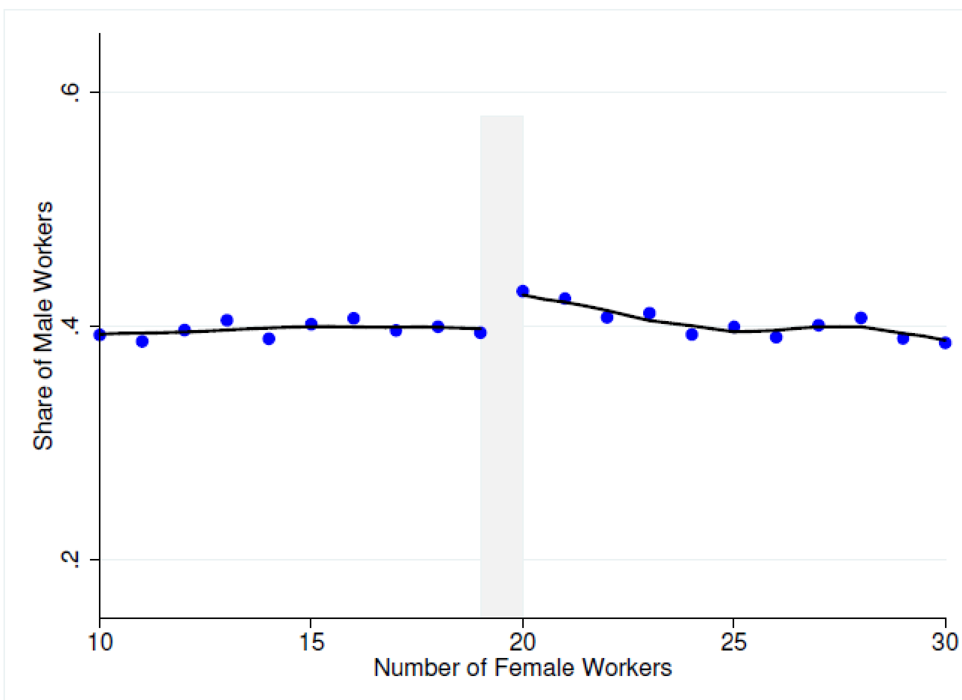


Figure 4: Share of Male Workers by Number of Female Workers.

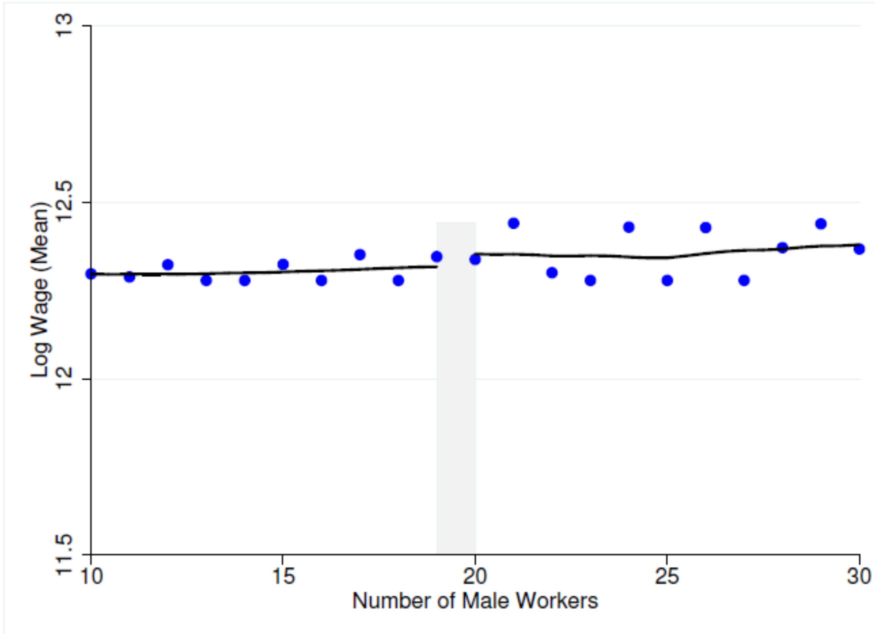


Figure 5: Log Wages (Mean) of Male Workers by Number of Male Workers.

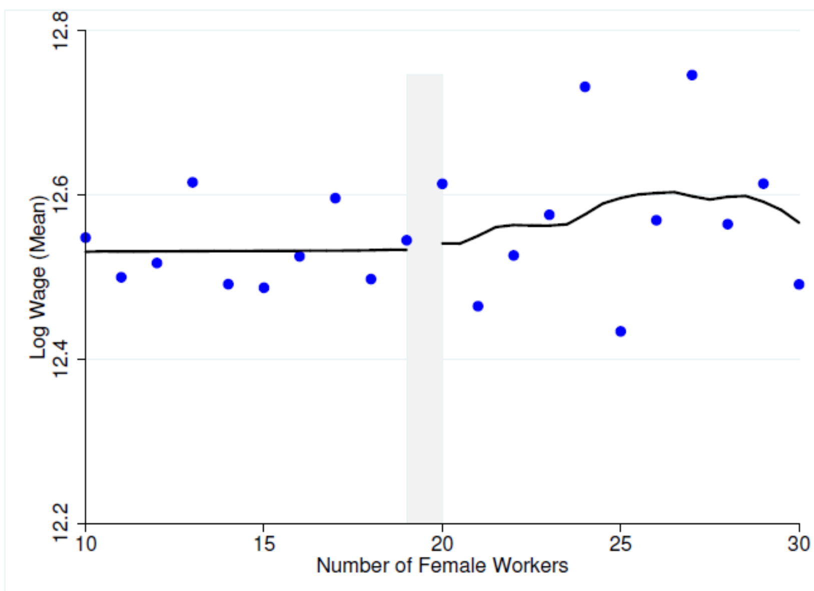


Figure 6: Log Wages (Mean) of Non Fertile Female Workers by Number of Non Fertile Female Workers.

Figure 7: Fertile Female Workers' Average Age by Number of Female Workers.

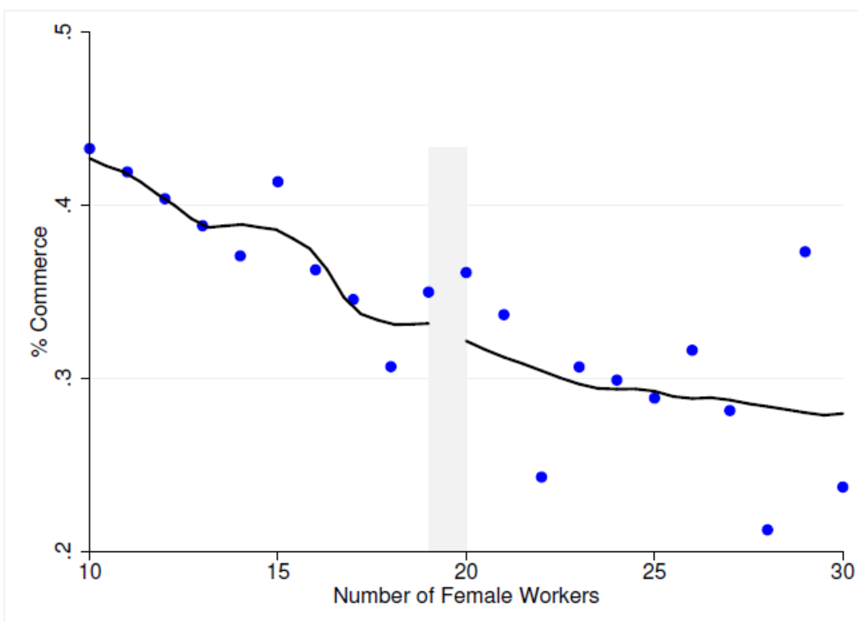


Figure 8: % Commerce of Fertile Female Workers by Number of Female Workers.

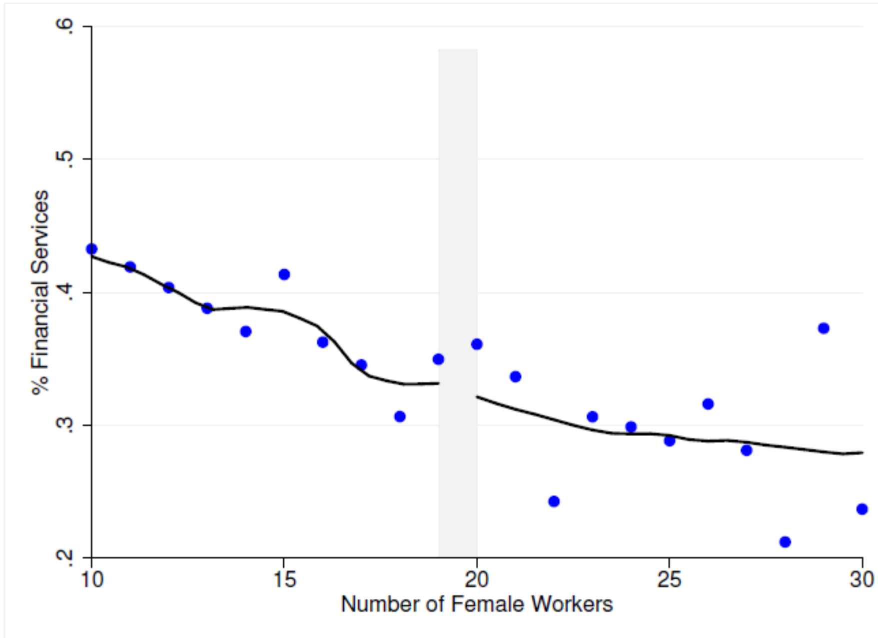


Figure 9: % Financial Services of Fertile Female Workers by Number of Female Workers.

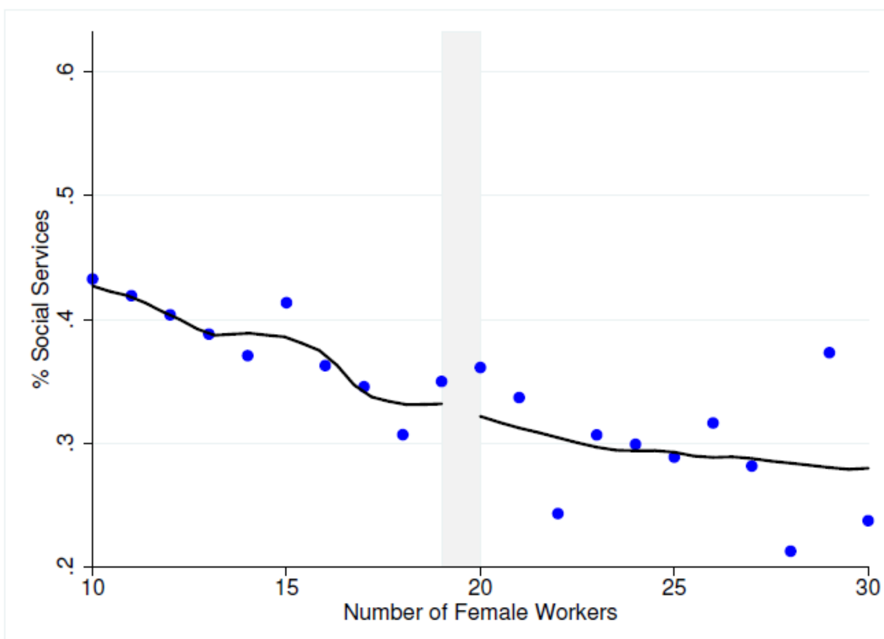


Figure 10: % Social Services of Fertile Female Workers by Number of Female Workers.

Figure 11: Non Fertile Female Workers' Average Age by Number of Female Workers.

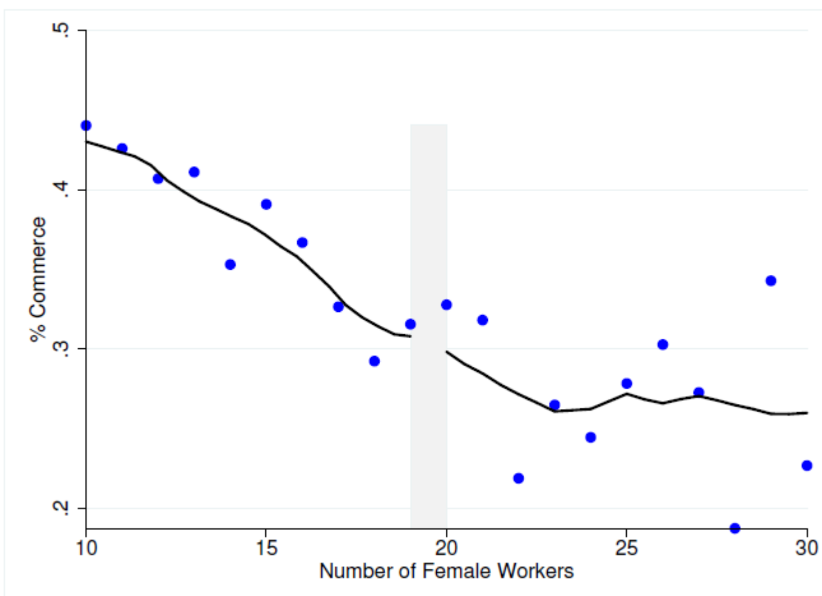


Figure 12: % Commerce of Non Fertile Female Workers by Number of Female Workers.

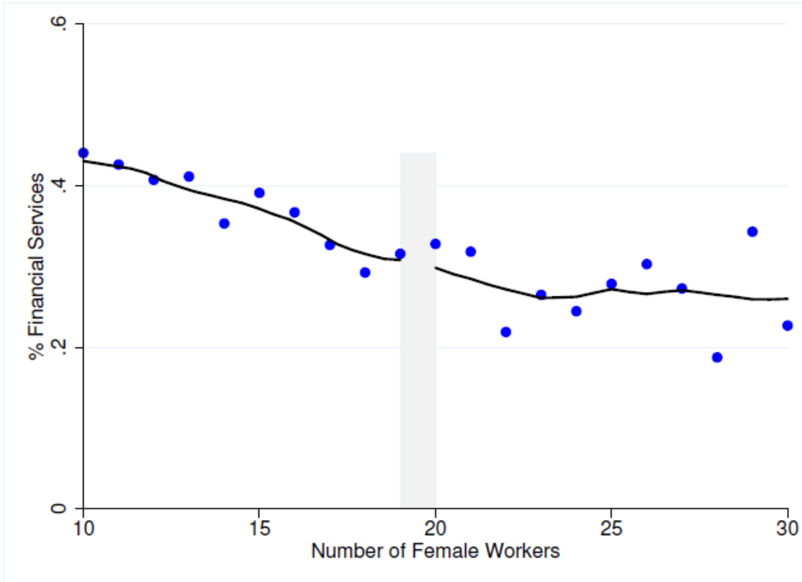


Figure 13: % Financial Services of Non Fertile Female Workers by Number of Female Workers.

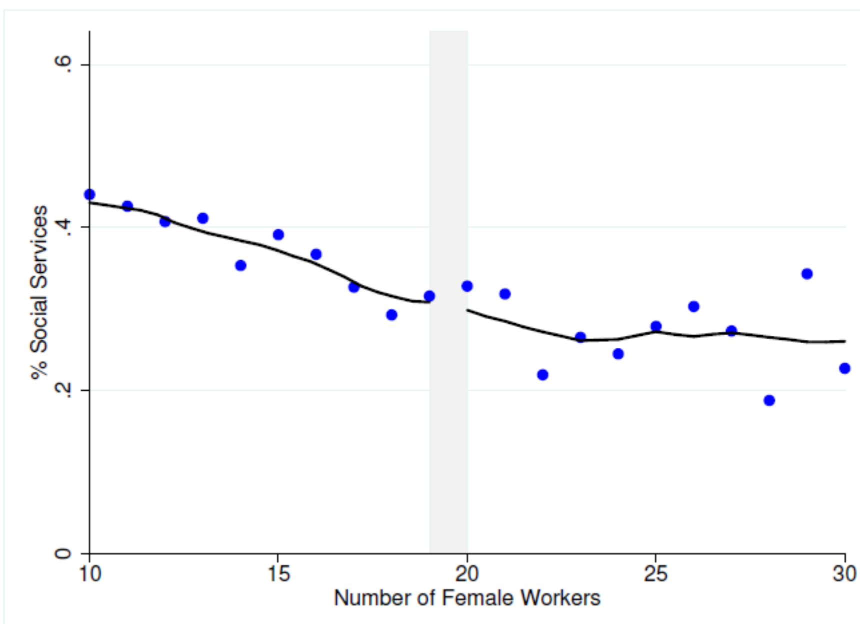


Figure 14: % Social Services of Non Fertile Female Workers by Number of Female Workers.

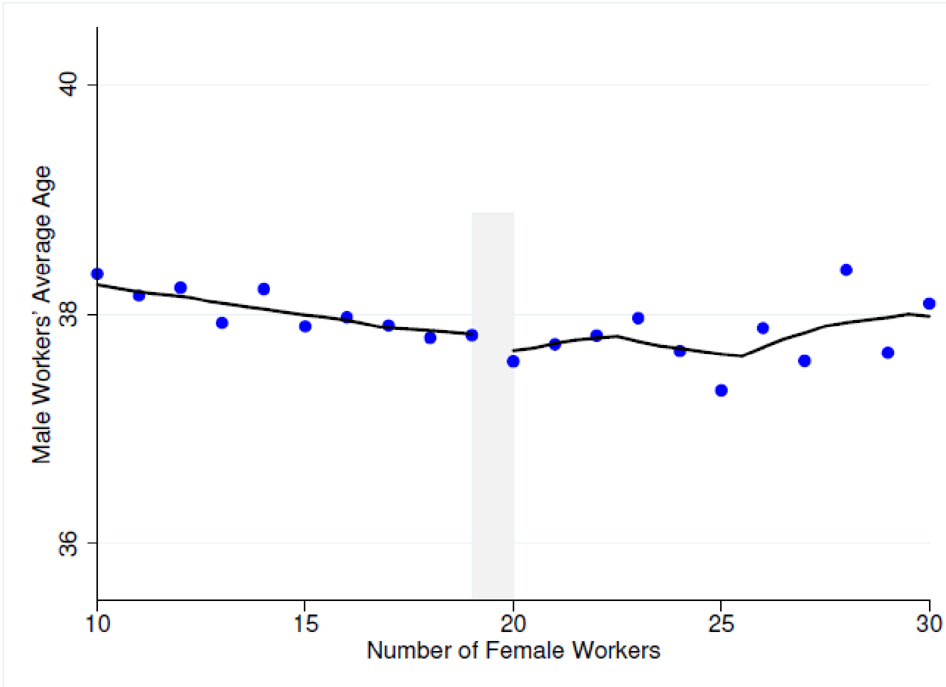


Figure 15: Male Workers' Average Age by Number of Female Workers.

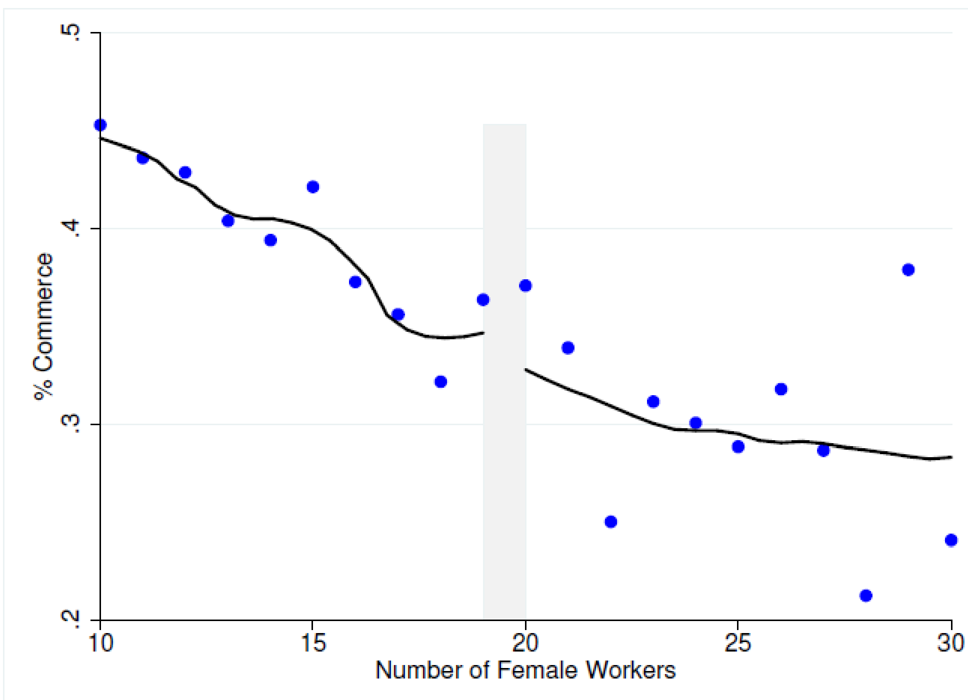


Figure 16: % Commerce of Male Workers by Number of Female Workers.

Figure 17: % Financial Services of Male Workers by Number of Female Workers.

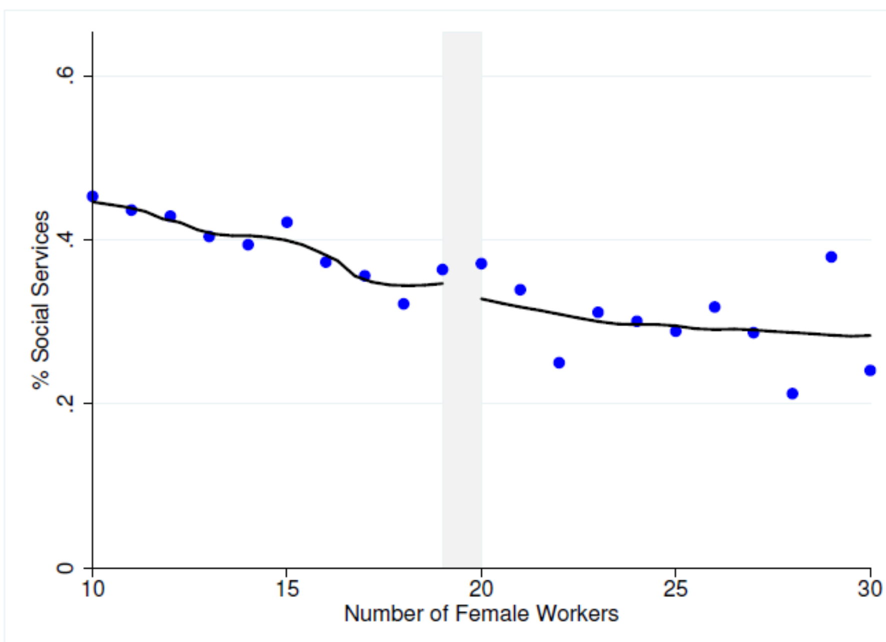


Figure 18: % Social Services of Male Workers by Number of Female Workers.

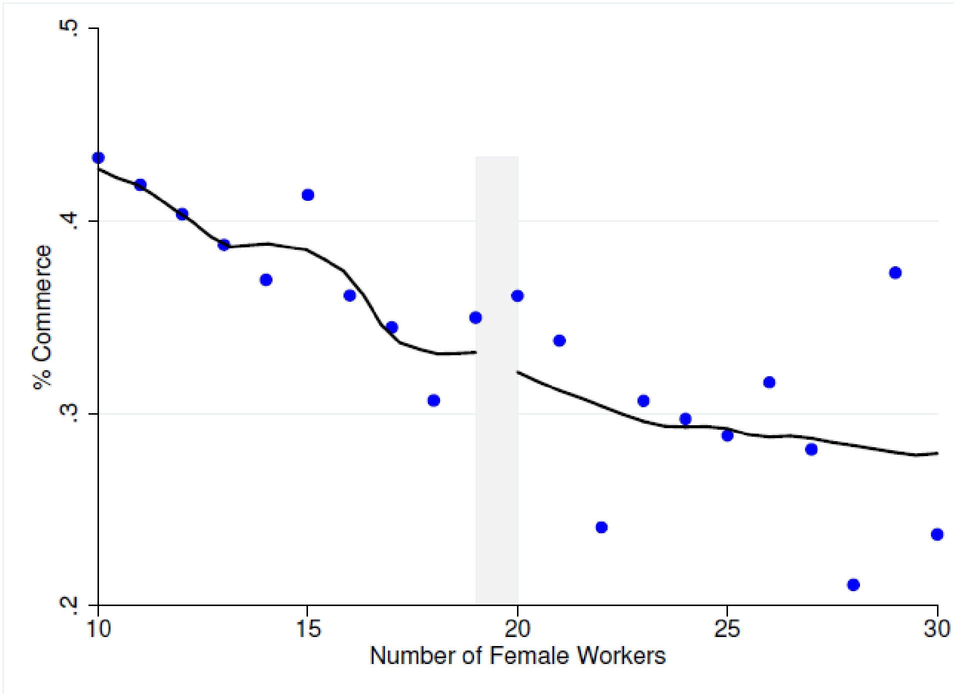


Figure 19: % Commerce by Number of Female Workers (Firms).

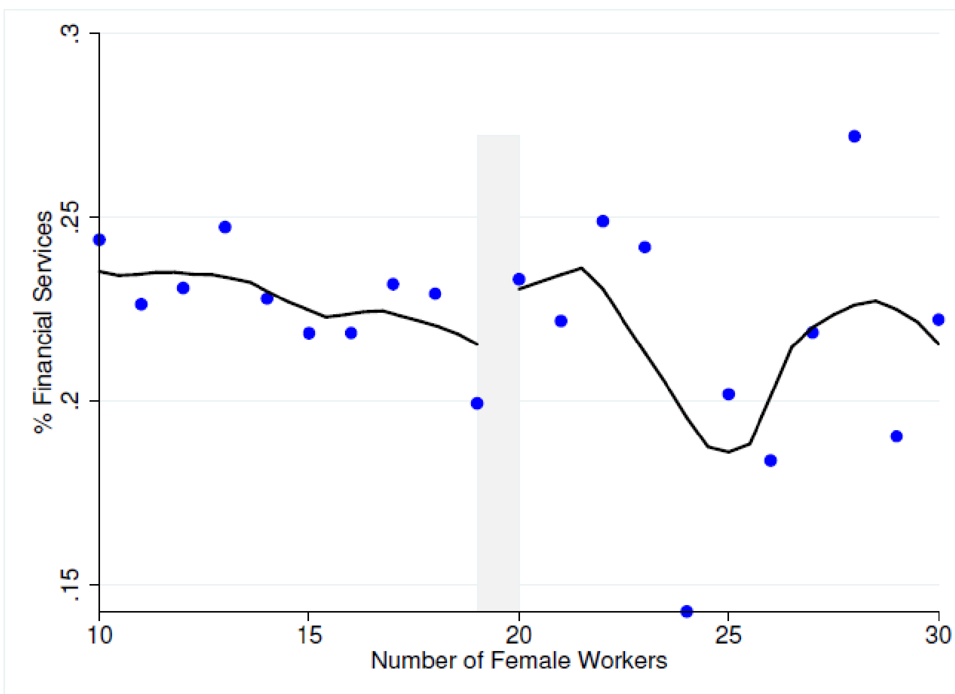


Figure 20: % Financial Services by Number of Female Workers (Firms).

Figure 21: % Social Services by Number of Female Workers (Firms).

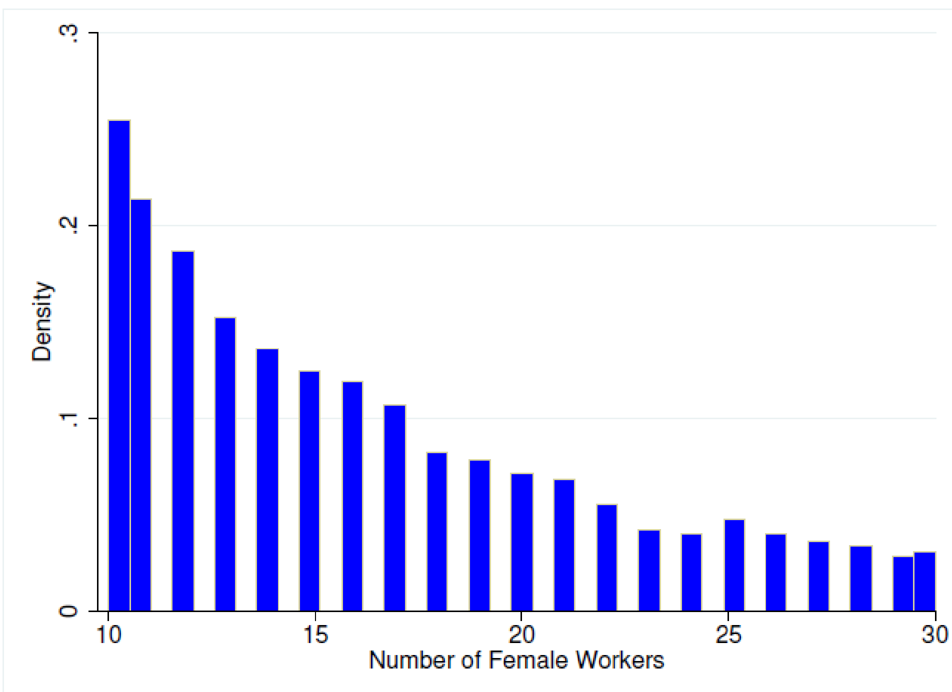


Figure 22: Density of the Number of Female Workers.

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