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THE LABOR IMPACT OF MINIMUM WAGES: A METHOD FOR ESTIMATING THE EFFECT IN EMERGING ECONOMIES USING CHILEAN PANEL DATA

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Abstract

We develop and use a statistical matching technique to construct a panel data set from the Chilean National Employment Survey; and then use this panel to test the short term impacts of minimum wage increases during the 1996-2005 period. We estimate wage increase effects for the treated group (people earning wages between ex ante an ex post minimum wages), the hours worked and the employment effects for this group. We also estimate the effect on the probability of obtaining a job for a theoretical treated group of unemployed and inactive workers constructed by estimating their likely wage in the case that they found one. We then estimate the integral of these three effects (wage increase, wage loss and lower probability of obtaining a job). We find that minimum wage increases do have a significant impact on the wages of the treated group, hence the suspicion that they are somehow made irrelevant by informal practices in Chilean labor markets seems to be unfunded. We find that there is a significant negative effect on the probability of staying employed, a negative effect on hours worked and a significant negative effect on the probability of finding the job. We find that the integral of the three effects is positive and has statistical significance. We conclude that, in general, minimum wage increases in Chile during the aforementioned period have increased the real income of treated and potentially treated workers. However, we also find that there is a redistribution of income among these workers in favor of currently employed workers. We submit our results to several robustness checks including a variety of definitions of income and wages, a continuous changing control group, a "dif-in-dif" approach and a pressure and distance.[a]pproach. We conclude that, if anything, minimum wage increases have generated real income redistribution towards the treated workers as well as among them in Chile.

The usual disclaimer applies. We are indebted to Tomás Rau , Dante Contreras and David Card forcritical advice, and to the MicroData Center of the University of Chile for allowing us to present the resultswithin their workshops and to use their computational infrastructure for statistical simulations. We are grateful for the help provided by the National Statistics Bureau (Instituto Nacional de Estadísticas, INE) with the construction of the panel, particularly to its former director Mariana Schkolnick and Bartolomé Payeras.

1 Introduction

At first sight it could seem surprising that the minimum wage (MW) continues to be at the center of the public debate in many countries. However, a quick review of the available literature shows that although theoretically the effects of this public policy instrument may be relatively well understood, empirically this continues to be an open question. The variety of results could indicate many things, from the possibility that MW laws have heterogeneous effects across markets, through the possibility that the MW has statistically significant but economically irrelevant effects to the possibility that the major effects of these laws are just difficult to identify (e.g. because of their dynamic nature). In any case, it seems clear that a transversal reason for all the difficulties is the lack of appropriate databases for this analysis, especially in the case of emerging markets. This generates the very obvious problem that however good the estimation of MW effects could be for a developed country database, it's applicability for emerging economies can always be questioned on the basis of structural differences that are expressed in different comparative advantages.

The objective of this paper is to develop a methodology for measurement of the impact effects of the MW that is generally aplicable for emerging economies. We develop this methodology using the Chilean national Employment Survey that has the virtue of being sufficiently common in it's surveying and sampling techniques to make our methods applicable in a large amount of countries. In applying this technique to Chilean data we also characterize and properly measure the effects and dimensions of MW laws for the particular case of the Chilean economy.

We believe that an adequate measurement can help us dimension not only the statistical significance but the economic relevance of this debate. it is very clear that the MW discussion serves a public purpose that exceeds its strict technical scope. This is probably a result of it being a visible representation of the different policy approaches available in modern democracies. There is a potential that a "statistically blind" theoretical discussion,that may be useful in politics, may be debating over policy issues that are not as crucial or dramatic as expected. It could be that some points of view are placing to much faith on the MW as an instrument for improving income inequality, while others are attributing to much of the volatility of the unemployment rate to its existence and variations. The risk of relying solely on a theoretical discussion is that it may consume policy attention on economically insignificant issues or on policy instruments that have very slight effects. Also the policy debate has usually a more broader objective than theoretical discussions, and is inserted in a political process which has (as everything must) constraints that limit the amount of available political effort and the number of policy issues that can be discussed rigorously. Hence, it is important for policy makers to have available calibrations of the effects to compare with other policy alternatives.

To develop this methodology we test the theoretical consequences of minimum wage implementations on wages, employment and unemployment and hours worked for the Chilean labor market. This will alow us to measure the effect of the MW on income distribution and employment. The usual problem with this research agenda all around the world has been the availability of databases that are adequate for providing rigorous answers to this question. This has driven a number of authors in several countries to use innovative empirical approaches to overcome data limitations in this area. Many of the elasticities available in this area of research are a result of these innovative papers. In our case we are able to take advantage of a database that allows us to construct a panel of workers for almost a decade. Our empirical strategy allows us to estimate elasticities that can be compared with those estimated (albeit usually from very different empirical approaches) in other countries and on other data sets. We believe this is a significant step forward in understanding the effects of MW laws on the labor market of emerging markets

To measure the impact of the MW on the labor market we need, primarily, individual data to identify who is going to be affected by the increase in the minimum wage. Secondly, it is important to have data for the same person before and after the change in the MW, in that case we can really compare the performance of the treatment group with the control group performance. Obviously, the success of our estimation relies on the equivalence of the control group respect the treatment group, which will never be perfect. One of the most important contributions of our paper is that the construction of a labor panel and its length allows us to get close to this ideal in a way that is replicable for other emerging countries.

The interest of this paper is also local. In Chile, as in many emerging countries, we have a spirited ideological debate about labor topics, and particularly about the MW. This is normal in modern capitalist democracies, but is particularly abstract in emerging countries as a result of the absence of the types of data bases that we need to address the issue properly. In spite of international evidence of the MW impact and the vibrant academic community in Chile there is a strong common sense that most of the volatility of unemployment is ultimately caused by the MW. On the other hand there is important political sectors that assume that the MW is a very effective distributive tool and lobby very intensively for it. Our intention, in addition to developing this internationally comparable methodology, is to provide some calibration to this local debate.

Finally, the evidence found in this study could be interesting for researchers and policy makers in other countries that are considering the effects of minimum wage laws. Different countries have different labor laws and institutions (particularly relevant for the subject: different degrees of institutional and legal tolerance of informality), human capital levels and types are different, productive sector structures are different and labor relations have different historical backgrounds. Hence, comparing evidence across countries could provide thought food for the formulation of hypotheses on the effects of different institutional arrangements in the context of MW laws.

There are a couple of things that the paper does not do, that must be clearly stated. First, the paper only estimates the short term "impact" of minimum wage laws. That is, the impact within a year. It is entirely possible that the existence of minimum wage policies of different degrees of generosity could contribute to medium term business confidence and investor willingness. We have no evidence whatsoever to prove that this effect exists or not and no information that would allow us to calibrate it. Second – the other side of this same argument – we have no way of calibrating the medium term effect of MW laws on labor participation or training and hence on wage spreads and income distribution. It is entirely possible that credible and effective wage policies can foster participation in the labor market. We are unable to measure this effect. Third, the paper has no way of addressing the political effects of MW laws. It is entirely possible that the existence of a MW policy generates stability within a particular political economy, by clearly stating what are the adequate pay levels that are considered acceptable and leaving lower levels in the scope of private decisions. This is plausible, but clearly out of our reach. Fourth, the paper provides no insight into the mechanisms of the economic effects of minimum wage laws that we find. We cannot test if they are a result of legal enforcing by public officials, crowding out effects by public salaries, or rather the result of an expectations formation process that is detonated by the "signal". Fifth, the paper only estimates the effect on the income of the affected workers and does not provide a welfare analysis. As we shall see, there are theoretically expected distributive effects towards and among workers that are combined. It will not be possible to provide a welfare analysis without assuming arbitrary social loss functions. Hence, unfortunately the paper does not provide a definite answer on whether "minimum wage" laws are "good" or "bad", just an estimation of what their impacts are that leaves the welfare discussion open but, we hope, slightly more calibrated.

We find that MW policy has, if anything, helped to compress the wage and income structure but has also generated some redistributions among treated workers. Theses results are consistent with the existing literature. Looking at our preferred specification we can conclude that the effects are more robust on labor income and probability of keeping the job (so there are winners and losers), than for hours worked and the probability of finding a job for unemployment and inactive people. Important for the Chilean debate, from our best strategy of identification -the Diff and Diff estimations- we can think that the big MW increases in the late 90's did contribute to a rise in unemployment, although the magnitude is much smaller than what is usually attributed, the effect is in the order of $(0 - 8\%) * (5\%) \leq 0.32\%)$ per year.[1] Given this estimation, we cannot attribute most of the variance of unemployment to the minimum wage. Finally, when we calculate the total effects (integrating the negative and positive effects) we ¯nd that they are positive or not significant. Hence, we conclude, there are level and also distribution effects of the minimum wage, that go in the theoretically expected direction but are not as meaningful as they seem to be from the public debate.

The paper is organized as follows. Section 2 briefly surveys the literatures on estimation of the impact of the minimum wage, with the objective of situating this paper in the literature. Section 3 describes the origin of the panel data that we use, it's virtues and limitations. Section 4 describes the empirical strategy that we will implement to estimate the different effects of the MW in Chile. Section 5 shows the results of the estimations

[1] In our diff and diff estimation the upper confidence interval, with a 95%, is less than 8%, depending on the control group this could be not significative. Moreover, the treatment group represents around 5% of the labor force.

and the robustness checks. Section 6 attempts to conclude by extracting stylized findings and discussing implications for future research agendas.

2 A Short Survey

The mainstream view from economists is that the MW is an effective policy instrument only when there are "enclave economies" or segmented labor markets where a monopsonistic demander can exploit workers by paying them less than the value of their marginal productivity. In this case, the MW can be an effective instrument in preserving the income of workers and will also increase total employment. If this is not so and labor markets are competitive, then the MW has the potential of generating unemployment or informality. The argument is that the geographical segmentation of labor markets has been diminished by the communications and transport revolutions of the last six decades, and hence, MWs tend to be a distorting influence on largely competitive labor markets. The other side argues that modern labor market segmentations are not in fact a result of geography and high transport costs (as in the traditional "enclave economies" of the XIXth Century) but rather of specialized sector-specific training skills, that limit the speed at which workers can churn and limit substantially the pool of jobs that low productivity workers can access. The response is that even if the economy was a collection of "skill enclaves", there is no reason to believe that they should all have the same competitive equilibrium price. Hence, the policy of setting "correct" MW for all enclaves is close to impossible. And so the argument continues.

On one hand, the standard theory predicts that in a competitive context an artificial rising in the wages, created by the MW, induces an increase in the unemployment (Stigler (1946)). On the other hand, there is a wide range of publications that, relaxing in some way the efficient markets assumption, have the opposite predictions. Maybe the best known of theses theories is the effect of the MW in a monopsony. As Maurice (1974) pointed out in this industrial environment a rising in the MW has a positive effects on employment and wages. So, basically, you can choose whatever direction of effects you desire, and there will be an industrial organization and labor market that will deliver those results.

Empirically, the discussion is still open and depends critically on the impact variable in which we are interested. In terms of income distribution, (Brown (1999)) points out that a positive effect of minimum wage on wage compression is a well established fact. For example, Lee (1999) shows for United States how the decrease in the real minimum wage (during 1980s) caused an increase in the observed wage inequality, particulary in the bottom of the distribution. In the same way, but for a developing country (Brazil), Lemos (2009) find out evidence that the minimum wage compresses the wage distribution in the formal and informal sector, even with no impact on employment.

Consequently, our finding in the impact of the MW on the wage compression are coherent with the literature, namely, MW has a role in getting better the income distribution, principally for people at the bottom of the distribution. We find that these effects have some statistical significance, but are not that large.

On the other hand, there is no consensus if we are interested in the impact of the MW on employment. The evidence until the beginning of the 80s showed a negative relationship between MW and employment. Indeed, based on time series studies and looking the impact on the teenage group, Brown, Gilroy, and Kohen (1982) pointed out that "the studies typically find that a 10 percent increase in the minimum wage reduces teenage employment by one to three percent". So the 80s evidence showed significant but modest effects. Moreover, theses papers were criticized not just for their difficulty to isolate the effect of MW of others possible explanations, which is a big issue principally with aggregated data, but also, as was documented by Card and Krueger (1995) with a simple and creative method, there is evidence that this literature has been affected by specification-searching and publication bias.

In the last two decades we have seen the appearance of studies contradicting the common sense "MW reduces the probability of keeping a job", that was coherent with the 80s evidence. For example, Card and Krueger (1994) and Card and Krueger (2000) ; for the United Sates, and Abowd, Kramarz, Lemieux, and Margolis (2000), Machin and Manning (1994); for the UK, find zero or positive effects of the MW on employment[2]. In the opposite argument, there is a body of research where that finds negative and significant effects of MW on employment (Currie and Fallick (1996) and Deere, Murphy, and Welch (1995)). So, now we don't have consensus around the employment implications of MW[3].

For developing countries the debate is rather similar, with no consensus in the sing and magnitude of the MW effect. However, Lemos (2009) states that as the effects of MW depend on its levels (and enforcement), labor market particularities and institutions in each country; so it is reasonable to expect finding differences between studies for developed and developing countries. Indeed, the author says the effects of the MW are stronger in Latin America than USA, both on employment and wage compression.[4]

In Chile, we have also a number of studies supporting the different theoretical hypothesis. According to the classical point of view, we can find evidence in Cowan, Micco, Mizala, Pagés, and Romag (2005), Corbo (1980) and Sapelli (1996). The principal problem of theses studies and surveys is the using of aggregate data or the derivation of the effects indirectly, so their identification of the MW effects are less trustworthy than studies using individual data.[5]. On the other hand, using a natural experiment approach Bravo and Contreras (1998) find

[2]A possible explanation for that results is that part of the pressure created by the rising in MW is dissipated by the increasing in the prices of the final goods, Lemos (2004) presents evidence for this phenomenon for Brazil.

[3]Even though is less researched, as we do in our paper, it is also interesting studying the impact of the minimum wage on other variables, for example: hours worked, and then to integrate those effects. A good example of this approach we find in Neumark, Schweitzer, and Wascher (2004), they report, instead of us, a negative total effect.

[4]In spite of her survey, she finds, for Brazil, non negative effect of the MW on employment.

[5]We can find a critical point of view oh theses papers in Landerretche (2005), Bravo (2005) and Bravo and Contreras (1998).

no evidence of negative impact of MW on employment[6]. With the same conclusion but using time series analysis, Bravo and Robbins (1995) find that the MW has no effects on employment.

Considering the Chilean studies our paper represents an important contribution in different areas. First, and most relevant, we use a built an individual panel data base, which is useful to isolate the MW effects. Second, it reviews a period which is quite interesting, with many macroeconomics shocks and a big variance in the MW increasing. Finally, our data and econometric approach allow us to define a more adequate control of group.

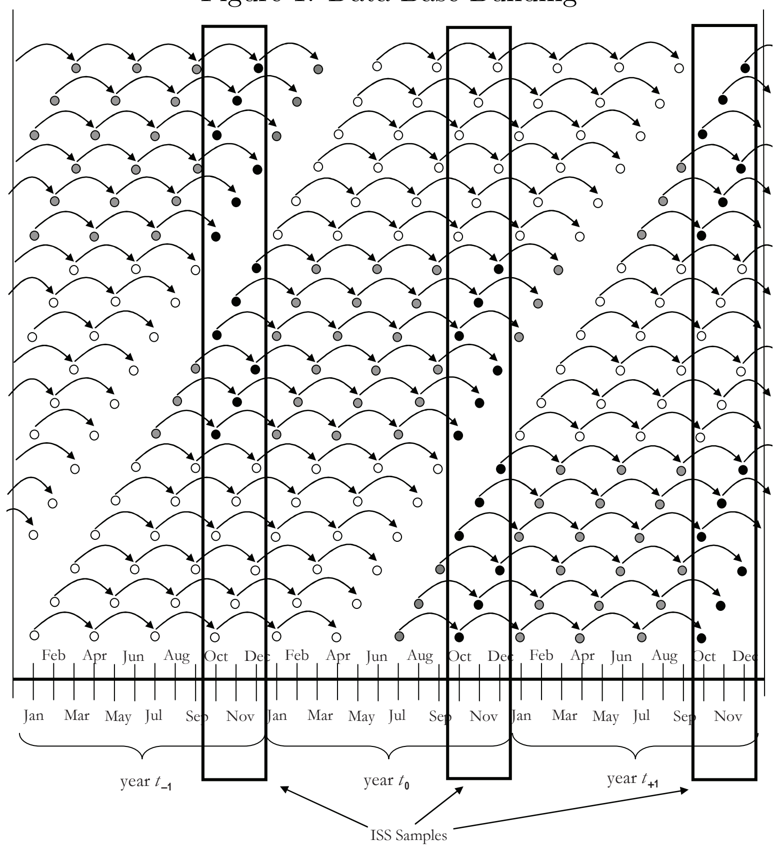
3 Data Description

We believe that one of the main features of this article is the use of an attractive new labor panel database that we constructed for Chile from the National Employment Survey (NES) and the Income Supplementary Survey (ISS) of the National Bureau of Statistics (Instituto Nacional de Estadísticas, INE). We end up with a data set composed of overlapping panels for the years 1996 to 2005, which has the interesting feature of containing the Asian Crisis and heterogeneous years as far as the generosity of MW increases is concerned. We think that a correct understanding of the virtues and limitations of the database is crucial for the reader to understand the usefulness of our calibration. That is why, in this section, we provide a detailed explanation of the database construction.

The NES is a survey that measures the situation of working-age people in the workforce in Chile. It is carried out on a monthly basis by the Chilean National Bureau of Statistics (Instituto Nacional de Estadísticas, INE) using the following procedure. The sample is divided in to sixths that are equally representative of the Chilean labor market. Each sixth of sampled households is interviewed six times during a period of eighteen months with three months of space between interviews. The sets of sample households are rotated so that, every time a set has been in the sample for eighteen months it is dropped and replaced by a new sample. Therefore the same household can potentially be observed six times (spaced quarterly) for a period of one and a halfyears.The ISS a survey that measures the income of the same households that are surveyed by the NES. However, the ISS is carried out as a module to the NES only during the months of October, November and December of each year.

[6]They use as control group the younger people (15-17 years), because this segment is affected by a chipper MW than the older group.

Figure 1: Data Base Building



Neither the NES or the ISS have been designed to provide panel data. Moreover, because of the strict regulations on statistical privacy of the INE, individual identities of the interviewees are destroyed. However we construct the panel using the rotating sample structure that is described in Figure 1 and taking advantage of the fact that INE does preserve the address of the household were the interview is made. The figure covers a hypothetical three years of surveying. Each sequence of six balls connected with bouncing arrows is a sample, and the location of the balls indicate the months in which the sample is used. If one counts vertically, one can verify that in any given month six samples are being used to construct the NES (hence the term "sixths" used by the INE). On the other hand the ISS is only taken in the last three months of every year. Hence, there are three colors of balls in Figure 1: white balls indicate sample sequences that we do not use since they only contain one income measurements; grey and black balls indicate sample sequences that we do use since they contain two income measurements. Grey balls indicate the sample application months that do not interest us as far as this paper is concerned. Black balls indicate the sample application months that do interest us for the estimations in this paper. As you can see in the figure, requiring the sample to contain two applications of the ISS implies loosing a significant amount of the available samples at any point in time by construction as a result of the rotation of samples.

As we have already mentioned, the main problem faced when trying to construct the panel is that the INE does not preserve the identities of the individuals in each application of the survey. However, since the sampling procedure is geographic, it does preserve the address and the structure of the household through the questionnaire. So what we do is to go through a statistical procedure of checking if the characteristics of the household living at the sampled address are preserved in the two moments of time when they coincide with an application of the ISS. The households that survive our check are preserved in the database.

Households are first uniquely identified by address and then the members within a household are identified by relationship, sex and age. The information does not make individuals uniquely identifiable over time, since two problems persist: different people with the same characteristics (which we term twins) and individuals with variables that have changed over time in more than an acceptable measure. Twins are individuals within households with identical values for the variable characteristics relationship, sex and age. If this effect is not considered, different people could be matched as though they were the same person. In order to avoid this problem, twins are eliminated from the matching process. The second problem is solved by controlling for two things: first, the structure of the households must not change (although we check this indirectly); second we must not have non-plausible changes in the same person over time in sex, age and schooling. If the structure of the household (number of people and types of persons) changes, they are eliminated from the matching process. If the variables sex or schooling change or the age variable changes by 2 units, they are immediately eliminated from the matching process.[7]

The whole process involves the loss of a significant amount of observations, additional to the loss of a third of the sample by construction. In the course of a year many things can happen that change the structure of a household, and many households move. Our main worry is the correlation between these changes and the performance in the labor market. Table 1 shows the number of individual observations that we can extract from the NES-ISS database in different stages of the production of the panel. We have, on average around 120 thousand observations in any ISS seasons. Referring back to Figure 1 this corresponds to the total individual observations in all the balls of lets say ISS sample for years t_{-1} and t_0 which turn out to be 30 sample sequences (feel free to count). Of these, only 18 have observations for both ISS seasons and are useful for our study. Hence a loss of 25% of the possible observations in the database. Table 1 illustrates the data loss, once we check for sample sequences that cross two ISS seasons we get close to 90 thousand observations. Once we check for the characteristics of the households (age, sex, schooling, structure of the family) we loose two thirds of the database and end up with 30 thousand observations per year. This is the size of our database.

[7] A similar data base and approach of how to build the panel data base is possible to find in Neumark, Schweitzer, and Wascher (2004).

Table 1: Data Loss

Years	Whole	Feasible	Actual
96-97	120,121	88,762	26,543
97-98	117,660	88,344	26,319
98-99	117,521	90,084	34,169
99-00	116,423	88,141	31,276
00-01	125,668	92,855	29,641
01-02	124,713	96,536	41,207
02-03	122,089	89,861	28,858
03-04	120,970	87,377	26,095
04-05	118,486	85,024	26,381
Average	119,601	89,665	30,054

Undoubtedly this process introduces some biases in our database that could potentially affect our results. It is highly likely that households that either perform very well or very bad in the job market will change their address and, hence, drop out of our database. Also, households that change their structure very much will look to us as a different family and we will drop them, although performance in the job market may determine important changes in the composition of the household (the children moving out or in for example).

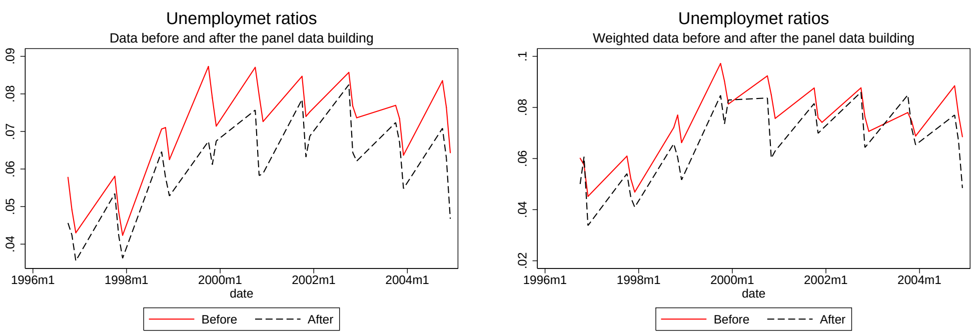
Table 2: Stylized Facts of Databases

Education		Whole	Actual	P Value
Without	Male	1,9%	2,0%	0,129
	Female	2,3%	2,4%	0,056
Primary	Male	18,1%	22,9%	0,000
	Female	19,5%	23,8%	0,000
Secondary	Male	21,4%	18,4%	0,000
	Female	23,3%	19,8%	0,000
Higher Education	Male	6,6%	5,6%	0,000
	Female	6,9%	5,2%	0,000
Age		Whole	Actual	P Value
15-30	Male	16,8%	14,5%	0,000
	Female	17,1%	14,2%	0,000
31-55	Male	21,1%	22,8%	0,000
	Female	22,9%	24,5%	0,000
55+	Male	10,1%	11,5%	0,000
	Female	11,9%	12,5%	0,000

Table 2 compares the stylized facts of the "whole" and "actual" databases. The third column of the Table shows a P-value of the proportion difference test between the two databases. We find that in the "actual" database there are more people of primary school and less for secondary and higher education than in the whole database, in both male and females. In terms of age, is quite clear that our actual database is older than the Whole. As we show, the proportional differences have statistical significance.

Another way to see whether our database is still representative of the labor markets of the Chilean economy is to calculate the national unemployment rates using the database that survives our filters and compare them with the official INE ones. This comparison can be seen in the first panel of Figure 2. The unemployment rates from the constructed panel database are slightly lower than the official numbers, but very close, and with almost identical dynamics (in the pure and weighted data). The fact that they are lower indicates that the predominant bias is that households move or are substantially modified when there is an adverse job shock. So the household that survive our calculations tend to be slightly better job market performers. This is a potentially disruptive bias for our study, although one we will have to live with. If we are mechanically selecting out of our sample many of the losers of MW increases, then we could be underestimating the adverse effects of MW policy particularly those who loose their jobs. We must bear in mind this potential bias when taking stock of the results of this paper.

Figure 2: Unemployment ratios



The other important variable in our study is the minimum wage. In Chile this variable is set by the government –after some public arm-wrestling with the workers federations and business guilds– in nominal terms, in the month of July of each year. It is active, immediately and for the next twelve months. In legal terms it is an hourly wage, however, the number that is communicated is the equivalent for a full 180 hour month[8], since almost nobody actually calculates the number month by month, but rather pays either the full amount or the corresponding proportion depending on the proportion of hours that have been negotiated into the contract. It is a gross wage, meaning that social security contributions, unemployment insurance contributions and compulsory health insurance must be subtracted to estimate the net disposable income of the worker. This amounted to 20% of the wage up to August 2002 when the unemployment insurance

[8]The working week in Chile is 45 hours (180 per month) since July 2004. Before that, and since 1924 it was 48 hours (196 per month).

system was created and the proportion retained was elevated to 23% [9] [10].

Figure 3: Percentage minimum wage variation

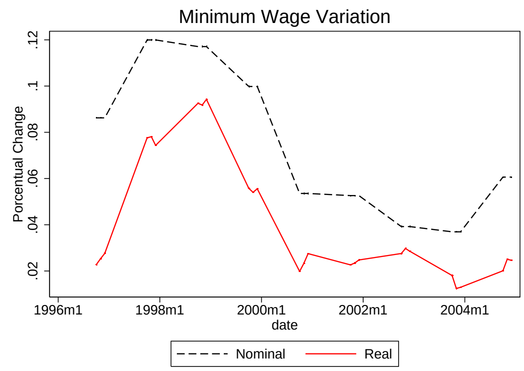


Figure 3 shows the evolution of the MW both in real and in nominal terms in Chile for the years covered by the constructed panel. As we can see, there is substantial heterogeneity in both the nominal and real MW policies with large nominal and real increases in the first half of the sample, and smaller ones in the second (with the exception of the last observation), this variance will be useful in our strategy of identification. The fact that the MW is set in July, that is, a full quarter before the ISS season starts is useful for our study, since it allows for the signal given by the authorities to act and

[9]In reality Chile has a mixed system. Part of the unemployment insurance is really forced saving in individual accounts, much like the pension system. Another part is a tax that contributes to a common pool that is accesses by the worker under certain conditions. Very recently the government has sought to relax the conditions under which a worker can use this common pool, since the experience of the system has shown that very few workers are eligible in practice. During the period that is covered by our database, and since the establishment of unemployment insurance, the rules have not changed. However, it is relevant to know that although in practice this mechanism has operated as a forced savings scheme rather than insurance or subsidy, we shall treat it's discount as a tax on the worker. Implicitly we are assuming that workers are either severely restricted in credit market access or myopic in some way or the other. Also it is important to know that the unemployment insurance system was made compulsory for all new contracts from the moment that the bill was passed. This means that the transition from the 20% to the 23% is something of an unknown quantity, although the practical application of this policy has actually surprised analysts with much higher churning rates than expected.

[10]There is some degree of heterogeneity in the size of the social security contribution that workers face. Young workers that have worked all their life after the 1979 Labor Reform that privatized social security have a 20% rate. This is also true of middle age workers that were moved from the old pay-as-you-go system to the new private fully funded system. Older workers that had accumulated many years under the old system were kept in a public system that is slowly winding down as demographic trends work through. In this case the rates can range from 20% to 27% depending on what type of pay-as-you-go system the worker comes from. This basically depends on what sector the worker was in when the system was reformed. The 23% is widely used in Chilean labor market analysis as an average rate. We believe that in the early 1990's it was probably much higher than that and it has probably been falling but there is no reasonable way to measure precisely what the actual level of this rate has been in different years.

affect the labor market before measurements are taken.

4 The Empirical Strategy

This section is divided into three parts. The first is dedicated to a detailed description of the variables. In our view a reading of this section is crucial for anyone interested in forming an opinion on the plausibility of our results, their robustness and the limits of the conclusions that can be extracted from them. The second to fourth subsections are dedicated to a detailed description of the different equations that compose our estimation model, the econometric techniques and inference systems that we apply. In section two we formulate an empirical strategy that uses interpersonal heterogeneity to formulate an identification of the effects we are interested in: we call this method "simple estimation". In section three we formulate an empirical strategy that uses intertemporal heterogeneity to formulate a"diff-in-diff" identification strategy: we call this method "diff-in-diff". In section four we formulate an empirical strategy that is similar to the "simple estimation" procedure only we try to estimate not only the effect of being treated to MW policy but the effect of the size of the MW increase: we call this method "elasticity".

4.1 Variables Robustness

We are interested in measuring the impact of the MW on hours worked, income, probability of keeping the job and probability of getting a job. To study the impact of MW we need to determine the treatment group (TG). Quite simply, we define the treatment group as individuals who earn in t_0 a wage between the MW of t_0 and t_1. That is:

$$i \in TG \Longleftrightarrow W_{m,t0} \leq W_{i,t0} \leq W_{m,t1}$$

$$DT_{i,t} = \begin{cases} 1 & \text{if } i \in TG \wedge t = t_1 \\ 0 & \text{if } \sim \end{cases} \tag{1}$$

where $W_{m,s}$ and $W_{i,s}$ are the minimum and individual wages in period s respectively.[11]

Robustness requires us to consider that the NES and ISS are perceptions surveys, that is, we have no way of actually verifying if the information reported by the worker is actually true and we depend on what the worker understands is being asked of him. Even in the NES, when the surveyor asks if the worker is or not employed, there is always some space for interpretation by the surveyee on what actually constitutes employment. In the case of the ISS it is not clear if the worker that is being surveyed, when asked about his incomes is answering in gross or net terms. So, we do it both ways: we generate a

[11] This treatment group definition is standard in the literature. However, we have a potential problem because we are not sure whether workers in the TG are really in that group when the MW is raised. In fact, some workers in the TG can change their jobs before the July (the moth of the MW definition). So, we are assuming that the majority of workers affected by the MW at the moment of the survey are also affected in July.

set of estimations assuming that surveyed workers answer in gross and net terms. As we shall see this does not seem to make much of a difference although it does mean that the edges of our treatment group could be rather soft (especially if there are both types of answers, which is very likely) that reduce the sharpness of our estimations.

As is the case in all surveys of this type, sampling is accompanied by the calculation of expansion coefficients that weigh the observations and also allow to calculate aggregate numbers that are comparable to the actual population of the country. Since our panel construction methodology sharply reduces the data we have at our disposal, the representativeness of the surviving data is much lower (as the comparison of the two databases in Table 2 and Figure 2 shows). The question then is what to do. We present two options to deal with this. Our favorite, the first, is to ignore the problem and treat the observations individually with no claim to representativeness (we will call these estimations "pure"). The second, is to weigh the observations according to the surviving weights (we will call these estimations "weighted"). This has the obvious problem that it is by no means likely that the surviving sample contracted in any proportional way. In the extreme, there could be weights for which no observations survived and other for which all observations survived. On the other hand, the advantage of this method is that we are using the unfettered expansion coefficients constructed with rigorous statistical methods by the INE.

Finally there is the possibility that workers adjust hours supplied to the market rather than their market participation in reaction to innovations in the MW, and that employers adjust hours demanded (using the extra hours margin for example) rather than the number of employees. This means that in the strictest of senses we should do all of our exercises in hourly wages by dividing the monthly income by the hours worked. It also means that we should estimate the effect on hours of MW innovations, which we do. Fortunately the NES does contain questions on the amount of hours worked. To check for robustness of our results we run all our estimations both on hourly wages and on total wages[12].

Our quest for some degree of robustness has driven us to several alternative configurations of the panel database that we use. Table 4.1 summarizes the different alternatives we use and gives the key to the codes which we will use to denominate each case.

[12] A somewhat uncomfortable characteristic of this database is the fact that there is a significant amount of observations (on average X by year pair) that show workers earning less than the minimum wage by hour. This can be observational error or the reality of informal labor markets. The existence of these data points questions the validity of our treatment group since it opens the possibility that many of the workers within the treatment group are informal (even if they don't say so) and hence are not affected by MW policy in the theoretically expected way. Unfortunately we are forced to mindfully dodge around this issue. With respect to the observations of workers under the minimum wage: we eliminate them from the sample, under the assumption that they are largely informal.

Table 3: Taxonomy of Estimations in this paper

Wage	Weight	Tax	Code
Hourly	Pure	Gross	HPG
		Net	HPN
	Weighted	Gross	HWG
		Net	HWN
Monthly	Pure	Gross	MPG
		Net	MPN
	Weighted	Gross	MWG
		Net	MWN

The definition of the treatment group in this study is even more difficult than the proper identification of the control group (CG). As we have discussed in the literature different studies that are related to ours use different methodologies. The issue boils down to choosing which subset of workers that earn more than the MW are similar enough to the treated workers to provide a good CG or what is the highest wage that we are willing to fix as a CG ceiling. Again we prefer the most open strategy possible given our data. We do this by running the whole set of regressions for an increasing CG that we generate by slowly pushing the CG ceiling upwards. This allows us to see things in the opposite way we will see what CGs give theoretically consistent results and which do not. So we define that Individual i_{t0} will belong to CG j for year t if[13]:

$$\begin{aligned} W_{m,t1} < W_{i,t0} \leq W_{m,t1} + Gap_{t0} * (0.5 + 0.1 * j) \\ Gap_{t0} = W_{m,t1} - W_{m,t0} \\ \forall j = 1, 2, ..., 30 \end{aligned} \tag{2}$$

Hence, in addition to the varieties of sampling and variable definition that we describe in Table 4.1, we also produce results for a continuum of control group definitions in each case. Our preferred regression throughout the paper will always be "HPN", that is hourly net wages without any weighting. We believe that this is the cleanest case. We will, however refer to and report the other cases if there are robustness issues in favor or against our results.

[13]There are many papers that use this idea of control group, namely, defining the groups of comparison in terms of segments of the real wage distribution in the initial period, particulary the people who earn in t_0 slightly more than the minimum wage in t_1, for example Currie and Fallick (1996), Abowd, Kramarz, Lemieux, and Margolis (2000) and Stewart (2004).On the other hand, the validity of the control group definition and so the precision of our estimation rely on the non existence of the spill over effects. Supporting this idea, we can find international evidence in Dickens and Manning (2004) and for Chile in Cowan, Micco, Mizala, Pagés, and Romaguera (2005).

4.2 Strategy 1: "Simple Estimation"

As we discussed in the motivation of the paper, our objective is to quantify as comprehensively as possible the different effects that innovations in the MW can have on the economy. This means that we will estimate effects on the wages of the treatment group to see how effective the policy measure is, their hours worked, and the probability of keeping employment. We are also interested in estimating the effect that the MW change can have on the probability of getting employment for unemployed and passive workers.

For the estimation of the effect of the MW on the wages or the hours worked of the treated group we estimate:

$$ln(Y_{i,t}) = \alpha + \beta_1 DT_{i,t} + \beta_2 \delta_i + \beta_3 \theta_t + \eta_{i,t} \tag{3}$$

with a fixed effect panel data estimator that allows us to control for invariant individual characteristics and time effects. Where $Y_{i,t}$ is either wages or hours worked; $DT_{i,t}$ is the treatment group dummy for individual i at time t; δ_i are the individual effects and θ_t are the time effects. We run this on a panel composed of all the overlapping pairs of years that we have in our database.

For the estimation of the effect of the MW on the probability of keeping employment in the treated group of workers we estimate:

$$T_{i,t} = \alpha + \beta_1 DT_{i,t} + \delta X_{i,t} + \mu_{i,t} \tag{4}$$

with a probit regression. Transition variable $T_{i,t}$ is the dummy that indicates changes in employment status, taking value 1 if the job is kept and value 0 if it is not[14]; $X_{i,t}$ is a vector of individual observation characteristics that includes: sex, age, year, schooling and economic sector.

For the estimation of the effect of the MW on the probability of finding employment for unemployed or inactive workers we follow a strategy consistent of two steps. First we find a "theoretical" treatment group by identifying the non-employed workers that have the potential of being affected by the MW hike. Second we estimate the effect of the measure on their employment status. The first stage of the estimation, then consists of running the following Mincer equation:

$$ln(w_{i,t}) = \alpha + \gamma_1 X_{i,t} + \nu_{i,t} \tag{5}$$

[14] It is important to stress that given the nature of our database it is entirely possible that the worker changes jobs and we assign a value of 1 to T since we do not observe the unemployment transition period if it lasts a couple of months between the two ISS seasons. We would, in this case, be ignoring an unexpected beneficial effect of aggressive MW policies. We believe that this effect i, at most, very small. Unfortunately, given the nature of the database, we cannot prove this. This is harmful for our inference but not necessarily for our estimation from an aggregate point of view. If there actually turned out to be some effect of the MW on these types of labor transitions, the MW would be spurring a churning in the labor market that we would not be capturing rather than the destruction of a workplace.

we then apply the estimated Mincer equation to predict what would be the "theoretical" wage that these workers would get if they were able to match into a job: $\hat{W}_{i,t}$. The underlying assumptions involved in this step is that most unemployment is involuntary and that the labor market is rationed either by regulations (including our MW) or by the effects of information asymmetry. We then find which of these workers fall within the two minimum wages and classify them as part of our treatment group. Then we estimate equation 4 only in this case $T_{i,t}$ will indicate changes in employment status such that it will take value 1 if the worker finds a job and value 0 if it does not.

The first part of the results section that you can find below has the results for all of these estimations. The second part attempts to estimate the integral of the effects to see if there are economically significant variations in the income of working families or the income distribution among them. This is done by calculating the unconditional expected value of the change in the hourly labor income $E(\Delta w)$ for the TG.

$$E(\Delta w) = \frac{E}{E+I+U} \sum_{i \in TG \wedge E} \frac{w_{it0}(\hat{\beta}_{income} + \hat{\beta}_{hours} + \hat{\beta}^{e}_{employment})}{N^{e}_{TG}} + \tag{6}$$

$$\frac{U}{E+I+U} \sum_{i \in TG \wedge U=1} \frac{w_{it1}\hat{\beta}^{u}_{employment}}{N^{u}_{TG}} + \frac{I}{E+I+U} \sum_{i \in TG \wedge I=1} \frac{w_{it1}\hat{\beta}^{i}_{employment}}{N^{i}_{TG}}$$

where E, I and U are the amount of people treated whose are employed, inactive and unemployed, respectively. On the other hand, $\hat{\beta}_{income}$ and $\hat{\beta}_{hours}$ are the MW impact on $ln(income)$ and $ln(hourworked)$; $hat\beta^{e}_{employment}$, $hat\beta^{i}_{employment}$ and $hat\beta^{u}_{employment}$ are the MW impact on the probability of keeping or finding a jobs for employed, inactive and unemployed, respectively.

We simulate the confidence bands through a bootstrap algorithm (simple with replacement) with 1,000 draws. We do this for all our samples and for all our control group sequence.

4.3 Strategy 2: "Diff-in-Diff"

One of the problems that strategy 1 has is that both the rise in the MW and the variations in income and employment may have other, common causes, that make the previous strategy spurious. In other words, it is possible that our CG is not as acceptable as we expect, namely, the TG has differences with the CG in some unobserved variables[15]. For example, a collective enthusiasm derived from a bubble that drives up wages and employment and relaxes the political constraints on MW setting may generate a spurious relation between labor income and MW increases. Or, the possibility that poor people have per se a greater probability of loosing their jobs, may generate a spurious relation

[15] We can expect this problem to be worst in the case of transitions (employment, inactivity and unemployment), because in the other cases we control for the variables that are invariants along different periods.

between being in the treated group and an increased probability of loosing a job[16]. If something like this was happening we would be over-estimating the effects through our previous methodology. To address this we exploit the time span of our database and the heterogeneity in the magnitude of MW adjustments. For that purpose we create another dummy: $TI_{i,t}$ (treatment intensity):

$$TI_{i,t} = \begin{cases} 1 & \text{if } i \in TG \wedge t = t_1 \wedge i \in T^* \\ 0 & \text{if } \sim \end{cases} \tag{7}$$

where T^* is the group of years when the real rise in the minimum wage is more than 5% [17]. Now, the panel estimation that we use for estimating the effect on income and working hours has the following structure:

$$Y_{i,t} = \alpha + \beta_1 DT_{i,t} * TI_{i,t} + \beta_2 DT_{i,t} + \beta_3 TI_{i,t} + \beta_4 \delta_i + \beta_5 \theta_t + \eta_{i,t} \tag{8}$$

and our probit estimation for the effect on :

$$T_{i,t} = \alpha + \beta_1 DT_{i,t} * TI_{i,t} + \beta_2 DT_{i,t} + \beta_3 TI_{i,t} + \delta X_{i,t} + \eta_{i,t} \tag{9}$$

The advantage of this estimation is that we take advantage of the heterogeneity in MW policies that there is in the covered period. The disadvantage is that this estimation assumes symmetric macro shocks. If macro shocks are non symmetrically distributed among labor income segments, then we will be estimating a smaller effect, so the biases will run in favor of our results. It is important to remember that the years in which the minimum wage was most increased (our TG of this section) are also those in which the economy was subject to the Asian Crisis. Hence, we may be attributing to the MW a macroeconomic effect that is not really related to labor policy. Still, since the main argument of this paper is to show that the effects are generally overestimated, we prefer to give the effects the benefit of the doubt.

4.4 Strategy 3: "Elasticity"

Up to this point we have relied on the implicit assumption that the distance of the wage from the MW is irrelevant. We have assumed that it is only relevant to know if the individual is or not in the treated group. We have searched for differential effects in a couple of definitions of treated group when compared to those in a series of alternative control groups. However, it is plausible that the effect of MW policy depends on the distance between the wage and the new regulated one. Consider the possibility that even MW contracts do not really comprehend the totality of the relationship between a worker and his or her employer. And consider a world where in some way the minimum

[16] Neilson and Ruiz-Tagle (2007) show that for Chile the probability of keeping the job is lower for less educated people.

[17] This threshold could appear quite arbitrary, but looking the figure 3 it is easy to divide the years in that way.

wage is close to the value of productivity of the workers that are subject to it. Even if there are competitive labor markets, the employer does not know with certainty the exact marginal contribution to value of every last worker, so there is some degree of tolerance to excess MW setting that is derived from the information bounds to rationality that any employer faces. And even if it did, there may be non monetary payments that end up making the total remuneration exceed the minimum wage, like holiday presents and parties, tolerance to emergencies, workplace conditions... etc. In this world it is entirely possible for an employer to marginally adjust these "non monetary" if the MW is a bit over what he or she is willing to pay the worker. On the other hand, if the distance between the wage and the MW is much larger, it is much less likely that the employer will be willing to tolerate this as an error within a diffuse estimation of marginal productivity, or much less capable to adjust by reducing non monetary benefits.

Hence, we have devised a strategy for testing if the distance between the ex-ante wage and the new MW has measurable affects on the variables that we look at in this study. The strategy consists of calculating a measure for distance and introducing it into the equations of the first estimation strategy described in Section 4.2.

For the estimation of the effect of the MW on the wages or the hours worked of the treated group we estimate:

$$Y_{i,t} = \alpha + \beta_1 DT_{i,t} + \beta_2 \delta_i + \beta_3 \theta_t + \beta_4 D_{i,t} + \eta_{i,t} \tag{10}$$

which is a modification of equation 3 that includes the measure of distance. And for the estimation of the effect of the MW on the transitions of employment status, we estimate:

$$T_{i,t} = \alpha + \beta_1 DT_{i,t} + \beta_4 D_{i,t} + \delta X_{i,t} + \mu_{i,t} \tag{11}$$

which is a modification of equation **??** that also includes the measure of distance. Our measure of distance is:

$$D_{i,t} = \left(\log\left(W_{m,t}/Q_t\right) - \log\left(W_{i,t_{-1}}/Q_{t_{-1}}\right)\right) \tag{12}$$

where Q is the general Consumer Price Index so that the measure of distance that we use is in real terms. This is important because there has been some considerable variation in the inflation rate during the period. This means that an X peso distance between the two wages can mean different things in different moments in time. The same nominal distance between the ex-ante wage and the minimum wage is much less important if inflation accelerates. Hence, we correct for inflation.

5 Estimation Results

In this section we present the results of the estimations on the equations described in Section 4. However, as we discussed in Section **??**, there are a great variety of definitions of critical variables that could potentially affect the composition of the control and treatment groups and, hence, the estimations we make in this paper. Despite this fact, once we look at the totality of the estimations a pretty clear picture emerges. So, we will present in this section our favorite specification, that is, the definition of variables, control and treatment group that is closest to what we think is theoretically correct. When the robustness checks make these results vary we will report this in the text but refer to the attachments of the paper for visual reference.

The section is organized as follows: we will devote a subsection to present the estimations of each of the equations that we have presented, and then we will present the estimation of the aggregate effects. In each case we will show the results for the three strategies discussed in the previous section.

As we said, our preferred specification is "HPN" according to the key of Table 4.1, that is: hourly net wages with no weighting at all. We prefer hourly wages because the MW in Chile is not exactly hourly but can be divided according to the working schedule according to certain rules. One of the problem of considering the monthly wages is the wrong application of the TG definition: if, for example, some people earn the double of the MW but work part time, his monthly wages could be considered affected by the MW increasing. We prefer net wages since we believe (we cannot prove it, but it is the survey specification) that most workers understand and talk of their wages in net terms and don't even feel the gross figure. We prefer not to weight the data in any way, although there are solid statistical reasons for doing so because we prefer to see the effects in pure form and treat weighting as robustness checks.

As we said each figure that we present in the text will have three panels showing the total effect on the studied variables of being in the treated group. The top one will always be from the "Simple Estimation" strategy; the middle one will always be from the "Diff-in-Diff" strategy; the last one will be from the"Elasticity" strategy.

5.1 Labor Income

It is important to clarify why we talk about labor income and not wages. Although it is clearly the case that in the absolute majority of cases, workers in this income range have just one wage, it is not possible for us to distinguish if this is true from the data that we are using. Hence, we are assuming that total income is just the result of one wage, and hence, when we divide the total labor income of a worker by the hours it works, we have our estimation of an hourly wage. It is possible, in principle that a member of our panel, for example, has two wages for a half of the working day: one that pays half a MW per hour, and another that pages 50% more than a MW per hour. We will see this worker as a full time worker with the minimum wage. The quality of our estimations rely on there not being very many cases such as this. However, there is not much that we can do to prevent it. But, at the very least, we must call the variable as "labor income" and not "wages"[18].

[18]This problem also could be an issue in the control and treatment group definition, because It depends on the relationship between the minimum wage and the total labor income reported.

Figure 4: Impact on labor income

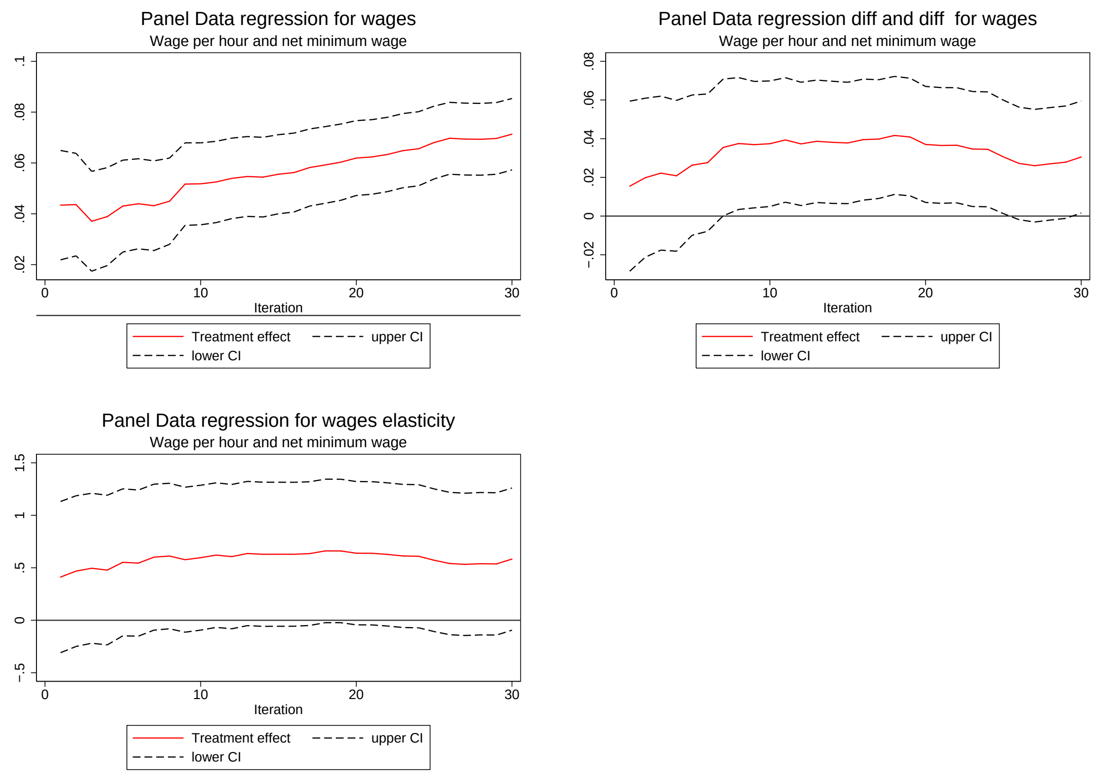


Figure 4 shows the results of the estimation of equation 3. The top panel shows the results of the “Simple Estimation” strategy, the second panel shows the results of the “Dif-in-Dif” strategy, and the lower panel shows the results of the “Elasticity strategy. Each panel shows the estimations for the 30 different definitions of the control group as described in Equation 2. We report the estimation of parameter β_1 of Equations 3, 8, 10 for the case where dependent variable Y is the net hourly labor income. Table 4 of the Appendix shows a sample of the complete estimation results for the three strategies: iterations 1, 15 and 30.

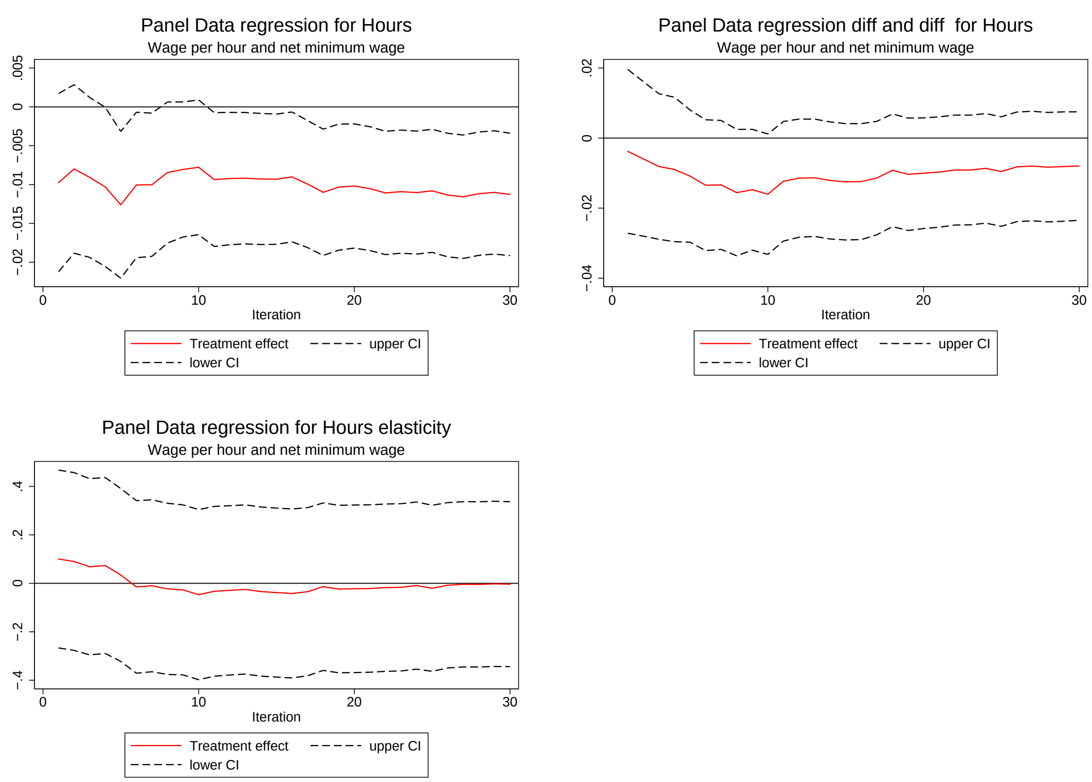
We find that there is evidence of a positive effect on wages of the treatment group of the MW policy. We find robust effects with the first estimation strategy of around 4 %. If we were to take this estimation literally it would means that on average, during the whole period, the MW policy has generated a wage compression of 4% per year of the treated group relative to the control group. This figure increases as we tolerate an increasingly heterogeneous control group. In the case of the Diff-in-Diff strategy we find the same magnitude of effect, but are only able to reject the zero for some iterations (control groups). The elasticity strategy does not deliver a significative result, although the magnitude of the estimated result is the same.

In summary, we find that there is at least some evidence to say that the idea that informality and spot labor contracts make MW regulations totally ineffective in increasing the wages of the treated group is unfounded. Although the result is not completely robust, neither can one robustly defend the hypothesis that the effect is zero. Figures 4, 5 and 6 of the Appendix show the 36 different combinations of estimation forms that we have run in this study. In the table resume 7.1 of the appendix we can observe the significance for all estimations (that is a summarize of the graphics presented in the appendix). A quick glance at these compilations should convince the reader that the results do not deviate much if we deviate from our preferred estimation specification.

5.2 Hours Worked

Figure 5 shows the estimation of parameter β_1 of Equations 3, 8, 10 for the case where dependent variable Y is hours worked. Table 5 of the Appendix shows a sample of the complete estimation results for the three strategies: iterations 1,15 and 30.

Figure 5: Impact on Hours Worked



It is important to remember that from a theoretical point of view the result of this estimation (as well as the one shown in the following two sections) could be ambiguous if there is a substantial degree of heterogeneity in the market structure of a segmented labor market. If a part of the labor market provides monopsonistic power to a labor demander, the fixation of a MW could increase hours worked (as well as actual employment). If the market is monopolistically controlled by a union, this should also be the case. If the market is competitive and the MW is binding then it should have the reverse effect. Accordingly we find much weaker effects.

The "Simple Estimation" method shows a significant negative effect on hours worked for almost all control group definitions, except the very most stringent. If one were to believe completely in this estimations. Workers forming part of a MW treated group have on average reduced in almost 2% hours worked during the period studied. However, this result is much less robust than the previous one. We find that the "Diff-in-Diff" (the preferred specification) and "Elasticity" do not have statistical significance. Figures 7, 8 and 9 of the Appendix show the 36 different combinations of estimation forms and strategies (there is a summarize in the Table 7.1). As the reader can see, the conclusions do not vary much.

5.3 Job Security

Figure 6 shows the estimation of parameter β_1 of Equations 4, 9, 11 for the case where dependent variable T is probability of keeping the job. Table 6 of the Appendix shows a sample of the complete estimation results for the three strategies: iterations 1, 15 and 30. Figures 15, 16 and 17 of the Appendix show the 36 different combinations of estimation forms and strategies.

Figure 6: Keeping an Employment

We find reasonably robust evidence that MW policy reduces the probability of keeping an employment among the treated group. The dimension of the effect is between a 1% to a 4%, but it is significant for almost all "Diff-in-Diff" estimations and "Elasticity" estimations. It is also sometimes significant in the "Simple Estimation" case.

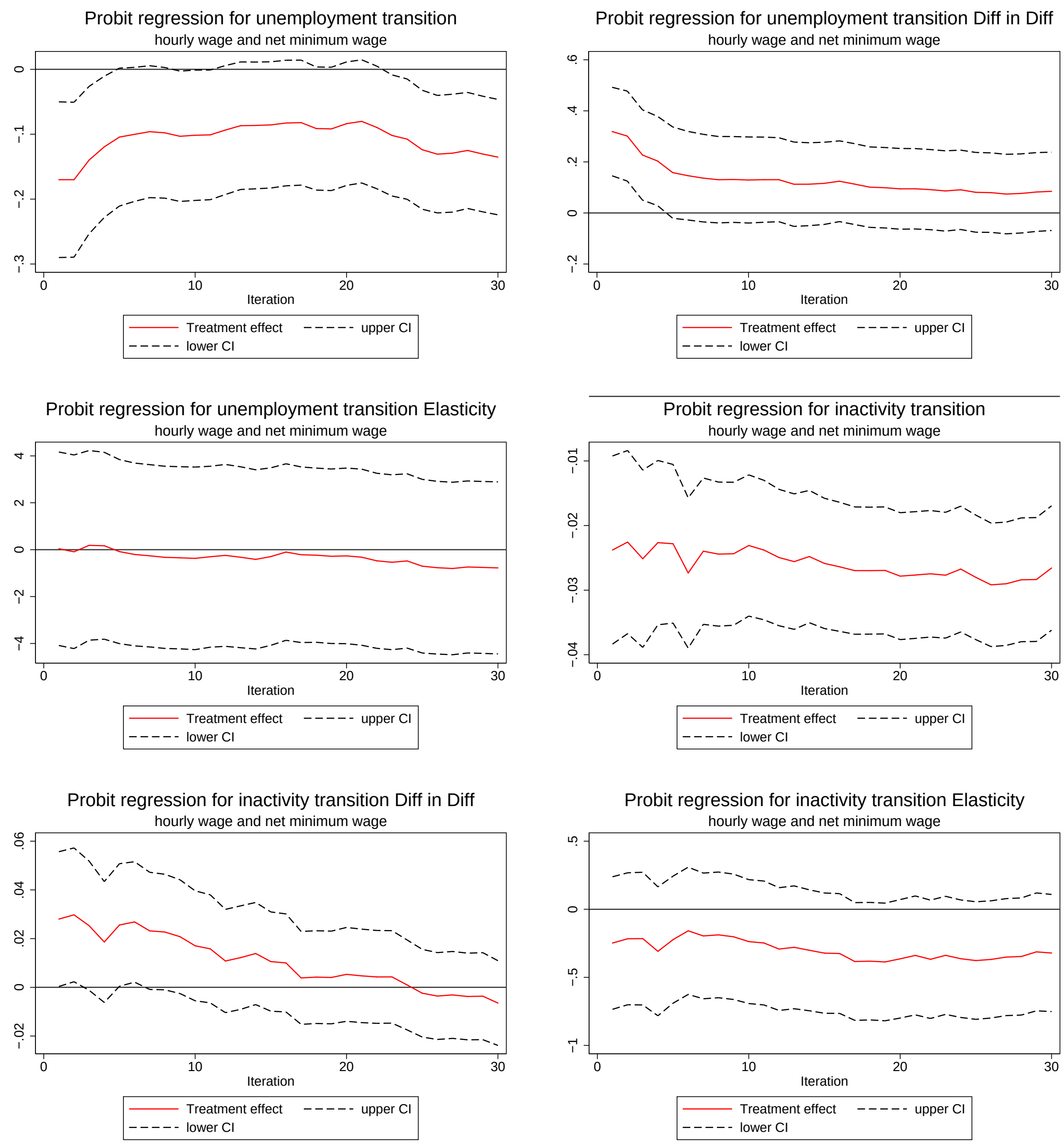
Looking the diff-in-diff estimation, our best strategy of identification, we can conclude that people affected by the MW increasing have a $3-4\%$ less of probability of keeping the job. From theses values, and an important point for the Chilean debate, we can think that the big MW increases in the late 90's did contribute to a rise in unemployment, although the magnitude is much smaller than what is usually attributed, the effect is in the range of $((0-8\%)*(5\%) \leq 0.32\%)$ per year. In fact, in our diff and diff estimation the upper confidence interval, with a 95%, is less than 8%, even depending on the control group this could be not significative. Moreover, the treatment group represents around 5% of the labor force. Given this estimation, we cannot attribute most of the variance of unemployment to the minimum wage.

5.4 Job Finding

Figure 7 shows the estimation of parameter β_1 of Equations 4, 9, 11 for the case where dependent variable T is probability of finding a job for the "theoretically" treated group that we construct using a Mincer equation. We run separate Mincer equations by year like the one in Equation 5 for two groups of workers: unemployed and inactive. Table 9 shows the results of these estimations. We then use these parameters to estimate a theoretical wage that will help us assign the worker to the treated or control group.

Table 7 and 8 **??** of the Appendix shows a sample of the complete estimation results for the three strategies: iterations 1, 15 and 30. Figures 18, 19 and 20 of the Appendix show the 36 different combinations of estimation forms and strategies for unemployment; and figures 21, 22 and 23 for inactivity.

Figure 7: Finding an Employment



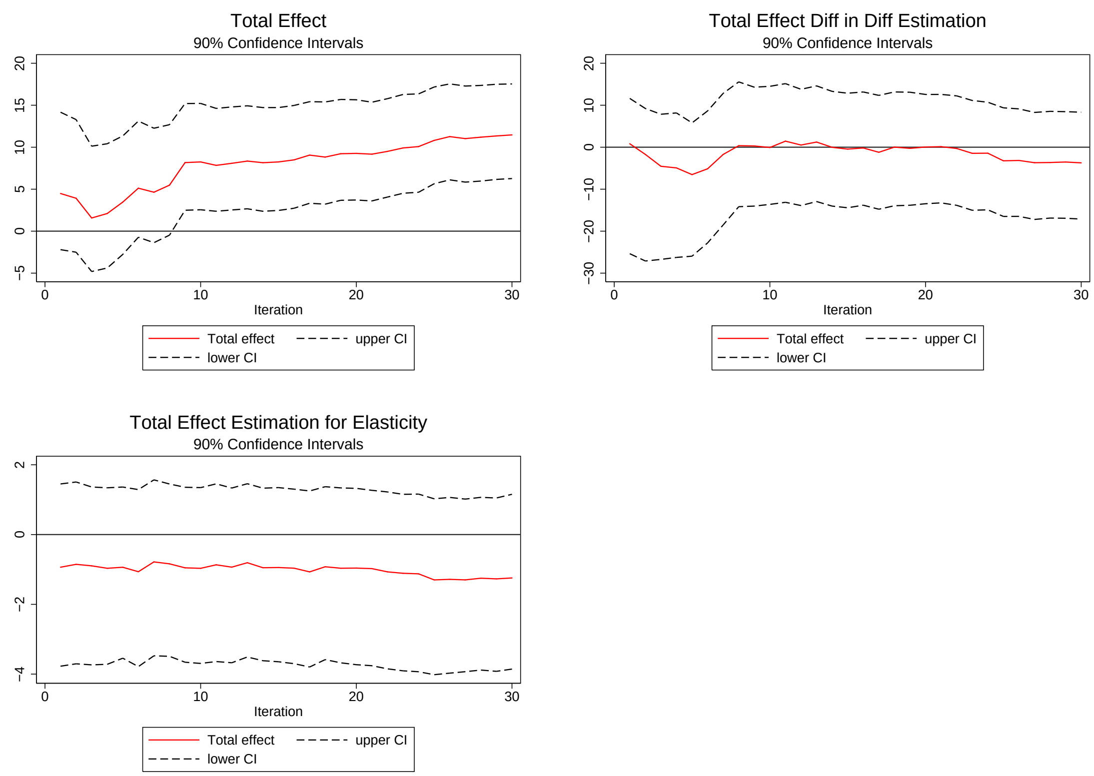
This is probably the estimation where results most differ among the three estimation strategies. The "simple Estimation" regressions show a significant yet small in magnitude reduction in the probability of finding a job for inactive people and bigger for unemployed. The "Elasticity" regression does not show significant effects. The "Diff-in-diff" method, on the other hand, shows a different result: the effect seems to be positive (even mildly significant),but that converges to zero, in both estimation: inactivity and unemployment transitions. Although statistically this does not contradict the other results, and although a positive effect is theoretically possible, it is still troubling.

We report the effect of MW policy on the probability of finding jobs of unemployed and inactive workers. We see that the effects are similar in form and significance, but very different in magnitude. All the effects on the unemployed are around ten times larger in percentage terms (regardless the sign) than the effects on the inactive. Of course since the inactive are around ten times the unemployed (on average) the absolute effect is similar. We conclude that we do not have evidence to show that there is any clear significant effect of the MW on the probability of finding a job.

5.5 Total Effect

Finally we estimate the total effect of the MW policy on workers income. We use the elasticities estimated in the previous sections to simulate an effect on the income of a working family that has treated workers and theoretically treated unemployed and inactive in the same proportions that there are each of these types of workers in the economy. To generate confidence intervals we simulate 1,000 samples and re-estimate the whole set of regressions using a standard Bootstrap method (with replacement).

Figure 8: Total Effects



We find that there is weak evidence of a positive effect on workers income or, at the very least, of no effect at all. We find positive and significant effects from the "Simple

Estimation" strategy and non significant effects from the other two methodologies.

6 Conclusions

We develop a methodology that allows a better identification of the effect of MW laws in emerging markets by developing a labor panel from national employment surveys that are commonly available in most emerging economies. A widespread application of this methodology would greatly increase the scope of the empirical study of MW laws in emerging economies with the additional virtue of substantially increasing comparability.

We apply this methodology to the Chilean National Employment Survey to test the short term impacts of minimum wage increases during the 1996-2005 period. We estimate wage increase effects for the treated group (people earning wages between ex ante an ex post minimum wages), the hours worked and the employment effects for this group. We also estimate the effect on the probability of obtaining a job for a theoretical treated group of unemployed and inactive workers constructed by estimating their likely wage in the case that they found one. We then estimate the integral of these three effects (wage increase, wage loss and lower probability of obtaining a job). We find that minimum wage increases do have a significant impact on the wages of the treated group, hence the suspicion that they are somehow made irrelevant by informal practices in Chilean labor markets seems to be unfunded. We find that there is a significant but modest negative effect on the probability of staying employed, we show also that there are not clear effect on hours worked and on the probability of finding the job. We find that the integral of the three effects is positive and has statistical significance. Finally, we show that the integral of the three effects is sometimes positive and significant, other times has not sadistical significance. We think that, in general, minimum wage increases in Chile during the aforementioned period have increased the real income of treated and potentially treated workers. However, we also find that there is a redistribution of income among these workers in favor of currently employed workers. We conclude that, if anything, minimum wage increases have generated real income redistribution towards the treated workers as well as among them.

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7 Appendix

7.1 Summary of results

Legend	
Always	a
Almost Always	aa
Sometimes	s
Never	n
Reverse sign	rs

Summarize the statistical significance of the parameters													
		Simple Estimation				Diff in Diff				Elasticity			
		Net MW		Gross MW		Net MW		Gross MW		Net MW		Gross MW	
		Hour	Month	Hour	Month	Hour	Month	Hour	Month	Hour	Month	Hour	Month
Labor Income	Pure	a	a	a	a	aa	n	s	n	n	a	n	n
	Weighted	a	a	s	s	n	n	s	s	n	n	n	n
Hours	Pure	aa	n	s	n	n	s	n	s	n	n	n	n
	Weighted	n	n	aa	n	n	n	n	n	n	n	n	n
Employment	Pure	s	s	s	aa	aa	s	n	n	a	a	aa	a
	Weighted	s	s	s	aa	n	n	n	n	aa	aa	n	n
Unemployment	Pure	aa	a	aa	a	n	rs-s	s	a	n	n	n	n
	Weighted	a	aa	av	a	n	rs-s	s	n	n	n	n	n
Inactivity	Pure	a	a	a	a	rs-s	rs-s	s	s	n	n	a	a
	Weighted	a	a	a	a	n	n	aa	aa	s	s	aa	aa
Non Employment	Pure	a	a	a	a	rs-s	rs-s	aa	aa	s	s	a	a
	Weighted	a	a	a	a	n	n	a	a	aa	aa	a	a

7.2 For Equations 3, 8, 10 with labor income as dependent variable

Table 4: Impact on labor income for three different controls groups

Iteration	j=0			j=15			j=30		
Strategy	1	2	3	1	2	3	1	2	3
D			0,394			**0,629**			0,583
			(0,369)			(0,350)			(0,346)
DT*TI		0,012			**0,038**			**0,031**	
		(0,024)			(0,016)			(0,015)	
DT	**0,043**	0,038	0,037	**0,056**	0,038	0,046	**0,071**	0,059	0,063
	(0,012)	(0,015)	(0,013)	(0,008)	(0,011)	(0,009)	(0,007)	(0,009)	(0,009)
TI		0,228			0,210			0,410	
		(0,016)			(0,007)			(0,011)	
1997	0,185	0,189	0,189	0,206	0,209	0,207	0,183	0,185	0,184
	(0,022)	(0,023)	(0,022)	(0,011)	(0,011)	(0,011)	(0,010)	(0,010)	(0,010)
1998	0,368	0,140	0,369	0,375	0,164	0,375	0,342	-0,069	0,342
	(0,030)	(0,021)	(0,030)	(0,016)	(0,011)	(0,016)	(0,013)	(0,013)	(0,013)
1999	0,533	0,072	0,530	0,510	0,083	0,507	0,460	-0,366	0,458
	(0,038)	(0,014)	(0,038)	(0,019)	(0,008)	(0,019)	(0,016)	(0,019)	(0,016)
2000	0,693	(dropped)	0,690	0,641	(dropped)	0,639	0,576	-0,663	0,574
	(0,044)		(0,044)	(0,021)		(0,021)	(0,018)	(0,026)	(0,018)
2001	0,827	0,138	0,825	0,760	0,124	0,758	0,677	-0,559	0,675
	(0,050)	(0,016)	(0,050)	(0,024)	(0,011)	(0,024)	(0,020)	(0,024)	(0,020)
2002	0,988	0,303	0,987	0,906	0,277	0,905	0,813	-0,419	0,813
	(0,057)	(0,025)	(0,057)	(0,027)	(0,016)	(0,027)	(0,022)	(0,022)	(0,022)
2003	1,116	0,434	1,115	1,042	0,422	1,042	0,937	-0,291	0,937
	(0,067)	(0,040)	(0,067)	(0,034)	(0,025)	(0,034)	(0,026)	(0,017)	(0,026)
2004	1,261	0,584	1,264	1,193	0,578	1,195	1,072	-0,153	1,073
	(0,078)	(0,054)	(0,078)	(0,039)	(0,032)	(0,039)	(0,031)	(0,007)	(0,031)
2005	1,442	0,770	1,448	1,368	0,763	1,374	1,223	(dropped)	1,225
	(0,087)	(0,066)	(0,087)	(0,043)	(0,038)	(0,043)	(0,032)		(0,032)
Constant	5,381	5,834	5,382	5,513	5,877	5,514	5,598	6,345	5,598
	(0,040)	(0,016)	(0,040)	(0,018)	(0,008)	(0,018)	(0,015)	(0,012)	(0,015)
Observations	7072	7072	7072	15536	15536	15536	242218	242218	242218
R-Square	0,4252	0,4251	0,4252	0,3902	0,3906	0,3904	0,4134	0,4136	0,4135

Figure 9: Simple Estimation

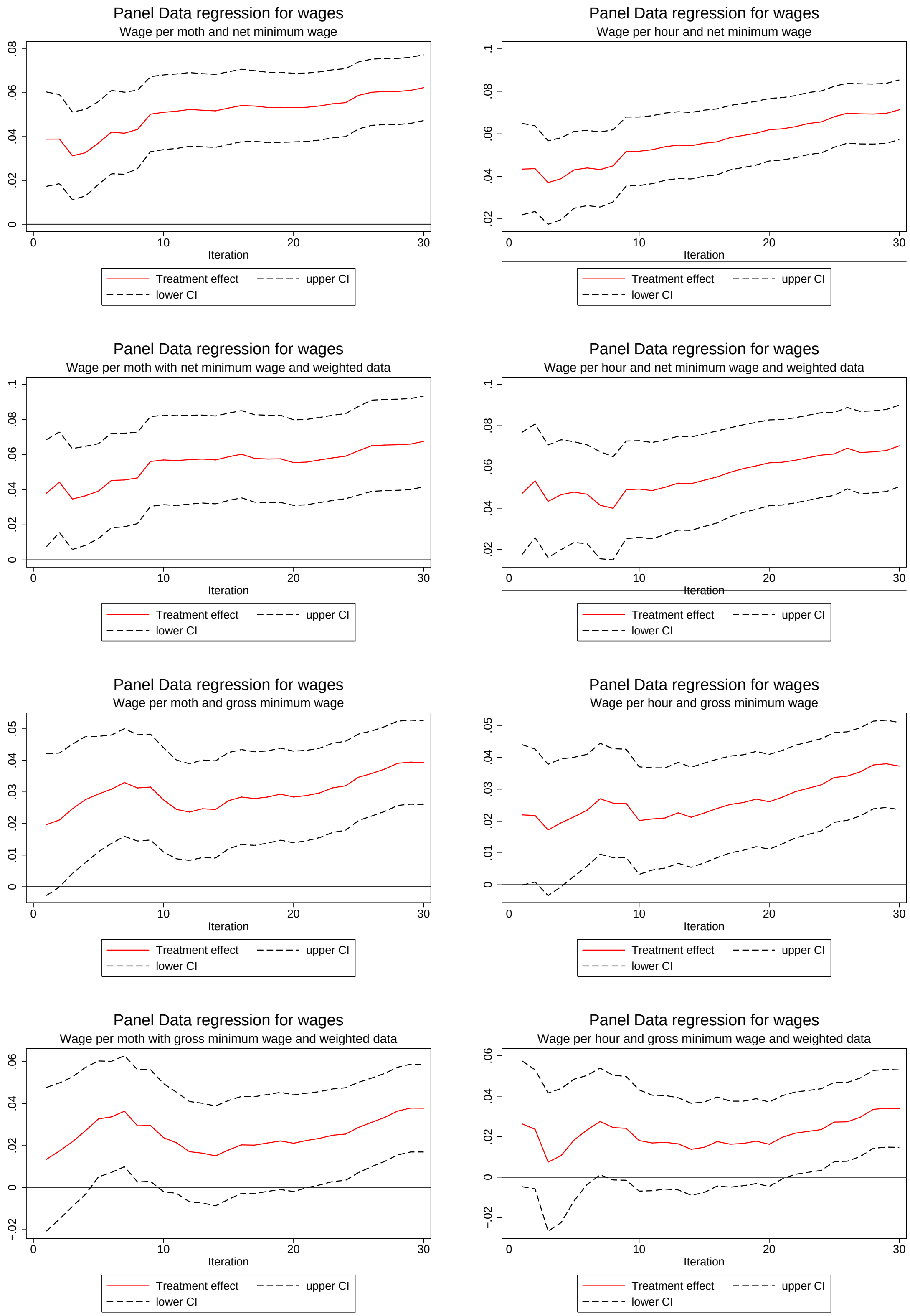


Figure 10: Diff in Diff

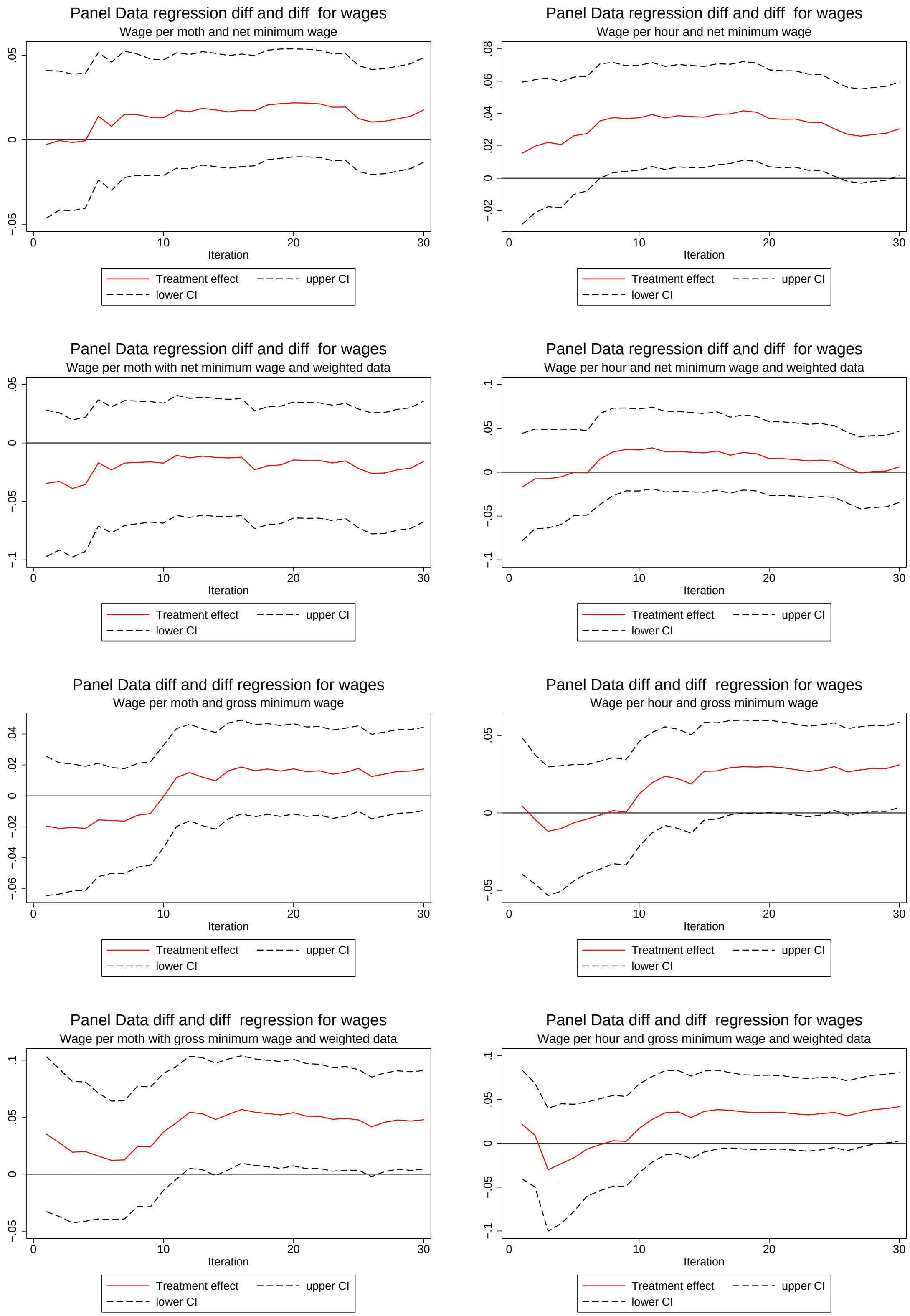
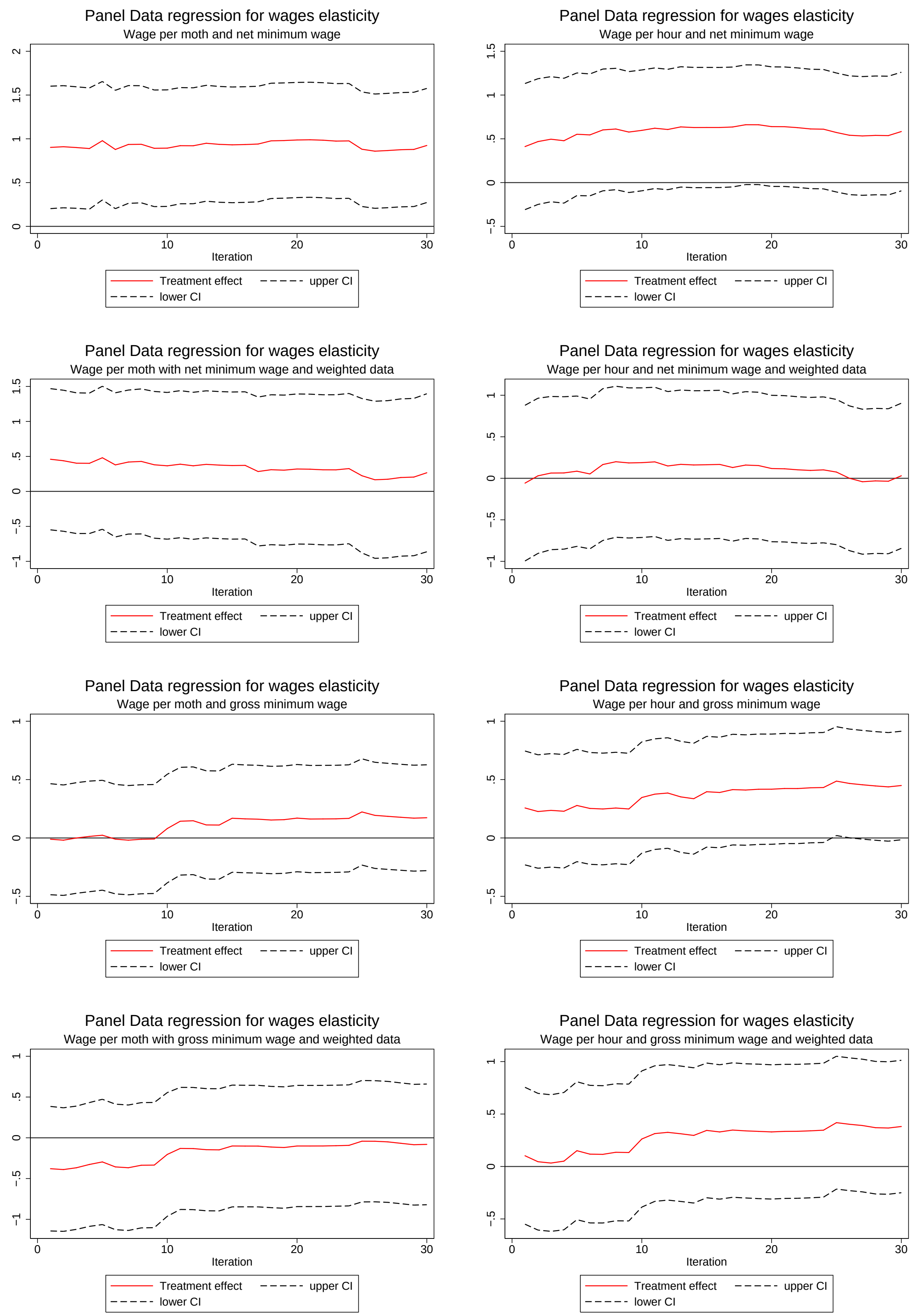


Figure 11: Elasticity



7.3 For Equations 3, 8, 10 with hours worked as dependent variable

Table 5: Impact on Hours worked for three different controls groups

Iteration		j=0			j=15			j=30	
Strategy	1	2	3	1	2	3	1	2	3
D			0,032			-0,038			-0,003
			(0,189)			(0,178)			(0,174)
DT*TI		-0,009			-0,012			-0,008	
		(0,012)			(0,008)			(0,008)	
DT	-0,004	0,000	-0,005	**-0,009**	-0,004	-0,009	**-0,011**	-0,008	-0,011
	(0,006)	(0,008)	(0,007)	(0,004)	(0,006)	(0,005)	(0,004)	(0,006)	(0,005)
TI		-0,017			-0,006			-0,033	
		(0,009)			(0,003)			(0,005)	
1997	-0,014	-0,017	-0,014	-0,005	-0,006	-0,005	-0,004	-0,004	-0,004
	(0,012)	(0,012)	(0,012)	(0,006)	(0,006)	(0,006)	(0,005)	(0,005)	(0,005)
1998	-0,038	-0,020	-0,038	-0,012	-0,006	-0,012	-0,010	0,023	-0,010
	(0,015)	(0,010)	(0,015)	(0,008)	(0,005)	(0,008)	(0,006)	(0,006)	(0,006)
1999	-0,049	-0,011	-0,049	-0,018	-0,004	-0,018	-0,015	0,052	-0,015
	(0,019)	(0,007)	(0,019)	(0,009)	(0,004)	(0,009)	(0,007)	(0,010)	(0,007)
2000	-0,058	(dropped)	-0,059	-0,022	(dropped)	-0,021	-0,017	0,083	-0,017
	(0,023)		(0,023)	(0,010)		(0,010)	(0,008)	(0,014)	(0,008)
2001	-0,061	-0,005	-0,061	-0,027	-0,008	-0,027	-0,020	0,079	-0,020
	(0,026)	(0,009)	(0,026)	(0,012)	(0,006)	(0,012)	(0,009)	(0,013)	(0,009)
2002	-0,073	-0,019	-0,073	-0,033	-0,016	-0,033	-0,023	0,076	-0,023
	(0,028)	(0,011)	(0,028)	(0,013)	(0,008)	(0,013)	(0,010)	(0,012)	(0,010)
2003	-0,079	-0,030	-0,079	-0,037	-0,022	-0,037	-0,030	0,068	-0,030
	(0,033)	(0,017)	(0,033)	(0,016)	(0,012)	(0,016)	(0,012)	(0,010)	(0,012)
2004	-0,074	-0,027	-0,074	-0,050	-0,037	-0,051	-0,041	0,056	-0,041
	(0,038)	(0,023)	(0,038)	(0,020)	(0,017)	(0,020)	(0,015)	(0,005)	(0,015)
2005	-0,146	-0,103	-0,145	-0,113	-0,104	-0,114	-0,096	(dropped)	-0,096
	(0,043)	(0,030)	(0,043)	(0,022)	(0,020)	(0,022)	(0,016)		(0,016)
Constant	5,304	5,268	5,304	5,271	5,260	5,271	5,268	5,208	5,268
	(0,020)	(0,007)	(0,020)	(0,009)	(0,004)	(0,009)	(0,007)	(0,006)	(0,007)
Observations	7072	7072	7072	15536	15536	15536	242218	242218	242218
R-Square	0,3851	0,385	0,3849	0,3656	0,3657	0,3655	0,3749	0,3749	0,4135

Figure 12: Simple Estimation

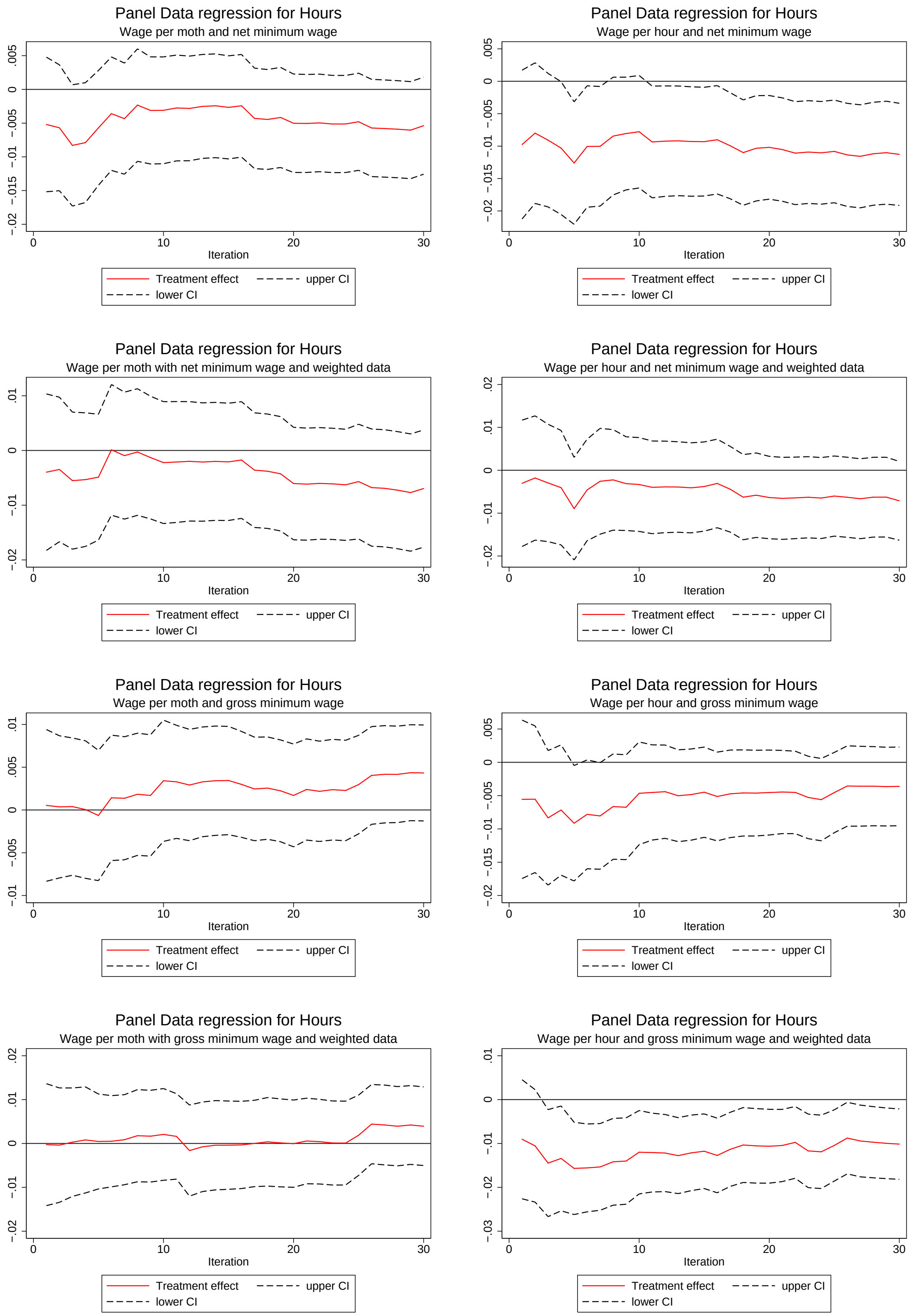


Figure 13: Diff in Diff

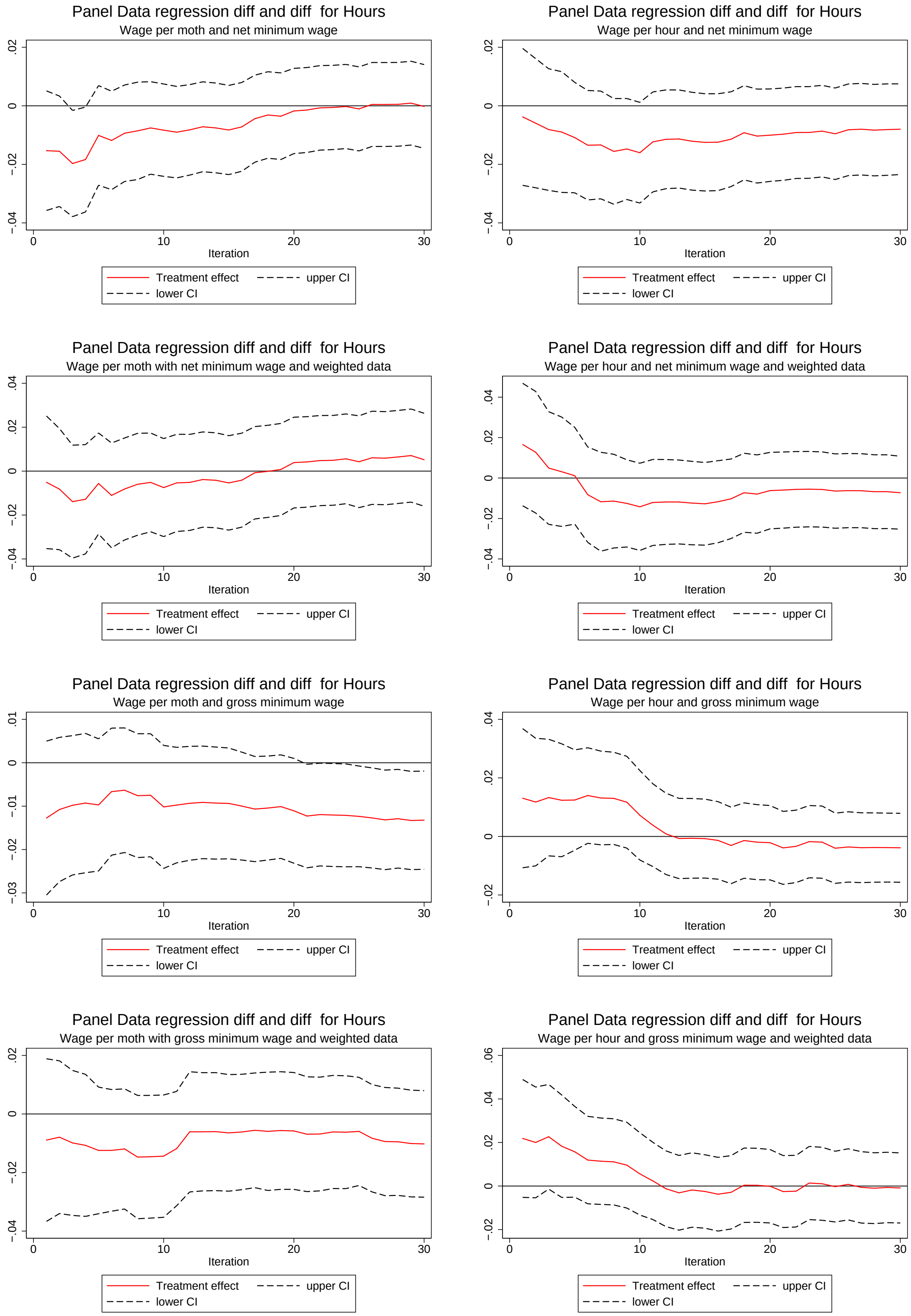
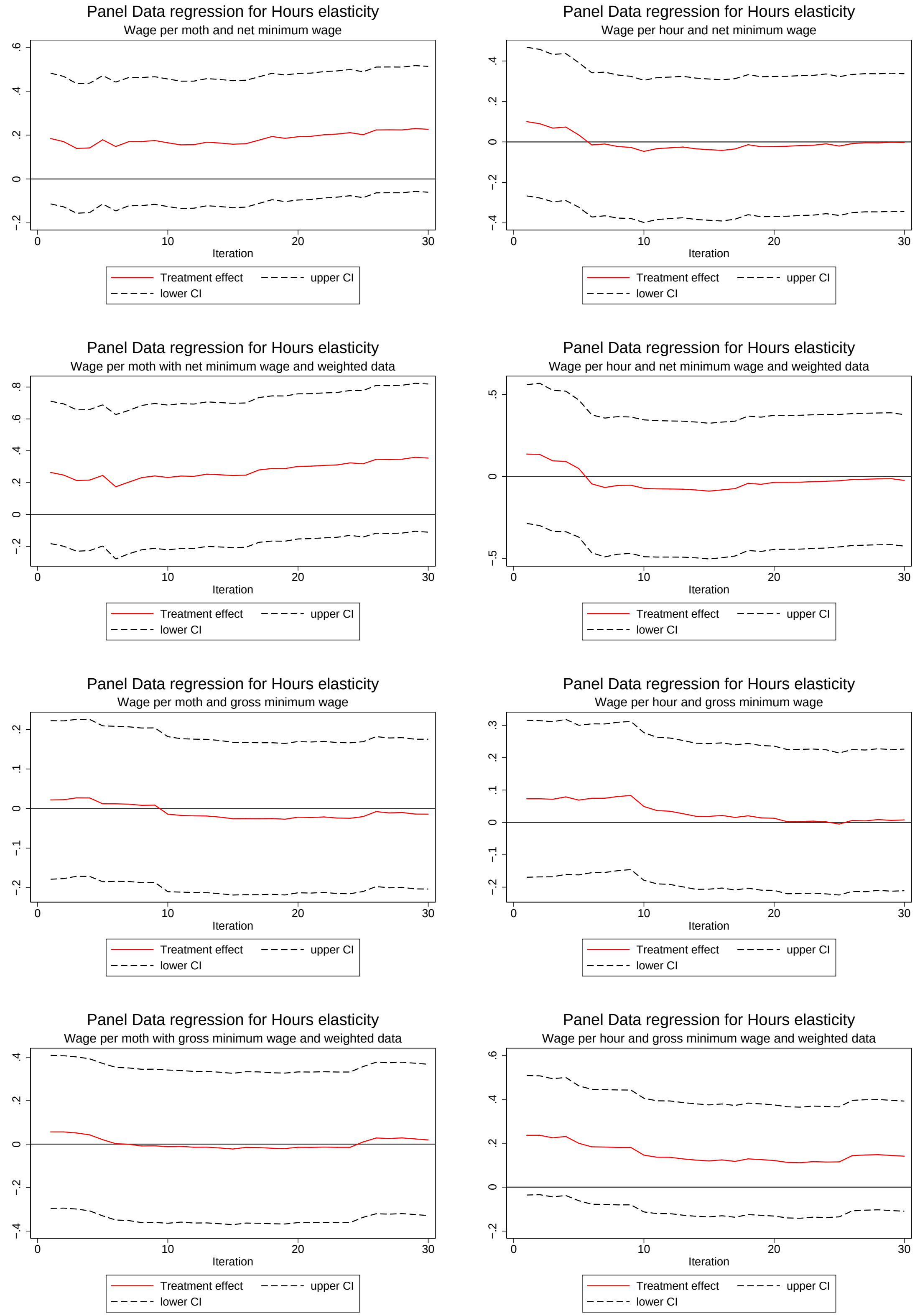


Figure 14: Elasticity



7.4 For Equations ??, 9, 11 with probability of keeping a job as dependent variable

Table 6: Impact on probability of keeping a job for three different controls groups

Iteration		j=0			j=15			j=30	
Strategy	1	2	3	1	2	3	1	2	3
D			-3.438*			-4.183**			-4.522**
			(1,490)			(1,414)			(1,384)
DT*TI		-0,092			-0.160*			-0.174**	
		(0,103)			(0,071)			(0,065)	
DT	-0,037	0,006	0,018	**-0,067**	0,010	0,002	**-0.075***	0,002	-0,003
	(0,051)	(0,071)	(0,057)	(0,036)	(0,049)	(0,043)	(0,032)	(0,044)	(0,040)
TI		-0,229			-0,090			-0,041	
		(0,136)			(0,068)			(0,057)	
1997	-0.288*		-0.238*	-0,126		-0,106	-0,069		-0,054
	(0,119)		(0,120)	(0,066)		(0,066)	(0,056)		(0,056)
1998	-0,222	0,074	-0,145	-0,027	0,106	0,008	0,000	0,075	0,027
	(0,114)	(0,086)	(0,119)	(0,063)	(0,058)	(0,065)	(0,054)	(0,049)	(0,055)
1999	-0,203	0,086	-0,176	-0,058	0,068	-0,050	-0,059	0,010	-0,053
	(0,117)	(0,090)	(0,118)	(0,065)	(0,059)	(0,065)	(0,055)	(0,050)	(0,055)
2000	-0,128	-0,125	-0,101	-0,022	-0,034	-0,015	-0,095	-0,102	-0,090
	(0,126)	(0,126)	(0,126)	(0,076)	(0,076)	(0,076)	(0,061)	(0,061)	(0,061)
2001	-0.225*	-0,221	-0,205	-0,101	-0,118	-0,101	-0,057	-0,067	-0,055
	(0,114)	(0,114)	(0,114)	(0,068)	(0,068)	(0,068)	(0,057)	(0,057)	(0,057)
2002	-0,184	-0,192	-0,161	-0,055	-0,079	-0,052	-0,053	-0,067	-0,050
	(0,133)	(0,134)	(0,134)	(0,084)	(0,084)	(0,084)	(0,068)	(0,068)	(0,068)
2003	-0,033	-0,030	-0,033	0,051	0,042	0,042	0,012	0,003	0,004
	(0,145)	(0,145)	(0,145)	(0,091)	(0,091)	(0,091)	(0,078)	(0,078)	(0,078)
2004	-0,111	-0,120	-0,110	-0,007	-0,037	-0,021	-0,018	-0,024	-0,020
	(0,128)	(0,128)	(0,128)	(0,083)	(0,083)	(0,083)	(0,058)	(0,058)	(0,058)
Age	0,002	0,002	0,002	0,002	0,002	0,002	0.003*	0.003*	0.003*
	(0,002)	(0,002)	(0,002)	(0,002)	(0,002)	(0,002)	(0,001)	(0,001)	(0,001)
Gender	0.671***	0.669***	0.667***	0.704***	0.703***	0.702***	0.723***	0.721***	0.721***
	(0,058)	(0,058)	(0,058)	(0,040)	(0,040)	(0,040)	(0,032)	(0,032)	(0,032)
Constant	0.699***	0.671**	0.663**	0.574***	0.559***	0.560***	0.523***	0.514***	0.513***
	(0,202)	(0,205)	(0,203)	(0,138)	(0,138)	(0,138)	(0,116)	(0,116)	(0,116)
Pseudo	0,063	0,063	0,064	0,067	0,068	0,068	0,066	0,067	0,067
R2	4721	4721	4721	10137	10137	10137	15686	15686	15686

Figure 15: Simple Estimation

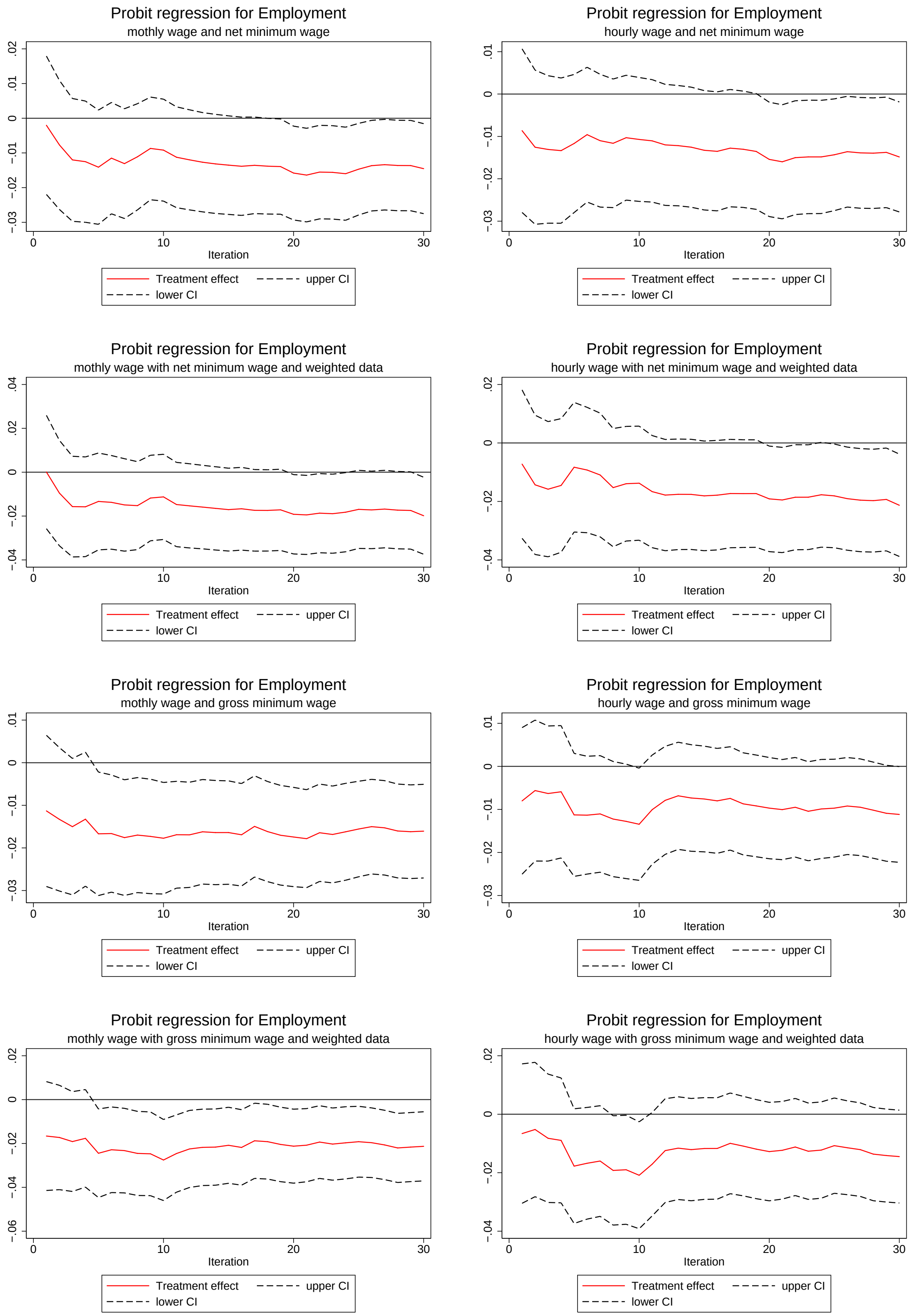


Figure 16: Diff in Diff

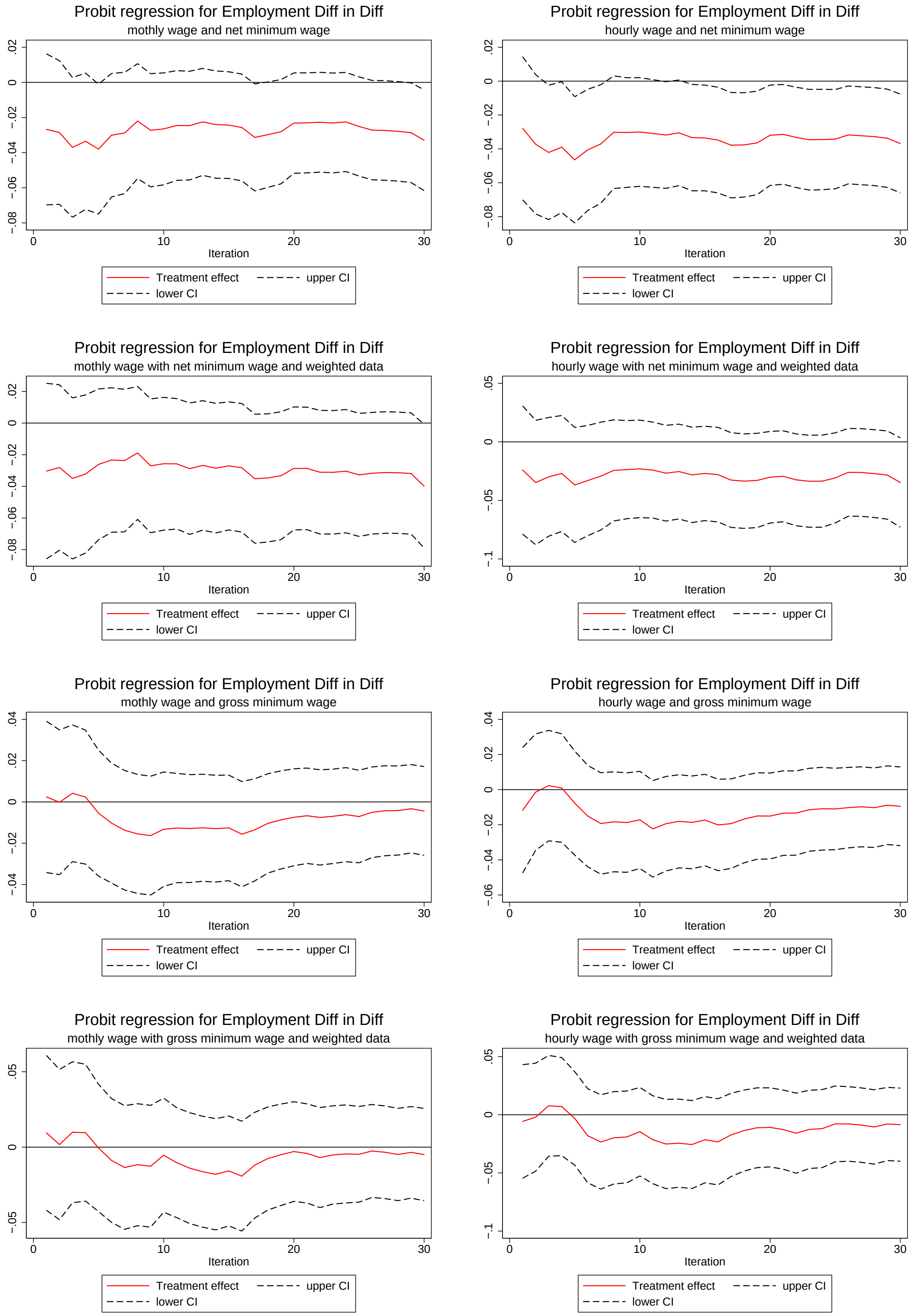
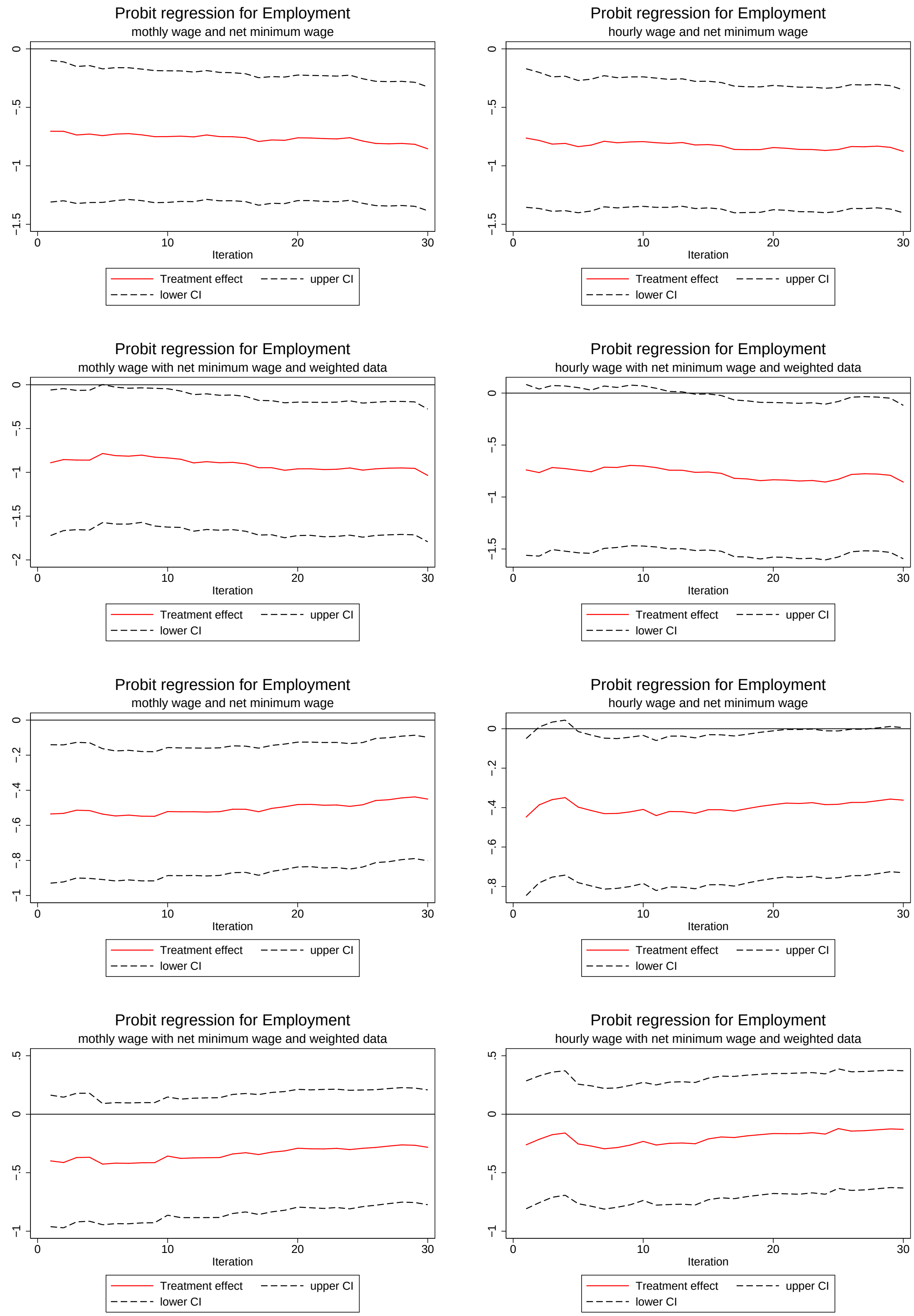


Figure 17: Elasticity



7.5 For Equations ??, 9, 11 with probability of finding a job as dependent variable (for unemployment and inactivity)

7.5.1 Unemployment

Table 7: Impact on probability for unemployed of finding a job for three different controls groups

Iteration		j=0			j=15			j=30	
Strategy	1	2	3	1	2	3	1	2	3
D			-6,406			-8,620			-8,888
			(6,415)			(5,946)			(5,904)
DT*TI		0.800*			0,259			0,231	
		(0,389)			(0,281)			(0,271)	
DT	-0,368	-0.708**	-0,335	**-0.333***	-0.448*	-0,237	**-0.367***	-0.472*	-0,258
	(0,210)	(0,241)	(0,216)	(0,163)	(0,189)	(0,174)	(0,156)	(0,184)	(0,170)
TI		-0,749			-0,322			-0,354	
		(0,506)			(0,261)			(0,210)	
1997	-0,159		-0,018	-0,239		-0,165	-0,308		-0,259
	(0,417)		(0,430)	(0,253)		(0,255)	(0,206)		(0,208)
1998	0,130	0,198	0,326	0,217	0,453	0,303	0,128	0.437*	0,182
	(0,390)	(0,380)	(0,411)	(0,244)	(0,234)	(0,250)	(0,201)	(0,181)	(0,205)
1999	0.983*	1.047**	1.110**	0,389	0.617**	0,435	0,185	0.490**	0,208
	(0,394)	(0,379)	(0,403)	(0,234)	(0,221)	(0,237)	(0,193)	(0,167)	(0,195)
2000	1.064*	0,850	1.164*	0,335	0,300	0,366	0,226	0,212	0,242
	(0,457)	(0,449)	(0,459)	(0,300)	(0,295)	(0,302)	(0,245)	(0,243)	(0,247)
2001	0,416	0,185	0,534	0,224	0,197	0,250	-0,002	-0,009	0,008
	(0,400)	(0,399)	(0,403)	(0,244)	(0,240)	(0,245)	(0,203)	(0,201)	(0,205)
2002	0,754	0,449	0,882	0,551	0,506	0.592*	0,338	0,321	0,357
	(0,516)	(0,510)	(0,527)	(0,285)	(0,282)	(0,288)	(0,233)	(0,231)	(0,235)
2003	0,729	0,428	0,847	0,432	0,389	0,467	0,198	0,182	0,213
	(0,537)	(0,518)	(0,540)	(0,285)	(0,282)	(0,287)	(0,235)	(0,234)	(0,237)
2004	1.366**	1.133*	1.509**	0.610*	0.567*	0.648*	0.491*	0.473*	0.504*
	(0,527)	(0,539)	(0,531)	(0,278)	(0,280)	(0,281)	(0,227)	(0,227)	(0,228)
Age	-0,019	-0,001	-0,033	-0,017	-0,015	-0,020	-0,010	-0,009	-0,012
	(0,025)	(0,026)	(0,026)	(0,013)	(0,013)	(0,013)	(0,008)	(0,008)	(0,008)
Gender	0,749	0.913*	0,596	0.556*	0.579*	0.535*	0.676***	0.689***	0.672***
	(0,390)	(0,406)	(0,397)	(0,239)	(0,242)	(0,239)	(0,149)	(0,151)	(0,148)
Constant	0,996	0,579	1,670	0,254	0,194	0,386	-0,054	-0,095	0,020
	(1,334)	(1,367)	(1,392)	(0,878)	(0,889)	(0,886)	(0,588)	(0,595)	(0,589)
Pseudo R2	0,14	0,153	0,143	0,099	0,1	0,102	0,123	0,123	0,125
N	352	352	352	917	917	917	1423	1423	1423

Figure 18: Simple Estimation

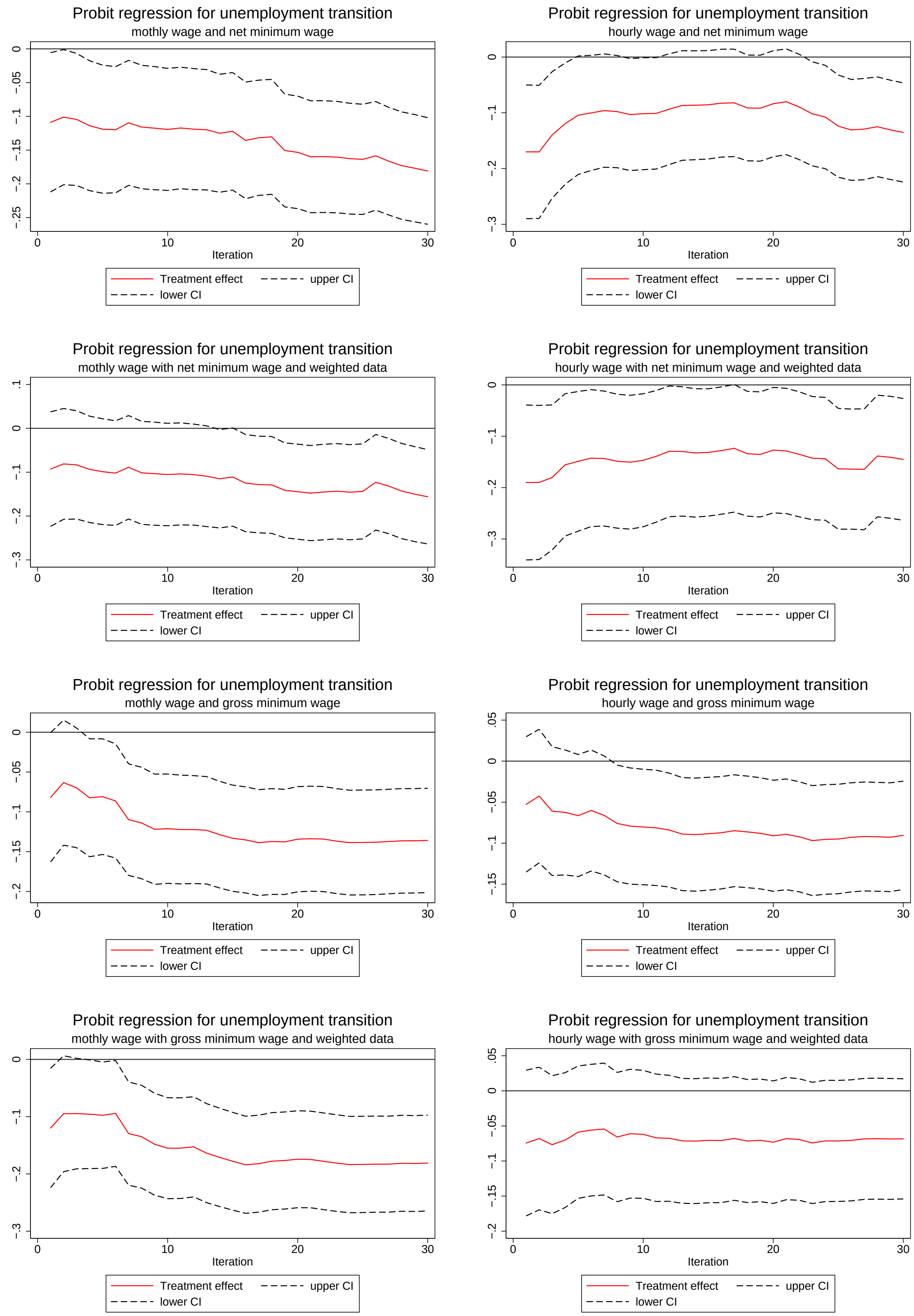


Figure 19: Diff in Diff

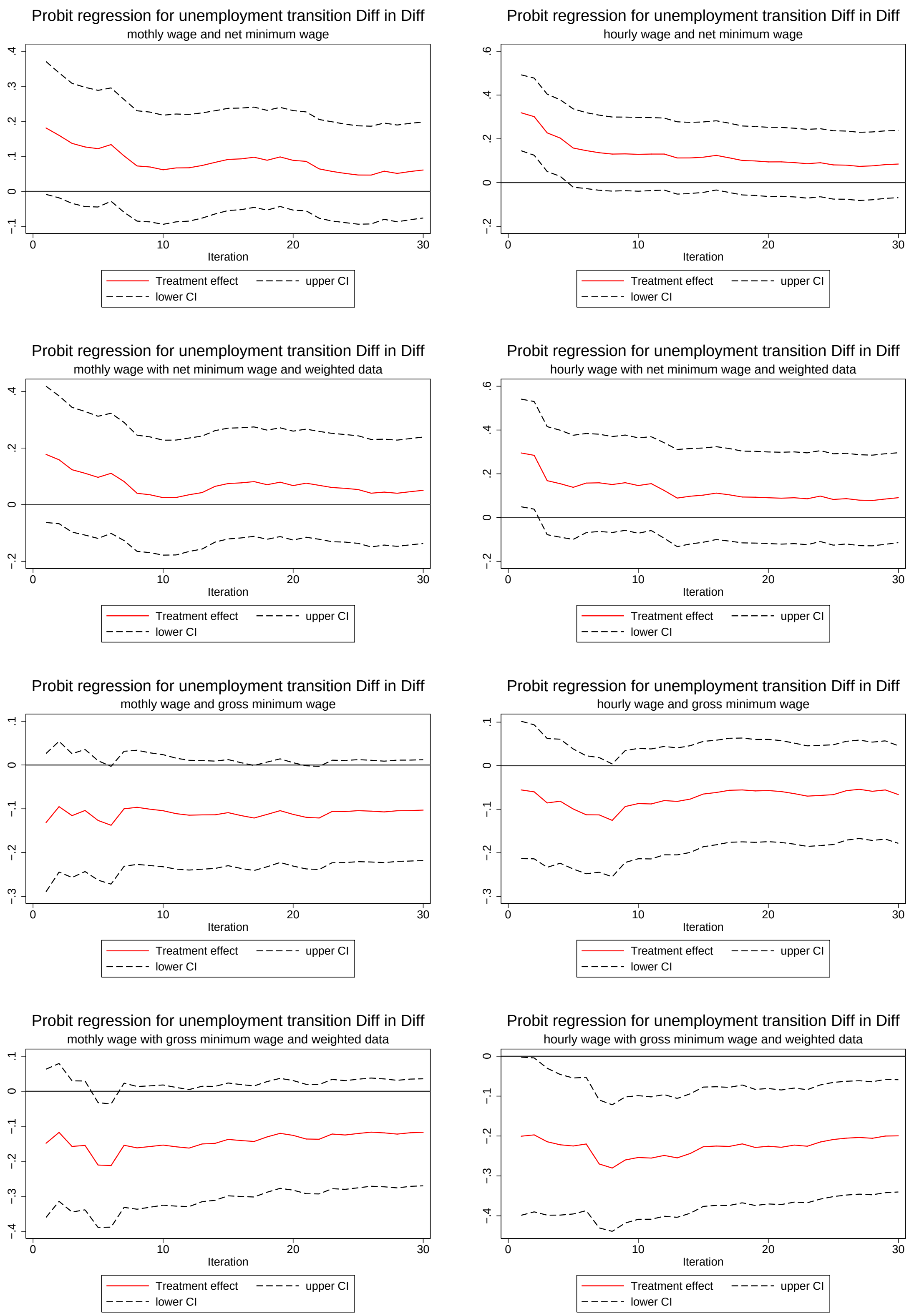
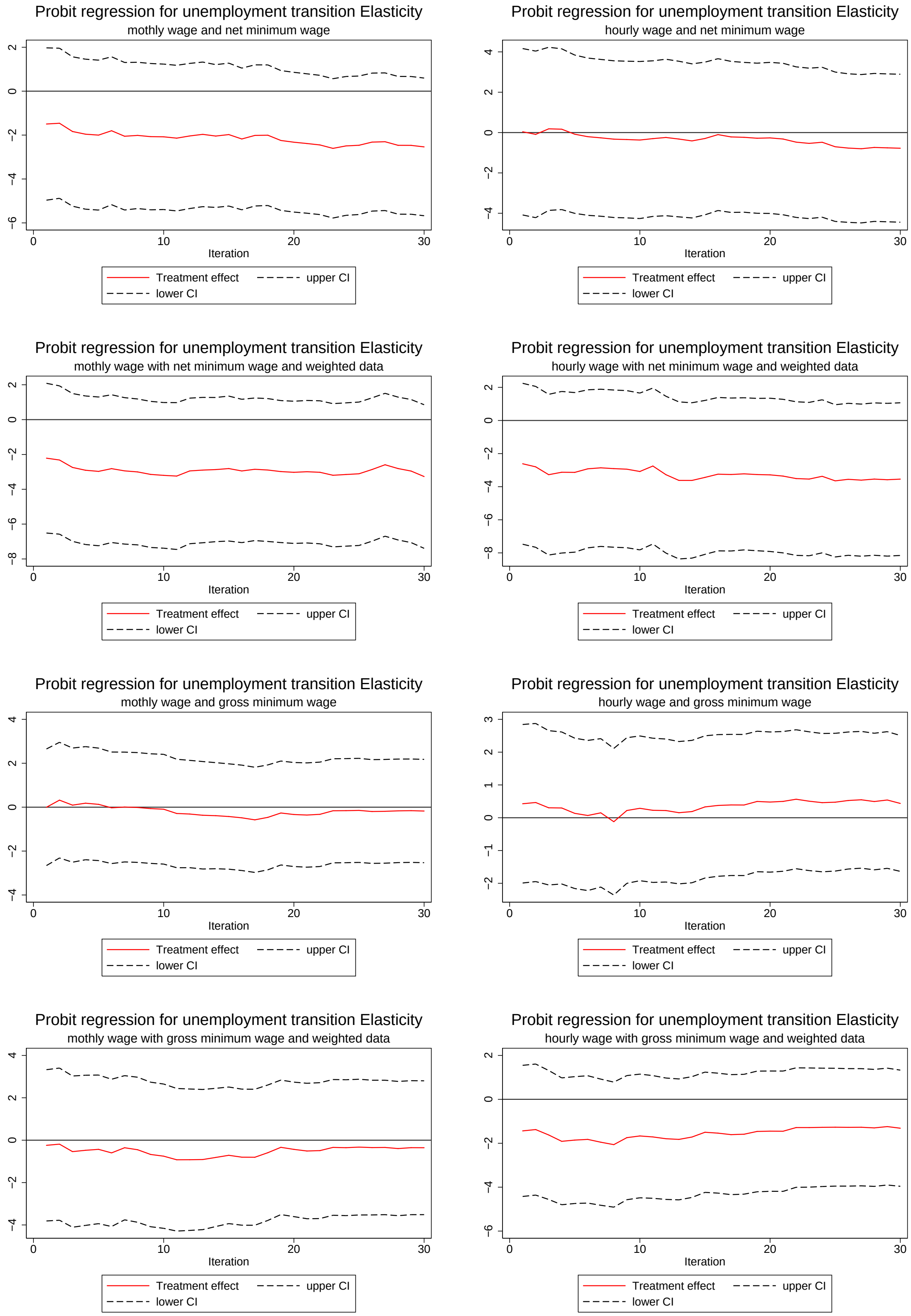


Figure 20: Elasticity



7.5.2 Inactivity

Table 8: Impact on probability for inactive of finding a job for three different controls groups

Iteration		j=0			j=15			j=30	
Strategy	1	2	3	1	2	3	1	2	3
D			-2,826			-2,932			-2,709
			(1,822)			(1,616)			(1,544)
DT*TI		0,032			0,026			-0,084	
		(0,104)			(0,075)			(0,069)	
DT	-0.191**	-0.203**	-0.174**	**-0.228*** **	-0.237***	-0.205***	**-0.235*** **	-0.203***	-0.212***
	(0,059)	(0,069)	(0,060)	(0,044)	(0,051)	(0,046)	(0,041)	(0,048)	(0,044)
TI		-0.238*			0,057			0.134*	
		(0,120)			(0,066)			(0,059)	
1997	-0,060	0,159	-0,017	0.129*	0,066	0.142*	0.120*		0.130*
	(0,109)	(0,098)	(0,116)	(0,063)	(0,067)	(0,064)	(0,058)		(0,059)
1998	-0.219*		-0,166	0,056	-0,007	0,073	0,023	-0,097	0,036
	(0,099)		(0,107)	(0,056)	(0,059)	(0,056)	(0,052)	(0,054)	(0,053)
1999	-0,173	0,042	-0,135	0,065		0,072	0,091	-0,024	0,097
	(0,112)	(0,088)	(0,118)	(0,062)		(0,062)	(0,055)	(0,058)	(0,055)
2000	-0,016	-0,020	0,012	0.203**	0.204**	0.202**	0,074	0,073	0,074
	(0,118)	(0,119)	(0,121)	(0,066)	(0,066)	(0,066)	(0,059)	(0,059)	(0,059)
2001	0,006	0,003	0,037	0.349***	0.350***	0.349***	0.156**	0.156**	0.158**
	(0,115)	(0,115)	(0,119)	(0,063)	(0,064)	(0,063)	(0,055)	(0,055)	(0,055)
2002	0,000	-0,006	0,035	0.263***	0.264***	0.266***	0,118	0,120	0,121
	(0,128)	(0,129)	(0,132)	(0,077)	(0,077)	(0,077)	(0,066)	(0,066)	(0,066)
2003	0,196	0,189	0,225	0.436***	0.436***	0.435***	0.314***	0.316***	0.315***
	(0,134)	(0,136)	(0,137)	(0,084)	(0,084)	(0,084)	(0,067)	(0,067)	(0,067)
2004	-0,074	-0,081	-0,034	0.205**	0.203**	0.211***	0.248***	0.252***	0.252***
	(0,140)	(0,142)	(0,146)	(0,062)	(0,062)	(0,063)	(0,057)	(0,057)	(0,057)
Age	-0,007	-0,006	-0,010	-0.015***	-0.015***	-0.016***	-0.015***	-0.016***	-0.016***
	(0,008)	(0,008)	(0,009)	(0,003)	(0,003)	(0,003)	(0,002)	(0,002)	(0,002)
Gender	0.311**	0.313**	0.287*	0.272***	0.273***	0.270***	-0,038	-0,040	-0,039
	(0,116)	(0,117)	(0,122)	(0,065)	(0,065)	(0,065)	(0,045)	(0,044)	(0,045)
Constant	-1.197**	-1.213**	-1.012*	-0.812***	-0.821***	-0.762***	-0.515**	-0.502**	-0.480**
	(0,413)	(0,418)	(0,451)	(0,188)	(0,192)	(0,192)	(0,159)	(0,159)	(0,161)
Pseudo R2	0,042	0,042	0,043	0,024	0,024	0,025	0,012	0,012	0,012
N	9105	9105	9105	21921	21921	21921	28992	28992	28992

Figure 21: Simple Estimation

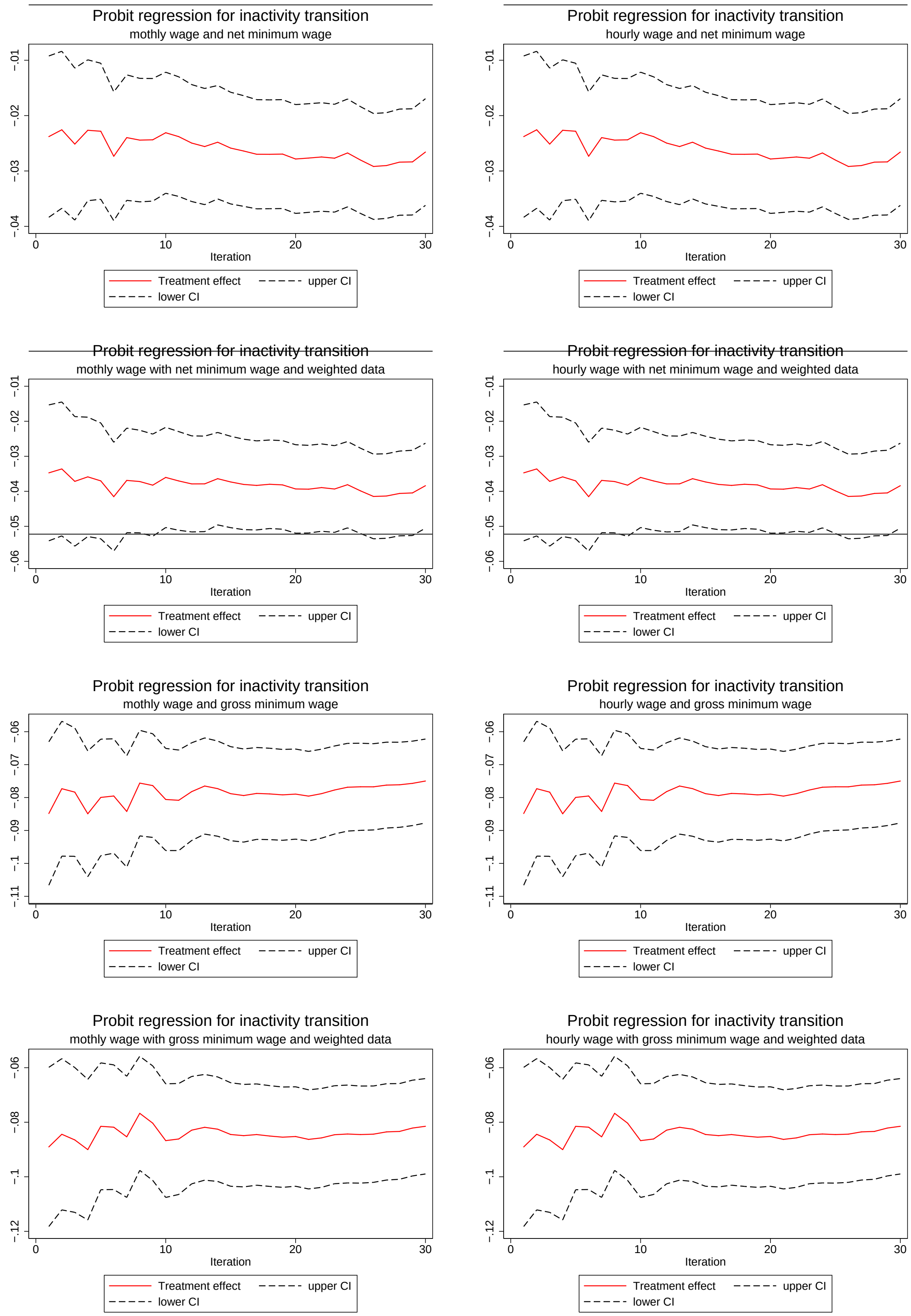


Figure 22: Diff in Diff

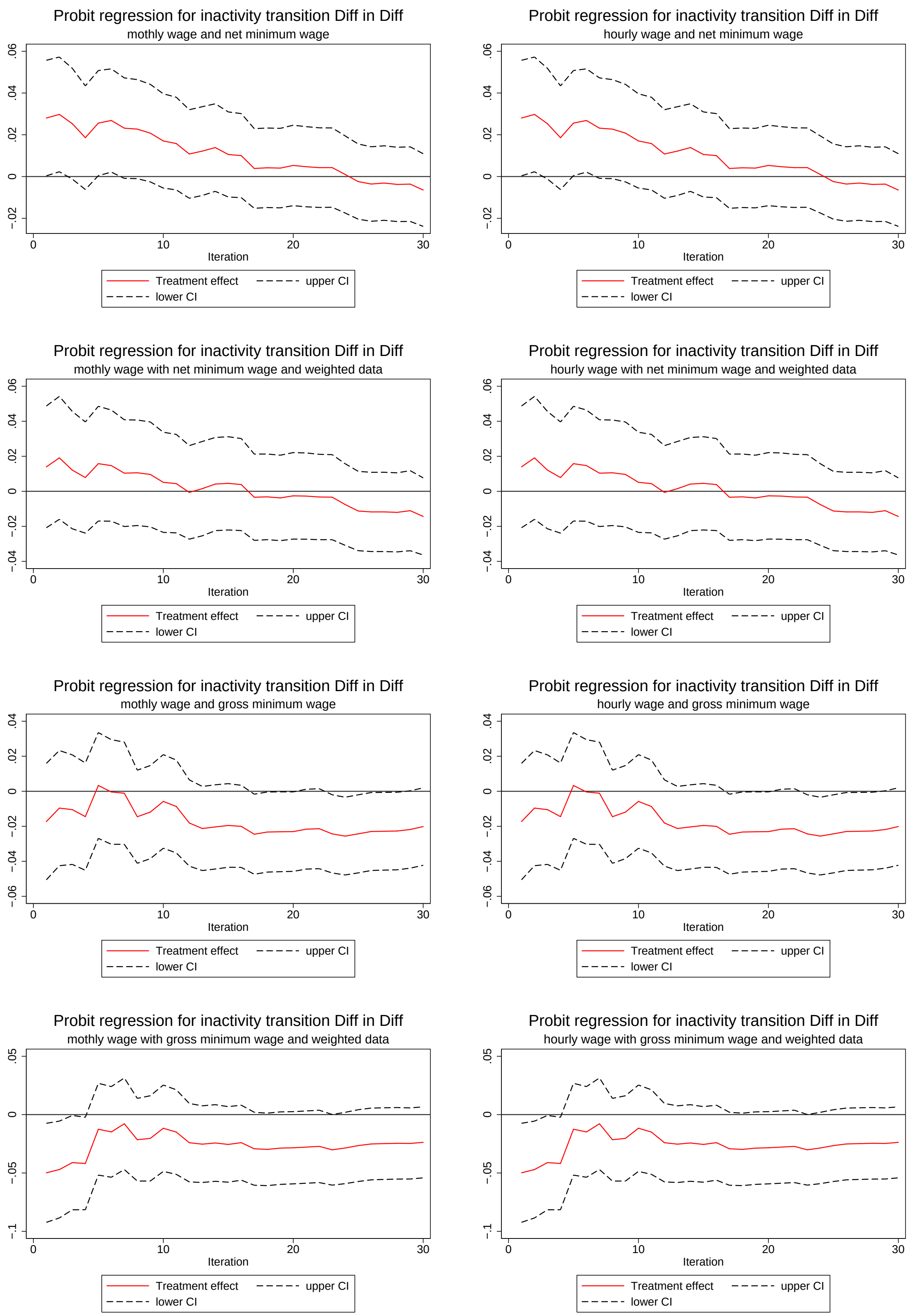


Figure 23: Elasticity

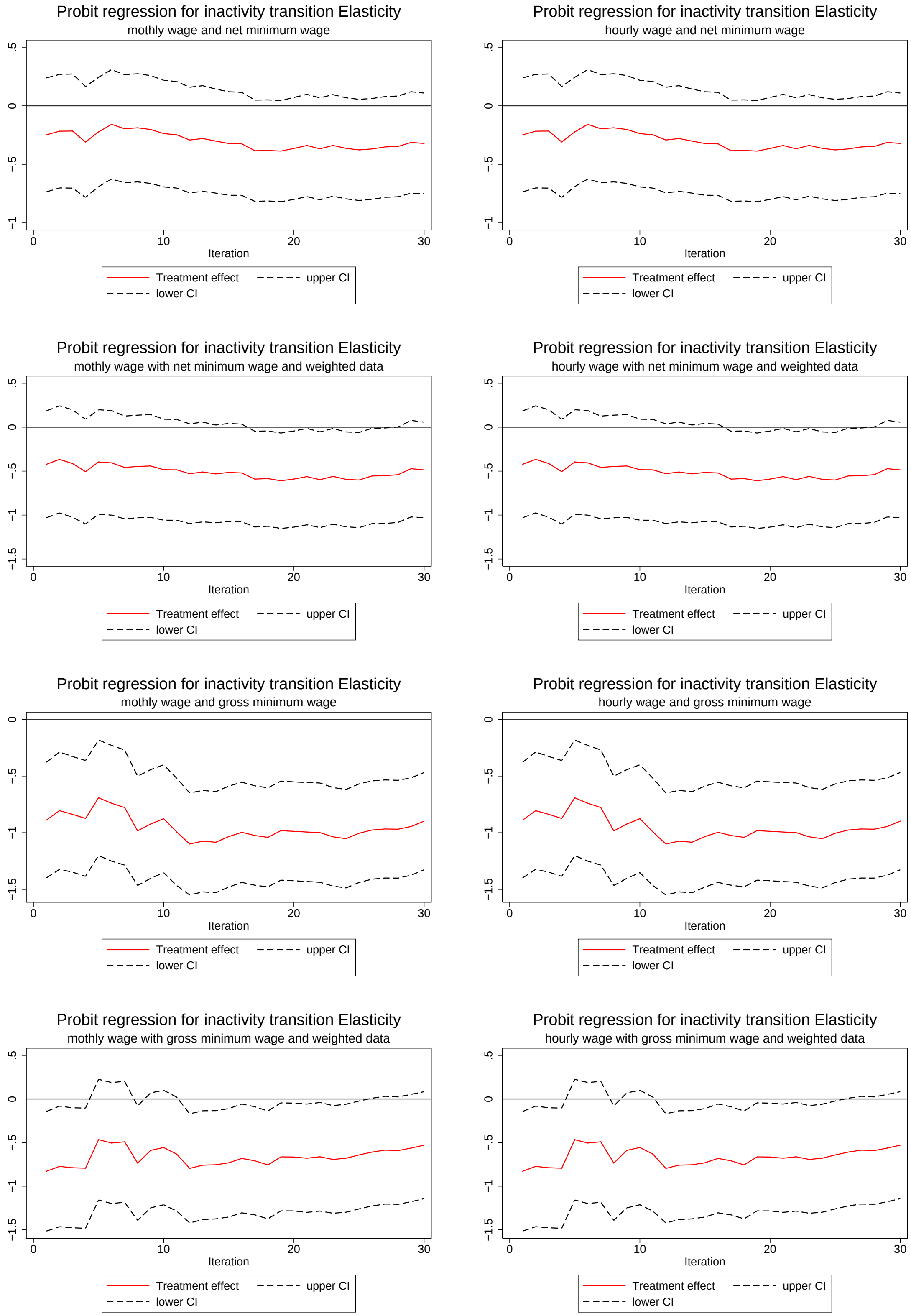


Table 9: Mincer equations by year

ln(hourly wage)	Year								
	1996	1997	1998	1999	2000	2001	2002	2003	2004
Cons	4,627	4,587	4,767	4,759	4,973	5,020	4,876	4,992	4,945
	(0,081)	(0,073)	(0,066)	(0,083)	(0,070)	(0,067)	(0,073)	(0,079)	(0,097)
Age	0,038	0,043	0,041	0,042	0,039	0,038	0,044	0,039	0,047
	(0,003)	(0,003)	(0,003)	(0,003)	(0,003)	(0,003)	(0,003)	(0,003)	(0,004)
Age2	-0,00037	-0,00044	-0,00038	-0,00039	-0,00035	-0,00031	-0,00040	-0,00033	-0,00044
	(0,000043)	(0,000040)	(0,000039)	(0,000041)	(0,000038)	(0,000031)	(0,000037)	(0,000038)	(0,000045)
Gender	0,235	0,262	0,265	0,234	0,246	0,219	0,190	0,206	0,195
	(0,016)	(0,015)	(0,013)	(0,014)	(0,013)	(0,012)	(0,014)	(0,015)	(0,016)
Basica	0,246	0,295	0,199	0,245	0,131	0,137	0,230	0,222	0,163
	(0,043)	(0,040)	(0,039)	(0,054)	(0,042)	(0,046)	(0,047)	(0,050)	(0,054)
media comun	0,675	0,748	0,640	0,662	0,516	0,544	0,617	0,608	0,500
	(0,047)	(0,043)	(0,041)	(0,055)	(0,044)	(0,047)	(0,048)	(0,052)	(0,056)
media tecnico	0,947	1,051	0,830	0,858	0,728	0,733	0,875	0,881	0,668
	(0,059)	(0,049)	(0,048)	(0,061)	(0,050)	(0,051)	(0,053)	(0,057)	(0,060)
humanidades	0,744	0,933	0,805	0,688	0,550	0,596	0,591	0,709	0,501
	(0,047)	(0,060)	(0,063)	(0,078)	(0,065)	(0,067)	(0,074)	(0,088)	(0,082)
normal	1,538	1,836	1,663	1,731	1,527	1,626	1,729	1,766	1,887
	(0,147)	(0,068)	(0,063)	(0,100)	(0,081)	(0,069)	(0,076)	(0,106)	(0,087)
cft	0,963	1,072	1,154	1,101	1,104	0,972	1,059	1,206	1,028
	(0,108)	(0,088)	(0,088)	(0,110)	(0,071)	(0,080)	(0,082)	(0,097)	(0,076)
IP	1,181	1,317	1,170	1,182	1,081	1,005	1,117	1,124	0,995
	(0,073)	(0,063)	(0,056)	(0,071)	(0,063)	(0,059)	(0,067)	(0,066)	(0,068)
Universitario	1,791	1,905	1,802	1,764	1,683	1,660	1,780	1,738	1,611
	(0,050)	(0,046)	(0,044)	(0,058)	(0,047)	(0,050)	(0,051)	(0,055)	(0,059)
Observations	5834	5758	7525	6901	6628	9263	6488	5883	6210
R-Square	0,4782	0,556	0,5377	0,5111	0,5332	0,4886	0,49049	0,5268	0,4496