Do non-wage cost rigidities slow down employment? Evidence from Turkey*

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Abstract

This paper contributes to the literature that examines the role of non-wage cost rigidities in slowing down employment creation by assessing the effect of a policy aimed at fostering employment for women and young men introduced in Turkey in 2008. Exploiting a difference-in-difference-in differences strategy, we assess the effect of the reduction in non-wage costs on employment. The results, net of the recent crisis effect, suggest a positive effect of the reduction in non-wage costs on employment creation for the targeted group (women) shortly after the announcement of the policy.

Keywords: non-wage cost rigidity, social security contribution cut, employment creation

JEL Classification: C31, J08, J21, J32

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1 Introduction

Turkey's economy has been experiencing low employment performance over the last two decades irrespective of the economic growth rate. With around fourty one percent of working age population, Turkey has the lowest employment rate in the Europe as of 2007[1]. While male employment rate of 63% is comparable to the EU-15 average, female employment rate of 21% is almost one third of the average as can be seen in Table 1. From the gender perspective the labor market performance of Turkey bears a noticeable resemblance to that of the Arabic states. Female employment rate in most of the Arabic countries is less than one third of male employment (except for Qatar). Although Jordan, Syria and Yemen with around 10% female employment rate rank well below Turkey; Egypt, Lebanon, Morocco and Tunisia have quite similar employment rates. Differently from the European countries a discernible factor in common between Turkey and Arabic countries is the islamic culture which could be relevant for explaining the dramatically low employment rates of women relative to their male counterparts. Many researchers from Turkey have pointed out the significant role of the cultural factors, particularly the dominance of the patriarchal relations in the society in explaining the low labor force participation of women (Aran et al., 2010; Dayioglu and Kirdar, 2009; Goksel, 2012; Gunduz-Hosgor and Smith, 2006).

Apart from the gender perspective, labor market performances substantially vary across countries even within the EU. More specifically, the employment rate among the EU countries, though not presented in Table 1, ranges from 77% in Denmark to 54.6% in Malta. Reasons behind the divergence in employment performances among countries have been central to the literature in labor economics. A large number of studies attribute poor performance of labor markets to their rigidities. A very common example of this flexibility oriented approach is the flex-

[1]Since the global economic crisis erupted in 2008 hit labor markets to different extents, and the timing of recovery from the crisis substantially varies among the countries, we find more reasonable to make the comparison before the crisis, as of 2007. See Table 1 for a comparison of Turkey with selected EU countries and Table 2 for the main labor market outcomes in Turkey. Note that the EU countries selected for the comparison with Turkey are including the countries which have the most similar GDP per capita (to be regarded as a proxy for development level) as for the last decade. These countries are also the ones with the most similar labor market characteristics at least in some aspects (e.g. sectoral distribution of employment).

ible labor markets of the Anglo-Saxon countries that exhibit better performance than the more rigid labor markets of Continental Europe. In fact, labor market flexibility has been suggested to many countries, including Turkey, as a remedy to overcome the bottlenecks in their labor markets (Nickell, 1997; OECD, 1994, 2007; Sengenberger, 2006; TCEA, 2006; World Bank, 2006).

The most pronounced type of inflexibility deemed as a cause for low employment creation in Turkey is high level of *non-wage cost* which refers to a part of labor costs not directly related to actual working hours, including income tax on wages, employers' and employees' contributions to social security fund and the contributions to unemployment insurance fund. The effects of non-wage costs on employment have received considerable attention in labor economics. A number of national and international surveys highlight that high labor-tax in Turkey creates a burden on employers and this in turn discourages employment creation in the formal sector while encouraging informal employment (OECD, 1994, 2007; TCEA, 2006; World Bank, 2006). This view also shared by the Turkish policy makers was embodied in a policy intervention. The policy with the aim of overcoming the poor employment performance of the labor market was legislated in May 2008. The law prescribed a cut (up to 100%) in social security contributions (SSC) borne by employers who hired young men (aged 19 to 29 years) and women (aged over 18 years) between July 1, 2008 and June 30, 2010. This paper documents the effectiveness of this policy in creating formal employment for the targeted group, something that, to the best of our knowledge, has not been done yet.

Employment subsidy policies in the form of SSC cut have taken place in several countries mostly in the northern Europe such as France (e.g. Kramarz and Philippon, 2001), Belgium (e.g. Goos and Konings, 2007), Sweden (e.g. Bennmarker et al., 2008; Egebark and Kaunitz, 2010) and Finland (e.g. Huttunen et al., 2009) over the last two decades. To the best of our knowledge, Chile (e.g. Gruber, 1997) and Turkey (e.g. Betcherman et al., 2010) are the only examples of developing countries where employment subsidies have empirically been analyzed. The employment subsidies generally target disadvantaged groups (e.g. low-wage workers, the young or the old), certain sectors or geographic locations rather than being applied to all workers and/or to all establishments. The availabil-

ity of certain target groups enables the researchers to analyze the effectiveness of employment subsidies through difference-in-differences (DD) and/or difference-in-difference-in-differences (DDD) strategy. The studies mostly find little or no evidence of employment effect of labor tax reduction with a few exemptions (i.e. Betcherman et al., 2010; Goos and Konings, 2007).

The existence of specific target groups in the policy of our interest also allows using a difference-in-differences strategy to analyze the employment effect of the policy on women and young men. More specifically, our empirical analysis is based on a DDD strategy in order to avoid the potential confounding effects of the crisis given that the policy was implemented during the recent economic crisis. We first apply standard DD estimator by comparing the change in the outcome of women and men of the same age who were and were not exposed to the policy between before and after the policy period. Then we contrast the resulting difference with the comparison of the relative outcomes of two treatment groups of the same age who were both subject to the policy and to the crisis between before and after the policy period. The second difference basically enables to cancel out the crisis effect on the targeted group under the assumption that both age groups have been affected by the crisis in a similar way. The DDD estimate could be interpreted as the causal effect of the intervention. We found positive and significant effect of the non-wage cost reduction policy on employment creation for the targeted group (women) only in certain periods, but insignificant effect as far as the average effect over the policy period is concerned.

This study is the first attempt to explore the causal relation between Turkish non-wage subsidy policy in 2008 and employment creation. In fact, there is only a little empirical research on developing countries in the field of employment subsidies. The existing literature, moreover, does not focus on total number of employment creation which constitutes the main interest of this study.

The paper is organized as follows: Section 2 discusses non-wage cost rigidity in Turkey in comparison with selected EU countries, while Section 3 introduces the data and the technique used to construct flow data. The evaluation strategy to identify the causal relationship is presented in Section 4, and main results are provided in Section 5. Section 6 summarizes the conclusions, and lastly the appendix where the regressions and all the figures and tables are presented takes place.

2 An Overview of Non-Wage Costs in Turkey in comparison with the Selected EU Countries

Recent research has pointed to the key role of labor market flexibility in adjusting the labor market outcomes to changes in economic conditions. Several studies suggest labor market flexibility as the main remedy to overcome labor market bottlenecks, though no consensus has been reached in the literature. The mainstream approach attributes poor performances of labor markets to various kind of rigidities, and points to the diverging path between well performing Anglo-Saxon countries and labor markets of Continental Europe [2]. In parallel, Turkish policy makers were also advised to adopt reforms aimed at achieving a greater degree of labor market flexibility (Nickell, 1997; OECD, 1994, 2007; Sengenberger, 2006). However, the question of flexibility being as important in a labor market with the characteristics of the one in Turkey as it is for the EU countries is still an open debate. In this paper, we focus on non-wage cost rigidities which are pronounced as one of the main causes for low employment creation in Turkey.

Non-wage costs refer to the part of total labor cost that is not directly related to actual working hours, including income tax on wages, employers' and employees' contributions to the social security fund and the contributions to the unemployment insurance fund[3]. These costs create a wedge between the cost the employer has to bear for hiring an employee and the wage received by the employee. As a result the higher labor cost incurred by employers, the less willing employers become to hire new workers in the formal sector. A widely used in-

[2] On the other hand, the recent economic and financial crisis has raised doubts about the capacity of the Anglo-Saxon model to deal with negative economic cycles. Several stakeholders have pointed that Anglo-Saxon countries could not prevent unemployment rates from upsurging during the crisis, while the relatively more rigid German labor market has shown an outstanding performance during the crisis despite a substantial economic contraction (Burda and Hunt, 2011; Zimmermann, 2011). We leave this discussion aside as it is beyond the scope of this paper.

[3] The level of the contributions paid by employers and employees depends on gross wage and a fixed premium rate determined by law differently for employers and employees. Income tax, however, depends on gross wage, contributions paid by employees and a premium rate changing according to the bracket of the income earner. These components make labor cost hard to adjust quickly in response to economic shocks, therefore nonwage cost is regarded as an element of labor market flexibility, particularly of labor cost flexibility which is composed of two elements: wage and nonwage cost flexibility. For further information about the types of labor market flexibility, see Atkinson (1984).

dicator to measure the weight of non-wage costs developed by the OECD is *tax wedge*. It is calculated as the ratio of income taxes plus employers' and employees' social security contributions (SSC) to total labor cost. Tax wedge actually points out the difference between after-tax wage of an employee and total cost of his/her employment [4].

In the majority of the OECD countries, including Turkey, the largest portion of the financial burden of taxes is incurred by employers as reported by the OECD (2010). Figure 1 shows that Germany and Poland are the only exemptions in the selected group[5] in which either income taxes or employees' SSC constitute the major financial instrument to finance the social insurance systems. Moreover, the tax wedge in Turkey, by all its components, is comparable to the EU-15 average as far as average income families or families without child are considered. Even the tax-wedge in Turkey is lower than the EU-15 average as for single earner families without children earning above the average income. On the other hand, the tax-wedge in Turkey becomes well above the EU-15 average as the family size gets larger and/or the income level decreases. To exemplify, the tax-wedge for a two-earner family without child whose total income is 133% of the average income of a single earner is almost the same in Turkey as in EU-15 on average. However, if we make the comparison with respect to the same income group of families with two children, the tax wedge in Turkey would be substantially higher than the EU-15 average, as can be seen in Figure 2. The gap in the tax-wedge between Turkey and EU-15 is narrower as for the same type of families with higher income -of 167% of the average income of a single earner-. This implies that tax wedge in Turkey is almost constant across different types of earners, whereas in most of the EU countries it varies depending on marital status, presence of children and earning level, namely the tax burden on labor gets lower as income level decreases and family size increases (OECD, 2007).

Another discernible point to be noticed in Figure 2 is the remarkable fall in the tax wedge in Turkey between 2007 and 2009. As reported by the OECD

[4]The contributions of employers and employees to the unemployment insurance fund are not included in the calculation of tax wedge, although they are counted as the components of non-wage cost.

[5]EU member states outside the OECD are excluded from the comparison group since data on tax-wedge is available only for the OECD countries.

(2010), Turkey is among the sixteen OECD countries with the most significant reduction in tax wedge during this period, accounting for a five percentage point decrease as for singles without children on average income. The decrease in the tax wedge is primarily due to the reduction in employers' SSC. In fact, the decline in the tax wedge corresponds to the period of the approval of a recent regulation that stipulated a cut in employers' SSC provided that women and young men were hired within a fixed time. Evaluation of this regulation constitutes the main interest of this paper.

2.1 The 2008 Employment Package in Turkey

In response to high non-wage costs in Turkey, the policy makers introduced a law also known as "employment package" in May 2008. The package basically provides an exemption for employers from paying SSC with intent to create new employment for women (aged over 18 years) and young men (aged between 18 and 29). The exemption would gradually phase out over a 5-year period. More specifically, the Unemployment Insurance Fund [6] would pay out 100% of employers' SSC for the first year, 80% for the second year, 60% for the third year, 40% for the fourth year and 20% for the fifth year. Employers can benefit from this subsidy provided that they hire individuals from the target group within the period between July 1^{st}, 2008 and June 30^{th}, 2010 [7] in additional to the average labor force in the workplace reported during the previous year [8] (Law No.5763, 2008).

[6] Unemployment Insurance Fund in Turkey has accumulated a large amount of surplus; equivalent of 46 billion TL (almost 20 billion Euros) by 31.12.2010. This money has been allocated for several purposes (TEI, 2011). The inefficiency of the unemployment insurance system is mostly due to the strict eligibility criteria so that very few people can get benefits despite a great deal of premium accumulation.

[7] The subsidy scheme was initially designed for one year, however after the current global crisis hit the Turkish labor market, a second employment package introduced in May 2009 extended the duration of the incentive to two years (Law No. 5838, 2009).

[8] Newly hired workers cannot be among the previously registered workers of the same employer (up to six months before 1 July 2008). In order to avoid benefitting from the incentive without creating new employment, the law excludes circulation of workers within sub-companies of the same employer; switching workers between direct or indirect partnerships, and also the situations in which an employer closes his company, open another one and transfers his workers from the old to the new one.

3 Data

We use quarterly data for the period between 2006 and 2010 from Turkish Household Labor Force Survey. The quarterly data allows distinguishing the periods before and after the policy intervention so that the policy period -between 1 July 2008 and 30 June 2010- ranges from the third quarter of 2008 to the second quarter of 2010, whereas the period before the policy introduction is between the third quarter of 2006 and the second quarter of 2008. The survey is a repeated cross-section and does not include a panel component. However, the retrospective questions in the survey enable to track individuals in two consecutive survey periods, which will be discussed in more detail below. The questionnaire involves around a hundred questions related to demographic and labor market characteristics of household members, including information on education, age, marital status, employment, working hours, income, unemployment, inactivity and past work experience.

Given that the subsidy phases out in a 5-year period, employers might be willing to fire an existing worker and hire a new one at the end of the first year to benefit from the 100% subsidy. In order to avoid such kind of exploitation, the eligibility for the subsidy is conditioned on hiring new employees in additional to the average number of workers reported in the previous year. In this sense we do not expect any destructive effect on employment caused by the policy, nevertheless our analysis captures both job destruction and job creation effects of the policy so as to measure new employment creation.

Labor supply, constituting the outcome variable of our analysis, can be measured either through static variables such as annual average hours worked and participation probabilities or based on flows between labor market states. These states are conventionally defined as employment, unemployment and nonparticipation. We consider flow analysis more useful for our analysis given that the aim of this paper is to evaluate the effectiveness of the policy in creating new employment which is unlikely to be captured by static variables. The literature related to labor market flows focus on two different kinds of flows: worker and job flows. The latter measures whether a new position has been created or destroyed by a firm rather than the changes in the labor market status of the worker which is cap-

tured by the former measure [9]. Basically, job flows are measured on the basis of establishment or firm level data [10], whereas worker flows are measured on the basis of individual or household level data such as population surveys or household labor force surveys [11] (Bleakley et al., 1999). The data set used in our analysis, household labor force survey, obliges us to focus on worker flows rather than job flows.

To construct flow data, following the literature, we rely on transitions between the labor market states. In the survey there is a series of retrospective questions about labor market transitions that are asked to the respondents such as their past work experiences and their labor market status one year before the survey. We exploit these retrospective questions to construct the flow data. For instance, flows from employment to unemployment include the respondents who report their current status as unemployed while their recalled status one year prior to the survey was employed [12].

[9] The main distinction between these two measures is that creating new jobs and destructing old ones reflects demand-side developments in the labor market, while worker flow measures capture supply-side events and job switching (Davis et al., 2006). Generally new job creation is associated with a worker transition into employment; similarly job destruction often leads a worker become unemployed. However every worker transition does not necessarily result in job creation and destruction. It may be that workers become unemployed because they are fired or because they quit, and if the vacant position is immediately filled, there would be no job destruction. Likewise, a worker whose job is destroyed can switch to another job, which is a transition within employed status without any spell of unemployment (Bleakley et al., 1999).

[10] The leading studies on measuring job flows are Davis and Haltiwanger (1992, 1999); Abowd et al. (1996); Contini and Pacelli (1995); Burda and Wyplosz (1994); Burgess et al. (1994); and Anderson and Meyer (1994). All these studies calculate gross job creation and destruction rates on the basis of establishment-level data from various sources especially from the United States.

[11] The leading studies on measuring worker flows are Blanchard and Diamond (1990), Bleakley et al. (1999), Fallick and Fleischman (2004), Shimer (2005) and Davis et al. (2006) which use different data sources from the U.S.; Kuroda (2003), Esteban-Pretel et al. (2011) and Lin and Miyamoto (2010) which use Japanese labor force survey; Bell and Smith (2002) and Elsby et al. (2010) which use labor force survey of the United Kingdom; Haltiwanger and Vodopivec (1999) and Merikull (2011) which use Estonian labor force survey.

[12] One could raise the issue of the so-called *recall bias* problem caused by the response errors in estimating the flows. Following Bell and Smith (2002) we test this by checking the number of 'inconsistent' transitions through the comparison of the responses to the question asking the current and the previous year's status of the individuals with those related to the duration of their current status. We observe consistency between the two responses a large extent, which avoid us being worried about the recall bias problem in our analysis. Moreover, the majority of the inconsistent transitions appear between the states of unemployment and inactivity which do not directly related to our main interest of new employment creation.

New employment creation defined as the difference between hirings and separations is calculated by subtracting flows into employment from flows out of employment (Davis et al., 2006). Following Bell and Smith (2002) and Elsby et al. (2010), hiring is defined to be the sum of flows from unemployment to employment (UE) and flows from inactivity to employment (NE), whereas separation is defined to be equal to flows from employment to unemployment (EU) plus flows from employment to inactivity (EN).

$$\begin{aligned} New\ Employment &= \sum_{i=1}^{N} Hirings - \sum_{i=1}^{N} Separations \\ &= (UE + NE) - (EU + EN) \end{aligned}$$

Employment subsidies could potentially change the job searching behaviour of individuals. To capture such a change we focus on the flows from unemployment to inactivity (UN) and inactivity to unemployment (NU). Moreover, the policy could affect flows within employed status (EE) through formalization of the exisiting employment and/or by changing the permanency status of the workers. However there is no information about the social security coverage status of the last job, therefore we are only able to track whether the individuals transit into a formal or an informal job without knowing the formal status of their previous job. Similarly, it is unlikely to examine whether the policy has changed the probability of transiting to a permanent job from a temporary job given the lack of information about the permanency status of the previous year's job. Therefore, the data limitation confines our analysis to the transitions from employment to employment with a different employer so as to see whether the policy has affected job changing behaviour of the individuals.

4 Identification Strategy

Identification is achieved by exploiting the fact that the intervention was targeted to specific groups of individuals in the population and that we can observe indi-

viduals before and after the policy period. This allows using a DD strategy to analyze the employment effect of the policy on women and young men. Given that the policy targets young men aged between 19 to 29 years old and women aged over 18 years, in order to explore the causal effect of the policy on employment creation for young men, we compare men aged 25 to 29 with those aged 30 to 34 between before and after the policy introduction. Similarly, to evaluate the employment effect of the policy on women we compare women aged 30 to 34 with men of the same age between before and after the policy introduction [13].

In particular, we first compare the change in the outcome of the treatment group between before and after the policy introduction by taking differences across time but within group, which enables to remove any group specific unobserved effects but not time fixed effects. We replicate the same comparison for the control group as well. Then we take the difference (across groups) of these two differences, which enables to get rid of any time trends [14]. In principle the coefficient obtained through the double differences yields the causal effect of the policy in which we are interested (Angrist and Pischke, 2008).

A challenge for applying DD strategy is that we cannot observe every potential outcome, more specifically we do not know what would have happened to the treated group if the policy had not been put into force. The so-called *common trend assumption* would rule out this problem by assuming that the outcomes of treatment and control groups would have had parallel trends in the absence of the new policy (Angrist and Pischke, 2008). Treatment is expected to induce a deviation from this common trend. Following Angrist and Pischke (2008), we validate the credibility of this identifying assumption by examining the long run employment trend in treatment and control groups prior to the policy period [15]. To our

[13]The data does not provide exact ages of the individuals, but 5-year age brackets. In this sense, the treatment group (those directly affected by the policy) could be selected from among men aged between 20 and 29 or women over 18 years old, and the control group (those not targeted by the policy) could be selected from among men aged over 29 years. We confine our analysis to the narrow age groups because it is more plausible to compare groups with similar characteristics rather than, for instance, comparing 60-year-old women with 30-year-old-men who bear quite different characteristics specific to their ages - i.e. young men are more likely to be hired relative to old women.

[14]See appendix A-1 for the formal expression of the DD strategy within a regression framework.

[15]They apply a similar validation method in criticizing the control group (i.e. Pennsylvania) used by Card and Krueger (2000).

observation, men and women in the concerned age groups show a parallel employment trend between 2000 and 2008 on a yearly and quarterly basis as shown in Figure 3. In relation to this, we test the difference in the mean of relevant observable characteristics between the treatment and control group. The results support our observation of no significant difference between the two groups. One potential problem in our analysis would be if employers had expected the enactment of the policy and strategically delayed hiring new workers or fired the existing workers in the control group until the law was introduced with intent to benefit from the subsidy. However, the policy was announced only two months before the implementation period, and benefitting from the incentives is conditioned on additional hiring as mentioned before.

The DD strategy would have been appropriate to analyze the causal effect of the policy intervention if the crisis had not affected the labor market outcomes of the subgroups differently. Given the differential effect of the crisis across treatment and control groups, as can be seen in Figure 4, the policy evaluation through DD strategy is potentially confounded by the crisis effect. In order to rule out the possible confounding effects of the crisis, we exploit DDD strategy[16] which is advantageous over the simple difference-in-differences strategy in policy evaluation, especially in the presence of an economic shock which could play a determining role in the effectiveness of the policy. As a first step, we implement standard DD estimator by comparing the change in the outcome of women aged 30 to 34 (affected by the policy) with the change in the outcome of men of the same age group (unaffected by the policy) between before and after the policy period, assuming that the outcomes of both groups would have had a parallel trend in the absence of the policy. Then we contrast the resulting difference with the comparison of the relative outcomes of two treatment groups (i.e. women and men aged between 25 and 29 years old) who are both subject to the policy and to the crisis between before and after the policy period. This difference basically enables to remove the crisis effect on women aged 30 to 34 under the assumption that that both age groups have been affected by the crisis in a similar way [17].

[16]Since the policy targets all women over 18 years old, there is no control group for evaluating the policy effect on young men through DDD strategy. Therefore, our evaluation based on DDD strategy is confined to the effect of the policy on women.

[17]See appendix A-2 for a formal expression of DDD strategy within a regression framework.

It is important to consider the fact that the policy effect may vary across periods. The policy may be more effective as time goes on or, on the contrary, the effect may be stronger just after the announcement of the policy, then phases out with the passing of time. In order to see whether the policy effect is quarter and/or year specific, we estimate three specifications based on the empirical strategy just outlined.

First we impose the policy effect to be constant across quarters over the period. More specifically, we do not allow for quarter and year specific dummies and compare the whole period after the policy introduction with the whole period before the policy. Thus we call this specification "period specific" policy effect. We next impose constant policy effect across quarters within year but allow for variation between years by introducing year specific dummies for the policy period. This specification enables us to estimate the policy effect for each year over the policy period. Lastly, we allow for heterogenous policy effect across quarters by including year specific quarter dummies. Comparing each quarter after the policy period with the whole period before the policy we get seperate estimates of the policy effect for each quarter [18].

5 Results

Table 3 presents the average effect of the policy over the policy period for each specification indicated in the previous section. The estimation results differ in terms of the restriction imposed. The magnitude of the estimate of the policy effect gets higher as variation among years and across quarters is allowed. For instance, regarding the most restricted specification which imposes constant policy effect across quarters the probability of being hired[19] for men aged 25 to 29 increased by 0.3% relative to their older counterparts (aged 30 to 34) after the pol-

[18] See appendix A-3 for formal expression of specifications within a regression framework.

[19] As discussed in the fourth section, the dependent variables are constructed on the basis of worker flows, namely the variable of $hiring$ refers to flows into employment whereas the variable of $separation$ indicates flows out of employment. In Linear Probability Models where the dependent variable is binary as in our case, the DD estimation results can be interpreted as the change in the probability of being hired or separated for the treated group relative to the control group after the policy intervention.

icy introduction. The same probability increases by 1.3% as for the less restricted specification which allows the policy effect to vary between years and by 5% as for the most flexible specification which allows for variation across quarters. On the other hand, the probability of being hired for a woman aged 30 to 34 relative to a man of the same age group decreased by 0.7%, 1.8%, 5.2% as going from the most restricted to the most flexible specification[20].

The DD estimation results show that according to the averages over the policy period, the probability of being hired for men aged 25 to 29 increased significantly above that for men aged 30 to 34 after the policy introduction, whereas we observe a negative change in the probability of being hired for women aged 30 to 34 relative to the same control group. The opposite effect of the policy on the two treated groups could be attributed to the substitution effect between women and men of roughly the same age. That is, employers may be more willing to hire men aged between 25 and 29 years rather than women aged between 30 and 34 years because of gender specific characteristics. Such a discrimination against women could result in a decline in the probability of being hired for a woman relative to their younger male counterparts who are both subject to the policy. Another explanation for the estimated negative effect of the policy on women could be the inability of the DD strategy in eliminating the differential effect of the crisis on genders.

The results discussed so far do not take into account the potential effects of the recent economic crisis on employment. We now turn to estimates of the policy effect on women based on the DDD strategy in order to check whether the recent economic crisis played a role in determining the effectiveness of the policy. The DDD strategy, by controlling for the crisis effects should yield more reliable results[21]. That the negative and significant estimate obtained from the DD strategy turns into positive after cancelling out the crisis effect implies the confounding role of the crisis in evaluating the policy[22]. However, neither the estimates of hir-

[20]We present the estimation results including all the control variables (i.e. education, marital status, number of children and urban area) at level and their interactions though additional controls do not change the significance of the results, but the precision of the estimates.

[21]The discussion henceforth is conducted based on the DDD estimation results although all the results reported for the DDD are also derived for the DD strategy. The DD results could be provided by the author upon request.

[22]To check whether the DDD results are reliable to evaluate the policy effectiveness net of the

ing nor separation are statistically significant as far as the average effect of the policy over the period is concerned, as shown in the bottom panel of Table 3[23].

Although the average estimated effect of the policy over the period -net of the crisis effect- is not statistically significant irrespective of the type of the specification, there may be significant effect in some quarters and no effect in others, thus aggregating the estimated effect over the policy period would be insignificant. We check the validity of this hypothesis through Wald test and find out an evidence of heterogeneous policy effect across quarters. The estimation results presented in Table 4 indicate that the disaggregation of the policy effect by quarter yields positive and statistically significant results for certain periods. In particular, the probability of being hired for women aged 30 to 34 above men of the same age group increased by 1.4% in the third quarters of 2008 and 2009, and by 1.6% in the fourth quarter of 2009, after removing the crisis effect[24].

What could be the underlying reason behind the significant policy effect in some quarters along with insignificant effect in the others? First we check whether seasonality matters in characterizing the policy effect by introducing nonyear specific quarter dummies in the interaction term. In particular, each quarter over the policy period is compared with the corresponding quarter before the policy intervention. For instance, the third quarter after the policy period (of 2008 and 2009) is compared with the third quarter before the policy period (of 2006 and 2007). The results indicate no evidence of seasonality in the sense that the coefficients of the interaction terms are found statistically insignificant.[25]

There is a simple explanation for the quarter-specific pattern in the policy ef-

crisis effect, we replicate the analysis on the basis of a different comparison period. Basically we compare the relative outcomes of the policy period (between the third quarter of 2008 and second quarter of 2010) with the period after the policy ended (third and fourth quarters of 2010). The estimation results, though not shown here, are consistent with what we presented in Table 3. The estimation results of this specification test can be provided by the author upon request.

[23]Table 3 also reports the estimation results by component, indicating which type of transition constitutes the major source of hiring and separation.

[24]As for the less flexible specification in which the policy effect is imposed to be constant across quarters within year but varying among years, we cannot reject the null hypothesis for either of the dependent variables, as can be seen in the second column of Table 3. Accordingly, there is no evidence of year specific policy effect even if we disaggregate the average policy effect by year. The estimation results are reported in the bottom panel of Table 4.

[25]The estimation results are available upon request though we did not report here for the sake of brevity.

fect that we highlighted above. As mentioned, the policy announced in May 2008 was initially designed for one year. After the economic crisis hit the labor market by the third quarter of 2008, the government decided to extend the policy period for one more year, and this was announced at the end of the first year (Law No.5838, 2009). Thus we detect significant effects precisely in quarters that follow the policy announcement.

We next shed light on how much new employment was created by the policy. Given that new employment is defined as the difference between total hirings and total separations, the relevant question is which aggregation level is proper for calculating total hirings and separations. Since our main interest is to explore the causal effect of the policy on the treated, we aggregate hiring and separation by multiplying the relevant probability with the number of women aged between 30 and 34 years (treated group) in the policy period. Focusing on the quarters in which there is evidence of significant policy effect, we find that the policy has created 92 new employment positions for women aged between 30 to 34 years in the third quarter of 2008, 54 positions in the third quarter of 2009 and 63 positions in the fourth quarter of 2009. This corresponds to totally 209 new employment positions over 14628 women in the relevant age group.

Furthermore, there could be a heterogeneity in the policy effect across sectors given that female labor force is not evenly distributed across sectors in Turkey. Services sector is the largest employer for women apart from the agriculture sector where mostly as unpaid family workers take place. Almost half of the total female employment (45% in 2010) is hired in the services sector, whereas the industry and construction account for less than one fifth of the female employment. We stratify the data by four main sectors, i.e. services, industry, agriculture and construction with intent to disaggregate the policy effect by sector. [26] We report the DDD results in Table 6. The estimation results suggest that the probability of being hired for women aged 30 to 34 in the services sector increased by 6.2% in the third quarter of 2008, 4.1% in the third quarter of 2009 and 4.2% in the fourth

[26] Given the limited size of the formal employment in rural area in which mostly unpaid family workers compose of the labor force, we derive the estimation results for the urban area in stead of controlling it as an explanatory variable. However, neither the significance nor the presicion of the estimates change to a considerable extent. Therefore, we did not report the results here, but available upon request.

quarter of 2009 relative to their male counterparts of the same age -after removing the crisis effect-. Consistently with previously reported results, there is no significant effect in the other quarters which results in an insignificant average effect over the policy period. Moreover, we find no evidence of significant policy effect in the other sectors for any quarter in line with the aforementioned expectations.

Lastly, we examine the policy effect on job searching behaviour of the individuals. The DDD analysis suggests positive but statistically insignificant policy effect on transitions from inactivity to unemployment, on the contrary strongly negative effect on transitions from unemployment to inactivity. These results imply that the policy avoids the women in the treatment group withdrawing from the labor market, but does not encourage the women outside the market to start looking for a job. Regarding the transitions within employed status, we find no evidence of it[27].

6 Conclusion

This paper evaluates the impact of a reduction in social security contributions incurred by employers on employment creation for the policy target groups. The policy design, targeting certain age groups of different genders within a fixed period, allows performing difference-in-differences strategy in analyzing the effectiveness of the policy intervention. Based on Turkish Household Labor Force Survey from the period of 2006-2010, we estimate the policy effect on women through difference-in-difference-in-differences technique. Although the average policy effect over the period -net of the crisis effect- is not significant, the disaggregation of the policy effect by quarter yields positive and statistically significant results for certain periods. We observe that the periods where the estimated policy effect is found significant correspond to the quarters shortly after the policy announcement.The importance of the announcement in the effectiveness of the policy could be taken as a key note of this paper for the policy makers. Moreover, services sector is found to be the only sector which benefits from the policy. This is not surprising when considered the substantially larger share of female employ-

[27]These results could be provided by the author upon request.

ment in the services sector, especially in the urban relative to the other sectors.

The results are robust to the change in the comparison period in the sense that comparing the relative outcomes of the policy period to the period after the policy abolishment yields consistent results with the comparison of the policy period to the period before the policy introduction. This finding suggests that the identification strategy is able to control for the potential confounding effects of the economic crisis, so that the evidence obtained from the triple difference technique could be interpreted as the causal effect of the policy.

This paper is not only the first study to conduct a causal evaluation of a very recent non-wage subsidy policy in Turkey, but also proposes the importance of announcement frequency in the effectiveness of the policy for the agenda of Turkish policy makers, which could be relevant also for future policy designs in Turkish labor market.

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Appendix

A Identification Strategy within a Regression Framework

1. *Difference-in-Differences Strategy*:

In this section, we introduce the specification which is estimated through difference-in-differences technique -using quarterly data from the third quarter of 2006 to the second quarter of 2010-. To do so, we begin with introducing the following notations:

1. Let $Q_{(i,j)}$ for $(i,j) \in T$ denote the time dummy variable which is equal to 1 for the ith quarter of the jth year, and 0 otherwise. The order of the years is the obvious one: 2006 is the first, 2007 is the second and so on. In particular, $Q_{(2,3)}$ is 1 if and only if the variable under consideration is the second quarter of the year 2008, and 0 otherwise. Given that the sample period is ranging from the third quarter of 2006 to the second quarter of 2010, we define the set T as:

$$\begin{aligned}\mathcal{T} = \{&(3,1),(4,1),(1,2),(2,2),(3,2),(4,2),(1,3),(2,3),\\ &(3,3),(4,3),(1,4),(2,4),(3,4),(4,4),(1,5),(2,5)\}.\end{aligned}$$

2. The group of people consisting of men aged 30 to 34 is defined to be the control group, while the group of people consisting of either men aged 25 to 29 or women aged 30 to 34 is defined to be the treatment group. Once the control and the treatment groups are introduced, the dummy variable G called the group indicator is defined to be 1 for the treatment group and 0 for the control group.

3. The dummy variable P called the time indicator is by definition equal to 1 for the period following the date of the policy introduction (i.e. between 3^{rd} quarter of 2008 and 2^{nd} quarter of 2010), and 0 for the period preceding the date of the policy introduction (i.e. between 3^{rd} quarter of 2006 and 2^{nd} quarter of 2008). For future reference we introduce the set

$$\mathcal{R} = \{(3,3),(4,3),(1,4),(2,4),(3,4),(4,4),(1,5),(2,5)\}.$$

4. X_k denotes a vector of other control variables including education level, marital status and the living area (urban/rural) of the individual k, and pair-wise interactions of these controls.

Finally, we estimate the following regression through difference-in-differences strategy:

$$Y_k = \sum_{(i,j)\in\mathcal{T}} \alpha_{(i,j)} \cdot Q_{(i,j)} + \beta_1 \cdot G + \beta_2 \cdot (P \cdot G) + X_k' \cdot \delta + \varepsilon_k \qquad (1)$$

where Y_k denotes the outcome variable of the individual k which is a measure of flow data (i.e. labor force transitions between employment, unemployment and inactivity). The coefficient of the interaction term β_2 constitutes the main interest of this analysis yielding the average effect of the policy over the period. This coefficient can simply be obtained through the following differences:

$$\begin{aligned}\beta_2 = &\{E(Y_k|G=1,P=1) - E(Y_k|G=1,P=0)\} \\ &- \{E(Y_k|G=0,P=1) - E(Y_k|G=0,P=0)\}\end{aligned}$$

2. *Difference-in-Difference-in-Differences Strategy*:

In this section, we exploit the triple difference strategy to evaluate the policy effectiveness with the aim of ruling out the potential confounding effects of the recent economic crisis which are unlikely to be eliminated through standard difference-in-differences technique (indicated in the previous section). While keeping the notations of the previous section intact, we introduce some new variables:

1. F denotes the gender dummy which is equal to 1 if the individual is female, 0 otherwise.

2. A denotes the age dummy which is equal to 1 for ages between 30 and 34, and 0 for ages between 25 and 29.

3. The products $(P \cdot A)$, $(A \cdot F)$ and $(P \cdot F)$ are pairwise interactions between the indicators of P, A and F and the product $(P \cdot F \cdot A)$ is the triple interaction of the same indicators.

We estimate the following regression through triple difference technique:

$$\begin{aligned} Y_k &= \sum_{(i,j)\in\mathcal{T}} \alpha_{(i,j)} \cdot Q_{(i,j)} + \gamma_1 \cdot F + \gamma_2 \cdot A + \gamma_3 \cdot (A \cdot F) \\ &+ \gamma_4 \cdot (P \cdot A) + \gamma_5 \cdot (P \cdot F) + \gamma_6 \cdot (P \cdot F \cdot A) + X_k' \cdot \delta + \varepsilon_k \qquad (2)\end{aligned}$$

where the coefficient (γ_6) of the triple interaction term indicates the average effect of the policy over the period.

γ_6 could also be obtained through the difference in double differences of:

$$\{[E(Y_k|A=1,F=1,P=1) - E(Y_k|A=1,F=0,P=1]\}$$
$$-\{[E(Y_k|A=1,F=1,P=0) - E(Y_k|A=1,F=0,P=0]\}$$

where women are subject to the policy, whilst men are not.

$$\{[E(Y_k|A=0,F=1,P=1) - E(Y_k|A=0,F=0,P=1)]\}$$
$$-\{[E(Y_k|A=0,F=1,P=0) - E(Y_k|A=0,F=0,P=0)]\}$$

where both groups are subject to the policy and to the crisis.

While the first double difference is exactly the same as the standard difference-in-differences estimator for the treatment group of women aged 30 to 34, the second double difference enables to cancel out the differential effect of the crisis, and the difference between these two yields the causal effect of the intervention.

3. *Specifications*:

In this section we introduce three types of specification depending on the restriction imposed on the time indicator in the interaction term.

(i) *Period specific policy effect*: Estimating equation (1) through difference-in-differences strategy and equation (2) through the triple difference strategy, we impose constant policy effect across quarters over years. The double difference estimator is constructed by interacting the group indicator (G) with a single time dummy (P) that is equal to 1 for the whole period following the date of the policy introduction, and 0 for the whole period preceding the date of the policy introduction. Likewise, the triple estimator is obtained by interacting the gender dummy (F) and age dummy (A) with the same time dummy (P).

(ii) *Year specific policy effect*: We impose constant policy effect across quarters within year, but allow variation between years. To this end, we introduce a new dummy variable S_m for $m \in \{1,2,3\}$, which is defined to be equal to 1 for the mth year within the policy period, and 0 otherwise. Indeed, S_1 is equal to 1 if year is 2008 and the period is after the policy introduction (i.e. third and fourth quarter), S_2 is equal to 1 if year is 2009 and S_3 is equal to 1 if year is 2010 and the

period is until the end of the policy period (i.e. the first and second quarter). The difference-in-differences estimator in equation (1) becomes the sum of three interaction terms each of which belongs to separate years within the policy period as specified in equation (3). Similarly, the triple difference estimator in equation (2) can be written as the interaction of the age, gender and time dummies aggregated over three years within the policy period as indicated in equation (4). Keeping the notations the same as the previous section, we estimate:

$$Y_k = \sum_{(i,j)\in\mathcal{T}} \alpha_{(i,j)} \cdot Q_{(i,j)} + \beta \cdot G + \sum_{m=1}^{3} \phi_m \cdot (S_m \cdot G) + X_k' \cdot \delta + \varepsilon_k \tag{3}$$

$$\begin{aligned} Y_k &= \sum_{(i,j)\in\mathcal{T}} \alpha_{(i,j)} \cdot Q_{(i,j)} + \gamma_1 \cdot F + \gamma_2 \cdot A + \gamma_3 \cdot (A \cdot F) + \sum_{m=1}^{3} \theta_m \cdot (S_m \cdot A) \\ &+ \sum_{m=1}^{3} \rho_m \cdot (S_m \cdot F) + \sum_{m=1}^{3} \phi_m \cdot (S_m \cdot F \cdot A) + X_k' \cdot \delta + \varepsilon_k \end{aligned} \tag{4}$$

where ϕ_m indicates the policy effect in the m-th year within the policy period.

(iii) *Quarter-year specific policy effect*: We allow the policy effect to vary across quarters over years. In this specification, The difference-in-differences estimator in equation (1) and the triple difference estimator in equation (2) become the sum of eight interaction terms each of which belong to separate quarters within the policy period as specificed in equation (5) and equation (6).

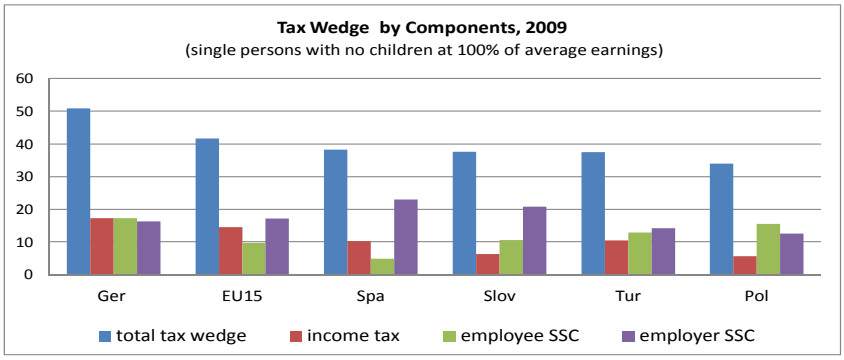
$$Y_k = \sum_{(i,j)\in\mathcal{T}} \alpha_{(i,j)} Q_{(i,j)} + \beta \cdot G + \sum_{(i,j)\in\mathcal{R}} \phi_{(i,j)} (Q_{(i,j)} \cdot G) + X_k' \cdot \delta + \varepsilon_k; \tag{5}$$

$$\begin{aligned} Y_k &= \sum_{(i,j)\in\mathcal{T}} \alpha_{(i,j)} Q_{(i,j)} + \beta_1 \cdot F + \beta_2 \cdot A \\ &+ \beta_3 \cdot (A \cdot F) + \sum_{(i,j)\in\mathcal{R}} \theta_{(i,j)} (Q_{(i,j)} \cdot A) + \sum_{(i,j)\in\mathcal{R}} \rho_{(i,j)} (Q_{(i,j)} \cdot F) \\ &+ \sum_{(i,j)\in\mathcal{R}} \phi_{(i,j)} (Q_{(i,j)} \cdot F \cdot A) + X_k' \cdot \delta + \varepsilon_k \end{aligned} \tag{6}$$

where $\phi_{(i,j)}$ indicates the policy effect in the i-th quarter of the j-th year.

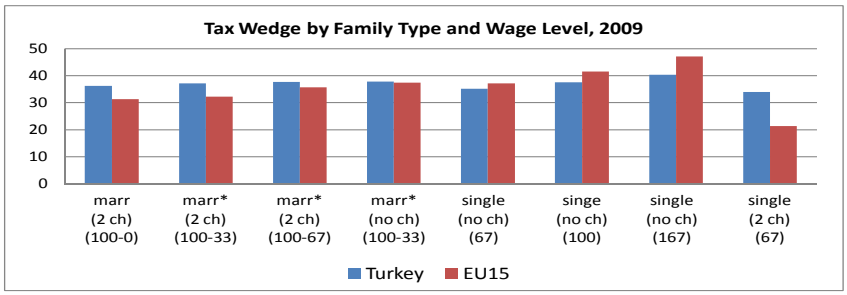
B Figures and Tables

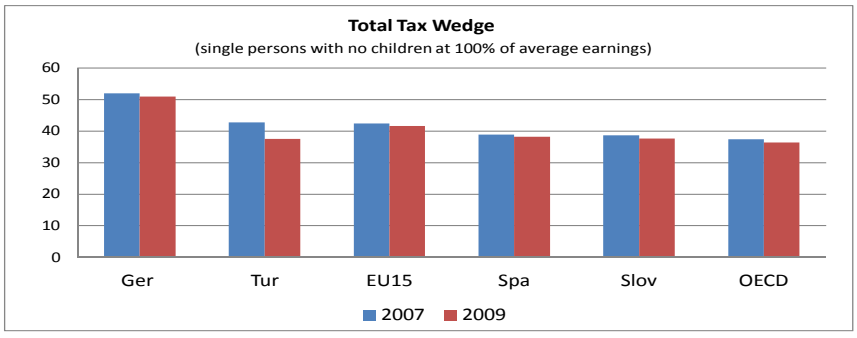
Figure 1: **Tax Wedge by Components**



Source: OECD, 2010.

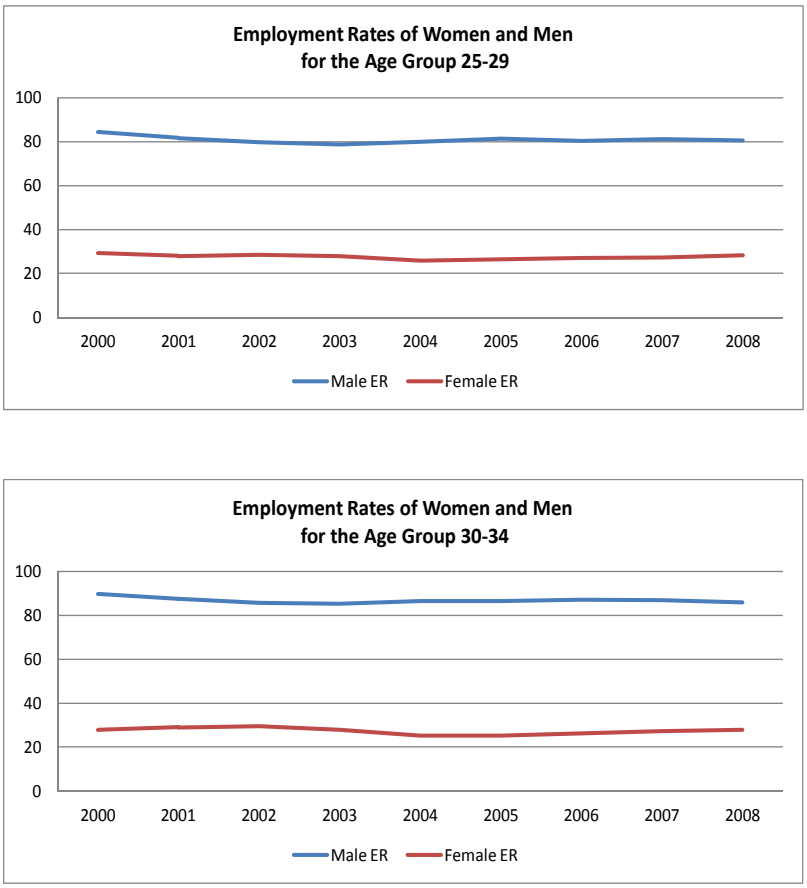
Figure 2: **Tax Wedge by Family Type and Earning Level**





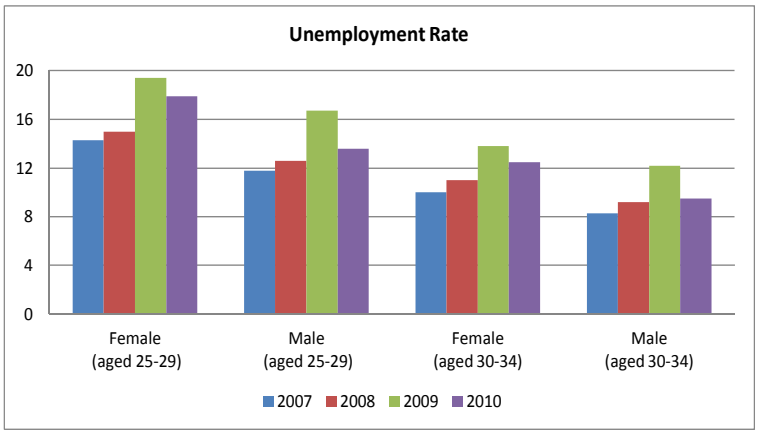
Source: OECD, 2010.

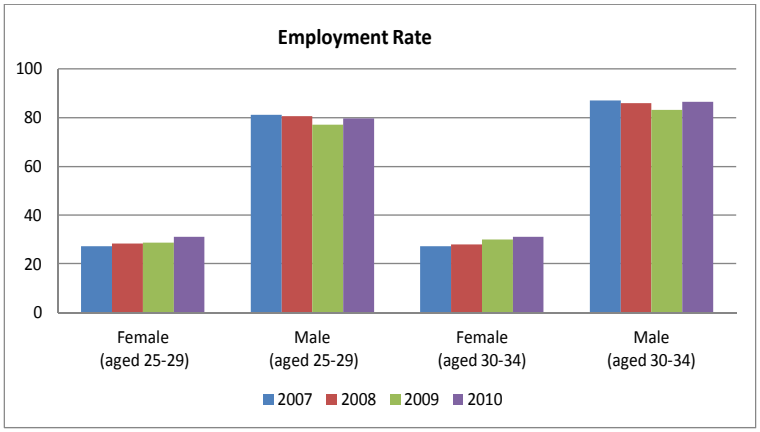
Figure 3: **Employment Rates of the Treatment and Control Groups Before the Policy Period**



Source: Turkstat, 2011.

Figure 4: **Unemployment and Employment Rates of Treatment and Control Groups between 2007 and 2010**





Source: Turkstat, 2011.

Table 1: **Employment Rates in Selected Countries**

Employment Rate, 2007		
	Male	**Female**
Turkey	62.7	21.0
EU Countries		
Bulgaria	66.0	57.6
Latvia	72.5	64.4
Lithuania	67.9	62.2
Poland	63.6	50.6
Romania	64.8	52.8
EU-15	74.2	59.5
EU-27	72.5	58.2
Arab States		
Egypt	65.5	18.6
Jordan	57.9	11.1
Morocco	68.8	22.3
Tunisia	60.0	20.8
Lebanon	61.2	19.0
Qatar	94.7	48.0
Saudi Arabia	71.4	15.6
Syria	67.7	10.1
Yemen	66.7	6.1

Source : Turkstat (2012) for Turkey,
Eurostat (2012) for the EU countries,
ILO (2012) for the Arab states.

Table 2: **Main Outcomes of the Turkish Labor Market**

	1988	**2000**	**2007**	**2008**	**2009**	**2010**
Working age pop./total pop.	63.3	69.8	72.6	72.8	73.2	73.7
Labor force participation rate	57.5	49.4	46.2	46.9	47.9	48.8
of which: Female	34.3	26.6	23.6	24.5	26.0	27.6
Male	81.2	73.7	69.8	70.1	70.5	70.8
Employment rate	52.6	46.7	41.5	41.7	41.2	43.0
of which: Female	30.6	24.6	21.0	21.6	22.3	24.0
Male	75.1	68.9	62.7	62.6	60.7	62.7
Unemployment rate	8.4	6.5	10.3	11.0	14.0	11.9
of which: Young (aged 15-24)	17.5	20.0	20.0	20.5	25.3	21.4
Non-agricultural	14.4	9.3	12.6	13.6	17.4	14.8
of which: Female	33.4	13.5	17.3	18.1	21.6	20.2
Male	10.7	8.4	11.4	12.3	16.0	13.2
Hidden unemployment rate	3.2	4.9	7.5	7.8	8.3	7.9
Underemployment rate	6.6	6.9	3.3	3.6	5.1	5.2
Informal employment rate	58.1	50.6	45.4	43.5	43.8	43.3

Source: Turkstat, 2011.

Table 3: **Average Effect of the Policy over the Period**

	Specifications		
	Quarter-year specific	**Year specific**	**Period specific**
DD$_{male}$			
Hiring	0.050*	0.013*	0.003
	(0.027)	(0.008)	(0.003)
UE	0.024	0.007	0.002
	(0.020)	(0.008)	(0.002)
NE	0.017*	0.006*	0.001
	(0.009)	(0.003)	(0.001)
Separation	0.005	0.000	0.001
	(0.019)	(0.007)	(0.002)
EU	0.019	0.004	0.002
	(0.017)	(0.006)	(0.002)
EN	-0.014	-0.004	-0.002
	(0.008)	(0.003)	(0.001)
DD$_{female}$			
Hiring	-0.052***	-0.018***	-0.007***
	(0.016)	(0.006)	(0.002)
UE	-0.056***	-0.021***	-0.007***
	(0.014)	(0.005)	(0.002)
NE	0.004	0.003	0.000
	(0.010)	(0.004)	(0.001)
Separation	-0.020	-0.004	-0.003
	(0.016)	(0.006)	(0.002)
EU	-0.073***	-0.023***	-0.009***
	(0.013)	(0.005)	(0.002)
EN	0.051***	0.019***	0.006***
	(0.010)	(0.004)	(0.001)
DDD			
Hiring	0.030	0.010	0.002
	(0.026)	(0.01)	(0.003)
UE	0.023	0.007	0.002
	(0.021)	(0.008)	(0.003)
NE	0.007	0.003	0.001
	(0.015)	(0.006)	(0.002)
Separation	0.007	0.001	0.001
	(0.023)	(0.009)	(0.003)
EU	0.010	0.001	0.001
	(0.018)	(0.007)	(0.002)
EN	-0.003	0.000	0.000
	(0.014)	(0.005)	(0.002)

Robust st. errors in parenth. (*** p<0.01, ** p<0.05, * p<0.1)

Table 4: **DDD Estimation Results**

	Hiring	**UE**	**NE**	**Separation**	**EU**	**EN**
	Year-Quarter Specific Policy Effect					
DDD_{Q3-08}	0.014**	0.005	0.009**	-0.005	-0.001	-0.004
	(0.007)	(0.006)	(0.004)	(0.006)	(0.005)	(0.004)
DDD_{Q4-08}	-0.007	-0.005	-0.002	-0.002	-0.004	0.002
	(0.007)	(0.005)	(0.004)	(0.006)	(0.005)	(0.003)
DDD_{Q1-09}	-0.008	-0.006	-0.002	0.002	0.004	-0.002
	(0.008)	(0.005)	(0.004)	(0.007)	(0.006)	(0.004)
DDD_{Q2-09}	-0.007	-0.007	-0.000	-0.001	0.004	-0.005
	(0.007)	(0.006)	(0.004)	(0.007)	(0.006)	(0.004)
DDD_{Q3-09}	0.014**	0.010*	0.004	0.004	0.007	-0.003
	(0.007)	(0.006)	(0.004)	(0.007)	(0.006)	(0.004)
DDD_{Q4-09}	0.016**	0.012**	0.004	0.003	0.001	0.002
	(0.007)	(0.006)	(0.004)	(0.006)	(0.000)	(0.004)
DDD_{Q1-10}	0.005	0.006	-0.001	0.003	-0.001	0.004
	(0.007)	(0.006)	(0.004)	(0.006)	(0.005)	(0.004)
DDD_{Q2-10}	0.003	0.008	-0.005	0.003	0.001	0.003
	(0.008)	(0.006)	(0.004)	(0.006)	(0.005)	(0.004)
Obs	305,590	305,590	305,590	305,590	305,590	305,590
R^2	0.68	0.56	0.29	0.49	0.42	0.18
Wald	0.05	0.19	0.35	0.97	0.93	0.64
	Year Specific Policy Effect					
DDD_{2008}	0.007	0.001	0.005*	-0.004	-0.002	-0.001
	(0.005)	(0.004)	(0.003)	(0.005)	(0.004)	(0.003)
DDD_{2009}	-0.001	-0.001	-0.001	0.002	0.004	-0.002
	(0.004)	(0.003)	(0.002)	(0.004)	(0.003)	(0.002)
DDD_{2010}	0.005	0.007	-0.002	0.003	0.000	0.003
	(0.005)	(0.005)	(0.003)	(0.004)	(0.004)	(0.003)
Obs	305,590	305,590	305,590	305,590	305,590	
R^2	0.68	0.56	0.29	0.48	0.41	0.18
Wald	0.46	0.51	0.26	0.60	0.48	0.42

Robust st. errors in parenth. (*** p<0.01, ** p<0.05, * p<0.1)

Table 5: **Estimation Results by Sector**

	DD_{male}			DD_{female}			DDD		
	Quarter-year specific	**Year specific**	**Period specific**	**Quarter-year specific**	**Year specific**	**Period specific**	**Quarter-year specific**	**Year specific**	**Period specific**
Hiring									
Services	0.070**	0.027**	0.009**	0.046	0.027	0.006	0.083	0.047	0.009
	(0.033)	(0.013)	(0.004)	(0.053)	(0.021)	(0.007)	(0.080)	(0.031)	(0.010)
Industry	-0.005	0.006	0.000	-0.202*	-0.080*	-0.026*	-0.117	-0.050	-0.021
	(0.046)	(0.018)	(0.006)	(0.108)	(0.041)	(0.013)	(0.152)	(0.059)	(0.019)
Agriculture	0.066	0.029	0.009	-0.186***	-0.064***	-0.022***	-0.042	-0.021	-0.006
	(0.070)	(0.027)	(0.009)	(0.064)	(0.024)	(0.008)	(0.098)	(0.037)	(0.012)
Construction	0.257**	0.101**	0.031**	0.612	0.145	0.056	0.336	0.315	0.113
	(0.124)	(0.048)	(0.015)	(0.440)	(0.156)	(0.053)	(0.374)	(0.219)	(0.073)
Separation									
Services	-0.004	-0.002	-0.001	0.110**	0.047**	0.014**	0.007	0.011	0.001
	(0.029)	(0.011)	(0.003)	(0.049)	(0.019)	(0.006)	(0.073)	(0.028)	(0.009)
Industry	0.128***	0.042**	0.016***	0.272**	0.106***	0.033**	0.243	0.088	0.034
	(0.045)	(0.017)	(0.006)	(0.108)	(0.041)	(0.013)	(0.151)	(0.058)	(0.021)
Agriculture	-0.044	-0.005	-0.005	-0.021	-0.007	-0.002	-0.127	-0.034	-0.016
	(0.053)	(0.020)	(0.006)	(0.053)	(0.020)	(0.007)	(0.078)	(0.030)	(0.010)
Construction	0.029	-0.006	0.005	0.855**	0.231	0.083*	0.257	-0.072	-0.003
	(0.115)	(0.044)	(0.014)	(0.426)	(0.051)	(0.150)	(0.588)	(0.218)	(0.072)

Robust st. errors in parenth. (*** p<0.01, ** p<0.05, * p<0.1)

Table 6: **DDD Estimation Results by Sector**

	Quarter-Year Specific Policy Effect							
	Hiring				**Separation**			
	Services	Industry	Agriculture	Construction	Services	Industry	Agriculture	Construction
DDD_{Q3-08}	0.062***	0.022	-0.009	0.174	-0.010	0.032	-0.024	-0.158
	(0.021)	(0.040)	(0.024)	(0.174)	(0.020)	(0.041)	(0.021)	(0.128)
DDD_{Q4-08}	-0.026	-0.033	-0.019	-0.130	0.014	-0.020	-0.032	0.094
	(0.020)	(0.042)	(0.028)	(0.173)	(0.019)	(0.042)	(0.022)	(0.163)
DDD_{Q1-09}	-0.014	-0.012	-0.006	-0.103	-0.002	0.007	-0.005	0.150
	(0.021)	(0.037)	(0.027)	(0.138)	(0.020)	(0.042)	(0.025)	(0.157)
DDD_{Q2-09}	-0.008	0.004	-0.037	0.166	-0.010	0.025	-0.028	-0.063
	(0.021)	(0.041)	(0.026)	(0.165)	(0.020)	(0.042)	(0.020)	(0.144)
DDD_{Q3-09}	0.041**	0.025	0.044	0.169	0.002	0.052	-0.021	0.140
	(0.021)	(0.039)	(0.025)	(0.173)	(0.021)	(0.043)	(0.022)	(0.184)
DDD_{Q4-09}	0.042**	-0.032	-0.006	0.164	-0.016	0.033	-0.021	0.223
	(0.021)	(0.040)	(0.030)	(0.215)	(0.020)	(0.020)	(0.020)	(0.213)
DDD_{Q1-10}	-0.020	-0.036	0.013	0.178	0.021	0.056	-0.002	-0.130
	(0.021)	(0.039)	(0.028)	(0.182)	(0.020)	(0.038)	(0.020)	(0.175)
DDD_{Q2-10}	0.006	-0.055	-0.022	0.046	0.008	0.058	0.006	0.001
	(0.021)	(0.039)	(0.026)	(0.152)	(0.018)	(0.039)	(0.018)	(0.153)
	Year Specific Policy Effect							
DDD_{2008}	0.043***	-0.006	-0.014	0.110	0.003	0.005	-0.018	-0.057
	(0.015)	(0.031)	(0.019)	(0.108)	(0.014)	(0.031)	(0.016)	(0.105)
DDD_{2009}	-0.009	-0.004	-0.001	0.128	-0.007	0.036	-0.018	0.044
	(0.012)	(0.023)	(0.015)	(0.097)	(0.116)	(0.024)	(0.012)	(0.096)
DDD_{2010}	0.013	-0.040	-0.006	0.037	0.015	0.047	0.002	-0.059
	(0.016)	(0.029)	(0.020)	(0.111)	(0.014)	(0.029)	(0.014)	(0.119)

Robust st. errors in parenth. (*** p<0.01, ** p<0.05, * p<0.1)