

Do Employment Subsidies Work? Evidence from Regionally Targeted Subsidies in Turkey

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Abstract

This paper studies the effects on registered employment, earnings, and number of registered establishments of two employment subsidy schemes offered by the Government of Turkey to encourage investment and employment in low-income provinces. Making use of the progressive expansion of the program, we implement a “difference-in-differences” methodology to construct appropriate counterfactuals for the covered provinces. Our findings suggest that both subsidy programs did lead to significant net increases in registered jobs in eligible provinces. Depending on the model specification, estimated registered employment gains range from 4%-13% for the first program and from 9%-15% for the second program. However, the cost of the actual job creation was high, because of substantial deadweight losses. This was particularly true for the first program where we estimate that between 50% and 80% of the subsidized jobs would have been created without the program. Because of better design features, the second subsidy program had lower, though still significant, deadweight losses (25-50%) and, as a result, this appears to have been more cost-effective even though the subsidies themselves were more generous than under the first scheme. Although data limitations constrain our capacity to test whether the dominant effect of the subsidies was to increase social security registration of firms and workers rather than boosting total employment and economic activity, the evidence we have suggests the former was more important. This supports the hypothesis that in countries with weak enforcement institutions, high labor taxes on low-wage workers may lead to substantial incentives for firms and workers to operate informally.

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1 Introduction

Persistently high unemployment rates have led many countries in Europe and elsewhere to implement job subsidies and cuts in employer's social security contributions with the objective of encouraging employment creation. On average, such programs amount to about one-quarter of total expenditures on active labor market policies in OECD member countries [OECD \(2003\)](#). While policymakers tend to equate the number of workers receiving a subsidy to the net employment effect of such programs, estimating the actual effect is not so straightforward. Many beneficiaries may have found jobs independently of the existence of the program, i.e., what in the economics literature is commonly referred as the deadweight loss. In addition, some existing employees who do not qualify for the subsidies may have lost their jobs because subsidies lowered the costs for other workers who were then hired because they were relatively less expensive (substitution effects). Finally, some workers may have lost their jobs because subsidies, by affecting product prices, reduced the market share of some firms relative to others (displacement effects). For all these reasons, then, the employment impact of the subsidies may be far from the administrative number of beneficiaries. In this regard, estimating the true impact of such initiatives requires building counterfactuals of what would have been the labor market performance in the absence of the subsidies. Despite the potential interest in such measures, there are very few studies that attempt to rigorously measure the impact of such programs on net employment creation and other economic outcomes.

This paper studies the employment impacts of regional incentives offered by the Government of Turkey to encourage investment and employment in low-income provinces. The research examines three different incentive regimes legislated through Law 4325 (1998), which covered 22 provinces, Law 5084 (2004), which expanded coverage to an additional 15 provinces, and Law 5350 (2005), which added 13 more provinces. However, given that our data do not cover the period before Law 4325 was enacted, the econometric analysis of the net employment impacts focuses on the subsidies offered under the two later laws. It should be noted, however, that our analysis of Law 5350 is limited by the fact that we are only able to observe the first 8 months of that scheme in operation. We are able to examine the effects of Law 5084 with more evidence. These laws have differed somewhat in terms of qualification requirements and the actual subsidies, but they all have included subsidies on employer social security contributions, employee personal income taxes, energy consump-

tion, and land. Employers in eligible provinces have qualified on the basis of meeting new job creation thresholds, either by opening new establishments or by expanding employment in existing ones.

Since these subsidies reduce the cost of labor in some provinces but not in others, the regional incentive programs can be examined to estimate how much new employment is likely to be created in Turkey when taxes or social security contributions are reduced. The employment impacts of the regional incentives are analyzed using “difference-in-difference” models that estimate registered employment, registered number of establishments, and taxable earnings (levels and growth) in a province in a given month as a function of whether the regional incentives program is in force, the time period, and provincial variables that cover province-specific effects. Various alternative specifications incorporating these determinants in different ways are estimated.¹

The Turkish economy comprises an appropriate setting to study this question because of its high level of taxation on labor.² In Turkey, combined employer-employee contributions to finance pensions and disability insurance, health insurance, unemployment benefits, and workers’ compensation constitute 36.5-42% of gross wages.³ Income tax ranges from 15-35% of the gross wage.⁴ Comparisons of the tax wedge on labor income in Turkey with the EU-15 countries (pre-2005 members) and a selection of (new accession) EU-10 countries for workers at different earnings levels and with different family characteristics indicates that for families and singles with children, Turkey’s taxes on labor are among the highest in the OECD.⁵ This is especially the case for low-wage workers with children where Turkey has the highest tax wedge of all of the OECD countries ([World Bank, 2006](#)).

Our findings suggest that both subsidy programs did lead to significant net increases in registered jobs in eligible provinces. Depending on the model specification, estimated

¹For an example of the general methodological approach, see [Autor et al. \(2006\)](#).

²Throughout the paper, “labor taxes” is used as a term to include both social security contributions (levied on employers and employees) as well as personal income taxes levied on employees.

³The range is due to contribution rates for work injury which vary by industry.

⁴Between 2000 and 2004, income tax rates ranged from 15-40%. In 2005, the top rate was cut to 35% and the number of brackets was reduced from six to five.

⁵The “tax wedge” is defined as income taxes and combined (employer-employee) social security contributions, minus cash benefits, as a percentage of total labor compensation. The calculations of the tax wedge are based on OECD estimates with additional calculations made by the World Bank to take into account Turkey’s consumption tax credits which were not included by the OECD. Note that payroll taxes account for about 70% of Turkey’s overall labor taxes.

registered employment gains range from 4%-13% for the subsidy scheme under Law 5084 and from 9%-15% for Law 5350. However, the cost of the actual job creation was high, because of substantial deadweight losses. This was particularly true for the first program where we estimate that between 50% and 80% of the subsidized jobs would have been created without the program. Because of better design features, the program under Law 5350 had lower, though still significant, deadweight losses (25-50%) and, as a result, this appears to have been more cost-effective even though the subsidies themselves were richer than under Law 5084. Although data limitations constrain our capacity to test whether the dominant effect of the subsidies was to increase social security registration of firms and workers or to boost total employment and economic activity, the evidence we have suggests the former was more important. This supports the hypothesis that in countries with weak enforcement institutions, high labor taxes on low-wage workers may lead to substantial incentives for firms and workers to operate informally.

The rest of the paper is organized as follows. Section 2 provides a brief review of the existing literature, while section 3 describes the regional incentives. Section 4 presents the data, our identification strategy, and the descriptive statistics. Main results are provided in section 5, and a simple cost-benefit analysis is conducted in section 6. Finally, section 7 concludes.

2 The Effect of Employment Subsidies

Employment subsidies aim to reduce the cost of labor to employers.⁶ They can be applied to all employment or only to new hires (marginal subsidies). They can also be general, in the sense of applying to all workers and establishments, or specific, if only certain types of workers (for example, low-wage, youth, long-term unemployed, women, or disabled workers) or certain sectors or geographic locations qualify. Subsidies can be implemented as direct wage refunds or, quite commonly, as credits on social security contributions and other labor taxes.

To determine the effects of employment subsidies, economists have resorted to two approaches. The first is to make inferences based on estimates of the elasticity of labor demand.

⁶We use the terms “employment subsidies” and “wage subsidies” interchangeably to refer to subsidies that reduce the cost of labor for employers.

The second is to directly estimate the employment effects of actual subsidy measures.

Regarding the first approach, labor demand elasticity estimates give a measure of the expected percentage change in employment given a percentage change in labor costs. However, this approach is confounded by the fact that, *a priori*, it is unclear whether the incidence of the subsidy falls on the employer or on the employee, which depends on the elasticity of the labor demand and labor supply. When the incidence is fully on the employer, subsidies will lead to lower total labor costs and increased labor demand. However, when the incidence is on the employee, the result will be higher take-home pay for workers and no effect on labor demand. In the intermediate case when the two parties share the burden of a tax, and assuming competitive labor markets, both employment and wages will increase. Other factors can also come into play. One is whether minimum wages are binding. In that case, excess supply of labor implies that firms can recruit more workers without having to increase wages. This situation is similar to the case when the labor supply is fully elastic: an employment subsidy yields maximum employment expansion and no effect on wages. It should be noted that while in the standard labor market model of full employment, employment and earnings do not depend on whether the subsidy is given to employers or employees, binding minimum wage floors alter that equivalence. In this case, subsidies to employers maximize employment effects, while subsidies to employees ensure higher earnings for workers.

The existing literature provides some guidance on the plausible range of labor demand elasticity estimates. The international evidence suggests that the likely range is between -0.30 and -0.50 (Hamermesh, 1993), although most of the estimates are obtained from developed economies. Recently, there have been a number of studies from developing and transition countries, with most of the (long run) elasticities estimates in the -0.20 to -0.40 range.⁷

As noted above, labor demand elasticities do not fully capture the employment effects of changes in subsidies (or labor taxes) because that depends also on the extent to which they are shifted on to employees (also commonly referred as “pass through”). Studies in middle-income countries provide a wide range of estimates, which indicate that, in some cases, the pass through can be quite large. For example, research in Latin America suggests that anywhere from 20-70% of the employer’s social security contributions are passed on to the worker (Heckman and Pagés, 2004). However, at least one study for Chile finds full wage shift and no employment effects (Gruber, 1997). For OECD countries, Nickell (2003)

⁷For a list of these studies, see Taymaz (2006).

concludes that the most reasonable assessment based on the literature available is that labor taxes have a modest effect on employment and therefore tax credits should also exert small effects. He concludes that a 10-percentage point change in the tax wedge can be expected to affect employment by between 1-3%, "... a relatively small but by no means insignificant effect" (p. 8). It should be noted however that this conclusion refers to across-the-board tax credits. The evidence suggests that the rate of pass-through declines around the minimum wage (Taymaz, 2006). Therefore, tax credits might have larger effects for low-wage workers.

The second approach to assessing the employment effects of different subsidy schemes has been to directly estimate them. Bishop (1981) employs time series methodologies to evaluate the employment effect of the New Jobs Tax Credit (NJTC), a U.S. countercyclical program designed to spur employment and to accelerate economic recovery. The program awarded a 50% tax credit to the first \$4,200 of wages per worker, provided that a firm increased employment by more than 2% relative to the previous year, up to a specified maximum per firm. Bishop estimates an economy-wide employment effect of the NJTC of 0.2-0.8%. It should be noted, however, that his approach makes it difficult to sort out the effect of the program from the cyclical effects.

Katz (1998) evaluates the Targeted Jobs Tax Credit (TJTC), also in the U.S., which offered wage subsidies of 50% of the first year, and 25% of the second year wages up to \$6,000, to employers of vulnerable and disadvantaged workers (economically disadvantaged youth, veterans, workers on public assistance, and disabled workers). Using a difference-in-differences methodology to assess the impacts of the TJTC, Katz estimates that a reduction in labor cost of 15% yields a net employment effect of 7.7% which, under the assumption of an infinitely elastic labor supply, implies an elasticity of demand of -0.5 percent, a value within the range found by Hamermesh (1993).

There are also some results available for the Netherlands regarding SPAK, a scheme introduced in 1996 to permanently reduce taxes and social security contributions paid by employers for workers with wages around the minimum wage. All workers could benefit from this subsidy including those already at work. Mühlau and Salverda (2000) use time-series analysis to estimate the effect of the introduction of SPAK, controlling for a number of variables that affect employment growth. They find that the introduction of this measure did not raise employment growth, not even in sectors, such as retail, that are intensive in the use of low-wage labor. As in Bishop (1981), however, a time-series approach may not provide

a good identification of the effects of the scheme relative to the effects of other factors that also influence employment.

In the context of middle-income countries, [Galasso et al. \(2001\)](#) evaluate a wage subsidy scheme targeted to workers in temporary employment in Argentina (Proempleo). The scheme subsidized 50 percent of the first 18 months of wages for workers employed in permanent, regular jobs. Unlike the case of SPAK, the authors find that the program provided assistance to low-wage workers in finding regular wage employment, although effects were only statistically significant among women and youth.

This review highlights that little is still known about the employment and wage effects of employment subsidy schemes. Given the amount of resources devoted to these programs, and their popularity across developed and developing countries, gathering better knowledge on their effects on a number of outcomes could improve policy design. Another issue of key importance and about which little is known is the deadweight loss of subsidies. This term refers to share of the subsidized employment that would have been created independently of the incentives. Most estimates are obtained from interviews with employers rather than from quantitative estimates. Even though employers may not have the right incentives to report on the actual numbers they would have employed in the absence of subsidies, such estimates still suggest large deadweight losses. These are associated with the impossibility of targeting subsidies to workers who would not have been hired without the subsidy. Estimates of deadweight loss range from around 53-70% for marginal subsidization under targeted programs and up to 93% for non-targeted, non-marginal general measures ([Marx, 2005](#)).

In the next sections we attempt to fill some of these gaps by estimating the employment, wage, and firm-creation effects, as well as the deadweight losses, of a marginal subsidization scheme targeted to low-wage workers in low-income provinces in Turkey.

3 Regional Subsidies in Turkey

We analyze the effects of a series of regional incentive schemes legislated through Law 4325 (1998), Law 5084 (2004), and Law 5350 (2005), aimed at increasing investments and employment opportunities in low-income provinces. Initially, 22 provinces were covered under Law 4325. Law 5084 expanded coverage to an additional 15 provinces, while Law 5350 further increased coverage to 13 additional provinces. Given that our data do not cover the period

before Law 4325 was enacted, we focus our attention on the subsidies offered under Laws 5084 and 5350. While such laws differed in terms of actual requirements and subsidy amounts, they included four subsidy components: (i) reductions in employers' social security contributions; (ii) credits on income taxes on wages; (iii) subsidies on electricity consumption; and (iv) land subsidies. Firms in an eligible province could receive subsidies provided they had monthly social security premium documents, electricity consumption records, and documentation showing income and corporation tax liabilities. No additional written application was required, which implied low transaction costs.

Law 5084 became effective in January 2004 and the duration of the scheme was set to be five years. It covered all provinces with per capita GDP of \$1,500 or less (in 2001) and provinces designated as priority development regions. All provinces (but one) covered under Law 4325 qualified for this program,⁸ and any firm that received subsidies under Law 4325 could choose to continue to receive subsidies under the first law even after the enactment of the second. In addition, 15 additional provinces not covered under Law 4325 qualified for benefits under Law 5084.⁹

Under Law 5084, firms could obtain: (i) a subsidy on the social security contributions due at the minimum contribution base¹⁰ and (ii) an income tax subsidy for the amount due at the minimum wage. Only new registered employment was subsidized. For establishments created on and after October 2003, subsidies were calculated based on all registered employees while for establishments that started operations before that date, subsidies were calculated based on the number of workers over and above those registered with social security on the reference date (August 2003). Establishments located within industrial zones were fully subsidized for their calculated social security contributions and income taxes, whereas establishments outside industrial zones received compensation for only 80% of such amounts.

In addition, newly created firms employing at least 10 registered workers could also claim an energy subsidy equal to 20% of their energy costs, plus 0.5% of that amount for

⁸Tunceli was covered under Law 4325 and Law 5350, but was not covered under Law 5084.

⁹See Appendix A for a full list of provinces covered under the three laws.

¹⁰Before July 2004 the minimum contribution base was above the minimum wage. During this period, employers who hired workers at a wage below the contribution base were required to pay, in addition to the regular employer contribution, the employee contribution for the difference between the minimum base and the minimum wage. As of July 2004, the minimum wage was raised to match the minimum contribution base. Changes in the minimum contribution base relative to the minimum wage lead to substantial changes in the cost of hiring low-wage workers, which in turn affected the demand for labor (Papps, 2007).

each additional new worker. Existing firms, on the other hand, could get the subsidy if their registered employment increased by at least 20% since the reference date and their current employment was at least 10 workers. The subsidy was capped at 40% or 50% (in industrial zones) of total energy costs. Finally, free land was also available in some cases for firms that would employ no less than 10 workers for at least 5 years.

Law 5350 came into effect in May 2005 and modified a number of provisions in Law 5084. It extended coverage to 13 additional provinces with low socio-economic development according to an index elaborated by the Turkish State Planning Organization (SPO). Again, any firm that received subsidies under the previous law (5084) could choose to continue to receive subsidies under that law even after the enactment of Law 5350. To qualify under the new law, newly-created firms were now required to have at least 30 registered employees, while existing firms had to increase employment by at least 20% from the new reference date (January 2005), and have at least 30 employees. For existing establishments, the number of workers for which firms could claim either tax or social security subsidies could not exceed the total number of initially employed workers (at the new reference date). Law 5350 also imposed a minimum threshold size of 30 employees for new and incumbent firms to claim energy subsidies. In addition, the latter had to demonstrate an employment increase of at least 20% in order to file for benefits. However, Law 5350 increased the amount of the subsidy for each eligible worker from one time the tax and social security payable at the minimum wage (or base) to 3 times that amount.¹¹ This implied that, for every eligible worker, the law subsidized the taxes and social security contributions of two already-employed workers. As in Law 5084, establishments located within industrial zones were fully subsidized for their calculated social security contributions and income taxes, whereas establishments outside industrial zones received compensation for 80% of such amounts.

Given the complexities of the legal framework it may be useful to provide a couple of examples. Consider, for example, a firm with 10 registered employees that increases registered employment by 20% from the reference date. Under Law 5084, this firm would obtain a subsidy for 2 workers during all periods in which employment was above employment at the reference date. Under Law 5350, this firm could not obtain any subsidy since 12

¹¹Two additional constraints are that (i) the total value of the tax subsidy cannot exceed the amount calculated multiplying the income tax payable at the minimum wage on the number of workers and (ii) the social security subsidy cannot exceed the value that results from multiplying the social security contribution payable at the minimum base by the number of workers.

employees is below the minimum threshold of 30. Law 5350 favors larger firms, relative to Law 5084.

Let us now assume the case of a firm that starts with 10 registered employees and increases its employment to 30. In this case, under Law 5084 the firm would obtain a subsidy for 20 employees. Under Law 5350, the calculation is more complicated. Although the incremental employment is 20, the number of subsidized workers cannot exceed initial employment (i.e., 10). Since the total subsidy under Law 5350 is three times the incremental employment, but only 10 new workers can be counted, the total subsidy is for 30 workers. Therefore, while fewer workers would be eligible, the total amount paid in subsidies increases in Law 5350 relative to Law 5084 under this example.

One problem with marginal subsidization is that it creates incentives for firms to manipulate employment. Thus, when subsidies are given only to new hires, firms have incentives to outsource labor to other firms which, in turn, can claim that employment as marginal.¹² Laws 5084 and 5350 explicitly prohibit such tactics, however enforcement is always problematic. Law 5350 may reduce these incentives since imposing a minimum of 30 employees reduces the incentives to outsource existing labor in small independent units in charge of activities such as cleaning, cafeterias, cashiers, etc., although such incentives may still exist for large firms. Figure 1 shows that if labor demand is above $L_{min} = 30$ and $L_{max} > L_2$, where L_{max} is equal to twice the initial employment ($2L_0$), employment increases by much more (from L_0 to L_2) under Law 5350 than under 5084 (from L_0 to L_1).

[Figure 1 about here.]

Given the discussion in this section and the section above, it is predicted that if: (i) the subsidy schemes had a sufficient take-up rate; (ii) the demand for labor in Turkey is sufficiently elastic (i.e., it is downward sloping); and (iii) subsidies were not fully transferred to workers in the form of higher wages, then employment and gross wages would increase in a magnitude given by the elasticity of the labor demand and supply. In addition, it is also expected that Law 5350 would yield larger employment effects concentrated in the intensive, rather than the extensive, margin (i.e., primarily through expansion of existing

¹²Marginal incentives can also motivate firms to lower the reference employment in order to be able to claim higher marginal subsidies. Under the two laws discussed, such incentives were limited by setting the reference employment sufficiently far prior to the enactment of the laws.

firms rather than creation of new ones). In the next sections we describe our data and empirical methodology and assess whether our results confirm these priors.

4 Data and Specification

4.1 Data

The data used in this study come from three sources. The main source, provided by the Social Security Administration of Turkey (henceforth SSK), is a monthly panel of province-level data.¹³ It includes information on the number of registered workplaces, registered employees, total taxable earnings that are subject to contributions, and SSK premiums. We compute average taxable earnings, dividing the total taxable earnings by the number of registered employees. SSK data also includes information for all provinces that were covered by the different subsidy schemes on the total number of employees, the number of establishments, and the base earnings subsidized according to the coverage rate (80 or 100 percent), as well as the newly registered workplaces and employment, number of work days, base earnings, and the amount of social security subsidies they received. While the data are available for the period January 1998 to December 2005, we restrict our analysis to 3,555 monthly observations on 79 provinces covering the period April 2002 to December 2005. This choice was motivated by a number of data problems and inconsistencies in the earlier part of the sample.¹⁴

Data on the cost of energy subsidies was provided by the Turkish Treasury and it gives information on the number of subsidized workplaces, subsidized employment, and the cost of the energy incentives per month and province.

Finally, since data on provincial GDP is not available for the period of study, we use information on electricity consumption per province and year in order to approximate the real level of economic activity in a province at a given point in time. This is done to assess whether employment increases in subsidized provinces are likely to be new jobs or the conversion of unregistered employment to registered jobs. This annual data set, available

¹³The SSK data are actually provided on a sub-provincial basis (i.e., SSK reporting unit). The sub-provincial data were aggregated up to a provincial basis for each month.

¹⁴Two provinces were excluded because of unreliable data due to inconsistent administrative reporting. See Appendix B for a full list of data elements, and Appendix C for a description of our consistency checks.

for 1995 to 2004, is provided by the Turkish Statistical Institute and includes information on the total electricity consumption of each province, disaggregated by type of consumer.

Table 1 provides the summary statistics for the main variables used in this study. In this table, the unit is an individual province in an individual month.

[Table 1 about here.]

4.2 Methodology

We use a “difference-in-differences” approach to estimate the effects of the subsidies under Laws 5084 and 5350. To do so, we compare the change in performance between the period pre- and post-introduction of subsidies in the provinces that benefit from them (“treated provinces”) with the change in performance in the provinces that are not covered (“control provinces”). Since the data span different subsidy regimes, covering different sets of provinces, we choose different “control” and “treatment” groups, depending on the regime being analyzed.

To clarify our characterization of control and treatment groups, we define the following groups of provinces: D_{4325} , which contains the 22 provinces that were subsidized under Law 4325; D_{5084} , which includes the 15 provinces that were subsidized under Law 5084 but not covered by Law 4325; and, finally, D_{5350} , which contains the 13 provinces that were subsidized by Law 5350 but not covered by Law 5084. In that way, each group only includes the eligible provinces added under each consecutive law. We also define the group D_{never} , which contains the provinces that were not subsidized under any law. The list of provinces covered under each law is graphically depicted in Figure 2 and detailed in the table in Appendix A. Similarly, it is also useful to define three separate time periods: $Period_0$, which covers the period before January 2004; $Period_1$ which covers the period between January 2004 to April 2005, when Law 5084 was in effect, and when both D_{4325} and D_{5084} provinces received subsidies under Law 5084; and $Period_2$, which covers the period between May 2005 to December 2005 in which provinces D_{4325} , D_{5084} and D_{5350} were eligible to receive subsidies under Law 5350.

[Figure 2 about here.]

At least two alternative control groups can be used to estimate the effect of Law 5084 on D_{5084} provinces. The first one is given by D_{5350} . These are provinces that are very

similar in terms of income, geographical location, and density of population to the provinces in the D_{5084} group but fell above the minimum income per capita threshold to qualify for subsidies under Law 5084 (see Table 2 and Figure 2). The second control group is the D_{never} provinces, which did not receive subsidies under any subsidy scheme. Both have their advantages and disadvantages. On the one hand, D_{5350} provinces are similar in terms of income and other characteristics to provinces D_{5084} and therefore may offer a better counterfactual of the evolution in a number of economic variables had the subsidies not taken place. On the other hand, it may be argued that economic incentives may create stronger substitution effects between similar and geographically close provinces than between provinces that differ substantially in income and other attributes. From that point of view, the D_{never} group may give a better idea of the effects of the subsidies net of substitution effects.

[Table 2 about here.]

In order to estimate the effect of Law 5350 on D_{5350} provinces, we use provinces D_{never} as a control group. Unfortunately, the evaluation of that program cannot benefit from the comparison with a similar, though untreated, control group. This, plus the fact that we can only observe the first 8 months of subsidies under Law 5350, means that our results for that scheme are based on less evidence than is the case for Law 5084.

We estimate the two following basic specifications - the first provides an estimate of the effect of Law 5084 using D_{5350} provinces as the control and the second provides estimates for both Law 5084 and Law 5350 using D_{never} provinces as the control:

$$X_{jt} = \alpha_0 + \delta_0 Period_1 + \delta_1 D_{5084} + \delta_2 D_{5084} * Period_1 + \epsilon_{jt} \quad (1)$$

and

$$\begin{aligned} X_{jt} = & \alpha_1 + \gamma_0 Period_1 + \gamma_1 Period_2 + \gamma_2 D_{4325} + \gamma_3 D_{5084} + \gamma_4 D_{5350} \\ & + \beta_0 D_{4325} * Period_1 + \beta_1 D_{4325} * Period_2 \\ & + \beta_2 D_{5084} * Period_1 + \beta_3 D_{5084} * Period_2 \\ & + \beta_4 D_{5350} * Period_1 + \beta_5 D_{5350} * Period_2 + \nu_{jt} \end{aligned} \quad (2)$$

where X_{jt} is an outcome variable (employment, wages, number of firms) either in log levels

or in growth rates and are error terms. Specification (1) studies the effect of Law 5084 using provinces $D_{.5350}$ as the control group. In order to estimate this effect, we only include observations pertaining to periods 0 and 1, and provinces $D_{.5084}$ and $D_{.5350}$. The coefficient on $D_{.5084} * Period_1$ measures whether there is a differential change in performance between period 0 and period 1 in the treatment ($D_{.5084}$) relative to the control group ($D_{.5350}$).

Specification (2) addresses the effect of both laws using provinces D_{never} as the control group. In order to estimate this specification, we use all periods and provinces contained in our sample. The coefficient on $D_{.5084} * Period_1$ measures the differential change in performance between period 0 and period 1 in provinces $D_{.5084}$ compared to provinces D_{never} , and hence estimates the effect of Law 5084. Similarly, the effect of Law 5350 can be calculated as the difference between the coefficient on $D_{.5350} * Period_2$ and the coefficient on $D_{.5350} * Period_1$.

We enrich these basic specifications in a number of ways. In most specifications, we control for province-specific effects to account, among other things, for differences in population as well as differences in the level and sector distribution of economic activity by adding a set of province dummies to our basic specification. Obviously, since these variables are collinear with the province group indicators, the latter are dropped from the specification. In some specifications we allow the time effects to differ month by month, by including a full set of dummies for each time observation in our sample. In addition, we also account for differences in the evolution of variables at the province level by including a full set of province specific-trends in some of the specifications.

Finally, following [Bertrand et al. \(2004\)](#), we allow for the error term to be auto-correlated within provinces by estimating Huber-White robust standard errors clustered at the province level in all specifications.

4.3 Summary statistics

Tables 3 to 5 provide summary statistics for monthly growth rates of registered employment, registered workplaces, and real wages. The row All provinces reports results for the 79 provinces included in this study. We also present descriptive statistics separately for provinces $D_{.4325}$, $D_{.5084}$, $D_{.5350}$, and D_{never} and for sub-periods determined according to the enactment date of each law.

[Table 3 about here.]

[Table 4 about here.]

These tables provide some preliminary, descriptive evidence on the effects of these subsidy schemes on the growth rates of the outcome variables. Thus, during *Period_0*, *D_4325* provinces - those receiving subsidies under Law 4325 - show the highest mean of monthly employment and workplace growth. Moreover, *D_5084* and *D_5350* provinces experience striking increases in their average monthly growth rates of employment and workplaces during the periods when they are first covered by a subsidy scheme. In particular, the mean monthly employment growth of *D_5084* provinces increases five-fold from 0.4% in *Period_0* to 2% in *Period_1*. Similarly, average monthly growth in the number of registered establishments in *D_5084* provinces increases almost two-fold from 0.7% in *Period_0* to 1.3% in *Period_1*. Employment and establishment growth in *D_5350* provinces between periods 1 and 2 also experience substantial increases, reaching 2.6% (from 1.0%) and 1.5% (from 0.5%), respectively. On the other hand, employment and establishment growth in the never subsidized provinces increases at a much slower pace than in the treated provinces.

Finally, Table 5 presents summary statistics of real wage growth (defined as average taxable earnings) across province groups and sub-periods. Unlike the employment and establishment trends, significant impacts of the subsidy programs are not apparent. There is little evidence that the effects of reduced taxes on employees are passed on to workers in the form of higher wages. In fact, wage growth in the treated provinces declines after the introduction of subsidies. For example, the monthly wage growth in *D_5084* provinces declines from 0.8% in *Period_0* to no growth in *Period_1*. A similar pattern characterizes *D_5350* provinces in *Period_2*, with a slight decline in average monthly wage growth from 0.3% to -0.1%. In comparison, wage growth is higher in the never treated provinces in both periods 1 and 2 than in the treated provinces.

[Table 5 about here.]

Before moving on to the results of our econometric analysis, it may be useful to visualize some of the results by presenting the month-to-month evolution of outcomes in treatment versus control provinces. Figures 3-6 illustrate graphically the impact of Law 5084 on treated provinces (*D_5084*) compared to the control provinces (*D_never* and *D_5350*). Figure 3

presents the ratio of employment in provinces *D_5084* relative to employment in the *D_never* provinces. It is quite clear that while the employment rate of the treatment provinces showed a secular decline relative to the employment of the control prior to the introduction of Law 5084, there is a marked increase in this ratio shortly after January 2004 when Law 5084 came into effect suggesting a much faster increase in the treated provinces than in the control group. The same observation of the laws making a difference is obtained from Figure 4, which presents the ratio of employment between *D_5084* and *D_5350* provinces. In this case, it is also quite clear that employment in the *D_5084* group increases at a much faster rate during *Period_1* when the *D_5350* provinces were not eligible for the subsidies but this reverses after Law 5350 came into effect and the *D_5350* also qualified for subsidies.

[Figure 3 about here.]

[Figure 4 about here.]

Figures 5 and 6 present the comparable trends for the number of establishments, rather than number of workers. They suggest that Law 5084 not only stimulated an increase in employment but also, an increase in the number of workplaces.¹⁵ Thus, Figure 5 shows that the growth in establishments in *D_5084* provinces relative to the never subsidized group increased markedly with the onset of Law 5084. Figure 6 illustrates how the *D_5084* establishment growth outstripped growth in the *D_5350* group until Law 5350 came into effect when the ratio leveled off and started turning downward slightly.

[Figure 5 about here.]

[Figure 6 about here.]

Figures 7 and 8 illustrate the effect of Law 5350 on the *D_5350* provinces relative to the *D_never* provinces. Although the period covered is short, these graphs still show some evidence of the law having an effect. Figure 7 shows that, while during *Period_1* employment in *D_5350* provinces followed a level path with the *D_never* provinces, after May 2005 when Law 5350 was enacted, employment in *D_5350* provinces started growing at a higher rate than in the *D_never* provinces. Similarly, Figure 8 shows that the decline in the ratio of the number establishments relative to the never subsidized provinces reversed once Law 5350 was introduced.

¹⁵Workplaces and establishments are treated as synonymous in this article.

[Figure 7 about here.]

[Figure 8 about here.]

5 Econometric Results

We move next to describing the results of estimating specifications (1) and (2) which are summarized in Tables 6, 7, and 8 for employment, number of establishments, and wages, respectively.

5.1 Employment

The first column of Table 6 presents the results of specification (2), which includes province groups and period dummies. The coefficient on the interaction between the variables D_{5084} and $Period_1$ is positive and statistically significant, indicating that after the introduction of subsidies, employment in provinces D_{5084} grew faster than in the never subsidized provinces. The magnitude of the coefficient indicates that, on average, during period 1 employment increased by 4.1% more in the treated than in the control provinces.

[Table 6 about here.]

Results do not change if the specification is augmented with a full set of province and date dummies (column 2). Similarly, allowing for province-specific time trends still yields positive and statistically significant effects of the subsidies (column 3). The magnitude of the coefficient, however, becomes larger, suggesting that Law 5084 subsidies boosted employment in the D_{5084} provinces by almost 8% above the level in the control (never-subsidized) provinces.

Similarly, the story remains when employment growth, rather than employment levels, is specified as the dependent variable (column 4). The coefficient on the $D_{5084} * Period_1$ interaction suggests that in Period 1, Law 5084 increased employment growth in the treated provinces by 0.9 percentage points a month.

The right-hand panel of Table 6 shows the estimates according to specification (1), with the D_{5350} provinces used as the control group for the effect of Law 5084. The results again suggest subsidies provided a significant boost to the employment levels and growth

of D_{5084} provinces. The coefficient on the interaction term $D_{5084} * Period_1$ in columns (5)-(8) is positive and statistically significant in all but one case and the implied magnitudes are sizeable: in Period 1, employment in the treated provinces were 12.7% above the D_{5350} control group in two of the three specifications. In terms of growth rates, the effect of the subsidies amounted to 1.8 percentage points per month.

Estimates of the effect of Law 5350 can be identified from columns (1)-(4). Thus, taking the never-subsidized provinces as the control group, the coefficient on the $D_{5350} * Period_2$ interaction provides an estimate of how much employment grew in these provinces between period 2 and period 0, relative to the control group. Notice however, that to correctly infer the effects of the subsidies between the two periods, we need to compare the magnitudes of the $D_{5350} * Period_1$ and $D_{5350} * Period_2$ coefficients and assess whether they are statistically different, which we do find in all specifications. (See row titled P5350 for the P-value of the hypothesis that $D_{5350} * Period_1 = D_{5350} * Period_2$). While the coefficient on $D_{5350} * Period_1$ is negative in most specifications, suggesting that this set of provinces had underperformed relative to the "never" treated provinces in Period 1, that trend is reversed in Period 2. Thus, employment performance in D_{5350} provinces, relative to that of the D_{never} provinces, improves after Law 5350 is introduced. Overall, columns (1)-(3) indicate that subsidies increased employment of provinces D_{5350} by between 9 and 15% relative to never-subsidized provinces.¹⁶ Column (4) suggests an increase in the growth of employment in D_{5350} provinces of about 1.8 percentage points per month.

Taken together, these results suggest a positive, sizable, and statistically significant effect of the regional subsidies on employment levels and growth in the covered provinces, with the magnitude of the effect greater for Law 5350. Regarding Law 5084 in particular, the estimated magnitude of the effect is larger when the D_{5350} provinces are considered as control group rather than the D_{never} ones. While the former provinces may provide a better counterfactual since they are more similar to the D_{5084} group, substitution effects may also be contributing to the results. It is plausible that the subsidies motivate some firms to change locations, or shift jobs across similar and geographically close provinces in order to gain access to the subsidies.

¹⁶These figures are calculated by subtracting the $D_{5350} * Period_1$ coefficient from the coefficient for $D_{5350} * Period_2$ in each column.

5.2 Number of establishments

We next turn to examining how subsidies affected the number of establishments (Table 7). Results indicate that relative to the never subsidized provinces, the number and growth rate of establishments increased in provinces D_{5084} as a result of Law 5084 (as indicated by a positive and statistically significant coefficient on $D_{5084} * Period_1$ in columns (1)-(4)). The estimates suggest that the subsidies increased the number of establishments by between 2.4% and 3.6% above the control group, while the establishment growth rate increased by about 0.5 percentage points.

[Table 7 about here.]

Similar results are obtained if D_{5350} provinces are used as the control group. In the specifications shown in columns (5) and (6), the estimated impact of Law 5084 on the number of establishments was 6.7%. The exception to this general result is the specification presented in column (7) in which the $D_{5084} * Period_1$ coefficient is not statistically significant. Nevertheless, province specific trends in the number of establishments are also potentially accounted for in specification (8) and this yields a positive and statistically significant effect.¹⁷

With regard to Law 5350, we find somewhat mixed evidence of its effects on the number of establishments. Controlling only for time and province indicators does not yield a statistically significant effect (columns 1 and 2). Adding province-specific time dummies yields a positive coefficient on $D_{5350} * Period_2$, but the test of the difference between $D_{5350} * Period_1$ and $D_{5350} * Period_2$ is only significant at the 18 percent level. Finally, a specification for growth rates, controlling for date and province dummies, indicates a positive and statistically significant effect, suggesting that Law 5350 would have increased the growth in the number of workplaces by 0.5 percentage points.

As with employment, we also find the effects of Law 5084 to be more sizeable if D_{5350} provinces are used as a control group, which again could signal substitution effects across provinces. All in all, our results indicate that Law 5084, and somewhat more tentatively, Law 5350 had a positive effect in the number of workplaces in the treated provinces (i.e., growth at the extensive margin). Such effects could be driven by either higher expected profit or firms' incentives to outsource workers to eligible firms in order to claim marginal

¹⁷Notice that specifications in columns (4) and (8) correspond to the first difference of models in columns (3) and (7) as province-specific trends become province-fixed effect after taking first differences.

benefits. As predicted by Figure 1, Law 5350 led to higher employment growth per firm. Combining the results in Tables 6 and 7 (columns 3), we find that Law 5084 and Law 5350 increased the number of workers per establishment by 5.5% and 13.1%, respectively. In other words, growth at the intensive margin was the most important part of the overall effect of these regional incentive programs.

5.3 Earnings

Finally, we examine the effects of the subsidies on earnings (Table 8). As discussed in section 2, there is substantial agreement in the literature that, to a large extent, taxes on wages are shifted on to workers in the form of lower wages. The same logic would indicate that, assuming symmetry, employment subsidies that reduce the cost of labor for employers would, to a large extent, be shifted back to workers in the form of wage increases. As discussed in section 4, we do not have data on individual wages or earnings. Instead, we approximate earnings per worker by dividing total taxable earnings by the number of workers in each province.¹⁸

[Table 8 about here.]

Subjecting the wage data to the same menu of specifications applied to employment and number of establishments provides little evidence of wage shifts. Regardless of the specification, we never find that the coefficients of the interactions $D_{5084} * Period_1$ and the difference between $D_{5350} * Period_1$ and $D_{5350} * Period_2$ are positive and statistically significant. In fact, in at least one case, the estimate is negative and statistically significant. The evidence seems to point towards a conclusion that the subsidies either had no effect on wages or, if there was an effect, it was negative.

These results run counter to those found by Taymaz (2006) for Turkey, where based on an analysis of manufacturing data, he finds strong evidence of a wage pass-through. His findings, however, also indicate that the pass-through is substantially reduced for wages around the minimum wage. One possible explanation for our results, then, is that minimum wages are binding in subsidized regions and therefore, the pass-through for low-wage workers is low.

¹⁸Due to a maximum and a minimum in the taxable earnings base, taxable earnings and earnings differ in the low and upper part of the distribution. This implies, for example, that if wage shifts occur only at the upper end of the distribution we would not capture it.

Table 9 shows that average taxable earnings are lower and closer to the minimum wage in *D_5084* and *D_5350* provinces than in the never-treated provinces, but our data do not allow us to infer whether minimum wages are effectively binding. Another possible explanation is that subsidies - which are set at minimum wage/contribution base levels - stimulate firms to hire workers with relatively lower earnings and that, as a result, on average, taxable earnings decline. Our data does not allow us to discriminate between these two alternative explanations.

[Table 9 about here.]

5.4 Robustness Checks

5.4.1 Anticipation of Laws

One potential problem is that the assumptions underlying difference-in-differences estimates are invalid if employers expect the enactment of subsidy laws and strategically delay hiring new workers or establishing new jobs until after the law is introduced. In this case, our estimates would be upward biased. In order to address this issue, we use an interrupted panel strategy. The idea is that, by dropping a few months before and after the enactment of each law, we can mitigate the effect of such strategic responses by eliminating periods when employers have the highest incentive to shift the creation of jobs and employment.

Tables 10-12 provide the results of these interrupted panel estimations. In these specifications, we drop the three months before and after the enactment of each law. Hence, *Period_0* covers the period between April 2002 to September 2003; *Period_1* covers the period between April 2004 to January 2005; and *Period_2* covers the period between August 2005 to December 2005. The results change little for both laws when applying the interrupted panels, with virtually no qualitative difference from our original estimates. Quantitatively, interrupted panel regressions yield somewhat larger estimates in absolute terms, which suggests that an upward bias resulting from strategic responses of employers is unlikely and that our difference-in-differences estimates do capture the *real* effects of the subsidy laws.

Table 10 shows that, on average, during period 1 employment increased by 5.6 to 15.7% more in *D_5084* provinces than in the never covered provinces. Similarly, our results suggest that in Period 1, Law 5084 increased employment growth in the treated provinces by 1.7 percentage points a month. Similar results are found when 5350 provinces rather than never

covered provinces are used as the control group. Estimates of the effect of Law 5350 are also comparable under the interrupted panel strategy. According to our findings, Law 5350 subsidies increased employment by 9 to 17% relative to never-subsidized provinces.

[Table 10 about here.]

Table 11 provides results for the number of establishments. As in the case of employment, we find the effect of Law 5084 to be larger under these specifications: the number of establishments increased by between 4% and 6.8% above the never covered provinces, and by 2.1% and 5.9% above the *D*-5350 provinces, while the establishment growth rate increased by about 1 to 1.4 percentage points. The results for Law 5350 are for the most part insignificant and quantitatively close to our previous findings.

[Table 11 about here.]

Finally, table 12 confirms our previous conclusions on taxable earnings per worker that the subsidies had either no effect on wages or a negative effect, if any. The magnitude of the effects of the laws are larger in these later regressions suggesting, at their lowest estimate, a reduction of 2.3% in the treated provinces compared to the never covered provinces for Law 5084 and a reduction of 5% for Law 5350.

[Table 12 about here.]

5.4.2 Formalization versus Job Generation

Taken together, the estimates presented in this section suggest that the different subsidy packages had important effects in stimulating the growth of registered employment and registered establishments in low-income regions of Turkey. Unfortunately, these data do not allow us to determine whether these increases actually represented new jobs or establishments or, instead, resulted from the formalization of previously non-registered (informal) employment and firms. To investigate this issue, ideally we would require household data on total employment - formal and informal - by province and period. Unfortunately, we do not have access to province identifiers in household data and, therefore, it is not possible to look at the evolution of formalization versus total employment generation in this way.

Another possible approach is to examine whether economic activity has increased in the treated relative to the control provinces. Unfortunately, province-level GDP data for the period of analysis were not yet available. As an alternative approach, we analyze electricity consumption data. Since electricity cannot be stored, its consumption is closely correlated with the level of economic activity and is a commonly used proxy for economic activity. If registered employment and establishment gains reflect an actual real increase in total employment and the number of establishments, then economic activity would have increased and, therefore, the consumption of energy should have increased as well. We examine this hypothesis by estimating specification (2) with consumption of electricity as the dependent variable. Since monthly data per province are not available, we estimate the model with annual data for the period 2002-2004. It should be recognized that data availability is a limiting factor since we are only able to use three observations per province, with only one after Law 5084 was introduced. Moreover, subsidies were paid only for the last 10 months of that year. The effect of Law 5350 cannot be tested with the available data.

In order to estimate this equation, we define the post-treatment period (*D_post*) as 2004. The coefficient on the *D_5084_post* variable measures the differential change in electricity consumption during the post-treatment period in provinces *D_5084* relative to the never subsidized provinces. A positive and significant coefficient on this variable would be consistent with the hypothesis that employment created because of the subsidies was due to an increase in economic activity. However, as shown in Table 13, the estimates suggest that electricity consumption did not increase after the enactment of Law 5084. While these results are limited by the factors noted above, they suggest that the gains in employment and number of firms correspond to a surge in formalization, rather than to real gains in economic activity.

[Table 13 about here.]

6 Cost of the programs

We next analyze the expenditure side of the regional incentives programs in order to estimate the cost of formal job creation. The calculation of the costs incurred under the programs includes the social security, income tax, and energy subsidies. The social security costs are provided in the SSK database. The income tax costs, while not directly available, can be

estimated as a proportion of the social security subsidies.¹⁹ The costs of the energy subsidies are obtained from Treasury data. Land has not been included in the cost calculations because neither data nor a method for approximating these costs is available. Hence, our estimates of total costs are downwardly biased.

Expenditures under Law 5084 and estimates of the cost per job are presented in Table 14. These cover the 14-month period from March 2004, when the first subsidies were paid, to April 2005, the last month before Law 5350 came into effect. Four calculations have been made to estimate the cost per job. The first divides the total cost of the subsidies by the number of subsidized “job-months” as reported in the SSK administrative files during the period.²⁰ In effect, this computation assumes that each subsidized job-month was actually created due to the subsidy. A comparison of the total number of jobs subsidized with the total number of jobs created as estimated through our models provides an estimate of the “deadweight loss” - i.e., the number of jobs that were unnecessarily subsidized. The other three calculations compute the cost per “net” job-month created, using our econometric modeling results to determine how many jobs were actually created because of the program. These three calculations are based on a low, a medium, and a high estimate of the effects.

[Table 14 about here.]

According to the SSK files, 739,757 “job-months” were subsidized under Law 5084 in all the *D_5084* provinces. The table also shows that our estimate of the total subsidy cost - based on the social security and energy subsidy data and our derived estimate of the income tax subsidy - was 112,275,769YTL (New Turkish lira). This implies that the cost per job-month was 152YTL. During the months when Law 5084 subsidies were being paid (and before law 5350 began), the average monthly labor cost for a minimum wage worker was 548YTL. So the cost per job-month, based on the number of subsidized jobs, was about 28%

¹⁹In order to calculate the income tax subsidy, we assumed that everyone who got the social security subsidy also got the income tax subsidy. The income tax rate on the minimum wage was 15%. So, income subsidy=(#eligible workers)*(100 or 80%)*(minimum wage)*15%. It is also described in the laws that the amount of SSK subsidy was calculated as: SSK subsidy=(#eligible workers)*(100 or 80%)*(contribution base)*20.5%. Hence, one can calculate the amount of total income tax subsidy received as a fraction of the SSK subsidy received. For the period April 2002 to June 2004, income subsidy=0.56*social security subsidy as the contribution base differs from the minimum wage. For the period July 2004 to December 2005: income subsidy=0.73*social security subsidy as the contribution base equals the minimum wage.

²⁰A “job-month” refers to a worker who has been subsidized during a particular month. We refer to job-months rather than jobs because the SSK data are provided on a monthly basis.

of the total labor cost for a minimum wage worker at that time.²¹ If this was the end of the story, it could be argued that this is a relatively low cost for creating jobs. However, our econometric estimates indicate that only a small portion of the subsidized jobs was actually created because of the incentives, which substantially reduces the cost-effectiveness of the program. Depending on the actual estimate of the employment impact, somewhere between 47% and 81% of the subsidized jobs under Law 5084 would have been created without the program. As a result of these substantial deadweight losses, costs per job-month range from 819YTL with our low estimate of jobs created to 286YTL for the upper-end estimate. These subsidy amounts represent roughly 150% and 50%, respectively, of the average monthly cost of a minimum-wage worker. Using the mid-range estimate of employment generation, the cost per job-month is 441YTL, about 80% of the total cost of employing a minimum-wage worker.

Similarly, Table 15 provides the cost estimates for Law 5350. These cover the 8-month period from May 2005 when the first subsidies were paid under the new law to December 2005, the last month for which we have data. According to the SSK files, 368,551 job months were subsidized under this law in the *D_5350* provinces. The table shows our estimate of the total cost of 92,970,931YTL. This means the cost per subsidized job-month was 252YTL, which was about 42% of the total labor cost for a minimum wage worker during this period (594YTL). Comparing this figure to that of cost per subsidized job under Law 5084 shows that the level of subsidies was considerably larger under the rules of Law 5350. However, once deadweight losses are taken into account, Law 5350 turns out to be more cost-effective. Our empirical results suggest that, depending on the specification, between 25% and 52% of the jobs subsidized under Law 5350 would have been created without the subsidy. These deadweight losses, although by no means insignificant, are much lower than those estimated for Law 5084. Accordingly, the costs per actual job created are much lower as well. We calculate the cost per job-month created under low-end job creation estimate at 529YTL and at 337YTL for our higher job estimate. These represent 89% and 57% of the total cost of a minimum wage worker, respectively.

[Table 15 about here.]

²¹Since the potential size of the subsidies for all of the different components is significantly higher than that, it seems that a significant proportion of subsidized employees worked less than a full month and/or worked part-time.

Two conclusions should be drawn from this cost analysis. First, the real costs of creating jobs under the subsidy programs are much higher than the actual subsidies paid for each subsidized job. This is largely because many subsidized jobs would have been created anyway. Second, because these deadweight losses were much smaller under Law 5350 than under Law 5084, the actual cost of creating jobs under the newer program was considerably less even though the financial outlay per subsidized worker was higher. Program design clearly matters and the eligibility thresholds established in Law 5350 (30 employees and 20% increase in employment) appears to have significantly reduced deadweight losses. Finally, it should be noted that our cost estimates are lower bounds of the actual costs. Other factors, such as the unobserved substitution effects and the fact that we have not considered land costs or the administrative costs of implementing and supervising the programs, mean that the real cost per job created is certainly higher than the ones reflected in Tables 14 and 15.

7 Conclusions

In this paper we examine the benefits and costs of subsidizing the cost of low-income workers in economically disadvantaged regions of Turkey. We take advantage of a series of consecutive programs, with widening regional coverage, to implement a pseudo-experimental approach. This allows us to compare the evolution of employment, number of establishments, employment per firm, taxable earnings per worker, and consumption of electricity before and after the different subsidies came into effect. Our results point to a substantial elasticity of employment to changes in labor costs. They also indicate that establishment creation is quite responsive to changes in labor costs and labor taxes particularly when minimum wages or other constraints may reduce pass through rates. All of these findings suggest the importance of labor costs as a factor in determining employment creation both in the intensive and the extensive margin.

Nonetheless, our results also suggest that implementing the subsidies can be a costly way to increase employment. Under Law 5084, the subsidies likely ended up paying for almost the full labor costs of the jobs created, rather than subsidizing employment at the margin. The main issue was deadweight losses - i.e., that many subsidized jobs would have been created anyway - which increased considerably the real cost of the program. This is particularly a concern in Turkey given that the subsidies analyzed in this study were

implemented in a period in which economic growth was picking up after a crisis and firms likely were prepared to hire new workers in any event. Law 5350 also had deadweight losses but these were considerably smaller than those under the previous law. So, even though the subsidies themselves were more generous, the program turns out to be more cost effective.

Although we were only able to undertake an imperfect test, our results suggest that subsidies increased formalization of existing firms and jobs more than creating new economic activity. This is a relevant distinction and deserves close attention in future studies. If confirmed, this would support the hypothesis that in countries with relatively weak enforcement institutions, high labor taxes on low-wage workers create a strong incentive for informality both for firms and workers. From that point of view, exempting (fully or partially) low-wage workers from income and social security taxes may be an effective way to reduce informality and expand the coverage of basic health and old age pension benefits to the labor force. Such exemptions are not unlike those found in many income tax systems in the world, where there is a certain minimum income that is exempted and/or where tax rates are progressively set.

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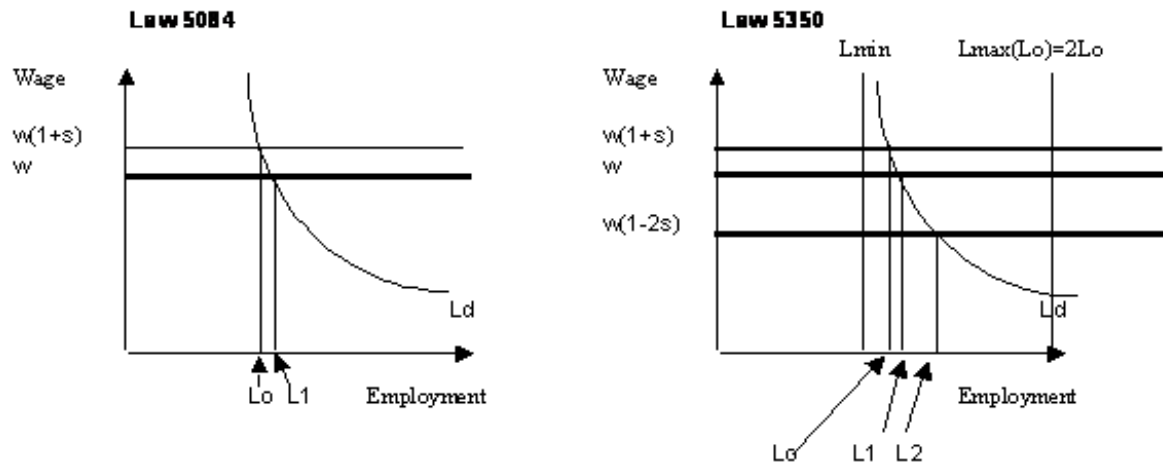


Figure 1: The employment effects of Law 5084 versus Law 5350

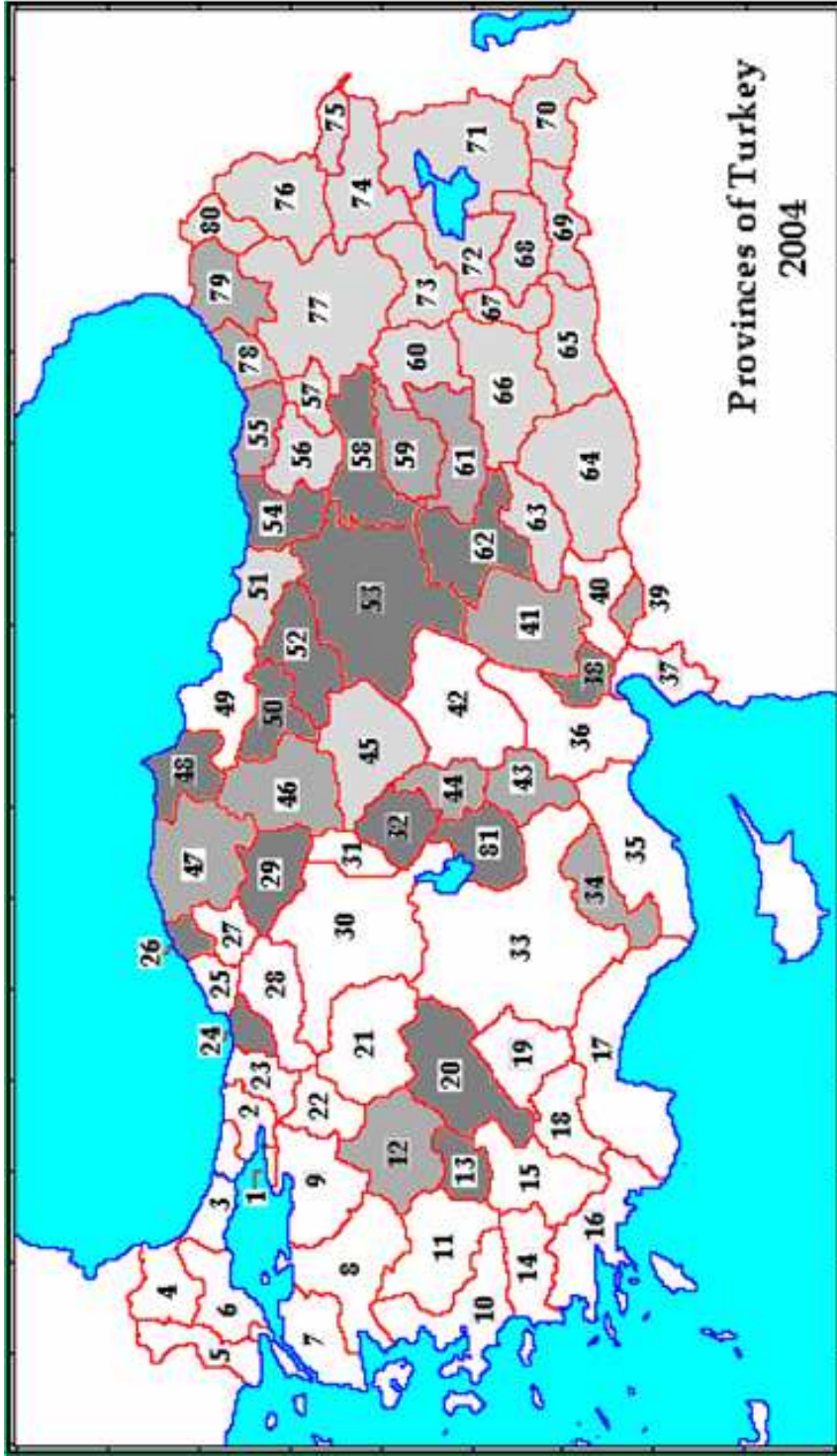


Figure 2: Location of provinces covered by laws 4325 (light grey), 5084 (dark grey) and 5350 (intermediate grey)

1 Yalova	10 Izmir	19 Isparta	28 Bolu	37 Hatay	46 Corum	55 Trabzon	64 Sanli Urfa	73 Mus
2 Kocaeli	11 Manisa	20 Afyon	29 Cankiri	38 Osmaniye	47 Kastamonu	56 Gumushane	65 Mardin	74 Agri
3 Istanbul	12 Kutahya	21 Eskisehir	30 Ankara	39 Kilis	48 Sinop	57 Bayburt	66 Diyarbakir	75 Igdır
4 Kirklareli	13 Usak	22 Bilecik	31 Kirikkale	40 Gaziantep	49 Samsun	58 Erzincan	67 Batman	76 Kars
5 Edirne	14 Aydin	23 Sakarya	32 Kirsehir	41 Maras	50 Amasya	59 Tunceli	68 Siirt	77 Erzurum
6 Tekirdag	15 Denizli	24 Duzce	33 Konya	42 Kayseri	51 Ordu	60 Bingol	69 Sirtak	78 Rize
7 Canakkale	16 Mugla	25 Zonguldak	34 Karaman	43 Nigde	52 Tokat	61 Elazig	70 Hakkari	79 Artvin
8 Balikesir	17 Antakya	26 Bartin	35 Mersin	44 Nevsehir	53 Sivas	62 Malatya	71 Van	80 Ardahan
9 Bursa	18 Burdur	27 Karabuk	36 Adana	45 Yozgat	54 Giresun	63 Adiyaman	72 Bitlis	81 Aksaray

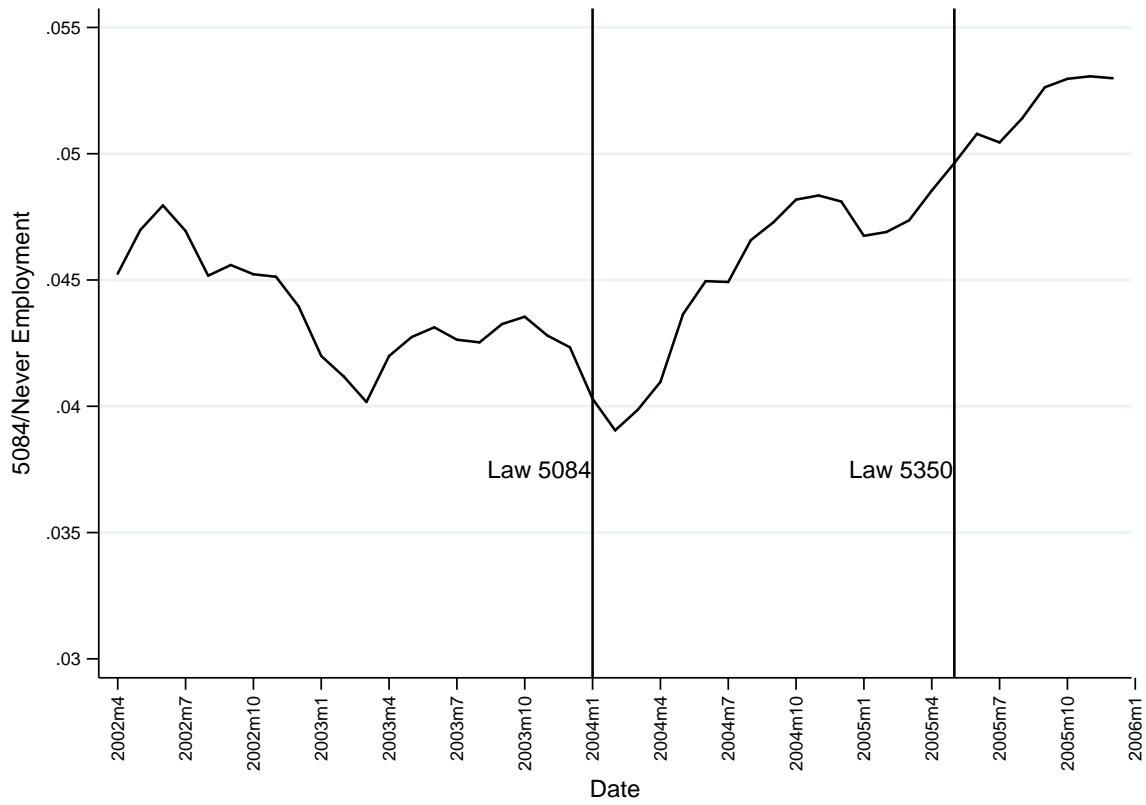


Figure 3: Ratio of employment in provinces D_{5084} relative to employment in D_{never} provinces

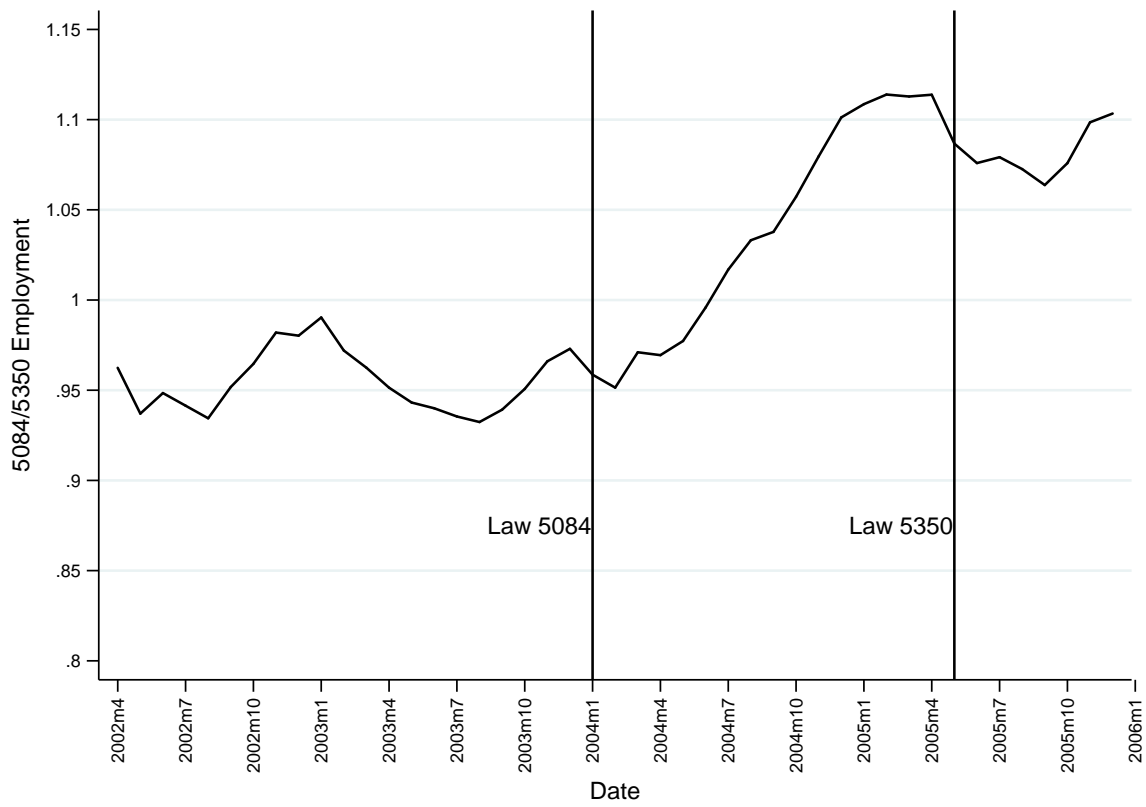


Figure 4: Ratio of employment in provinces D_{5084} relative to employment in D_{5350} provinces

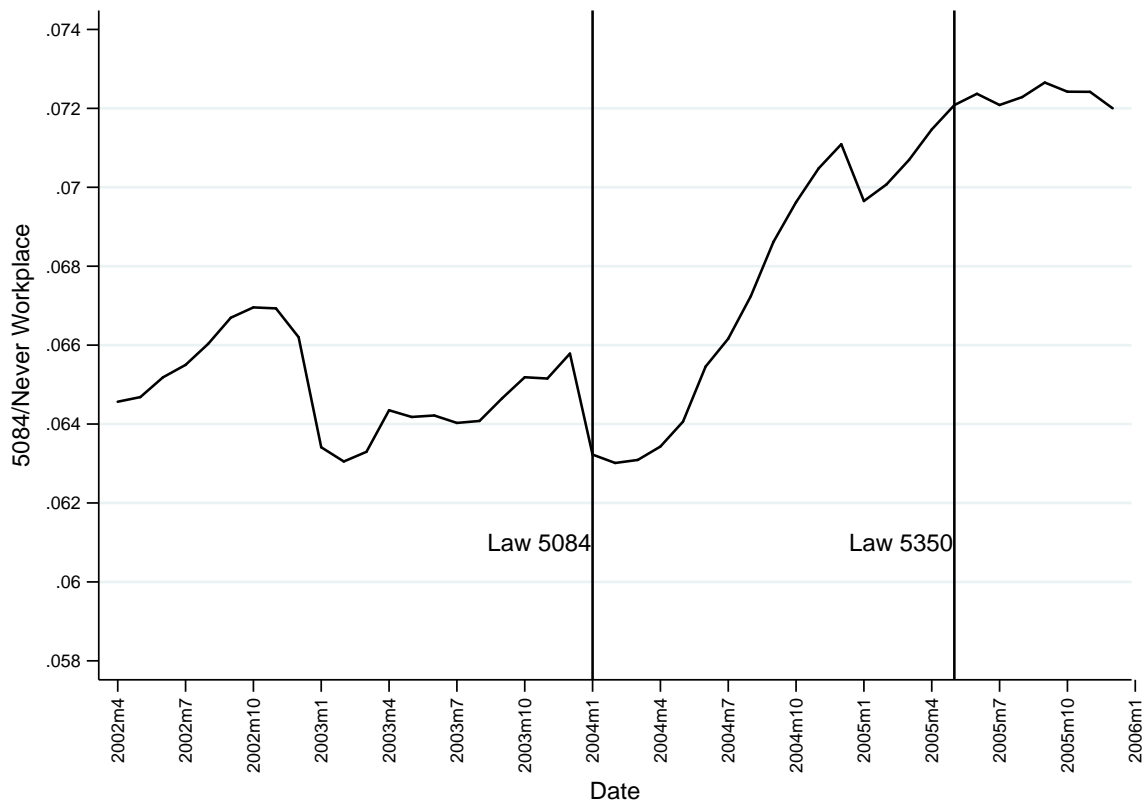


Figure 5: Ratio of number of establishments in provinces $D_{.5084}$ relative to employment in D_{never} provinces

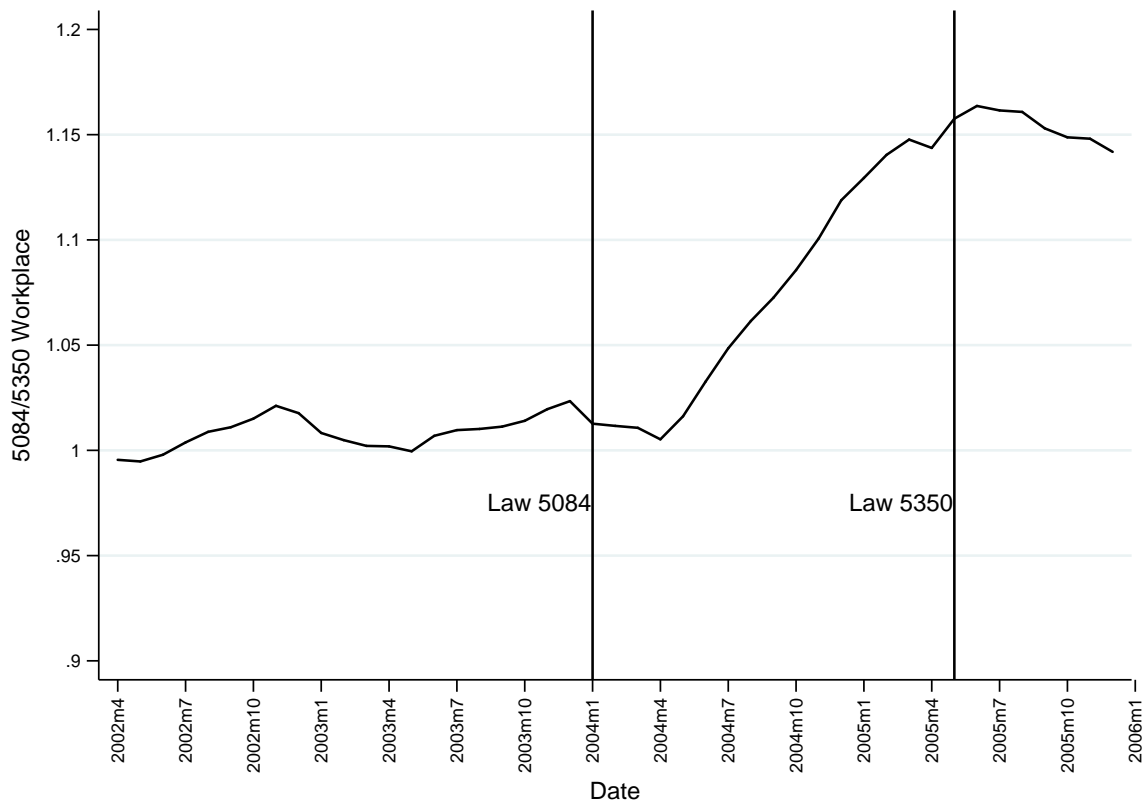


Figure 6: Ratio of number of establishments in provinces $D_{.5084}$ relative to employment in $D_{.5350}$ provinces

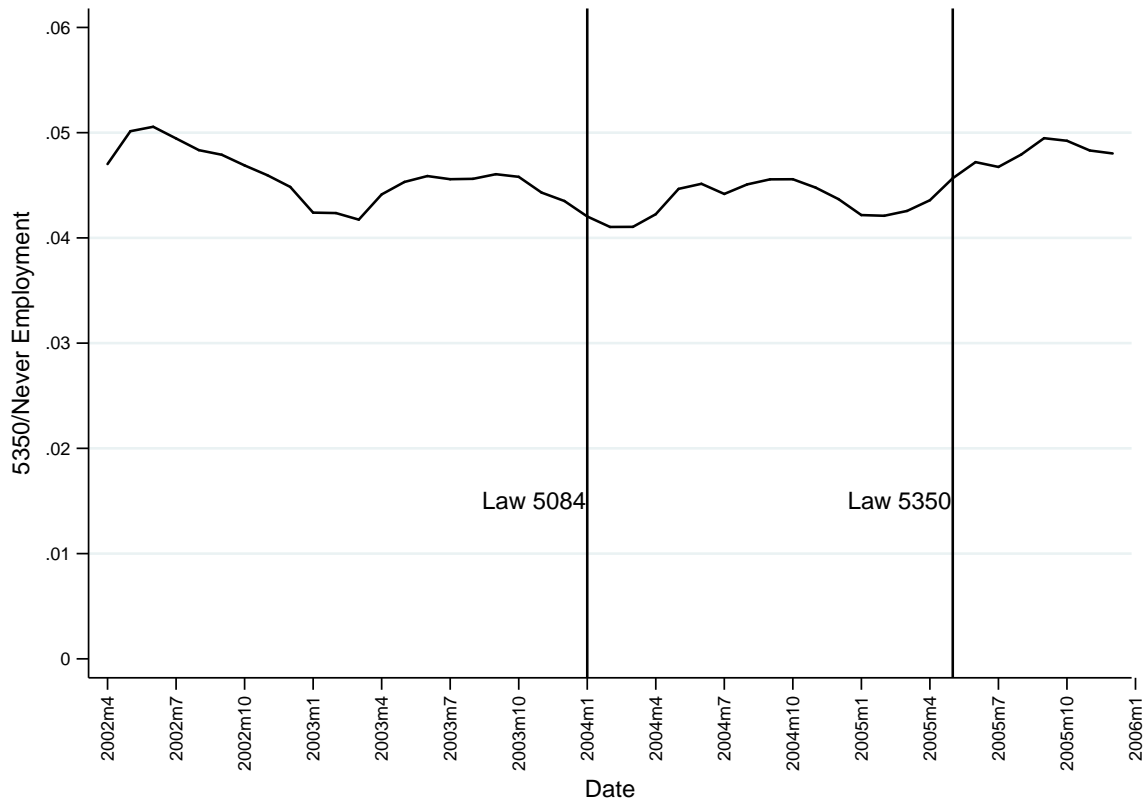


Figure 7: Ratio of employment in provinces D_{5350} relative to employment in D_{never} provinces

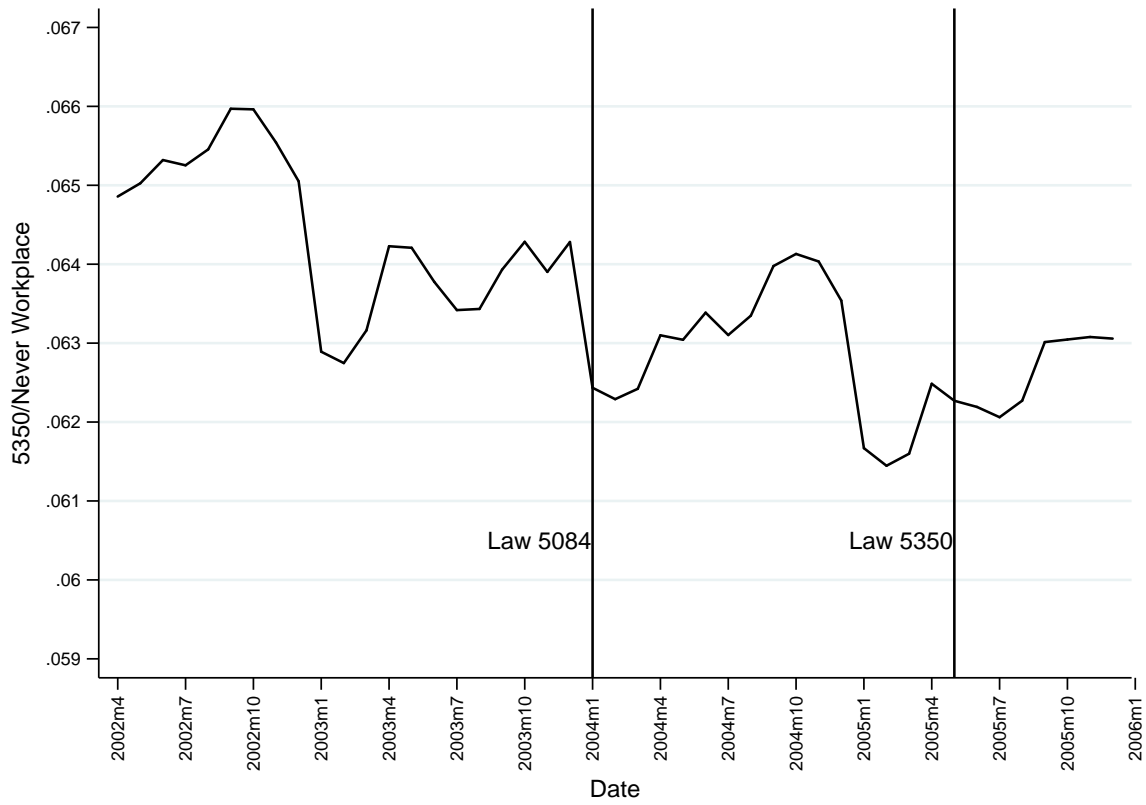


Figure 8: Ratio of number of establishments in provinces *D.5350* relative to employment in *D_never* provinces

Table 1: Summary statistics of key variables (province-month as unit observation)

Variable	Obs	Mean	Std. Dev.
Employment	3555	65831.18	216342.8
Workplaces	3555	9781.886	28286.44
Taxable earnings (YTL)	3555	39944968	153574145
SSK subsidy provided under Law 4325 (YTL)	945	56376.43	176026.9
SSK subsidy provided under Law 5084 (YTL)	675	153739	213317.8
SSK subsidy provided under Law 5350 (YTL)	585	47354.04	156905.3
Energy subsidy (YTL)	700	185594	358826.1

Note: SSK subsidy statistics are calculated using the sample of provinces covered by the corresponding law. Statistics for the energy subsidy are calculated using all subsidized provinces.

Table 2: Comparison of groups of provinces

		Provinces			
		<i>D_4325</i>	<i>D_5084</i>	<i>D_5350</i>	<i>D_never</i>
GDP per capita (USD 2001)	mean	941.14	1355.6	1798.38	2545.31
	sd	242.34	222.88	199.94	928.32
Population per km2 (2000)	mean	53.18	76.4	63.85	176.88
	sd	31.25	67.68	52.19	333.52

Source: Turkish Statistical Institute and Census.

Table 3: Summary statistics for registered employment growth

		Full Period (April 2002- December 2005)	Period 0 (April 2002- December 2003)	Period 1 (January 2004- April 2005)	Period 2 (May 2005 December 2005)
All	mean	0.013	0.008	0.016	0.02
provinces	sd	0.088	0.101	0.085	0.046
<i>D_4325</i>	mean	0.018	0.012	0.02	0.029
provinces	sd	0.102	0.121	0.088	0.07
<i>D_5084</i>	mean	0.013	0.004	0.02	0.023
provinces	sd	0.042	0.045	0.043	0.029
<i>D_5350</i>	mean	0.013	0.012	0.01	0.026
provinces	sd	0.124	0.173	0.06	0.052
<i>D_never</i>	mean	0.01	0.007	0.013	0.012
provinces	sd	0.078	0.062	0.107	0.031

Table 4: Summary statistics for registered workplace growth

		Full Period (April 2002- December 2005)	Period 0 (April 2002- December 2003)	Period 1 (January 2004- April 2005)	Period 2 (May 2005 December 2005)
All	mean	0.009	0.007	0.01	0.013
provinces	sd	0.032	0.034	0.033	0.024
<i>D_4325</i>	mean	0.012	0.009	0.014	0.017
provinces	sd	0.052	0.057	0.051	0.041
<i>D_5084</i>	mean	0.01	0.007	0.013	0.011
provinces	sd	0.024	0.023	0.028	0.013
<i>D_5350</i>	mean	0.008	0.007	0.005	0.015
provinces	sd	0.03	0.033	0.032	0.019
<i>D_never</i>	mean	0.007	0.006	0.007	0.011
provinces	sd	0.018	0.018	0.019	0.012

Table 5: Summary statistics for real wage growth

		Full Period (April 2002- December 2005)	Period 0 (April 2002- December 2003)	Period 1 (January 2004- April 2005)	Period 2 (May 2005 December 2005)
All provinces	mean	0.003	0.004	0.003	0
	sd	0.104	0.115	0.109	0.05
<i>D_4325</i> provinces	mean	-0.003	-0.009	0.002	-0.001
	sd	0.102	0.123	0.086	0.068
<i>D_5084</i> provinces	mean	0.004	0.008	0	-0.001
	sd	0.071	0.084	0.066	0.037
<i>D_5350</i> provinces	mean	0.007	0.012	0.003	-0.001
	sd	0.125	0.171	0.073	0.048
<i>D_never</i> provinces	mean	0.005	0.006	0.006	0.001
	sd	0.108	0.09	0.146	0.041

Table 6: Employment Results

	Control Group: "Never" Provinces				Control Group: "D_5350" Provinces			
	Dependent Variable: Log(employee)		Dep. Var: Emp. Growth		Dependent Variable: Log(employee)		Dep. Var: Emp. Growth	
	1. period dummies	1. date dummies	1. date dummies	2. province dummies	1. period dummies	1. date dummies	1. date dummies	2. province dummies
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>D_4325 * Period_1</i>	-0.013 (0.030)	-0.013 (0.030)	0.166*** (0.026)	0.002 (0.006)				
<i>D_4325 * Period_2</i>	0.194*** (0.040)	0.194*** (0.040)	0.452*** (0.052)	0.012** (0.005)				
<i>D_5084 * Period_1</i>	0.041* (0.023)	0.041* (0.024)	0.079*** (0.023)	0.009*** (0.003)	0.127*** (0.037)	0.127*** (0.038)	0.021 (0.026)	0.018** (0.008)
<i>D_5084 * Period_2</i>	0.181*** (0.036)	0.181*** (0.036)	0.235*** (0.038)	0.015*** (0.003)				
<i>D_5350 * Period_1</i>	-0.085** (0.036)	-0.085** (0.037)	0.045*** (0.016)	-0.009 (0.008)				
<i>D_5350 * Period_2</i>	0.005 (0.041)	0.005 (0.042)	0.194*** (0.030)	0.009 (0.007)				
<i>D_4325</i>	-2.616*** (0.290)							
<i>D_5084</i>	-1.685*** (0.252)				-0.006 (0.315)			
<i>D_5350</i>	-1.477*** (0.292)							
<i>Period_1</i>	0.144*** (0.013)				0.057* (0.031)			
<i>Period_2</i>	0.281*** (0.018)							
Constant	10.924*** (0.202)	11.501*** (0.014)	11.538*** (0.012)	0.041*** (0.006)	9.246*** (0.276)	10.053*** (0.019)	10.049*** (0.018)	-0.018* (0.009)
Observations	3555	3555	3555	3476	1036	1036	1036	1008
Adjusted R-squared	0.559	0.995	0.996	0.135	0.005	0.985	0.99	0.131
P4325	0	0	0	0.045				
P5084	0	0	0	0.127				
P5350	0	0	0	0.002				

Notes: Robust standard errors in parentheses. Specifications (1)-(4) are estimated with all provinces and periods. Specifications (5)-(8) are estimated with only periods 0 and 1 and *D_5084* and *D_5350* provinces. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 8: Taxable earnings per worker

	Control Group: "Never" Provinces				Control Group: "D_5350" Provinces			
	Dependent Variable: Log(employee)		Dep. Var: Emp. Growth		Dependent Variable: Log(employee)		Dep. Var: Emp. Growth	
	1. date dummies	1. date dummies 2. province dummies specific trend	1. date dummies 2. province dummies	Emp. Growth	1. date dummies 2. province dummies specific trend	1. date dummies 2. province dummies	1. date dummies 2. province dummies	Emp. Growth
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>D_4325 * Period_1</i>	-0.096*** (0.021)	-0.096*** (0.021)	0.135*** (0.023)	0.011** (0.004)				
<i>D_4325 * Period_2</i>	-0.148*** (0.016)	-0.148*** (0.017)	0.186*** (0.029)	0.013*** (0.004)				
<i>D_5084 * Period_1</i>	0.003 (0.014)	0.003 (0.015)	-0.006 (0.011)	-0.009** (0.004)	-0.019 (0.022)	-0.019 (0.023)	-0.007 (0.019)	0 (0.007)
<i>D_5084 * Period_2</i>	-0.039** (0.015)	-0.039** (0.015)	-0.051*** (0.017)	-0.005 (0.003)				
<i>D_5350 * Period_1</i>	0.029 (0.017)	0.029 (0.018)	-0.009 (0.014)	-0.01 (0.007)				
<i>D_5350 * Period_2</i>	0.002 (0.019)	0.002 (0.019)	-0.054** (0.021)	-0.01 (0.007)				
<i>D_4325</i>	-0.032 (0.030)							
<i>D_5084</i>	-0.125*** (0.040)				0.02 (0.046)			
<i>D_5350</i>	-0.143*** (0.041)							
<i>Period_1</i>	0.099*** (0.006)				0.121*** (0.018)			
<i>Period_2</i>	0.051*** (0.008)							
Constant	-2.844*** (0.026)	-2.630*** (0.020)	-2.626*** (0.015)	-0.012** (0.006)	-2.990*** (0.035)	-3.024*** (0.020)	-3.039*** (0.015)	-0.01 (0.008)
Observations	3555	3555	3555	3476	1036	1036	1036	1008
Adjusted R-squared	0.191	0.744	0.794	0.169	0.134	0.774	0.824	0.151
P4325	0	0	0.007	0.76				
P5084	0	0	0.001	0.464				
P5350	0.002	0.002	0.001	0.972				

Notes: Robust standard errors in parentheses. Specifications (1)-(4) are estimated with all provinces and periods. Specifications (5)-(8) are estimated with only periods 0 and 1 and *D_5084* and *D_5350* provinces. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 9: Comparison of Minimum Wage to Average Taxable Earnings Per Worker

Date	4325 provinces	5084 provinces	5350 provinces	Never provinces	Minimum wage
April 2002-June 2006	496.00	324.21	308.77	442.61	222.00
July 2002-December 2002	397.23	357.95	332.66	461.04	250.88
January 2003-December 2003	439.28	438.16	424.99	556.91	306.00
January 2004-June 2004	568.53	565.34	551.88	694.34	423.00
July 2004-December 2004	509.23	505.77	499.15	663.45	444.15
January 2005-December 2005	537.56	542.62	540.45	718.77	488.70

Table 10: Interrupted Panel Results (Employment)

	Control Group: "Never" Provinces				Control Group: "D_5350" Provinces			
	Dependent Variable: Log(employee)		Dep. Var: Emp. Growth		Dependent Variable: Log(employee)		Dep. Var: Emp. Growth	
	1. period dummies	1. date dummies	1. date dummies	1. date dummies	1. period dummies	1. date dummies	1. date dummies	1. date dummies
	2. group dummies	2. province dummies	2. province dummies	2. province dummies	2. group dummies	2. province dummies	2. province dummies	2. province dummies
		specific trend	specific trend	specific trend		specific trend		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>D_4325 * Period_1</i>	0.028 (0.032)	0.028 (0.032)	0.331*** (0.041)	0.005 (0.009)				
<i>D_4325 * Period_2</i>	0.226*** (0.042)	0.226*** (0.043)	0.699*** (0.072)	-0.014 (0.010)				
<i>D_5084 * Period_1</i>	0.056* (0.028)	0.056* (0.029)	0.157*** (0.034)	0.017*** (0.005)	0.127*** (0.037)	0.127*** (0.039)	0.059 (0.039)	0.032*** (0.010)
<i>D_5084 * Period_2</i>	0.194*** (0.039)	0.194*** (0.039)	0.351*** (0.051)	0.010 (0.006)				
<i>D_5350 * Period_1</i>	-0.073** (0.034)	-0.073** (0.035)	0.073*** (0.021)	-0.015 (0.009)				
<i>D_5350 * Period_2</i>	0.014 (0.045)	0.014 (0.046)	0.242*** (0.040)	-0.014 (0.010)				
<i>D_4325</i>	-2.618*** (0.291)							
<i>D_5084</i>	-1.681*** (0.252)				-0.008 (0.317)			
<i>D_5350</i>	-1.472*** (0.294)							
<i>Period_1</i>	0.168*** (0.014)				0.097*** (0.028)			
<i>Period_2</i>	0.305*** (0.018)							
Constant	10.914*** (0.202)	11.507*** (0.013)	11.529*** (0.012)	-0.018*** (0.006)	9.240*** (0.278)	10.053*** (0.019)	10.046*** (0.017)	0.027*** (0.007)
Observations	2607	2607	2607	2370	784	784	784	728
Adjusted R-squared	0.558	0.995	0.996	0.116	0.008	0.986	0.99	0.10

Notes: Robust standard errors in parentheses. Specifications (1)-(4) are estimated with all provinces and periods. Specifications (5)-(8) are estimated with only periods 0 and 1 and *D_5084* and *D_5350* provinces. All regressions exclude the three months before and after each law. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 11: Interrupted Panel Results (Number of establishments)

	Control Group: "Never" Provinces				Control Group: "D_5350" Provinces			
	Dependent Variable: Log(employee)				Dependent Variable: Log(employee)			
	1. date dummies		1. date dummies		1. date dummies		1. date dummies	
	1. period dummies	1. date dummies	1. date dummies	1. period dummies	1. date dummies	1. date dummies	1. date dummies	Dep. Var: Emp. Growth
	2. group dummies	2. province dummies	2. province specific trend	2. group dummies	2. province dummies	2. province dummies	2. province specific trend	2. province dummies
<i>D_4325 * Period_1</i>	0.025 (0.017)	0.025 (0.017)	0.182*** (0.028)	0.025 (0.017)	0.008*** (0.003)	0.008*** (0.003)		
<i>D_4325 * Period_2</i>	0.141*** (0.031)	0.141*** (0.031)	0.386*** (0.055)	0.141*** (0.031)	-0.010** (0.005)	-0.010** (0.005)		
<i>D_5084 * Period_1</i>	0.040** (0.016)	0.040** (0.017)	0.068*** (0.015)	0.040** (0.017)	0.010*** (0.002)	0.010*** (0.002)	0.059*** (0.021)	0.014*** (0.003)
<i>D_5084 * Period_2</i>	0.097*** (0.027)	0.097*** (0.027)	0.140*** (0.026)	0.097*** (0.027)	-0.004* (0.003)	-0.004* (0.003)		
<i>D_5350 * Period_1</i>	-0.021 (0.018)	-0.021 (0.019)	0.032* (0.016)	-0.021 (0.019)	-0.005* (0.003)	-0.005* (0.003)		
<i>D_5350 * Period_2</i>	-0.032 (0.034)	-0.032 (0.034)	0.051 (0.033)	-0.032 (0.034)	-0.000 (0.004)	-0.000 (0.004)		
<i>D_4325</i>	-2.250*** (0.271)	-2.250*** (0.271)		-2.250*** (0.271)				
<i>D_5084</i>	-1.355*** (0.225)	-1.355*** (0.225)		-1.355*** (0.225)				
<i>D_5350</i>	-1.282*** (0.286)	-1.282*** (0.286)		-1.282*** (0.286)				
<i>Period_1</i>	0.134*** (0.007)	0.134*** (0.007)		0.134*** (0.007)				
<i>Period_2</i>	0.262*** (0.012)	0.262*** (0.012)		0.262*** (0.012)				
Constant	9.095*** (0.188)	9.633*** (0.008)	9.645*** (0.007)	7.639*** (0.268)	0.002 (0.004)	8.441*** (0.009)	8.419*** (0.009)	0.022*** (0.002)
Observations	2607	2607	2607	784	2370	784	784	728
Adjusted R-squared	0.518	0.998	0.999	0.012	0.364	0.997	0.998	0.581

Notes: Robust standard errors in parentheses. Specifications (1)-(4) are estimated with all provinces and periods. Specifications (5)-(8) are estimated with only periods 0 and 1 and *D_5084* and *D_5350* provinces. All regressions exclude the three months before and after each law. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 13: Consumption of Electricity in treated and control provinces

	Dependent Variable: Log(total electricity)			Dependent Variable: Growth of Electricity
	1. period dummies 2. group dummies	1. date dummies 2. province dummies	1. date dummies 2. province specific trend	1. date dummies 2. province dummies
	(1)	(2)	(3)	(4)
<i>D_4325_post</i>	-0.069* (0.036)	-0.069 (0.044)	0.03 (0.095)	0.039 (0.078)
<i>D_5084_post</i>	-0.055** (0.027)	-0.055* (0.033)	0.006 (0.052)	0.006 (0.049)
<i>D_5350_post</i>	-0.03 (0.024)	-0.03 (0.030)	-0.021 (0.064)	-0.025 (0.060)
<i>D_post</i>	0.140*** (0.022)			
<i>D_4325</i>	-2.038*** (0.258)			
<i>D_5084</i>	-1.731*** (0.222)			
<i>D_5350</i>	-1.468*** (0.250)			
<i>Year_2003</i>		0.067*** (0.015)	0 (0.018)	0.002 (0.037)
<i>Year_2004</i>		0.174*** (0.031)	0 0.000	0 0.000
Constant	14.316*** (0.169)	14.909*** (0.014)	14.875*** (0.006)	0.122*** (0.018)
Observations	243	243	243	162
Adjusted R-squared	0.529	0.996	0.997	-0.043

Notes: Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 14: Expenditures under Law 5084 in the 15 newly subsidized provinces and cost per job creation under different assumptions, January 2004 to April 2005

	Source of data	Cost (nominal YTL)	Number of job-months	Cost per job-month in YTL
Subsidies for social security	Actual subsidies (SSK)	54,435,194		
Subsidies for income tax	Derived estimates	38,062,783		
Energy subsidies	Actual subsidies (Treasury)	19,777,792		
Total subsidy costs (excl. land)		112,275,769		
Jobs receiving subsidies	Actual data (SSK)		739,757	152
Estimated net jobs created (low)	Estimated from specification in Table 6 column (1): 9,787 jobs ("D_never provinces" as control)		137,017	819
Estimated net jobs created (medium)	Estimated from specification in Table 6 column (3): 18,194 jobs ("D_never provinces" as control)		254,710	441
Estimated net jobs created (high)	Estimated from specification in Table 6 column (5): 28,002 jobs ("D_5350 provinces" as control)		392,031	286

Employment is reported as "number of job months" because employees receiving subsidies are reported by SSK on a monthly basis. These monthly figures have been added up for the March 2004-April 2005 months (the months in Period 1 when subsidies were paid). To calculate job-months for our estimates of the net employment effect, we have multiplied the estimated jobs by the number of months when subsidies were received in Period 1.

Table 15: Expenditures under Law 5350 in the 13 newly subsidized provinces and cost per job creation under different assumptions, May to December 2005

Source of data	Cost (nominal YTL)	Number of job-months	Cost per job-month in YTL
Subsidies for social security	27,702,115		
Subsidies for income tax	27,659,233		
Energy subsidies	37,609,583		
Total subsidy costs (excl. land)	92,970,931		
Jobs receiving subsidies		368,551	252
Estimated net jobs created (low)		175,590	529
Estimated net jobs created (high)		275,771	337

Employment is reported as “number of job months” because employees receiving subsidies are reported by SSK on a monthly basis. These monthly figures have been added up for the May 2005-December 2005 months. To calculate job-months for our estimates of the net employment effect, we have multiplied the estimated jobs by the number of months when subsidies were received in Period 2.