The Effects of Subsidizing Social Security Contributions: Job Creation or Informality Reduction?*

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Abstract

This paper evaluates the impact of an employment subsidy scheme covering employers' social contribution costs on registered employment in small firms in Turkey. It utilizes a rich, firm-level administrative data set with monthly frequency, which allows for closely following the dynamics of registered employment in firms before and after the implementation of the subsidy. The empirical approach utilizes the geographically targeted implementation of the subsidy and unique legislation changes in estimating the effects using a difference-in-difference specification. The paper finds that the subsidy scheme had a sizable and positive impact on registered employment in small firms, which is larger than evidenced by prior literature. Estimates are robust across specifications, to the choice of control groups, and to new inference methods addressing potential biases in staggered difference in differences designs. Corroborative evidence suggests that the positive effects on registered employment are mainly driven by the formalization of existing workers as opposed to new job creation. Therefore, the results indicate that social security contribution subsidies in small firms can be effective in reducing informality in contexts where informal employment remains common.

JEL codes: H32, J23, J32.

Keywords: Employment Subsidies, Social Security Contributions, Formal Employment, Regional Subsidies.

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1 Introduction

Active labor market policies (ALMPs) have been used globally to help ease a wide range of labor market problems, including youth unemployment and persistent joblessness among displaced adults. Several active labor market policies are available to policymakers to boost employment. Among them, private employment subsidies have been used to cover part of the labor costs borne by employers, particularly for low-wage workers. In developing economies where unregistered – or informal – employment remains common, employment subsidies can also serve an additional purpose. In addition to increasing total employment, they can also help increase the proportion of workers who are employed formally – as opposed to being unregistered – by partially covering the labor costs of formal employment. By doing this, employment subsidies reduce the relative attractiveness of hiring workers "off the books" for firms compared to formally employing workers. This is a potential benefit of importance, as reducing informality in the labor market is a common policy priority in many developing countries.

In this paper, we examine the effects of a geographically targeted subsidy that covered part of the employers' social security cost on registered employment in Turkey between 2012-2018. While small firms with fewer than 10 employees were not eligible for the subsidy before 2016, they became eligible after the legislation change in 2016, which we exploit as a quasi-natural experiment in our first set of estimations. Furthermore, in August 2020, the Turkish government passed yet another legislation which no longer classified subsidy regions at the level of provinces, but at the level of counties. This legislation change meant that unlike in the previous classification, a county could still be eligible for subsidies even if the associated province had high levels of development. Thus the legislation recognized that some of the previously untreated counties had the same socio-economic development levels as our treated counties and were included in the same subsidy region in August 2020, -after our sample period. In our second set of estimations, we further exploit the reclassification of subsidy regions and rely on an identification strategy which uses the firms in later covered counties as credible control units for the treated firms between 2016 and 2018.

In estimating the causal effects of the subsidy on registered employment, we use a rich monthly firm-level administrative data with 37.2 million observations. We employ several difference-in-difference specifications which yield similar results. In the context of regional employment subsidies in Turkey, taking up the subsidy is not automatic. Firms in eligible regions may start benefiting at different times although the legislation change is the same for all firms, making our case a staggered design. Recent work by Goodman-Bacon (2021), Wooldridge (2005), Borusyak et al. (2021), De Chaisemartin and d'Haultfoeuille (2020) point out that in staggered difference in difference models, if the effects are heterogenous over time, two way fixed effects models may suffer from negative weights which may lead to serious biases in estimations. To address these concerns, we employ several robustness checks, follow the newly proposed unbiased estimators for staggered

designs and show that our results remain very similar under both cases.

We find that the subsidy significantly increased registered employment in small firms and that the effects are sustained over time. The magnitude of the estimated employment effects is on the high end of the literature. The subsidy, which covered about 25 percent of social security costs and about 6 percent of total labor costs, increased registered employment in small firms that received the subsidy by 5 percent to 9 percent depending on the specification used. Our results are quite robust to narrowing the geographical control group of firms used for the estimation. In addition, positive effects on registered employment appear fairly constant over the 3-year period during which the subsidy was implemented. Finally, we also provide corroborative evidence suggesting that the rise in registered employment in small firms attributable to the subsidy mostly resulted from the formalization of existing workers, rather than new job creation. Cost-benefit analysis suggests that the range of the effect size is not sufficient to equalize the cost of the subsidy to the government to the benefit received in terms of taxes and social security premiums. However, if a worker stays in the same firm after benefiting from the subsidy for four years, the net present value of the costs associated with covering the worker's subsidy equalizes the net present value of premium and tax payments by the worker in less than a year after the end of the subsidy period.

The paper makes a number of contributions to the literature. First, although the literature on wage subsidies has been growing in recent years, there remain relatively few studies that rigorously measure the impact of wage subsidies, particularly of those that cover the costs of social security contributions by employers. One reason for the scarcity of evidence is that wage subsidies, in the form of targeted cuts to employers' social security contributions, have not been implemented in practice in many countries. Despite the large agreement in the theoretical literature that these subsidies should be effective, there is relatively little empirical research that has examined the effectiveness of the (employer-side) wage subsidies. This is in marked contrast to a large literature that has examined the effects of the targeted tax cuts for employees. Such evidence is even more sparse in the context of developing economies that face a high level of informal employment compared to high-income countries.

Second, our paper uses rich and unique firm-level data from national social security records on the universe of firms and registered employees in the country. It is one of the very few studies that uses detailed firm-level information to study this question and, to the best of our knowledge, the first to do so in the context of a developing economy where informal employment remains common. In contrast, prior studies on the topic mostly rely on more aggregate administrative data at a large geographical level (Betcherman et al., 2010) or on samples from nationally-representative survey (Balkan et al., 2016) to evaluate employment effects. In contrast, we are able to take advantage of granular administrative data, where registered employment is reported with monthly frequency

over the past 15 years for each firm in the country. This allows to closely look at firm-level employment dynamics over time before and after the subsidy implementation.

Another important way in which the paper differs from the previous literature is that it studies the effect of a social security contribution subsidy on small firms.[1] This allows to gain new insights on the effectiveness on such schemes. The focus on small firms is especially valuable in contexts where unregistered employment remains common, like the one studied in this paper. Small firms tend to be both less productive and typically face a lower probability of detection of informal employment due to their size. As a result, they are more prone to hire workers informally, in order to avoid the costs of registered employment, as opposed to larger firms where most workers are employed formally. Hence, the effectiveness of employment subsidy schemes aimed at increasing employment and reducing informality is likely to differ by firm size. Second, small firms – with fewer than 10 employees – still constitute the large majority of firms in middle-income countries, as in Turkey, and represent a substantial share of total employment.

Turkey is a relevant country case to study for several reasons. First, Turkey has a relatively higher firm tax wedge compared to the OECD average, with on average about 39 percent of the total cost of labor to the employer paid to the government in the form of taxes or social security contributions (OECD, 2019).[2] Second, despite substantial progress in recent years to reduce informality, employing workers "off the books" without paying any social security contribution remains common, particularly among small firms.[3] This is the case not only in Turkey, but also in the vast majority of developing economies. The effects of employment subsidies on employment in a context where registered employment is common may thus differ from a high-income context where there is hardly any informality. In addition to job creation, an increase in registered employment may thus also result from a formalization of already employed workers enabled by the subsidy.

The paper proceeds as follows. Section 2 discusses related literature. Section 3 provides some institutional background on employer social contributions in Turkey, and on the additional 6-point subsidy studied in this paper. Section 4 describes our administrative dataset. Section 5 presents our methodology and identification strategy. Section 6 reports and discusses our main results. Section 7 presents the results of the cost-benefit analysis of the subsidy, Section 8 concludes.

[1] The employment subsidy studied by Betcherman et al. (2010) in the Turkish context was targeted to firms larger than 10 employees.

[2] In 2020, Turkey had the 15th highest tax wedge among the 37 OECD member countries, occupying the same position in 2019 (calculated for a single worker).

[3] As of 2018, about one third of workers in non-agriculture in Turkey were informally employed according to the Turkish Household Labor Force Survey (HLFS).

2 Related Literature

Employment subsidies aim to reduce the cost of labor to employers. They can be applied to all employees or only to new hires (marginal subsidies). They can also be general, in the sense of applying to all workers and establishments, or specific groups, 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. While wage subsidies can be directed to either employees or employers, in line with the policy analyzed in our study this literature overview focuses on hiring subsidies going to employers.

Prior evidence on the employment effects of employment subsidies primarily comes from high-income countries where labor markets are characterized by very little informal employment. In addition, most studies for high-income countries examine the effect of programs targeted to the unemployed or disadvantaged workers and in many cases, estimates of effects are based on surveys to employers. One of the best-known programs is the one implemented in France in the 1990s, which reduced the employer portion of the payroll tax for the low-wage workers. The impact of the French program was estimated by several papers including Kramarz and Philippon (2001) and Crépon et al. (2003). Kramarz and Philippon (2001) base their evaluation on household survey data and examine the effects of changes in the minimum labor costs - hence capturing the effects of both the changes in the minimum wage and the changes in payroll tax subsidies at the minimum wage level. Their analysis regarding the effects of a decrease in labor costs due to an increase in the payroll tax subsidy reveals no significant employment effects. In contrast, Crépon et al. (2003) perform their analysis with firm-level employment as the key dependent variable. They calculate the ex-ante change in labor costs due to the payroll tax subsidies, using payroll tax parameters and the composition of the firm's labor force before the introduction of the payroll tax changes. They find that employment in firms that received larger subsidies grew more than employment in firms that employed fewer low-wage workers and hence received fewer subsidies. The authors interpret this as strong evidence for the employment effects of low-wage subsidies.

In Belgium, Goos and Konings (2007) evaluated the employment effects of the "Maribel subsidies" implemented in the late 1990s that targeted manual workers. Unlike the French subsidy program, the program did not directly target low-wage workers but the ex-post outcomes suggest that it operated mostly on low-wage workers. They evaluate the employment effects of the subsidy using firm-level data and report that the program has been effective in improving the employment prospects of those workers in the target group. Huttunen et al. (2013) evaluate another subsidy program implemented by the Finnish government in 2006. Their subsidy program was similar to the French one in the sense that the target was low-wage workers. However, the Finnish program imposed an additional constraint: it focused on the older low-wage workers. Thus, identification of causal effects would be easier than the French case since there were clearly defined treatment

and control groups among the low-wage workers. The authors show, using a triple difference-in-difference approach, that the employment subsidy program had no significant effect on the employment probabilities of those workers in the target group.

In the UK, Blundell et al. (2004) find that hiring subsidies have increased the employment probabilities of those in the target group by around 5 percent, but this effect likely vanishes in the long run. In Italy, Deidda et al. (2015) document using a propensity-score matching approach that employment subsidies in the Sicily region had significant effects on the employment probabilities of older and unskilled women. In Germany, Boockmann et al. (2012) find using a DID strategy that hiring subsidies in Germany have partial effects, i.e., only on women in East Germany. In the developing country context, De Mel et al. (2019) test the impact of wage subsidies given to microenterprises to encourage them to hire workers in Sri Lanka. They find 24 percent of firms used the subsidy to hire a worker, resulting in an increase in employment while the subsidy is in effect and an increase in survival rate. But the employment effects of the subsidy vanish in the long run, with much of this impact disappearing as when the subsidy is removed, and no long-term impact after two years.

The few studies available for developing economies come primarily from Latin America and suggest that the effects of employment subsidies on formal employment are larger in contexts where levels of informality are high. Heckman and Pages (2003) estimate that a 10 percent tax cut leads to an increase in total employment (formal and informal) by 4.47 percent in Latin America and the Caribbean. Kugler and Kugler (2009) evaluate the impact of payroll tax increases in Colombia in 1996. In particular, payroll taxes in Colombia increased from 35.5 percent in 1980 to 51.5 percent in 1996. Kugler and Kugler (2009) estimate that a 10 percent increase in payroll taxes reduced formal employment by between 4 percent and 5 percent. One study from Argentina which exploits the fact that payroll taxes were reduced at differential rates at 85 regions of Argentina in 1993 does not find an impact on formal employment (Cruces et al. (2010)). In addition, they show that tax collection as a percentage of total wage income fell by almost half throughout the next decade as employment gains were null under this subsidy. In Chile, however, Gruber (1997) finds that a reduction in payroll taxes by 25 percent over 6 years had no effect on total employment but impacted wages.

Two meta-analyses by Card et al. (2010) and Card et al. (2018) have also summarized the results of evaluations of a large number of ALMPs, including private employment subsidies. Evidence from the meta-analyses primarily comes from high-income countries, in particular Germanic, Nordic and Anglo-Saxon countries. The authors report positive employment effects of employment subsidy programs which tend to strengthen over time. In addition, they report heterogeneous effects across target groups: female participants and those drawn from the pool of long-term unemployed tend to have larger program effects than other groups. In contrast, the program estimates for youths and

older workers are typically less positive than for other groups. Finally, they report that ALMPs, in particular employment subsidies, tend that to have larger impacts in periods of slow growth and higher unemployment.

A handful of papers have studied the impact of employment subsidies in the Turkish context. The most closely related study to ours is the study by Betcherman et al. (2010) who estimate the effect of two employment subsidies targeted to firms with more than 10 employees in Turkey. Similarly, as the subsidy studied in this paper, the subsidy was geographically targeted which allowed the authors to carry out a difference-in-difference estimation to estimate the effects of the subsidy. However, contrary to our study, they use aggregate data on labor market outcomes at the regional level to study those impacts. In addition, the subsidy was at the time only targeted to firms of more than 10 employees. In addition, it was applicable only to additional hires while the subsidy we study in this paper is applied to all employees. They find that the subsidies had a sizeable and positive impact on registered employment.

Uysal (2013), Ayhan (2013), and Balkan et al. (2016) study the effect of another employment subsidy in Turkey targeted to women and to youth introduced in 2011. All three papers carry out a difference-in-difference estimation to estimate the employment effects of the subsidy, with Ayhan (2013) conducting a triple difference-in-difference strategy. They use nationally representative survey data on workers from the Household Labor Force Survey (HLFS). The three studies report different results on the impact of the subsidy, although they all find that treatment effects tend to be larger for women. Uysal (2013) uses aggregate labor market data by demographic groups and emphasizes that the program has been effective for older rather than younger women, while Ayhan (2013) uses micro-level data in order to control for individual factors relevant for employment outcomes and finds that the program has been effective in increasing the employment probabilities of women, but the effect lasted only a short period of time after the intervention. Finally, Balkan et al. (2016) find that on aggregate, the subsidy program did not significantly affect the employment probabilities of individuals in the target group. However, they find that some subgroups have been disproportionately positively affected by the subsidy, such as older women. The divergence in findings of the different studies evaluating the same subsidy highlights the need for high-quality granular data and adequate identification to estimate causal effects.

3 Institutional Background

Turkey has a higher firm tax wedge compared to the OECD average, with on average about 39 percent of the total cost of labor to the employer paid to the government in the form of taxes or social security contributions (OECD, 2019). Social security contributions include disability, old

age and death insurance, unemployment insurance, short-term insurance branches including insurance for occupational accidents and illnesses and maternity, and general health insurance (Table 1).

Employment subsidies in Turkey have been used as an instrument to convert the existing informal employment into formal employment, as most subsidies cover part of registration costs for formal employment.[4] Despite a substantial decrease in informality, informal employment is still significantly high in Turkey: From around 50 percent at the beginning of the 2000s, the share of workers employed informally decreased to 33 percent in 2018 (Figure 1).[5] The Government of Turkey used a series of campaigns and legislations to bring down the level of informality (Erdogan and Del Carpio, 2019). The majority of employment subsidies in Turkey were originally implemented to boost the formal registration of workers by reducing the high formalization costs for employers, as social security premiums alone represent 34.5 percent of the gross wage, with additional costs for income and stamp taxes.

There were over a dozen employment subsidies in effect at the end of 2018 with different eligibility criteria and implemented to cover part of the registration costs for formal employment. Among the pool of employment subsidies which were implemented, the one with the largest coverage is the subsidy under Law No. 5510, commonly known as the "5 points reduction". The subsidy reduces the employer's share of disability, old age and death insurance from 11 percent to 6 percent, and is automatically applied if the firms meet the conditions of eligibility (please refer to the next part for details of the eligibility conditions for this subsidy). In practice, firms can benefit from a combination of different subsidies that they are eligible for as long as the subsidies are compatible. A maximum of two subsidies can be exploited for each worker in the same firm for each month.

3.1 The additional 6 points subsidy

The subsidy evaluated in this paper is the continuation of a series of subsidies targeted at firms in socioeconomically disadvantaged regions of Turkey. The first subsidy of this kind was initiated in 1998 under Law No. 4325. The subsidy targeted firms in the 22 provinces with annual per capita income levels below USD 1,500 and under the State of Emergency that was implemented from the late 1980s until the early 2000s. The subsidy covered deductions on income and establishment taxes. Firms with at least 10 employees in the target regions were eligible for the subsidy, and it applied only to the newly hired employees. This first subsidy remained in effect until the end of 2003.

Figure 2 provides the details of the timeline for the three subsidies that will be discussed in the

[4]The government also offers a limited number of wage subsidies that cover part of the net wage, e.g., to subsidize firms in the face of the significant increase in minimum wages in 2016, or to subsidize women and youth employment.

[5]The definition of informality used in this study concerns workers who are not registered in the social security system through their main job.

remainder of this section. In January 2004, a new and more comprehensive subsidy was initiated for similar target provinces: The "Incentive for Investment and Employment" (Law No. 5084) included deductions in employers' social security contributions, credits on income taxes on wages, subsidies on electricity consumption, as well as land subsidies. The subsidy initially included an additional 15 provinces, with an additional 13 provinces added to the list of eligible provinces in 2005 (Law No. 5350). There were significant changes made to the eligibility criteria of the subsidy as implementation progressed, and the final list of eligible provinces included a total of 49 provinces in Turkey. The impact of this employment subsidy on formal employment growth was studied by Betcherman et al. (2010) who find positive and sizeable effects of the subsidy on formal employment, which mainly originate from the formalization of already employed wage workers by firms. The subsidy was removed from effect at the end of 2012.

The abolition of the subsidy under Law No. 5084 led to significant negative feedback from firms in the eligible regions, and to avoid an increase in informality in these regions, the Government of Turkey initiated a new subsidy, the additional 6 points subsidy (Law No. 6486), in May 2013. To compensate for the period with no regional subsidy in the first five months of 2013, the subsidy included an option of benefiting retrospectively, such that firms that satisfied the conditions between January to May 2013 could ask for the reimbursement of their expenses that would have been covered by the subsidy if the subsidy was in effect during that period.

Fifty-one provinces and two additional districts are eligible for this subsidy, including all 49 provinces formerly eligible for the previous subsidy.[6] The Government of Turkey divided Turkey's provinces into 6 regions according to their development status, and the 6 points subsidy is available for Regions 4, 5 and 6, the three least developed regions of Turkey (Figure 3). The duration of the subsidy depends on the region of the province, and Region 4 provinces can benefit from the subsidy for 4 years, Region 5 for 5 years, and Region 6 for 6 years.

In line with the vast majority of employment subsidies in Turkey, the 6 additional points subsidy covers a percentage of employers' share of social security contributions. In particular, for eligible firms, this regional subsidy provides a reduction of 6 points in the employers' share of the Disability, Old Age, and Death Insurance premium. Benefiting from the 5 points subsidy is a necessary condition to benefit from the 6 additional point subsidy, and the two subsidies together eliminate the (11 percent) requirement for payment for the employer's share of the Disability, Old Age and Death Insurance. This implies that the 6 additional points subsidy alone leads to a reduction of 22 to 28 percent in the amount that needs to be contributed to the social security premium by the employer, and the combination of the 5 and 6 point subsidy together can increase this percentage to over 40 percent. To give an example, the 6 point-subsidy corresponded to 121.7 TL (around 19

[6]The two new additions were Hatay and Kirikkale. The two additional districts are Gokceada and Bozcaada, two islands in the province of Canakkale.

USD) on top of the 101.4 TL (around 16 USD) for the 5 point-subsidy for the minimum wage in 2018, which was 2,029 TL (around 314 USD) gross.

To be eligible for the 5 point-subsidy (Law No. 5510), firms must (i) have no arrears on premiums or administrative fines, (ii) pay their insurance premium on time, (iii) not have any informal workers or fictitious workers, which is verified through regular checks. In effect, the 5 points subsidy is an incentive for firms all around Turkey to abide by the formal labor market regulations, and over half of the firms operating in Turkey benefit from this subsidy. Eligibility for the 6 additional points subsidy (Law no. 6486) requires that in addition to benefiting from the 5 points subsidy, (i) firms should operate in the eligible provinces, and (ii) if the firm is one of the different branches under the same establishment, all branches must have no arrears on premiums or administrative fines. In the first few years of implementation, the subsidy included a third eligibility condition such that only firms with at least 10 employees were eligible, but it was removed starting from March 2016. As a result, small firms that were not eligible for the additional 6-point subsidy prior to 2016 became eligible to the subsidy.

Condition (ii) above for the 6 points subsidy implies a stricter condition on eligibility compared to the 5 points subsidy: While the 5 points subsidy requires not having any outstanding debt on premiums or administrative fines for the firm itself[7], a firm that satisfies this condition in an eligible province cannot benefit from the 6 additional points subsidy if any other firm or branch under the same establishment with this firm has any outstanding debts or administrative fines. Importantly, the amount implied by the 5 points reduction in the disability, old age, and death insurance is calculated from the actual wage reported to the Social Security Institution, whereas the amount of reduction through the 6 additional points is calculated from the minimum wage irrespective of the actual wage of the employee.

Until August 2020, the subsidy system in Turkey was defined at the province level. The provinces before then were classified under Regions 1 to 6 depending on their development levels (with Region 1 constituting the wealthiest provinces and 6 constituting the poorest provinces). In August 2020, the subsidy regime was changed by a Presidential Decree and eligibility was redefined at the county level and not at province level. This meant that should the socio-economic development index of a county remained below the national average, that county became eligible for subsidy even if the province it administratively belongs to remains above the national average. Due to this legislation change, 99 counties which were previously classified under Regions 1 to 3 were reclassified under Region 4 and became eligible to benefit from investment subsidies after year 2020. This means that although these counties had the same development levels as Region 4 counties between 2012 and 2018, they were not eligible as they were administratively a part of wealthier

[7] For the 5 point subsidy, eligibility criteria was changed in end-2021 to not having any outstanding debts in Turkey.

provinces. In our final set of difference in difference estimations, we use these counties as credible control units. Table 7 provides a list of these counties.

4 Data

4.1 Description of the administrative data set and data construction

The Social Security Institution (SSI) hosts all data related to formal employment in Turkey. To formally register workers under the SSI, each firm must first register as a formal employer under SSI, which then uses the employer identification number to track employers. SSI also assigns separate identification numbers for each worker registered under the employer, who can be linked to their employers by matching the employer and employee identification numbers.

SSI collects detailed information about formal employers and employees in various databases and we rely on two different anonymized databases provided by SSI in this study. The first one is the registry database that includes all relevant information on employers, including date of establishment, location, sector as well as other details. The declaration database is the main database that covers information on all registered workers on a monthly basis, and includes details such as occupation, location, gross wage and days worked in that month, and any subsidy that the individual benefits from.

SSI provided a random sample of the full database for the current analysis, which includes a 30 percent sample of the universe of all employers that ever benefited from the 5-point subsidy, irrespective of the duration they benefited from it. Once these establishments were randomly selected, the SSI staff then tracked all relevant variables for all time periods available in the database for that employer. In practice, though, information prior to 2004 was scattered and unreliable, and we decided to keep observations starting from 2004 only.

A source of duplication in the raw dataset can come from edits in declarations that the employers submit after the end of the month, such as editing the days worked or gross wages. We investigated and prepared the data meticulously together with the technical team of SSI in order to help them prepare and share the anonymized dataset with us for the purpose of this analysis. Finally, we reorganized the dataset we received from SSI to generate a dataset that can be readily analyzed.

While the dataset we received includes the universe of firms that ever benefited from the 6 points subsidy, for this analysis, we focus on firms that have started benefiting from the subsidy only after 2016, when firms with fewer than 10 employees also became eligible for the subsidy. More precisely, we exclude from our dataset all firms that were eligible in 2013 when the subsidy first

became available, but only for larger firms. The final dataset used in this analysis is a monthly panel of firms from January 2004 to September 2018, including firm demographics, total employment, and subsidy status, as detailed below.

4.2 Descriptive Statistics

The final dataset includes the following variables:

- **Employment:** This variable includes the total number of employees under the same firm during that month.
- **Firm Age:** We calculate firm age as the difference between the firm closure date and establishment date. If the firm is not closed, we calculate the age as the duration of months between September 2018 and the establishment date.
- **Treatment:** This variable is defined as firms that ever benefited from the 6 additional points subsidy, irrespective of the duration of benefiting.
- **After2016:** This is a dummy variable taking on the value 1 from March 2016 onward when the minimum employee condition for benefiting from the subsidy is removed.
- **Region4, Region5, and Region6:** Dummy variables taking on the value 1 if the firm operates in Region 4, 5, or 6, respectively.
- **Nuts2:** This variable defines which of the 26 NUTS-2 regions that the firm operates in.
- **Province:** This variable defines which of the 81 provinces that the firm operates in.
- **County:** This variable defines the county within each province that the firm operates in.
- **Sector:** This variable is defined in 4-digit NACE Rev.2 and later aggregated under 19 broad sectors.

Table provides summary statistics for these variables. The total number of observations in the dataset is over 37 million. Mean employment is close to 7, initial employment in firms is close to 5, firms age is around 6, and around 10 percent of observations belong to treatment firms. A total of around 15 percent of observations belong to firms in the regions eligible for the subsidy. Looking at the sector breakdown, close to 30 percent of observations are in firms in retail, 31 percent in other services, 16 percent in manufacturing and 11 percent in construction.

5 Identification strategy

Our identification strategy exploits the policy change which took place in 2016, when firms with at most 10 employees in eligible provinces which were not eligible in 2013 became eligible to benefit

from the additional 6-point subsidy. Thus, the population of interest in this evaluation are small firms at the time the policy change was implemented. This policy change, combined with the fact that small firms in only specific provinces became eligible, provides the opportunity to estimate the causal effect of the subsidy on employment using a difference-in-difference setting. Our treatment group thus consists of firms in eligible provinces, while our control group consists of firms in non-eligible provinces. Note that, while we expect small firms in eligible provinces to be more likely included in our treatment group due to the policy change in 2016, we do not formally impose any restrictions on firm size in generating the treatment group, implying that any large firms that, for any reason, started benefiting from the subsidy only after 2016 would still be included as a treatment firm in our dataset.

Since being eligible for the additional 6-point subsidy requires benefiting from the 5-point subsidy, we also restrict our set of control firms to firms that are receiving the 5-point subsidy. This restriction is motivated by the fact that the 5 points subsidy requires not having any outstanding debt on premiums with SGK or administrative fines. As a result, firms that do not benefit from the 5-point subsidy may systematically differ from those that do benefit from it for example because they have poor management or are in a difficult situation. They are therefore presumably not a valid control group for firms benefiting from the 6-point subsidy. The causal effect of the policy change is estimated as the difference in formal employment creation in eligible firms before and after 2016 relative to the change in employment creation in non-eligible firms receiving 5 points deduction before and after 2016. Given this restriction, the treatment effect we estimate is the marginal effect of the additional 6-point subsidy on employment, on top of the already received 5-point subsidy. Formally, to assess the causal effects of the policy change on formal employment, we estimate the following equation:

$$Ln(Y_{i,m,t}) = \beta_0 + \beta_1 Treated_{i,m,t} * After_t^{2016} + \phi_i + \tau_m + \tau_t + \epsilon_{i,m,t} \quad (1)$$

Where i stands for firm, m stands for month and t stands for year. The dependent variable is the growth of total firm employment. To account for firm-specific factors which do not vary over time, the specification includes firm fixed effects, denoted by ϕ_i. We also include dummies to control for month and year effects, denoted by τ_m and τ_t respectively. In addition, we also include province-specific time trends and region-specific year effects to relax the common trends assumption. We carry out our difference-in-difference estimation for two periods of analysis: 2008-2018 and 2012-2018, although our preferred estimation is for 2012-2018 due to some irregularities in the employment time series prior to 2012.

A common problem in the evaluation of employment subsidies is that the assignment is not random. In our case, the geographical eligibility criteria of the subsidy provide an advantage for impact

evaluation but at the same time poses challenges of identification because although the geography is exogenous, these regions are still not randomly selected. In fact, they are chosen precisely because they have lower level of socio-economic development compared to the rest of Turkey.[8] As there may be systematic differences between firms operating at eligible and non-eligible provinces in ways that may also affect trends in employment growth, we employ a series of identification strategies. While we estimate Average Treatment on the Treated (ATT) and Intention to Treat (ITT) effects[9], our most conservative strategy involves restricting our set of control firms to firms in counties which are contiguous to counties in treated counties and to firms in counties which became eligible only after 2020.

6 Results

6.1 Average Treatment Effect on the Treated (ATT)

We start by presenting descriptive evidence on trends in employment over time in control and treatment firms before and after the policy change in 2016. As shown in Figure 4, there is a noticeable break in employment trends in treated firms after 2016, when formal employment starts growing at a faster rate compared to periods prior to the policy change. This break in trends is robust to using either total aggregate employment or mean firm employment in control or treatment groups. In contrast, one does not observe a noticeable break in employment trends in control firms after 2016. In addition, employment trends for the control and treatment group prior to 2016 do not appear to noticeably differ for the mean employment measure, which provides some visual reassurance on the validity of our identification strategy.

We first estimate Average Treatment Effect on the Treated (ATT) effects by estimating the effect of the subsidy on firms that received the treatment. First, we use as our control group all firms that received the 5-point subsidy but are in regions that are not eligible to the additional 6-point subsidy. As displayed in Table 3, the estimated ATT of the subsidy on employment growth in firms that received the subsidy is positive and statistically significant at the 5 percent level. Receiving the additional 6-point subsidy is estimated to increase formal employment between 6 percent and 8 percent, depending on the reference period chosen for the Difference-in-Difference estimation.

In Table A1, we also estimate Intention-to-Treat Effects, which measure the impact of the policy change on all firms in eligible provinces, irrespective of whether they actually received the treatment – the additional 6-point subsidy. This parameter estimate is also of relevance for policy makers, as it measures the overall effect on employment creation in firms that were targeted by

[8] According to the classification of the government, there are three subsidy regions, namely region 4, region 5 and region 6. The classification of each province is based on a Socio-Economic Development Index which takes into account a large number social and economic indicators. This index divides provinces into 6 groups: 1= high development, 2= middle high, 3=middle but above national average, 4=middle but below national average, 5= close to low, and, 6=low development.

[9] ITT estimates are provided in Table A.1.

the intervention, even if they did not register for it. As for the ATT, the estimated effects of the subsidy on employment are positive and statistically significant at the 1 percent level. Since not all small firms in eligible provinces actually benefited from the subsidy, the estimated ITT is lower than the ATT and ranges between 3 percent and 5 percent depending on the estimation period used for the estimation. This indicates that even the overall effect of the policy change on employment growth in eligible provinces has been positive overall, even if some of the eligible firms did not benefit from the subsidy.

Our previous estimations used all firms in regions not eligible for the additional 6 points subsidy but received the five point subsidy as control firms. One may argue, however, that employment dynamics in firms in non-eligible regions may systematically differ from those in eligible provinces, although we do not find much support for this claim (Figure 4). To alleviate concern that our results may be driven by heterogeneity in firm dynamics in different regions of Turkey, we first restrict our control group to firms located in contiguous provinces to firms eligible for the additional 6-point subsidy.[10] As displayed in Table 4, ATT estimates with this restricted set of control firms are very similar to those using the full set of controls firms. More specifically, we keep only the control provinces that are adjacent to our treatment regions. Among the treated regions, only the region 4 and very few region 5 provinces have neighboring control provinces. Region 6 is completely cut off by region 4 and 5 provinces. So we keep control provinces that are neighbors with region 4 provinces. Those remaining control provinces are Bilecik, Eskisehir, Balikesir, Manisa, Burdur, Isparta, Karabük, Samsun, Sakarya, Ankara, Adana and Kayseri.

As further robustness checks, we drop Ankara, Adana and Kayseri (AAK) because those are the main industrial cities of Cental Anatolia and they are considerably wealthier cities. In Table A2, we display the ATT estimations which range between 2.7 and 5 percentage points across the three subsidy regions. Table A3 provides the ITT estimations which are between 1.4 and 2.8 percentage points. The coefficients are highly significant under both the ATT and ITT.

Table A4 shows the estimated employment effects separately by sector of activity. Estimates are positive in all sectors and statistically significant in the vast majority of them, but the magnitude of the effects varies across sectors. Among the largest sectors of activity, estimated effects are particularly large in construction and manufacturing. In contrast, estimated effects are smaller in high-skilled services such as finance and real estate, where the rates of formal employment have been higher to start with.

Contiguous counties

[10] Turkey consists of a total of 81 provinces.

Even after narrowing our control group to firms in non-eligible contiguous provinces, one may argue that there may still be systematic differences with firms in provinces in the treatment group, in ways that may differentially affect employment growth in those firms. To further alleviate concerns, we restrict the control group to firms in counties contiguous to eligible provinces – where a county is a smaller geographical unit than the province.[11] Table 5 reports the list of contiguous counties used as the control group for eligible provinces. In this specification, we exclude region 6 completely and only compare treatment firms in regions 4 and 5 with control firms that are in contiguous counties. So geographically, the distance between our control and treatment firms is almost never more than about 100 km and in the majority of cases, the distance is less than 50 km. Furthermore, we exclude the city centers (capital city) from both treatment and control provinces and we only compare firms in contiguous county municipalities in each group. It is also important to highlight that we do not pool the counties together, but instead run estimations for one treatment county at a time. Due to the very large sample size, we still have hundreds of thousands observations for estimating the impact on firms in each treatment county.

The results are displayed in Table 6 and show that our findings are robust to narrowing down the geographical coverage of our control group of firms. The estimated effect of the subsidy is positive and significant at the 1 percent percent level for most counties of the treated provinces. In addition, the magnitude of the difference-in-difference coefficient is very similar to that of our baseline specification in all three regions. This provides further comfort that our baseline estimates are not driven by potential confounding effects of systematic differences between firms in eligible and non-eligible provinces that may affect employment trends in the two groups even in the absence of the subsidy.

One question of interest beyond these overall positive employment effects is how they vary over time after the policy change, and in particular how persistent they are. In Tables A5, A6, and A7, we further investigate how persistent these positive effects on formal job creation are over time by interacting the difference in difference indicator variable with year dummies for each year following the policy change. The magnitude of the formal job creation treatment effect increases over time in the three years that followed the subsidy introduction. In addition, the magnitude of the additional job creation for each year is fairly constant over time, indicating that treatment effects are sustained several years after the subsidy introduction. These patterns of the results are robust to using either province-level controls (Tables A5 and A6,) or county-level control (Table A7).

[11] Turkey has a total of 973 counties (Turkish Statistics Institute, 2021).

6.2 Results based on comparison with control counties which became eligible after 2020

Next, we exploit the legislation change in 2020 which provides us with a set of even more credible set of control units. Our identification strategy relies upon using the firms in counties that were not eligible for the subsidy in our sample period between 2012 and 2018 but were re-classified under region 4 after the sample period as the only control firms for our treatment firms. The list of these counties are provided in Table A7. As in our estimations in the previous sub-section, we exclude firms in central cities in treatment provinces. The control firms by definition are located in counties outside administrative center of the provinces they belong to.

Panel A of Figure 5 shows the mean employment in the treatment firms in comparison to control counties that were included under region 4 after 2020. As before, the trends are very similar before 2016 but the mean employment starts increasing relative to the firms in control counties. In Panel B of Figure 5 we take out the control firms which had larger than 10 employees before year 2013 to be able compare whether the trends in small firms in both groups were comparable before the treatment, -though we still keep the large control firms in our estimations below. Panel B shows that employment trends were very similar in small treatment and control firms two years leading to 2016 while the employment in treated firms increased significantly in comparison to the control firms after 2016.

In Table 8 we provide the our estimations using the same set of treatment and control firms as in Panel A of Figure 5. The first two columns uses only the treatment firms in Region 4, and columns (3) and (4) uses all firms in Regions 4, 5 and 6. The results are quite robust and indicate an effect size of 8.7 to 9 percent increase in formal employment growth in treatment firms in comparison to control firms that were not included in Region 4 until August 2020. In columns (5) to (8) we check whether the effects are heterogeneous over time. The estimations show that indeed the effects are increasing in the three years that followed the subsidy introduction and the magnitude of the additional job creation for each year is fairly constant over time, as in the previous section.

Recent literature on the difference in differences methods has shown that OLS with time and panel unit fixed effects, also known as the two-way fixed effects (TWFEDD), can yield severely biased estimates when the treatment is adopted by different units at different times (De Chaisemartin and d'Haultfoeuille (2020), Baker et al. (2021), Callaway and Sant'Anna (2020), Borusyak et al. (2021), Sun and Abraham (2020), Goodman-Bacon (2021)). In the canonical DID setup where there are two groups and two time periods, the identification is based on common parallel trends in a potential outcomes framework. Recent literature on the other hand has shown that the TWFEDD is a weighted average of all possible 2x2 DID estimators that compare timing groups (Goodman-Bacon (2021), Borusyak et al. (2021), De Chaisemartin and d'Haultfoeuille (2020), Sun and Abraham

(2020)). In this setting, timing groups consist of i) treated units versus never treated units, ii) treated units versus not yet treated units, and iii) later treated units versus earlier treated units. Goodman-Bacon (2021) shows that the causal estimand that TWFEDD can identify is a variance weighted average treatment effect on the treated, under the assumption that a variance weighted average of the untreated potential outcome changes are zero and average treatment effects for each timing group do not change over time. In the case of time varying average treatment effects, the comparison between the later treated units versus the earlier units may lead to negative weights and cause severe bias in the estimates (Goodman-Bacon (2021), Wooldridge (2005), Borusyak et al. (2021), De Chaisemartin and d'Haultfoeuille (2020)).[12]

Some of the new literature attempt to solve these problems by offering inference procedures that avoid bad comparisons across units. Callaway and Sant'Anna (2020) proposes group time average treatment effects, $ATT(g,t)$, that are average treatment effects in period t for the group of units treated in period g. The assumptions required for identification are that i) the data is panel or repeated cross-sectional; ii) conditional on covariates, there are parallel pre-trends; iii) the treatment is irreversible; and iv) treatment and comparison groups have common support. Callaway and Sant'Anna (2020) propose three different types of DID estimands in staggered treatment adoption setups, namely; the outcome regression, inverse probability weighting or doubly-robust estimands. The estimation algorithm uses the never treated or not-yet treated units as the comparison group. The advantage of this method is that it is suitable for cases where the parallel trends assumption holds only after conditioning on covariates, and, also for the cases when units can anticipate participating in the treatment and adjust their behavior before the treatment.

In our sample period, there was only one timing associated with the legislation change that defined the eligibility for small firms. However, our case constitutes an example of a staggered design where different firms benefit from the subsidy at different times simply because they did not meet the criteria or they were established at a later date. As Figure 4 shows and as we previously discussed, the parallel pre-trends assumption holds in our case. The legislation was also irreversible in our sample period.

In order to check the robustness of our results associated with potential negative weights and bias in staggered designs, we employ the inference procedure proposed by Callaway and Sant'Anna (2020). We use the inverse probability weighting and never treated units as controls in our robustness check. As before, we only include treatment firms in Region 4 counties and control firms in counties that became eligible after 2020. This gives us a data set of 4.1 million observations and 276.9 thousand treatment and control firms all together. The event study estimations based on Callaway and Sant'Anna (2020) are displayed in Figure 6. As the figure clearly shows, there is no

[12] Goodman-Bacon (2021) provides a decomposition algorithm which shows the DID estimates across the timing groups as well as their associated weights.

difference in employment growth across the treated and control firms before the legislation change. The treatment firms start growing significantly higher in comparison to the control firms and the effect size is increasing over time, which is consistent with earlier results. The coefficient sizes are also at par with our two-way fixed effects specifications, which gives us further confidence on the validity of the earlier difference in difference results.

Comparison of results with earlier literature

Our estimates suggest that the additional 6-point subsidy increases formal employment in benefiting firms from 6 percent to 9 percent depending on the specification used. These results are robust to the inclusion of control groups in narrower geographical locations, and ITT regressions provide further evidence that the overall effect of the policy change on employment growth in eligible provinces has been positive and sizeable in magnitude.

Overall, estimated positive effects on formal employment are on the high end among studies that have evaluated the employment effects of labor costs subsidies. In terms of labor demand elasticity, the effect size we find implies that covering an additional 1 percent of employer labor costs through workers' gross wages increases formal employment by about 0.8 percent. Our estimate is thus on the high end of the labor demand elasticity literature where estimates typically range from 0.3 to 0.5 (Hamermesh (1993)). In particular, the estimated treatment effects on registered employment are larger than those estimated in high-income country contexts where informal employment is rare, such as Finland (Huttunen et al. (2013)), France (Kramarz and Philippon (2001)) or Sweden and Norway (Pagés et al. (2017)) who find little or no effect of targeted wage subsidies on registered employment.

In the context of developing economies, Heckman and Pages (2003) estimate that a 10 percent tax cut leads to an increase in total employment (formal and informal) by 4.47 percent in Latin America and the Caribbean. Similarly, Lehmann and Muravyev (2012) show that a reduction in payroll taxes in Eastern European countries was associated with a decline in informality, measured by the ratio of production of goods and services that are not declared to public authorities to countries' official GDP. Kugler and Kugler (2009) evaluate the impact of payroll tax increases in Colombia in 1996. In particular, payroll taxes in Colombia increased from 35.5 percent in 1980 to 51.5 percent in 1996. Kugler and Kugler (2009) estimate that a 10 percent increase in payroll taxes reduced formal employment by between 4 percent and 5 percent. On the other hand, another study from Argentina which exploits the fact that payroll taxes were reduced at differential rates in 85 regions of Argentina in 1993 does not find impact on formal employment (Cruces et al. (2010)). But more importantly, the study shows that tax collection as a percentage of total wage income fell by almost half throughout the next decade as employment gains were null under this subsidy.

Regarding prior studies on employment subsidies in Turkey, the study by Betcherman et al. (2010) for the previous version of the subsidy targeted to only to larger firms in Turkey finds that employment effects range from 5 percent to 13 percent for large firms. However, the scheme also included an energy subsidy that the author cannot separate from the employment subsidy scheme. Our results thus indicate that employment subsidies are also effective in increasing formal employment among smaller firms, which are also presumably more prone to hire workers informally in the absence of employment subsidies compared to larger firms.

6.3 Distinguishing between job creation and formalization effects

A question of interest for policy purposes is whether the observed increase in formal employment originates from new job creation in treated firms or, instead, from the formalization of already existing jobs in the same firm. Our social security dataset, by design, does not capture workers who are employed informally, i.e. not registered with social security institutions. To provide some indicative evidence on whether the increase in employment was driven by formalization or new job creation, we use data from the nationally representative Household Labor Force Survey in Turkey, which asks each working individual in the household whether they are registered with social security institutions. To provide indicative evidence of whether the rise in formal employment was mostly driven by the formalization of existing informal jobs, we run similar regression as our firm-level estimation, but at the worker level. We run two separate regressions: one in which the dependent variable is a dummy variable for being wage employed, and a second specification where the dependent variable is a dummy variable for being a formal wage employee. Tables 9 and 10 show that while the likelihood of being a wage employee did not increase after the policy change, there was a statistically significant increase in the likelihood of being formally employed after the policy change. This indicates that the increase in formal employment originated mainly from the conversion of existing jobs into formal jobs, rather than from new job creation by firms.

In Figure 7, we follow Betcherman et al. (2010) by looking at changes in firm electricity consumption before and after the policy change in treated and control firms, as a proxy for economic activity. The rationale behind this corroborative analysis is that if firms actually hired new workers in treated provinces thanks to the subsidy, one should observe an increase in economic activity, proxied by electricity consumption, in those provinces after the policy change. In contrast, if the subsidy resulted in the pure conversion of existing informal workers into formal workers through social security registration, one should not observe any noticeable break in the trend in corporate electricity consumption post-2016 in treated provinces. Figure 5 does not show any break in trend in treated provinces after the subsidy implementation, in contrast with the control provinces where one observes an uptick in electricity consumption. This evidence is further indicative of the pos-

itive effects on formal employment being largely driven by the formalization of existing workers, rather than by new job creation.

7 Cost-benefit analysis

The final part of our study focuses on a basic cost-benefit analysis of the subsidy by monetizing the benefits of the subsidy, taking into account its potential to increase formal employment, and the cost of the subsidy for the government. Whereas such cost-benefit analyses are rarely conducted in ALMP evaluations (as asserted in Card et al. (2018)), they provide important information for policy makers. We use the data from September 2018, the latest available month in our dataset. The analysis is straightforward in the sense that it includes the government's direct (opportunity) costs – i.e. the monetary benefits in terms of social security contributions and tax payments it sacrifices to implement the subsidy – and its direct benefits, i.e. the additional social security contributions and tax payments it would receive due to the subsidy's effect in increasing formal employment.

The design does not take into account costs and benefits from the perspective of workers, for example, in the sense of an increase in income of informal workers due to formalization. Similarly, it does not account for any increase in tax revenues to the government caused by an increase in consumption as a result of income increase. Finally, costs that the government could bear, for example, to provide social assistance to individuals or households with no formal employment are not considered in this analysis.

The cost of the subsidy for the government is the amount of revenues sacrificed to cover part of the social security premiums for workers in eligible firms. As defined, the government would forgo 6 percent of the social security premium payments (based on the minimum wage) for eligible firms that the firm would otherwise have needed to pay to the government. The benefit of the subsidy for the government is the increase in social security premiums and tax payments due to the subsidy's effect on increasing workers that are registered to the Social Security Institution by the firms. The net benefit of the subsidy for the government at time t is thus calculated as:

$$\textit{Net Benefit}_t = [\text{New formal}_t * \text{Payment to government}_t] - [0.06 * \text{Minimum wage}_t * \text{Total covered}_t]$$

Where $\textit{New formal}_t$ is the additional formal employment caused by the subsidy taking into account the average treatment effect on the treated, $\textit{Payment to government}_t$ is the total payments by the firm to the government, $\textit{Minimum wage}_t$ is the minimum wage that the 6 percent subsidy would be calculated from, and $\textit{Total covered}_t$ is the formal worker population covered by the subsidy. We

need to make several assumptions on the values of these variables to complete this calculation, and we provide each of these assumptions in detail below.

Formal worker population covered by the subsidy. We use the total number of workers in September 2018 taken from the Social Security Institution and multiply it with the coverage rate of the subsidy to get the total number of workers in all treated firms. We assume that the coverage rate, i.e., the percentage of workers that are covered by the subsidy in the eligible regions, is roughly the same as the coverage rate in our dataset. 86 percent of workers in eligible regions were covered by the 6 points subsidy in our dataset, and we take this percentage throughout the cost-benefit analysis whenever needed. Note that this coverage is highly likely to be an overestimation, as our dataset includes the firms that benefit from the 5 points subsidy, which is already a subset of all firms in operation in the eligible regions. However, unfortunately, we do not have a good estimate of coverage for the 5 points subsidy in the regions eligible for the 6 points subsidy to further refine this assumption. We expect the overall bias in net benefit that this possible overestimation to cause to be limited as we use the same coverage assumption in both the cost and the benefit sides of the analysis. Furthermore, an overestimation of the coverage rate would cause an underestimation of the net benefit.

Additional formal employment. Since our effect size is calculated as the average effect on the treated, we first need to make a reasonable assumption on the number of workers in the treatment group, and estimate the additional formal employment generated by the subsidy using this total number. We use the formal worker population covered by the subsidy (explained above) as the total number of workers after the implementation of the subsidy in the treatment group, and calculate the total number of workers in the counterfactual (i.e., if the subsidy was not implemented) using the effect size. The total increase in formal employment as a result of the subsidy may then be calculated as follows:

$$\text{New formal}_t = \text{Total covered}_t - [\text{Total covered}_t / (1 + \text{effect size})]$$

Total payments by the firm to the government. Payments to the government include social security premium payments as well as income and stamp taxes. We use the payments that would be associated with the average worker wage for September 2018.

Minimum wage. Minimum wage is set by the government and is updated twice each year, one at the beginning of the year and a second, mid-year update. We take the minimum wage in September 2018 for the purposes of this analysis.

Table 11 provides the results of the cost-benefit analysis. Our results suggest that assuming an

effect size of 6 percent, it costs the government around 720 TL, or around 35 percent of the minimum wage in 2018, to generate each additional formal employment using the subsidy. Benefits equalize the cost of subsidy with an effect size of around 9.5 percent.

Another way to identify whether benefits equalize costs is to investigate the return of investment based on each worker. Assuming that the worker benefits from the subsidy for an average of 4 years, which is the maximum duration of subsidy offered for Region 4 provinces, constant real wages, and a 5 percent discount rate, the net present value of the costs associated with covering the worker's subsidy equalizes the net present value of premium and tax payments by the worker in less than a year after the end of the subsidy period.

As mentioned above, the analysis considers purely the forgone vs. gained revenues in terms of formal labor for the government. In other words, we do not include the benefits to the government in terms of increased tax revenues due to increased consumption, or the reduction in costs due to reduced social assistance payments as individuals with formal employment are not eligible for some of the social assistance schemes in Turkey. In this sense, the analysis underestimates the benefits of the subsidy.

8 Conclusion

This paper studied the impact of social security contribution subsidies on formal employment in small firms, in a context of a developing economy where informality is common. It showed that subsidizing social security contributions has positive and sizeable effects on formal employment in small firms which are more prone to hire workers informally. Corroborative evidence also indicates that these positive are mostly driven by the conversion of informal employment into formal employment, rather than new job creation. Our findings overall indicate that the high costs of formal employment, in the form of high social security contributions borne by employers, may be contributing to the persistence of informal employment in economies with low firm productivity and imperfect enforcement of labor regulations. In this context, reducing the cost of formal employment by subsidizing social security contributions can contribute to reducing informality, at least in the short run while the subsidies are being implemented.

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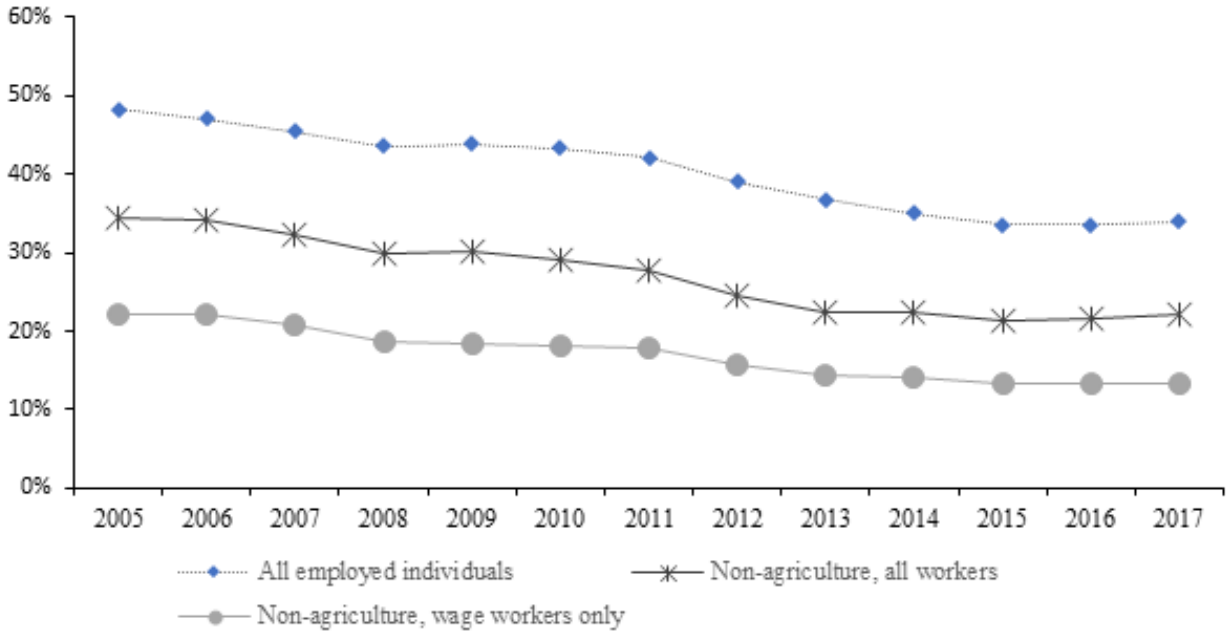
Figure 1: Share of workers not registered with the Social Security Institution, 2005 - 2017



Source: Authors' calculations based on Turkish Household Labor Force Survey (HLFS). Note. Employment is categorized as unregistered if the worker is not registered with the Turkish Social Security Authority at the time of the survey.

Figure 2: Timeline and implementation details of Incentive for Investment and Employment and the Additional 6 Point Subsidy

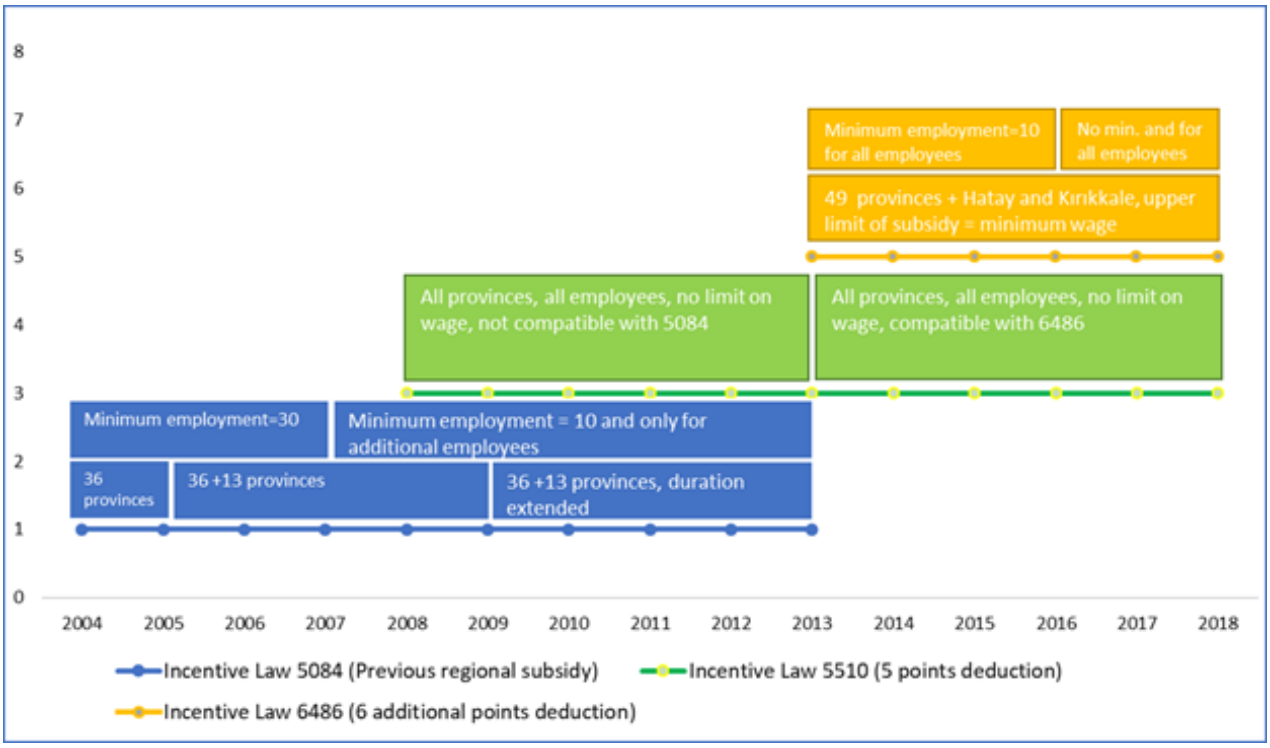
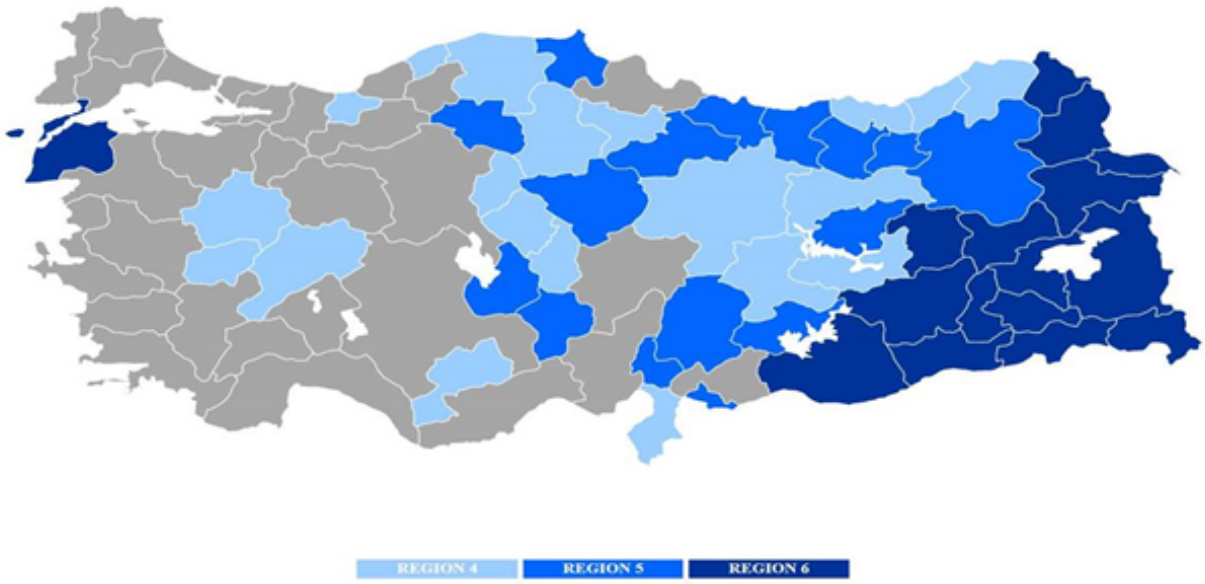


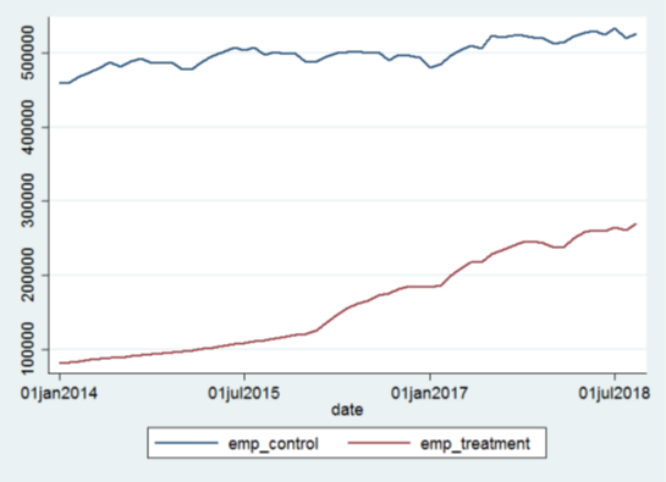
Figure 3: Provinces that are eligible for the 6 additional points reduction



Note: For the west, most province in Region 6, Canakkale, the only eligible districts are two islands, Gokceada and Bozcaada.

Figure 4: Formal wage employment over time in control and treatment firms

Panel A: Aggregate employment



Panel B: Mean employment in the firm

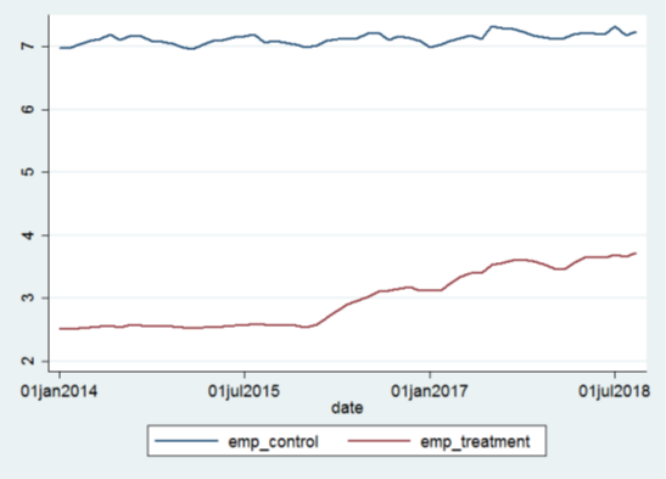
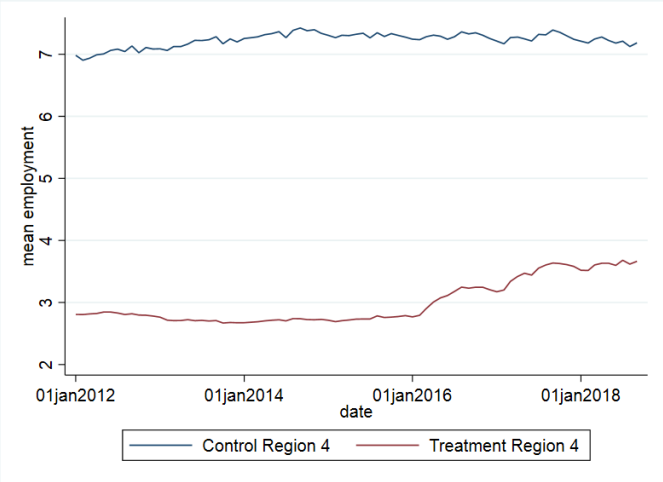


Figure 5: Formal wage employment of firms at region 4 control and treatment counties

Panel A: Mean employment



Panel B: Mean employment excluding control firms with size >10 before 2013

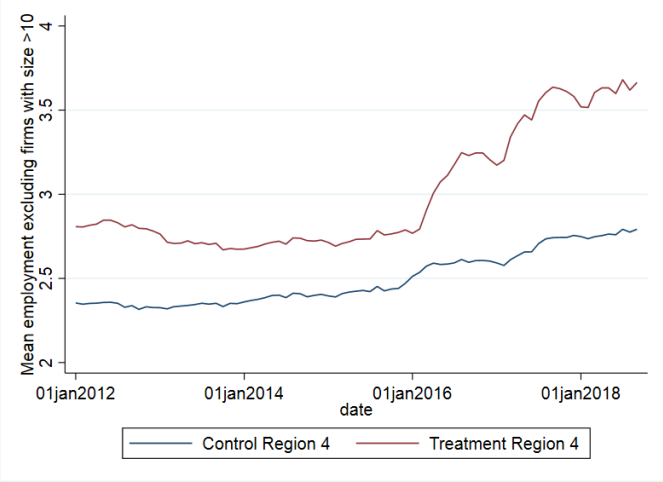
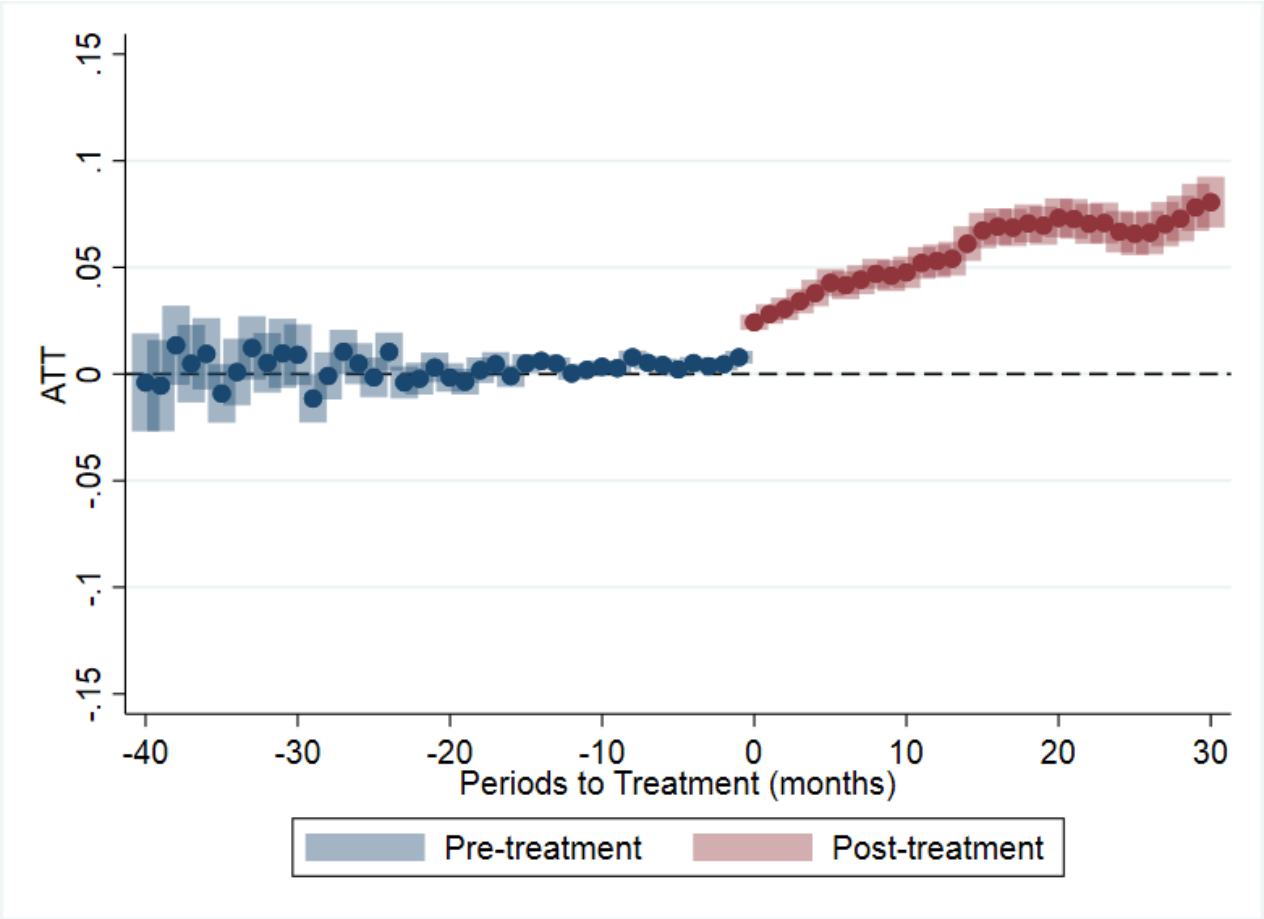


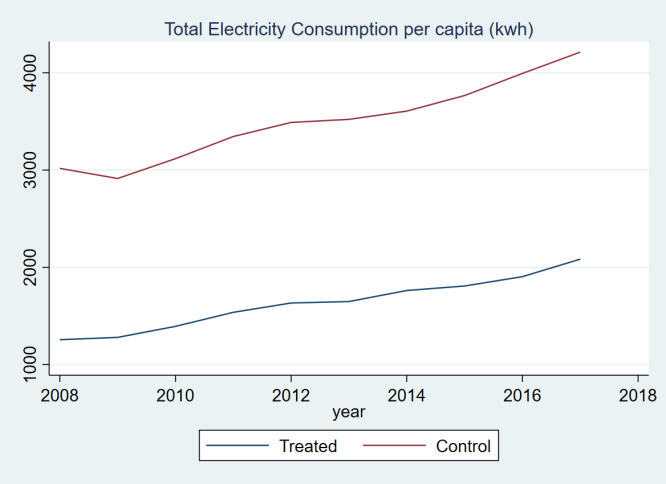
Figure 6: Event Study Estimations Based on Callaway and Sant'Anna (2020)



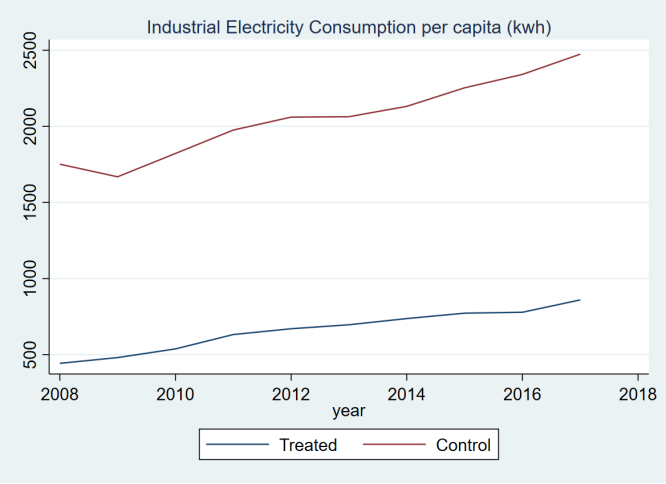
Note: The event study graph is estimated using the csdid command in STATA using the doubly robust stabilized inverse probability weighting.

Figure 7: Firm electricity consumption in eligible and non-eligible provinces

Panel A: Total electricity consumption per capita



Panel B: Total industrial consumption per capita



Source: Turkish Statistical Institute.

Table 1: Payments to government from gross wages in Turkey (%), 2020

Social Security Institution (SSI) contributions	Employee	Employer	Total
Disability, Old Age and Death Insurance	9%	11%	20%
General health insurance	5%	7.5%	12.5%
Short-term insurance branches	-	1-6.5%	1-6.5%
Unemployment insurance	1%	2%	3%
TOTAL	15%	21.5-27%	36.5-42%
Income tax	Calculated based on 0.85* gross salary (after employee's share for SSI is deducted from gross salary). Percentage changes according to wage.		
Stamp tax	0.759% on gross salary.		

Note: Short-term insurance reflected the ranges for the firms according to the hazard levels of the tasks that was valid until September 1st, 2013. The short-term insurance was fixed at 2% after that date.

Table 2: Summary statistics

	Region 4		Region 5		Region 6		Other regions		All provinces	
	Mean	Sd	Mean	Sd	Mean	Sd	Mean	Sd	Mean	Sd
Employment	3.29	7.66	3.24	9.7	4.28	17.13	7.32	39.18	6.74	36.36
Firm age	6.05	8	5.62	7.52	5.1	6.79	6.47	8.03	6.36	7.98
Treatment	0.69	0.46	0.71	0.45	0.56	0.5	0	0	0.1	0.3
After2016	0.44	0.5	0.45	0.5	0.47	0.5	0.42	0.49	0.43	0.49
Initial employment	2.41	4.95	2.41	6.41	3.01	10.43	5.1	28.88	4.71	26.75
Agriculture	0.01	0.11	0.01	0.11	0.01	0.11	0.01	0.09	0.01	0.1
Construction	0.14	0.35	0.15	0.35	0.12	0.33	0.11	0.31	0.11	0.32
Education	0.01	0.09	0.01	0.09	0.01	0.11	0.01	0.11	0.01	0.11
Financial	0.02	0.12	0.02	0.12	0.02	0.13	0.01	0.12	0.01	0.12
Food and accommodation	0.07	0.26	0.07	0.25	0.07	0.25	0.07	0.25	0.07	0.26
Health	0.01	0.11	0.01	0.11	0.02	0.13	0.01	0.12	0.01	0.12
Manufacturing	0.13	0.33	0.12	0.33	0.1	0.3	0.16	0.37	0.16	0.36
Retail	0.3	0.46	0.3	0.46	0.29	0.45	0.28	0.45	0.29	0.45
Other industry	0.02	0.14	0.03	0.17	0.04	0.19	0.01	0.11	0.01	0.12
Other services	0.28	0.45	0.28	0.45	0.33	0.47	0.31	0.46	0.31	0.46
N	2,830,405		1,620,907		1,185,156		31,521,060		37,157,528	

Table 3: Difference-in-difference estimates, Average Total Treatment Effects (ATT) 2012-2018

	All firms	All firms excluding construction	All firms excluding agriculture	All firms	All firms excluding construction	All firms excluding agriculture
	Fixed Effects			Random Effects		
	(1)	(2)	(3)	(4)	(5)	(6)
DiD Coefficient	0.0767***	0.0705***	0.0767***	0.0760***	0.0698***	0.0760***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Initial Employment				0.0165***	0.0146***	0.0164***
				(0.002)	(0.002)	(0.002)
Year effects	Yes	Yes	Yes	Yes	Yes	Yes
Month effects	Yes	Yes	Yes	Yes	Yes	Yes
Sector effects				Yes	Yes	Yes
Observations	37,111,501	32,861,813	36,754,509	37,111,501	32,861,813	36,754,509

Robust and clustered standard errors in parentheses.*** $p < 0.01$, ** $p < 0.05$, *$p < 0.1$

Table 4: Difference-in-difference estimates, Restricted Control Provinces Average Total Treatment Effects (ATT) 2012-2018

	All Regions	Region 4	Region 5	Region 6
	(1)	(2)	(3)	(4)
DiD Coefficient	0.036***	0.031***	0.044***	0.050***
	(0.003)	(0.003)	(0.004)	(0.004)
Observations	11,539,996	9,722,546	8,920,639	8,420,727
R-squared	0.004	0.002	0.002	0.002
Number of firms	384471	325304	302047	287936
Firm Fixed Effects	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes
Month Effects	Yes	Yes	Yes	Yes
Province Specific Linear Trends	Yes	Yes	Yes	Yes
Nuts2-Region-Year Fixed Effects	Yes	Yes	Yes	Yes

Robust and clustered standard errors in parentheses.*** $p < 0.01$, ** $p < 0.05$, *$p < 0.1$

Note: Control provinces consist of Bilecik, Eskisehir, Balikesir, Manisa, Burdur, Isparta, Karabük, Samsun, Sakarya, Ankara, Adana and Kayseri

Table 5: List of control counties contiguous to provinces eligible for the additional 6 point subsidy

Treatment Province	County	Control Province	County
Usak	Sivasli, Karahallı, Ulubey, Esme	Denizli	Civril, Bekilli, Cal, Güney
		Manisa	Alaşehir, Sarıgöl, Kula, Selendi
Kütahya	Pazarlar, Simav, Tavşanlı, Domaniç, Saphane	Manisa	Selendi, Demirci, Harmancik,
		Bursa	Keles, İnegöl
		Bilecik	Bozuyuk
Afyon	İhsaniye, Işçehisar, Bayat, Emirdağ, Çay,	Eskisehir	Seyitgazi, Han, Çifteler, Sivrihisar
	Sultandağ, Suhut, Dinar, Basmakçı, Evciler	Konya	Celtik, Yunak, Akşehir, Tuzlukçu
		Isparta	Yalvac, Senirkent, Uluborlu, Keçiborlu, Yeşilova
		Denizli	Bozkurt, Çardak, Baklan, Civril
Bartın-Kastamonu	Ulus, Pınarbaşı, Daday, Arac	Karabük	Yenice, Ovacik, Safranbolu, Eflani
Düzce	Akcakoca, Cumayeri, Gümüşova, Gölyaka,	Zonguldak	Alapli
	Kaynaşlı, Yığılca	Sakarya	Kocaali, Hendek
		Bolu	Mudurnu, Mengen
Kırşehir-Kırıkkale	Yahsihan, Bahsilli, Karakeçili, Çelebi, Sulakyurt	Ankara	Kalecik, Elmadağ, Bala, Evren, Şereflikoçhisar

Table 6: Average Treatment Effects (ATT), control group restricted to firms in contiguous counties, 2012-2018

	Kutahya	Kutahya	Usak	Usak	Afyon	Afyon	Duzce	Duzce	Kirikkale	Kirikkale	Bartin Kastamonu	Bartin Kastamonu
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
DiD Coefficient	0.039	0.075	0.048*	0.075**	0.074***	0.091***	0.033	0.047	0.089***	0.127***	0.094	0.136**
	(0.048)	(0.078)	(0.027)	(0.036)	(0.018)	(0.023)	(0.049)	(0.063)	(0.023)	(0.03)	(0.065)	(0.068)
Observations	232588	232588	163518	163518	287810	287810	141161	141161	190025	190025	72138	72138
R-squared	0.001	0.001	0.001	0.002	0.005	0.006	0.001	0.002	0.001	0.002	0.002	0.003
Number of firms	8920	8920	7263	7263	12458	12458	4835	4835	7368	7368	2785	2785
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province Linear Trends	Yes		Yes		Yes		Yes		Yes		Yes	
Clustered SE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province Year Fixed Effects		Yes		Yes		Yes		Yes		Yes		Yes

Robust and clustered standard errors in parentheses.*** $p < 0.01$, ** $p < 0.05$, *$p < 0.1$

Table 7: Control countries which were included under Region 4 after 2020.

Province	Region	County	Subsidy region after 2020
Adana	3	Yumurtalık, İmamoğlu, Karataş, Karaisali	4
Bilecik	3	Gölpazarı, İnhisar, Yenipazar	4
Ankara	1	Haymana, Bala, Çamlıdere	4
Aydın	2	Kuyucak, Germencil, Karacasu, Sultanhisar, Köşk, Buharkent, Yenipazar, İncirliova, Bozdoğan	4
Balıkesir	2	Savaştepe, Dursunbey, Sındırgıı Havran, Kepsut	4
Bolu	2	Yeniçağa, Mudurnu, Göynük	4
Burdur	3	Kemer, Ağlasun, Cavdır, Çeltikçi, Yeşilova, Altınyayla	4
Bursa	1	Haymancık, Keleş	4
Çanakkale	2	Bayramiç, Yenice	4
Gaziantep	3	Nizip, Islahiye, Oğuzeli, Nurdağı, Karkamış, Araban, Yavuzeli	4
Isparta	2	Şarkıkaraağaç, Aksu	4
Karabük	2	Eskipazar, Yenice	4
Kayseri	2	İncesu, Felahiye, Yahyalı, Bünyan, Yeşilhisar, Pınarbaşı	4
Konya	3	Kulu, Sarayönü, Hadim, Taşkent, Güneysınır, Kadınhanı, Doğanhisar, Tuzlukçu, Yalıhüyük, Bozkır, Derebucak	4
Denizli	2	Babadağ, Kale, Beyağaç, Baklan, Güney, Çameli	4
Edirne	2	Enez, İpsala, Meriç	4
Eskişehir	1	Alpu, Günyüzü	4
Kırklareli	2	Demirköy, Pehlivanköy	4
Manisa	2	Saruhanlı, Köprübaşı, Ahmetli, Gölmarmara, Selendi	4
Sakarya	2	Kaynarca, Ferizli	4
Mersin	3	Aydıncık, Mut, Gülnar	4
Samsun	3	Kavak, Havza, Alacam, Yakakent, Salıpazarı, Vezirköprü, Asarcık, Ayvacık	4
Zonguldak	3	Kilimli, Gökçebey	4

Table 8: Average Treatment Effects Relative to Region 4 Control Countries which Became Eligible After 2020.

	Region 4 (1)	Region 4 (2)	Regions 4, 5, 6 (3)	Regions 4, 5, 6 (4)	Region 4 (5)	Region 4 (6)	Regions 4, 5, 6 (7)	Regions 4, 5, 6 (8)
Did coefficient	0.087***	0.090***	0.094***	0.094***				
	(0.004)	(0.004)	(0.003)	(0.003)				
Treatment*Year 2016					0.090***	0.098***	0.094***	0.102***
					(0.004)	(0.006)	(0.004)	(0.005)
Treatment*Year 2017					0.138***	0.158***	0.157***	0.169***
					(0.006)	(0.008)	(0.005)	(0.007)
Treatment*Year 2018					0.167***	0.196***	0.187***	0.199***
					(0.007)	(0.010)	(0.006)	(0.008)
Observations	4,138,697	4,138,697	6,944,622	6,944,622	4,138,697	4,138,697	6,944,622	6,944,622
R-squared	0.006	0.007	0.007	0.008	0.007	0.007	0.008	0.009
Number of firms	161,129	161,129	276,814	276,814	161,129	161,129	276,814	276,814
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province Linear Trends	Yes		Yes		Yes		Yes	
Clustered SE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province Year Fixed Effects		Yes		Yes		Yes		Yes

Robust and clustered standard errors in parentheses.*** $p < 0.01$, ** $p < 0.05$, *$p < 0.1$

Table 9: Wage Employment Probability in Labor Force, 2009-2018

	All	Firmsize=< 10	Firmsize> 10	Firmsize=< 10 Agriculture	Firmsize=< 10 Construction	Firmsize> 10 Agriculture	Firmsize> 10 Construction
Treated Provinces	-0.051***	-0.025**	-0.013**	-0.036*	0.072***	-0.047*	-0.012
	(0.011)	(0.011)	(0.005)	(0.02)	(0.026)	(0.027)	(0.012)
Treated after 2013	0.003	-0.003	0.002	0.004	0	-0.005	0.003
	(0.006)	(0.006)	(0.003)	(0.009)	(0.017)	(0.024)	(0.008)
Treated after 2016	0.005	0.007	0.008***	-0.006	0.009	0.013	-0.001
	(0.006)	(0.008)	(0.002)	(0.009)	(0.02)	(0.027)	(0.009)
Observations	931,176	628,714	302,462	263,914	50,121	8,171	25,874
Full individual controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Robust and clustered standard errors in parentheses.*** $p < 0.01$, ** $p < 0.05$, *$p < 0.1$

Table 10: Formal Wage Employment Probability in Labor Force, 2009-2018

	All	Firmsize=< 10	Firmsize> 10	Firmsize=< 10 Agriculture	Firmsize=< 10 Construction	Firmsize> 10 Agriculture	Firmsize> 10 Construction
Treated Provinces	-0.075***	-0.040**	-0.064***	-0.006**	-0.05	-0.031	-0.110***
	(0.02)	(0.017)	(0.017)	(0.002)	(0.038)	(0.024)	(0.04)
Treated after 2013	-0.007*	-0.021***	0.015*	-0.001	-0.012	-0.015	0.037
	(0.004)	(0.005)	(0.009)	(0.002)	(0.017)	(0.033)	(0.031)
Treated after 2016	0.019**	0.015*	0.027***	0	0.047***	-0.025	0.062**
	(0.009)	(0.009)	(0.009)	(0.001)	(0.017)	(0.034)	(0.025)
Observations	931,176	628,714	302,462	263,914	50,121	8,171	25,874
Full individual controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Robust and clustered standard errors in parentheses.*** $p < 0.01$, ** $p < 0.05$, *$p < 0.1$

Table 11: Cost-benefit analysis results (monthly values)

	Total cost to government	Total benefit to government	Net Benefit	Net Benefit per additional formal worker generated by the subsidy
In TL	309,522,860	206,134,194	-103,388,667	-718.58
In USD	58,400,540	38,893,244	-19,507,296	-136

Results provided are based on data from September 2018. Exchange rate for December 2018 is used to convert values from TL to USD

Appendix A

Table A1: Intention to Treat Effects (ITT) 2012-2018

	All Regions	Region 4	Region 5	Region 6	All Regions	Region 4	Region 5	Region 6
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(9)
DiD Coefficient	0.024***	0.025***	0.047***	0.016***				
	(0.002)	(0.003)	(0.004)	(0.004)				
impact2016					0.031***	0.036***	0.059***	-0.006
					(0.004)	(0.005)	(0.010)	(0.009)
impact2017					0.061***	0.072***	0.115***	-0.012
					(0.006)	(0.008)	(0.015)	(0.014)
impact2018					0.079***	0.095***	0.137***	-0.015
					(0.008)	(0.01)	(0.02)	(0.018)
Observations	13,398,566	10,592,641	9,382,615	8,947,226	13,398,566	10,592,641	9,382,615	8,947,226
R-squared	0.003	0.002	0.002	0.002	0.003	0.002	0.002	0.002
Number of Firms	495,310	379,625	332,962	313,539	495,310	379,625	332,962	313539
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province Specific Linear Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Robust and clustered standard errors in parentheses.*** $p < 0.01$, ** $p < 0.05$, *$p < 0.1$

Table A2: Difference-in-difference estimates, Restricted Control Provinces and Further Excluding Ankara, Adana and Kayseri, Average Total Treatment Effects (ATT) 2012-2018

	All Regions	Region 4	Region 5	Region 6
	(1)	(2)	(3)	(4)
DiD coefficient	0.033***	0.027***	0.042***	0.050***
	(0.003)	(0.004)	(0.004)	(0.005)
Observations	7,004,999	5,187,549	4,385,642	3,885,730
R-squared	0.006	0.004	0.004	0.004
Number of firms	239,951	180,784	157,527	143,416
Firm Fixed Effects	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes
Month Effects	Yes	Yes	Yes	Yes
Province Specific Linear Trends	Yes	Yes	Yes	Yes
Region-Year Fixed Effects	Yes	Yes	Yes	Yes

Robust and clustered standard errors in parentheses.*** $p < 0.01$, ** $p < 0.05$, *$p < 0.1$

Note: Control provinces consist of Bilecik, Eskisehir, Balikesir, Manisa, Burdur, Isparta, Karabük, Samsun, Sakarya

Table A3: Intention to Treat Effects (ITT) Excluding Ankara, Adana and Kayseri 2012-2018

	All Regions	Region 4	Region 5	Region 6	All Regions	Region 4	Region 5	Region 6
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(9)
DiD coefficient	0.019***	0.019***	0.028***	0.014***				
	(0.002)	(0.003)	(0.003)	(0.004)				
impact2016					0.024***	0.031***	0.058***	-0.006
					(0.005)	(0.006)	(0.013)	(0.009)
impact2017					0.050***	0.065***	0.108***	-0.012
					(0.007)	(0.009)	(0.021)	(0.014)
impact2018					0.071***	0.092***	0.147***	-0.015
					(0.009)	(0.011)	(0.027)	(0.018)
Observations	8,863,569	6,057,644	4,847,618	4,412,229	8,863,569	6,057,644	4,847,618	4,412,229
R-squared	0.004	0.002	0.003	0.004	0.004	0.003	0.003	0.004
Number of Firms	350,790	235,105	188,442	169,019	350,790	235,105	188,442	169,019
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province Specific Linear Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Robust and clustered standard errors in parentheses.*** $p < 0.01$, ** $p < 0.05$, *$p < 0.1$

Table A4: Average Treatment Effects, by sector of economic activity

	Admin. and Support Serv.	Agricul. Agricul.	Arts, entert. recreation	Info. and Com. Info. and Com.	Construc. Construc.	Edu. Edu.	Elec. gas, steam	Finance Insur.	Accom. and food serv.	Human health Human health	Manufact. Manufact.	Mining Mining	Other services	Profes., technic. act.	Public admin defence	Real est. activities	Transp. Transp.	Water sup. sewerage	Wholesale, retail
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)
Treatment*After	0.043***	0.064**	0.048	0.042	0.247***	0.129***	0	0.055***	0.080***	0.058***	0.141***	0.267***	0.106***	0.080***	0.558	0.016**	0.103***	0.316***	0.074***
	(0.012)	(0.027)	(0.033)	(0.044)	(0.014)	(0.043)	(0.016)	(0.02)	(0.011)	(0.021)	(0.01)	(0.067)	(0.014)	(0.021)	(0.377)	(0.008)	(0.009)	(0.077)	(0.005)
Observations	3574839	481297	397205	488950	6096656	577185	605912	741744	3446686	776071	8380403	188296	2034236	1775424	9587	3194587	4301199	84642	14660469
R-squared	0.004	0.021	0.019	0.012	0.005	0.042	0.027	0.011	0.022	0.045	0.016	0.045	0.015	0.008	0.271	0.008	0.019	0.064	0.015
Number of firms	99706	12500	11331	14125	480284	14126	10641	11795	106383	16611	181844	5090	55037	41312	435	56470	106689	2644	340271
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Prov.Spec. Lin. Tre.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Reg.-Year Fixed Eff.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Robust and clustered standard errors in parentheses.*** $p < 0.01$, ** $p < 0.05$, *$p < 0.1$

Table A5: Difference-in-difference estimates, Restricted Control Provinces Time Varying Average Total Treatment Effects (ATT) 2012-2018

	All Regions	Region 4	Region 5	Region 6
	(1)	(2)	(3)	(4)
Treatment*Year 2016	0.030***	0.027***	0.045***	0.084
	(0.005)	(0.005)	(0.011)	(0.057)
Treatment*Year 2017	0.061***	0.054***	0.094***	0.136
	(0.008)	(0.008)	(0.016)	(0.084)
Treatment*Year 2018	0.072***	0.067***	0.100***	0.171
	(0.01)	(0.011)	(0.021)	(0.115)
Observations	11,539,996	9,722,546	8,920,639	8,420,727
R-squared	0.004	0.002	0.002	0.002
Number of firms	384471	325304	302047	287936
Firm Fixed Effects	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes
Month Effects	Yes	Yes	Yes	Yes
Province Specific Linear Trends	Yes	Yes	Yes	Yes
Nuts2-Region-Year Fixed Effects	Yes	Yes	Yes	Yes

Robust and clustered standard errors in parentheses.*** $p < 0.01$, ** $p < 0.05$, *$p < 0.1$

Note: Control provinces consist of Bilecik, Eskisehir, Balikesir, Manisa, Burdur, Isparta, Karabük, Samsun, Sakarya, Ankara, Adana and Kayseri

Table A6: Difference-in-difference estimates, Restricted Control Provinces and Further Excluding Ankara, Adana and Kayseri, Time Varying Average Total Treatment Effects (ATT) 2012-2018

	All Regions	Region 4	Region 5	Region 6
	(1)	(2)	(3)	(4)
Treatment*Year 2016	0.024***	0.021***	0.047***	0.084
	(0.006)	(0.006)	(0.014)	(0.057)
Treatment*Year 2017	0.050***	0.044***	0.088***	0.136
	(0.009)	(0.009)	(0.022)	(0.084)
Treatment*Year 2018	0.067***	0.061***	0.112***	0.171
	(0.011)	(0.012)	(0.028)	(0.115)
Observations	7,004,999	5,187,549	4,385,642	3,885,730
R-squared	0.006	0.004	0.004	0.004
Number of firms	239,951	180,784	157,527	143,416
Firm Fixed Effects	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes
Month Effects	Yes	Yes	Yes	Yes
Province Specific Linear Trends	Yes	Yes	Yes	Yes
Nuts2-Region-Year Fixed Effects	Yes	Yes	Yes	Yes

Robust and clustered standard errors in parentheses.*** $p < 0.01$, ** $p < 0.05$, *$p < 0.1$

Note: Control provinces consist of Bilecik, Eskisehir, Balikesir, Manisa, Burdur, Isparta, Karabük, Samsun, Sakarya

Table A7: Average Treatment Effects Relative to Contiguous Counties, Time-Variant Effects

	Kutahya	Kutahya	Usak	Usak	Afyon	Afyon	Duzce	Duzce	Kirikkale	Kirikkale	Bartin Kastamonu	Bartin Kastamonu
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treatment*Year 2016	0.016	0.049	0.04	0.093*	0.075***	0.099***	0.048	0.071	0.088***	0.164***	0.078	0.189
	(0.048)	(0.127)	(0.027)	(0.053)	(0.017)	(0.026)	(0.048)	(0.068)	(0.023)	(0.043)	(0.064)	(0.115)
Treatment*Year 2017	0.064	0.128	0.076**	0.122**	0.085***	0.119***	0.011	0.042	0.115***	0.183***	0.11	0.183*
	(0.066)	(0.139)	(0.035)	(0.057)	(0.023)	(0.034)	(0.066)	(0.104)	(0.031)	(0.054)	(0.089)	(0.103)
Treatment*Year 2018	0.089	0.164	0.095**	0.136*	0.120***	0.135***	-0.006	0.023	0.147***	0.205***	0.031	0.061
	(0.085)	(0.126)	(0.043)	(0.072)	(0.028)	(0.037)	(0.079)	(0.11)	(0.039)	(0.057)	(0.11)	(0.124)
Observations	232,588	232,588	163,518	163,518	287,810	287,810	141,161	141,161	190,025	190,025	72,138	72,138
R-squared	0.001	0.001	0.001	0.002	0.005	0.006	0.001	0.002	0.002	0.002	0.002	0.003
Number of firms	8920	8920	7263	7263	12458	12458	4835	4835	7368	7368	2785	2785
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province Linear Trends	Yes		Yes		Yes		Yes		Yes		Yes	
Province Year Fixed Effects		Yes		Yes		Yes		Yes		Yes		Yes

Robust and clustered standard errors in parentheses.*** $p < 0.01$, ** $p < 0.05$, *$p < 0.1$