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Roupakias, Stelios

Aristotle University of Thessaloniki, School of Economics

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Stelios Roupakias[a*]

[a]*Aristotle University of Thessaloniki, School of Economics, Faculty of Law, Economics and Political Sciences Building 54124, Thessaloniki Greece*

Abstract

This paper provides explores the short-run effects of minimum wage policies on the distribution of earnings and employment. We exploit the variation in the 'bite' of the minimum wage across region-industry cells, employing data from the Greek Labour Force Survey over the period 2016-2020. Using a Difference-in-Differences strategy, we estimate unconditional quantile regressions that yield economically important effects up to the 40th quantile of the earnings distribution. Importantly, we find that this does not come at the expense of disemployment effects, either at the extensive or at the intensive margin. Interestingly, there is some evidence that an increase in the minimum wage intensity is correlated with higher female employment. We attribute this finding to the fact that female labour markets are usually less competitive.

Keywords: Minimum wage, Earnings, Employment

1. Introduction

One of the long-standing issues in the economics literature is the impact of minimum wages on employment opportunities of affected individuals and the distribution of earnings. Recently, the interest has further been stimulated by the widespread tendency among Western countries to increase nominal rates as means to combat inequalities. Overall, the minimum wage puzzle remains largely unresolved. Previous US empirical studies have shown that minimum wage policies may bring either negative (e.g., Neumark and Wascher, 1992; Neumark et al., 2014) or positive employment impacts (e.g., Card and Krueger, 1994; Dube et al., 2010;). Likewise, the evidence from European labour markets is mixed (see, among others, Dolton et al., 2012; Caliendo et al., 2018; Harasztosi and Lindner, 2019; Holtemöller and Pohle, 2020).[1] On the other hand, economists usually agree that

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* Corresponding author. Email address: sroupakias@econ.auth.gr

minimum wages tend to produce a more compressed wage structure (e.g., Gramlich, 1976; Lee, 1999; Autor et al., 2016).

Economic theory identifies market power as the fundamental factor determining the labor market impact of the minimum wage. Through the lens of neoclassical economics, setting wage floors above the market-clearing level induces wage taker firms to cut employment (Stigler, 1946). By contrast, if employers have some degree of monopsonistic wage-setting power, the introduction of minimum wages could raise employment (Manning, 2003).[2] Within this context, minimum wage effects could be gender-specific, insofar as the elasticity of labour supply, that is inversely related to the employers' market power, differs by gender (see, e.g., Ransom and Oaxaca, 2010; Hirsch et al., 2010; Webber, 2016; Detilleux and Deschacht, 2021).[3] Search theoretic models suggest increased job search intensity as an alternative explanation why the minimum wages could lead to an equilibrium with higher employment (Cahuc et al., 2014). Minimum wages may induce spillover effects, as well, when employers adjust the wages of higher-paid employees, in order to prevent them from reducing their work effort (Grosssman, 1983). However, this might not always be the case, as have been shown in Gregory and Zierahn (2022). Under a constant elasticity of substitution production function and monopolistic competition in production, the net effect of a minimum wage rise on employees slightly above the minimum depends upon whether the scale or the substitution effect dominates. This, in turn, is determined by the relative size of the elasticity of substitution between labour inputs and the price elasticity of the output. However, within this setting, top earners who perform tasks not easily substitutable with tasks assigned to minimum wage earners, are expected to suffer wage losses.

We are aware of one published study with Greek data, analyzing the impact of the introduction of the subminimum wage for the youth (below the age of 25 years) in 2012, which was part of the Second Economic Adjustment Programme, agreed between the Greek government and the so-called *Troika* (EU Commission, European Central Bank, International Monetary Fund).[4] Specifically, Georgiadis et al. (2020), exploiting administrative data drawn from the Unified Social Security Fund, maintain that the subminimum wage did not translate into employment gains for. They attribute their findings to labour market frictions that prevent adjustments according to a perfectly competitive

[1] For a detailed, critical review of the so-called New Minimum Wage Research, see Neumark and Shirley (2021).
[2] As summarized in Manning (2003), there are mainly three sources conducive to the creation of a modern monopsony, namely imperfect information, preferences for non-pecuniary job 'amenities', and mobility costs.
[3] We refer to the elasticity of supply in a single firm.
[4] More details are available at: European Commission: The second economic adjustment programme for Greece: March 2012, European Economy Occasional Papers No. 94, Mar. 2012.

paradigm.[5]

This study focuses on a more recent policy aimed to restore minimum wages in Greece at the pre-memoranda levels. In particular, we make use of the minimum wage increase (+11%) and, at the same time, the abolishment of the subminimum (+27%) that went in effect since the first quarter of 2019. In absolute figures, these changes correspond to a rise in the minimum and the subminimum from 684€ and 596€, respectively, to 758€ per month, after the reform.[6] We estimate the impact of these developments in the Greek labour market by combining unconditional quantile regressions à la Firpo et al. (2009), with the Difference-in-Differences (DiD) approach. Since Greece has a national statutory minimum wage, we identify treated and control groups according to the variation in the minimum wage intensity across regions and industries. To that aim, we consider two continuous measures. The first is the fraction of individuals whose earnings were between the minimum wage in 2018 and the new minimum in 2019. Alternatively, we calculate a 'bite' variable that accounts for employees, eligible for the subminimum in the pre-increase period, as well.

The main advantage of the current paper, as compared to previous studies with Greek data, concerns the distinction between the treated and the counterfactual units. As we show in section 2, only half of the youth considered by existing work as the group receiving the treatment, appear to be affected by the minimum wage policy. Hence, comparisons between different young and senior employees might be less likely to uncover the true average treatment effect of the minimum wage. In addition, this is the first paper to study potential effects on wage structure, leveraging on the most recent data from the Greek Labour Force Survey. We, also, contribute to the strand of the literature identifying spillover effects on employers above the minimum wage (e.g., Bossler and Schank, 2020; Pérez, 2020; Gregory and Zierahn, 2022).

Our DiD strategy produces the following results. There is robust evidence that the abolishment of the subminimum wage and the sharp increase in the minimum wage caused the earnings at the bottom end of the earnings distribution to increase. Separate analysis by gender level reveals that the effects are felt by both sexes. By contrast, we find no evidence that the treatment intensity variable correlates with lower employment, either on intensive or the extensive margin. Instead,

[5] There are also two unpublished studies focusing on the 2012 reform. Both studies compare employment outcomes between age groups, before and after the reform, assuming a cutoff level at 25 years old. Yannelis (2014), based on micro-data from the Greek Labor Force Survey over the period 2009-2013, argues that the subminimum helped the youth to retain their jobs during the Greek crisis. More recently, Kakoulidou et al. (2018) estimate probit regressions using data from the same source, finding opposing results.

[6] These figures are based on 40 hours per week and 12 months of pay.

there is some evidence that female employment rises in the post-increase period. Importantly, we show that these findings are robust to heterogeneous pre-trends by running placebo regressions and accounting for region-specific linear trends.

The rest of the paper is structured as follows: Section 2 describes the data and the empirical strategy we employ to study the employment and inequality effects of the minimum wage policy. Section 3 summarizes the results. Finally, Section 4 concludes the paper.

2. Data and empirical model

To uncover the effects of the minimum wage policy, this study employs individual-level data over the period 2016-2020, drawn from the Greek Labour Force Survey (GR-LFS). This is a representative survey, being released since 1987 by the Hellenic Statistical Authority (*ELSTAT*),[7] containing rich information on the respondents' employment status and earnings,[8] as well as several demographic and background characteristics.[9] Approximately, there are about 240,000 individual observations on annual basis (60,000 quarterly). Following common practice in the literature, our analysis considers adult wage earners (between the age of 18 and 64 years), who do not participate in formal education.[10] We exclude individuals who report working uninsured and further restrict our sample to individuals with strong attachment in the labor market, i.e., those employed between 30 and 50 hours per week. Our final sample includes about 125,000 observations.

We exploit the variation in outcomes and minimum wage intensity across 91 region-industry cells. Originally, GR-LFS sectors of economic activity are classified into 21 1-digit categories according to Nace Rev.2. To strengthen our empirical strategy, in terms of sample sizes, we convert the classification of industries to the 7 ISIC classification, following *ELSTAT's* crosswalk. On the other hand, we consider 13 regions at the NUTS2 level,[11] the lowest level of spatial aggregation used by *ELSTAT*.

Following closely Pérez (2020), we estimate the effects of the minimum wage policy using the Difference-in-Differences specification displayed below: [12]

[7] Available at: https://www.statistics.gr/.
[8] We deflate the earnings by using the CPI issued by *ELSTAT*.
[9] It is worth noting, that *ELSTAT* used to report wages in intervals. However, since 2015, wages have been reported in absolute figures.
[10] We, therefore, exclude students, the self-employed and family workers from the sample.
[11] NUTS is an acronym for Nomenclature des Unités Territoriales Statistiques. Greece is divided in 13 NUTS2 regions: Eastern Macedonia; Central Macedonia; Western Macedonia; Epirus; Thessaly; Ionian Islands; Western Greece; Central Greece; Attica; Peloponnese; North Aegean; South Aegean; Crete.
[12] We do so, using the command rifhdreg in STATA (Rios-Avila, 2020).

$$RIF\left(w_{icjt},q_\tau\right)=\varphi_{cj}+\varphi_t+\theta Bite_{cj}\times Post+\delta\boldsymbol{X_{cjt}}+\varepsilon_{cjt} \quad (1)$$

where the outcome variable is the Recentered Influence Function (RIF) of monthly earnings in region *c*, industry *j*, and year *t*, defined as (Firpo et al., 2009):

$$RIF\left(w_{icjt},q_\tau\right)=q_\tau+\frac{\tau-1(w\le q_\tau)}{f_w(q_\tau)} \quad (2)$$

where $f_w(q_\tau)$ stands for the density at estimated quantile q_τ, and $1(w\le q_\tau)$ is a dummy indicator for earnings below q_τ. We estimate the density $f_w(q_\tau)$ using Gaussian kernel and setting a bandwidth according to Silverman's rule. We prefer monthly earnings for two main reasons. First, they are less prone to measurement error than hourly wages. By restricting the sample to strongly attached salaried earners, we feel that this strategy is more credible within the context of the current paper. This choice is also motivated by previous related studies which also consider monthly wages as the dependent variable (e.g., Bossler and Schank, 2020; Pérez, 2020). Terms φ_{ci} and φ_t stand for region-industry fixed effects and time effects, respectively; $\boldsymbol{X_{cjt}}$ is a vector of macroeconomic factors at the regional level (GDP, Bartik employment)[13] and individual controls, that include age (and its square), and dummies for sex, nationality, and the highest level of educational attainment; ε_{cjt} is a random error term.

Identification comes from the interaction between the post-treatment indicator, *Post*, and the $Bite_{cj}$ variable that is intended to capture differences in the minimum wage intensity across region-industry cells. Specifically, we use Card's (1992) "fraction affected" variable, defined as the share of employees whose earnings range between the minimum wage in the pre-increase period (year 2018) and the new minimum wage in 2019:

$$Fraction\ affected_{cj,2018}=\frac{\sum_i\left(mw_{2018}\le w_{icj,2018}\le mw_{2019}\right)}{n_{cj,2018}} \quad (3)$$

However, the "fraction affected" variable does not account for the youth (below the age of 25 years), who were eligible for the subminimum wage in 2018, and thus were strongly affected by the reform. Hence, alternatively, we measure the incidence of minimum wage by the "fraction at" variable,

[13] Our Bartik (1991) variable serves as an exogenous proxy for local labour demand shocks. It is given by the product of each region's (lagged) industry j employment share, and the log difference of the national employment in industry j, aggregated at the regional level. To further reduce endogeneity concerns, we exclude own region contribution to employment:

$$Bartik_{ct}=\sum_1^J\left(\frac{E_{cj,t-1}}{E_{c,t-1}}\right)\times \mathrm{d}logE_{j(-c)}$$

computed as the ratio of salaried employees between the subminimum wage in 2018 and the new minimum thereafter:[14] Both indices range between 0 and 1, with higher values suggesting greater exposure to the minimum wage policy. Since both treatment variables are continuous, coefficient θ should be interpreted as measuring the short-run effect of a percentage change in the minimum wage on the distribution of earnings in the post-treatment period. Concerning statistical inference, we remain conservative to avoid over-rejection of the null and use wild bootstrap-corrected standard errors, clustered at the region-industry level (see, e.g., Cameron et al., 2008).[15]

An illustration of the variation in the treatment intensity across regions and sectors of economic activity is shown in Figure 1, which were ranked from the least to the most affected. Interestingly, both measures uncover an overly consistent picture, suggesting that the regions Western Macedonia, Western Greece, and Pelopennese are exposed more heavily to the minimum policy. On the other hand, the sectors where there is a widespread prevalence of the minimum wage appear to be "Wholesale and retail trade", "agriculture", and "construction". Not surprisingly, the least affected sector is "Administration" where the majority of workers are public servants who are not covered by the minimum wage legislation. Notably, the capital city of Greece, Athens (Attica), and the industries within it, usually display the lowest exposure rates. This pattern is consistent with the idea that workers in large urban centers receive higher wages than their counterparts in small areas.

[Insert Figure 1 about here]

We also provide some preliminary graphical evidence in Figure 2, based on kernel density estimates, before (2018) and after (2020) the drastic increase in the minimum wage in 2019, distinguishing between high and low 'bite' region-industry groups, (i.e., groups which fall into the top tercile of the minimum wage 'bite' and others). We observe that the density of the earnings distribution has shifted rightward in clusters displaying high exposure.[16] By contrast, this does not appear to be the case for the least exposed groups, as the estimated densities remain largely unaffected in 2020.

[Insert Figure 2 about here]

[14] Some studies rely on the so-called Kaitz ratio, that is national statutory minimum wage divided by the average earnings of full-time salary earners. However, note that this indicator may be prone to endogeneity, stemming from the potential correlation between the denominator and employment (see, e.g., Card et al., 1994). Hence, we abstain from running regressions using this index as a measure of treatment intensity in the main text. We nevertheless, report some findings in the Appendix Figure A3.

[15] To that aim, we use the command boottest in STATA (Roodman et al., 2019).

[16] For better visualization, we estimate the densities with a bandwidth of 0.2.

Before concluding this section, it is important to highlight that the strategy described above permits causal identification of the minimum wage effect, provided that treated and control (counterfactual) units display common trends before the reform. As can be seen in appendix figure A1, the wage densities for the pre-treatment period remain quite stable between 2016 and 2018 for both the high and the low minimum wage intensity units. We consider these patterns as preliminary evidence against heterogeneous pre-trends. Nevertheless, we delve more formally into this issue in the next section by estimating placebo effects, using the pre-increase sample only. In addition, as discussed above, the vector of explanatory variables includes a rich set of fixed effects (and interactions between the fixed effects), as well as macroeconomic factors, which are intended to control for heterogeneous, region-specific shocks. Lastly, there is yet another important condition that needs to be satisfied, the so-called Stable Unit Treatment Value Assumption (SUTVA), which corresponds to the absence of spillovers running from the treated toward the control units (Imbens and Rubin, 2015). As discussed in Gregory and Zierahn (2022), spillover effects may be due to inter-sectoral mobility or complementarities in production, inducing a downward bias in the estimates. Unfortunately, we cannot directly test whether minimum wages affect the control units through these channels. At least, we could argue that employees cannot self-select into industries, over a short-term period, due to different skill requirements. With these caveats in mind, we consider the estimates shown in this study as representing the lower bound effect of the 2019 minimum wage reform.

3. Empirical findings

In this section we discuss the main results, estimating the RIF-DID regressions described above with the latest available data from *ELSTAT*. We consider three outcome variables, namely monthly earnings, employment and hours worked. We conduct a battery of sensitivity analysis to confirm the validity of our approach and the robustness of the findings, using alternative indicators of minimum wage intensity, region-specific linear trends, individual covariates and estimating placebo effects.

Table 2 summarizes the first set of the results from our RIF-DiD approach, described in section 2. We present estimates for the effect of the minimum wage at selected quantiles.[17] Column (1) includes regional dummies interacted with industry dummies, to control for unobserved, time-invariant

[17] We have also experimented with regressions account for the task content of the occupations. The results (not reported for brevity, available upon request) obtained after these amendments appear to be quite identical to the ones shown in the text.

heterogeneity. As can easily be identified, there is an economically important effect at the bottom end of the wage structure. The estimated coefficients of interest suggest that a 10 per cent rise in the minimum wage causes the earnings of the lowest paid workers to increase by about 1 percent. On the other hand, we interpret the fact that we fail to detect significant effects at the upper end of the earnings distribution, as evidence against spillovers toward the highest paid employees. Nevertheless, caution is needed when interpreting the results for workers well above the minimum wage, since our main independent variable of interest is intended to capture workers up to new minimum in 2019.

The second specification introduces year fixed effects to purge the equation from period-specific shocks that are common across region-industry defined labour markets. This modification yields positive and significant effects up to the 40^{th} quantile. Next, we introduce two macroeconomic variables, namely the lagged GDP per capita, and Bartik employment, that may affect the evolution of earnings. Once again, the results we obtain appear to be in line with the previous ones. Lastly, we pay attention to Neumark et al. (2014), who suggest testing whether the results withstand the inclusion of region-specific trends. The authors emphasize that any potential discrepancies arising after this amendment, should be interpreted as evidence of bias, due to deviations from the common trend assumption. To that aim, we re-estimate eq (1) by adding NUTS2 linear trends (i.e., interactions between the region dummies and a continuous trend variable). The results shown in Row (4) remain rather consistent after having added a region-specific time trend, indicating that the rise in the minimum wage is robustly correlated with higher wages at the lower end of the earnings distribution.

At the bottom part of Table 2 we present coefficient estimates using an alternative definition of the minimum wage intensity, namely the "fraction at", that is, the share of employees between the subminimum in 2018 (which corresponds to about 75% of the minimum in that same year) and the new minimum wage since the first quarter of 2019. Once again, we observe significant increases in wages at the bottom end of the earnings distribution.[18] In sum, the effects of the reform appear to be in line with policymakers' intentions to enhance the well-being of workers with low earnings.

[Insert Table 2 about here]

Thus far, we have tested the common trends assumption by comparing results from specifications

[18] We have also run regressions after having discretized the treatment variable into two groups, namely above and below the median minimum wage intensity. The results, (not reported for brevity, available upon request) appear to be consistent with the ones we find using the continuous measure.

with and without region-specific time trends. We have found that there are no systematic differences between these specifications. In the Appendix figure A2, we perform a placebo test, to further establish the validity of our empirical design. Specifically, we use data for the pre-reform period only, fictitiously assuming that the minimum wage increase went in effect in 2018. We estimate an identical specification as in Table 2, Row 4, using the fraction of affected individuals as the treatment intensity measure. Significant and positive effects on the earnings around the level of the minimum wage should be considered as suggestive of potentially severe heterogeneous pre-trends issues. Reassuringly, this empirical exercise turns out correlations not statistically different from zero. What is more, the sign of the point estimates at the bottom end of the wage distribution appears to be negative. We have also considered an alternative robustness check, estimating the unconditional quantile regressions on the sample of public servants only. The rationale is that this group of workers is unlikely to be affected by the minimum wage policy directly, as the national minimum wage applies to private sector employees only.[19] As shown at the bottom part of the Appendix figure A2, this is largely confirmed by the insignificant effects which tend to cluster around the zero-reference line across the unconditional quantile distribution.

Next, we re-estimate eq. (1), by expanding the set of controls to include demographic and human capital variables. In particular, we control for age (as a proxy for labour market experience), sex, nationality, and education. This is the most complete specification employed in this study. Table 3 presents the estimation results. The main coefficients of interest are also illustrated in Figure 3. We find no significant differences with respect to the effects of treatment intensity, whether we use the fraction affected (panel a) or the fraction at measure (panel b). Between the 10th and the 40th quantiles, the coefficients on the minimum wage variable range from about 0.002 to 0.013, indicating important earnings gains for the low-paid workers. On the other hand, the individual covariates enter with their expected signs. Education displays an inverse U-shaped relationship with earnings. On the other hand, being male have a positive correlation with wages, whilst the opposite holds true for immigrants (i.e., workers born abroad). University and high-school graduates have higher earnings as compared to the unskilled.

[Insert Table 3 about here]

[Insert Figure 3 about here]

[19] Nevertheless, it is likely that the national minimum wage and the introductory wage in the public sector might be positive related, and, thus, public servants' earnings might also be affected indirectly by the 2019 reform.

What is more, we examine whether the effects of the 2019 reform are heterogenous by gender. This empirical exercise serves as a preliminary test on the contribution of the minimum wage on the gender wage gap. Most importantly, it would facilitate the interpretation of gender-specific employment effects, at which we look by the end of this section. As we observe in Table 4, low-paid employees are significantly affected by the reform, irrespective of gender. Nevertheless, the estimated coefficients appear to be significant up to the 40th quantile in male regressions, whereas cease to be significant in female regressions above the 20th quantile. Once again, we fail to detect spillovers into employees above the minimum wage.

[Insert Table 4 about here]

Lastly, we estimate the effects on employment, aggregating the data at the region-industry level. We consider the logarithm of total employment as the outcome variable, and report estimates by gender. We also report the impact on average hours worked, estimating individual level regressions, in order to partially establish whether the effects on the wage structure are driven by adjustments at the intensive margin. Both analyses are based on specifications with the usual set of fixed effects and region-specific trends.[20] Table 5 clearly illustrates that there are no significant disemployment effects either at the extensive or at the intensive margin, once we measure the 'bite' using the fraction affected variable. Similar patterns emerge when we replicate the analysis measuring the minimum wage incidence with the fraction at variable. There is, however, one important exception concerning female employment. Specifically, we find that a 10% increase in the treatment intensity causes employment to rise by about 0.75% in the post-treatment period. These findings corroborate with our expectations, insofar as firms' market power is higher against female employees. If that were the case, it can be easily shown that a minimum wage increase can lead to an improvement in the employment opportunities of affected employees. Unfortunately, it is not possible to measure labour market competition by gender with the data at our disposal. However, based on international evidence (see, e.g., Ransom and Oaxaca, 2010; Hirsch et al., 2010; Webber, 2016; Detilleux and Deschacht, 2021), it is reasonable to attribute this finding to the fact that female supply to a single employer is less elastic than the supply of their male counterparts.

[20] We have also estimated employment regressions which use gdp per capita and population as further controls. The results (not reported for brevity, available upon request) are in the same ballpark as those presented in Table 5.

[Insert Table 5 about here]

4. Conclusion

Proponents of the minimum wage policy argue that minimum wages constitute an important instrument to boost the earnings of low-paid employees. Others argue that minimum wages may engender unintended consequences, such as reductions in the employment opportunities of the 'outsiders'. We test these ideas by exploiting a drastic minimum wage reform in 2019, using the latest available data from the Greek Labour Force Survey. The analysis leverages differences in treatment intensity between regions and sectors of economic activity. We go beyond average effects by estimating effects on the distribution of wages, combining a Difference-in-Differences approach and unconditional quantile regressions.

We report positive effects up to the 40^{th} quantile, that are robust to several modifications. On the other hand, we do not find spillover effects on the earnings of the salaried employees at the top end of the wage structure. Importantly, we show that these findings are unlikely to be driven by heterogeneous pre-trends between high- and low-exposed units. By contrast, we do not find economically important disemployment effects. Instead, there is some evidence that the increase in the minimum wage and the abolishment of the subminimum wage in 2019 are associated with higher female employment. A potential reasonable explanation to this finding concerns differences in the labour supply elasticity between male and female employees.

Overall, the results appear to be consistent with Bossler and Schank (2020) and Pérez (2020) for Germany and Colombia, respectively. They, nevertheless, do not corroborate with the idea that labour markets function as the standard textbook model of perfect competition dictates (see, e.g., the discussion in Manning, 2021). There are, however, two caveats which should be kept in mind. First, more detailed microdata and variation at lower levels of spatial aggregation are needed to explore more comprehensively these issues. Second, considering that the reform considered in this study aimed to restore the minimum wage at the levels prevailing prior Greece's fiscal issues over the previous decade, we cannot rule out the possibility that further raises may yield results different from the ones we found above.

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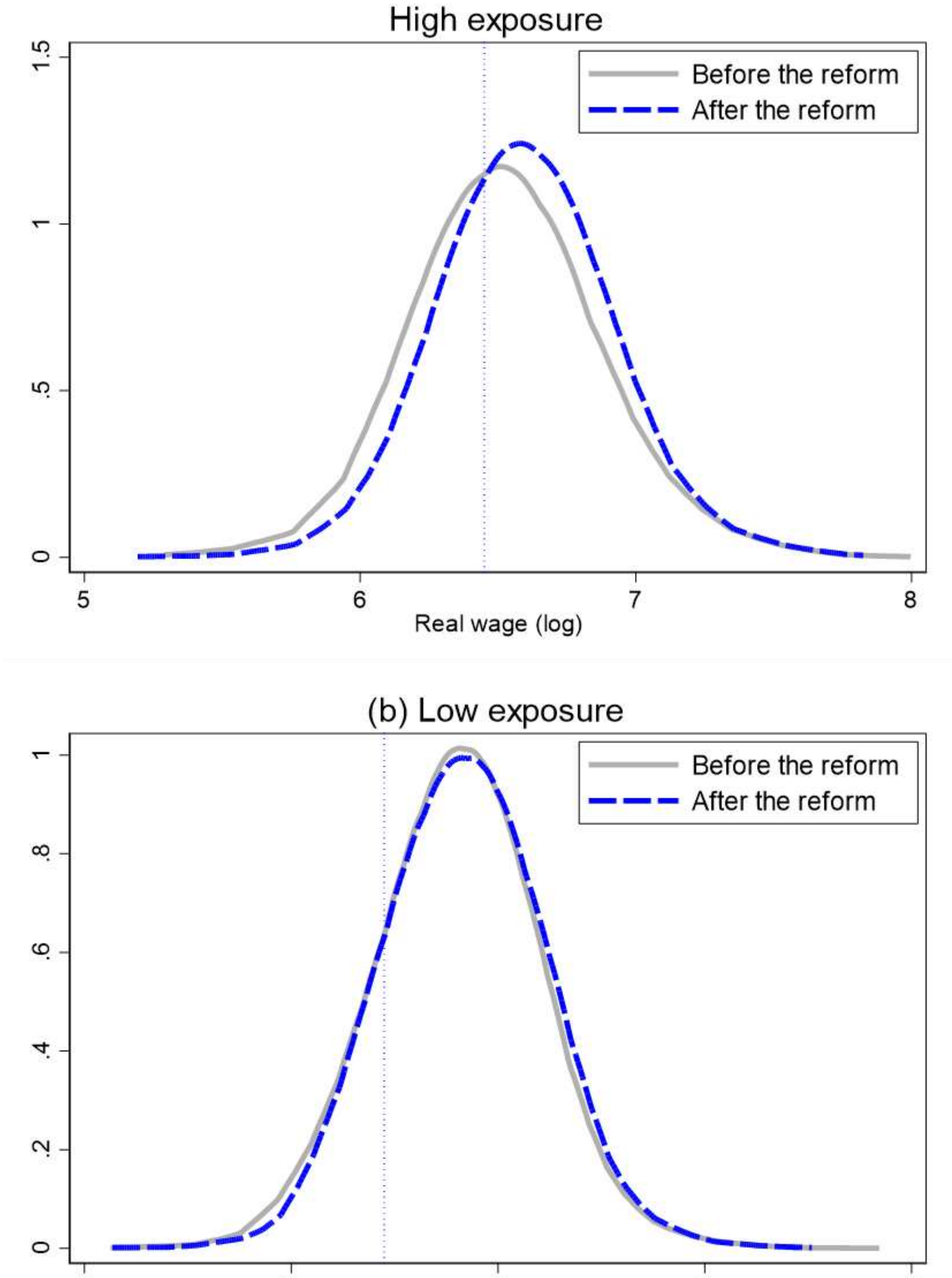


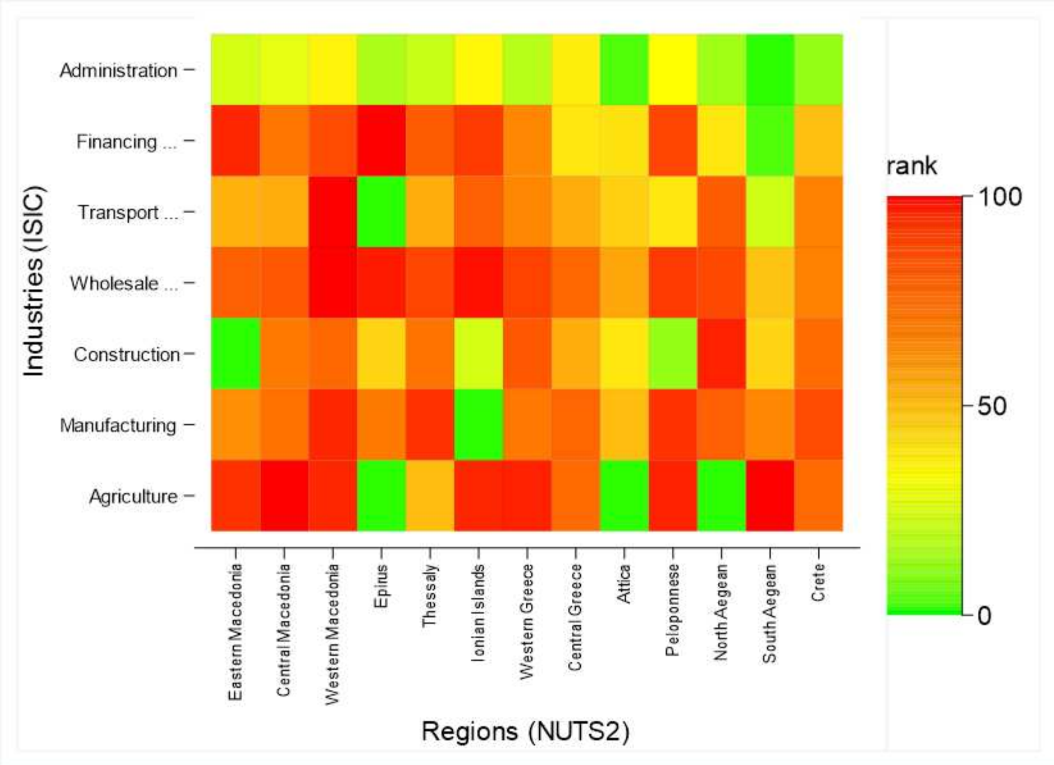
Figure 1. Kernel density estimates of real monthly wage in region-industry cells above and below the top tercile of minimum wage “bite”. The vertical line line indicates the level of the real minimum wage in 2019. The estimates are obtained using the weights provided by *ELSTAT*. Earnings deflated using the CPI from *ELSTAT*. Own elaborations on GR-LFS data.

Table 1. Descriptive Statistics

Variables	Obs	Mean	Std. Dev.	Min	Max
Real monthly wage (logged)	124,502	6.76	.36	4.48	8.89
Hours worked usually	124,502	40.62	4.37	30	50
Age	124,502	42.64	9.96	18	64
Agriculture, Hunting, Forestry and Fishing	124,502	.02	.15	0	1
Manufacturing	124,502	.14	.35	0	1
Construction	124,502	.04	.19	0	1
Wholesale and Retail Trade and Restaurants and Hotels	124,502	.26	.44	0	1
Transport, Storage and Communication	124,502	.08	.27	0	1
Financing, Insurance, Real Estate and Business Services	124,502	.1	.29	0	1
Services (Administration, Education and Health)	124,502	.37	.48	0	1
Foreign-born	124,502	.09	.29	0	1
Male	124,502	.56	.5	0	1
Secondary education	124,502	.44	.5	0	1
Tertiary education	124,502	.49	.5	0	1

This table reports summary statistics, using weights provided by *ELSTAT*.

a. "Fraction affected": share of employees between the minimum wage in 2018 and the new minimum in 2019



b. "Fraction at": share of employees between the subminimum wage in 2018 and the new minimum in 2019

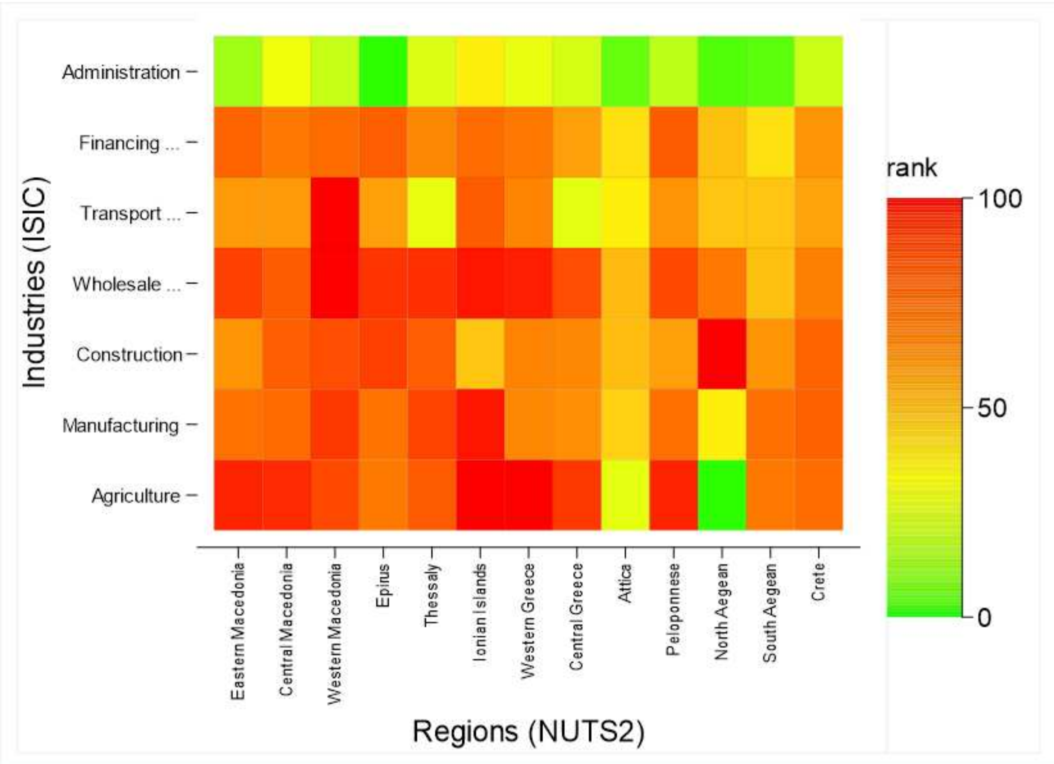


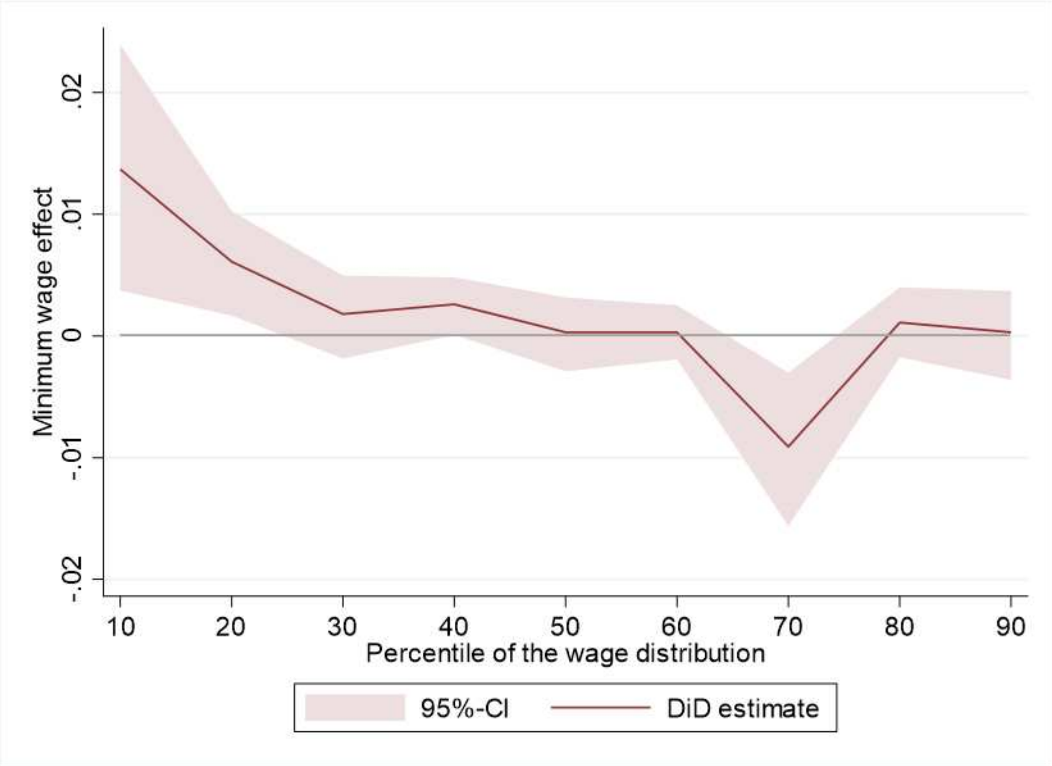
Figure 2 Treatment intensity by region-industry cells, as measured by the 'fraction affected' and the "Fraction at" variables in 2018. Own elaborations on GR-LFS data.

Table 2 Impact of the minimum wage on the distribution of earnings

	P10	P20	P30	P40	P50	P75	P90
Panel A. Bite Measure: Fraction Affected							
[1] Region x Industry FE							
Bite x Post	0.0096	0.0043	0.0017	0.0016	0.0021	0.0006	0.0008
	(0.0351)	(0.0551)	(0.1779)	(0.1531)	(0.1029)	(0.4035)	(0.4105)
	[0.0017, 0.0191]	[-0.0004, 0.0078]	[-0.0016, 0.0041]	[-0.0012, 0.0036]	[-0.0009, 0.0042]	[-0.0015, 0.0018]	[-0.0019, 0.0027]
[2] Time FE							
Bite x Post	0.0146	0.0067	0.0022	0.0028	0.0008	0.0014	0.0010
	(0.0337)	(0.0388)	(0.0860)	(0.0295)	(0.3545)	(0.2143)	(0.5948)
	[0.0032, 0.0280]	[0.0012, 0.0122]	[-0.0005, 0.0046]	[0.0006, 0.0049]	[-0.0018, 0.0027]	[-0.0011, 0.0038]	[-0.0038, 0.0053]
[3] GDP, Bartik							
Bite x Post	0.0145	0.0069	0.0023	0.0028	0.0006	0.0012	0.0006
	(0.0339)	(0.0288)	(0.0670)	(0.0260)	(0.3075)	(0.2343)	(0.7625)
	[0.0036, 0.0273]	[0.0023, 0.0113]	[-0.0003, 0.0045]	[0.0008, 0.0046]	[-0.0009, 0.0020]	[-0.0013, 0.0035]	[-0.0037, 0.0048]
[4] Region-specific trends							
Bite x Post	0.0149	0.0072	0.0026	0.0032	0.0010	0.0013	0.0005
	(0.0248)	(0.0127)	(0.1064)	(0.0189)	(0.3520)	(0.1963)	(0.7445)
	[0.0048, 0.0261]	[0.0028, 0.0114]	[-0.0010, 0.0056]	[0.0011, 0.0051]	[-0.0017, 0.0032]	[-0.0011, 0.0032]	[-0.0029, 0.0034]
Panel B. Bite Measure: Fraction At							
[5] Region x Industry FE							
Bite x Post	0.0035	0.0015	0.0007	0.0006	0.0008	0.0002	0.0003
	(0.0432)	(0.0613)	(0.1440)	(0.1302)	(0.0741)	(0.3125)	(0.3337)
	[0.0002, 0.0067]	[-0.0001, 0.0027]	[-0.0004, 0.0014]	[-0.0003, 0.0014]	[-0.0001, 0.0014]	[-0.0003, 0.0008]	[-0.0006, 0.0010]
[6] Time FE							
Bite x Post	0.0068	0.0029	0.0012	0.0015	0.0005	0.0007	0.0004
	(0.0142)	(0.0213)	(0.0504)	(0.0115)	(0.2445)	(0.1578)	(0.5933)
	[0.0036, 0.0126]	[0.0012, 0.0053]	[0.0000, 0.0023]	[0.0008, 0.0021]	[-0.0007, 0.0015]	[-0.0006, 0.0020]	[-0.0017, 0.0033]
[7] GDP, Bartik							
Bite x Post	0.0068	0.0030	0.0012	0.0015	0.0004	0.0006	0.0003
	(0.0131)	(0.0157)	(0.0513)	(0.0069)	(0.1871)	(0.2011)	(0.7585)
	[0.0036, 0.0125]	[0.0014, 0.0055]	[0.0000, 0.0024]	[0.0008, 0.0021]	[-0.0004, 0.0011]	[-0.0007, 0.0022]	[-0.0015, 0.0028]
[8] Region-specific trends							
Bite x Post	0.0071	0.0031	0.0013	0.0016	0.0005	0.0006	0.0002
	(0.0114)	(0.0178)	(0.0701)	(0.0065)	(0.3040)	(0.1845)	(0.7256)
	[0.0044, 0.0101]	[0.0015, 0.0050]	[-0.0001, 0.0026]	[0.0007, 0.0023]	[-0.0008, 0.0016]	[-0.0005, 0.0016]	[-0.0011, 0.0016]
Clusters				91			
Observations				124,502			

This table presents RIF-OLS estimates for 13 Greek NUTS2 regions and 7 sectors of economic activity, using GR-LFS data for the period 2016-2020. Exposure to minimum wage is measured by the fraction of individuals between the minimum wage in 2018 and the new minimum in 2019. Wild Bootstrapped p-values (confidence intervals) in parentheses (brackets), clustered by region-industry unit, obtained through 999 iterations. *** p<0.01, ** p<0.05, *p<0.1

a. Bite measure: Fraction affected



b. Bite measure: Fraction at.

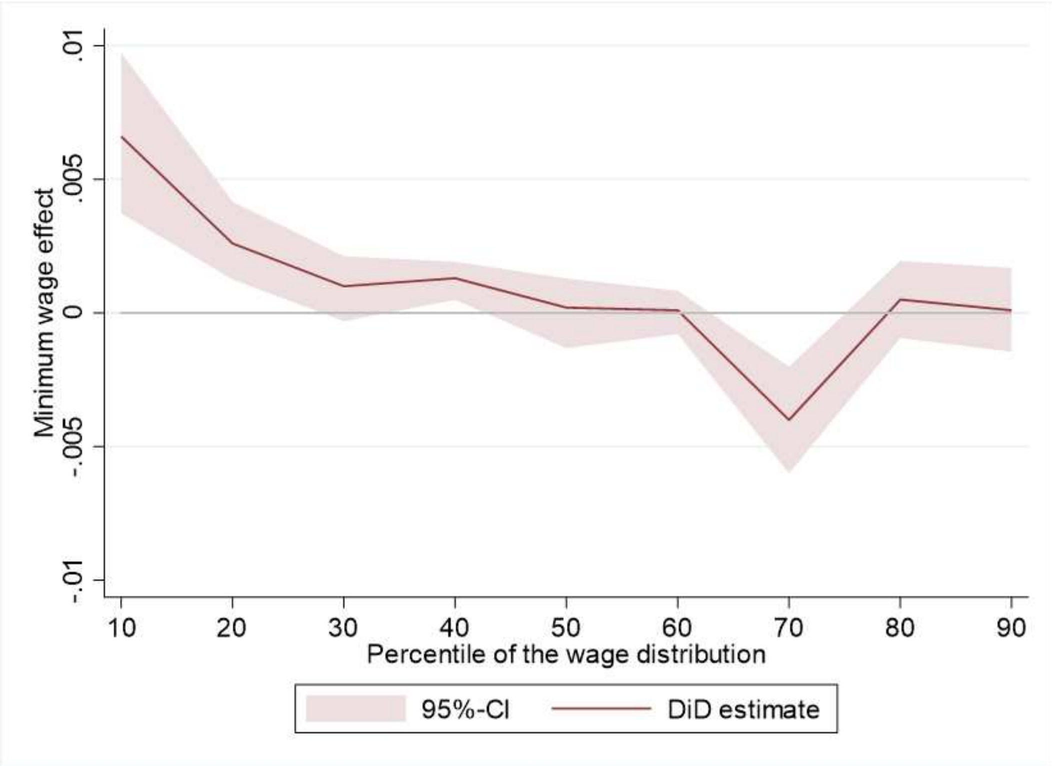


Figure 3 This figure plots the effects of the 2019 reform on the distribution of earnings, measuring the incidence of minimum wage by the fraction affected variable (Panel a) and by the fraction at variable (Panel b). Both specifications include Time and region by industry fixed effects; Region-specific linear trends; GDP, Bartik; age (and its square); dummies for sex, nationality, and level of educational attainment. The shaded region is the 95% confidence interval, generated though a wild bootstrapped process with 999 replications. Own elaborations on GR-LFS data.

Table 3 Impact of the minimum wage on the distribution of earnings, individual covariates

	P10	P20	P30	P40	P50	P75	P90
Bite x Post	0.0137 (0.0184) [0.0037, 0.0239]	0.0061 (0.0243) [0.0016, 0.0102]	0.0018 (0.2051) [-0.0018, 0.0049]	0.0026 (0.0465) [0.0001, 0.0048]	0.0003 (0.7962) [-0.0029, 0.0031]	.0009 (0.4450) [−.0021, .0037]	0.0003 (0.8280) [-0.003613, 0.003699]
Age	0.0931*** (0.0017)	0.0807*** (0.0011)	0.0551*** (0.0008)	0.0468*** (0.0007)	0.0460*** (0.0009)	0.0143*** (0.0009)	-0.0000 (0.0012)
Age squared	-0.0009*** (0.0000)	-0.0008*** (0.0000)	-0.0005*** (0.0000)	-0.0004*** (0.0000)	-0.0004*** (0.0000)	-0.0000*** (0.0000)	0.0001*** (0.0000)
Male	0.1673*** (0.0047)	0.1512*** (0.0031)	0.1327*** (0.0021)	0.1285*** (0.0019)	0.1596*** (0.0024)	0.1363*** (0.0024)	0.1237*** (0.0031)
Medium skilled	0.1308*** (0.0094)	0.1172*** (0.0062)	0.0945*** (0.0043)	0.0828*** (0.0039)	0.1426*** (0.0049)	0.1266*** (0.0049)	0.1029*** (0.0063)
High skilled	0.2420*** (0.0099)	0.2356*** (0.0065)	0.1990*** (0.0045)	0.1984*** (0.0041)	0.3238*** (0.0051)	0.3280*** (0.0051)	0.3323*** (0.0065)
Immigrant	-0.1993*** (0.0084)	-0.1986*** (0.0055)	-0.1420*** (0.0038)	-0.1290*** (0.0035)	-0.1442*** (0.0043)	-0.0678*** (0.0044)	-0.0410*** (0.0056)
GDP, Bartik				Yes			
Region × Industry FE				Yes			
Time FE				Yes			
Region-specific trends				Yes			
Clusters				91			
Observations				124,502			

This table presents RIF-OLS estimates for 13 Greek NUTS2 regions and 7 sectors of economic activity, using GR-LFS data for the period 2016-2020. Exposure to minimum wage is measured by the fraction of individuals between the subminimum wage in 2018 and the new minimum wage in 2019. Bootstrapped standard errors clustered by region, obtained through 999 iterations. *** p<0.01, ** p<0.05, *p<0.1

Table 4 Impact of the minimum wage on the distribution of earnings, Estimates by gender

	P10	P20	P30	P40	P50	P75	P90	N
Panel A. Bite Measure: Fraction Affected								
Male:								
Bite x Post	0.0137	0.0064	0.0026	0.0030	-0.0001	0.0007	0.0004	
	(0.0207)	(0.0052)	(0.0715)	(0.0138)	(0.9263)	(0.6051)	(0.8462)	69,475
	[0.0041, 0.0248]	[0.0031, 0.0101]	[-0.0003, 0.0051]	[0.0009, 0.0048]	[-0.0032, 0.0028]	[-0.0028, 0.0041]	[-0.0044, 0.0051]	
Female:								
Bite x Post	0.0152	0.0067	0.0017	0.0025	0.0014	0.0019	0.0003	
	(0.0343)	(0.0630)	(0.4314)	(0.2047)	(0.2633)	(0.2209)	(0.7688)	55,027
	[0.0021, 0.0276]	[-0.0003, 0.0128]	[-0.0046, 0.0070]	[-0.0021, 0.0062]	[-0.0018, 0.0045]	[-0.0016, 0.0051]	[-0.0025, 0.0032]	
Panel B. Bite Measure: Fraction At								
Male:								
Bite x Post	0.0067	0.0028	0.0014	0.0016	0.0000	0.0003	-0.0001	
	(0.0158)	(0.0135)	(0.0475)	(0.0043)	(0.9532)	(0.7255)	(0.9388)	69,475
	[0.0035, 0.0110]	[0.0011, 0.0045]	[0.0000, 0.0027]	[0.0010, 0.0021]	[-0.0012, 0.0012]	[-0.0016, 0.0022]	[-0.0021, 0.0021]	
Female:								
Bite x Post	0.0070	0.0026	0.0007	0.0011	0.0006	0.0009	0.0003	55,027
	(0.0135)	(0.0426)	(0.3398)	(0.1005)	(0.2804)	(0.1571)	(0.5137)	
	[0.0036, 0.0104]	[0.00017, 0.0049]	[-0.0013, 0.0024]	[-0.0003, 0.0023]	[-0.0008, 0.0017]	[-0.0005, 0.0021]	[-0.0007, 0.0014]	

This table presents RIF-OLS and OLS estimates for 13 Greek NUTS2 regions and 7 sectors of economic activity, using GR-LFS data for the period 2016-2020. Exposure to minimum wage is measured by the fraction of individuals between the subminimum wage in 2018 and the new minimum in 2019. Bootstrapped standard errors clustered by region, obtained through 100 iterations. *** p<0.01, ** p<0.05, *p<0.1

Table 5. Impact of the minimum wage on employment and hours worked

	(Total)	(Male)	(Female)
	Panel A. Bite Measure: Fraction Affected		
Employment			
BitexPost	-0.0010	-0.0033	0.0184
	(0.7887)	(0.3872)	(0.1677)
	[-0.0108, 0.0083]	[-0.0130, 0.0052]	[-0.0083, 0.0493]
Hours			
BitexPost	0.0035	0.0010	0.0088
	(0.8199)	(0.9607)	(0.7256)
	[-0.0306, 0.0451]	[-0.0483, 0.0526]	[-0.0385, 0.0733]
	Panel B. Bite Measure: Fraction At		
Employment			
BitexPost	-0.0022	-0.0028	0.0075
	(0.3066)	(0.1224)	(0.0426)
	[-0.0071, 0.0018]	[-0.0068, 0.0012]	[0.0001, 0.0145]
Hours			
BitexPost	0.0026	0.0003	0.0063
	(0.6617)	(0.9596)	(0.4684)
	[-0.0106, 0.0185]	[-0.0163, 0.0182]	[-0.0127, 0.0281]
Cluster	91	91	91
N	450	450	450

This table presents OLS estimates for 13 Greek NUTS2 regions and 7 sectors of economic activity, using GR-LFS data for the period 2016-2020. Exposure to minimum wage is measured by the fraction of individuals between the subminimum wage in 2018 and the new minimum in 2019. Wild Bootstrapped p-values (confidence intervals) in parentheses (brackets) clustered at the region-industry level, obtained through 999 iterations. *** p<0.01, ** p<0.05, *p<0.1

Appendix

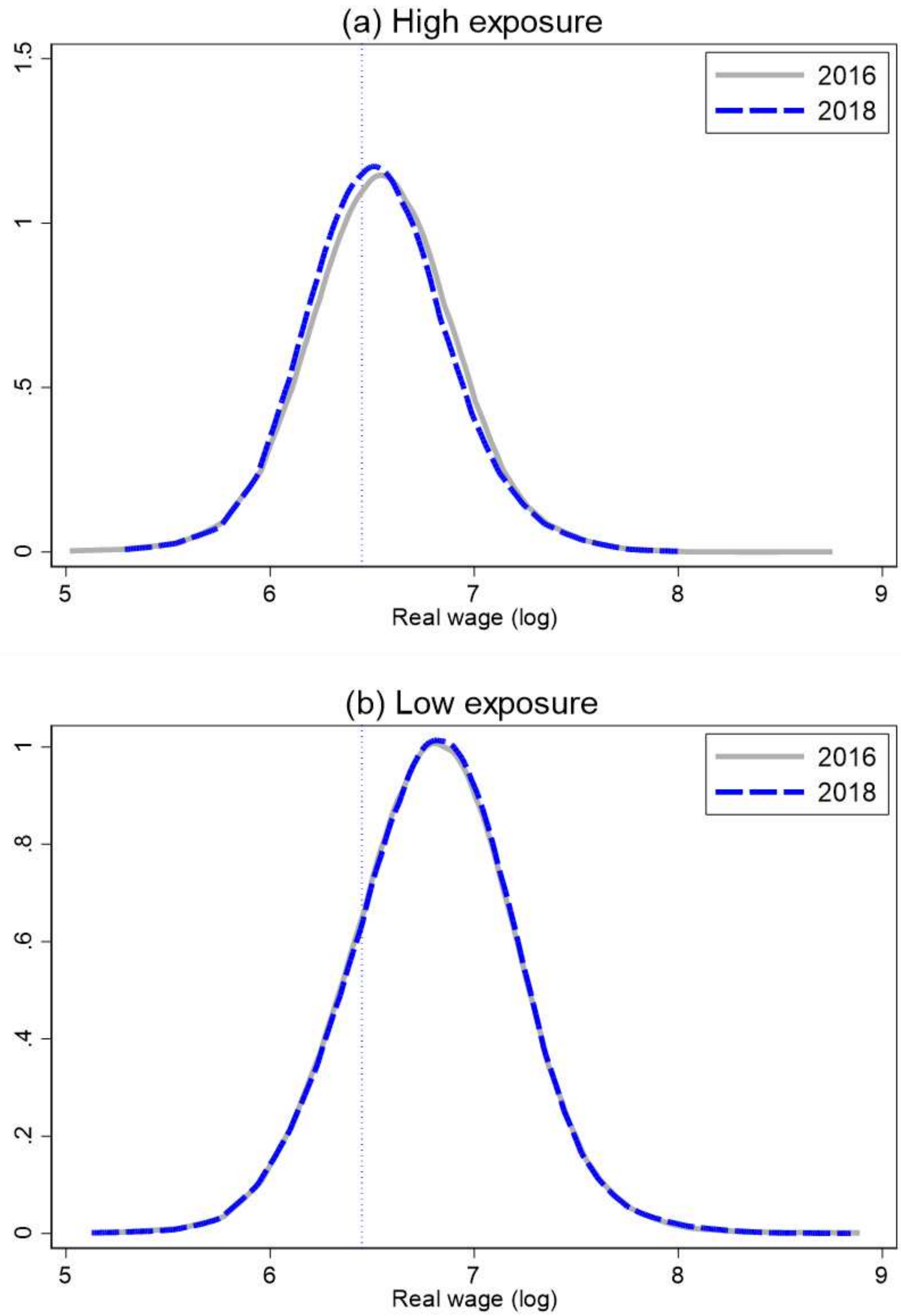
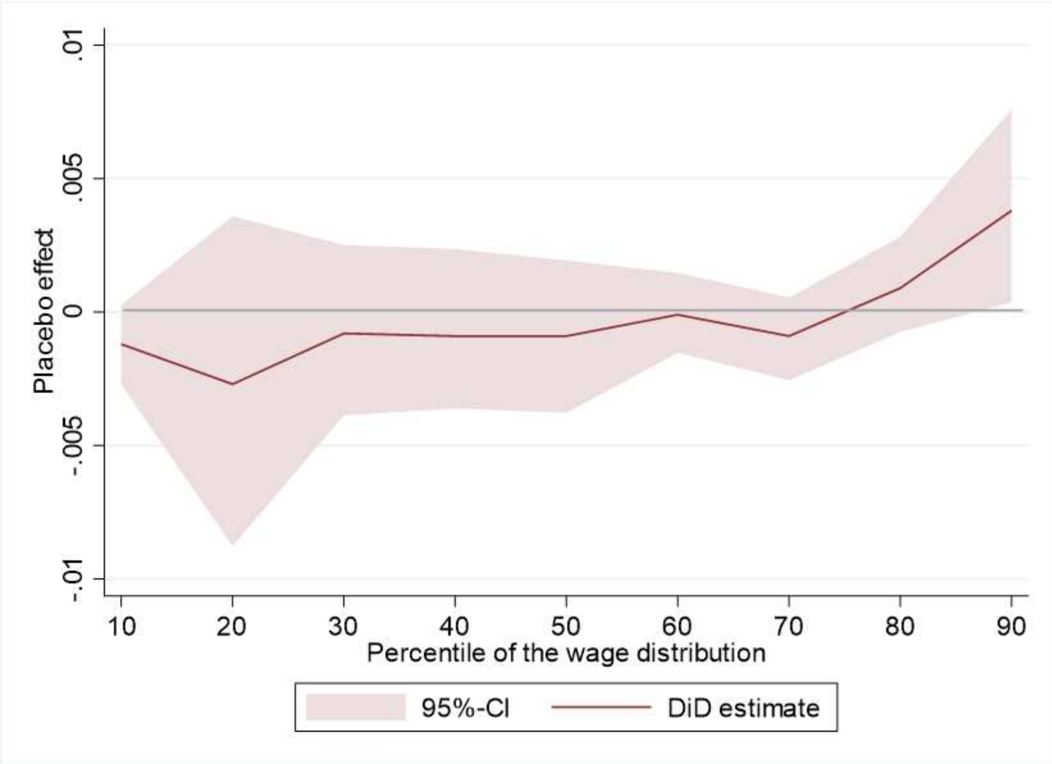


Figure A1. Kernel density estimates of real monthly wage in region-industry cells above and below the top tercile of minimum wage "bite" in 2016 and 2018. The vertical line indicates the level of the real minimum wage in 2019. The estimates are obtained using the weights provided by ELSTAT. Earnings deflated using the CPI from ELSTAT. Own elaborations on GR-LFS data.

a. Placebo effects for the pre-increase period, assuming that treatment takes place in 2018.



b. Placebo effects on the wage structure. Public servants only in the sample

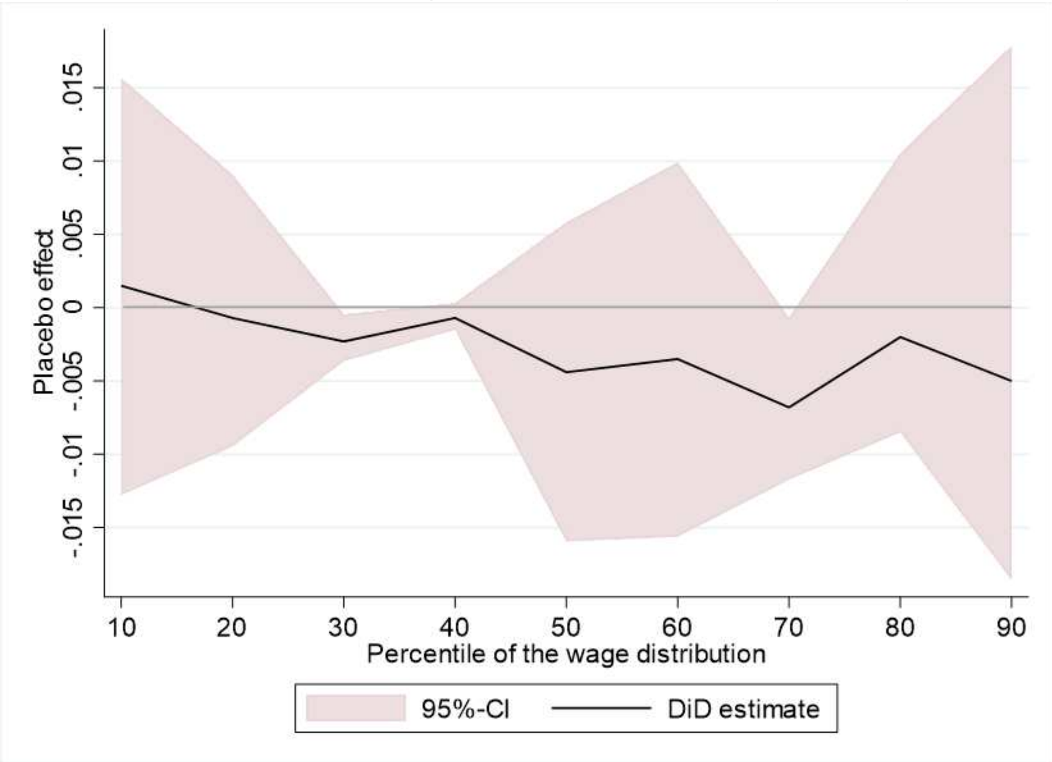


Figure A2 Robustness checks using the sample during the pre-treatment period (Panel a) and the sample of public servants (Panel b).