I Z A Institute
of Labor Economics
Initiated by Deutsche Post Foundation

DISCUSSION PAPER SERIES

IZA DP No. 16023

The Limitations of Overtime Limits to Reduce Long Working Hours: Evidence from the 2018-2021 Working Time Reform in Korea

Stephane Carcillo
Alexander Hijzen
Stefan Thewissen

MARCH 2023

I Z A Institute
of Labor Economics

Initiated by Deutsche Post Foundation

DISCUSSION PAPER SERIES

IZA DP No. 16023

The Limitations of Overtime Limits to Reduce Long Working Hours: Evidence from the 2018-2021 Working Time Reform in Korea

Stephane Carcillo
OECD, IZA and Sciences Po

Alexander Hijzen
OECD and IZA

Stefan Thewissen
CPB

MARCH 2023

ISSN: 2365-9793

IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

The Limitations of Overtime Limits to Reduce Long Working Hours: Evidence from the 2018-2021 Working Time Reform in Korea*

This paper provides a first assessment of the causal impact of the 2018-2021 reform in Korea meant to combat its long working-hour culture. The reform consists of lowering the statutory limit on total weekly working hours from 68 to 52. We apply a difference-in-difference approach in which we take advantage of the stepwise implementation of the reform by firm size using individual-level data. We present three main findings. First, the introduction of the 52-hour limit reduced but far from eliminated the incidence of working more than 52 hours. Second, there is some evidence that the introduction led to a reallocation of working hours, with more employees shifting from working fulltime to working overtime within the new limit (41-52 hours). Third, and more tentatively, this reallocation more likely took place within firms to account for fewer overtime hours worked by their employees, rather than within households to compensate for any income effects. Overall, our results show that a lower statutory limit can help to lessen a long working-hour culture, but is an insufficient measure by itself to fully eradicate it.

JEL Classification: J16, J22, K31

Keywords: working time regulation, working hours, time use, labour legislation, overtime, reallocation

Corresponding author:
Alexander Hijzen
OECD
2, rue André-Pascal
75775 Paris Cedex 16
France

E-mail: alexander.hijzen@oecd.org

* This paper has benefited from financial support from the Korean Ministry of Employment and Labour, as part of project on working time. The Economically Active Population Survey dataset in this study is confidential. Access can be obtained from Statistics Korea. Do-files are available upon request from the authors. The authors have no competing interests to declare. The authors would like to thank the editor of this journal, Keith Bender, and two anonymous references for their comments and suggestions. The authors are also very grateful to Agnes Puymoyen for her help with the Korean Labour and Income Panel Survey (KLIPS). The views expressed in this paper are those of the authors and should be attributed to the OECD or its member states.

1. Introduction

Very long working hours can reduce worker wellbeing. Working long hours for an extended period increases stress and fatigue while reducing the time available for recovery. This can increase the risk of work accidents and lower worker health and hourly productivity. Furthermore, a long working-hour culture can render work less attractive and less sustainable by increasing the risk that people are forced to leave the labour force prematurely (Bassanini and Caroli, 2015[1]; Saint-Martin, Inanc and Prinz, 2018[2]; Dolton, 2017[3]; Pencavel, 2014[4]).

Korea is notorious for its persistent long working-hour culture. In 2017, before implementation of the working time reform analysed in this paper, almost one in five Korean employees worked 55 or more hours per week. This is more than double the on average across OECD countries (Hijzen and Thewissen, 2020[5]). In an effort to change this, the Korean government adopted a major working time reform that lowered the statutory limit on total weekly working hours – the sum of normal and overtime hours – from 68 to 52. The new 52-hour limit has been implemented in a stepwise manner during 2018-2021. The new limit became first applicable to large firms and was gradually extended to smaller firms.

This paper provides a first assessment of the implementation of the 52-hour limit on working hours. We take advantage of the stepwise implementation of the reform by firm size, which provides a quasi-experimental setup. The reduction of the statutory limit on total weekly working hours from 68 to 52 became binding as of July 2018 only for large firms (300 or more employees). We identify the impact of the reform using a difference-in-difference strategy that compares the change in hours worked before and after the reform among workers in large firms compared with those in smaller firms (100-299 employees) who were not initially affected by the reform. We make use of the Economically Active Population Survey (EAPS), the Korean monthly labour force survey, which contains information on reported actual hours worked in the main job over the past week.

We present three main findings.

First, we find that the introduction of the 52-hour limit reduced but did not eradicate the incidence of working more than 52 hours. We find an estimated reduction in the incidence of working such long hours of about 1.3 percentage points, or 10% of its pre-reform level. This means that many employees work in firms that still do not fully comply with the new regulation.

Second, there is some evidence that the reform led to a reallocation of working hours, with a strong shift from working fulltime to working overtime within the new legal limit. The incidence of working overtime within the new limit (41-52 hours) went up by about 4.7 percentage points, or about 12% of its pre-reform level. Instead, the reform reduced the incidence of working fulltime (40 hours) by 3.4 percentage points, a decrease of 8% of its pre-reform level. We do not find effects on the probability of working fewer than 40 hours. These findings indicate that employees who initially did not work overtime started doing

so as a result of the reform. Because of this reallocation, we find no or limited effects of the reform on the average number of working hours per worker.

Third, we report suggestive evidence that the reallocation of working hours does not the result from a reallocation of work within households, to compensate for income losses due to fewer overtime hours (labour supply effects) but rather from a reallocation across workers within firms to compensate for the reduction in very long hours by some employees (labour demand effects). We do not find stronger increases in working hours among second earners or spouses. Instead, more experienced workers with longer tenure more often shifted from working fulltime to working overtime within the legal limit. This is suggestive that the reallocation more likely took place across workers within firms.

Our paper contributes to the literature on working time reforms in two important aspects.

- First, to the best of our knowledge, this paper is the first evaluating a reduction in the statutory limit of *total* weekly working hours (the sum of normal and overtime hours). Statutory limits on total working hours at first sight may seem an adequate policy instrument to reduce the incidence of very long working hours. Most previous policy attention however has focused on reducing statutory limits on normal working hours. While such reforms may be driven by a combination of policy objectives (e.g. work-sharing/employment), this does not prevent workers to work very long hours and may even incite them to do so in the presence of overtime premiums –. The incidence of very long working hours is of particular policy relevance given that such long hours are most likely to entail adverse health, productivity and wellbeing effects (Ahn, 2016[6]; Lee and Lee, 2016[7]; Park and Park, 2019[8]).[1] We show evidence for Korea that overtime limits reduce but do not eradicate very long hours, and can increase the share of workers working overtime within legal limits.
- Second, our paper provides evidence on the effects of working time reforms on the reallocation of working hours in the workforce and, more tentatively, its underlying mechanisms, i.e. whether reallocation likely took place across workers within firms (labour demand effects) or within households (labour supply effects).[2] There is a large literature on preferences and bargaining power that define the distribution of a household's time (Becker, 1981[9]). Empirical applications generally examine how changes in bargaining power, usually proxied by changes in wage rates, alter

[1] Unfortunately, our evaluation is restricted to effects on working hours as lags in data availability prevent us from analysing such broader effects.

[2] The existing literature, instead, focuses heavily on employment effects. Studies generally do not find evidence that lower statutory working time limits boost employment or lower unemployment ("work sharing") (see amongst others (Park and Park, 2019[8]) for Korea, (Andrews et al., 2015[30]; Hunt, 1999[23]) for Germany, (Zveglich and Van der Meulen Rodgers, 2003[27]) for Taiwan, (Kawaguchi, Naito and Yokoyama, 2017[28]) for Japan, (Sánchez, 2013[29]) for Chile, (Crépon and Kramarz, 2002[15]; Estevão and Sá, 2008[24]) for France and (Skuterud, 2007[25]) for Canada). Employment may not rise because unemployed workers are not good substitutes for individuals working overtime (Oaxaca, 2014[31]).

the allocation of a household's time (Guner, Kaygusuz and Ventura, 2012[10]; Bick and Fuchs-Schündeln, 2018[11]). Our study adds to the few studies that exploit exogenous shocks to a spouse's working hours following changes in legislation. Goux et al. (2014[12]) study the impact of the French workweek reduction that was implemented from 1998-2002 on the labour supply of spouses. They find little effect on the labour supply of women to the reduction in working time by their husband, possibly due to inflexibilities in the organisation of the workweek. Kawaguchi et al. (2013[13]) examine workweek reductions in Japan from 1994-1997 and in Korea from 2004-2011. The authors find that spouses' non-market time does not change as a result of reduction in working hours by the husband. Our findings are in line with these studies.

From a policy perspective, our evaluation is particularly relevant since it assesses the first step of a multi-year reform process. This paper provides early insights on the effectiveness of the reform to the Korean government and the possible need to make adjustments to the reform or introduce complementary measures. Such information is also helpful for governments in other countries that are currently considering to lower statutory working hour limits in an effort to reduce long working hours.[3] In particular, while we find that the 52-hour limit has significantly reduced the incidence of very long working hours, they remain prevalent even among firms that have to abide. Therefore, our results show that a lower statutory limit can help to lessen a long working-hour culture, but is an insufficient measure by itself to eradicate it and complementary measures are needed to make further progress.

The remainder of this paper is structured as follows. Section 2 provides the institutional background of the working time reform. Section 3 describes the data and empirical strategy employed in this paper and provides descriptive statistics. Section 4 contains the empirical results. Finally, we summarise our main findings and conclude in Section 5.

2. Institutional background

Following campaign promises of President Moon Jae-in, elected in May 2017, the Korean government committed to reduce the incidence of very long working hours in an effort to improve worker health, productivity and wellbeing. The government reached a tentative agreement on the 24th of November 2017 and passed a bill on the 28th of February 2018 (Figure 1).

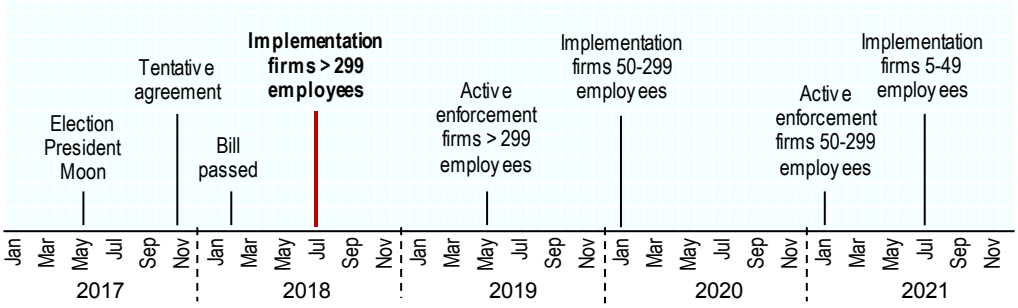
The principal element of the working time reform was to lower the statutory limit of total weekly working hours (the sum of normal and overtime hours) from 68 to 52 in the Korean Labour Standards Act. The lower total working hours limit consisted of a reduction of the statutory limit on weekly overtime hours (when an overtime premium is due and overtime

[3] For instance, such debates are currently ongoing in Austria, Chile, France, Japan, Spain and the United Kingdom (Hijzen and Thewissen, 2020[5]).

conditions are applicable) from 28 to 12. The limit on normal weekly working hours was kept at 40.

The government decided to implement the 52-hour limit in a staggered fashion by firm size between 2018-2021 in order to give smaller firms more time to adjust (Figure 1). As of July 2018, the new legislative maximum applied to firms with 300 or more employees, with active enforcement as of March 2019. The reform was extended to firms with 50 to 299 employees in January 2020, with active enforcement postponed until the end of that year and to extended to firms with 5 to 49 employees in July 2021. Firms with fewer than five employees remain exempt. The 52-hour working limit brings Korea's working time limit in line with prevailing practice of other OECD countries (Hijzen and Thewissen, 2020[5]).

Figure 1. Overview of the legislative process of the 52-hour working limit



The working-time reform contained two other measures aiming at reducing very long working hours (Hijzen and Thewissen, 2020[5]). For the purpose of this paper, it is important to note that these two measures were implemented either across all firm sizes or in a staggered fashion by firm size but with a different timeline. First, the number of sectors exempt from working hour limits was reduced from 26 to five as of July 2018 across all firm sizes.[4] Second, the number of paid leave days increased between 2020 and 2022, starting in January 2020 for firms with 300 or more employees.[5] Other working-time elements, such as the level of the overtime premium and possibilities to average working hours over a reference period, were not reformed during the period of our analysis.[6]

[4] As of July 2018, wholesale and retail, hotel and restaurant services, finance, broadcasting and social service now have to abide by the maximum overtime limit. Exemptions still apply to certain types of transportation services and healthcare. Because of the coarse sectoral coding in the dataset we use (EAPS), it is not possible to evaluate the effect of this part of the reform.

[5] Firms are obliged to provide paid leave during the public holidays (about 15 per year) to increase the number of paid leave days. This reform has a staggered implementation by firm size between 2020 and 2022, starting with firms with 300 or more employees as of January 2020. Furthermore, the government also substantially increased the minimum wage 16.4% in January 2018 and 10.9% in January 2019. The minimum wage applies to all employees across all firm sizes.

[6] The statutory overtime premium in Korea is 50%. Korea has two hours-averaging systems for employers to stay within working time limits and/or reduce overtime payments. The first system allows employers to average normal working hours over a period of three months. This system requires a written agreement of an employee representative. Normal working hours cannot exceed

The 52-hour limit reform builds on a previous reform implemented between 2004 and 2011, when the statutory limit on normal weekly working hours was reduced from 44 to 40. This reform was also implemented in a staggered fashion by firm size.

3. Methods

3.1 Data

This evaluation makes use of the Economically Active Population Survey (EAPS), which is the Korean cross-sectional monthly labour force survey. The EAPS contains information on actual hours worked in the main job last week by firm size in six categories. The EAPS allows comparing working hours in firms with 300 or more employees to those in firms with 100-299 employees – our treatment and control group, respectively (as further explained in Section 3.2). The EAPS has multiple advantages: (1) it contains the crucial data on working hours by firm size; (2) it has recent information; (3) data are monthly, allowing us to compare working hours just before and after the implementation of the reform; and (4) it is the source used by Korean authorities to map labour market trends and is therefore of high quality.[7] However, the database also has disadvantages for our purposes. As it is designed as a typical labour force survey, it does not have information on monthly wages, it does not have a household or firm identifier, and it does not have a panel dimension.

For our main specification, we select as our sample all employees on a permanent contract aged 18 and older employed in a firm with 100 or more employees (to be part of the treatment and control group) who have to abide by working time legislation.[8]

For our main specification, we examine the period between nine months before implementation of the reform (October 2017) until the last month for which we have micro data (November 2019). The end of our observation period just precedes the extension of the reform to firms with 50 to 299 employees.[9]

3.2 Empirical strategy

Our evaluation takes advantage of the stepwise implementation of the reform by firm size. The reduction of the statutory limit on total weekly working hours from 68 to 52 became binding as of July 2018 only for firms with 300 or more employees. The identification of the causal impact of the reform on actual working hours is based on a difference-in-difference design, which compares the change in the probability to work different sets of

12 per day and 52 per week. The second system consists of a shorter reference period (two weeks) and a lower maximum of 48 normal working hours per week, but does not require a written agreement with a labour representative.

[7] With our micro data, we are able to replicate fully the macro figures on the number of employed persons by hours worked published online by the Korean authorities.

[8] The following groups of workers do not have to abide by working time legislation: self-employed and unpaid family workers; workers in exempt occupations (agriculture, fishing, managerial and supervisory occupations); and workers in exempt sectors because of legislation prior to July 2018 (wholesale and retail, hotel and restaurant services, finance, broadcasting and social service, transportation services and healthcare).

[9] Moreover, by restricting the analysis up to November 2019, our analysis is not affected by the economic consequences of the COVID-19 pandemic that hit Korea early 2020.

hours following the reform among workers in *large firms*, i.e. with 300 or more employees (the treatment group) compared with those in *smaller firms, i.e.* with 100-299 employees (the control group). Studies that evaluate the effects of the 2004-2011 working time reform in Korea, which was also implemented in a staggered fashion, rely on a similar difference-in-difference design (Ahn, 2016[6]; Lee and Lee, 2016[7]; Park and Park, 2019[8]).

For our main analysis, we assess the impact of the reform on the incidence in employment of four working hour bands the reform: (1) working 1-39 hours (part-time); (2) working 40 hours (fulltime); (3) working 41-52 hours (overtime but within the new legal limit); and (4) working more than 52 hours (overtime above the new legal limit).

We perform the estimations using a seemingly unrelated regression (SUR) system to account for the fact that the disturbances are correlated across equations.[10] For this purpose, we have to exclude one working hour group; otherwise, the dependent variables are perfectly linear and cannot be estimated. We exclude the group working 1-39 hours. As we show in later sections, the probability of working 1-39 hours is essentially constant before and after the reform in both control and treatment group. The SUR system takes the following form:

$$\left\{\begin{array}{ll} (1) & 40\ hours_{it} = \alpha_0 + \beta_1 reform_t + \beta_2 treatment_{it} + \beta_3 reform_t \cdot treatment_{it} + \beta_4 X_{it} + \pi_t + \epsilon_{it} \\ (2) & 41 to\ 52\ hours_{it} = \gamma_0 + \delta_1 reform_t + \delta_2 treatment_{it} + \delta_3 reform_t \cdot treatment_{it} + \delta_4 X_{it} + \pi_t + \sigma_{it} \\ (3) & >52\ hours_{it} = \theta_0 + \vartheta_1 reform_t + \vartheta_2 treatment_{it} + \vartheta_3 reform_t \cdot treatment_{it} + \vartheta_4 X_{it} + \pi_t + \tau_{it} \end{array}\right\}$$

The dependent variables are three dummies indicating whether the individual worked (1) exactly 40 hours; (2) between 41 and 52 hours; or (3) more than 52 hours. Hours are defined as actual hours in her or his main job. We present results for average hours worked across all workers (as a continuous variable).[11] We do not discuss average hours in detail, since we do not find strong evidence of an effect of the reform.

The $reform_t$ dummy indicates the implementation of the 52-hour limit (equal to 1 from July 2018, and 0 before). It absorbs variations in working time common across individuals after the implementation of the 52-hour limit relative to the period before implementation. The $treatment_{it}$ dummy identifies the treatment and control groups which is set to 1 for workers in large firms and 0 for workers in small firms. This dummy captures time-invariant characteristics of individuals working in small and large firms.

The main coefficients of interest are those associated with the interaction terms $reform_t \cdot treatment_{it}$ which capture the average effects of the implementation of the 52-hour limit on the treatment group.

The vector X_{it} includes a set of socio-demographic control variables (gender, age by four categories, education by three categories, relation to household head by five categories, living in a rural or urban area, reasons why working less than 36 hours by three categories, tenure in months and marital status) and a full set of 1-digit industry and occupation

[10] A SUR system allows for a straightforward interpretation of the interaction term in a difference-in-difference model (Goodman-Bacon, 2021[32]; Karaca-Mandic, Norton and Dowd, 2012[26]). Linear probability models with robust standard errors for the different dependent variables yields the same coefficient sizes and significance levels, as reported in Table 2. A logit model to account for the dummy dependent variables also yields comparable results (results in Table A4).

[11] Average hours are sensitive to extreme reporting values. We therefore apply top- and bottom coding (at the 1% and 99%, which refer to 24 hours and 70 hours per week respectively).

dummies. The vector π_t consists of month-year dummies that flexibly control for common time shocks across all individuals.[12]

A causal interpretation of our findings rests on two identifying assumptions.

Our first identifying assumption is that, absent the reform, working hours would have evolved in the same manner for individuals working in smaller and larger firms, controlling for socio-demographic variables (conditional independence assumption). The presence of common trends in working hours across the two groups before the reform suggest this is the case, as we will show in Section 3.4. In addition, no other reform or shock after the implementation of the reform in July 2018 is susceptible to have a differential impact on working hours in the treatment and control groups.

The second identifying assumption is that working hours in the control group (smaller firms who were excluded from the reform) are not influenced by the induced change in working hours in the treatment group (larger firms) (stable unit treatment value assumption). Such an influence could firstly stem from product markets interactions, if larger firms outsource some of their production to smaller firms to meet the new working time limit. Given the relatively short timeframe of our analysis and the large number of companies operating in many sectors, this is unlikely to happen at a scale that could influence our results. There could also be an influence through labour market interactions. The reform may induce strategic employment behaviour among workers and firms across firm sizes if workers with certain working time preferences sort into different firm size categories (endogenous sorting). Firms may also strategically reduce employment to avoid compliance with the new working limit. Such strategic employment behaviour by workers and firms is further discussed in Section 4.2.3. In addition, smaller firms in competition with larger firms could start offering better working times conditions after the reform to attract workers. If this were the case, then our estimates of the impact of the reform will be biased downward.

3.3 Composition of the treatment and control group

The composition of workers in the treatment and control group is quite comparable before the reform for most observable characteristics (first and second columns of Table 1). In our sample , workers are more likely to be male, prime-aged, higher educated, household head, not married, and living in an urban area. Moreover, the composition of the treatment and control groups is not perfectly identical. Individuals in the treatment group (who work in larger firms) are more likely to be males, better educated, household head and living in an urban area than those belonging to the control group (who work in smaller firms). For this reason, we reweight the two groups using entropy balancing, a multivariate reweighting method described in Hainmueller (2012[14]). This allows reweighting the control group such that the distributions of the covariates are comparable with those of the treatment group before the reform.[13] We use this technique to balance the means of the four variables with the largest pre-reform differences: gender, education, rural vs urban status, and relation to household head. We leave aside marital status to the extent that this variable is correlated with the relation to household head. After the procedure is performed, the composition of the two groups is very similar along all observable characteristics considered as seen in the last column of Table 1. However, in the main specification we control for any remaining

[12] The last month-year dummy is left out to estimate the implementation period dummy.

[13] We implement this procedure with the Stata package *ebalance*.

differences in individual characteristics. As we show in the appendix and discuss in the text, our results also hold without applying entropy weights.

Table 1. Comparing the composition of the treatment and control group before the reform

without and with entropy weights

		(i) Treatment	(ii) Control group Unweighted	(iii) P-value	(iv) Control group weighted	(v) P-value
Gender	Females	0.17	0.30	0.0000	0.16	0.0064
Age group	21_24	0.03	0.05	0.0000	0.03	0.1546
	25_54	0.87	0.85	0.0000	0.88	0.1371
	55_64	0.09	0.10	0.0822	0.09	0.3955
Education	Below_sec	0.02	0.06	0.0000	0.02	0.7778
	Upper_sec	0.36	0.42	0.0000	0.37	0.0188
	Tertiary	0.62	0.53	0.0000	0.61	0.0251
Relation to household head	Hh_Head	0.77	0.64	0.0000	0.79	0.0015
	Spouse	0.09	0.14	0.0000	0.08	0.0030
	Child	0.12	0.20	0.0000	0.12	0.2349
	Other	0.01	0.02	0.0000	0.01	0.4173
Area	Rural	0.13	0.21	0.0000	0.12	0.0178
Marital status	Married	0.23	0.32	0.0000	0.24	0.0641
Number of observations		10,958	8,881		8,881	

Note: Treatment group: large firms with 300 or more employees. Control group: smaller firms with 100-299 employees. 15-64 population. The data cover the pre-reform period (October 2017 – June 2018).
Source: EAPS.

3.4 Descriptive evidence

The principal assumption for a causal interpretation of our evaluation is that the control group of smaller firms with 100-299 employees provides the appropriate counterfactual of the trend that the treatment group of large firms with 300 or more employees would have followed if they had not been treated (conditional independence assumption) and that the effect of the reform did not affect the control group (stable unit treatment value assumption).

The parallel trends assumption appears to be satisfied for all four different hour groups during the pre-reform period (Figure 2). The incidence of working different sets of hours (1-39, 40, 41-52 or more than 52 hours) followed comparable trends for the treatment and control groups before the implementation of the reform.[14] Average hours worked as a continuous variable also followed comparable trends for the treatment and control groups before the implementation of the reform (Figure A1 in the Appendix).After the reform,

[14] Data are seasonally adjusted by regressing the probability to work a particular hour group on a set of month dummies, annual leave patterns and election dummies, and expressing the data per quarter. In Korea, elections are days off. Two elections took place in the weeks when labour force survey data were gathered during the time sample that we consider.

patterns for treatment and control are also comparable.[15] The parallel trends look very similar even without applying entropy balance weights (Figure A2 in the Appendix).

At the time of the reform, we observe a stable pattern for part-time workers but some changes for the other working time groups. The incidence of working 1-39 hours is very stable over time and hovers around 8% pre- and post-treatment in both the treatment and control group (Figure 2 Panel A). This further increases the case for using this group as the reference in the seemingly unrelated (SUR) difference-in-difference regressions. Changes in incidence of working 40 hours, 41-52 hours or more than 52 hours per week are concentrated around the implementation of the reform (Figure 2 Panels B-D). This is mainly driven by the treatment group for the incidence of working more than 52 hours in Panel D, consistent with the stable unit treatment assumption. However, the change in the incidence of working 40 hours or between 40-52 hours at the time of the reform is mainly driven by the control group (Panel B and C). Further inspection shows that trends in the treatment group stand out, however. Trends in other firm sizes closely follow those of the control group, providing further evidence that the reform only affected the treatment group (Figure A5 in the Appendix).

We do not have a good explanation for the reduction in the incidence of working time 41-52 and the increase in that working exactly 40 hours at the time of the reform in the control group (as well as other firm size groups). We can confirm, however, that this is not some sort of artefact of the data. The same pattern is observed in the Korean Labour and Income Panel Survey (KLIPS).[16] Also, we are not aware of any other reforms that coincided with the reform analysed in this paper that could explain these patterns. While the minimum wage was increased significantly in the same year this happened in January rather than in July.[17] Finally, we do not find any evidence of labour demand shocks that can explain the reduced demand for overtime at the time of the reform. Vacancies by firm size do not

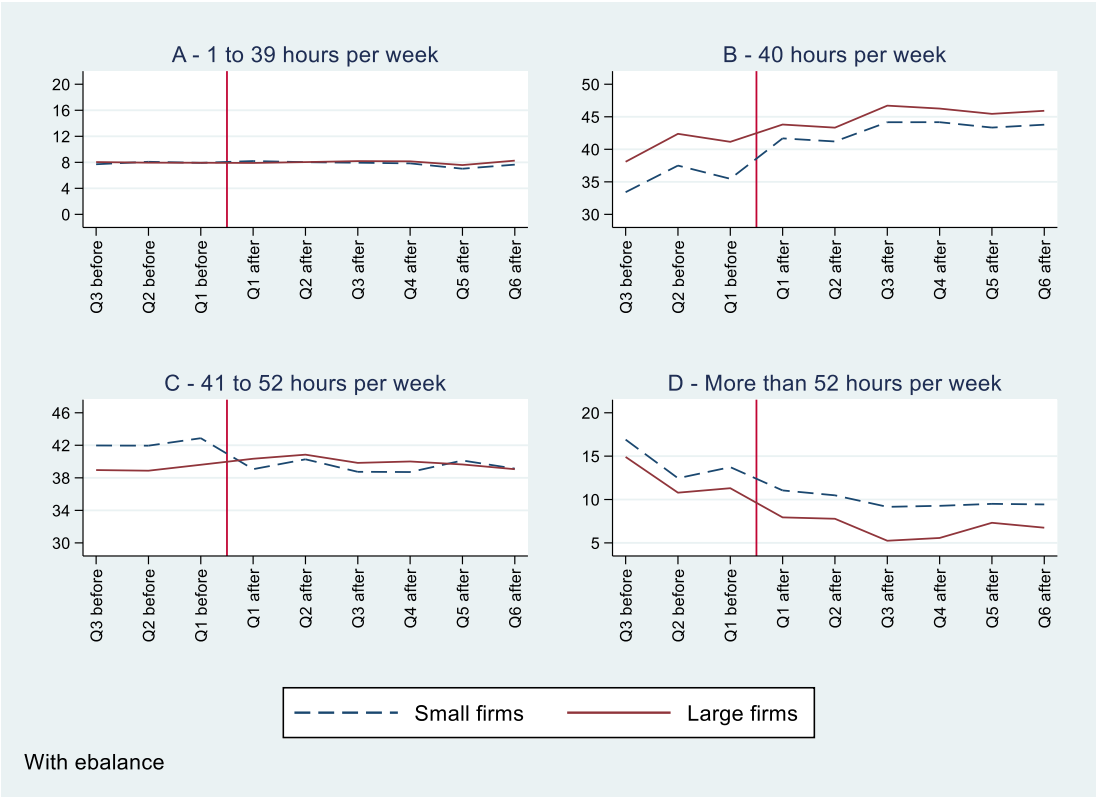
[16] We cannot not use the KLIPS data for our main analysis, since they do not provide information of the extent overtime, i.e. whether overtime is between 40 and 52 hours or 52 and more.

[16] We cannot not use the KLIPS data for our main analysis, since they do not provide information of the extent overtime, i.e. whether overtime is between 40 and 52 hours or 52 and more.

[17] Moreover, the incidence of minimum wage workers is strongly concentrated in small firms with less than 100 employees and among part-time workers. Among firms with more than 100 employees the incidence of minimum wage workers is twice as large among part-time workers than among workers working 40 hours or more. The modest incidence of minimum wage workers among firms with more than 100 employees and its strong concentration among part-time workers further suggest that the increase in the minimum wage in January 2018 is unlikely to explain the reduction in the incidence of working between 41-52 hours and the increase in the incidence of working exactly 40 hours.

change in any significant way at the time of the reform. Consequently, our results with respect to the reallocation of working time should be interpreted with some caution.

Figure 2. Parallel trends in incidence of working particular weekly hours for treatment and control group



Note: Data are seasonally adjusted and weighted by entropy balance weights. The red line represents the implementation date of the reform.
Source: EAPS.

4. Results

4.1 Main results

4.1.1 Average effects on the post-reform period

The implementation of the 52-hour limit reduced the incidence of working long hours and did so through a reallocation of working hours. The reform (1) *decreased* the probability to work more than 52 hours, (2) *increased* the probability to work between 41-52 hours, and (3) *decreased* the probability to work exactly 40 hours. Thus, the reform decreased the probability to work more than 52 hours as was envisaged but did not eradicate such long working hours. At the same time, it increased overtime work within the legal limit (41-52 hours) by persons previously working exactly 40 hours (Table 2). If anything, the reform

had a small positive effect on average working hours (as a continuous variable), which went up by about 15 minutes, significant at the 10% significance level.[18]

The implementation of the 52-hour limit lowered the probability to work more than 52 hours among employees in large firms by about 1.2 percentage points. This implies that the reform decreased the incidence of working more than 52 hours among this group from a level of 13% in the pre-reform period by 10%.[19] Thus, while the reform reduced the incidence of very long working hours, it remains prevalent, pointing at widespread non-compliance. Evaluations of the Korean working time reform of 2004-2011 Korean working time reform also revealed compliance problems (Park and Park, 2019[8]; Kawaguchi, Lee and Hamermesh, 2013[13]; Ahn, 2016[6]; Lee and Lee, 2016[7]). Compliance issues have also been highlighted for working time reforms in other countries, including in France and Sweden (Crépon and Kramarz, 2002[15]; Skans, 2004[16]). We reflect further on this finding in Section 5. The reform is further associated with an increase of the probability to work overtime within the new legal limit (41-52 hours) by about 4.3 percentage points, which is an increase of 11% from a pre-reform level of 39%. The reform is associated with a reduction in the probability to work exactly 40 hours by 3.1 percentage points, or a decrease of 8% from a pre-reform level of 40%.[20]

Our regression results, shown in Table A1 in the Appendix, indicate that prime-age, lower and middle-educated males work on average more often long hours, as also reported elsewhere in the literature (Hamermesh, Kawaguchi and Lee, 2017[17]; Blundell, Bozio and Laroque, 2013[18]).[21] Significance levels and treatment effect size remain very comparable without applying entropy balance weights (Table A2 in the Appendix). Significance levels and treatment effect size also remain similar in empirical specifications without control variables, sector and occupation dummies, and/or month times year dummies (available upon request).

Our main estimation is based on a pre-reform period of three quarters. Extending this period to one full year does not affect the main results, although extending it beyond one year diminishes both the size and significance of the estimated treatment effect on the probability to work more than 52 hours, while leaving the effects on the probability to work 41-52 hours and 40 hours, respectively, broadly unchanged (Table A3 in the Appendix). However, using more than a year before the reform makes it more difficult to balance the treatment and control groups in the pre-reform period.

---

[18] Results from a linear probability model suggest a treatment effect of 0.25 (p = 0.067) (results available upon request).

[19] The mean values of the dependent variables are presented in Table A5 in the Appendix.

[20] An OLS model for the group working 1-39 hours shows an insignificant treatment effect, confirming that this group was not affected by the reform (-0.000 with p = 0.93 with entropy balance weights, and -0.000 with p = 0.77 without entropy balance weights).

[21] The coefficient for the reform dummy indicates that the probability to work more than 52 hours decreased on average across treatment and control groups by almost 8 percentage points since the implementation of the 52-hour limit, conditional on the other independent variables. The probability to work 41-52 hours decreased by 4 percentage points whereas the probability to work 40 hours increased by 11 percentage points. The results correspond to the decreasing trend in very long working hours towards working fulltime shown earlier in Section 3.4 (Hijzen and Thewissen, 2020[5]). It highlights the importance of our difference-in-differences approach to control for common trends in the treatment and control groups.

Table 2. The 52-hour limit led to a reallocation of working hours

	(1) Working more than 52 hours	(2) Working 41-52 hours	(3) Working 40 hours
Reform * large firm dummy (treatment effect)	-0.012**	0.043***	-0.031***
	(0.005)	(0.008)	(0.008)
Reform dummy	-0.075***	-0.030**	0.101***
	(0.009)	(0.015)	(0.015)
Large firm dummy (treatment group)	-0.008**	-0.032***	0.043***
	(0.004)	(0.007)	(0.007)
Control variables	Yes	Yes	Yes
Sector & occupation dummies	Yes	Yes	Yes
Month * year dummies	Yes	Yes	Yes
Number of observations	56072	56072	56072
Adjusted R^2	0.045	0.058	0.089

Note: The sample consists of employees in firms with 100 or more employees. Regression results of a seemingly unrelated difference-in-difference model with entropy balance weights covering October 2017-November 2019. Control variables include the following individual characteristics: gender, age, education, relation to household head, living in a rural vs urban area, marital status, and tenure. Detailed results are presented in Table A1 in the Appendix. Standard errors in parenthesis. *, **, *** indicate statistical significance at 10%, 5%, and 1% levels, respectively.

Source: EAPS.

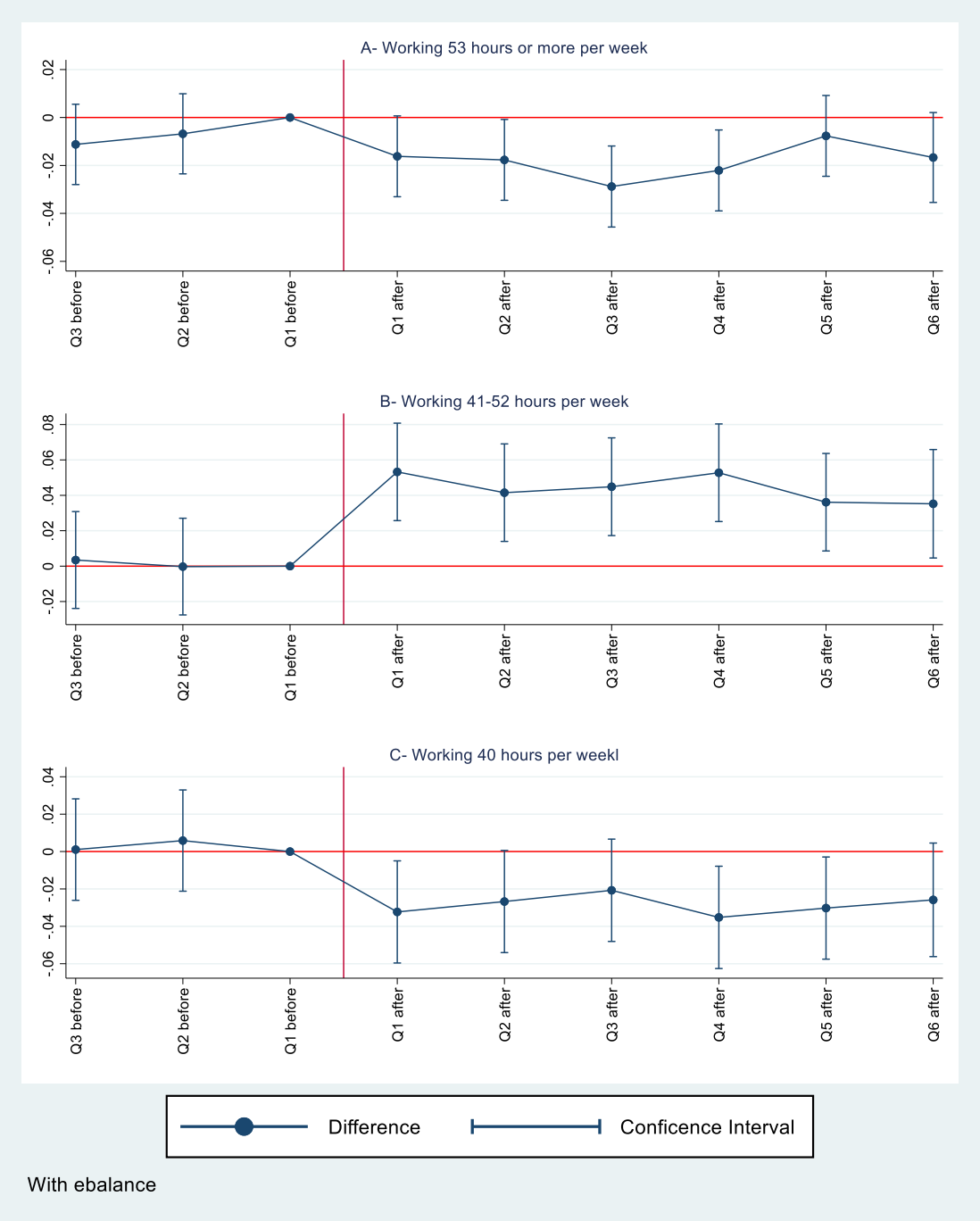
4.1.2 Dynamic effects

Figure 3 plots the dynamic effects of the 52-hour limit on the probabilities to work 40 (Panel A), 41-52 (Panel B) or more than 52 working hours (Panel C) at each quarter after the reform. This dynamic regression consists of a set of dummy variables for each quarter of the pre- and post-period, which capture the effect of the difference between the treatment and control groups conditional on controls relative to the quarter just before the reform was implemented (the quarter for which the quarter dummy is omitted).

For all dependent variables, the difference-in-difference coefficients become statistically significant at the time of the reform. The coefficients remain statistically significant throughout virtually all of the post-reform period. The estimated effects are fairly stable, about 1-2 percentage points for working more than 52 hours, about 5 percentage points for working 41-52 hours, and about 3-4 percentage points for working 40 hours, and comparable to the average effect reported in our main results (Table 2). The difference-in-difference coefficients for the pre-reform period are very small and not statistically different from zero. This suggests that the estimated effects of the reform are unlikely to be driven by differential pre-reform trends between the control and treatment groups. In other words, the common trends assumption underlying the difference-in-difference estimator seems appropriate in the present context. Results are again similar without applying entropy balance weights (Figure A3 in the Appendix). We do not find significant dynamic effects before and after the reform on average working hours (as a continuous variable) (Figure A4 in the Appendix).

Figure 3. The reform had a direct effect on the reallocation of working hours

Difference-in-difference in the incidence of working different working hours between large and smaller firms, relative to the quarter before the reform (April-June 2018), percentage points.



Note: The sample consists of employees in firms with 100 or more employees. Regression results of a seemingly unrelated difference-in-difference model with entropy balance weights covering October 2017-November 2019. The estimation is based on the specification of Table 2. Vertical bands indicate the 90% confidence intervals of each point estimate. The red line represents the implementation date of the reform.

Source: EAPS.

4.2 Sensitivity tests

The estimated effects of the 52-hour limit on the probability to work different sets of hours are robust to a wide set of sensitivity tests (Table 3). For convenience, the estimated effects of the reform from our baseline specification are reported as Model 1.[22]

4.2.1 Enlarging the sample

Our main estimations are based on a sample of employees who work on a permanent contract. Enlarging the sample by including employees on a temporary contract hardly affects the size of the coefficients (Model 2).

4.2.2 Different time specification

There are no signs of anticipation effects of the 52-hour limit among large firms following different stages of the legislative process of the reform (see Figure 2). Such anticipation effects would lead to an underestimation of the effect size of the reform. Including additional treatment effects for the tentative agreement of the 52-hour limit reached in November 2017 (Model 3), passing of the bill end February 2018 (Model 4) and the active enforcement date among large firms as of March 2019 (Model 5) does not affect the main estimations. We do not find an additional significant effect of any of these dates on the probability to work particular hours.

There may also be anticipation effects in the control group of small firms, as the 52-hour limit became applicable to this group as of January 2020. Such anticipation effects would again lead to an underestimated effect size of the reform for large firms. Restricting the period up to August 2019 rather than November 2019 does not change the results (Model 6). The absence of anticipation effects for large firms before the reform also reduces concerns about comparability of the control group.

4.2.3 Strategic employment behaviour and measurement error

The reform may induce strategic employment behaviour among workers and firms. Such behaviour could lead to endogenous sorting of workers and firms into treatment and control group, which could violate our identification strategy (stable unit treatment value assumption). Employees who prefer to work very long hours may move from large to smaller firms, whereas workers who prefer to not work very long hours may move to large firms. Employees may further circumvent limits on long working hours by taking a second job (Friesen, 2001[19]). Firms heavily relying on overtime may strategically reduce their firm size to avoid having to comply with the regulation.

We do not find indications for strategic employment behaviour. The neatly parallel trends in the probability to work more than 52 hours in treatment and control group before as well

[22] All results from the sensitivity analysis are similar without using entropy balance weights or using Logit estimation instead of OLS (see, respectively, Table A2 and Table A4 in the Appendix for the main specifications). Due to the well-known difficulty of interpreting interactions terms in Logit models, we use OLS for the baseline (Ai and Norton, 2003[33]).

as from one quarter after the reform onwards (Section 3.4) imply that strategic employment shifts should have taken place exactly in the month of the implementation of the reform. This seems highly unlikely given the time it takes to hire or start a new job, and the uncertainty in starting month. Moreover, restricting our sample to individuals with at least two years tenure in the company leads to virtually identical results (Model 7).[23] We also do not find evidence of circumventing behaviour by taking a second job: we find similar results when using working hours across all jobs rather than the main job (Model 8). We do not find an increase in the probability of having a second job.[24] Strategic reductions in firm size by large firms are unlikely as well.[25] First, any gains would only be temporary since smaller firms will have to comply to the 52-limit as of January 2020. Second, macro data on the Census of Establishments does not show any change in the trend in number of firms just below and above the firm size cut-off from 2017 to 2019.[26] Third, a previous study exploiting the same firm size threshold using data with more granular firm size information did not find evidence of endogenous sorting of firms during the 2004-2011 Korean reform (Park and Park, 2019[8]).[27]

Last, individuals may misclassify whether they work in a large or smaller firm. Such misclassification would lead to attenuation bias in the estimated effect of the reform. Redefining the control group to firms with 30 to 99 employees to lessen these concerns, at the cost of lower comparability with larger firms, almost doubles the effect size for the probability to work more than 52 hours (Model 9). This may suggest that the reported effect size in our preferred specification is a lower-bound estimate of the effect of the reform on the probability to work very long hours.

[23] Furthermore, if employees wanting to work long hours moved to smaller firms, we would expect an increase in the incidence of working more than 52 hours since the reform in this group. Instead, the share decreased. Section 4.3 includes a further discussion of the effects by tenure.

[24] Estimates using a linear probability model yield a coefficient of -0.001 with $p = 0.48$.

[25] The EAPS dataset does not have an indicator for number of firms, nor a more granular firm size variable which would allow us to exclude individuals working in firm sizes close to the cut-off that are most likely to strategically reduce their firm size.

[26] The number of firms with 300-499 workers was almost identical in 2017-2019 (2,093 in 2017, 2,092 in 2018 and 2,102 in 2019) according to the online macro data of the Census of Establishments (www.kosis.kr/eng). The number of firms with 100-299 persons shows a consistent trend upwards (14,758 in 2017, 14,907 in 2018 and 15,232 in 2019). This upward trend is also visible between 2012 and 2016. The data for 2018 partly cover the post-reform period, as firms are surveyed throughout the year.

[27] As an additional test for employment effects, we calculate the share of total employment in large and smaller firms at the 1-digit industry level. A difference-in-difference regression with 1-digit industry and year-month dummies weighted by industry employment size yields insignificant effects (results available upon request). These results are in line with the evidence base that lower statutory working time limits do not increase employment or lower unemployment ("work sharing") (Park and Park, 2019[8]; Andrews et al., 2015[30]; Hunt, 1999[23]; Zveglich and Van der Meulen Rodgers, 2003[27]; Kawaguchi, Naito and Yokoyama, 2017[28]; Sánchez, 2013[29]; Crépon and Kramarz, 2002[15]; Estevão and Sá, 2008[24]; Skuterud, 2007[25]). Employment may not rise because unemployed workers are not good substitutes for individuals working overtime (Oaxaca, 2014[31]).

4.2.4 Industry and occupation-specific shocks

Above it was argued that extending the time-window pre-reform is complicates balancing the treatment and control due to the role of group-specific shocks. We investigate the role of group-specific shocks by controlling for either industry-time dummies or occupation-time dummies (Models 10 and 11). This does not change the results in any apparent way. Also not controlling for the observable characteristics of workers does not change the results (Model 12). This provides further evidence that the use of entropy weights effectively balance treatment and control groups.

Table 3. The main results are robust to a set of sensitivity tests

Model		(1) Working more than 52 hours	(2) Working 41-52 hours	(3) Working 40 hours	Number of observations
(1) Main result from Table 2		-0.012**	0.043***	-0.031***	56072
A. Enlarging the sample					
(2)	Including temporary workers	-0.012**	0.039***	-0.027***	59058
B. Different time specification					
(3)	Including temporary agreement date	-0.014**	0.048***	-0.034***	56072
(4)	Including bill passing date	-0.016**	0.037***	-0.024**	56072
(5)	Including active enforcement date	-0.014**	0.045***	-0.028***	56072
(6)	Anticipation effects control group	-0.014**	0.047***	-0.032***	49772
C. Employment effects and measurement error					
(7)	At least two year tenure	-0.014**	0.059***	-0.047***	47326
(8)	Total hours worked (all jobs)	-0.012**	0.043***	-0.031***	56072
(9)	Control group 30-99 employees	-0.021***	0.056***	-0.035***	72196
D. Different control specifications					
(10)	Adding industry X time dummies	-0.010*	0.042***	-0.030***	56072
(11)	Adding occupation X time dummies	-0.013**	0.046***	-0.033***	56072
(12)	Without controls on individual characteristics	-0.012**	0.047***	-0.028***	56072

Note: Results show the treatment effect (the interaction of the reform and large firm dummy). Regression results of a seemingly unrelated difference-in-difference model with entropy balance weights covering October 2017-November 2019. *, **, *** indicate statistical significance at 10%, 5%, and 1% levels, respectively.
Source: EAPS.

4.3 Channels for the reallocation of working hours

An interesting question is whether the reallocation of working hours is the result of reallocation across workers within firms to account for fewer overtime hours worked among some workers (labour demand effects), or across workers within the household, when other family members start working more hours to compensate for income losses by the head (labour supply effects). With our data at hand without a household or firm component, we can only provide crude indications to examine this question.

We do not find indications for reallocation of working hours within households to compensate for fewer hours worked by the household head.[28] Splitting the sample by household heads and other family members shows comparable effect sizes (Table 4, Models 2 and 3). The probability to work more than 52 hours declines slightly more for other family members than household heads but the difference is not statistically significant. Consistently, we find a somewhat larger decline of very long hours for women than for men, but no significant difference for other hours worked[29]. (Table 4, Models 4 and 5). Our results are in line with an evaluation of the French 1998-2002 working time reform, which also did not find evidence that spouses increased their labour supply to compensate for fewer hours worked by household heads (Goux, Maurin and Petrongolo, 2014[12]). Kawaguchi et al. (2013[13]) find that spouses do not change their non-market time as a result of exogenous reductions in working hours by the husband induced by workweek reductions in Japan from 1994-1997 and in Korea from 2004-2011.

Since we do not find strong indications for reallocation within households, within-firm reallocation of working hours may be more plausible. Some evidence may further underscore this. Employees with shorter tenure decrease their very long working hours more, whereas those with longer tenure shift from working fulltime to working overtime within the legal limit (Table 4 Models 6 and 7). One explanation for this may be that firms demand their most experienced workers to work more hours within legal limits, to compensate for a decrease in working hours. The effects by tenure are not driven by an effect by age.[30]

[28] Triple-differences designs show comparable results. The triple difference itself is not significant. The estimations by household head exclude gender as a control variable and those by gender exclude family relation. Results are comparable when the full vector of control variables is used. The results by household head are very comparable without entropy balance weights or when only applying entropy balance weights by education (all results available upon request).

[29] The probability to work more than 52 hours remains insignificant for males without entropy balance weights or when only applying entropy balance weights by education. The triple difference by gender on the probably to work more than 52 hours is not significant, probably due to sample size (results available upon request).

[30] Triple-differences designs by tenure show comparable results. The triple differences for the probability to work more than 52 hours and 40 hours are significant. The estimations exclude age as a control variable. Results are comparable when the full vector of control variables is used. Instead, a triple difference by age is not significant. The results by tenure are very comparable without entropy balance weights (all results available upon request).

Table 4. Reallocation across different sub-samples

Difference-in-difference in the incidence of working different working hours between large and smaller firms, percentage points.

	(1) Working more than 52 hours	(2) Working 41-52 hours	(3) Working 40 hours	Number of observations
(1) *Full sample (Table 2)*	-0.012**	0.043***	-0.031***	56072
(2) Household heads	-0.012*	0.043***	-0.030***	40698
(3) Other family members	-0.018*	0.048***	-0.033**	15374
(4) Males	-0.010	0.043***	-0.032***	43466
(5) Females	-0.022**	0.046***	-0.029*	12606
(6) Less than 60 months tenure	-0.038***	0.047***	-0.005	18190
(7) More than 60 months tenure	-0.002	0.047***	-0.046***	37882

Note: The sample consists of employees in firms with 100 or more employees. Regression results of a seemingly unrelated difference-in-difference model with entropy balance covering October 2017-November 2019. *, **, *** indicate statistical significance at 10%, 5%, and 1% levels, respectively.
Source: EAPS.

5. Discussion

In this paper, we examine the effect of the 2018-2021 reform in Korea to lower the statutory limit on total weekly working hours on hours actually worked. We exploit the staggered implementation of the new lower 52-hour limit by firm size and make use of nationally representative labour force survey data. First, we find that the implementation of the 52-hour limit for large firms reduced but did not eradicate the incidence of working more than 52 hours. Second, the reform was associated with a reallocation of working hours, where workers shifted from working fulltime (40 hours) to working overtime within the legal limit (41-52 hours). The average working hours has not changed much. Third, we show suggestive evidence that the reallocation is more likely the result of labour demand factors within firms, to account for fewer overtime hours worked by their employees.

Our findings prompt a number of questions.

First, why did the 52-hour limit not fully eradicate the incidence of working more than 52 hours in large firms? This is a major question for policy makers to effectively change a long working-hour culture. The reason for this does not seem to be a lack of regulation, as the Korean Labour Standards Act contains penal provisions in case of breach by employers, leading to imprisonment of maximum two years or a fine of maximum ten million won (about 8,000 euro). The reason rather seems to be non-compliance and the lack of effective enforcement of labour legislation. These issues have also been highlighted in previous studies. For instance, about half the hours eligible to an overtime premium were left unpaid in 2016. About 10% of the employees in 2016-2017 were paid below the minimum wage (Choi, 2018[20]). Around 40% of all wage workers in Korea engage in some form of informal work not fully covered by minimum wage regulation, labour standards and social insurance (OECD, 2019[21]). There are estimations showing that about 80% of informal work occurs under the relevant regulation, again emphasising low level of policy enforcement (Lee, 2017). Moreover, it can be that firms have incentives to demand very long working hours rather than

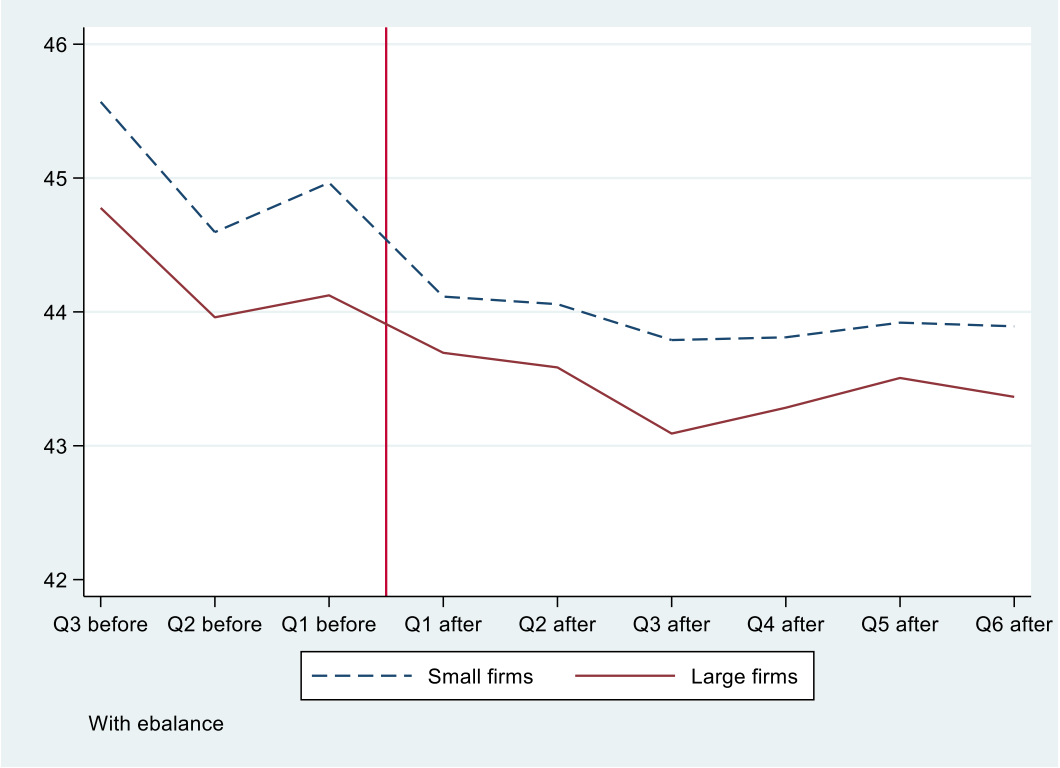
hiring new staff, for instance because of hiring risks due to stringent employment protection legislation. Workers may have incentives to supply very long working hours out of financial necessity and social security coverage gaps (Hijzen and Thewissen, 2020[5]; OECD, 2020[22]).

Second, will the next phases of the ongoing reform, when also smaller firms have to abide by the 52-hour limit, lead to a further reduction in the incidence of very long working hours? This is difficult to predict. On the one hand, as we show in this paper, slightly smaller firms (100-299 employees) are largely comparable to large firms in their working hour practices and worker composition. However, compliance and enforcement may well be even more challenging in smaller firms. Furthermore and more generally, the economic landscape has changed drastically with the start of the COVID-19 pandemic.

Last, what will be the effects of the reform on other labour market outcomes such as employment and wages, worker safety and health, productivity and wellbeing? Unfortunately, such effects cannot yet be identified due to the lag in data availability. Evaluations of the 2004-2011 reduction in the Korean normal workweek report positive health and productivity outcomes, without significantly affecting monthly earnings and employment. Still, these findings do not necessarily fully generalise to the current 2018-2021 reform given differences in the nature of the reform. The current reform caps overtime rather than normal working hours. As a result, it mainly affects those working very long hours, which on the one hand is a smaller group, but on the other also one for which working hours are more likely to entail particularly adverse health and productivity effects. Evaluating the broader labour market consequences of the reform is therefore an important topic for further study in the next few years.

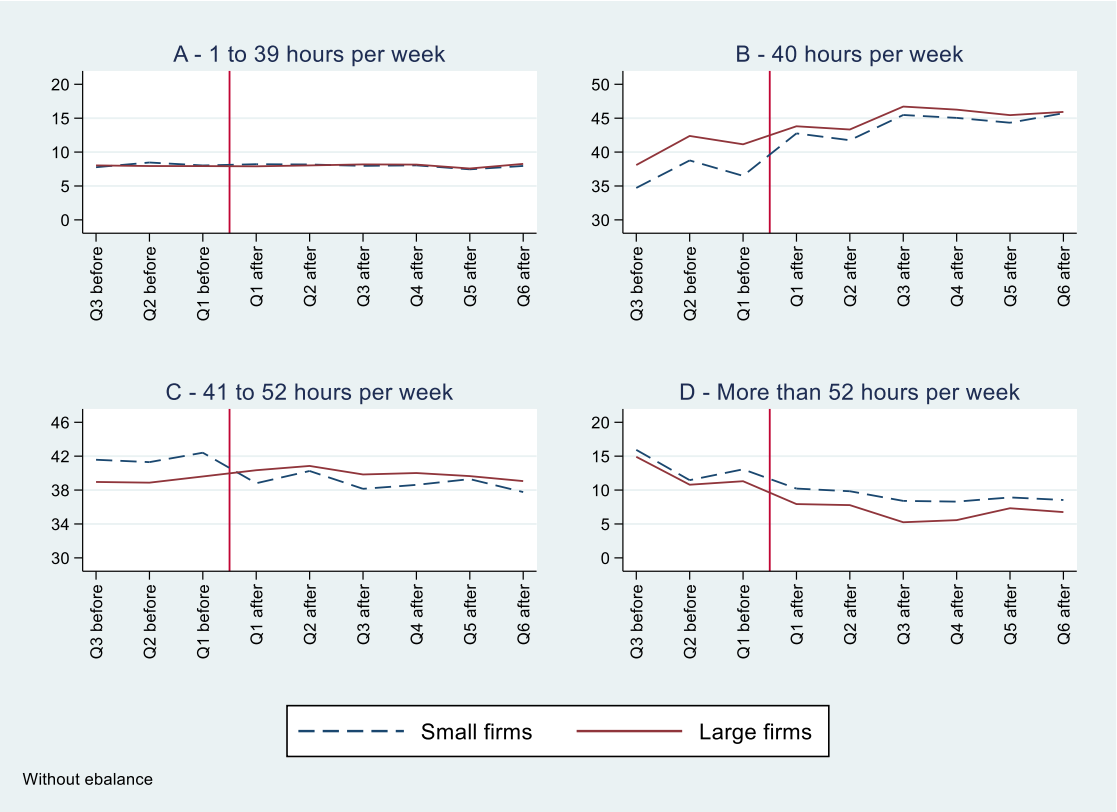
Appendix

Figure A1. Parallel trends for average hours worked



Note: Data are seasonally adjusted and weighted by entropy balance weights. Total hours worked in the main job are top- and bottom coded at the 1% and 99% percentile (24 and 70 hours per week). The red line represents the implementation date of the reform.
Source: EAPS.

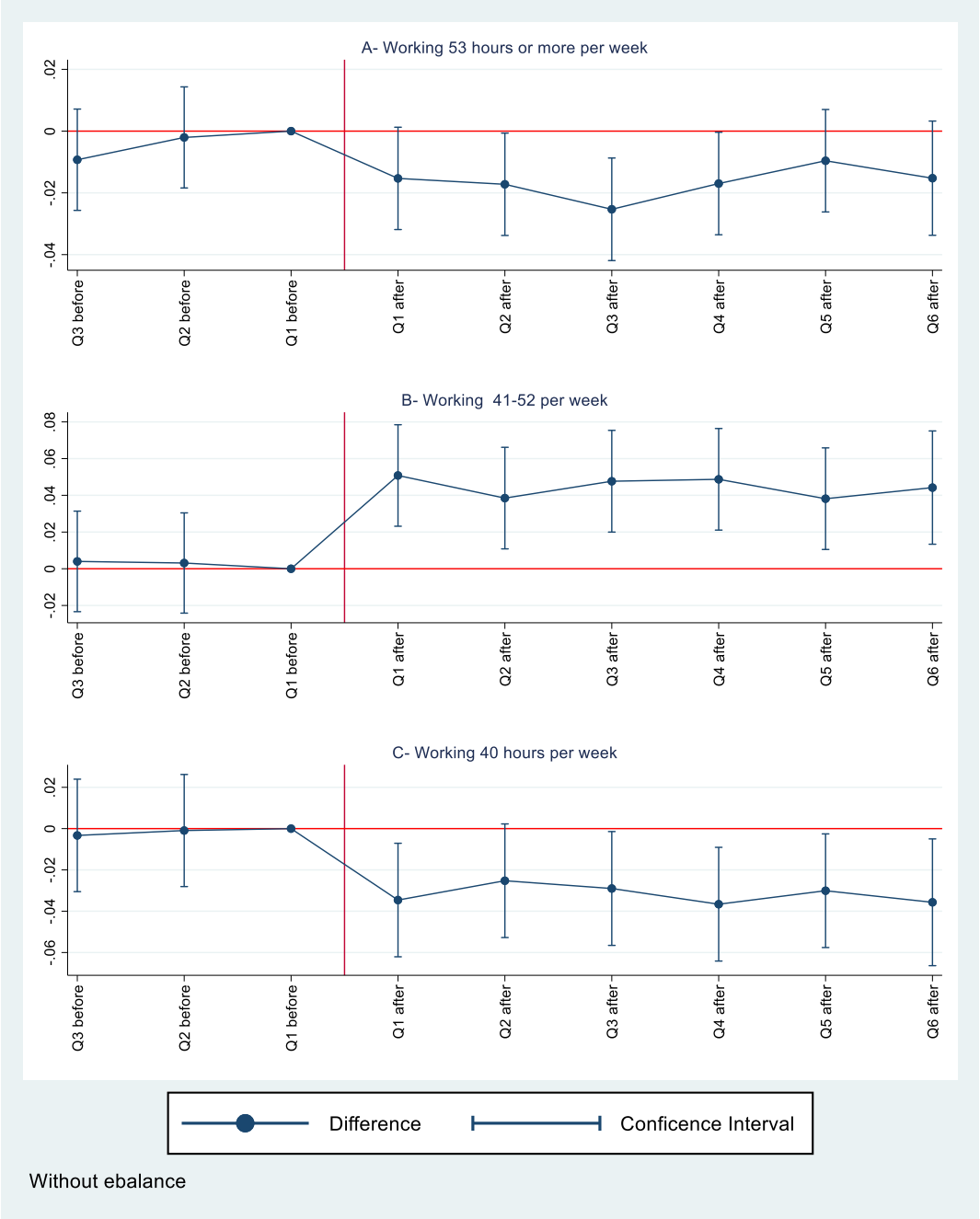
Figure A2. Parallel trends in incidence of working particular weekly hours for treatment and control group without entropy balance weights



Note: Data are seasonally adjusted and not weighted by entropy balance weights. The red line represents the implementation date of the reform.
Source: EAPS.

Figure A3. Dynamic effects without entropy balance weights

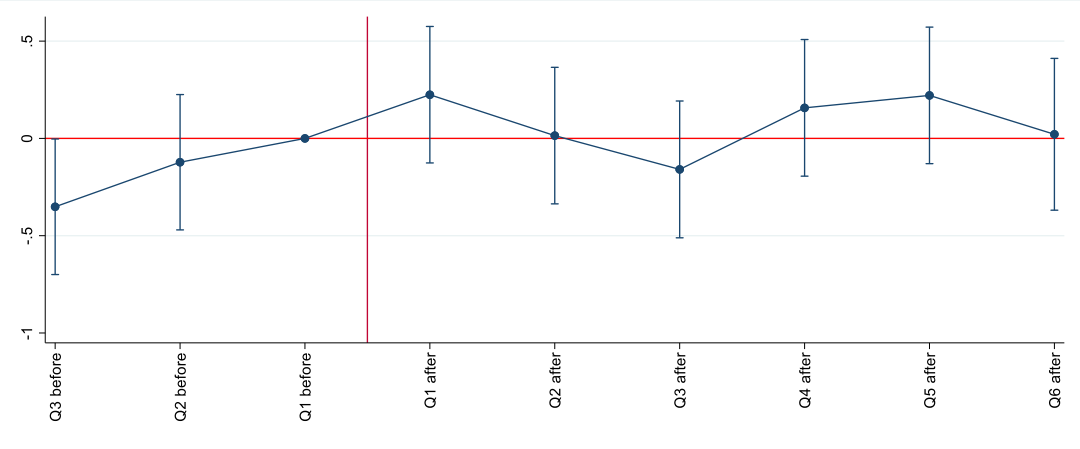
Difference-in-difference in the incidence of working different working hours between large and smaller firms, relative to the quarter before the reform (April-June 2018), percentage points.



Note: The sample consists of employees in firms with 100 or more employees. Regression results of a seemingly unrelated difference-in-difference model covering October 2017-November 2019. The results are based on a regression that does not apply entropy balance weights. Vertical bands indicate the 90% confidence intervals of each point estimate. The red line represents the implementation date of the reform.
Source: EAPS.

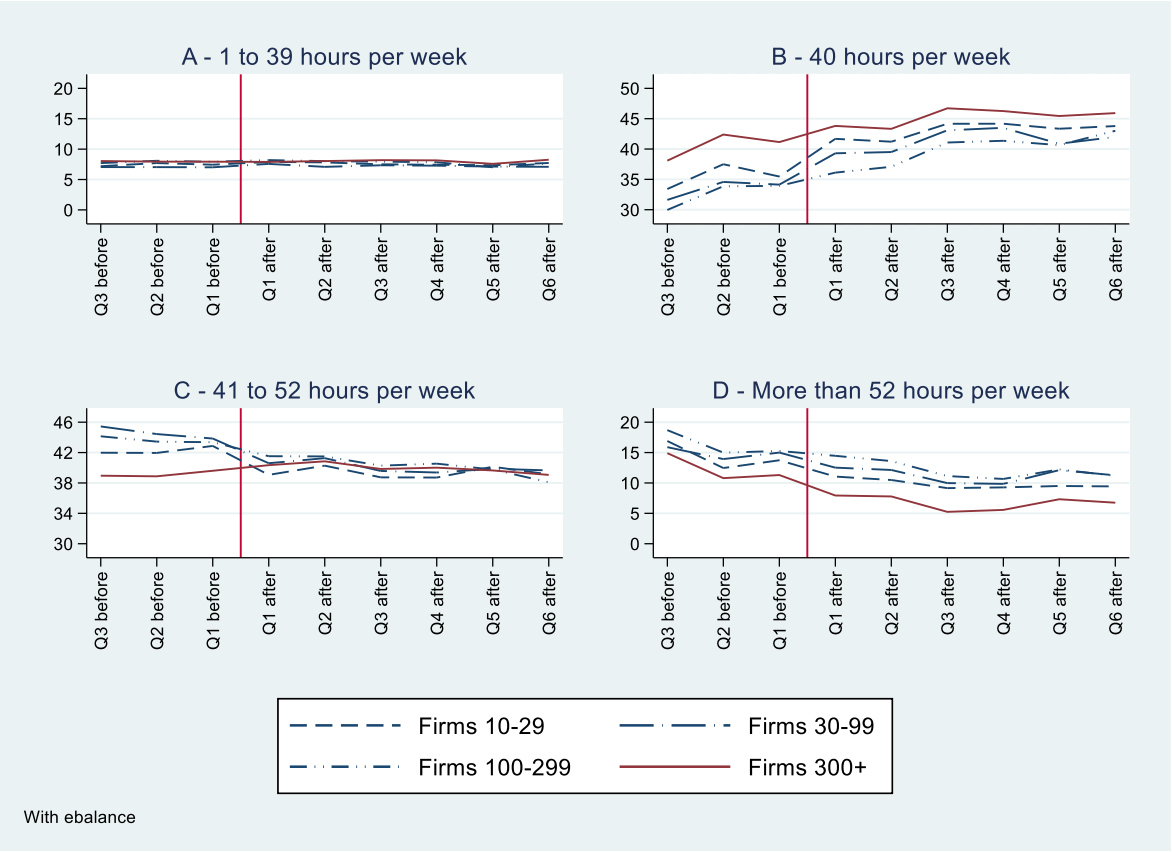
Figure A4. Dynamic effects for average hours worked

Difference-in-difference in total hours worked between large and smaller firms, relative to the quarter before the reform (April-June 2018), percentage points.



Note: The sample consists of employees in firms with 100 or more employees. Regression results of linear probability model with entropy balance weights covering October 2017-November 2019. Total hours worked in the main job are top- and bottom coded at the 1% and 99% percentile (24 and 70 hours per week). Vertical bands indicate the 90% confidence intervals of each point estimate. The red line represents the implementation date of the reform.
Source: EAPS.

Figure A5. Parallel trends in incidence of working particular weekly hours for other firm sizes



Note: Data are seasonally adjusted and weighted by entropy balance weights. The red line represents the implementation date of the reform.
Source: EAPS.

Table A1. Main regression results with control variables

	(1) Working more than 52 hours	(2) Working 41-52 hours	(3) Working 40 hours
Reform * large firm dummy (treatment effect)	-0.012**	0.043***	-0.031***
	(0.005)	(0.008)	(0.008)
Reform dummy	-0.075***	-0.030**	0.101***
	(0.009)	(0.015)	(0.015)
Large firm dummy (treatment group)	-0.008**	-0.032***	0.043***
	(0.004)	(0.007)	(0.007)
Female dummy	-0.039***	-0.060***	0.099***
	(0.005)	(0.008)	(0.008)
Age group 15-24	-0.035***	0.022*	0.006
	(0.008)	(0.013)	(0.012)
Age group 55-64	-0.000	-0.014*	0.018**
	(0.005)	(0.007)	(0.007)
Education below upper secondary	0.005	0.089***	-0.095***
	(0.009)	(0.015)	(0.015)
Education tertiary or +	-0.025***	-0.018***	0.045***
	(0.003)	(0.005)	(0.005)
Relation to head: spouse	-0.018***	-0.014	0.031***
	(0.006)	(0.010)	(0.010)
Relation to head: child	0.001	-0.042***	0.043***
	(0.005)	(0.008)	(0.008)
Relation to head: other	0.043***	-0.004	-0.027
	(0.012)	(0.020)	(0.020)
Living in rural area	-0.027***	0.005	0.024***
	(0.004)	(0.006)	(0.006)
Married	-0.046***	-0.002	0.051***
	(0.004)	(0.006)	(0.006)
Tenure	-0.000***	-0.000***	0.000***
	(0.000)	(0.000)	(0.000)
Constant	0.148***	0.691***	0.103
	(0.043)	(0.070)	(0.069)
Sector & occupation dummies	Yes	Yes	Yes
Month * year dummies	Yes	Yes	Yes
Number of observations	56,072	56,072	56,072
Adjusted R^2	0.045	0.058	0.089

Note: The sample consists of employees in firms with 100 or more employees. Regression results of a seemingly unrelated difference-in-difference model with entropy balance weights covering October 2017-November 2019. Standard errors in parenthesis. *, **, *** indicate statistical significance at 10%, 5%, and 1% levels, respectively.
Source: EAPS.

Table A2. Main regression results without entropy balance weights

	(1) Working more than 52 hours	(2) Working 41-52 hours	(3) Working 40 hours
Reform * large firm dummy (treatment effect)	-0.013**	0.042***	-0.030***
	(0.005)	(0.008)	(0.008)
Reform dummy	-0.074***	-0.038**	0.106***
	(0.009)	(0.015)	(0.015)
Large firm dummy (treatment group)	-0.004	-0.029***	0.038***
	(0.004)	(0.007)	(0.007)
Control variables	Yes	Yes	Yes
Sector & occupation dummies	Yes	Yes	Yes
Month * year dummies	Yes	Yes	Yes
Number of observations	56,090	56,090	56,090
Adjusted R^2	0.045	0.058	0.089

Note: The sample consists of employees in firms with 100 or more employees. Regression results of a seemingly unrelated difference-in-difference model covering October 2017-November 2019. The results are based on a regression that does not apply entropy balance weights. Standard errors in parenthesis. *, **, *** indicate statistical significance at 10%, 5%, and 1% levels, respectively.
Source: EAPS.

Table A3. Main results with varying durations of the pre-reform period

		(1) Working more than 52 hours	(2) Working 41-52 hours	(3) Working 40 hours
3 quarters before (baseline) N=56072	Reform * large firm dummy (treatment effect)	-0.012**	0.043***	-0.031***
	Reform dummy	-0.075***	-0.030**	0.101***
	Large firm dummy (treatment group)	-0.008**	-0.032***	0.043***
4 quarters before N=62733	Reform * large firm dummy (treatment effect)	-0.010**	0.039***	-0.031***
	Reform dummy	-0.074***	0.01	0.137***
	Large firm dummy (treatment group)	-0.009**	-0.027***	0.041***
5 quarters before N=69388	Reform * large firm dummy (treatment effect)	-0.000	0.028***	-0.027***
	Reform dummy	-0.090***	-0.036**	0.116***
	Large firm dummy (treatment group)	-0.019***	-0.017***	0.039***

Table A4. Main regression results based on alternative estimation methods

	(1)	(2)	(3)	(4)	(5)	(6)
	Working more than 52 hours		Working 41-52 hours		Working 40 hours	
	OLS	Logit	OLS	Logit	OLS	Logit
Reform * large firm dummy (treatment effect)	-0.012*	-0.277***	0.043***	0.190***	-0.031***	-0.147***
	(0.007)	(0.070)	(0.010)	(0.045)	(0.010)	(0.046)
Reform dummy	-0.075***	-0.760***	-0.030	-0.128	0.101***	0.447***
	(0.012)	(0.134)	(0.019)	(0.082)	(0.019)	(0.083)
Large firm dummy (treatment group)	-0.008	-0.031	-0.032***	-0.140***	0.043***	0.202***
	(0.006)	(0.054)	(0.009)	(0.037)	(0.008)	(0.038)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Sector & occupation dummies	Yes	Yes	Yes	Yes	Yes	Yes
Month * year dummies	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	56072	52509	56072	52616	56072	52616
Adjusted R^2	0.044		0.057		0.088	

Note: The sample consists of employees in firms with 100 or more employees. Regression results are based on the period October 2017-November 2019, using either a linear probability model with robust standard errors (OLS) or la Logit model (Logit). Entropy balance weights are used. Standard errors in parenthesis. *, **, *** indicate statistical significance at 10%, 5%, and 1% levels, respectively.
Source: EAPS.

Table A5 - Mean value of dependent variables pre-reform

	Treatment	Control
Working more than 52 hours	12.5	14.5
Working 41-52 hours	39.3	42.7
Working 40 hours	40.5	35.2
Working less than 40 hours	7.7	7.6

Note: Incidence of various categories of working hours, in percentage. The sample consists of employees in firms with 100 or more employees. The pre-reform period runs from October 2017 to June 2018, Entropy balance weights are used.
Source: EAPS.

References

Ahn, T. (2016), "Reduction of Working Time: Does It Lead to a Healthy Lifestyle?", *Health Economics*, Vol. 25/8, pp. 969-983, http://dx.doi.org/10.1002/hec.3198. [6]

Andrews, M. et al. (2015), "More hours, more jobs? The employment effects of longer working hours", *Oxford Economic Papers*, Vol. 67/2, pp. 245-268, http://dx.doi.org/10.1093/oep/gpu026. [30]

Bassanini, A. and E. Caroli (2015), "Is Work Bad for Health? The Role of Constraint versus Choice", *Annals of Economics and Statistics* 119/120, p. 13, http://dx.doi.org/10.15609/annaeconstat2009.119-120.13. [1]

Becker, G. (1981), *A Treatise on the Family*, Harvard University Press, Cambridge MA. [9]

Bick, A. and N. Fuchs-Schündeln (2018), "Taxation and labour supply of married couples across countries: A macroeconomic analysis", *Review of Economic Studies*, Vol. 85/3, pp. 1543-1576, http://dx.doi.org/10.1093/restud/rdx057. [11]

Blundell, R., A. Bozio and G. Laroque (2013), "Extensive and Intensive Margins of Labour Supply: Work and Working Hours in the US, the UK and France", *Fiscal Studies*, Vol. 34/1, pp. 1-29, https://doi.org/10.1111/j.1475-5890.2013.00175.x. [18]

Choi, K. (2018), "Employment Effect of Minimum Wage Increase", *KDI Focus*, No. 90, https://papers.ssrn.com/sol3/papers.cfm?abstract_id=3212587. [20]

Crépon, B. and F. Kramarz (2002), "Employed 40 hours or not employed 39: Lessons from the 1982 mandatory reduction of the workweek", *Journal of Political Economy*, Vol. 110/6, pp. 1355-1389, http://dx.doi.org/10.1086/342807. [15]

Dolton, P. (2017), "Working hours: Past, present, and future", *IZA World of Labor*, Vol. 406, http://dx.doi.org/10.15185/izawol.406. [3]

Estevão, M. and F. Sá (2008), "The 35-Hour Workweek in France: Straightjacket or Welfare Improvement?", *Economic Policy*, Vol. 23/55, pp. 417-463, https://www.jstor.org/stable/40071859. [24]

Friesen, J. (2001), "Overtime pay regulation and weekly hours of work in Canada", *Labour Economics*, Vol. 8/6, pp. 691-720, http://dx.doi.org/10.1016/S0927-5371(01)00053-7. [19]

Goodman-Bacon, A. (2021), "Difference-in-differences with variation in treatment timing", *Journal of Econometrics*, Vol. 225/2, pp. 254-277, http://dx.doi.org/10.1016/J.JECONOM.2021.03.014. [32]

Goux, D., E. Maurin and B. Petrongolo (2014), "Worktime Regulations and Spousal Labor Supply", *American Economic Review*, Vol. 104/1, pp. 252-276, http://dx.doi.org/10.1257/aer.104.1.252. [12]

Guner, N., R. Kaygusuz and G. Ventura (2012), "Taxation and household labour supply", *Review of Economic Studies*, Vol. 79/3, pp. 1113-1149, http://dx.doi.org/10.1093/restud/rdr049. [10]

Hainmueller, J. (2012), "Entropy Balancing for Causal Effects: A Multivariate Reweighting Method to Produce Balanced Samples in Observational Studies", *Political Analysis*, Vol. 20/1, pp. 25-46, http://dx.doi.org/10.1093/PAN/MPR025. [14]

Hamermesh, D., D. Kawaguchi and J. Lee (2017), "Does labor legislation benefit workers? Well-being after an hours reduction", *Journal of the Japanese and International Economies*, Vol. 44, pp. 1-12, http://dx.doi.org/10.1016/J.JJIE.2017.02.003. [17]

Hijzen, A. and S. Thewissen (2020), "The 2018-2021 working time reform in Korea: A preliminary assessment", *OECD Social, Employment and Migration Working Papers*, No. 248, https://doi.org/10.1787/1815199X. [5]

Hunt, J. (1999), "Has Work-Sharing Worked in Germany?", *The Quarterly Journal of Economics*, Vol. 114/1, pp. 117-148, http://dx.doi.org/10.1162/003355399555963. [23]

Karaca-Mandic, P., E. Norton and B. Dowd (2012), "Interaction Terms in Nonlinear Models", *Health Services Research*, Vol. 47/1pt1, pp. 255-274, http://dx.doi.org/10.1111/j.1475-6773.2011.01314.x. [26]

Kawaguchi, D., J. Lee and D. Hamermesh (2013), "A gift of time", *Labour Economics*, Vol. 24, pp. 205-216, http://dx.doi.org/10.1016/J.LABECO.2013.09.003. [13]

Kawaguchi, D., H. Naito and I. Yokoyama (2017), "Assessing the effects of reducing standard hours: Regression discontinuity evidence from Japan", *Journal of the Japanese and International Economies*, Vol. 43, pp. 59-76, http://dx.doi.org/10.1016/j.jjie.2016.12.002. [28]

Lee, N.H. (2017) "Informal work in the Republic of Korea: Non-regulation or non-compliance?", in: Fenwick & Van Goethem (eds) *Regulating for Equitable and Job-Rich Growth*, Edward Elgar Press

Lee, J. and Y. Lee (2016), "Can working hour reduction save workers?", *Labour Economics*, Vol. 40, pp. 25-36, http://dx.doi.org/10.1016/J.LABECO.2016.02.004. [7]

Oaxaca, R. (2014), "The effect of overtime regulations on employment", *IZA World of Labor*, http://dx.doi.org/10.15185/izawol.89. [31]

OECD (2020), *OECD Economic Surveys: Korea 2020*, OECD Publishing, Paris, https://doi.org/10.1787/2dde9480-en. [22]

OECD (2019), *Rejuvenating Korea: Policies for a Changing Society*, OECD Publishing, Paris, https://dx.doi.org/10.1787/c5eed747-en. [21]

Park, W. and Y. Park (2019), "When Less is More: The Impact of the Regulation on Standard Workweek on Labor Productivity in South Korea", *Journal of Policy Analysis and Management*, Vol. 38/3, pp. 681-705, http://dx.doi.org/10.1002/pam.22136. [8]

Pencavel, J. (2014), "The productivity of working hours", *The Economic Journal*, Vol. 125, pp. 2052-2076, http://dx.doi.org/10.1111/ecoj.12166. [4]

Saint-Martin, A., H. Inanc and C. Prinz (2018), "Job Quality, Health and Productivity: An evidence-based framework for analysis", *OECD Social, Employment and Migration Working Papers*, No. 221, OECD Publishing, Paris, https://dx.doi.org/10.1787/a8c84d91-en. [2]

Sánchez, R. (2013), "Do reductions of standard hours affect employment transitions?: Evidence from Chile", *Labour Economics*, Vol. 20, pp. 24-37, http://dx.doi.org/10.1016/J.LABECO.2012.10.001. [29]

Skans, O. (2004), "The impact of working-time reductions on actual hours and wages: evidence from Swedish register-data", *Labour Economics*, Vol. 11/5, pp. 647-665, http://dx.doi.org/10.1016/J.LABECO.2003.06.002. [16]

Skuterud, M. (2007), "Identifying the Potential of Work-Sharing as a Job-Creation Strategy", *Journal of Labor Economics*, Vol. 25/2, pp. 265-287, http://dx.doi.org/10.1086/511379. [25]

Zveglich, J. and Y. Van der Meulen Rodgers (2003), *The Impact of Protective Measures for Female Workers*, The University of Chicago Press, http://dx.doi.org/10.1086/374958. [27]