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## Does Weaker Employment Protection Lower the Cost of Job Loss?

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https://doi.org/10.3368/jhr.0725-14408R2

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# Online Appendix

## Does Weaker Employment Protection Lower the Cost of Job Loss?

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June 1, 2026

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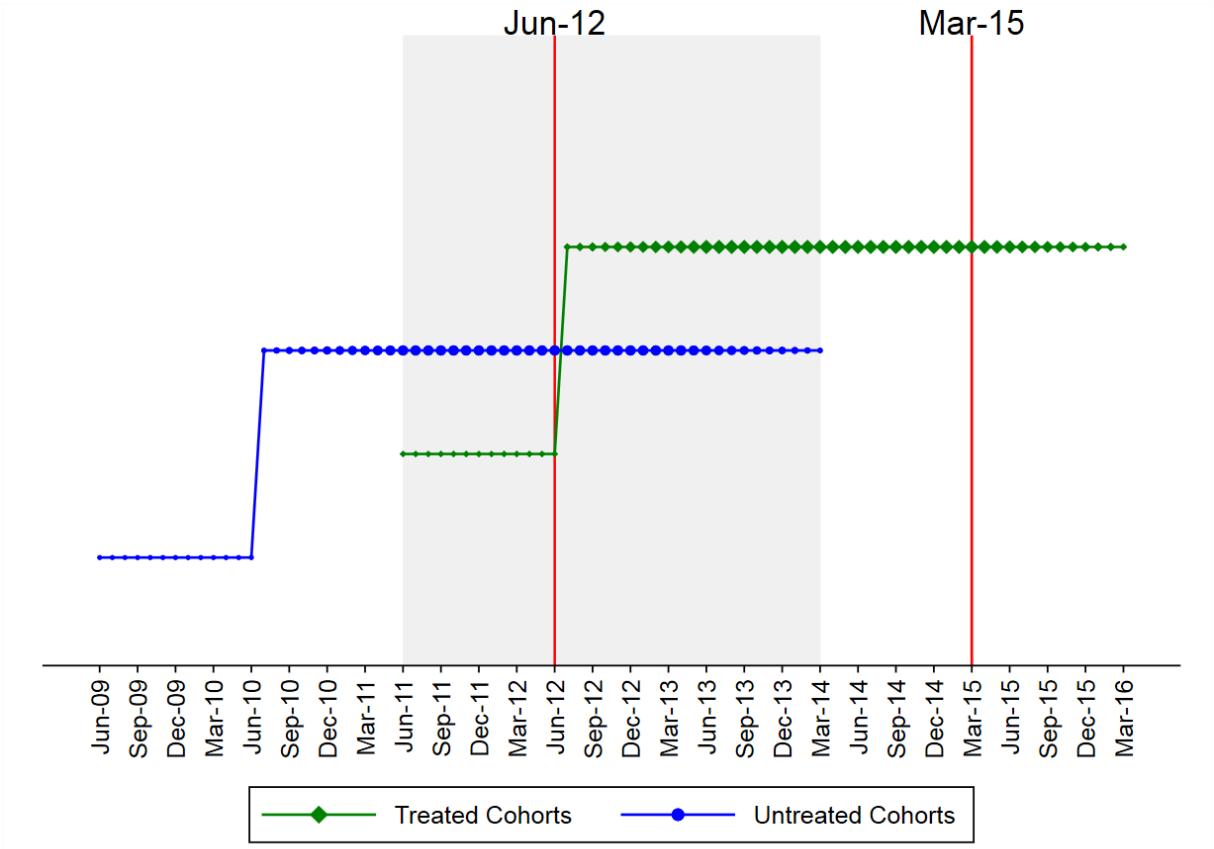
# A Supplementary Material: Section 3

## A.1 Description of Time/Period Coverage of Each Cohort (Subsection 3.2)

Figure A1 illustrates the time-period coverage of each cohort, showing that all cohorts are observed 13 months before job termination and 33 months after. There are calendar time periods where cohorts from the two groups overlap, although by design, the overlapping treated and untreated cohorts cannot be in the same event period. Following the most recent untreated cohort for 33 months after job termination in June 2011 (i.e., up to March 2014) ensures that no individual in untreated cohorts was affected by the 2015 Jobs Act. Therefore, we can cleanly evaluate whether the Jobs Act attenuated or magnified the changes set in place by the Fornero reform using the last 12 periods for the treated group.

In the figure, post-event periods of the eldest cohort in each group are identified with a jump, which for treated cohorts coincides with the introduction of the Fornero reform. This is repeated exactly for untreated cohorts, just 24 months earlier. For both groups, the size of the diamonds is proportional to the number of cohorts contributing to the estimation in each time period. Thus, for each group, the first and last month are populated only by one cohort, the second and second-to-last month have two cohorts, and so on. These lines get thinner after 33 months from the jump when the earliest displaced workers are no longer observed and workers in the other cohorts progressively disappear. In April 2014 (untreated group) and in March 2016 (treated group), only one cohort per group remains in the sample. Note, however, that the estimates in each period are a weighted average of different time periods.

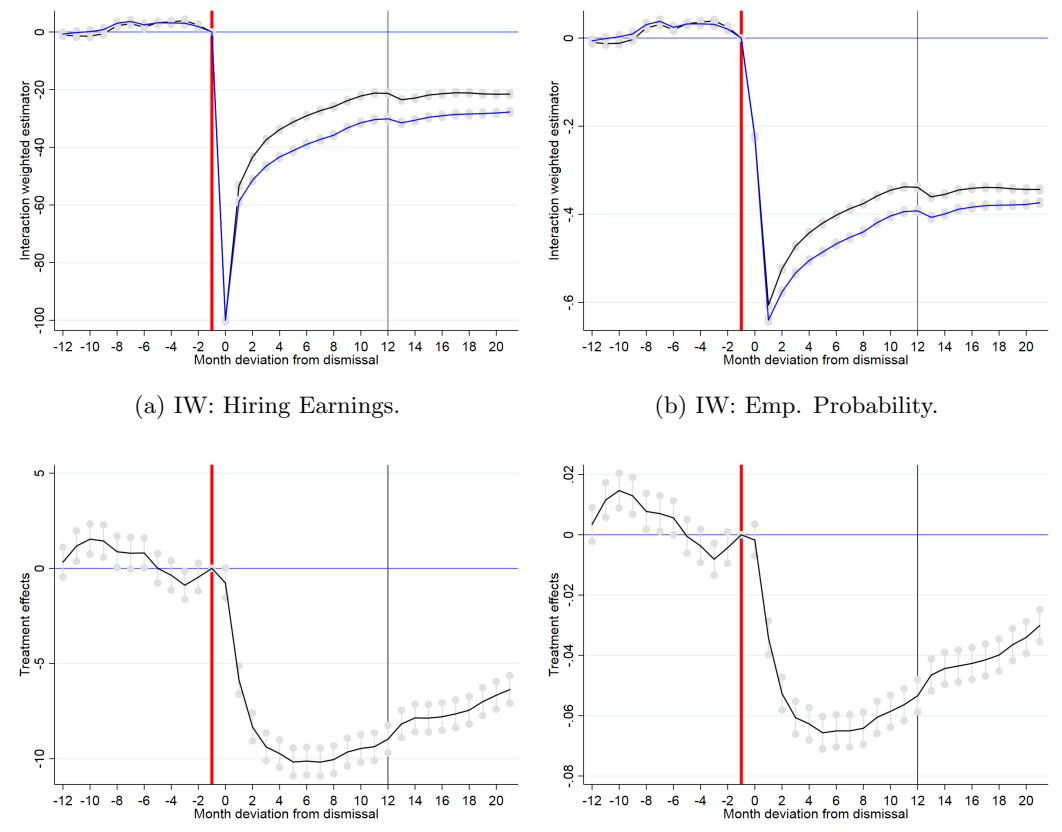
Figure A1: Time Coverage of Treated and Untreated Cohorts




*Notes*: The size of the diamonds and circles is proportional to the size of the cohorts that contribute to estimation at each point in time. "Treated" refers to the cohorts of individuals who lost their job after the introduction of the Fornero reform (June 2012) between July 2012 and June 2013. "Untreated" refers to the cohorts of individuals who lost their job before the Fornero reform, and between July 2010 and June 2011. The two vertical red lines indicate the enactment of the Fornero reform (left, in June 2012) and the Jobs Act (right, in March 2015).

## A.2 Estimates Found When Limiting the Time Window from 12 Months before to 21 Months After Job Termination (Subsection 3.2)

Figure A2: Treatment effects of the reform on labour market outcomes




(a) IW: Hiring Earnings.

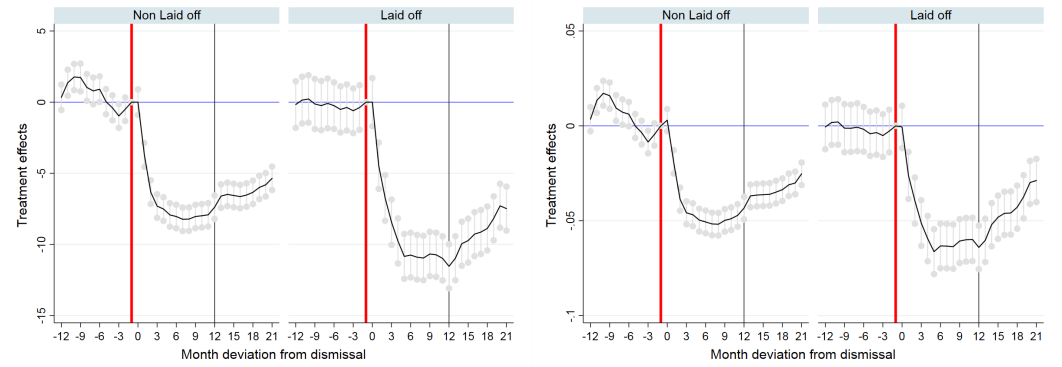
(b) IW: Emp. Probability.

(c) D-IW: Hiring Earnings.

(d) D-IW: Emp. Probability.

*Notes:* The gray bar around each point estimate refers to the 95% confidence interval. For hiring earnings in panels (a) and (c), the impacts are expressed in percent terms, while in the other two panels the estimates are expressed in percentage point terms. Treated cohorts are in blue and untreated cohorts are in black.

Figure A3: Treatment Effects of the Reform on Labor Market Outcomes by Laid-off Status



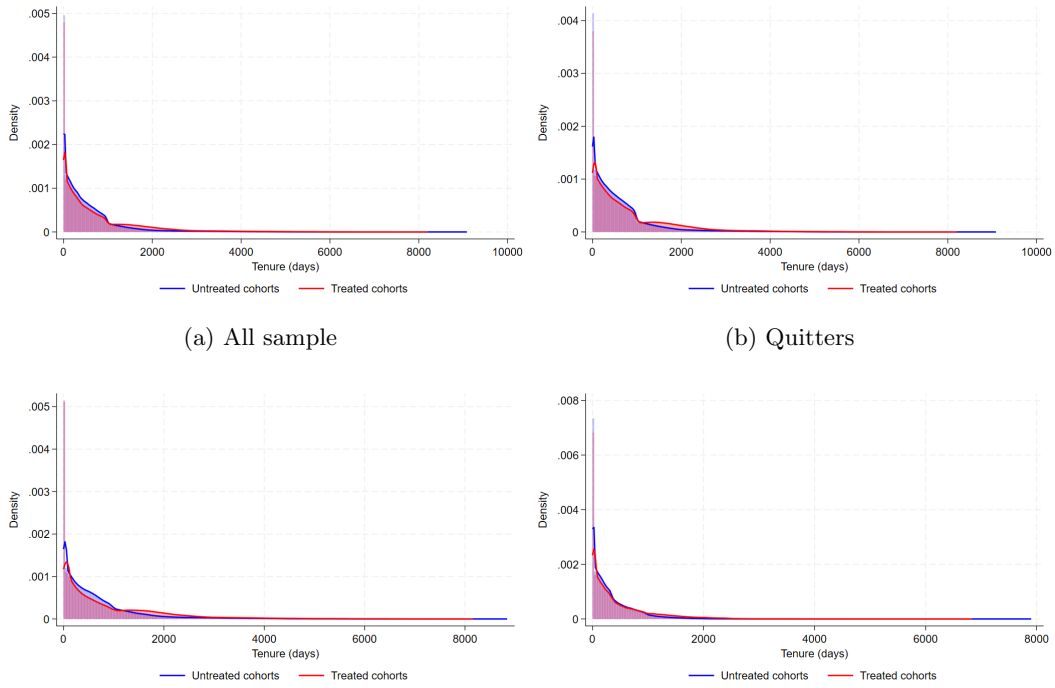

(a) D-IW: Hiring Earnings.

(b) D-IW: Emp. Probability.

*Notes:* The gray bar around each point estimate refers to the 95% confidence interval. For hiring earnings in panels (a), the impacts are expressed in percent terms, while in the other panel the estimates are expressed in percentage point terms.

## A.3 Descriptive Statistics (Subsection 3.2)

Figure A4: Comparison between tenure distributions




(a) All sample

(b) Quitters

(c) Laid-off workers

(d) End of temporary job

Table A1: Summary Statistics for Treated and Untreated Cohorts: Quitters

| | **Overall** | | | **Before dismissal** | | |
|---|---|---|---|---|---|---|
| | Mean Untreated (Std. Dev.) | Mean Treated (Std. Dev. | Diff (Std. Err.) | Mean Untreated (Std. Dev.) | Mean Treated (Std. Dev. | Diff (Std. Err.) |
| Age (years) | 31.071 | 31.641 | $-0.571^{***}$ | 29.653 | 30.224 | $-0.571^{***}$ |
| | (4.979) | (3.612) | (0.008) | (4.859) | (3.444) | (0.015) |
| Male (=1 if yes) | 0.607 | 0.586 | $0.021^{***}$ | 0.607 | 0.586 | $0.021^{***}$ |
| | (0.489) | (0.493) | (0.001) | (0.489) | (0.493) | (0.002) |
| Education: | | | | | | |
| Below high school | 0.510 | 0.487 | $0.023^{***}$ | 0.510 | 0.487 | $0.023^{***}$ |
| | (0.500) | (0.500) | (0.001) | (0.500) | (0.500) | (0.002) |
| High school | 0.348 | 0.355 | $-0.007^{***}$ | 0.348 | 0.355 | $-0.007^{***}$ |
| | (0.476) | (0.479) | (0.001) | (0.476) | (0.479) | (0.002) |
| Post-secondary | 0.142 | 0.158 | $-0.016^{***}$ | 0.142 | 0.158 | $-0.016^{***}$ |
| | (0.349) | (0.365) | (0.001) | (0.349) | (0.365) | (0.001) |
| Work experience (top quartile) | 0.542 | 0.537 | $0.005^{***}$ | 0.056 | 0.101 | $-0.045^{***}$ |
| | (0.498) | (0.499) | (0.001) | (0.229) | (0.301) | (0.001) |
| Separations (top three quartiles) | 0.041 | 0.039 | $0.002^{***}$ | 0.000 | 0.000 | $0.000^{***}$ |
| | (0.199) | (0.193) | (0.000) | (0.000) | (0.000) | (0.000) |
| Net Job Flows (top three quartiles) | 0.040 | 0.036 | $0.004^{***}$ | 0.017 | 0.013 | $0.003^{***}$ |
| | (0.197) | (0.186) | (0.000) | (0.128) | (0.115) | (0.000) |
| Costs (top three quartiles) | 0.021 | 0.024 | $-0.003^{***}$ | 0.039 | 0.057 | $-0.018^{***}$ |
| | (0.142) | (0.152) | (0.000) | (0.193) | (0.232) | (0.001) |
| Job Tenure (days) | 624.803 | 807.031 | $-182.229^{***}$ | 1038.603 | 1331.651 | $-293.048^{***}$ |
| | (766.968) | (919.008) | (1.499) | (1063.631) | (1138.764) | (3.781) |
| Lagged Experience (days) | 971.783 | 1067.370 | $-95.587^{***}$ | 69.559 | 143.385 | $-73.827^{***}$ |
| | (1150.327) | (1287.055) | (2.187) | (336.264) | (511.683) | (1.401) |
| Firm size above 15 (=1 if yes) | 0.078 | 0.083 | $-0.005^{***}$ | 0.066 | 0.071 | $-0.005^{***}$ |
| | (0.268) | (0.276) | (0.001) | (0.248) | (0.257) | (0.001) |
| Employed (=1 if yes) | 0.738 | 0.703 | $0.035^{***}$ | 0.960 | 0.965 | $-0.005^{***}$ |
| | (0.440) | (0.457) | (0.001) | (0.195) | (0.183) | (0.001) |
| Self-employed (=1 if yes) | 0.005 | 0.006 | $-0.001^{***}$ | 0.005 | 0.005 | $0.000^{***}$ |
| | (0.073) | (0.078) | (0.000) | (0.071) | (0.071) | (0.000) |
| Hiring earnings (in 2013 Euros) | 851.281 | 816.912 | $34.369^{***}$ | 858.342 | 935.210 | $-76.869^{***}$ |
| | (835.299) | (809.298) | (1.509) | (864.249) | (860.741) | (2.996) |

*Notes:* The table reports mean (and standard deviation) of variables for treated and untreated cohorts, both over the whole period and before dismissal. "Work experience (top quartile)" is an indicator variable equal to 1 if the worker's past-experience is in the top quartile of the distribution, conditional on worker's age. "Separations (top three quartiles)" is an indicator variable equal to 1 if the job is linked to a number of job separations in a month above the bottom quartile of the distribution, conditional on age and region of birth. "Net flows (top three quartiles)" is an indicator variable equal to 1 if the job is associated with a number of net flows (hirings minus separations) in a month above the bottom quartile of the distribution, conditional on age and region of birth. "Costs (top three quartiles)" is an indicator variable equal to 1 if the job benefited from tax rebates above the bottom quartile of the distribution, conditional on age.

Table A2: Summary Statistics for Treated and Untreated Cohorts: Laid-off workers

| | **Overall** | | | **Before dismissal** | | |
|---|---|---|---|---|---|---|
| | Mean Untreated (Std. Dev.) | Mean Treated (Std. Dev. | Diff (Std. Err.) | Mean Untreated (Std. Dev.) | Mean Treated (Std. Dev. | Diff (Std. Err.) |
| Age (years) | 32.155 | 32.142 | 0.013 | 30.737 | 30.721 | 0.016 |
| | (4.374) | (3.552) | (0.010) | (4.238) | (3.381) | (0.017) |
| Male (=1 if yes) | 0.651 | 0.629 | $0.022^{***}$ | 0.651 | 0.629 | $0.022^{***}$ |
| | (0.477) | (0.483) | (0.001) | (0.477) | (0.483) | (0.002) |
| Education: | | | | | | |
| Below high school | 0.618 | 0.592 | $0.026^{***}$ | 0.618 | 0.592 | $0.026^{***}$ |
| | (0.486) | (0.492) | (0.001) | (0.486) | (0.492) | (0.002) |
| High school | 0.326 | 0.346 | $-0.021^{***}$ | 0.326 | 0.346 | $-0.021^{***}$ |
| | (0.469) | (0.476) | (0.001) | (0.469) | (0.476) | (0.002) |
| Post-secondary | 0.057 | 0.062 | $-0.005^{***}$ | 0.057 | 0.062 | $-0.005^{***}$ |
| | (0.232) | (0.242) | (0.001) | (0.232) | (0.242) | (0.001) |
| Work experience (top quartile) | 0.505 | 0.523 | $-0.019^{***}$ | 0.036 | 0.076 | $-0.040^{***}$ |
| | (0.500) | (0.500) | (0.001) | (0.187) | (0.266) | (0.001) |
| Separations (top three quartiles) | 0.046 | 0.043 | $0.003^{***}$ | 0.000 | 0.000 | $0.000^{***}$ |
| | (0.210) | (0.204) | (0.001) | (0.000) | (0.000) | (0.000) |
| Net Job Flows (top three quartiles) | 0.042 | 0.038 | $0.003^{***}$ | 0.010 | 0.008 | $0.002^{***}$ |
| | (0.200) | (0.192) | (0.001) | (0.102) | (0.089) | (0.000) |
| Costs (top three quartiles) | 0.035 | 0.043 | $-0.008^{***}$ | 0.078 | 0.116 | $-0.038^{***}$ |
| | (0.183) | (0.202) | (0.000) | (0.268) | (0.320) | (0.001) |
| Job Tenure (days) | 662.206 | 873.968 | $-211.763^{***}$ | 1134.745 | 1524.650 | $-389.905^{***}$ |
| | (843.675) | (1041.691) | (2.198) | (1086.146) | (1201.910) | (5.089) |
| Lagged Experience (days) | 967.993 | 1132.185 | $-164.192^{***}$ | 50.377 | 123.894 | $-73.516^{***}$ |
| | (1175.846) | (1383.087) | (2.987) | (288.894) | (507.269) | (1.780) |
| Firm size above 15 (=1 if yes) | 0.044 | 0.053 | $-0.009^{***}$ | 0.021 | 0.028 | $-0.006^{***}$ |
| | (0.205) | (0.224) | (0.001) | (0.145) | (0.164) | (0.001) |
| Employed (=1 if yes) | 0.658 | 0.630 | $0.027^{***}$ | 0.979 | 0.983 | $-0.005^{***}$ |
| | (0.475) | (0.483) | (0.001) | (0.145) | (0.129) | (0.001) |
| Self-employed (=1 if yes) | 0.003 | 0.004 | $-0.002^{***}$ | 0.001 | 0.002 | $-0.001^{***}$ |
| | (0.050) | (0.063) | (0.000) | (0.029) | (0.044) | (0.000) |
| Hiring earnings (in 2013 Euros) | 718.431 | 690.718 | $27.713^{***}$ | 865.931 | 922.446 | $-56.515^{***}$ |
| | (816.670) | (770.106) | (1.876) | (878.515) | (822.978) | (3.826) |

*Notes:* The table reports mean (and standard deviation) of variables for treated and untreated cohorts, both over the whole period and before dismissal. "Work experience (top quartile)" is an indicator variable equal to 1 if the worker's past-experience is in the top quartile of the distribution, conditional on worker's age. "Separations (top three quartiles)" is an indicator variable equal to 1 if the job is linked to a number of job separations in a month above the bottom quartile of the distribution, conditional on age and region of birth. "Net flows (top three quartiles)" is an indicator variable equal to 1 if the job is associated with a number of net flows (hirings minus separations) in a month above the bottom quartile of the distribution, conditional on age and region of birth. "Costs (top three quartiles)" is an indicator variable equal to 1 if the job benefited from tax rebates above the bottom quartile of the distribution, conditional on age.

Table A3: Summary Statistics for Treated and Untreated Cohorts: End of Temporary Job

| | **Overall** | | | **Before dismissal** | | |
|---|---|---|---|---|---|---|
| | Mean Untreated (Std. Dev.) | Mean Treated (Std. Dev. | Diff (Std. Err.) | Mean Untreated (Std. Dev.) | Mean Treated (Std. Dev. | Diff (Std. Err.) |
| Age (years) | 30.987 | 30.477 | $0.510^{***}$ | 29.568 | 29.058 | $0.510^{***}$ |
| | (4.983) | (3.528) | (0.013) | (4.862) | (3.357) | (0.024) |
| Male (=1 if yes) | 0.475 | 0.486 | $-0.011^{***}$ | 0.475 | 0.486 | $-0.011^{***}$ |
| | (0.499) | (0.500) | (0.002) | (0.499) | (0.500) | (0.003) |
| Education: | | | | | | |
| Below high school | 0.378 | 0.370 | $0.008^{***}$ | 0.378 | 0.371 | $0.008^{***}$ |
| | (0.485) | (0.483) | (0.001) | (0.485) | (0.483) | (0.003) |
| High school | 0.327 | 0.328 | 0.000 | 0.327 | 0.328 | 0.000 |
| | (0.469) | (0.469) | (0.001) | (0.469) | (0.469) | (0.003) |
| Post-secondary | 0.295 | 0.302 | $-0.007^{***}$ | 0.295 | 0.302 | $-0.007^{***}$ |
| | (0.456) | (0.459) | (0.001) | (0.456) | (0.459) | (0.003) |
| Work experience (top quartile) | 0.380 | 0.396 | $-0.016^{***}$ | 0.106 | 0.153 | $-0.047^{***}$ |
| | (0.485) | (0.489) | (0.001) | (0.307) | (0.360) | (0.002) |
| Separations (top three quartiles) | 0.054 | 0.051 | $0.003^{***}$ | 0.000 | 0.000 | $0.000^{***}$ |
| | (0.226) | (0.219) | (0.001) | (0.000) | (0.000) | (0.000) |
| Net Job Flows (top three quartiles) | 0.067 | 0.060 | $0.007^{***}$ | 0.063 | 0.057 | $0.006^{***}$ |
| | (0.250) | (0.237) | (0.001) | (0.243) | (0.232) | (0.001) |
| Costs (top three quartiles) | 0.017 | 0.014 | $0.003^{***}$ | 0.026 | 0.019 | $0.007^{***}$ |
| | (0.130) | (0.117) | (0.000) | (0.158) | (0.137) | (0.001) |
| Job Tenure (days) | 337.046 | 453.220 | $-116.174^{***}$ | 355.910 | 507.126 | $-151.216^{***}$ |
| | (430.427) | (569.606) | (1.418) | (530.746) | (665.082) | (3.237) |
| Lagged Experience (days) | 547.015 | 587.818 | $-40.803^{***}$ | 134.923 | 204.078 | $-69.155^{***}$ |
| | (704.532) | (859.503) | (2.232) | (441.558) | (621.930) | (2.856) |
| Firm size above 15 (=1 if yes) | 0.164 | 0.152 | $0.012^{***}$ | 0.159 | 0.153 | $0.006^{***}$ |
| | (0.370) | (0.359) | (0.001) | (0.366) | (0.360) | (0.002) |
| Employed (=1 if yes) | 0.624 | 0.574 | $0.050^{***}$ | 0.779 | 0.788 | $-0.009^{***}$ |
| | (0.484) | (0.495) | (0.001) | (0.415) | (0.409) | (0.002) |
| Self-employed (=1 if yes) | 0.008 | 0.014 | $-0.005^{***}$ | 0.008 | 0.013 | $-0.005^{***}$ |
| | (0.091) | (0.115) | (0.000) | (0.089) | (0.115) | (0.001) |
| Hiring earnings (in 2013 Euros) | 623.742 | 574.123 | $49.619^{***}$ | 703.086 | 700.954 | $2.133^{***}$ |
| | (798.201) | (749.674) | (2.279) | (842.512) | (786.555) | (4.565) |

*Notes:*The table reports mean (and standard deviation) of variables for treated and untreated cohorts, both over the whole period and before dismissal. "Work experience (top quartile)" is an indicator variable equal to 1 if the worker's past-experience is in the top quartile of the distribution, conditional on worker's age. "Separations (top three quartiles)" is an indicator variable equal to 1 if the job is linked to a number of job separations in a month above the bottom quartile of the distribution, conditional on age and region of birth. "Net flows (top three quartiles)" is an indicator variable equal to 1 if the job is associated with a number of net flows (hirings minus separations) in a month above the bottom quartile of the distribution, conditional on age and region of birth. "Costs (top three quartiles)" is an indicator variable equal to 1 if the job benefited from tax rebates above the bottom quartile of the distribution, conditional on age.

# B Supplementary Material: Section 4

## B.1 Illustrative Examples of Cohort-Specific DiD Comparisons in the IW Estimates

To fix ideas, consider the untreated group made up solely of workers who separate into non-employment in one of twelve consecutive months, from July 2010 (cohort 1) through June 2011 (cohort 12).[1] We use here the Sun and Abraham (2021) notation for a direct comparison with their paper. We refer to each cohort by its separation month, so cohort $e$ denotes workers who separate in month $e$. Following Sun and Abraham (2021), a 13th cohort—whose workers separated in July 2011—can be interpreted as a *reference cohort*, being the latest not-yet-separated cohort were we to stop after 12 months from job separation.[2] This cohort serves as the comparison group for cohorts that separate in June 2011, and, as suggested by (Sun and Abraham, 2021, p. 186), since there are no never-treated units in our analysis, the regression should be run without including it. The pre-period baseline for each cohort is always one month before separation (event time $\ell = -1$). The cohort-specific DiD estimator for cohort $e$ at event time $\ell$, as in (Sun and Abraham, 2021, eq. 29, p. 187), is:

$$\hat{\delta}_{e,\ell} = \frac{E_N[(Y_{i,e+\ell} - Y_{i,s}) \cdot \mathbf{1}\{E_i = e\}]}{E_N[\mathbf{1}\{E_i = e\}]} - \frac{E_N[(Y_{i,e+\ell} - Y_{i,s}) \cdot \mathbf{1}\{E_i \in \mathcal{C}\}]}{E_N[\mathbf{1}\{E_i \in \mathcal{C}\}]}, \tag{B1}$$

where $s < e$ is the chosen pre-period (here $s = e - 1$, i.e. $\ell = -1$), and $\mathcal{C}$ is the set of not-yet-treated cohorts. The first term in (B1) is the mean outcome change for cohort $e$ between month $e + \ell$ and the baseline $s$, while the second is the corresponding change for the control cohorts. Their difference identifies the Cohort Average Treatment Effect on the Treated, $\text{CATT}_{e,\ell}$.

We now provide some examples of how to interpret the estimated coefficients.

**Cohort 1 (July 2010)** For cohort 1 the pre-period baseline is **June 2010** ($\ell = -1$), so $Y_{i,s} = Y_{i,\text{Jun 2010}}$ is held fixed across all horizons. Because cohort 1 is the earliest-separated cohort, the full set of later cohorts (cohorts 2–12, plus the reference cohort 13) is available as controls.

**One month post-separation ($\ell = +1$; calendar time: August 2010)** The estimator compares the change in outcomes between **August 2010** and **June 2010** for cohort 1 workers against the same change for all not-yet-separated cohorts—here cohorts 2 through 13:

$$\hat{\delta}_{1,1}: \quad \underbrace{(Y_{i,\text{Aug 2010}} - Y_{i,\text{Jun 2010}})}_{\text{cohort 1}} - \underbrace{(Y_{i,\text{Aug 2010}} - Y_{i,\text{Jun 2010}})}_{\text{cohorts 2–13}}.$$

**Twelve months post-separation ($\ell = +12$; calendar time: July 2011)**

$$\hat{\delta}_{1,12}: \quad \underbrace{(Y_{i,\text{Jul 2011}} - Y_{i,\text{Jun 2010}})}_{\text{cohort 1}} - \underbrace{(Y_{i,\text{Jul 2011}} - Y_{i,\text{Jun 2010}})}_{\text{cohorts 2–13}}.$$

**Twenty-four months post-separation ($\ell = +24$; calendar time: July 2012)**

$$\hat{\delta}_{1,24}: \quad \underbrace{(Y_{i,\text{Jul 2012}} - Y_{i,\text{Jun 2010}})}_{\text{cohort 1}} - \underbrace{(Y_{i,\text{Jul 2012}} - Y_{i,\text{Jun 2010}})}_{\text{cohorts 2–13}}.$$

[1] Analogous considerations apply to treated cohorts and are omitted for brevity.

[2] Had the observation window ended after twelve post-separation months, cohort 13 would always have remained in its pre-event period without contributing to the post-separation outcome. It therefore acts as the reference group that pins identification of the reform in the first 12 months. Therefore, after the first 12 months, we cannot isolate the pure reform effect.

**Cohort 2 (August 2010)** For cohort 2 the pre-period baseline is **July 2010** ($\ell = -1$), so $Y_{i,s} = Y_{i,\text{Jul 2010}}$ is held fixed across all horizons. The control pool narrows at longer horizons as later cohorts themselves separate.

**One month post-separation ($\ell = +1$; calendar time: September 2010)** The estimator compares the change between **September 2010** and **July 2010** for cohort 2 workers against the same change for cohorts 3–13, none of whom have yet separated:

$$\hat{\delta}_{2,1}: \quad \underbrace{(Y_{i,\text{Sep 2010}} - Y_{i,\text{Jul 2010}})}_{\text{cohort 2}} - \underbrace{(Y_{i,\text{Sep 2010}} - Y_{i,\text{Jul 2010}})}_{\text{cohorts 3–13}}.$$

**Twelve months post-separation ($\ell = +12$; calendar time: August 2011)**

$$\hat{\delta}_{2,12}: \quad \underbrace{(Y_{i,\text{Aug 2011}} - Y_{i,\text{Jul 2010}})}_{\text{cohort 2}} - \underbrace{(Y_{i,\text{Aug 2011}} - Y_{i,\text{Jul 2010}})}_{\text{cohorts 3–13}}.$$

**Twenty-four months post-separation ($\ell = +24$; calendar time: August 2012)**

$$\hat{\delta}_{2,24}: \quad \underbrace{(Y_{i,\text{Aug 2012}} - Y_{i,\text{Jul 2010}})}_{\text{cohort 2}} - \underbrace{(Y_{i,\text{Aug 2012}} - Y_{i,\text{Jul 2010}})}_{\text{cohorts 3–13}}.$$

**The Last Treated Cohort: Cohort 12 (June 2011)** A special case arises when the last of the treated cohorts—cohort 12, separating in **June 2011**—moves into non-employment. At that point there are no remaining not-yet-separated cohorts other than the reference cohort 13. Cohort 13 therefore serves as the sole control group for *all* post-separation horizons of cohort 12. The pre-period baseline for cohort 12 is **May 2011** ($\ell = -1$).

**One month post-separation ($\ell = +1$; calendar time: July 2011)**

$$\hat{\delta}_{12,1}: \quad \underbrace{(Y_{i,\text{Jul 2011}} - Y_{i,\text{May 2011}})}_{\text{cohort 12}} - \underbrace{(Y_{i,\text{Jul 2011}} - Y_{i,\text{May 2011}})}_{\text{cohort 13 only}}.$$

**Twelve months post-separation ($\ell = +12$; calendar time: June 2012)**

$$\hat{\delta}_{12,12}: \quad \underbrace{(Y_{i,\text{Jun 2012}} - Y_{i,\text{May 2011}})}_{\text{cohort 12}} - \underbrace{(Y_{i,\text{Jun 2012}} - Y_{i,\text{May 2011}})}_{\text{cohort 13 only}}.$$

**Twenty-four months post-separation ($\ell = +24$; calendar time: June 2013)**

$$\hat{\delta}_{12,24}: \quad \underbrace{(Y_{i,\text{Jun 2013}} - Y_{i,\text{May 2011}})}_{\text{cohort 12}} - \underbrace{(Y_{i,\text{Jun 2013}} - Y_{i,\text{May 2011}})}_{\text{cohort 13 only}}.$$

**Summary** Table B1 generalises these examples to all twelve cohorts and three post-separation horizons. The key pattern is that the control pool is largest for early cohorts—because many later cohorts have not yet separated—and shrinks to the single reference cohort 13 for later cohorts and longer horizons. For all cohorts, the pre-period baseline $Y_{i,s}$ is anchored at the month immediately before separation ($\ell = -1$).

Table B1: Untreated Cohorts by Separation Time and Post-Separation Horizon

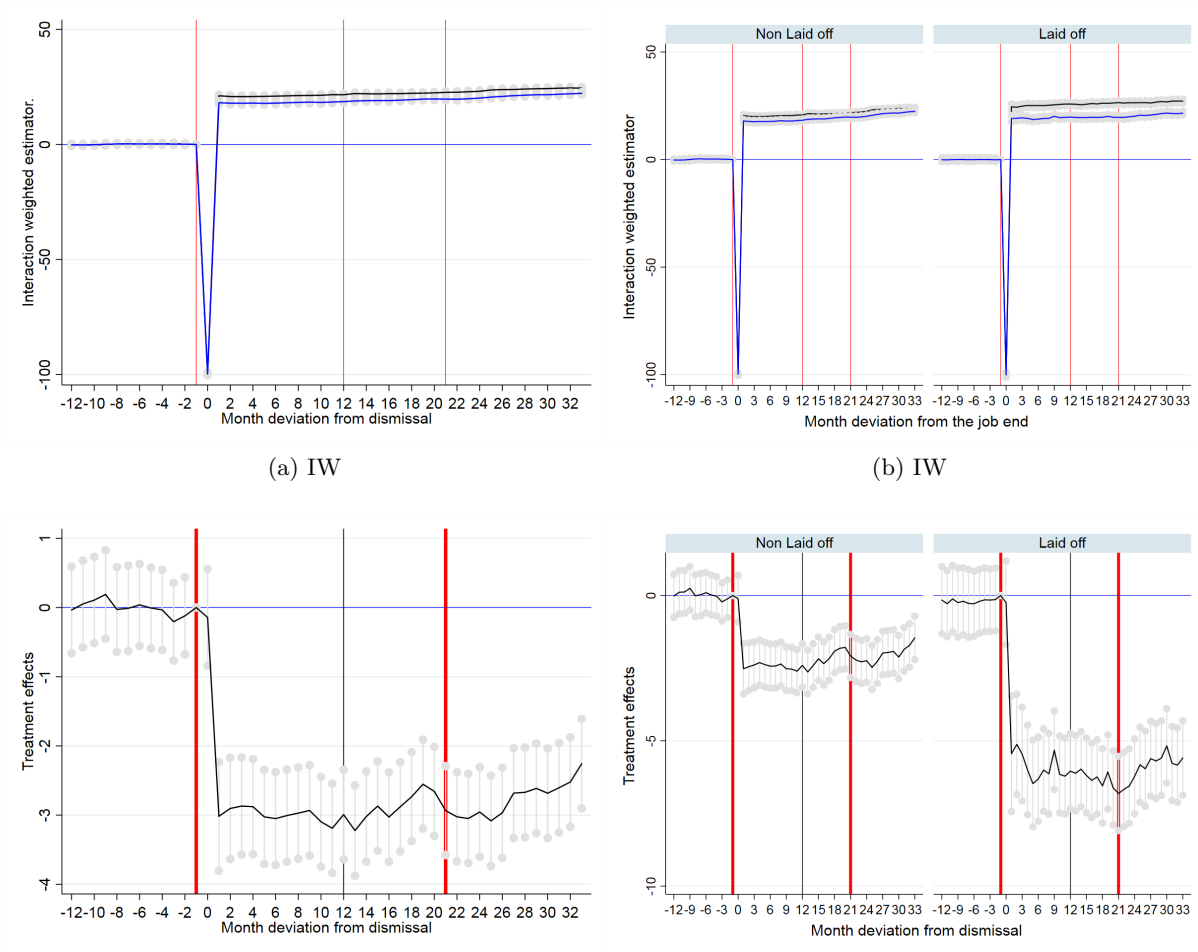
| | Post-separation horizon ($\ell$) | | |
|---|---|---|---|
| Separation time | $\ell = +1$ | $\ell = +12$ | $\ell = +24$ |
| C1: Jul 2010 | C2–C13 | C2–C13 | C2–C13 |
| C2: Aug 2010 | C3–C13 | C3–C13 | C3–C13 |
| C3: Sep 2010 | C4–C13 | C4–C13 | C4–C13 |
| C4: Oct 2010 | C5–C13 | C5–C13 | C5–C13 |
| C5: Nov 2010 | C6–C13 | C6–C13 | C6–C13 |
| C6: Dec 2010 | C7–C13 | C7–C13 | C7–C13 |
| C7: Jan 2011 | C8–C13 | C8–C13 | C8–C13 |
| C8: Feb 2011 | C9–C13 | C9–C13 | C9–C13 |
| C9: Mar 2011 | C10–C13 | C10–C13 | C10–C13 |
| C10: Apr 2011 | C11–C13 | C11–C13 | C11–C13 |
| C11: May 2011 | C12–C13 | C12–C13 | C12–C13 |
| C12: Jun 2011 | C13 only | C13 only | C13 only |

*Notes:* C13 = July 2011, the reference (latest-separated) cohort. At each cell, the control set $\mathcal{C}$ comprises all listed cohorts that have not yet separated at calendar time $e$. For $\ell \geq +13$, cohort C13 has itself separated but remains the reference group as per Sun and Abraham (2021, p. 186).

# C Supplementary Material: Section 5

## C.1 Results on Earnings for Individuals with Positive Earnings (Subsection 5.1)

Figure C1: Treatment Effects of the Reform on Hiring Earnings




(a) IW

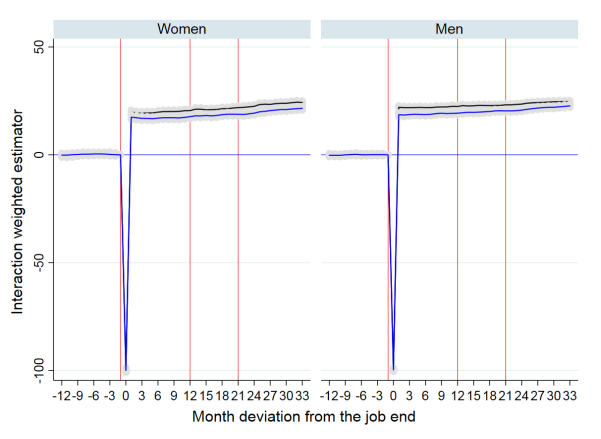
(b) IW

(c) D-IW

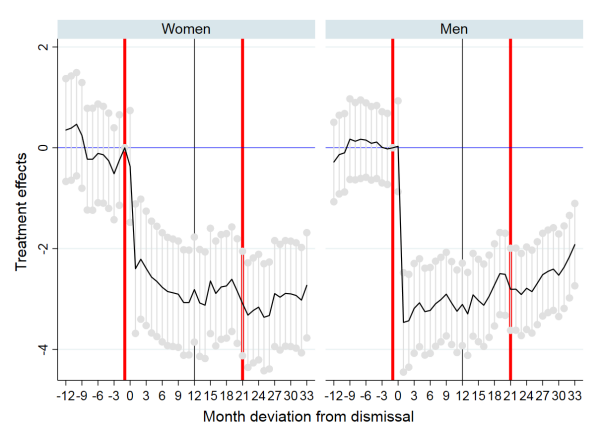
(d) D-IW

*Notes:* The gray bar around each point estimate refers to the 95% confidence interval obtained (for D-IW) from clustered bootstrapped standard errors (with 100 replications). The impacts are expressed in percent terms. Treated cohorts are in blue and untreated cohorts are in black.

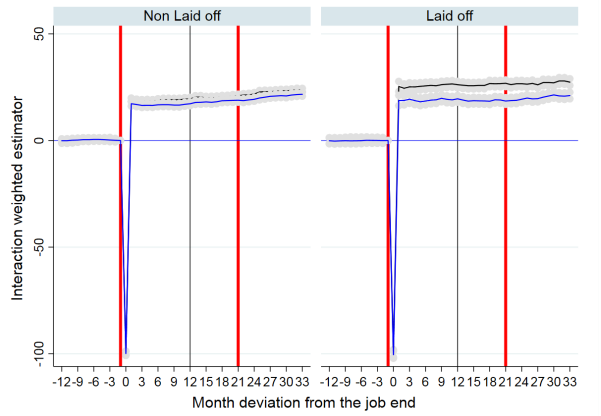
Figure C2: Treatment Effects of the Reform on Hiring Earnings by Gender



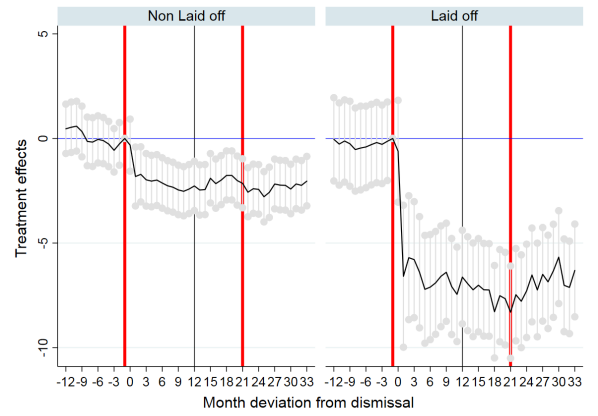

(a) IW: By Gender



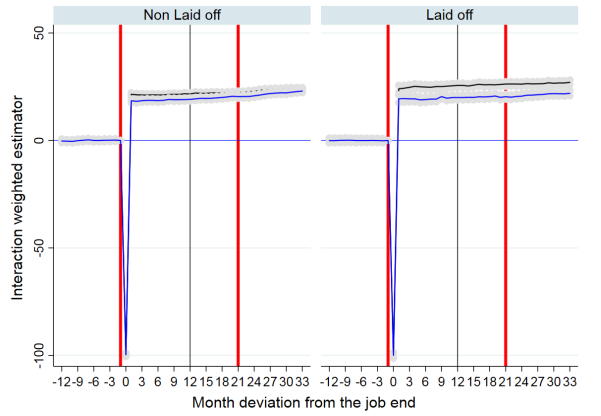

(b) D-IW: By Gender



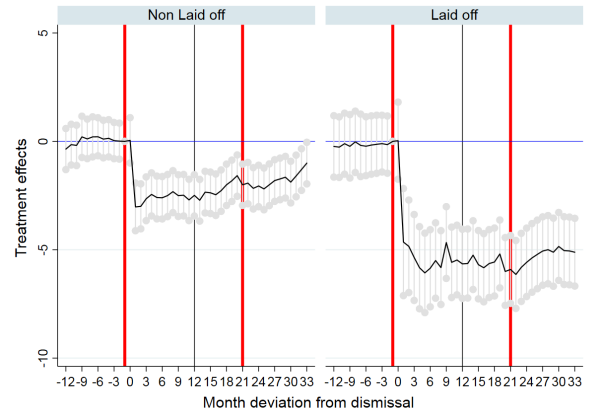

(c) IW: Women




(d) D-IW: Women



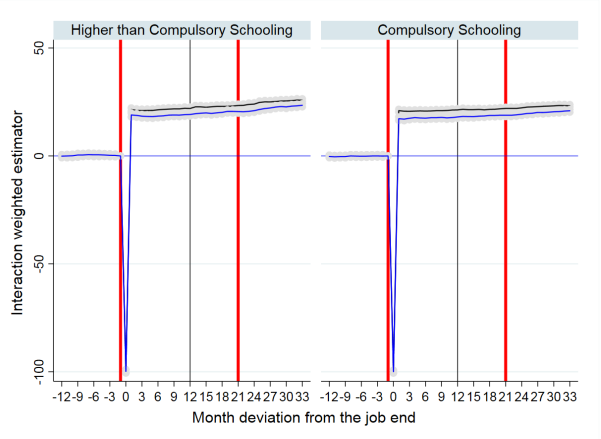

(e) IW: Men



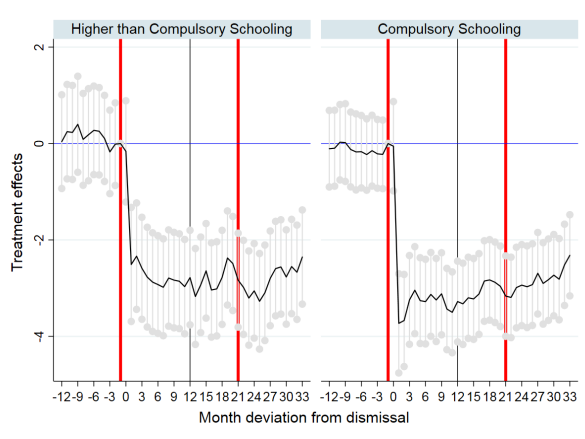

(f) D-IW: Men

*Notes:* The gray bar around each point estimate refers to the 95% confidence interval. The impacts are expressed in percent terms. Treated cohorts are in blue and untreated cohorts are in black.

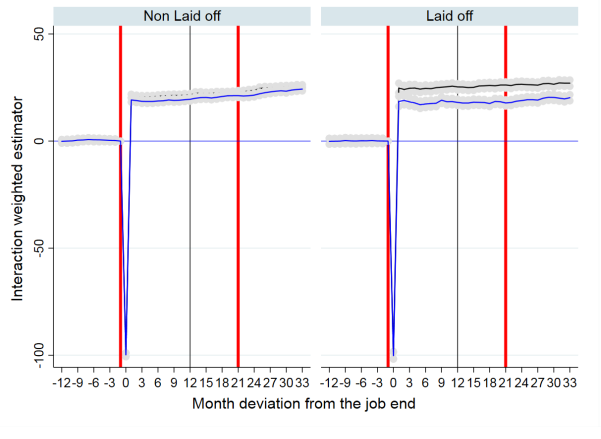
Figure C3: Treatment Effects of the Reform on Hiring Earnings by Education



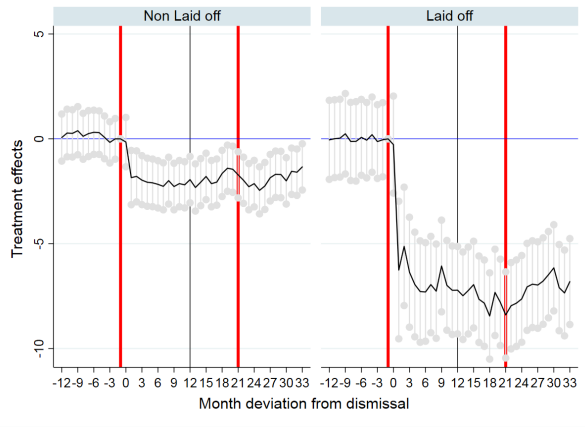

(a) IW



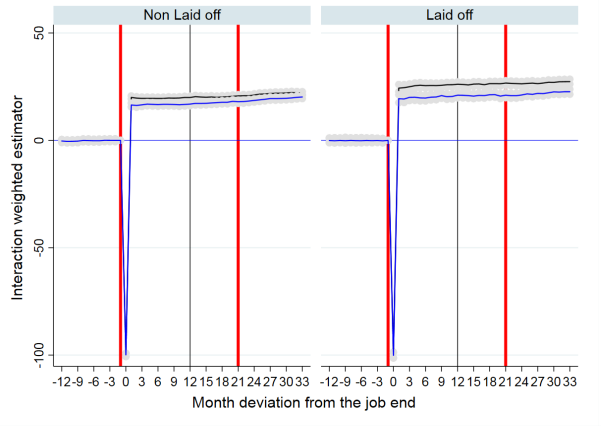

(b) D-IW



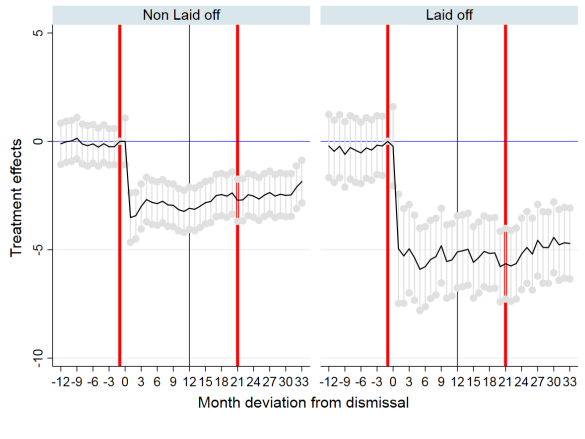

(c) IW: Higher education




(d) D-IW: Higher education



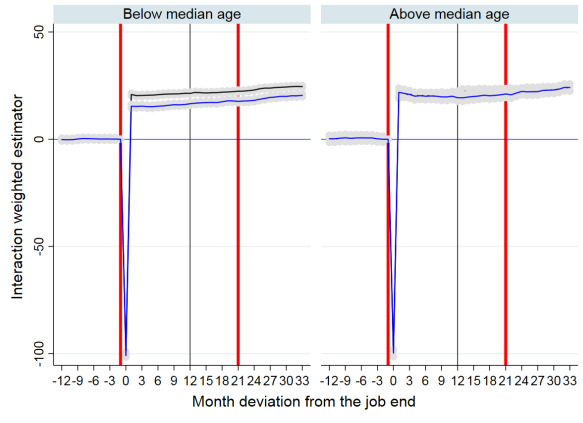

(e) IW: Compulsory Schooling



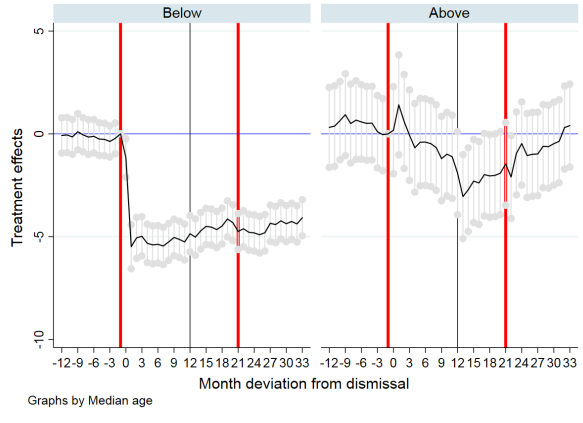

(f) D-IW: Compulsory Schooling

*Notes:* The gray bar around each point estimate refers to the 95% confidence interval. The impacts are expressed in percent terms. Treated cohorts are in blue and untreated cohorts are in black.

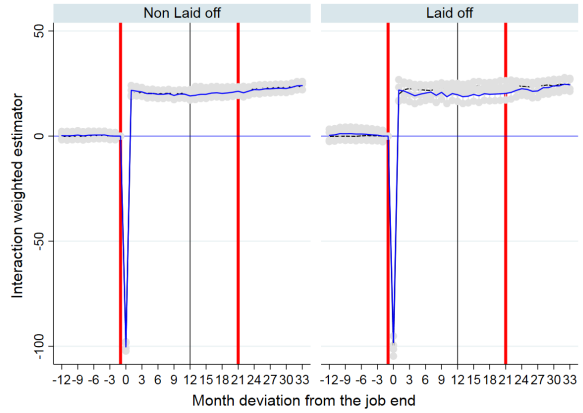
Figure C4: Treatment Effects of the Reform on Hiring Earnings by Age



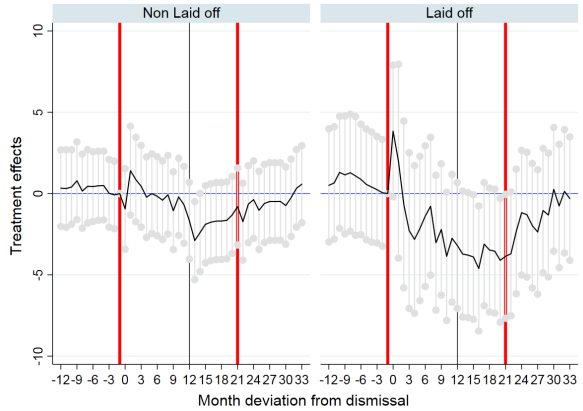

(a) IW



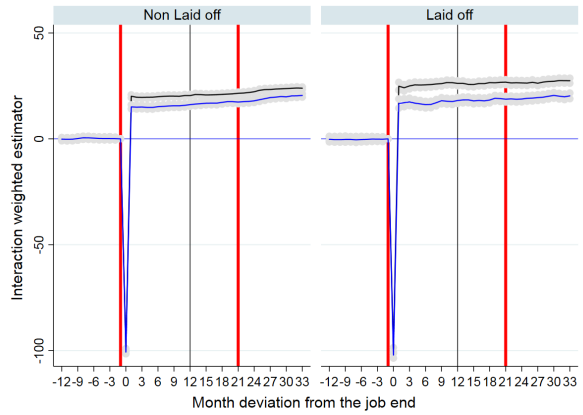

(b) D-IW



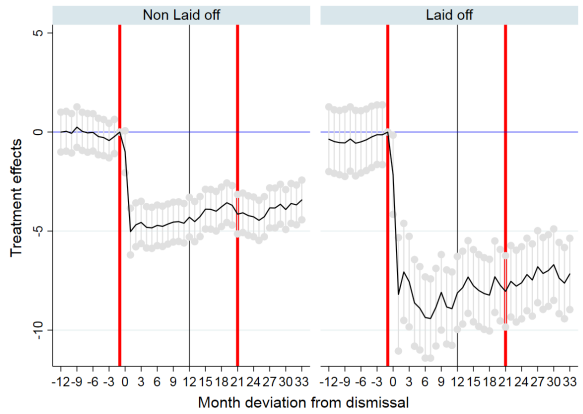

(c) IW: above median age




(d) D-IW: above median age



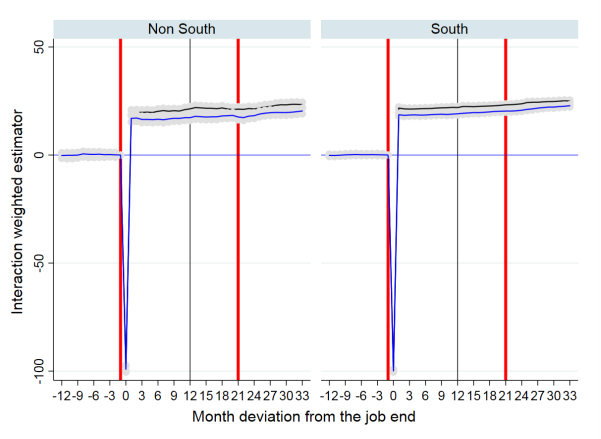

(e) IW: below median age



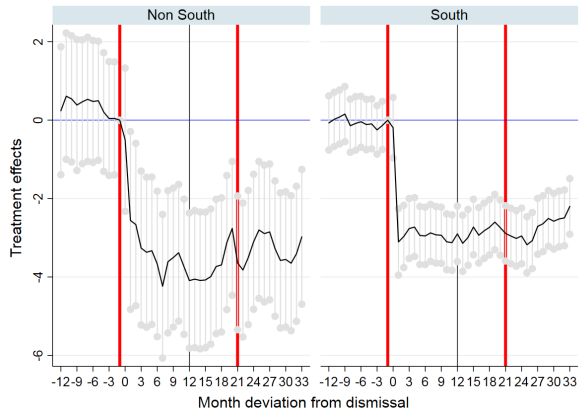

(f) D-IW: below median age

*Notes:* The gray bar around each point estimate refers to the 95% confidence interval. The impacts are expressed in percent terms. Treated cohorts are in blue and untreated cohorts are in black.

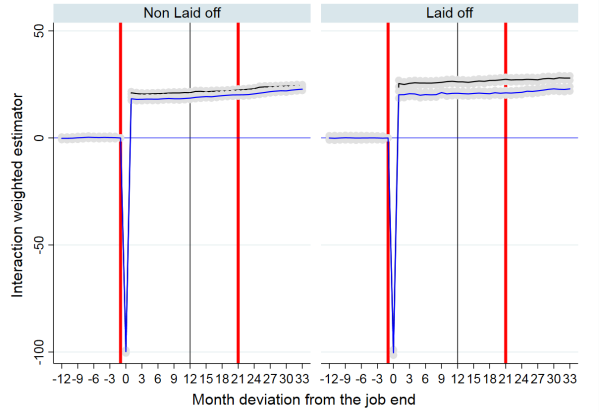
Figure C5: Treatment Effects of the Reform on Hiring Earnings by Workplace Location



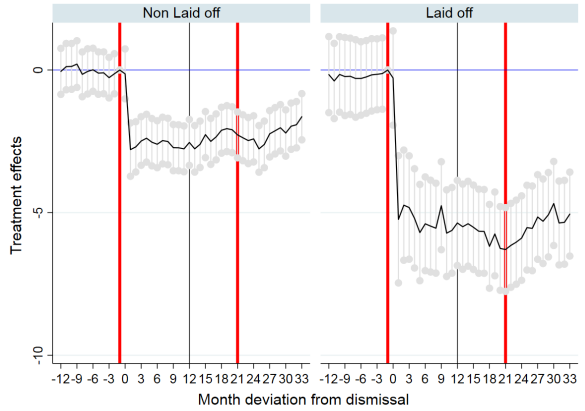

(a) IW



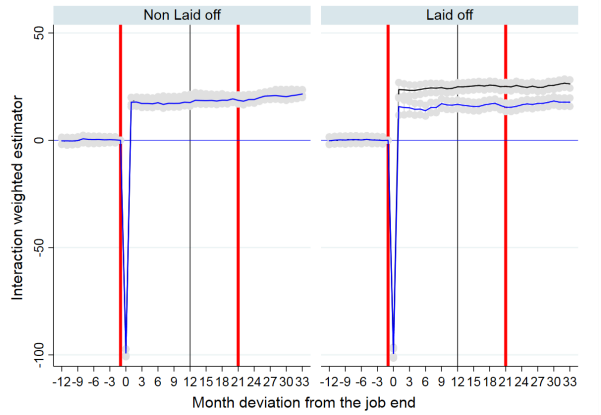

(b) D-IW



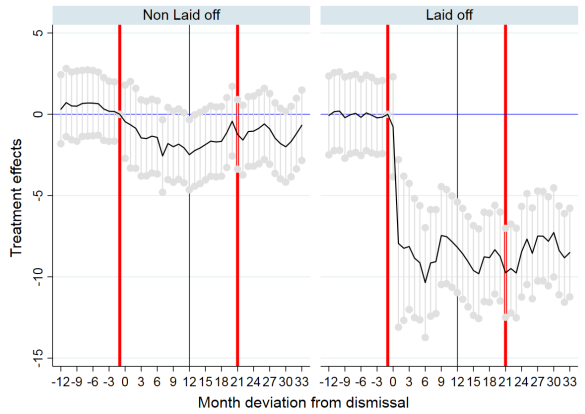

(c) IW: South of Italy




(d) D-IW: South



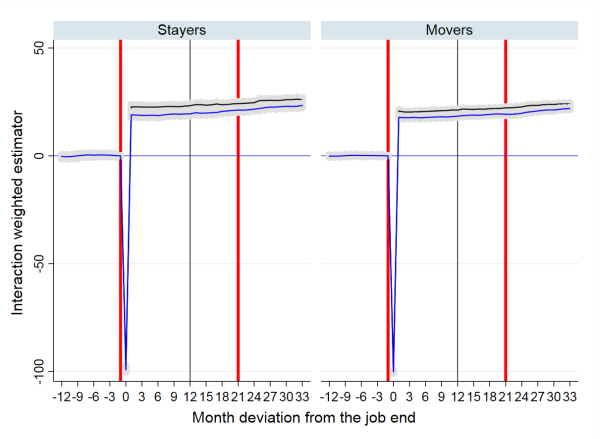

(e) IW: Centre/North



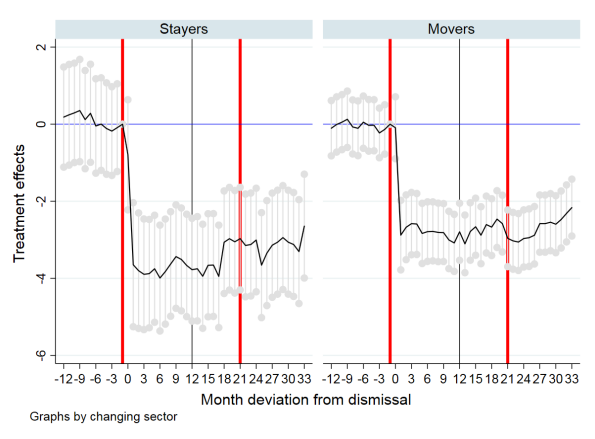

(f) D-IW: Centre/North

*Notes:* The gray bar around each point estimate refers to the 95% confidence interval. The impacts are expressed in percent terms. Treated cohorts are in blue and untreated cohorts are in black.

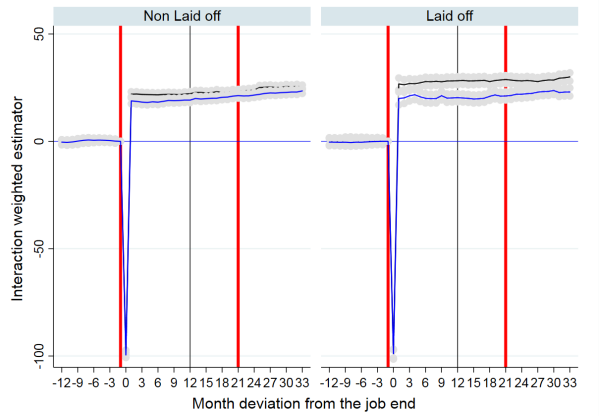
Figure C6: Treatment Effects of the Reform on Hiring Earnings by Mobility Across Industries



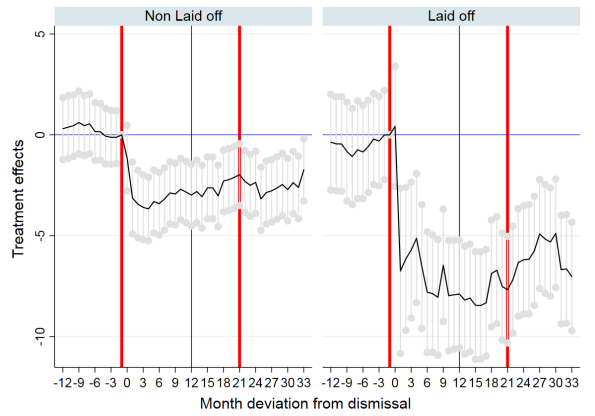

(a) IW: Sector switchers



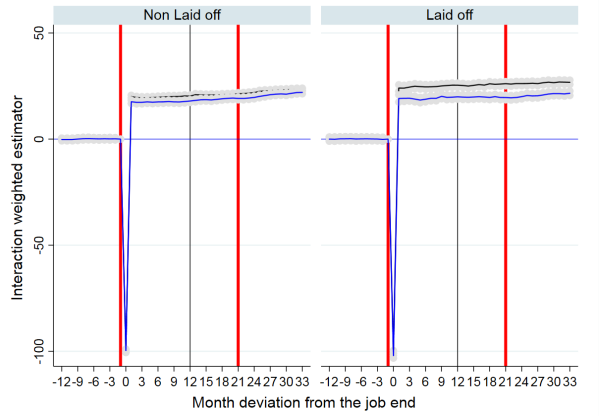

(b) D-IW: Sector switchers



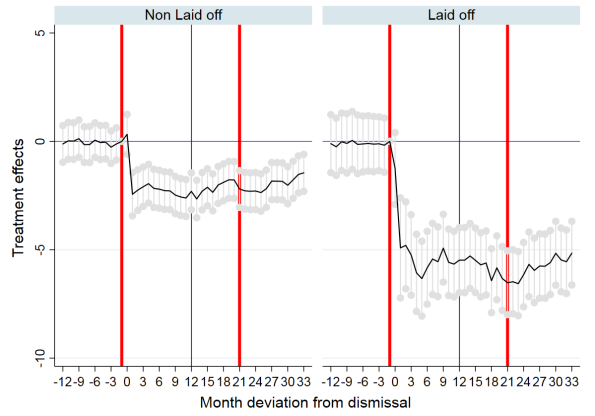

(c) IW: Sector stayers




(d) D-IW: Sector stayers



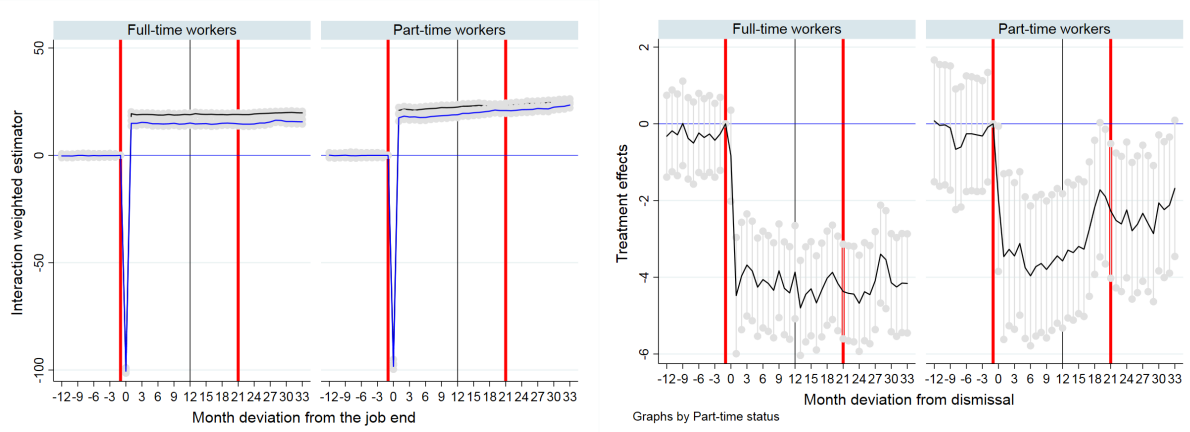

(e) IW: Sector movers



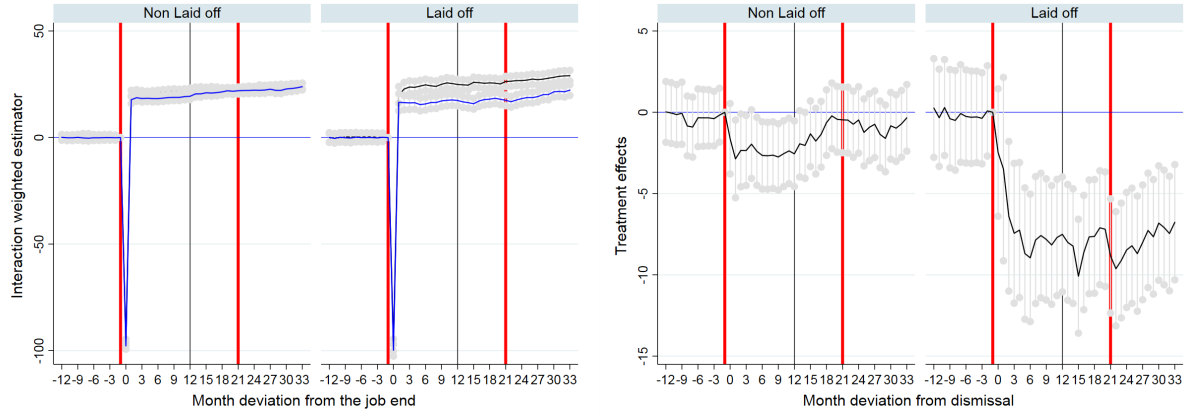

(f) D-IW: sector movers

*Notes:* The gray bar around each point estimate refers to the 95% confidence interval. The impacts are expressed in percent terms. Treated cohorts are in blue and untreated cohorts are in black.

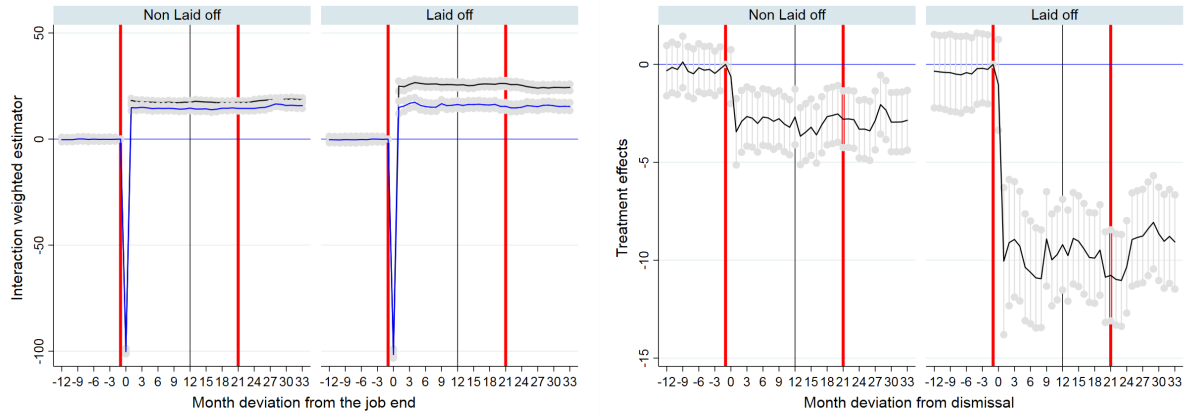
Figure C7: Treatment Effects of the Reform on Hiring Earnings by Part-time Status




(a) IW (b) D-IW




(c) IW: Part-time workers (d) D-IW: Part-time workers




(e) IW: Full-time workers (f) D-IW: Full-time workers

*Notes:* The gray bar around each point estimate refers to the 95% confidence interval. The impacts are expressed in percent terms. Treated cohorts are in blue and untreated cohorts are in black.

## C.2 A Sketch of the Model Used to Interpret the Results (Subsection 5.1)

Our framework borrows from Gibbons (1992)'s hybrid equilibrium model to offer an insight into how the re-hiring market might have changed as a result of the 2012 reform. We focus on displaced workers both before and after the reform.

A firm chooses a wage-effort package, $\{w, e\}$. Effort $e$ is costly to workers, who are either of

type $H$ or of type $L$, where $H$ and $L$ refer respectively to high and low ability (or skill). Low-ability workers are assumed to have steeper indifference curves (black curve in Figure C8) and a productivity profile given by the straight line $y(L, e)$, which increases with effort. High-ability workers have flatter indifference curves (colored curves) and a higher, and possibly steeper, productivity profile $y(H, e)$. Let $\phi$ (and 1-$\phi$) indicate the fraction of $H$- (and $L$)-type workers in the re-hiring market.

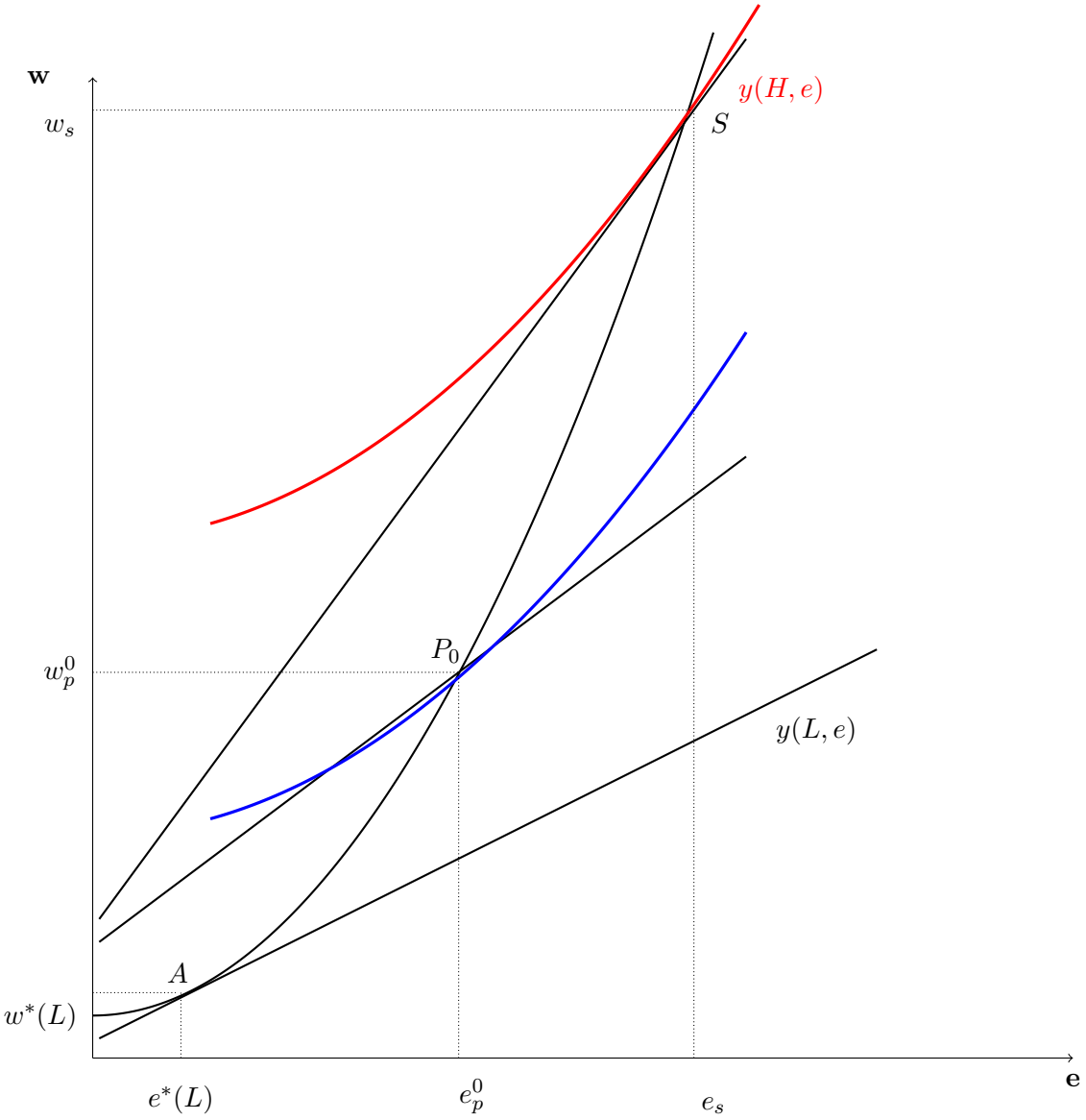



Figure C8: Hybrid and Separating Equilibria Before the Reform

In a world with perfect information, the firm could separate low- from high-skill workers by offering $\{w^*(L), e^*(L)\}$ and $\{w_s, e_s\}$ in Figure C8. The former package is chosen by $L$-type workers, while the latter is selected by their $H$-type counterparts. However, this perfect-information equilibrium is not what we are interested in, as it is unlikely to characterize the re-hiring market. In such a market, the firm may have use imprecise ways of ascertaining, workers' unobserved ability, It offers more likely an intermediate wage-effort contract, such as $\{w_p^b, e_p^b\}$. This identifies the hybrid pooling equilibrium. In $P_0$, in fact, besides all high-ability workers, the firm will also draw low-skill workers with probability $\psi$. With probability $1 - \psi$, other low-skill workers will be attracted to the contract in $A$, which delivers the same level of utility, since both packages lie on the same indifference curve in Figure C8. No $H$-type worker however would accept the $\{w^*(L), e^*(L)\}$ combination as it yields lower utility.[3]

[3]This package lies on the intermediate productivity profile, which is a weighted mean of $y(L, e)$ and $y(H, e)$,

Using Bayes' rule, the firm's belief to recruit a high-ability worker with the $\{w_p^b, e_p^b\}$ package is given by

$$\pi(H|\{w_p^b, e_p^b\}) = \frac{\phi}{\phi + (1-\phi)\psi}. \tag{C1}$$

From (C1), three observations are in order. First, and most important, $\pi > \phi$, as $\phi + (1-\phi)\psi < 1$ since all $H$-type workers choose the $\{w_p^b, e_p^b\}$ contract and only a fraction $\psi$ of low-ability types do. This means that, when offering this package, the firm's belief to hire a high-skill worker exceeds what it would expect if recruiting randomly in the re-hiring market. Second, when $\psi$ approaches zero, low-skill workers rarely pool with their high-ability counterparts. Third, as $\psi$ approaches one, low-ability workers always pool with high-ability types. In such a case, the firm's posterior belief approaches the prior $\phi$.

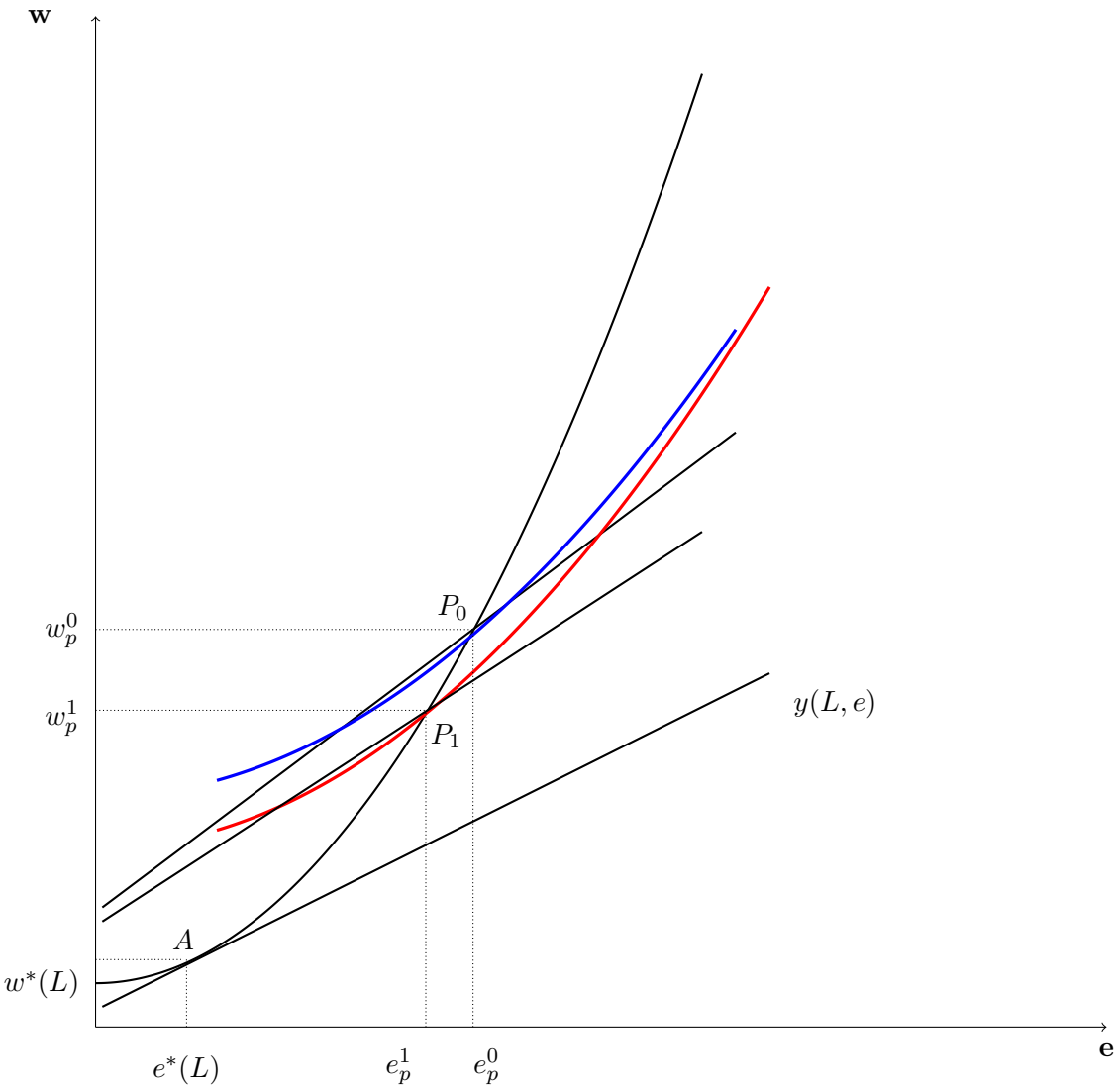



Figure C9: Hybrid Equilibria Before and After the Reform

For $w_p^b$ to be an equilibrium wage, it must be that $w_p^b < y(H, e_p^b)$, where

$$w_p^b = \pi(H|w_p^b, e_p^b)y(H, e_p^b) + (1-\pi)(H|w_p^b, e_p^b)y(L, e_p^b). \tag{C2}$$

Equation (C2) determines the unique value of $\psi$ consistent with the hybrid equilibrium. The level of $w_p^b$ that solves for (C2) must be on the same $L$-type worker's indifference curve of the

with weights that depend on firm beliefs about the proportion of low and high skilled workers in the pooling equilibrium, $q$ and $1-q$, respectively (see below).

$\{w^*(L), e^*(L)\}$ package. Let $q$ be the probability that guarantees $w_p^b = qy(H, e_p^b) + (1-q)y(L, e_p^b)$. This probability must equal $\pi(H|w_p^b, e_p^b)$ to yield the equilibrium wage. After substituting it into (C1), we obtain $q = \frac{\phi}{\phi+(1-\phi)\psi}$ and $\psi = \frac{\phi(1-q)}{(1-\phi)q}$. Since $w_p^b < y(H, e_p^b)$, then $e_p^b < e_s$, where $e_s$ is the level of effort in the $\{w_s, e_s\}$ combination, which emerges in the separating equilibrium.[4]

Figure C9 captures the new environment after the enactment of the Fornero reform by showing an alternative pooling contract $\{w_p^a, e_p^a\}$, with $w_p^a < w_p^b$ and $e_p^a < e_p^b$. The impact of the policy $R$ on the probability of offering a pooling contract to low-ability workers, $\psi$, consistent with the hybrid equilibrium is ambiguous as shown in the following expression

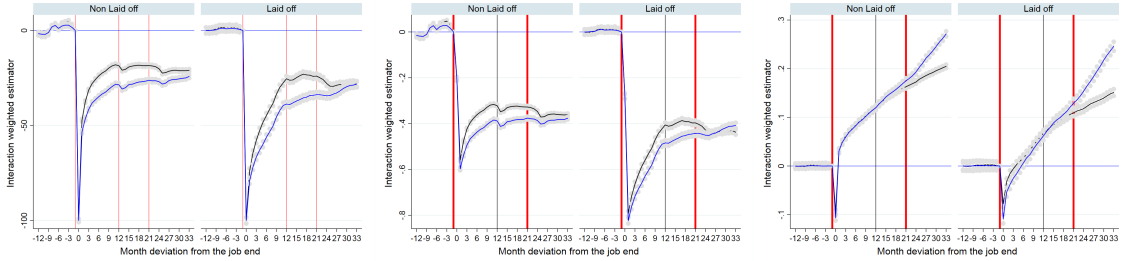
$$\underset{(+)}{\frac{\partial \psi}{\partial R}} = \underset{(+)}{\frac{\partial \psi}{\partial \phi}} \underset{(-)}{\frac{d\phi}{dR}} + \underset{(-)}{\frac{\partial \psi}{\partial q}} \underset{(-)}{\frac{dq}{dR}}. \tag{C3}$$

This means that the new $\{w, e\}$ equilibrium in Figure C9 could be below or above $P_0$.

[4] As $e_p$ tends to $e_s$, the probability $q$ approaches one and $\psi$ converges to zero.

## C.3 IW Estimates and Treated and Control Groups Referred to in Subsection 5.2

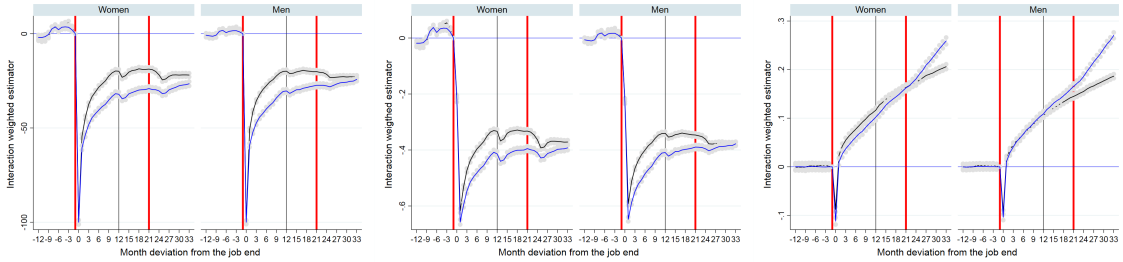
Figure C10: Treatment Effects of the Reform on Labor Market Outcomes by Laid-off Status



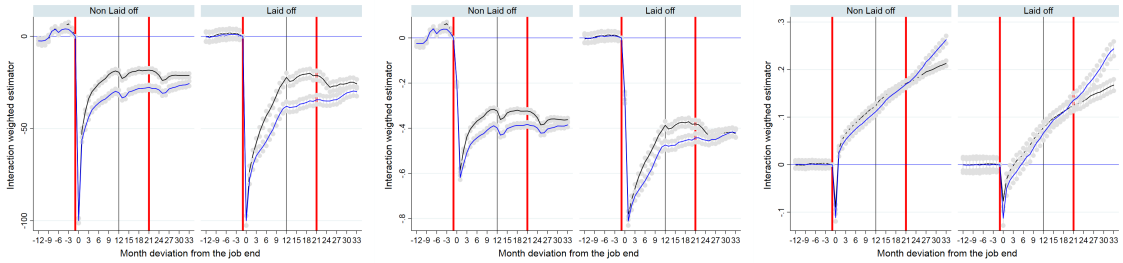

(a) IW: Hiring Earnings (b) IW: Emp. Probability (c) IW: Open-ended job Prob.

*Notes:* The gray bar around each point estimate refers to the 95% confidence interval. For hiring earnings in panel (a), the impacts are expressed in percent terms, while in the other two panels the estimates are expressed in percentage point terms. Treated cohorts are in blue and untreated cohorts are in black.

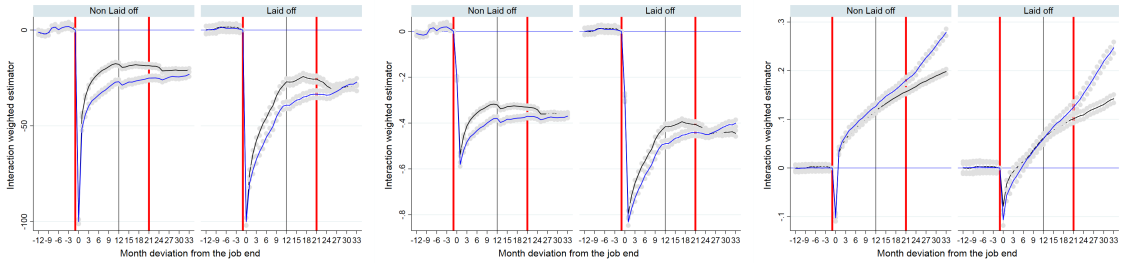
Figure C11: Treatment Effects of the Reform on Labor Market Outcomes by Gender




(a) IW: Hiring Earnings (b) IW: Emp. Probability (c) IW: Open-ended job Prob.



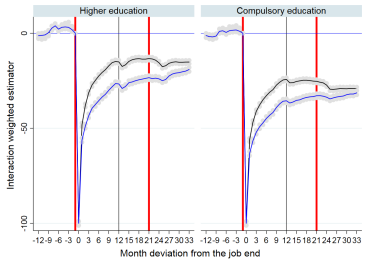

(d) Women: Hiring Earnings (e) Women: Empl. Probability (f) Women: Open-ended job Prob.



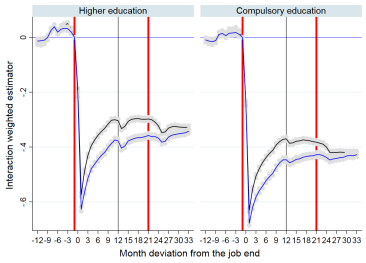

(g) Men: Hiring Earnings (h) Men: Emp. Probability (i) Men: Open-ended job Prob.

*Notes:* The gray bar around each point estimate refers to the 95% confidence interval. For hiring earnings in panels (a), (d) and (g), the impacts are expressed in percent terms, while in all the other panels the estimates are expressed in percentage point terms. Treated cohorts are in blue and untreated cohorts are in black.

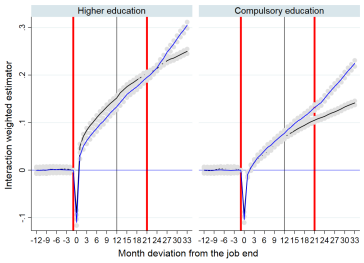
Figure C12: Treatment Effects of the Reform on Labor Market Outcomes by Education



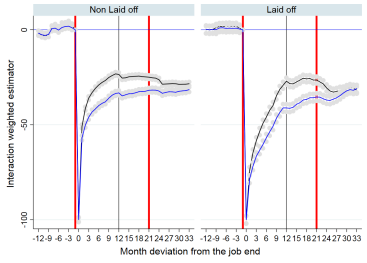

(a) IW: Hiring Earnings



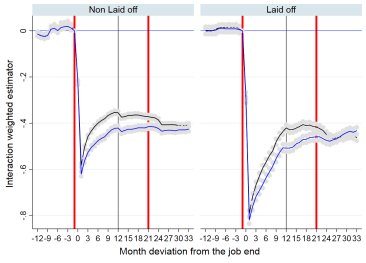

(b) IW: Emp. Probability



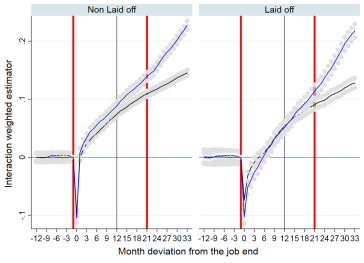

(c) IW: Open-ended job Prob.



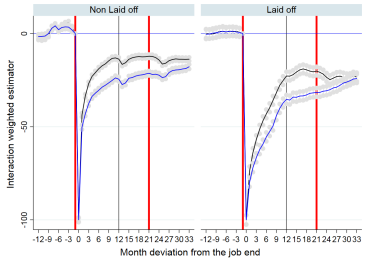

(d) Compulsory: Hiring Earnings



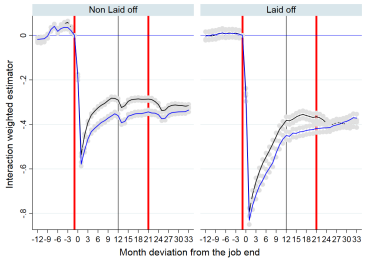

(e) Compulsory: Emp. Probability



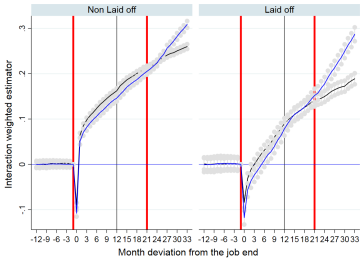

(f) Compulsory: Open-ended job Prob.




(g) Higher Education: Hiring Earnings



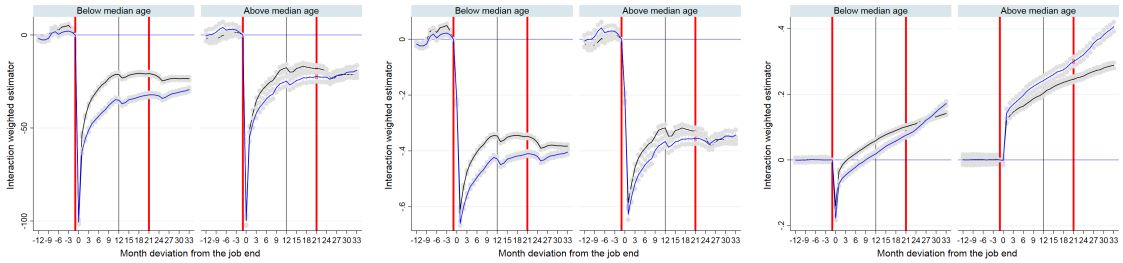

(h) Higher Education: Emp. Probability



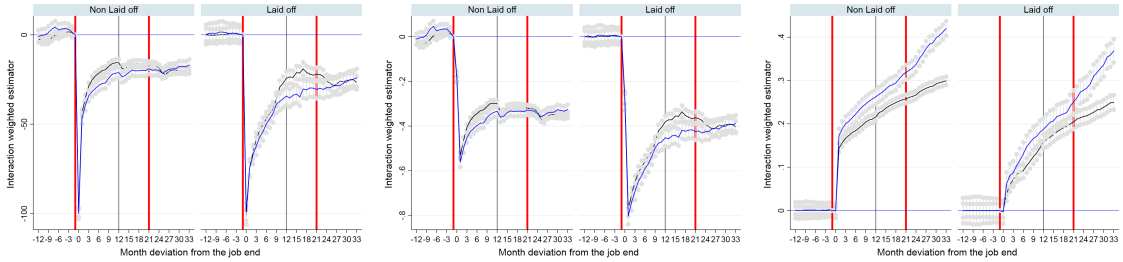

(i) Higher Education: Open-ended Prob.

*Notes:* The gray bar around each point estimate refers to the 95% confidence interval. For hiring earnings in panels (a), (d) and (g), the impacts are expressed in percent terms, while in all the other panels the estimates are expressed in percentage point terms. Treated cohorts are in blue and untreated cohorts are in black.

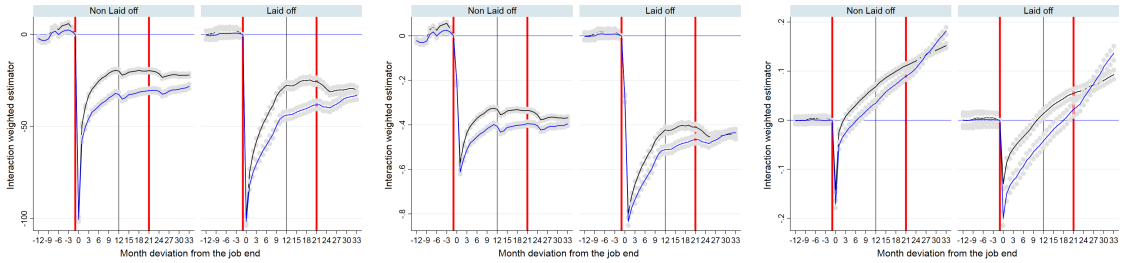
Figure C13: Treatment Effects of the Reform on Labor Market Outcomes by Age




(a) IW: Hiring Earnings (b) IW: Emp. Probability (c) IW: Open-ended job Prob.



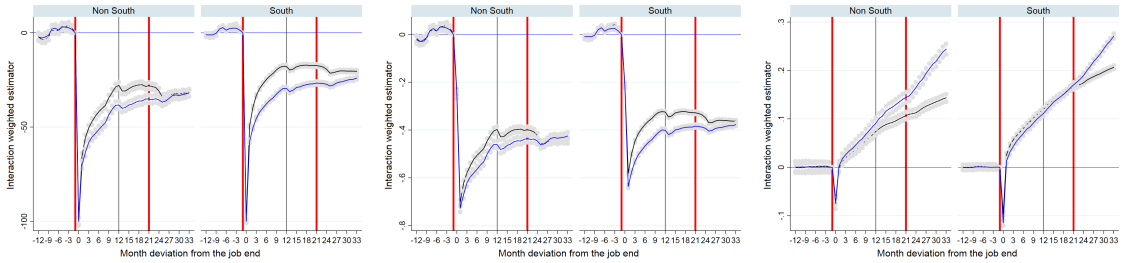

(d) Above: Hiring Earnings (e) Above: Emp. Probability (f) Above: Open-ended job Prob.



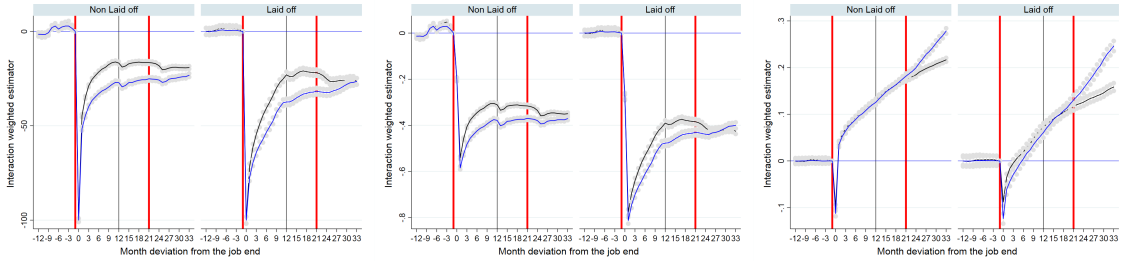

(g) Below: Hiring Earnings (h) Below: Emp. Probability (i) Below: Open-ended job Prob.

*Notes:* The gray bar around each point estimate refers to the 95% confidence interval. For hiring earnings in panels (a), (d) and (g), the impacts are expressed in percent terms, while in all the other panels the estimates are expressed in percentage point terms. Treated cohorts are in blue and untreated cohorts are in black.

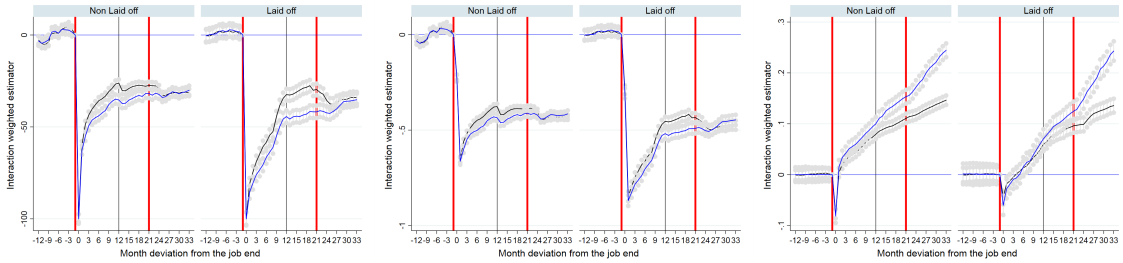
Figure C14: Treatment Effects of the Reform on Labor Market Outcomes by Workplace Location




(a) IW: Hiring Earnings (b) IW: Emp. Probability (c) IW: Open-ended job Prob.



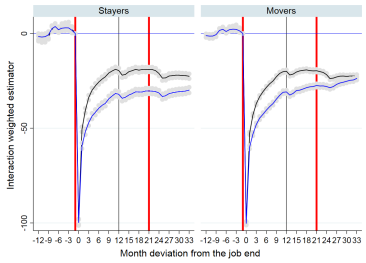

(d) South: Hiring Earnings (e) South: Emp. Probability (f) South: Open-ended job Prob.



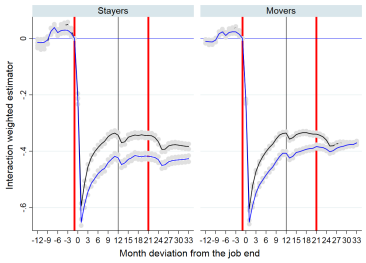

(g) Centre/North: Hiring Earnings (h) Centre/North: Emp. Probability (i) Centre/North: Open-ended job Prob.

*Notes:* The gray bar around each point estimate refers to the 95% confidence interval. For hiring earnings in panels (a), (d) and (g), the impacts are expressed in percent terms, while in all the other panels the estimates are expressed in percentage point terms. Treated cohorts are in blue and untreated cohorts are in black.

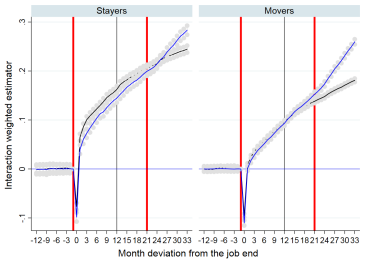
Figure C15: Treatment Effects of the Reform on Labor Market Outcomes by Mobility Across Industries



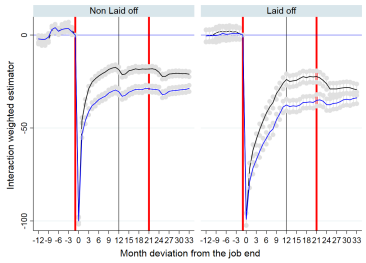

(a) IW: Hiring Earnings



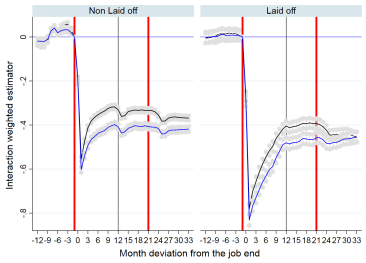

(b) IW: Emp. Probability



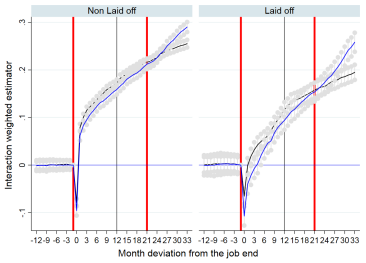

(c) IW: Open-ended job Prob.



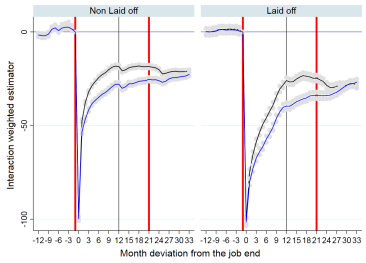

(d) Stayers: Hiring Earnings



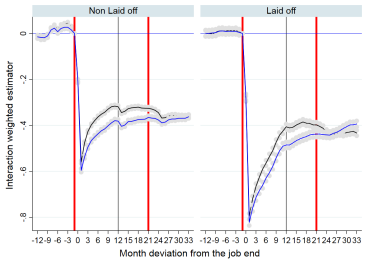

(e) Stayers: Emp. Probability



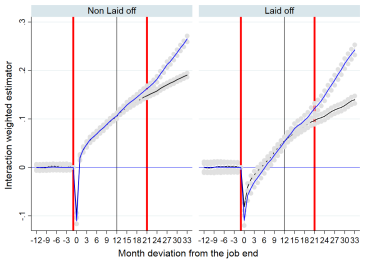

(f) Stayers: Open-ended job Prob.




(g) Movers: Hiring Earnings



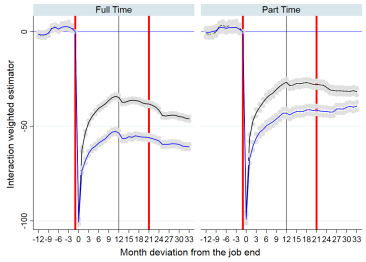

(h) Movers: Emp. Probability



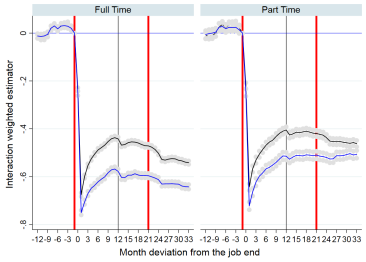

(i) Movers: Open-ended job Prob.

*Notes:* The gray bar around each point estimate refers to the 95% confidence interval. For hiring earnings in panels (a), (d) and (g), the impacts are expressed in percent terms, while in all the other panels the estimates are expressed in percentage point terms. Treated cohorts are in blue and untreated cohorts are in black.

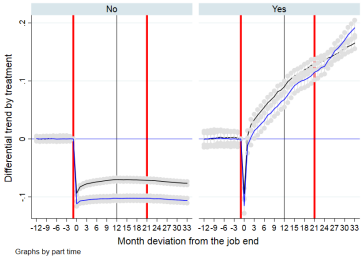
Figure C16: Treatment Effects of the Reform on Labor Market Outcomes by Part-time Status



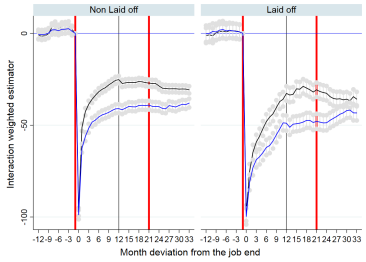

(a) IW: Hiring Earnings



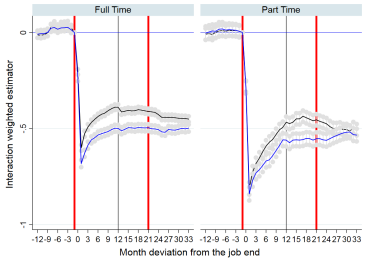

(b) IW: Emp. Probability



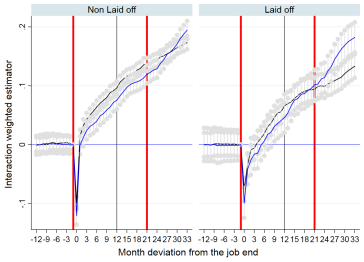

(c) IW: Open-ended job Prob.



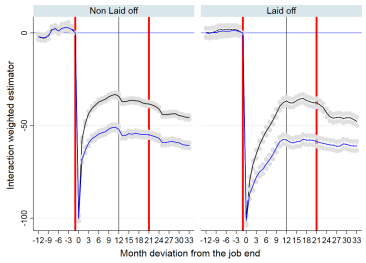

(d) PT: Hiring Earnings



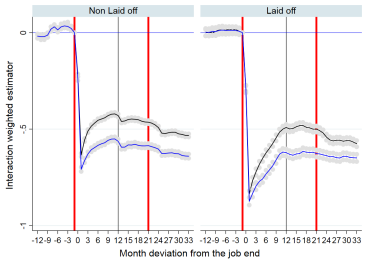

(e) PT: Emp. Probability



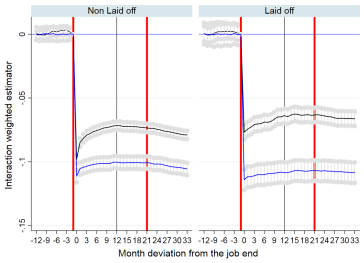

(f) PT: Open-ended job Prob.




(g) FT: Hiring Earnings




(h) FT: Emp. Probability

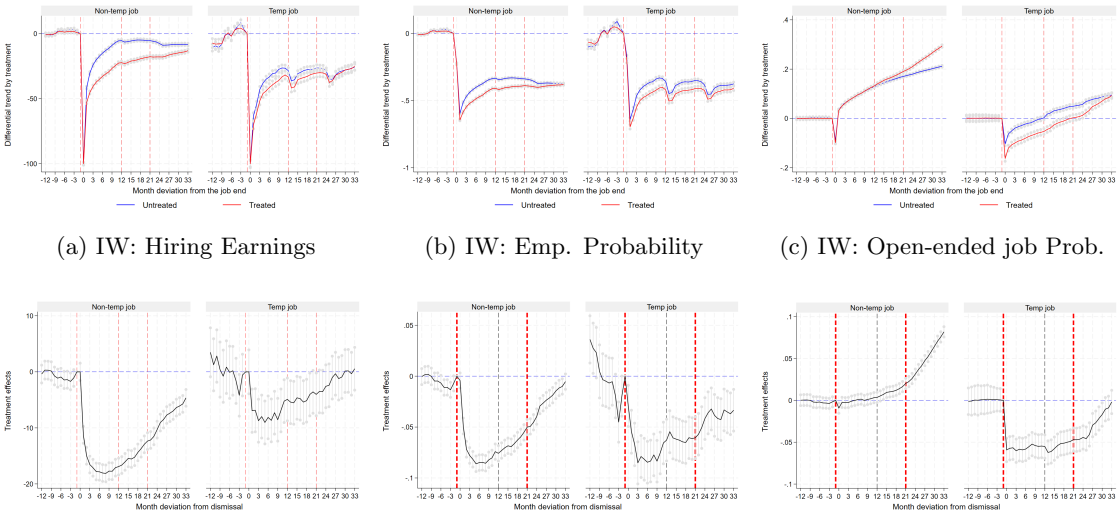



(i) FT: Open-ended job Prob.

*Notes:* The gray bar around each point estimate refers to the 95% confidence interval. For hiring earnings in panels (a), (d) and (g), the impacts are expressed in percent terms, while in all the other panels the estimates are expressed in percentage point terms. Treated cohorts are in blue and untreated cohorts are in black.

## C.4 Results for Temporary, Non-temporary workers (Subsection 5.2)

Figure C17: Treatment Effects of the Reform on Labor Market Outcomes: non-temporary workers versus temporary workers




(a) IW: Hiring Earnings (b) IW: Emp. Probability (c) IW: Open-ended job Prob.

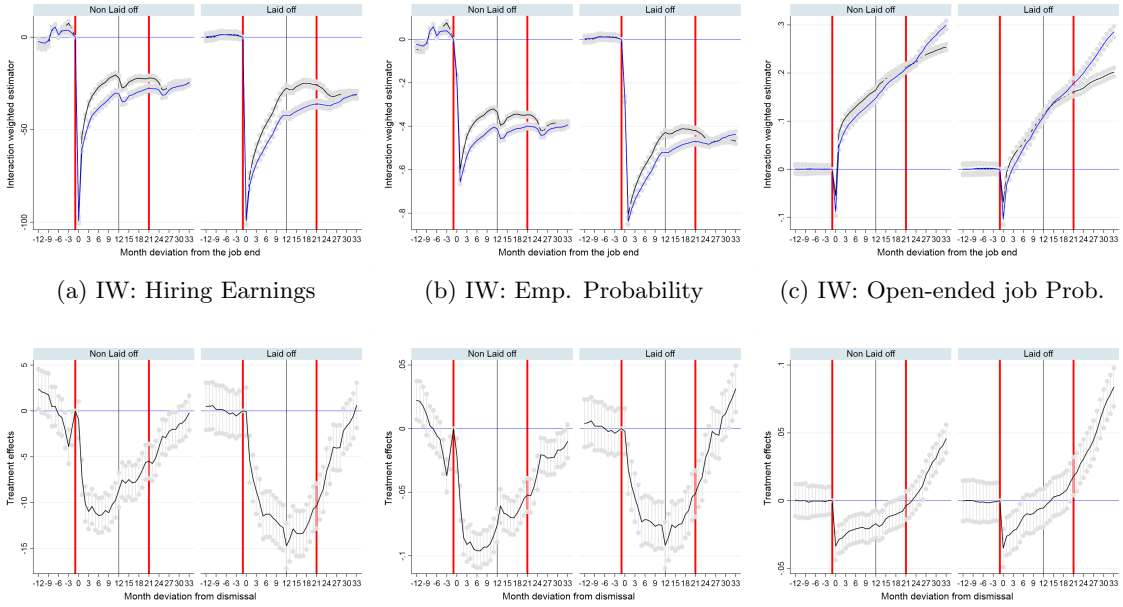



(d) D-IW: Hiring Earnings (e) D-IW: Emp. Probability (f) D-IW: Open-ended job Prob.

*Notes:* The gray bar around each point estimate refers to the 95% confidence interval. For hiring earnings in panels (a) and (d), the impacts are expressed in percent terms, while in the other four panels the estimates are expressed in percentage point terms. Treated cohorts are in red and untreated cohorts are in blue.

## C.5 Results for Quitters, Non-quitters (Subsection 5.2)

Figure C18: Treatment Effects of the Reform on Labor Market Outcomes: Non-quitters by Layoffs

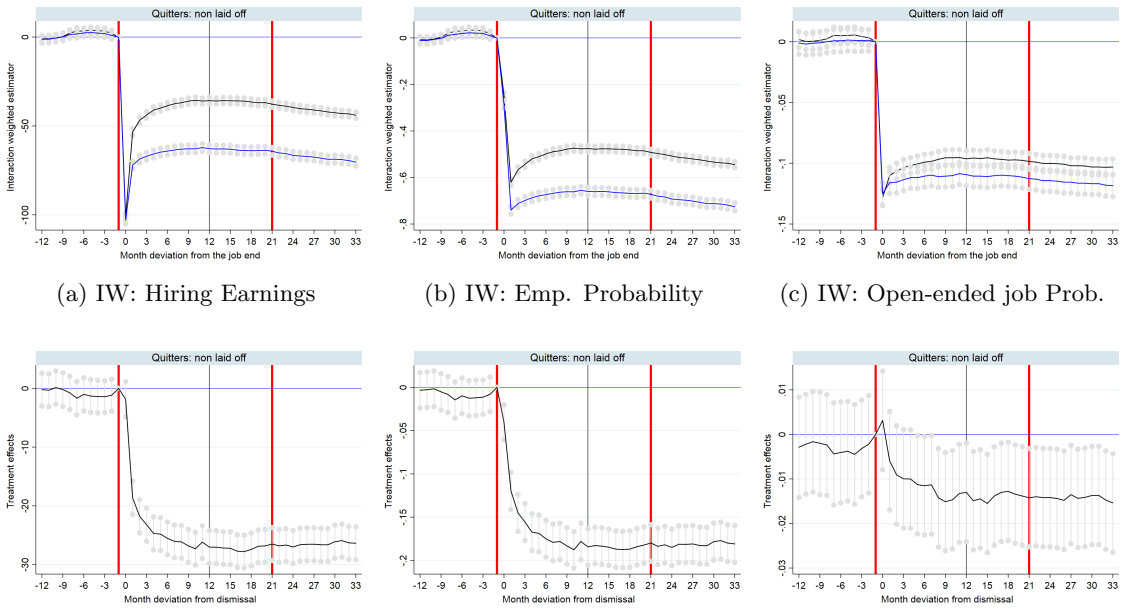



(a) IW: Hiring Earnings (b) IW: Emp. Probability (c) IW: Open-ended job Prob.

(d) D-IW: Hiring Earnings (e) D-IW: Emp. Probability (f) D-IW: Open-ended job Prob.

*Notes:* The gray bar around each point estimate refers to the 95% confidence interval. For hiring earnings in panels (a) and (d), the impacts are expressed in percent terms, while in the other four panels the estimates are expressed in percentage point terms. Treated cohorts are in blue and untreated cohorts are in black.

Figure C19: Treatment Effects of the Reform on Labor Market Outcomes: Quitters

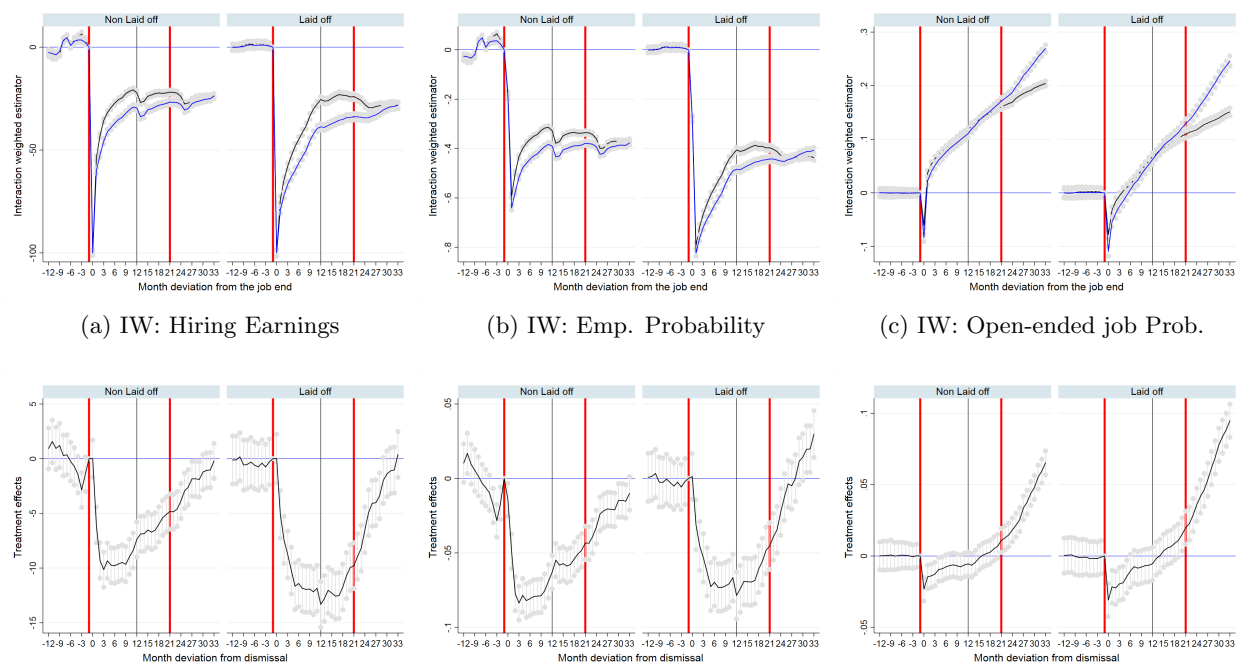



(a) IW: Hiring Earnings (b) IW: Emp. Probability (c) IW: Open-ended job Prob.

(d) D-IW: Hiring Earnings (e) D-IW: Emp. Probability (f) D-IW: Open-ended Prob.

*Notes:* The gray bar around each point estimate refers to the 95% confidence interval. For hiring earnings in panels (a) and (d), the impacts are expressed in percent terms, while in the other four panels the estimates are expressed in percentage point terms. Treated cohorts are in blue and untreated cohorts are in black.

Figure C20: Treatment Effects of the Reform on Labor Market Outcomes: Excluding Quitters by Layoffs Status




(a) IW: Hiring Earnings (b) IW: Emp. Probability (c) IW: Open-ended job Prob.

(d) D-IW: Hiring Earnings (e) D-IW: Emp. Probability (f) D-IW: Open-ended job Prob.

*Notes:* The gray bar around each point estimate refers to the 95% confidence interval. For hiring earnings in panels (a) and (d), the impacts are expressed in percent terms, while in the other four panels the estimates are expressed in percentage point terms. We exclude those who are quitters at the major event. We are comparing those who were laid-off with those who were not (these workers were in temporary jobs). Treated cohorts are in blue and untreated cohorts are in black.

## C.6 Synthetic Difference-in-Differences (Subsection 5.3)

In the main text, we employ the SDiD estimator proposed by Arkhangelsky et al. (2021), which combines features of synthetic control and difference-in-differences by introducing reweighting over both units and time. The estimator is doubly robust and requires weaker identifying assumptions. As detailed in Arkhangelsky et al. (2021), SDID balances pre-treatment averages in expectation rather than achieving a perfect pre-treatment fit, making it less prone to overfitting than traditional synthetic control. Thus, the weights need not make pre-treatment trends identical but ensure that they are parallel on average.

For comparability with our baseline results, the weights are constructed using individual and time (event period) fixed effects. We do not exploit the staggered structure of job termination and treatment (control) is assigned if the worker belongs to the treated (untreated) cohorts.

Let $Y = (Y_{it})$ denote the $N \times T$ matrix of observed outcomes for worker $i = 1, \ldots, N$ and event periods $t = 1, \ldots, T$, and let $D_{it} = 1$ if worker $i$ is treated at time (event period) $t$, and 0 otherwise. We assume:

$$Y_{it}(0) = \alpha_i + \gamma_t + \varepsilon_{it}, \quad Y_{it}(1) = Y_{it}(0) + \tau, \tag{C4}$$

where $\alpha_i$ and $\gamma_t$ are worker and time fixed effects, respectively, and $\tau$ is the treatment effect.

The SDID estimator combines worker weights $\omega$ and time weights $\lambda$ to balance pre-treatment outcomes:

$$\hat{\tau}^{SDID} = \left(\omega_{\text{tr}}^{\top} Y_{\text{post,tr}} \lambda_{\text{post}} - \omega_{\text{tr}}^{\top} Y_{\text{pre,tr}} \lambda_{\text{pre}}\right) - \left(\omega_{\text{co}}^{\top} Y_{\text{post,co}} \lambda_{\text{post}} - \omega_{\text{co}}^{\top} Y_{\text{pre,co}} \lambda_{\text{pre}}\right) \tag{C5}$$

$$\omega_{\text{tr}}^{\top} Y_{\text{pre,tr}} \lambda_{\text{pre}} \approx \omega_{\text{co}}^{\top} Y_{\text{pre,co}} \lambda_{\text{pre}}. \tag{C6}$$

In the implementation without covariates (i.e., with only workers and time fixed effects), worker and time weights are obtained from ridge-regularised least-squares problems and subject to $\omega \geq 0$, $\sum_i \omega_i = 1$, $\lambda \geq 0$, and $\sum_t \lambda_t = 1$. Two penalty parameters are introduced in the formula to help prevent overfitting (see page 4090 in Arkhangelsky et al. (2021)).

Including workers and time fixed effects implies that SDID operates on demeaned data: worker effects absorb level differences, and time effects absorb common shocks. After demeaning, worker weights align residual pre-treatment trends of treated and control workers, while time weights reweight pre-treatment periods to resemble treated periods. This reweighting minimises the concern that treated and control workers are exposed to different phases of the business cycle, by ensuring that their pre-treatment trends are comparable and that post-treatment comparisons are made between similar economic conditions. Hence, SDID can be viewed as analogous to a weighted two-way fixed-effects estimator that optimally balances pre-treatment outcomes via reweighting, rather than relying on unweighted averages as in standard difference-in-differences (see Figure 1 in Arkhangelsky et al. (2021)).

## C.7 Matched (with Propensity Score) Difference-in-Differences, Stacked Difference-in-Differences, and Other Estimators Robust to Heterogeneous Treatment Effects (Subsection 5.3)

### C.7.1 Matched (with Propensity Score) Difference-in-Differences

We follow the codes provided by Bertheau et al. (2023) as closely as possible. We deviate slightly from their approach because we do not have access to the same variables[5] and because, while they use never-treated workers (those who did not lose their job) as the control group, we use the full set of untreated cohorts as the control group in our one-to-one matching procedure.

We begin by identifying the cells for the matching algorithm. Whereas Bertheau et al. (2023) define cells by sector, gender, and year, we define them by sector, gender, and event period, as we cannot use calendar time. The propensity score is then estimated within each cell without imposing the restriction of continuous employment up to the event. While in Bertheau et al. (2023) the treatment is job loss compared with continued employment, in our setting the treatment is losing the job after the introduction of the EPL reform compared with losing the job before the reform. This defines the dependent variable in the probit regression used to estimate the propensity score.

We adapt the set of control variables used by Bertheau et al. (2023), in particular because we do not observe earnings but only hiring earnings. We therefore use lagged employment status instead of lagged earnings, as well as age; lagged experience; the interaction between region and time dummies (months), an indicator equal to 1 if the job is associated with a number of separations in a month above the bottom quartile of the distribution (conditional on age and region of birth); an indicator equal to 1 if the job is associated with net job flows (hirings minus separations) above the bottom quartile of the distribution (conditional on age and region of birth); an indicator equal to 1 if the job benefited from tax rebates above the bottom quartile of the distribution (conditional on age); and an indicator equal to 1 for being above the top quartile of the education distribution (conditional on age).

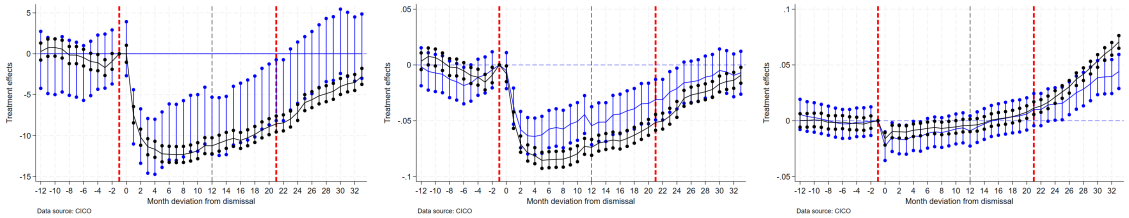
We use these indicator functions rather than the raw variables because distributional quartiles are more exogenous to workers' choices and capture more effectively the worker's relative position in, for instance, the process of finding a new job. For example, consider the distribution of job flows in each month or year: for each event period in which we observe a worker, we assign them to a percentile of the distribution conditional on age and region of birth. The idea is that job seekers face competition primarily from individuals of similar age within the same labour market. We proxy the local labour market using region of birth rather than region of work to avoid endogeneity related to mobility; however, the relative position in the distribution does not change if region of work is used instead. Being in different positions within

[5] They use a different administrative dataset, *LoSai*.

these distributions corresponds to facing different labour market conditions (slacker or tighter), as would be expected in the presence of business-cycle variation.

It is worth noting that these variables are already relatively well-balanced across treated and untreated cohorts even before matching. Nevertheless, we use them directly to estimate the propensity score, and—as in Bertheau et al. (2023)—we apply nearest-neighbour matching to obtain a one-to-one match. Once the one-to-one matching is completed, we estimate a two-way fixed effects model on the matched sample, also controlling for time fixed effects.

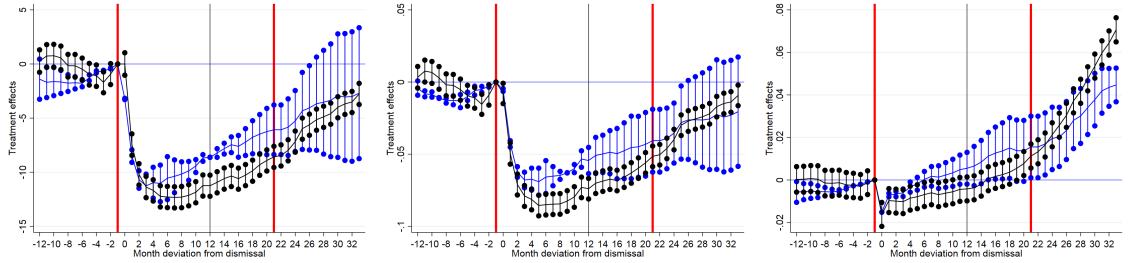
Figure C21: Differences Between (Propensity Score) Matched Difference in Differences and D-IW estimators




(a) Hiring Earnings (b) Emp. Probability (c) Open-ended job Prob.

*Notes:* For hiring earnings in panel (a), the impacts are expressed in percent terms, while in the other two panels the estimates are expressed in percentage point terms. Matched difference-in-differences estimates are reported in blue, D-IW estimates are in black.

Figure C22: Differences Between Stacked Difference in Differences and D-IW



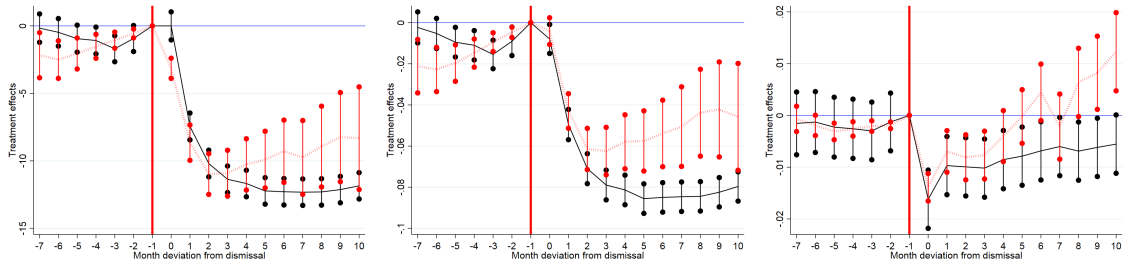

(a) Hiring Earnings (b) Emp. Probability (c) Open-ended job Prob.

*Notes:* For hiring earnings in panel (a), the impacts are expressed in percent terms, while in the other two panels the estimates are expressed in percentage point terms. Stacked difference-in-differences estimates are reported in blue, D-IW estimates are in black.

### C.7.2 Balanced Panel

Here, we compare our results with those obtained using the original Sun–Abraham estimator in a staggered event-study framework. To ensure an accurate comparison, we needed to adjust some aspects of our basic sample selection. Specifically, we now construct a balanced panel (rather than a balanced event study) in which the cohorts span from June 2010 to June 2011 for the untreated group, and from June 2012 to June 2013 for the treated group. As in Sun and Abraham (2021), owing to the staggered timing of job terminations, not all cohorts are necessarily observed in every period. This revised setup also implies that the estimates cannot extend beyond 11 months after job loss, as the last yet-to-be-treated cohort within the untreated group would otherwise also become treated. The estimates reported in Figure C23 uphold our benchmark results.

Figure C23: Differences Between D-IW in a Balanced Event Sample and D-IW in a Balanced Panel Sample (a la Sun-Abraham)




(a) Hiring Earnings (b) Emp. Probability (c) Open-ended job Prob.

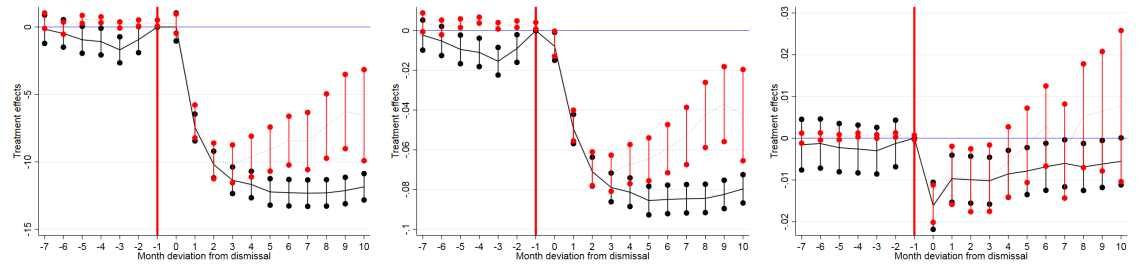
*Notes:* The gray bar around each point estimate refers to the 95% confidence interval obtained (for D-IW) from clustered bootstrapped standard errors (with 100 replications). For hiring earnings in panels (a) and (d), the impacts are expressed in percent terms, while in the other four panels the estimates are expressed in percentage point terms. Difference-in-interaction weighted estimator are in black and difference in Sun and Abraham with a balanced panel are in red (dot line)

We now adopt the estimator proposed by Callaway and Sant'Anna (2021). The earlier points regarding the balanced panel and the yet-to-be-treated cohort apply here as well. The Callaway–Sant'Anna doubly robust estimator requires less stringent modelling assumptions than the Sun–Abraham estimator, while still accounting for parallel trends after conditioning on covariates. However, the two procedures are expected to produce similar estimates when no covariates are included, as is the case in our setting. Indeed, Figure C24 clearly illustrates this similarity across all outcomes with our baseline estimates.

Including covariates yields comparable results. In Figure C25, we report our D-IW estimates, including additional control variables. These comprise age, gender, an indicator equal to one if the new job is located in a different region from the previous one, and an indicator equal to one if the worker's past experience is in the top quartile of the distribution conditional on age. We also include an indicator equal to one if the job is associated with a number of job separations in a month above the bottom quartile of the distribution, conditional on age and region of birth; an indicator equal to one if the job is linked to a number of net flows (hirings minus separations) above the bottom quartile of the distribution, conditional on age and region of birth; an indicator equal to one if the job benefited from tax rebates above the bottom quartile of the distribution, conditional on age; and two further indicators equal to one if the worker's education is in the top or bottom quartile of the distribution, respectively.

## C.8 The CATTs within each treatment group, averaged over the first 12 months after job termination (Subsection 5.3)

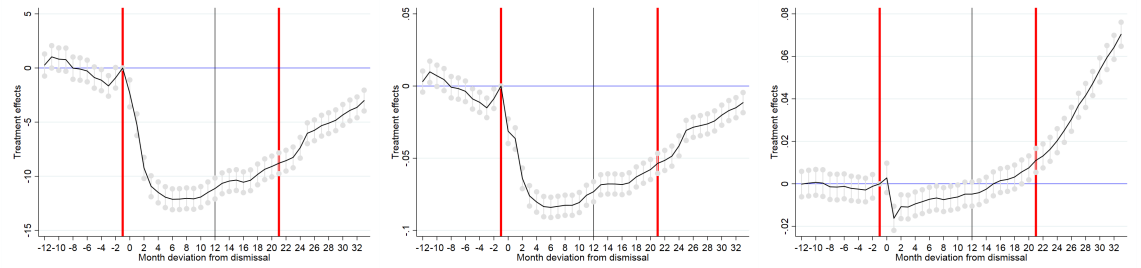
Figure C24: Differences Between D-IW in a Balanced Event Sample and D-IW in a Balanced Panel Sample (a la Callaway-Sant'Anna's)




(a) Hiring Earnings (b) Emp. Probability (c) Open-ended job Prob.

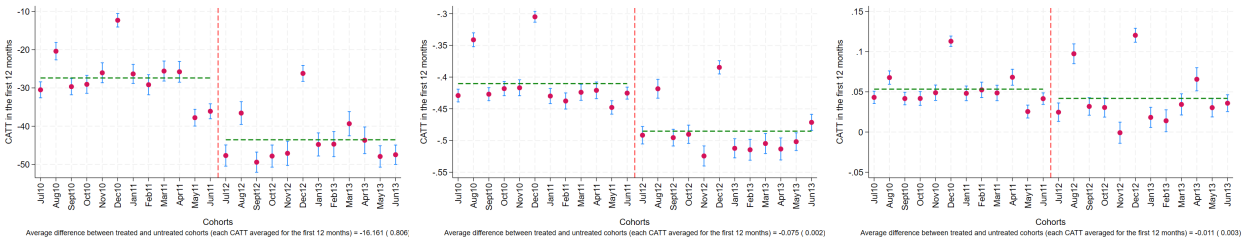
*Notes:* For hiring earnings in panel (a), the impacts are expressed in percent terms, while in the other two panels the estimates are expressed in percentage point terms. D-IW estimates are reported in black, those using the Callaway and Sant'Anna's estimator are reported in red.

Figure C25: D-IW when we include additional covariates




(a) Hiring Earnings (b) Emp. Probability (c) Open-ended job Prob.

Figure C26: CATT over the first 12 months within each treatment group




(a) Hiring Earnings (b) Employment Probability (c) Open-ended Job Prob.

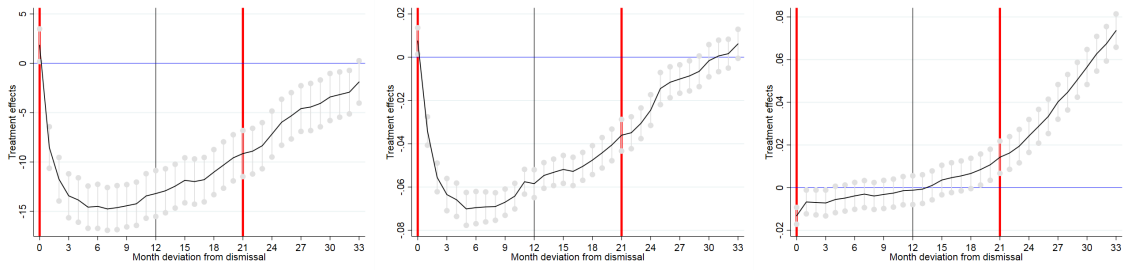
## C.9 Accounting for Trend Deviations in the Pre-event Periods (Subsection 5.3).

In the spirit of Rambachan and Roth (2023), we perform an analysis examining potential threats to our parallel trends assumption arising from differing macroeconomic factors between treated and untreated cohorts. Although both groups experience job loss during a recession, variations in the stock of unemployed individuals re-entering the workforce may affect the probability of securing new employment.

To address this, we re-run our regressions with the restriction that the magnitude of these time-varying confounds is equal to the largest deviation in outcomes between treated and untreated cohorts during the pre-event periods. This largest deviation is negative for employment probability (but zero for the permanent employment probability), indicating that the treated cohorts could have found themselves in less favourable circumstances compared to the untreated ones.

The graphs below demonstrate that our estimated coefficients[6] in the post-event periods remain statistically significant and negative, even under this restriction. Although not shown here, it is evident that in the first 12 months following job loss, the estimated impacts would be negative and statistically significant even if we quadrupled the largest deviation for both employment probability and re-hiring earnings.

Figure C27: Subtracting the Largest Deviation in the Pre-trends



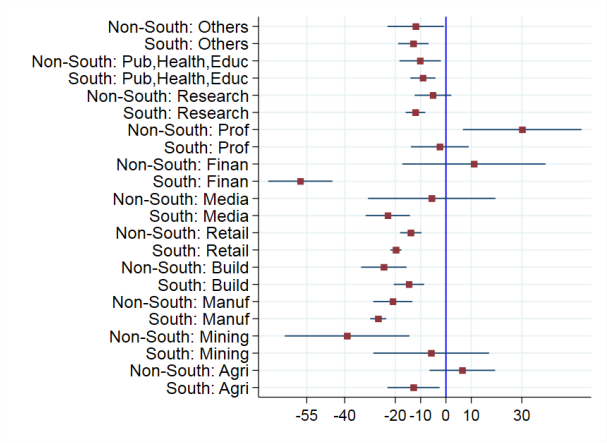

(a) Hiring Earnings (b) Employment Probability (c) Open-ended Job Prob.

*Notes:* The largest pre-trend deviation has been subtracted from each coefficient. Bootstrapped standard errors under this restriction.

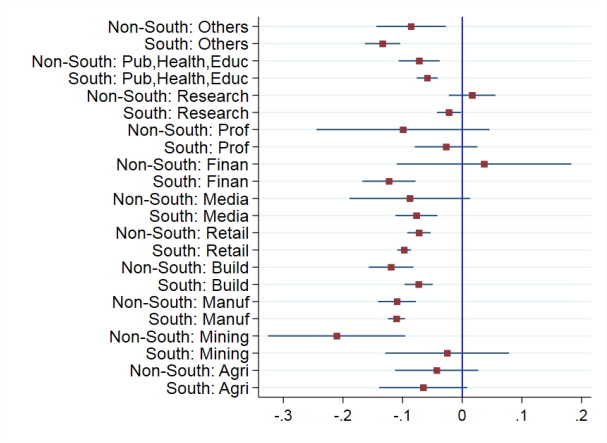
[6]These coefficients are equal to our original D-IW estimates minus the largest deviation in the pre-trends.

## C.10 Heterogeneity Analysis Across Industries and Firms' Location (Subsection 5.3)

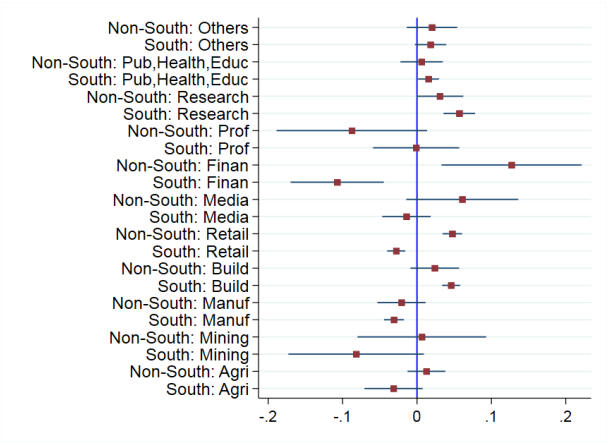
Figure C28: Heterogeneity at 12 Months from Job Loss




(a) Hiring Earnings




(b) Employment Probability

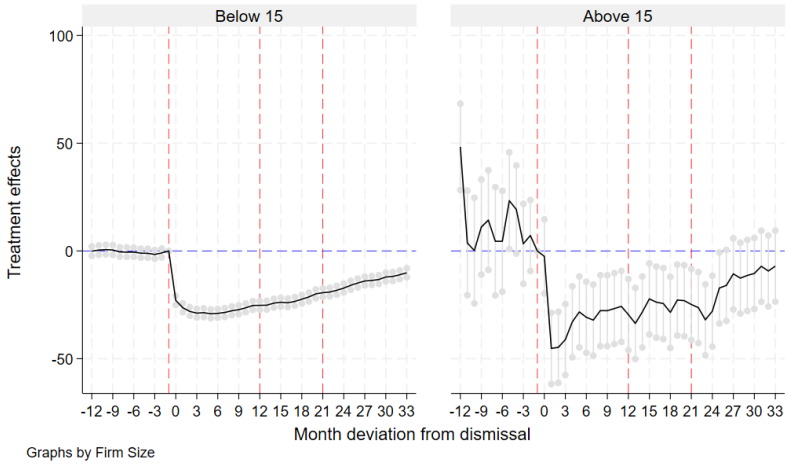



(c) Open-ended Job Prob.

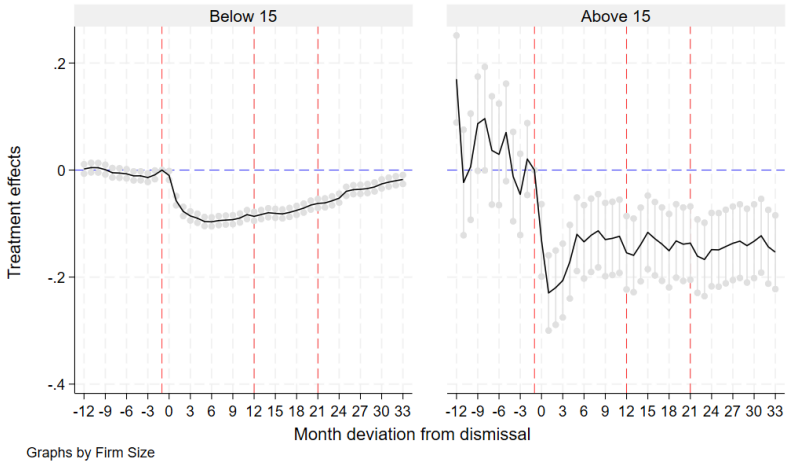
*Notes:* Estimated effects after 12 months from job loss across industries and within locations and within industries and across locations.

## C.11 Separate analysis for workers coming from firms with more than 15 employees and those with fewer. (Subsection 5.3).

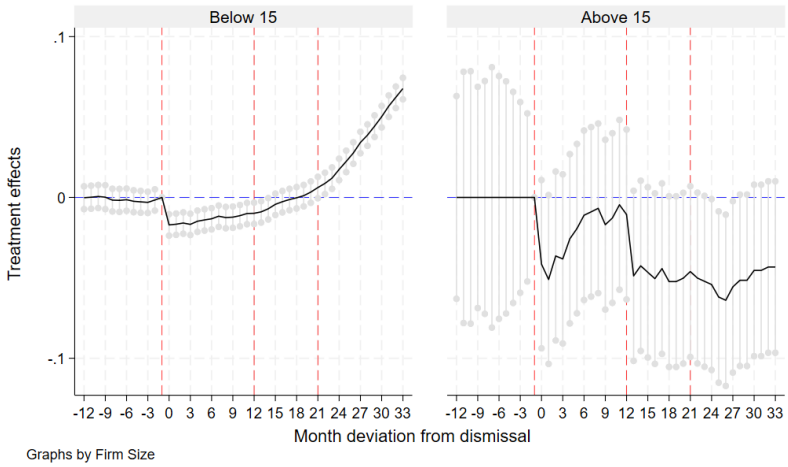
Figure C29: Treatment effects by firm size




(a) Hiring Earnings




(b) Employment Probability

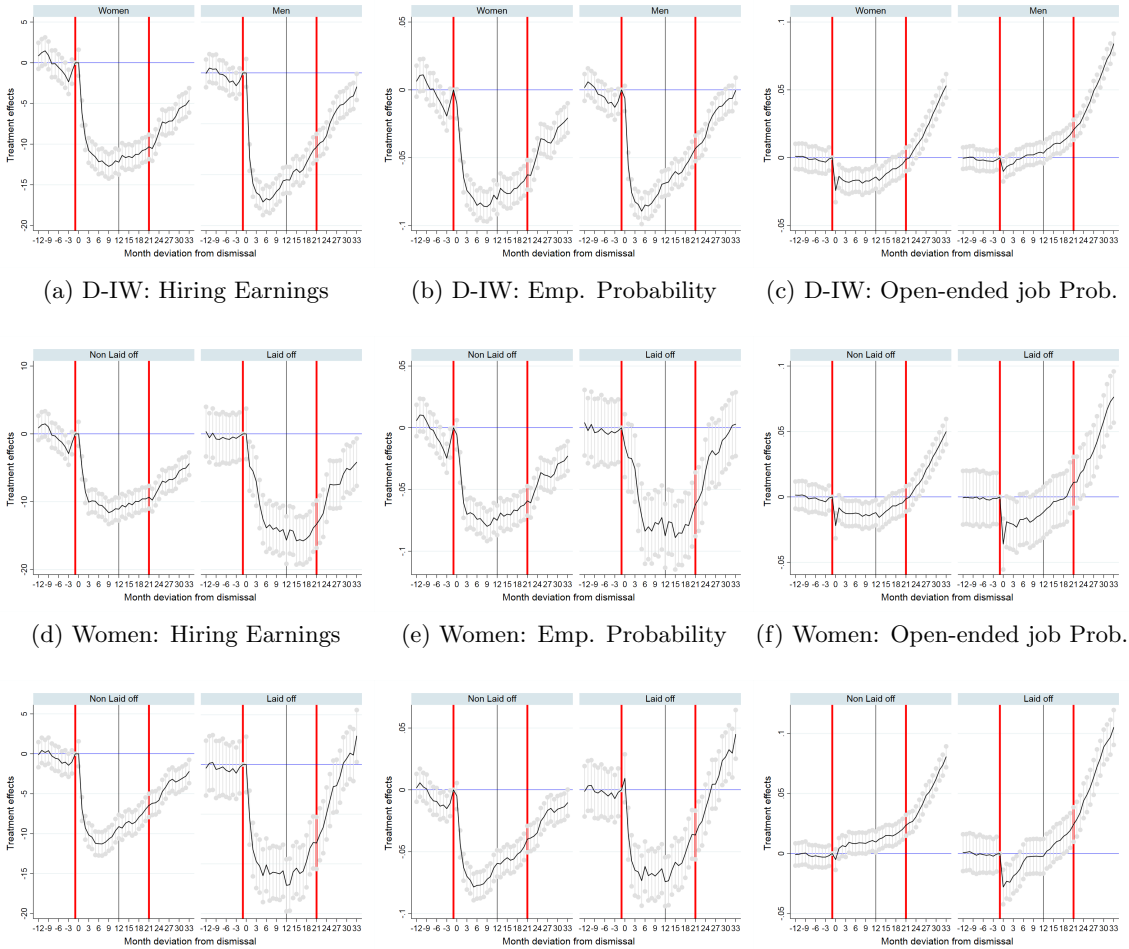



(c) Open-ended Job Prob.

# D Supplementary Material: Section 6

## D.1 D-IW Heterogeneity Analysis

Figure D1: Treatment Effects of the Reform on Labor Market Outcomes by Gender



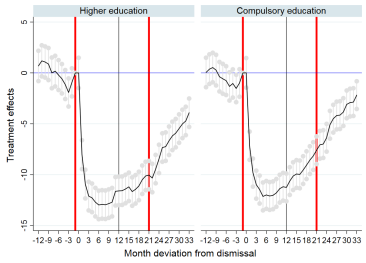

(a) D-IW: Hiring Earnings (b) D-IW: Emp. Probability (c) D-IW: Open-ended job Prob.

(d) Women: Hiring Earnings (e) Women: Emp. Probability (f) Women: Open-ended job Prob.

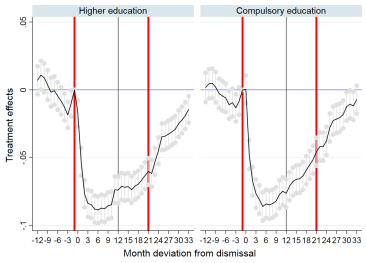
(g) Men: Hiring Earnings (h) Men: Emp. Probability (i) Men: Open-ended job Prob.

*Notes:* The gray bar around each point estimate refers to the 95% confidence interval obtained (for D-IW) from clustered bootstrapped standard errors (with 100 replications). For hiring earnings in panels (a) and (d), the impacts are expressed in percent terms, while in the other four panels the estimates are expressed in percentage point terms.

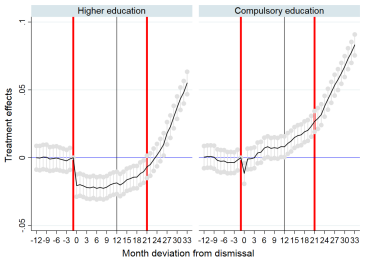
Figure D2: Treatment Effects of the Reform on Labor Market Outcomes by Education



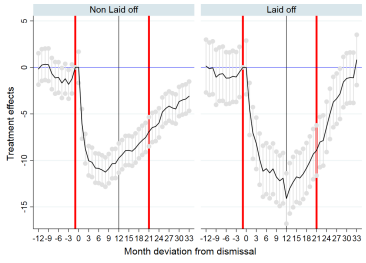

(a) D-IW: Hiring Earnings



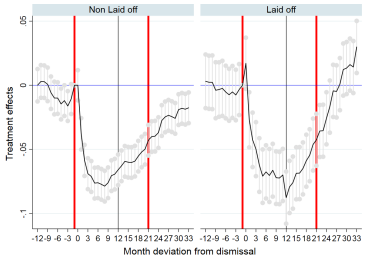

(b) D-IW: Emp. Probability



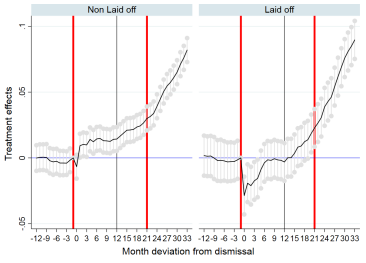

(c) D-IW: Open-ended job Prob.



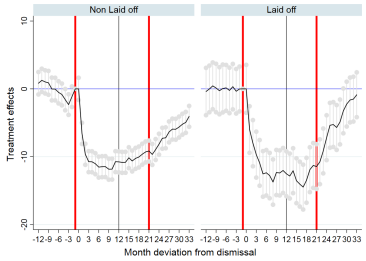

(d) Compulsory: Hiring Earnings



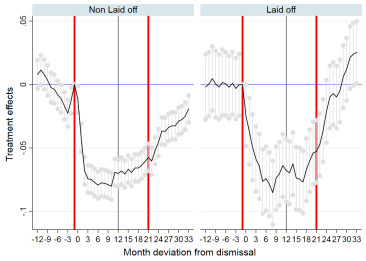

(e) Compulsory: Emp. Probability



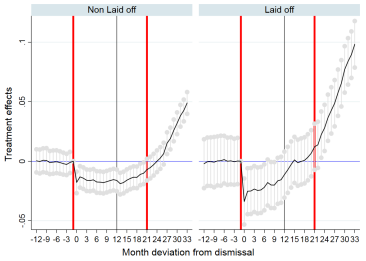

(f) Compulsory: Open-ended job Prob.




(g) Higher Education: Hiring Earnings



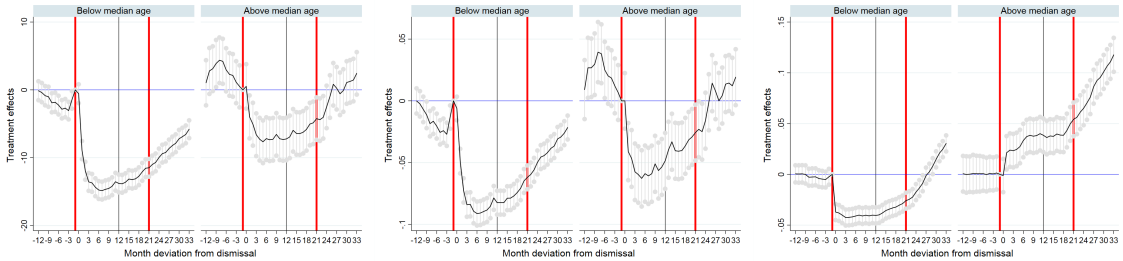

(h) Higher Education: Emp. Probability



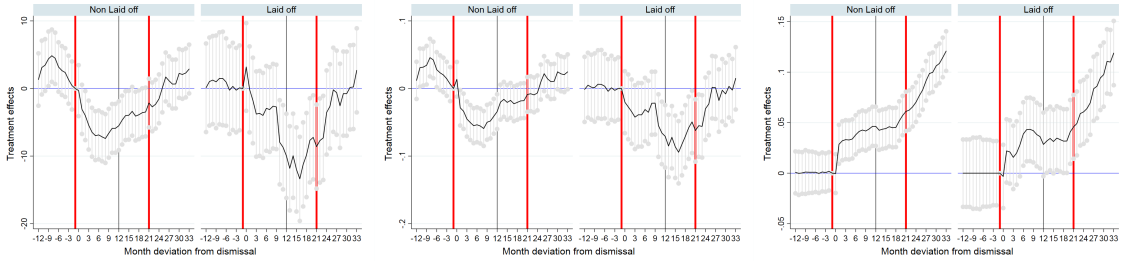

(i) Higher Education: Open-ended job Prob.

*Notes:* The gray bar around each point estimate refers to the 95% confidence interval obtained (for D-IW) from clustered bootstrapped standard errors (with 100 replications). For hiring earnings in panels (a) and (d), the impacts are expressed in percent terms, while in the other four panels the estimates are expressed in percentage point terms.

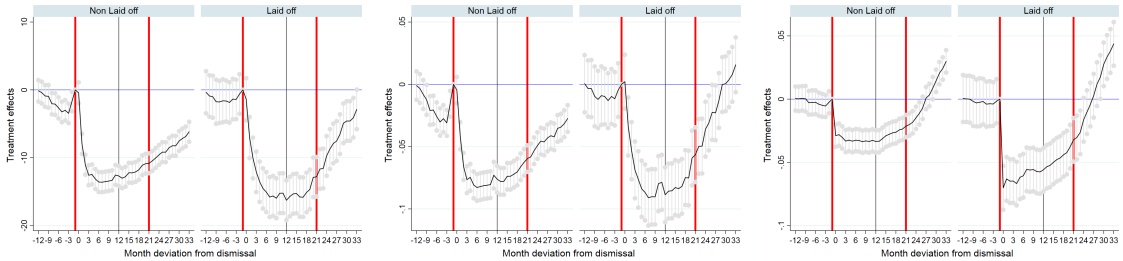
Figure D3: Treatment Effects of the Reform on Labor Market Outcomes by Age




(a) D-IW: Hiring Earnings (b) D-IW: Emp. Probability (c) D-IW: Open-ended job Prob.



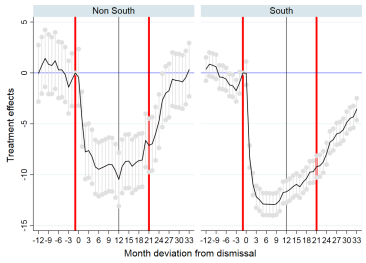

(d) Above: Hiring Earnings (e) Above: Emp. Probability (f) Above: Open-ended job Prob.



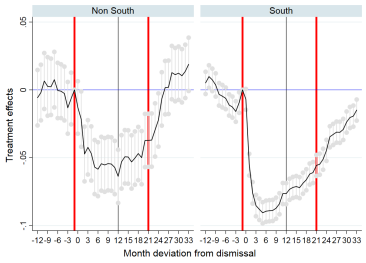

(g) Below: Hiring Earnings (h) Below: Emp. Probability (i) Below: Open-ended job Prob.

*Notes:* The gray bar around each point estimate refers to the 95% confidence interval obtained (for D-IW) from clustered bootstrapped standard errors (with 100 replications). For hiring earnings in panels (a) and (d), the impacts are expressed in percent terms, while in the other four panels the estimates are expressed in percentage point terms.

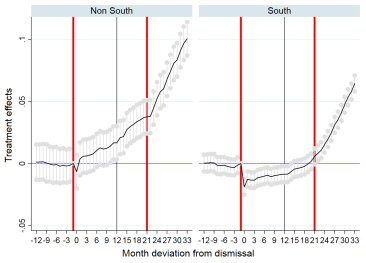
Figure D4: Treatment Effects of the Reform on Labor Market Outcomes by Workplace Location



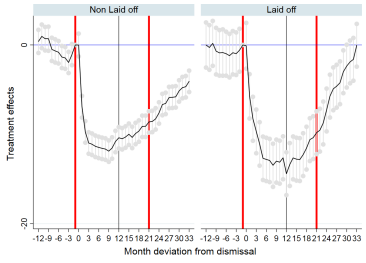

(a) D-IW: Hiring Earnings



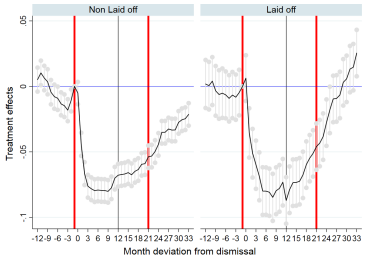

(b) D-IW: Emp. Probability



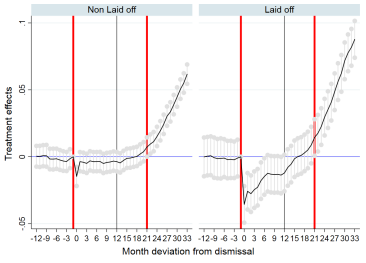

(c) D-IW: Open-ended job Prob.



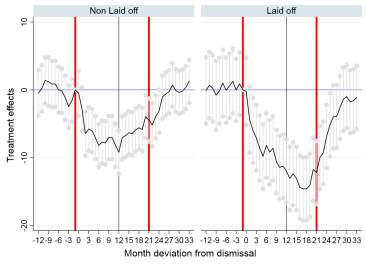

(d) South: Hiring Earnings



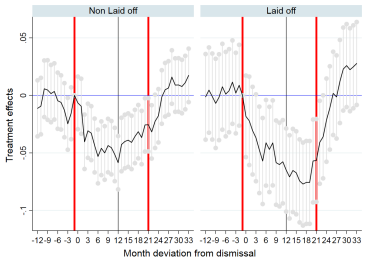

(e) South: Emp. Probability



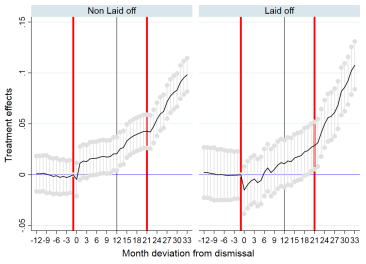

(f) South: Open-ended job Prob.




(g) Centre/North: Hiring Earnings



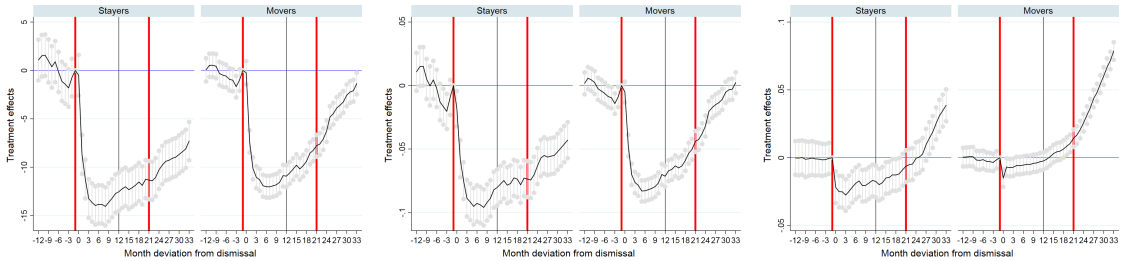

(h) Centre/North: Emp. Probability



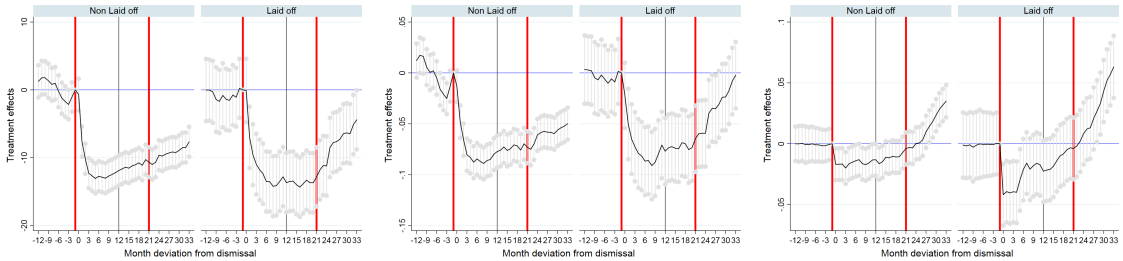

(i) Centre/North: Open-ended job Prob.

*Notes:* The gray bar around each point estimate refers to the 95% confidence interval obtained (for D-IW) from clustered bootstrapped standard errors (with 100 replications). For hiring earnings in panels (a) and (d), the impacts are expressed in percent terms, while in the other four panels the estimates are expressed in percentage point terms.

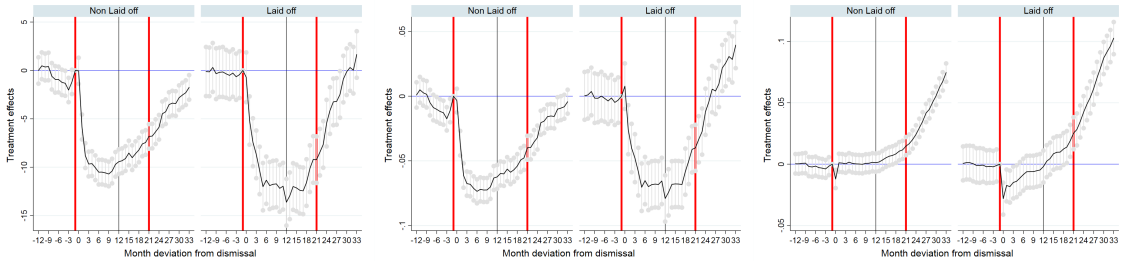
Figure D5: Treatment Effects of the Reform on Labor Market Outcomes by Mobility Across Industries




(a) D-IW: Hiring Earnings (b) D-IW: Emp. Probability (c) D-IW: Open-ended job Prob.



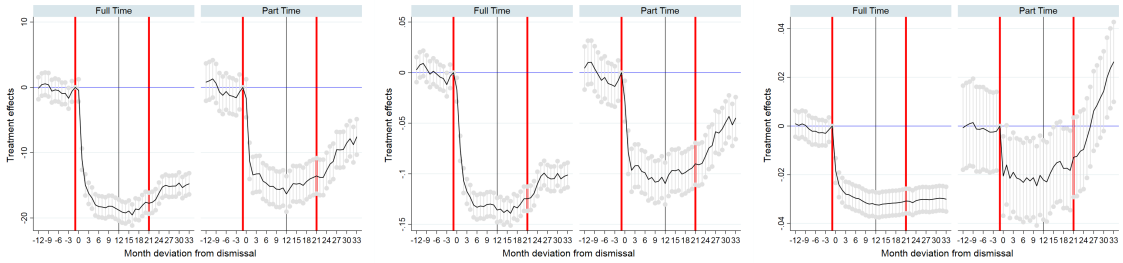

(d) Stayers: Hiring Earnings (e) Stayers: Emp. Probability (f) Stayers: Open-ended job Prob.




(g) Movers: Hiring Earnings (h) Movers: Emp. Probability (i) Movers: Open-ended job Prob.

*Notes:* The gray bar around each point estimate refers to the 95% confidence interval obtained (for D-IW) from clustered bootstrapped standard errors (with 100 replications). For hiring earnings in panels (a) and (d), the impacts are expressed in percent terms, while in the other four panels the estimates are expressed in percentage point terms.

Figure D6: Treatment Effects of the Reform on Labor Market Outcomes by Part-time Status



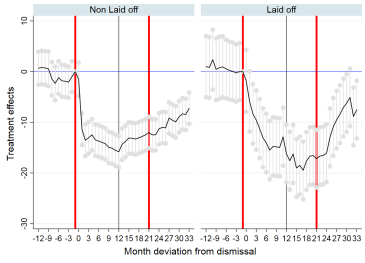

(a) D-IW: Hiring Earnings



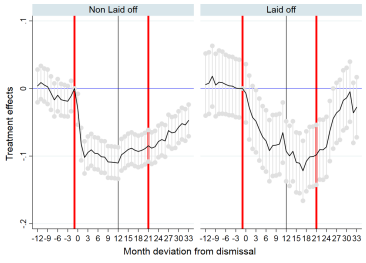

(b) D-IW: Emp. Probability



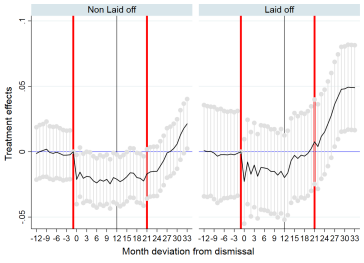

(c) D-IW: Open-ended job Prob.



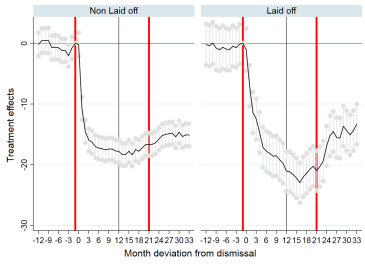

(d) PT: Hiring Earnings



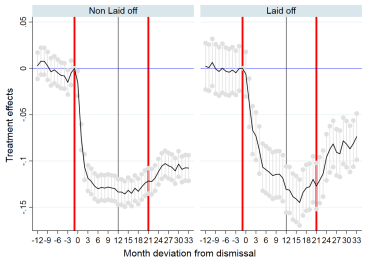

(e) PT: Emp. Probability



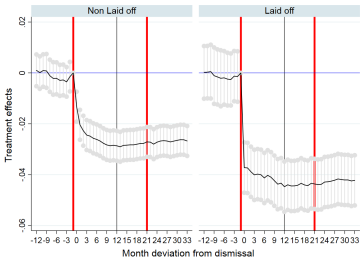

(f) PT: Open-ended job Prob.




(g) FT: Hiring Earnings



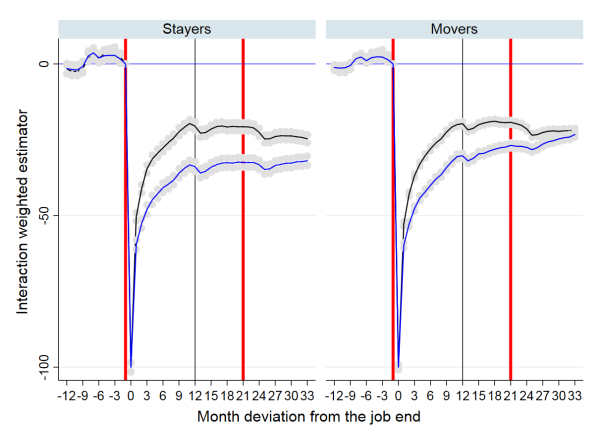

(h) FT: Emp. Probability



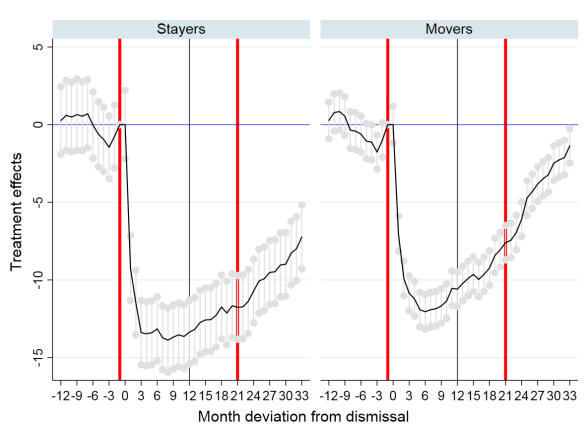

(i) FT: Open-ended job Prob.

*Notes:* The gray bar around each point estimate refers to the 95% confidence interval obtained (for D-IW) from clustered bootstrapped standard errors (with 100 replications). For hiring earnings in panels (a) and (d), the impacts are expressed in percent terms, while in the other four panels the estimates are expressed in percentage point terms.

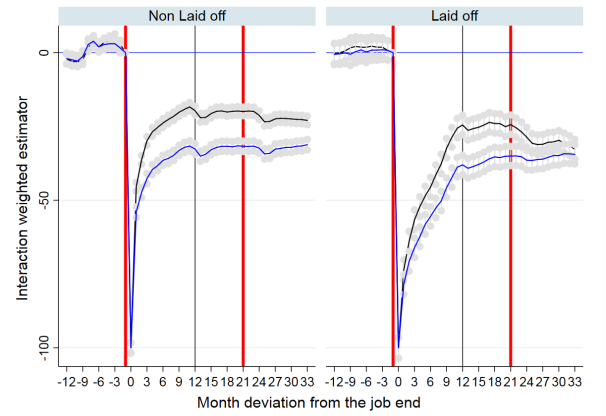
Figure D7: Treatment Effects of the Reform on Hiring Earnings by Mobility Across Occupations



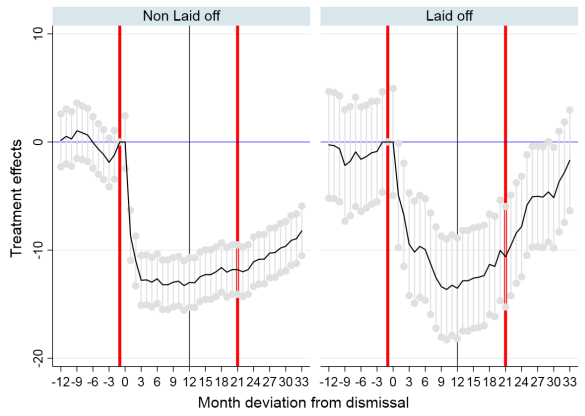

(a) IW: Hiring Earnings



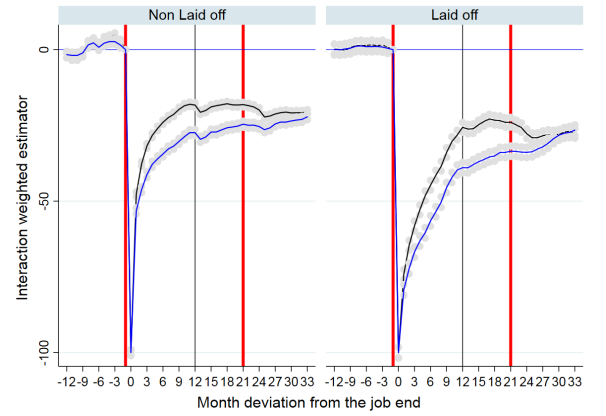

(b) D-IW: Hiring Earnings



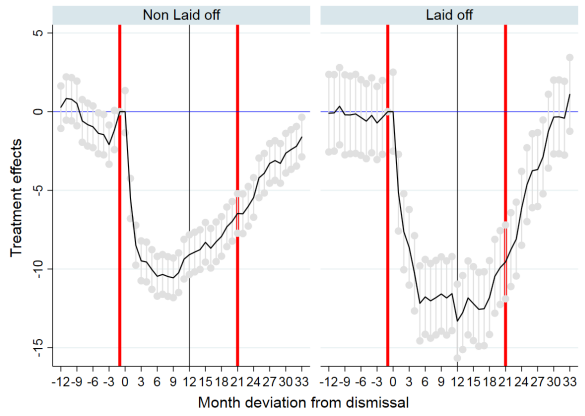

(c) IW: Occupation Stayers




(d) D-IW: Occupation stayers




(e) IW: Occupation Movers




(f) D-IW: Occupation movers

*Notes:* The gray bar around each point estimate refers to the 95% confidence interval obtained (for D-IW) from clustered bootstrapped standard errors (with 100 replications). The impacts are expressed in percent terms. Treated cohorts are in blue and untreated cohorts are in black.

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