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Minimum Wages and the Structure of Training*

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Abstract

We find a positive effect of minimum wages on continuing professional training. Several Swiss cantons introduced high and strongly binding minimum wages in the period 2018-2022. We apply a stacked diff-in-diff estimation model to identify the dynamic policy effect on training. Drawing on several surveys with extensive details on employees' training, we find robust evidence of an increase in training incidence and intensity. The positive effect is mainly driven by firm-financed formal training during working hours that covers contents beyond current professional activities. There are substantial ripple effects and most workers experience extra training, irrespective of their tenure and wage level. We argue that the strong minimum wage bite and our ability to measure the full dynamic training effects on all employees in treated cantons explain the difference between our findings and those in the previous theoretical and empirical literature.

JEL-Code: J08, J51, M53
Keywords: minimum wages, adult training, staggered policy introduction

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1 Introduction

The introduction and modifications of minimum wages in almost all developed countries lead to well-documented changes in the wage structure (Bossler and Schank, 2023), employment (Manning, 2021), employer composition (Dustmann et al., 2022), and the skill composition of the workforce (Clemens et al., 2021). Changes in continuing professional training efforts are less well researched although they may be important drivers or consequences of minimum wages.

The direction and structure of the training effects of minimum wages are disputed (Dube and Lindner, 2024). On the one hand, the training incidence for retained workers may decrease because wage reductions to pay for training costs are not viable any more for low-wage employees (Hashimoto, 1982). Employers also may reduce training in an effort to reduce fringe benefits as compensation for earnings increases (Levin-Waldman, 1996). Lower training efforts may be feasible because the applicant pool improves and job entrants need less initial training, accordingly (Meer and West, 2016; Giuliano, 2013). Finally, a decrease in personnel turnover may reduce training needs (Coviello et al., 2022).

On the other hand, employers may prefer to retain the rents they can extract from their low-wage workers. This option is especially attractive if training costs are relatively low in comparison to the productivity effects induced (Acemoglu and Pischke, 2003). Employers also may replace low-wage workers by higher skilled labor market entrants or employer movers who obtain higher initial and continuing training than the previous employees (Butschek, 2022). In addition, some low-wage workers may be willing to pay for additional training to be able to keep their better paid jobs (Leighton and Mincer, 1981). The disappearance of less competitive firms finally may induce low-wage workers to move to establishments with higher training incidence (Acemoglu, 2001).

The empirical evidence on the impact of minimum wages on training so far points to a negative effect. Doucouliagos and Zigova (2025) find a significantly negative, but economically irrelevant, effect of minimum wages on on-the-job training. Their meta-analysis is based on 14 studies that report 432 estimates. The only extant study that finds a small but significantly positive training effect is Arulampalam et al. (2004). Hardly any of the existing studies goes beyond the calculation of the effects of minimum wages on training incidence and intensity. Notable exceptions are Neumark and Wascher (2001) and Hara (2017) that differentiate between formal and informal training. Baker (2005) and Hara (2017) separately look at employer-initiated and employee-initiated training and Bellmann et al. (2017) differentiate between fully and partly firm-financed training.

In this paper, we empirically assess which of the potential training reactions can be found after the introduction of high regional minimum wages. More specifically, we address in detail the structure of the changes with respect to training characteristics, recipients, and sponsors to reveal the mechanisms behind the minimum wage training effects. We use the staggered implementation of minimum wages on the cantonal level in Switzerland between 2018 and 2022. We show that employers substantially intensify training incidence and intensity by about ten percent. Training efforts increase stronger for low-wage workers than for workers earning higher wages than the new minimum wages. We however also find strong positive "ripple" effects on training levels of employees with much higher than the new minimum wages. Additional training efforts are mainly directed at retained employees they are concentrated on human capital beyond the topical professional needs, they are offered in formal courses during working time, and they are paid for by employers. We conclude that minimum wages induce employers to invest in the productivity of their workforce to keep the rents generated by their low-wage workers instead of incurring costs from hiring higher qualified workers.

We argue that Switzerland is a perfect case in point for an analysis of the training consequences of minimum wages for several reasons. First, the Swiss cantonal minimum wages are at the highest absolute levels in an international comparison (ILOSTAT, 2024). They had an average *Kaitz* index level of 0.54 in the years of their respective introduction which is about the topical level in the OECD area.[1] The cantonal minimum wages therefore immediately had a strong bite. In addition, the cantonal minimum wages were induced by popular votes that were

[1] This average minimum level however increased strongly recently, up from 0.48 in 2008, compare the OECD Earnings and Wage Data Base (https://data-explorer.oecd.org).

not all successful. In some successful cases, it took a considerable time period to implement the positive votes. We therefore argue that the endogeneity risk is smaller in the institutional setting in Switzerland than in situations in which (regional) politicians or industrial relation agents directly can decide on the introduction or adjustment of minimum wage levels (Dube and Lindner, 2024).

Second, the staggered introduction of regional minimum wages with a strong bite allows us to measure the effect of the policy on all employees in the treated cantons. We therefore do not have to reduce the treatment analysis to selected employee sub-groups. Most previous papers concentrate on a strongly exposed sub-group (young employees, employees with low experience, restaurant employees, low-educated or low-wage employees, for example) or selected regional sub-areas (bordering regions with and without minimum wage modifications, for example).[2] The first reason for the focus on a sub-group of potentially treated employees is that a small regional minimum wage variation does not allow the measurement of an overall effect (Hara, 2017). The second reason is that the introduction or adjustment of national minimum wages requires the definition of treatment and control groups within one region. The definition of selected treatment and control groups or sub-regions however may bias the measurement of treatment effects in the presence of confounds and spill-overs (Allegretto et al., 2017).[3] In addition, the generalizability of group-specific or sub-region treatment effects to an overall effect may be problematic.

Third, the staggered introduction of cantonal minimum wages and the availability of training data some years before the first cantonal minimum wage introduction give us the opportunity to use a stacked event-study approach (Callaway and Sant'Anna, 2021). We therefore can jointly measure the potentially dynamic and heterogeneous effects of cantonal minimum wage introductions over a period of several years and test the common trends assumption (Clemens and Strain, 2021).

Our study makes several contributions to the minimum wage literature. We are not aware

[2]Notable exceptions are the papers by Card and Krueger (1995) and Meer and West (2016) that look at the overall employment effects of minimum wages.

[3]Examples for confounders are cross-border spill-over effects because workers move between regions (Neumark and Wascher, 2008; Berger and Lanz, 2020) or so-called "ripple" effects when employees who earn more than the minimum wage before the policy change also experience treatment effects, see Cengiz et al. (2019).

about any other study that estimates the overall training effects on all employees in regions that introduced minimum wages. We can use the causal effect of the introduction of minimum wages with a strong bite in a stacked event-study approach over several years. Furthermore, using a detailed survey on training structure, we can shed light on the mechanisms behind the training reaction by identifying which training forms, sponsors, and recipients drive the changes. We also can explain the differences between our results and those derived in the extant literature by replicating their estimation approaches. Finally, we perform a careful analysis on confounding factors and control for potential additional changes in the workforce and employer structure.

2 Background

The first Swiss minimum wages were introduced for selected economic sectors in the late 1990s. Their introduction was initiated in a large-scale public campaign by Swiss unions and they were implemented in collective wage bargaining agreements in selected economic sectors. Unions aimed at reducing wage inequality and mainly targeted low-wage sectors such as the hospitality, retail, or the garment industry. The implementation of sectoral minimum wages effectively decreased the share of low-wage employees in these sectors (Oesch and Rieger, 2006). Since 1999, the level and scope of sector minimum wages increased gradually.

The sector minimum wages however always entailed many exceptions for specific employee groups and loop-holes for employers covered by the relevant collective agreements (Berger and Lanz, 2020).[4] If the collective agreement is not declared generally binding, only employers covered by a collective agreement have to implement it. Except for the construction and the hospitality sectors, a general binding agreement however is rarely achieved. As a consequence, the share of employees covered by a collective bargaining agreement usually is substantially lower than 50% in Switzerland (Baumberger, 2021). Therefore many low-wage earners and employ-

[4] Although for example the minimum wage clause in the hospitality sector is applicable nationwide and generally binding also for employers not covered by collective agreements, it features pervasive exceptions for employers and employee groups. The regular sector hourly minimum wage in 2018 for example was 20.33 (Swiss) Francs. Small establishments with less than four full time employees and seasonal establishments with at least one "peak season" could apply a lower minimum wage of 18.98 Francs and 19.58 Francs, respectively.

ers remain untreated or the applicable minimum wage level is lower than the regular minimum wage in their sector. The bite of sector minimum wages is further reduced by the fact that only a small subset of collective bargaining agreements contains a minimum wage regulation. Even when cantonal minimum wages are introduced at comparable levels to the regular sector minimum wages, they still have a strong bite in sectors with sectoral minimum wage regulation, as a consequence.

Switzerland has an outright liberal approach to labor market regulations and therefore started relatively late with binding wage regulations for entire regions. Moreover, important regulation decisions that apply to everybody such as the introduction of regional minimum wages are taken by popular vote instead of politicians or industrial relation partners. The first popular vote on minimum wages on the federal level failed in 2014. Some of the subsequent attempts on the cantonal level were successful however. Table 1 presents a chronological overview of successful cantonal popular vote and adoption dates. The first cantonal minimum wage was implemented in 2018 in the canton of Neuchâtel. Canton Jura adopted a minimum wage in 2020, followed by Genève and Ticino at the turn of the year 2020/2021. In July 2022, minimum wages were introduced in Basel-Stadt.[5] Three minimum wage votes were swiftly implemented. In Neuchâtel and Ticino, however, it took about six years between popular vote and implementation.[6]

The five cantons with minimum wages altogether account for about 15% of the Swiss population. Cantonal minimum wages are generally binding for all employees habitually working in the canton. Three groups of workers are exempted from cantonal minimum wages: apprentices, students on internships, and workers employed via work integration programs. Additionally, employees in the agricultural sector are either fully exempt (Ticino) or have a minimum wage that is between 3-6 Swiss Francs lower than the cantonal hourly minimum wage (Neuchâtel and Genève).

[5] Two other cities, Zürich and Winterthur, approved a minimum wage in cantonal popular votes in June 2023. The implementation dates are still open, however. The popular votes on minimum wages in the cantons Solothurn and Basel-Landschaft were rejected in 2025.

[6] In Neuchâtel the long lag between popular vote and implementation was a consequence of appeals by employer associations against the vote. After the Federal Supreme Court unexpectedly decided that the appeals were repealed at the end of October 2017, the minimum wage was promptly introduced in January 2018.

In Figure 1 we plot the development of cantonal minimum wages in the period 2016-2024. The highest hourly minimum wage of around 23 Francs was applied at the introduction in the canton of Genève. At the lower end is the initial minimum wage in Ticino of around 19.5 Francs.[7] Cantonal minimum wages gradually increased over time, largely to account for inflation. To set the cantonal minimum wages in perspective, we also show the collective agreement minimum wage, and the bottom 5% and 10% wage percentiles of hourly wages in the hospitality sector. This economic sector traditionally includes a large share of low-wage employees and has a substantial share of labor costs in total costs (Berger and Lanz, 2020). In addition, with more than 200'000 employees, hospitality is one of the largest sectors in Switzerland. Compared to the low-wage sector, the minimum wages are are above the the 5% percentile (except Ticino). For the high wage cantons Genève and Basel-Stadt, minimum wages are even above the 10% percentile of the wages paid in the hospitality sector.

The differences in cantonal minimum wage levels and wage percentiles should translate into a different bite across cantons. The bite however also depends on the cantonal wages in the years before the minimum wage introduction. To integrate average wage differences between cantons, we calculate for each canton, the so-called *Kaitz* index (the cantonal minimum wage divided by the median wage in the last year before the minimum wage introduction). Higher *Kaitz* values stand for a higher bite of the new minimum wage levels. For initial minimum wages, the *Kaitz* index levels range from 0.465 in Basel-Stadt, through 0.509 in Genève, 0.536 in Neuchâtel, 0.547 in Jura, to 0.579 in Ticino. Although Ticino has the lowest absolute minimum wages, it has the strongest bite. Also in absolute levels the Swiss minimum wages are unprecedentedly high. According to the most recent data collected by the International Labour Organisation (ILOSTAT), the cantonal minimum wages in Switzerland have the highest level worldwide with on average 3,226$ per month.[8]

[7] Note that the Swiss union campaign "No wages below 3'000 Francs" that lead to the implementation of minimum wages in sector collective agreements since 1999 translates roughly to hourly earnings of 20 Francs.

[8] The second highest minimum wage level is reported for Luxembourg (2,459$). Further selected monthly minimum wage levels are: Germany (2,363$), France (2,016$), and United Kingdom (1,987$), compare ILOSTAT (2024). Adema et al. (2018) also find considerably higher minimum wage levels in Switzerland than in any other country.

Besides the strong bite of the cantonal minimum wages in Switzerland, non-compliance with minimum wages is penalized. Unions, public institutions, and employer associations form control bodies—so-called parity or tripartite commissions—that closely control wage dumping. The few employers that have been found not to follow the minimum wage rules had to pay fines up to 30,000 Francs, their violation of the minimum wage regulation was published, and in severe cases they had to temporarily stop their economic activities. To demonstrate the effectiveness of Swiss minimum wages we depict the evolution of low-wage worker fractions in cantons treated by minimum wages and in all remaining cantons, see Figure 2. We focus on two categories of workers: those earning below the actual minimum wage and those earning up to 15% more. The fraction of low wage workers in treated cantons gradually decreased from 6% in 2016 to about 3% in 2022. A portion of this decrease is clearly compensated by an increase of workers in the slightly above minimum wage category. We do not observe these developments in the group of non-treated cantons - the fraction of low wage workers remained largely constant during the same period. We verify the effectiveness of minimum wages and regress a minimum wage treatment variable on log wages of low-wage workers (cf. Manning, 2021). On the basis of our data, we find cantonal wage semi-elasticities with respect to minimum wage dummies between 2-5%.

3 Data

We study the impact of the staggered introduction of cantonal minimum wages on training on the basis of several survey data. Our primary data set is the *Swiss Labour Force Survey* (SLFS), which is the most comprehensive source of information on labor market issues based on a representative sample of the Swiss working age population.[9]. The SLFS is a quarterly survey and has a rolling panel structure. Each participant answers the survey four times, referred to as survey waves. The first two waves take place in two consecutive quarters in year t and the other two waves take place in the same two quarters of the year $t + 1$. We use an annual dataset that includes the responses from the first and the third wave of each individual. We use our data as

[9] See www.bfs.admin.ch/bfs/en/home/statistics/work-income/surveys/slfs.html.

repeated cross-section because we observe each individual only twice - in the same quarter of two consecutive years. The annual dataset contains around 60,000 individuals.

In addition to demographic variables, the SLFS data contain a rich set of information on education, employment, training, and earnings. For our analysis, we exclude employees who are older than the normal retirement age (65 years), or work in jobs that are not subject to minimum wages or have specific minimum wage rules (agricultural sector, internships, apprenticeships, or work integration programs). These restrictions leave us with a sample size of about 237,500 individual observations for the period 2016–2023.[10] This time frame entails a period before the first minimum wage adoption in 2018 and the years of all five cantonal minimum wage adoptions so far, while 21 cantons remain never treated. We define all employees in treated cantons as treatment group.

The SLFS survey offers only basic information on further training. To be able to explore the structure of the training changes, we additionally use the *Swiss Adult Education Survey* (SAES).[11] The SAES offers a high level of detail on financial participation, timing, and content of individual training. Furthermore it collects information about formal and informal training on-the-job. The higher level of detail on training comes at a cost in sample size and collection frequency, however. The SAES is conducted only every 5 years and comprises a much smaller sample than the SLFS. The SAES survey always draws a new representative sample of the permanent resident population that is between 15 and 74 years old. Equivalently to SLFS, we limit the SAES to the currently employed working age population (15-65 years) that is not exempted from minimum wages. We employ the last two SAES waves from 2016 with about 10,000 individuals and from 2021 with 16,000 individuals. The two waves fit well to the policy setting: in 2016 there was no cantonal minimum wage in place and in the year 2021, four cantons had introduced minimum wages.

Table 2 describes our SLFS and SAES samples. Training is self-reported in both surveys and relates to courses, private lessons, seminars, or conferences in the last four weeks prior to the

[10]In 2016, there was a relevant change in the wording of the training questions. Thus, we start our analysis with the year 2016.

[11]See www.bfs.admin.ch/bfs/de/home/statistiken/bildung-wissenschaft/erhebungen/mzb.html.

interview (SLFS) or one year prior to the interview (SAES). If participants affirm this question, they are asked to recollect the number of training hours. This is our measure of training intensity. Training incidence and intensity in SLFS are asked separately with respect to any course and with respect to job-related courses. We do not know however whether respondents participated in one or more courses. We thus construct four different training outcomes based on SLFS: training incidence and intensity in terms of training hours for all and job-related training (cf. Table 2, left panel).[12]

SAES also collects training incidence and intensity in training hours. Additionally, we can separate training into fully firm-financed training and training with workers' financial participation and distinguish training by timing: only during working hours and partly or fully outside working hours. In addition, we can match course content with a classification of economic activities and hereby distinguish courses aligned or not aligned with workers' current job content.[13] Finally, SAES allows us to distinguish between formal and informal training on-the-job.

The SLFS and SAES surveys show that more than 85% of further training is job-related and that on average there are about 3.5 training hours per month. Our descriptive analysis–based on SAES–in addition reveals that employees only finance a minor part of training and that most training takes place partly outside working hours. Finally, training content is frequently not directly aligned with the core economic activity of workers (Table 2).

Our data are not experimental and we cannot exclude that treated and control cantons do not have the same trends before treatment. Therefore we test the common trends assumption and control for further variables that may have an impact on structural differences in training between cantons. In Table 3 we report statistical properties of our control variables. To align our analyses based on SLFS and SAES, we use a highly similar set of control variables for both data sets.[14]

[12] We combine training information from both consecutive waves of the same year. For training incidence our measure equals one if the respondent reports training in any of the two waves. For training intensity we calculate average training hours reported in the two quarters.

[13] We use details on the training content coded as ISCO-08 fields provided in the SAES and match them with the classification of economic activities in the European Community (NACE). Switzerland uses its own variant of economic sector classification, referred as NOGA-08 (*Nomenclature générale des activités économiques*). This classification is however very similar to NACE. See the alignment between the two classifications in Table A4.

[14] These are the only differences between both data sets: The migrant variable is a dummy in SLFS that equals one

Finally, we use the *Employment Structure Survey* (ESS)–a survey of Swiss establishments to measure changes in the workforce structure.[15] The survey is conducted bi-annually and surveys a representative sample of around 35,000 firms. We use the four most recent waves 2016, 2018, 2020, and 2022. The strength of the ESS is that it reports a multitude of employee-level data in addition to firm-level information. Minimum wages may cause changes in workforce skill levels, hiring or contract patterns. ESS allows us to study changes in working conditions such as temporary contracts, working hours, and tenure levels. Of particular relevance to our study is information about the education and skill-level of employees as well as the employment of foreign workers. Additionally, we can analyze possible changes in the share of apprentices. Even if ESS does not contain information on apprentices, we could match ESS survey with Swiss registry data on the number of currently trained apprentices for each firm involved in apprenticeship training.[16] Such match allows us to create a dummy variable whether a firm trains apprentices or not, and a count variable that measures the number of apprentices. Similarly to our analysis based on SAES, ESS ends in 2022, when the minimum wage policy applies only in four cantons. But allows us to make a number of relevant tests of potential minimum wage effects on the workforce structure up to year 2022. We report descriptive statistics of the ESS variables we use in our workforce structure analysis in our online appendix (Table A3).

4 Econometric approach

The introduction of cantonal minimum wages gives us the opportunity to assess the training effect of minimum wages on all employees in treated cantons in comparison to employees in cantons that did not introduce a minimum wage. Our primary estimation strategy employs an event-

for people without the Swiss nationality, in SAES it also includes all first generation migrants. The SLFS includes four occupational level indicators, the SAES only includes the highest occupational level defined as employees with a leadership or managerial position. The SLFS includes a 50-99 employees firm size group, in SAES it is defined more broadly–from 50 to 249 employees–instead.

[15]The German name of the data set is: *Lohnstrukturerhebung*, compare www.bfs.admin.ch/bfs/de/home/statistiken/arbeit-erwerb/erhebungen/lse.html.

[16]The registry is referred to as SBG (*Statistik der beruflichen Grundbildung*), or *Vocational Education Statistics*, compare www.bfs.admin.ch/bfs/de/home/statistiken/bildung-wissenschaft/bildungsabschluesse/sekundarstufe-II/berufliche-grundbildung.html.

study difference-in-differences estimator that accommodates heterogeneous treatment effects and staggered adoption timing (Callaway and Sant'Anna, 2021). This approach is based on doubly robust methods that protect against model mis-specification and avoid biases from inappropriate control group composition (Sant'Anna and Zhao, 2020).

Our event study model captures the so-called group-time average minimum wage effect on our training indicator T of individual i in year t:

$$T_{it} = \sum_{k} \gamma_k \mathbb{I}(t - E_g = k) + \mathbf{X}_{it}\beta + \alpha_c + \tau_t + \varepsilon_{it}, \tag{1}$$

where E_g is the year in which one or more cantons c of "group (g)" introduced minimum wages and $(t - E_g = k)$ is an indicator for being k years from the minimum wage introduction. Our primary dataset (SLFS) is a repeated cross section and thus the matrix $\mathbf{X}_{it}$ comprises a large set of time-varying individual-level variables. These variables include sociodemographic and job-specific variables, and employers' economic sector dummies (see Table 3). Fixed effects for canton and year are given, respectively, by α_c and τ_t. Failure to reject the hypothesis that $\gamma_k = 0 \; \forall \; k < 0$ supports the parallel trends assumption. We cluster standard errors on canton-year level.

Our average treatment effect $(ATT_{g,k})$ is derived for each group g and year relative to the minimum wage introduction in the group k. We apply inverse probability weighting of the OLS estimates of $(ATT_{g,k})$ to get an overall ATT over all groups and periods. Practically each $(ATT_{g,k})$ is multiplied by a weight $w_{g,k}$ that measures the effect of participating in the treatment k time periods after the treatment was adopted and that is equal to the average effect across all groups that have participated in the treatment for k time periods (Callaway and Sant'Anna, 2021). We use the specification Equation 1 in all baseline regressions based on SLFS and ESS survey data. Furthermore to ensure credible comparisons, we exclude not-yet-treated periods of treated cantons from the control group. Lastly, to balance the covariates across control and treatment group, we use the inverse probability weighting based on observed characteristics of individuals ($\mathbf{X}_{it}$).

The SAES dataset leaves no room for group-wise heterogeneity because only two years are observable and all treated cantons have equal treatment length. The inclusions of an interaction term with treatment variables or non-binary treatments are not compatible with the stacked diff-in-diffs. Thus in some specifications we use the standard diff-in-diffs method (Callaway and Sant'Anna, 2021, equation 3.2 or Bellmann et al., 2017, equation 2):

$$T_{it} = \gamma(D_{ct}) + \alpha_c + \tau_t + \mathbf{X}_{it}\beta + \varepsilon_{it}, \tag{2}$$

where T_{it} is our training measure and D_{ct} is a minimum wage treatment dummy. The estimated effect of interest on the canton-year level is $\hat{\gamma}_0$.

To explore heterogeneity by workers' characteristics, we introduce a specification with an interaction term of the minimum wage dummy and a dummy marking individuals who are more likely affected by minimum wages, B_{ict}. In the minimum wage-training literature such a specification has been used previously for example by Grossberg and Sicilian (1999) or Arulampalam et al. (2004):

$$T_{ict} = \beta_0 + \gamma_0 \cdot D_{post} + \gamma_1 B_{ict} + \gamma_2 B_{ict} \cdot D_{post} + \mathbf{X}_{it}\beta + \theta_c + \tau_t + \varepsilon_{ict}. \tag{3}$$

In our empirical analysis, we interact the minimum wage dummy with an indicator of low-wage workers and short tenure workers.

Some studies exploit the variation in the minimum wage intensity within treated regions to identify the minimum wage treatment effect. They use a continuous treatment bite variable MW_{ct} instead of the treatment dummy D_{ct}:

$$T_{it} = \gamma(MW_{ct}) + \alpha_c + \tau_t + \mathbf{X}_{it}\beta + \varepsilon_{it}. \tag{4}$$

We specify our treatment bite MW_{ct} as the proportion of real minimum wage levels in year t in median real wages in canton c in year $t - 1$, the so-called *Kaitz* index Cengiz et al., 2019.

In our sensitivity analyses we replace the canton fixed effects with individual fixed or random effects and estimate the models within an unbalanced panel structure. However, the SLFS sample observes the same individual only twice in two consecutive years.[17] Such data structure allows us to identify the effect only based on $t, t+1$ changes of individuals who experience the minimum wage introduction between the two years they are observed. We show in our event analysis that the training efforts usually increase for more than one year after minimum wage introduction (Meer and West, 2016). Thus, in most of our analyses we treat our data as being of a repeated cross-section format. Nevertheless, we also show and discuss effect estimates based on the individual panel fixed effects estimation.[18]

5 Effects of minimum wages on training

5.1 Baseline effects

In Table 4 we report the Average Treatment Effect of the Treated (ATT) of minimum wage introduction on training incidence (columns (1) to (3)) and on training intensity (columns (4) to (6)). Panel A shows the results for all training types and Panel B the results for job-related training only. The introduction of cantonal minimum wages increases the probability of training incidence on average by 4 percentage points and increases the training intensity on average by about 25 minutes per month. Given that the training incidence is at 40%, the share of trained individuals increases by 10%. On the intensive margin, minimum wages are associated with a 12% increase in training hours, reflecting the mean training intensity of 3.5 hours/month. Note that the inclusion of socio-demographic and job-specific controls has no impact on the minimum wage ATT for training incidence, and only slightly reduces ATT for training intensity. More than 85% of all training is job-related. Accordingly, the training incidence effects hardly change when only job-related training is considered instead of all training measures. The minimum wage effect on job-related training intensity is 21 minutes instead of 25 minutes for all training, see

[17] The rolling panel structure has the consequence that individuals at the beginning and at the end of the sample period are in the dataset just once. Furthermore some survey individuals rejected participation in one or more later waves. Thus there are also few individuals in the middle of the observation period who do not have an observation pair one year later.

[18] A Hansen test rejects the more efficient random effects specification for both, training incidence and training intensity variables.

Panel B in Table 4.[19]

The minimum wage effect on training may not evolve immediately. Enterprises may want to assess the consequences of the policy on their employees first instead of changing their training effort without much delay and it may take some time to organize additional training courses. In Figure 3 we depict the average leads and lags of minimum wage introduction for training incidence and intensity based on the event study specification variant of Callaway and Sant'Anna (2021). In the treated cantons, training incidence and intensity significantly increase already in the year of implementation and stay at the significantly higher level during the first five years after implementation. The confidence bounds for the estimated lags are larger in the first three years of the policy, implying a substantial effect variance in this period. The event analysis rejects the existence of pre-trends.[20]

Our estimation approach allows us to identify differences in the dynamic minimum wage effect on canton-groups by introduction year. In Figure 4 we depict the minimum wage effect on training incidence and in Figure 5 on training intensity for the four minimum wage introduction years 2018, 2020, 2021, and 2023. For Jura, Genève, and Ticino we see an immediate increase in training. In Neuchâtel, the increase evolves only after three years, leading to a zero average treatment effect on the treated over six years. In Basel-Stadt, we see an initial drop in training incidence, but no change in training intensity. This finding may be a consequence of the fact that we have only one full post-policy year for this canton available. The parallel pre-trends assumption is violated for some canton-groups.[21] The Callaway and Sant'Anna (2021) estimator allows for flexible treatment effects and improves robustness, identification however still relies on a form of the parallel trends assumption conditional on covariates. The stabilizing inverse probability weighting also contributes to the support of the parallel trends assumption. In some cases weighting does not secure that cantons are comparable on unobservables, however. Dube and Lindner (2024) argue that pre-trend testing is not always necessary or informative, especially if differences are unrelated to potential outcomes.

[19] If we use the two-way fixed effects specification, our results remain largely the same (cf. Table A1).

[20] Figure 3 shows conditional pre-trends using the full set of controls, e.g. Table 4, columns (3) and (6).

[21] For Basel-Stadt, the last lag year is very likely not parallel because the policy actually started midyear 2022.

5.2 Training structure

We now want to understand which mechanisms are behind the positive average training effect. The SAES dataset allows us to explore several training characteristics. More specifically, we look at whether employers or employees pay for training and differentiate between specific skills needed in the current job or human capital that increases the skill breadth of employees to make them more flexible in their jobs. We also differentiate between formal training, such as courses or workshops informal training, such as learning from colleagues or learning-by-doing. Due to the single jump treatment between 2016 and 2021, we use the TWFE specification (Equation 2).[22] We obtain comparable results for training incidence and intensity based on equivalent samples from SLFS and SAES, see Table 5, columns (1) and (2).

We find that the effect is mainly driven by fully firm-financed–training efforts. Training measures that are also paid by the employee do not change after minimum wage treatment (Table 5, columns (3) and (4)). The minimum wage effect is significant on conventional levels only for training that takes place exclusively during working hours. The increase in training partly outside working hours is not significant, compare Table 5, columns (5) and (6). We can group training measures into directly aligned and not-aligned with the reported sector of economic activity when we compare the ISCED fields of training courses with current economic sectors of each individual (Table A4). Here we find that after the introduction of minimum wages mainly training that is not directly aligned with the worker's economic sector increases rather than sector-aligned training, compare Table 5, columns (7) and (8). Last, there is no minimum wage effect on informal training at the workplace, compare Table 5, column (9). The training increases therefore seem to by driven exclusively by formal training.

5.3 Effect heterogeneity

Prior studies on minimum wages often look specifically at outcomes for sub-groups of workers who are directly, or more likely, affected by minimum wages. In line with this literature, we

[22] In the SAES data, two pairs of very small neighboring cantons in the control group are merged into one. This reduces the number of canton fixed effects to 24 instead of 26.

now focus on the effect of minimum wages on low-wage workers. We therefore add worker sub-group indicator variable (B_{ict}) and an interaction term of the indicator variable with the minimum wage treatment dummy ($B_{ict} \cdot D_{post}$). To replicate the previous papers with sub-groups of treated workers, we use a traditional two-way fixed effects model (TWFE).

We define two low-wage groups. The first group consists of employees who earn up to 25 Francs, i.e. wages below, at, or slightly above the range of cantonal minimum wages. The second group consists of employees who earn up to 30 Francs.[23] The second specification therefore aims at including employees whose wages increased as a consequence of the ripple effect that may lead to wage increases of those who earned somewhat higher wages than the minimum wage level before the policy (Dube and Lindner, 2024). We find that low-wage earners experience a training increase that is about 3 % higher than the control group of employees with higher wages. The minimum wage effect for the low-wage earners below 25 Francs is somewhat stronger than that for those earning between 25 and 30 Francs, compare Table 6, Panel A. We also find a small but significant positive training effect of about 3 % for all employees in the treated cantons, i.e. including also those employees with wages higher than 30 Francs.

Employers may provide training to be able to retain employees or to accommodate newly hired employees via an entry training. If the training increases are concentrated on entry training for new hires, the training effects we find might be mechanical based on more hiring activity in treated cantons. Table 6 demonstrates that although recently hired workers with a tenure less than one year are trained more intensively on average, there is no minimum wage effect on training for this sub-group (cf. Panel B). This finding is robust if we expand the group of recently hired workers by workers who work for the employer for only a short period of time (cf. column (4)). We therefore conclude that the training effect is not driven by job entrants.

[23] The main reason for the generous treatment definition is that wages in the SLFS are self-reported and may be relatively imprecise.

5.4 Minimum wage effects beyond training

This section explores whether there are changes in other outcomes that potentially confound our training effects. For this analysis we use further worker outcomes based on the SLFS data.

Firms exposed to minimum wages may hire more labor market entrants or employer switchers. An increase in hiring activity may induce additional initial training needs. Changes in hiring activity can be revealed by differences in the average tenure length after the introduction of cantonal minimum wages. We find no effect of minimum wages on tenure length, neither measured in tenure years nor in tenure days (cf. Table 7, columns (1) and (2)).

Employers may reduce working hours or increase the share of part-time contracts to reduce their labor costs. Part-time employees however usually receive less training. We use two part-time measures, a precise fraction of full-time work and a dummy for employees who work less than 90% of full time. For none of the two part-time measures, we find a minimum wage effect (cf. Table 7, columns (3) and (4)).

Employers also may reduce paid overtime to reduce their wage bill. Less overtime may allow more training activities. We however do not find a change in self-reported frequent overtime, compare Table 7, column (5).[24] Finally, employees with a temporary contract may obtain less training. We also do not find a change in the incidence of temporary contracts, however, compare Table 7, column (6).

5.5 Workforce composition

To study potential minimum wage effects on the workforce composition, we employ the ESS data . We can use staggered diff-in-diffs for this analysis.

A majority of employees in Switzerland obtains firm-provided apprenticeship training. Wages of apprentices are substantially lower than minimum wages and they are exempted from minimum wage regulation. Firms therefore may be tempted to substitute low-skilled workers by apprentices. Firms however also may cut the number of apprenticeships because skilled labour

[24]We do not include self-reported overtime incidence because 0.94 of employees on average report to work overtime in our sample.

market entrants get more expensive after the introduction of minimum wages. Table 8 shows ATTs of minimum wages on our two apprenticeship measures (column (1) and (2)). Neither the share of training firms nor the share of apprentices in the workforce were significantly influenced by minimum wages.

Firms also did not change their shares of low-skilled workers, measured by education or occupation level (Table 8, columns (3) and (4)). The fraction of foreign workers in the workforce—workers with temporary work permits and cross-border workers—also remained unchanged (column(5)). Equivalent to our results based on the SLFS survey (Table 7), working conditions measured as average fraction of workers with full-time or part-time workers also remained unchanged (columns (6) and (7)). Finally, the shares of short-tenured workers, columns (8) and (9) and the share of workers with temporary contracts were not affected by minimum wages either (column (10)).

5.6 Sensitivity analyses

Our first sensitivity analysis controls the impact of the SLFS sampling weights on our results. When we drop the weights altogether or adjust them so that each canton has equal weight, this does not have a large impact on the baseline results (cf. Table 9, columns (1) and (2)). The removal of either the retail or the hospitality sectors - sectors with the largest fractions of low-wage workers - also does not change our main results (cf. Table 9, columns (3) and (4)). The inclusion of a date-of-vote dummy, to account for a possible anticipation effect for the implementation of the cantonal minimum wage, also has no impact on our results (cf. Table 9, column (5)). When we use the short individual panel of two consecutive years that is available in the SLSF, we still get a positive effect on training incidence of comparable magnitude. The effect on training intensity is reduced by half and ceases to be significant. Note however that the effect is only identified by individuals from treated cantons for whom the minimum wage was introduced during the two observed consecutive years.[25] Our baseline estimates use a linear probability model for training incidence and assume a continuous training intensity. Column (7) of Table 9 therefore reports

[25] For this exercise, we excluded individuals who changed the canton between the two observation years.

the underlying nonlinear estimates. The logit estimate for the training incidence dummy implies an increase in the training probability if an employee is treated by 18%. Our tobit model for the number of training hours shows an increase by 1.43 hours, conditional on nonzero training. The nonlinear results therefore corroborate our estimates from linear probability models.

To strengthen the validity of our baseline estimates and remove potential doubts that they may be driven by an accidental correlation between minimum wage adoptions and other unobserved changes that impact training measures, we conduct a placebo analysis. Focusing on the subset of the 21 never-treated cantons, we impose placebo minimum wage treatments in five randomly picked cantons and randomly picked years over the 2018-2022 period. We repeat the randomization procedure 1,000 times and estimate the ATT of minimum wage on training incidence and intensity. Figure 6 displays the empirical cumulative distribution of minimum wage ATTs from the placebo randomizations. The probability that the baseline effect on training incidence of 0.04 would appear by an accidental correlation is 0.01. The accidental probability of the treatment effect size of 0.385 lies at 0.02. The potential for unobserved impacts on training that could generate training effect sizes comparable to our baseline estimate therefore seems to be negligible.

5.7 Treatment intensity

Our paper mainly uses stacked dynamic event estimations to measure the training effects of minimum wages for all employees in the treated regions. We now replicate traditional estimation approaches for the Swiss case to be able to align our findings with previous evidence. Several papers use a measure of minimum wage treatment intensity rather than a minimum wage dummy. Here the exogenous variation comes from the differences between regional minimum wage levels (Fairris and Pedace, 2004; Cengiz et al., 2019), instead of the difference between regions with and without minimum wages. A widespread measure of regional differences in minimum wage is the so-called *Kaitz* index. We use the cantonal minimum wages levels and relate them to regional median wages to construct cantonal *Kaitz* indices. First, we calculate the median wage for each canton and year and therefore obtain one *Kaitz* index observation per treated canton per year.

Then we additionally exploit the variation in the wage structure between and within cantons and also calculate cantonal median wages for each economic sector or each occupational level. To mimic a situation in which firms have to adapt wages to a new minimum wage level in year t, we calculate the median wages in $t-1$.[26]

First, we replace our minimum wage dummy by the *Kaitz* index in our baseline regressions. Our new results are robust (cf. Table A2). Second, we use regional minimum wage differences instead of minimum wage introduction. We implement the *Kaitz* index analysis on the subset of cantons that have a minimum wage. We build two samples. The first sample uses data from the period 2021-23 that includes the four cantons that implemented minimum wages during the treatment period. Our second sample for the year 2023 includes all five treated cantons. For the *Kaitz* index measured on canton-year level, we find no effect of minimum wages on any training measure. For the *Kaitz* indices based on median wages on canton-year-occupation or canton-year-sector levels, we obtain a negative minimum wage effect (cf. Table 10).

6 Discussion

Our paper is among the first to show substantial positive training effects of minimum wages. The only other paper that demonstrates a positive effect, we are aware of, is Arulampalam et al. (2004). After the introduction of the national minimum wage in the UK, the authors report training increases ranging between 8 and 11%. There may be a couple of reasons for the obvious differences between our findings and the bulk of the existing literature that either shows insignificant or small negative training effects.

First, the Swiss cantonal minimum wages have the highest absolute levels worldwide and a stronger bite than minimum wages at their point of introduction in many other countries. The introduction of cantonal minimum wages in Switzerland therefore may have induced stronger

[26] Several variants of minimum wage treatment intensity were used in previous empirical applications. Neumark and Wascher (2001) use percent by which the state minimum exceeded the federal minimum over the previous 3 years, Acemoglu and Pischke (2003) divide minimum wage levels by the median wage of older workers over the sample period. Baker (2005) divides the regional minimum wage by the aggregated industrial wage for each region. Leighton and Mincer (1981) use the ratio between regional minimum wage and the index of the standardized regional wage.

effects than elsewhere Manning (2021).

Second, we measure the impact of the introduction of minimum wages in regions in which wages of many employers and employees have not been regulated before. We therefore do not measure the impact of relatively small minimum wage changes in regions that all had minimum wages before. If we reduce our sample to treated cantons and use differences in minimum wage intensity and their changes over time, cantons, and occupations or sectors, training indeed becomes negatively correlated with minimum wages. We therefore find that regions, sectors, or occupations with relatively strong minimum wage bite have lower training levels compared to those with a weaker bite. It therefore makes a crucial difference to analyze the effects of the introduction of minimum wages rather than the variations in their bite over time, cantons or occupations. This result supports the hypothesis that large jumps in the minimum wage level induce a stronger reaction than small changes (Clemens and Strain, 2021).

Third, in contrast to most previous studies, we assess the full effect on all employees in the treated cantons. When we differentiate the effect by earnings bins, we find strong ripple effects. In other words, there is an increase in training incidence and intensity also for employees who were not directly affected by minimum wages because they earned much more than the minimum wage.[27] The training effect decreases with the inclusion of employees who earn clearly more than the minimum wage, but there is a significantly positive effect even if we include all employees, also compare the hypothesis by Neumark and Wascher (2001). We conclude that strong ripple effects may lead to negatively biased training effect measures in papers that compare a selected treatment group of employees who earn not much more than the new minimum wage to a control group of employees who earn much more than the new minimum wage.

Fourth, we do not only calculate the immediate treatment effect but investigate the mid-term dynamic effects of the minimum wage introduction for up to five years (Clemens and Strain, 2021). We find that the minimum wage effect on training incidence is measurable already in the introduction year and that the effects remain roughly stable during the first few years in which the policy is in place. We however also find that in the cantons of Neuchâtel and Basel Stadt, the

[27] Berger and Lanz (2020) also find ripple effects for earnings in the restaurant industry in the canton of Neuchâtel.

immediate effect was negative. In Neuchâtel the effect turned positive after some years.

To study the mechanisms behind the positive training effects, we analyze the changes in training characteristics after minimum wage introduction. We find that the extra training is mainly concentrated on formal training instead of informal training and training in human capital not directly aligned with the economic sector of the main professional activity.[28] Extra training after the introduction of minimum wages usually is fully financed by employers instead of co-financed by employees and extra training takes place during working hours instead of during the leisure time.[29] In addition, we show that all workers regardless of their tenure length experience a higher training incidence and intensity.[30] We also do not find further indications that confounding factors bias the training effects. More specifically, we do not find a reduction in working time (Dube et al., 2016), over-time or the share of temporary, higher skilled or foreign workers. Minimum wages also do not trigger a shift towards more apprenticeship training although apprentices are exempted from minimum wages in Switzerland.

We conclude that labor market frictions seem to create room for rents that lead employers to invest in training of their retained low-wage employees in the face of substantial increases in minimum wages (Acemoglu and Pischke, 2003). Employers even fully pay for training that is not directly related to the topical professional activities of their employees to increase their productivity level. This interpretation matches the argument that Swiss employers do not have to share the returns from productivity increases induced by training with their employees.[31]

[28]Our results are in analogy with Neumark and Wascher (2001) and Hara (2017) who mainly find (albeit negative) effects of minimum wages on formal training and with the theoretical papers by Becker (1962) and Simpson (1984) who predict a stronger (albeit negative) minimum wage effect on general than on job-specific human capital training.

[29]Baker (2005), Bellmann et al. (2017) and Hara (2017) also report a stronger (albeit negative) minimum wage effect for employer-initiated training than for employee-initiated training.

[30]In accordance to findings on the worker flow effects of minimum wages in the UK (Albagli et al., 2024) and the US (Dube et al., 2016 and Coviello et al., 2022), we do not find an increase in the hiring intensity after the introduction of minimum wages.

[31]This argument was also made by Gerfin (2004) who finds that Swiss employees can only realize wage increases if they switch their employer after training.

7 Conclusion

Our paper shows that employers invest in the productivity of their workers after the introduction of minimum wages. Training investments allow employers to keep rents from retaining their low-wage employees (Levin-Waldman, 1996, p. 27; Acemoglu and Pischke, 2003). These results are in contrast to most extant theoretical and empirical papers that argued, on the basis of the standard human capital theory, that minimum wages reduce on-the-job training or at best have no impact.

We look at the consequences of the cantonal minimum wages on the provision of on-the-job training in Switzerland. We use the staggered minimum wage introduction in a handful of cantons and their gradual level adjustments as exogenous variations. We find that cantonal minimum wages lead to a positive training effect already in the year of introduction and a comparable dynamic effect over at least four further years. The strongest training effect can be found for low-wage earners below and some percentage points above the new minimum wage levels. We however also find pervasive ripple effects on the training level on employees in much higher earnings groups in the treated cantons. The training effect is not concentrated on newly hired workers but also workers with higher tenure receive extra training. The content of the extra training usually is not directly related to the main professional activity of the employees. The extra training is provided in formal courses during working time and paid for by the employers.

We argue that differences in minimum wage training effects found between our study for Switzerland and earlier studies can be partly explained by differences in the identification strategies. We measure the dynamic training effects for all employees in the treated regions on the basis of a stacked events study (Dube and Lindner, 2024). When we replicate previous empirical approaches and reduce the analysis to minimum wage adaptations in the treated regions and to sub-groups of strongly exposed employees, the training effects are not significant and in some specifications even turn negative.

This paper contributes to a new chapter of minimum wage research that uses high quality data on the introduction of recent minimum wages that have a stronger bite than minimum wages in the past (Manning, 2021, pp.22/23). Our analysis suggests that even in the liberal Swiss labor market, employers can acquire rents from the employment of their low-wage workers and a large part of training benefits. Our results therefore seem to be transferable to other regions and predict that employers prefer to increase the productivity of their workers after the introduction of strongly biting minimum wages instead of incurring hiring costs for newly hired better qualified workers.

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Figures and Tables

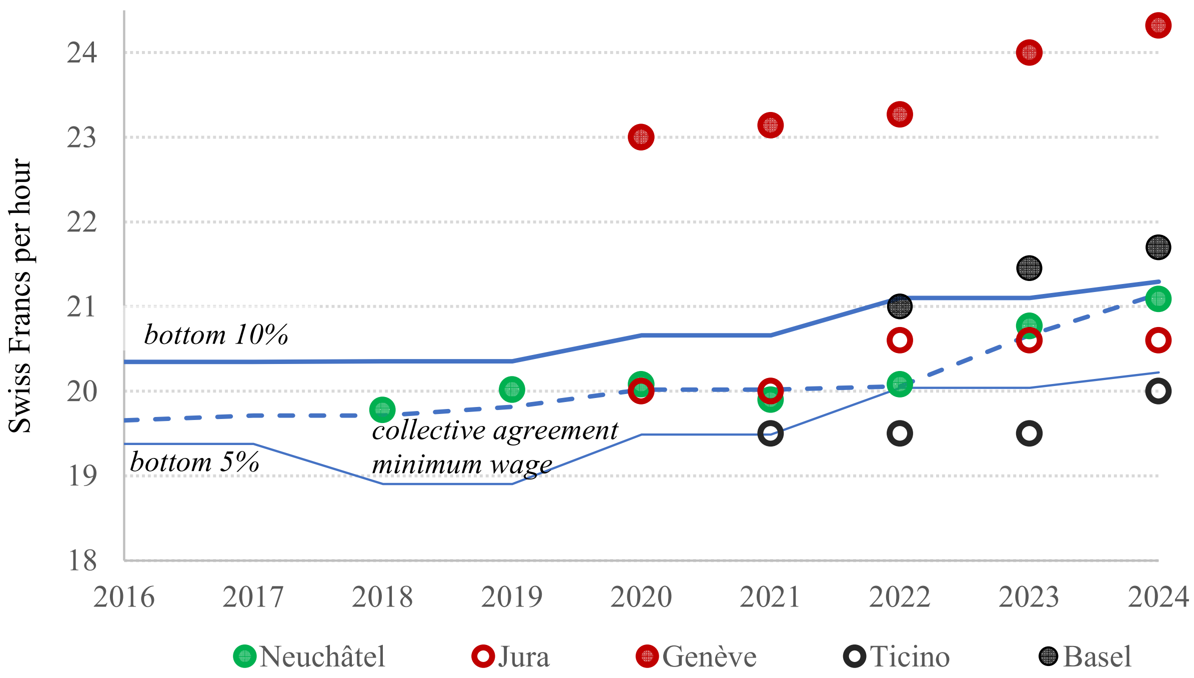

Figure 1: Cantonal minimum wages, hospitality sector minimum wages as per collective agreements, and 5% and 10% wage percentiles

Notes: Mandated hourly minimum wages in the five treated cantons (circles); regular hourly minimum wages from collective agreements in hospitality sector (dashed line); 5% and 10% percentiles in hospitality industry (thin and thick solid lines).

Sources: Own compilation based on: Minimum wage information of cantons, texts of collective agreements for hospitality sector, ESS survey data 2016-2022. ESS is biannual, thus percentiles in odd years are mechanically the same as in the previous–even–year. Percentiles in 2024 are based on 2022 wages adjusted by 0.9% nominal wage growth.

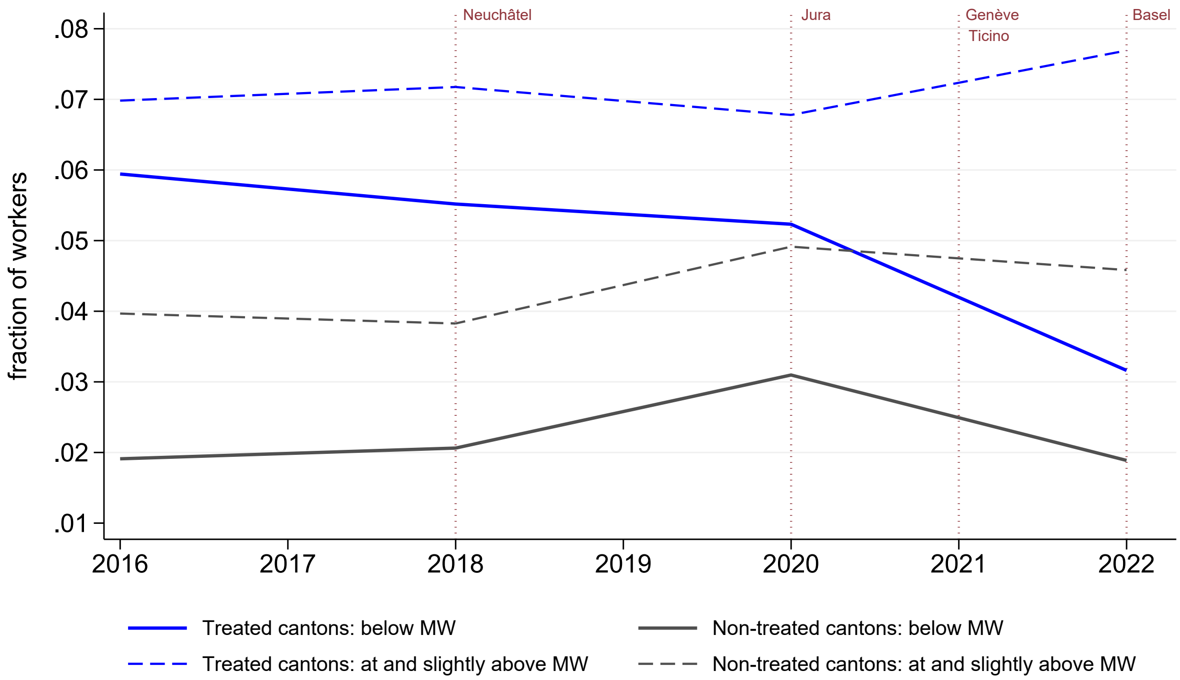

Figure 2: Fractions of low-wage workers by minimum wage treatment status

Notes: Own depiction based on ESS 2016-2022. Fractions are calculated in each year for the group of the five minimum wage (MW) treated cantons and the group of the 21 non-treated cantons. Vertical dotted lines indicate first year of binding minimum wage regulation for canton(s) indicated at the upper right of each line. Actual MW levels are used for treated cantons. For the non-treated cantons, we impose an artificial MW level calculated as average of the valid MW(s). At and slightly above MW lines include workers with hourly wages starting from actual MW up to 15% above the MW level. ESS is biannual, thus there are no numbers behind the odd years.

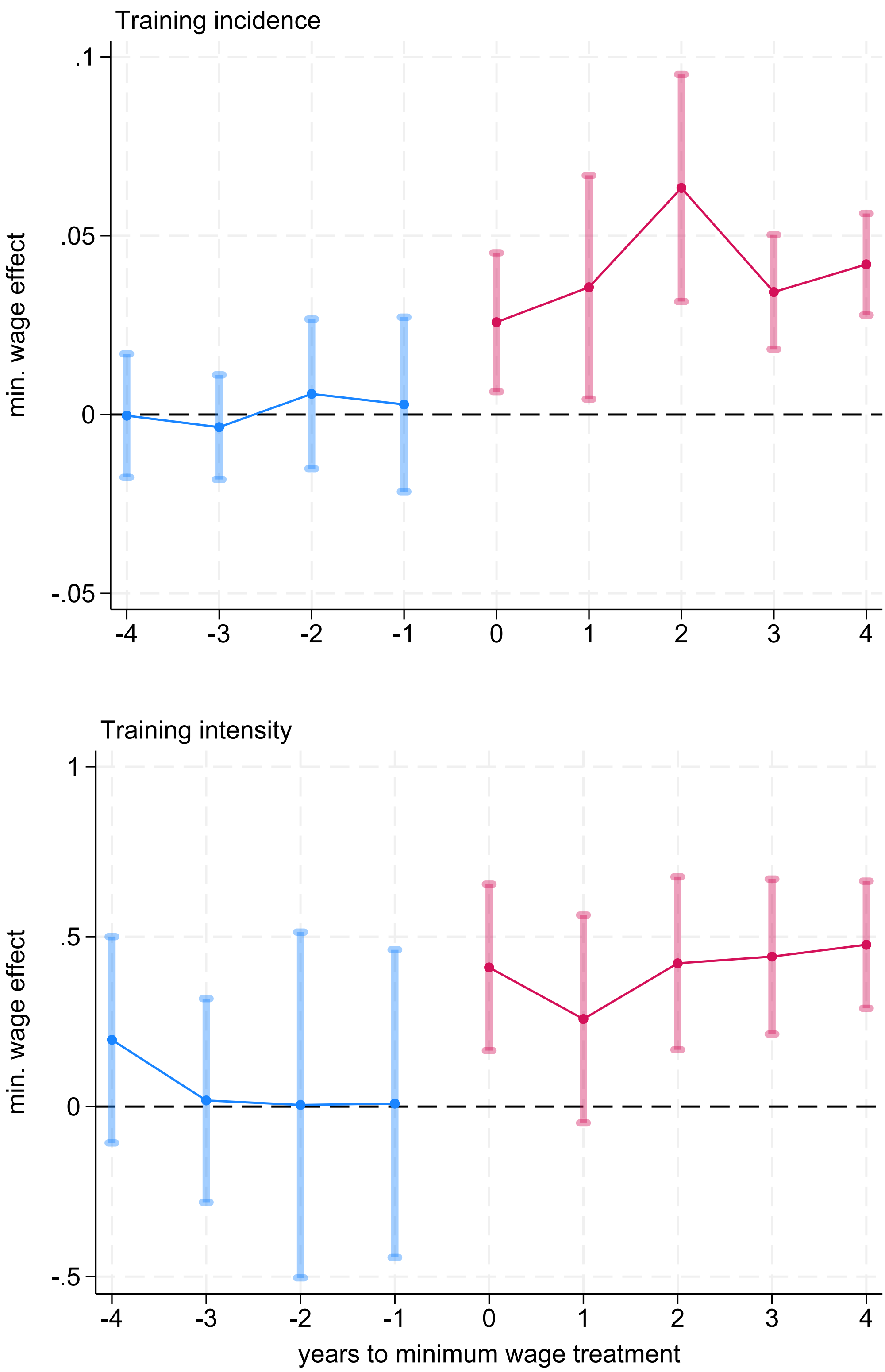

Figure 3: Dynamic treatment effects of minimum wage on training incidence and intensity

Notes: Dynamic minimum wage effects on training incidence and intensity estimated under the conditional parallel trends assumption, based on Callaway and Sant'Anna (2021). The estimates use the doubly robust estimator proposed by Sant'Anna and Zhao (2020) with inverse probability weighting. Estimations include canton and year fixed effects, and the full set of control variables (cf. Table 4, columns (3) and (6)).

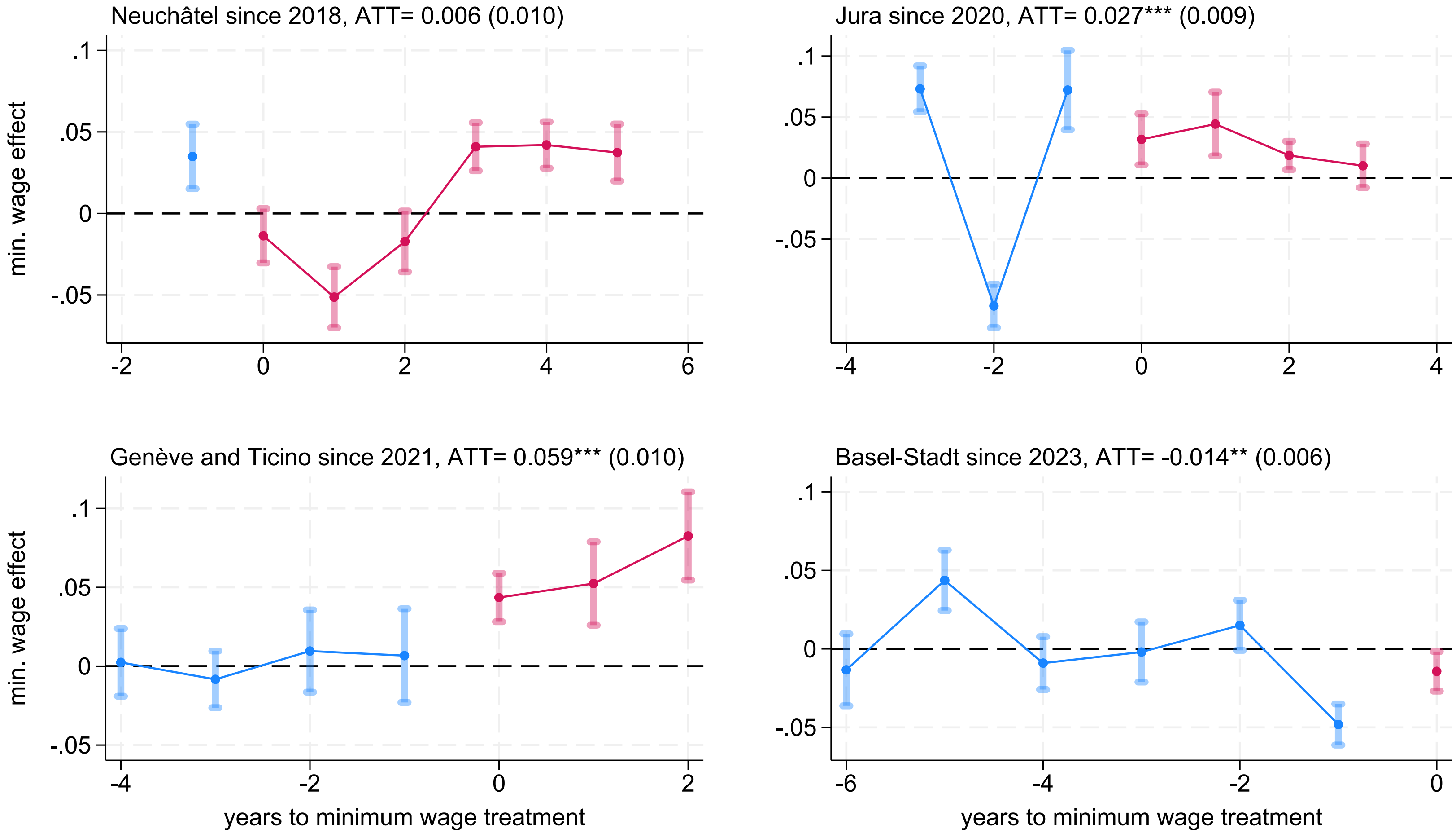

Figure 4: Group-time dynamic treatment effects of minimum wage on training incidence

Notes: Dynamic minimum wage effects on training incidence using Callaway and Sant'Anna (2021) approach. The estimates use the doubly robust estimator proposed by Sant'Anna and Zhao (2020) with inverse probability weighting. The four groups are based on the four different policy adoption years. Basel-Stadt, adopted the policy in 07/2022, but we assume the first year of treatment the first full policy year: 2023. Estimations include canton and year fixed effects, and the full set of control variables (cf. Table 4, column (3)). For each group we provide group average treatment effect (ATT) and its clustered–canton×year standard errors. Significance on 0.1, 0.05, and 0.01 is indicated as *, **, ***.

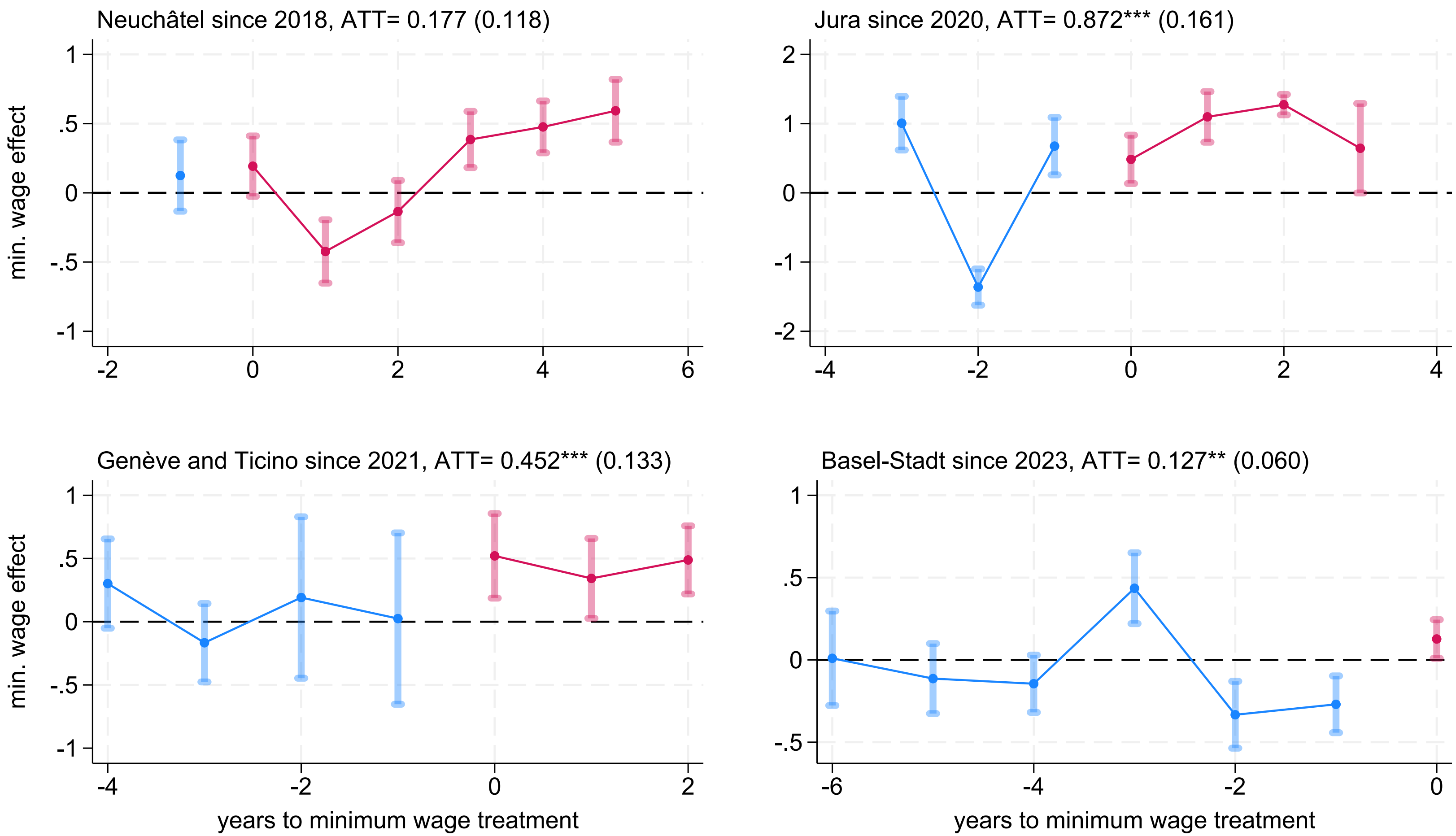

Figure 5: Group-time dynamic treatment effects of minimum wage on training intensity

Notes: Dynamic minimum wage effects on training intensity using Callaway and Sant'Anna (2021) approach. The estimates use the doubly robust estimator proposed by Sant'Anna and Zhao (2020) with inverse probability weighting. The four groups are based on the four different policy adoption years. Basel-Stadt, adopted the policy in 07/2022, but we assume the first year of treatment the first full policy year: 2023. Estimations include canton and year fixed effects, and the full set of control variables (cf. Table 4, column (6)). For each group we provide group average treatment effect (ATT) and its clustered–canton×year standard errors. Significance on 0.1, 0.05, and 0.01 is indicated as *, **, ***.

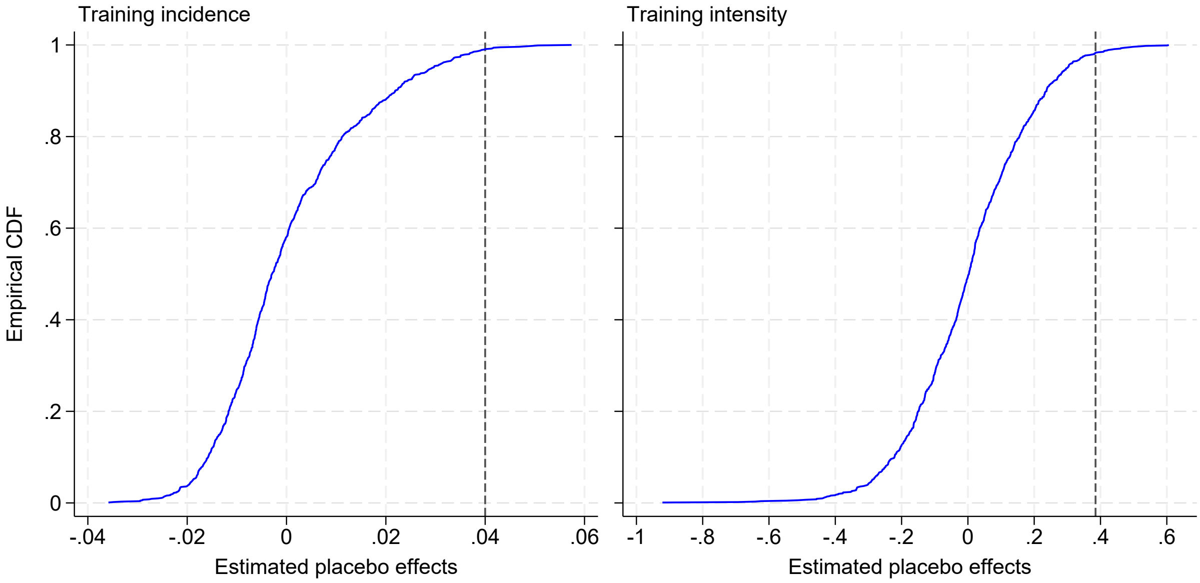

Figure 6: Distribution of minimum wage effects on training measures from 1'000 placebo randomizations

Notes: The figures display the empirical distribution of placebo estimates of minimum wage ATTs on training incidence (left) and training intensity (right) based on 1,000 randomizations of minimum wage treatment among the 21 never treated cantons. Each placebo treatment is based on specification (3) for training incidence and (6) for training intensity of Table 4 that include canton and year fixed effects, and the full set of control variables. The vertical lines corresponds to the true effect estimate of 0.04 (left) and 0.385 (right). As in the baseline Table 4 placebo estimates use the doubly robust estimator with inverse probability weighting (Callaway and Sant'Anna, 2021).

Table 1: Minimum wage across Swiss cantons: Vote and adoption dates

Canton	Minimum wage vote	Minimum wage adoption
Neuchâtel	Popular vote in November 2011	January 2018
Jura	Parliament vote in November 2017	January 2020
Genève	Popular vote in September 2020	November 2020
Ticino	Parliament vote in June 2015	January 2021
Basel-Stadt	Popular vote in June 2021	July 2022

Sources: www.ne.ch; www.nzz.ch; www.arbeitsrecht-aktuell.ch; www.awa.bs.ch

Table 2: Descriptive statistics of training outcomes in SLFS and SAES samples

	SLFS 2016-2023		SAES 2016, 2021	
	mean	std. dev.	mean	std. dev.
Dependent variable:				
Training incidence:				
All	0.398	(0.490)	0.473	(0.499)
Job-related	0.349	(0.477)	0.464	(0.499)
Fully firm-financed			0.372	(0.483)
Partly employee-financed			0.056	(0.230)
During working hours			0.294	(0.456)
Outside working hours			0.379	(0.485)
Aligned training			0.223	(0.416)
Not-aligned training			0.418	(0.493)
Informal at work			0.303	(0.460)
Training intensity (hours/month):				
All	3.567	(8.540)	3.461	(12.292)
Job-related	3.101	(8.053)	2.456	(11.060)
Fully firm-financed			1.372	(7.198)
Partly employee-financed			0.450	(7.586)
During working hours			0.929	(4.824)
Outside working hours			2.438	(11.133)
Aligned training			1.026	(6.335)
Not-aligned training			2.436	(9.763)
Informal at work			0.782	(5.941)
Observations	237,536		15,785	

Notes: Based on *Swiss Labor Force Survey* (SLFS) 2016-2023 and *Swiss Adult Education Survey* (SAES) 2016, 2021 estimation samples. SLFS does not contain further details on training types.

Table 3: Descriptive statistics of control variables in SLFS and SAES samples

	SLFS 2016-2023		SAES 2016, 2021	
	mean	std. dev.	mean	std. dev.
Controls:				
Female	0.475	(0.499)	0.490	(0.500)
Age	41.700	(11.926)	41.323	(12.174)
Migrant	0.273	(0.445)	0.311	(0.463)
Education:				
Primary	0.094	(0.292)	0.098	(0.297)
Secondary	0.455	(0.498)	0.483	(0.500)
Job tenure:				
$<$ 1 year	0.159	(0.366)	0.085	(0.279)
1-3 years	0.259	(0.438)	0.312	(0.463)
4-7 years	0.206	(0.405)	0.217	(0.412)
Occupation level:				
Qualified man.	0.086	(0.280)		
Qualified non-man.	0.218	(0.413)		
Intermediary	0.341	(0.474)		
High	0.290	(0.454)	0.268	(0.443)
Firm size:				
1-19 employees	0.291	(0.454)	0.305	(0.461)
20-49 employees	0.176	(0.381)	0.156	(0.363)
50-99 employees	0.124	(0.329)	0.245	(0.430)
Observations	237,536		15,785	

Notes: Based on *Swiss Labor Force Survey* (SLFS) 2016-2023 and *Swiss Adult Education Survey* (SAES) 2016, 2021 estimation samples. SAES does not contain information on occupation levels, except the highest level what is defined as managerial/leadership level. The 50-99 employees firm size is for SAES defined as 50-249 employees. Tertiary education, job tenure of longer than 8 years, unskilled occupation level, and firm size of 100+ employees are the underlying reference categories. In our regressions, we additionally control for 13 industry dummies.

Table 4: Minimum wages effects on training incidence and intensity

	training incidence			training intensity		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All training						
ATT (minimum wage)	0.038**	0.037***	0.040***	0.457**	0.422***	0.385***
	(0.018)	(0.012)	(0.010)	(0.194)	(0.136)	(0.096)
Panel B: Job training						
ATT (minimum wage)	0.037**	0.036***	0.038***	0.398**	0.361***	0.321***
	(0.018)	(0.014)	(0.012)	(0.183)	(0.131)	(0.101)
year & canton FEs	yes	yes	yes	yes	yes	yes
Covariates:						
sociodemographic		yes	yes		yes	yes
job-specific			yes			yes

Notes: Using SLFS 2016-2023 and Callaway and Sant'Anna (2021) doubly robust estimator with inverse probability weights, reported are the ATT of minimum wage treatment. ($N = 237,536$) Dependent variable: training incidence dummy (columns 1 to 3) or training hours (columns 4 to 6). In Panel A, the dependent variable includes all training types, in Panel B it includes only job-related training. Sociodemographic control variables include gender, age, age squared, migration background, two education dummies, and three tenure dummies. Job-specific variables include four firm size dummies, four occupation level dummies, and 12 sector dummies. Standard errors, clustered on the canton$\times$year level, are reported in parentheses. Significance on 0.1, 0.05, and 0.01 is indicated as *, **, ***.

Table 5: Minimum wage effects using Swiss Adult Education Survey

	Equiv. estimates: SLFS	Equiv. estimates: SAES	Financial participation: only firm	Financial participation: partly employee	Training timing: only during working hours	Training timing: partly outside working hours	Training topic: aligned with econ. sector	Training topic: not aligned with econ. sector	Training only at the workplace
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Training incidence									
min. wage	0.067***	0.051***	0.052***	-0.021***	0.027***	0.020	-0.010	0.025***	-0.029
	(0.009)	(0.016)	(0.010)	(0.007)	(0.009)	(0.014)	(0.013)	(0.008)	(0.018)
Adjusted R^2	0.123	0.070	0.110	0.037	0.089	0.099	0.172	0.142	0.080
Panel B: Training intensity									
min. wage	0.807***	0.857***	0.381**	-0.198	0.439**	0.440*	0.067	0.753***	0.255***
	(0.266)	(0.256)	(0.148)	(0.155)	(0.187)	(0.256)	(0.091)	(0.212)	(0.077)
Adjusted R^2	0.058	0.021	0.018	0.010	0.012	0.017	0.032	0.019	0.015

Notes: TWFE estimations based on SAES 2016 & 2021, except column (1), where SLFS 2016 & 2021 is used to compare equivalent estimates from two surveys. The number of observations is 60,062 for the SLFS column and 15,785 for all other–SAES–columns. All columns include canton and year fixed effects and a full set of controls (columns (3) or (6) of Table 4). Observations are weighted by their sampling weights. Standard errors, clustered on the canton$\times$year level, are reported in parentheses. Significance on 0.1, 0.05, and 0.01 is indicated as *, **, ***.

Table 6: Minimum wage effects on training incidence and intensity: Bite specifications

	Training incidence		Training intensity	
	(1)	(2)	(3)	(4)
Panel A: Low-wage groups				
min. wage	0.036***	0.033**	0.393***	0.312**
	(0.013)	(0.013)	(0.150)	(0.150)
wage below 25 Fr.	-0.078***	-0.105***	-0.493***	-0.765***
	(0.004)	(0.005)	(0.090)	(0.093)
wage below 25 Fr. $\times$ min. wage	0.030**	0.034**	0.490*	0.581**
	(0.014)	(0.014)	(0.259)	(0.254)
wage 25-30 Fr.		-0.075***		-0.771***
		(0.004)		(0.075)
wage 25-30 Fr. $\times$ min. wage		0.026**		0.593**
		(0.013)		(0.242)
Adjusted R^2	0.118	0.120	0.049	0.050
Panel B: Short-tenure groups				
min. wage	0.042***	0.039***	0.498***	0.457***
	(0.013)	(0.013)	(0.147)	(0.161)
tenure $<$ 1 year	0.015***	0.015***	0.810***	0.807***
	(0.004)	(0.004)	(0.098)	(0.098)
tenure $<$ 1 year $\times$ min. wage	-0.007	-0.005	-0.289	-0.248
	(0.009)	(0.010)	(0.244)	(0.249)
tenure 1-3 years		0.009***		0.499***
		(0.003)		(0.056)
tenure 1-3 years $\times$ min. wage		0.009		0.141
		(0.010)		(0.187)
Adjusted R^2	0.117	0.117	0.049	0.049

Notes: Estimates stemming from bite specification (Equation 3) using SLFS 2016-2023 ($N = 237,536$). Dependent variable: training incidence dummy (columns (1) and (2)) or training hours (columns (3) and (4)). In Panel A, the bite relates to low wage and regards short tenure, in Panel B. All estimations additionally include canton and year fixed effects and a full set of covariates (columns 3 or 6 of Table 4). Observations are weighted by their sampling weights. Significance on 0.1, 0.05, and 0.01 is indicated as *, **, ***.

Table 7: Minimum wage effects on outcomes other than training

	Tenure		Part time employment		Frequent	Temporary
	in years	in days	in %	less than 90%	overtime	contract
	(1)	(2)	(3)	(4)	(5)	(6)
ATT (minimum wage)	-0.181	-66.113	-0.261	-0.003	-0.009	-0.000
	(0.222)	(80.863)	(0.721)	(0.009)	(0.027)	(0.003)
Means of d.v.	8.4	3073	83.14%	0.36	0.36	0.08
Observations	237,513	237,513	237,536	237,536	237,536	241,862

Notes: Using SLFS 2016-2023 and Callaway and Sant'Anna (2021) doubly robust estimator with inverse probability weights, reported are the ATT of minimum wage treatment. All estimations include canton and year fixed effects. Estimates in columns (3) to (6) use full set of controls (as in columns (3) or (6) of Table 4). In the first two columns tenure is dropped from control variables. Number of observations is smaller in the first two columns due to missing values in tenure reports. Number of observations is larger in column (6) because we include workers having all types of contracts, including seasonal, employment program, jobber, or internship contracts. Standard errors, clustered on the canton×year level, are reported in parentheses. Significance on 0.1, 0.05, and 0.01 is indicated as *, **, ***.

Table 8: Minimum wage effects on workforce composition

	Apprenticeship training		Skills of workforce		Foreign
	firm trains	nr. apprentices	primary educ.	low-skilled	workers
	(1)	(2)	(3)	(4)	(5)
ATT (minimum wage)	0.007	-0.004	-0.001	0.002	-0.005
	(0.015)	(0.169)	(0.010)	(0.013)	(0.008)
Means of d.v.	0.18	1.24	0.16	0.06	0.12
	Working time		Tenure		Temporary
	avg. fraction	$< 90\%$	< 1 year	1-3 years	contracts
	(6)	(7)	(8)	(9)	(10)
ATT (minimum wage)	-0.007	0.006	-0.003	-0.013*	-0.011
	(0.009)	(0.009)	(0.007)	(0.008)	(0.009)
Means of d.v.	0.76	0.49	0.15	0.27	0.05

Notes: Table reports ATT of minimum wage treatment based on Callaway and Sant'Anna (2021) doubly robust estimator with inverse probability weights using firm-level aggregated ESS 2016, 2018, 2020, 2022 data. Each estimate stems from a separate regression of a workforce structure measure (table headings) on minimum wage treatment dummy. For the estimates in the first two columns we matched the ESS data with registry data of all firms involved in apprenticeship training in Switzerland (SBG). Dependent variable in column (1) is a dummy that equals one if firm has at least one apprentice and in column (2) it is the number current of apprentices in firm. In column (6) the dependent variable is the firm-level average fraction of full time equivalent across all workers. In all remaining columns dependent variable is a share of workers of particular type (cf. column heading). All estimations include canton and year fixed effects, four firm size dummies, public firm dummy, and 13 industry dummies. Number of observations in each column is 319,733. Significance on 0.1, 0.05, and 0.01 is indicated as *, **, ***.

Table 9: Minimum wage effects on training incidence and intensity: Sensitivity analyses

	Weights		Excluding		Including	Panel	Non-linear
	no	adjusted	retail	hospitality	vote dummy	individual FE	model
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Training incidence							*logit*
Minimum wage	0.048***	0.038***	0.046***	0.043***	0.042***	0.034***	0.190***
	(0.012)	(0.013)	(0.013)	(0.013)	(0.013)	(0.012)	(0.061)
Minimum wage vote					-0.021		
					(0.020)		
R^2	0.120	0.120	0.119	0.114	0.117	0.006	0.093
Panel B: Training intensity							*tobit*
Minimum wage	0.478***	0.570***	0.502***	0.458***	0.448***	0.227	1.485***
	(0.133)	(0.174)	(0.164)	(0.144)	(0.145)	(0.165)	(0.424)
Minimum wage vote					0.298		
					(0.513)		
R^2	0.052	0.047	0.048	0.048	0.049	0.004	0.028
Observations	237,536	237,536	210,044	229,774	237,536	232,637	237,536

Notes: Estimates reported in columns (1) to (5) are based on Table 4 using TWFE specification and SLFS 2016-2023. All estimations include canton and year fixed effects and the full set of controls (as in columns (3) or (6) of Table 4). Observations are weighted by their sampling weights. Column (6) reports individual panel fixed effects (FE) results. Column (7) reports in the upper panel a marginal effect of minimum wage on the training incidence based on a logit regression and in the lower panel it shows a tobit estimate of the marginal effect of minimum wages on number of training hours. In the R^2 rows we report adjusted R^2s (columns (1) to (5)), within R^2 in column (6), and pseudo R^2 in column (7). Standard errors, clustered on the canton×year level, are reported in parentheses. Significance on 0.1, 0.05, and 0.01 is indicated as *, **, ***.

Table 10: Minimum wage effects using *Kaitz* index

	canton-year		canton-year-occupation		canton-year-sector	
	2021-23	2023	2021-23	2023	2021-23	2023
	4 cantons	5 cantons	4 cantons	5 cantons	4 cantons	5 cantons
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Training incidence						
Kaitz index $(w_{\text{MW}}/\bar{w})$	0.086	0.650**	-0.674***	-0.717***	-0.289***	-0.235
	(0.147)	(0.175)	(0.068)	(0.058)	(0.073)	(0.133)
Adjusted R^2	0.116	0.120	0.115	0.118	0.104	0.102
Panel B: Training intensity						
Kaitz index $(w_{\text{MW}}/\bar{w})$	1.882	1.067	-3.765***	-5.851*	-1.826**	-0.860
	(3.420)	(1.596)	(0.685)	(2.151)	(0.800)	(1.043)
Adjusted R^2	0.027	0.031	0.026	0.028	0.023	0.027
FEs			canton	canton	canton	canton
	year		year		year	
Controls:	occupation	occupation			occupation	occupation
	sector	sector	sector	sector		

Notes: Estimations based on SLFS 2021-2023 and a subset of treated cantons during treatment (see table heading). Observations are weighted by their sampling weights. $N = 11,890$ (odd columns) and $N = 4,784$ (even columns). Due to the variation of the *Kaitz* index values, estimations include varying set of fixed effects and controls (see bottom part of the table). But all estimation include the full set of sociodemographic controls: gender, age, age squared, migration background, two education dummies, and three tenure dummies. Standard errors, clustered on the canton×year level, are reported in parentheses. Significance on 0.1, 0.05, and 0.01 is indicated as *, **, ***.

Appendix

Table A1: Minimum wage effects on training incidence and intensity: TWFE

	training incidence			training intensity		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All training						
minimum wage	0.038***	0.038***	0.041***	0.415***	0.420***	0.456***
	(0.012)	(0.012)	(0.013)	(0.140)	(0.146)	(0.144)
Adjusted R^2	0.018	0.085	0.118	0.012	0.037	0.049
Panel B: Job training						
minimum wage	0.034***	0.034***	0.038***	0.378***	0.384***	0.417***
	(0.011)	(0.011)	(0.012)	(0.121)	(0.124)	(0.123)
Adjusted R^2	0.013	0.080	0.116	0.010	0.036	0.048
canton & year FEs	yes	yes	yes	yes	yes	yes
Controls:						
sociodemographic		yes	yes		yes	yes
job-specific			yes			yes

Notes: Replication of Table 4 using TWFE specification. Estimations based on SLFS 2016-2023. All estimations include canton and year fixed effects and an expanding set of controls (see bottom of the table). Observations are weighted by their sampling weights. Number of observations is in all specifications $N = 237,536$. Dependent variable: training incidence dummy (columns 1 to 3) or training hours (columns 4 to 6). In Panel A, the dependent variable includes all training types, in Panel B it includes only job-related training. Sociodemographic control variables include gender, age, age squared, migration background, two education dummies, and three tenure dummies. Job-specific variables include three firm size dummies, four occupation level dummies, and 12 sector dummies. Standard errors, clustered on the canton×year level, are reported in parentheses. Significance on 0.1, 0.05, and 0.01 is indicated as *, **, ***.

Table A2: Minimum wage effects on training incidence and intensity using minimum wage treatment intensity

	training incidence			training intensity		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All training						
minimum wage intensity	0.078***	0.079***	0.082***	0.918***	0.933***	0.961***
	(0.022)	(0.023)	(0.024)	(0.260)	(0.270)	(0.268)
Adjusted R^2	0.018	0.083	0.114	0.012	0.037	0.047
Panel B: Job training						
minimum wage intensity	0.069***	0.070***	0.073***	0.814***	0.830***	0.852***
	(0.021)	(0.021)	(0.022)	(0.226)	(0.232)	(0.231)
Adjusted R^2	0.013	0.078	0.112	0.010	0.036	0.046
Fixed effects:						
canton & year FEs	yes	yes	yes	yes	yes	yes
Covariates:						
sociodemographic		yes	yes		yes	yes
job-specific			yes			yes

Notes: Replication of Table 4 using TWFE specification. Dependent variable: minimum wage treatment intensity and is equal to zero if no treatment and to proportion of the cantonal minimum wage in median cantonal wage in $t-1$ during treatment. Estimations based on SLFS 2016-2023. All estimations include canton and year fixed effects and an expanding set of controls (see bottom of the table). Observations are weighted by their sampling weights. Number of observations is in all specifications $N = 237,536$. Sociodemographic control variables include age, age squared, gender, three education dummies, and three tenure dummies. Job-specific variables include three firm size dummies, four occupation level dummies, and 13 sector dummies. Standard errors, clustered on the canton×year level, are reported in parentheses. Significance on 0.1, 0.05, and 0.01 is indicated as *, **, ***.

Table A3: Descriptive statistics of outcome and control variables used in the workforce structure analysis (see Table 8)

	mean	(std. dev.)	min	max
Outcomes:				
Firm trains apprentices	0.185	(0.388)	0	1
Number of apprentices	1.240	(11.359)	0	1,305
Share of workers with only primary education	0.156	(0.270)	0	1
Share of low-skilled workers	0.065	(0.171)	0	1
Share of foreign workers	0.120	(0.219)	0	1
Fraction of full-time	0.762	(0.225)	0.01	1.44
Share of workers $< 90\%$	0.490	(0.355)	0	1
Share of newly hired workers ($<$ 1 year)	0.155	(0.221)	0	1
Share of workers with a short tenure (1-3 years)	0.271	(0.260)	0	1
Share of workers with temporary contracts	0.046	(0.149)	0	1
Controls:				
Firm size 20-49	0.090	(0.286)	0	1
Firm size 50-249	0.214	(0.410)	0	1
Firm size 250-999	0.117	(0.322)	0	1
Firm size $\geq 1,000$	0.312	(0.463)	0	1
Public sector firm	0.204	(0.403)	0	1

Notes: Based on *Employment Structure Survey* (ESS) 2016, 2018, 2020, 2022 estimation sample. Original ESS data are on the employee level. We use and report here firm-level aggregated data (N = 319,733). Firms size 1-19 is a reference category.

Table A4: Alignment between economic sectors and fields of training courses we used to codify aligned training (see Table 5, column (7))

General Classification of Economic Activities (NOGA 2008, 1 digit)	*Courses classifications* (ISCED fields, 1 digit)
A Agriculture, forestry and fishing	**8** Agriculture, Forestry, Fisheries and Veterinary
C Manufacturing **F** Construction	**7** Engineering, Manufacturing and Construction
J Information and communication	**6** Information and Communication Technologies **3** Social Sciences, Journalism and Information
K Financial and insurance activities **L** Real estate activities **N** Administrative and support service activities **O** Public administration and defense; compulsory social security	**4** Business, Administration and Law
P Education	**1** Education
Q Human health and social work activities	**9** Health and Welfare
R Arts, entertainment and recreation	**2** Arts and Humanities
M Professional, scientific and technical activities	**5** Natural Sciences, Mathematics and Statistics
S Other service activities	**10** Services

Notes: Based on codebook to *Swiss Adult Education Survey* (SAES) 2016, 2021. All above "pairs" of economic activities and training fields are accounted in the aligned training measures. All other pairs, or when the course field is missing, we account in the not-aligned training measures (Table 2, SAES columns).