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Minimum Wages and Provision of Training

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Abstract

We find a substantial and long-lasting positive effect of the introduction of regional minimum wages on training incidence and intensity. We apply a stacked difference-in-differences estimation to identify the dynamic effect of high minimum wages introduced in several Swiss cantons between 2018 and 2022. Employers invest more in formal training for retained employees during working hours, covering content beyond their main economic activity. We conclude that employers increase the productivity of their employees to retain the underlying rents. We carefully rule out confounding factors and offer an explanation for the difference between our and some earlier findings.

JEL-Code: J08, J51, M53
Keywords: minimum wages, continuing 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 level and 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 training efforts induced by minimum wages are much less documented (Dube and Lindner, 2024). Changes in training efforts however may be the central alternative to dismissals of low-wage employees and they potentially affect many individuals over a long time period after minimum wage increases (Neumark and Wascher, 2003).

The direction of the training effects of minimum wages is disputed. Employers may reduce training for retained low-wage workers because wage reductions to pay for training costs are not viable any more (Hashimoto, 1982). Training also may be considered a fringe benefit and thus be reduced to compensate the wage increases (Neumark and Wascher, 2001). Lower training efforts may be feasible because the applicant pool improves with rising minimum wages and job entrants need less initial and continuing training than before (Meer and West, 2016; Giuliano, 2013). Finally, a decrease in personnel turnover may reduce training needs (Coviello et al., 2022). On the other hand, minimum wages may induce an increase in training efforts if employers are able to retain the rents they extract from their low-wage workers. Their incentive to increase the productivity of retained low-wage employees is higher if training costs are relatively low for the productivity effect needed (Acemoglu and Pischke, 2003). Employers may also 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 more continuing training to be able to keep their better paid jobs (Leighton and Mincer, 1981). Finally, the disappearance of less competitive firms may induce low-wage workers to move to establishments with higher training incidence (Acemoglu, 2001; Dustmann et al., 2022).

In this paper we empirically assess which of the potential training reactions evolve after the

introduction of regional minimum wages in Switzerland. Leveraging the staggered implementation of minimum wages in several cantons[1] between 2018 and 2022, we show that employers substantially intensify the training incidence by about eight and the training intensity by about fourteen percent over several years. Training efforts increase stronger for low-wage workers, we however also find substantial positive "ripple" effects on training levels of employees who earn much more than the new minimum wage.

Our study directly contributes to the, up until now, relatively thin literature on the effects of minimum wages on training. In their recent meta-analysis, Doucouliagos and Zigova (2025) identify only 14 relevant studies and collect 432 distinct estimates of minimum wages on training reported in these studies. A large majority of these estimates are insignificant but there also is evidence of training effects in both directions. Grossberg and Sicilian (1999) for example find that male workers who hold a minimum wage job in the United States (US) receive less training. Acemoglu and Pischke (2003) show that increases in the US federal minimum wage in the years 1990 and 1991 lead to modest positive but also to negative effects on training investments of low-wage workers depending on the competitiveness of the sector. Arulampalam et al. (2004) show positive effects of the introduction of the national minimum wages in the United Kingdom (UK) on training of affected workers.[2]

In line with Arulampalam et al. (2004), we find positive effects of the introduction of minimum wages on training. Moreover, we show the empirical relevance of measuring the training effects on all employees instead of focusing only on employee sub-groups that are stronger affected by minimum wages, such as young employees, employees with low experience, low-wage employees or low-productivity employees (e.g., Neumark and Wascher, 2001; Allegretto et al., 2017; Ku, 2022). The definition of selected treatment and control groups may bias the measurement of treatment effects in the presence of confounders and spillovers (Cengiz et al., 2022). Specifically, we show that the measurement of training effects on the basis of sub-groups such as

[1] A Swiss canton is one of the 26 member states of the Swiss Confederation, each with its own constitution, legislature, and judiciary.

[2] As a consequence of this range of results, it is not surprising that Doucouliagos and Zigova (2025) estimate only a small and significantly negative meta-mean. This meta-mean turns to zero when correcting for publication bias.

employees below or slightly above the minimum wage in comparison to employees with much higher earnings would have negatively biased the effect estimates (Cengiz et al., 2019).

Additionally, 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 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). We find that the positive training effects hold for several years after minimum wage introduction and increase over time for some cantons.

Our training effects are induced by the exogenous introduction of intensive regional minimum wages, instead of small and possibly endogenous changes in the minimum wage level. Swiss cantonal minimum wages have the highest absolute level in an international comparison.[3] The average *Kaitz* index level was 0.54 in the years of their respective introduction.[4] The cantonal minimum wages therefore had immediately a strong bite. In addition, there are random elements with respect to the timing of the introduction of cantonal minimum wages. More specifically, in Switzerland regional minimum wage introductions are induced by popular votes. This procedure arguably reduces the endogeneity risk in comparison to other countries in which (regional) politicians or industrial relation agents directly decide on the introduction or adjustment of minimum wage levels (Dube and Lindner, 2024). We also show that the measured effects change when we use, as source of identification, minimum wage intensity *during* treatment, instead of minimum wage introductions. The estimated training effects are significantly positive only if we compare cantons with and without minimum wages. When we limit our analysis to cantons treated by minimum wages and identify the effect of changes in minimum wage intensity after their intro-

[3] 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 USD 3,226 per month. The second highest minimum wage level is reported for Luxembourg (USD 2,459). Further selected monthly minimum wage levels are: Germany (USD 2,363), France (USD 2,016), and United Kingdom (USD 1,987), compare ILOSTAT (2024).

[4] The *Kaitz* index measures the nominal minimum wage divided by the median wage in the previous period. A level of 0.54 is relatively high in an international comparison (see the OECD Earnings and Wage Data Base: https://data-explorer.oecd.org. The *Kaitz* indices of national minimum wages in Germany and the UK were 0.48 and 0.42 at the point in time of their introduction, for example.

duction (as in e.g. Neumark and Wascher, 2001; Acemoglu and Pischke, 2003; Hara, 2017), the training effect becomes more ambiguous and turns significantly negative in some specifications.

Our study further contributes to the literature by delineating mechanisms behind the training effect. We can show that the additional training is mainly sponsored by the employer and is on topics not directly aligned with the main economic activity of the employer, i.e. very likely of a general nature. We also show that the additional training efforts are directed at retained employees. This finding rules out changes in hiring behavior. We can also exclude changes in the labor force structure induced the training increase. More specifically, we demonstrate that there are no changes in the average tenure, workforce skill or education composition. These findings are in line with Forsythe (2023) who finds no evidence that employers reorganize their occupational mix when minimum wages increases. Finally, we can also exclude the option that employers change their apprenticeship training, as shown by Schumann (2017) for the construction sector in Germany. Likewise we find no evidence that employers substitute their low-wage workers away with apprentices. We conclude that employers invest into training to retain their low-wage workers and acquire the rents they provide (Acemoglu and Pischke, 1999, 2003).

2 Policy background

The introduction of cantonal minimum wages in Switzerland is rooted in earlier sectoral wage-setting initiatives. In 1998, Swiss unions launched a large-scale public campaign advocating for minimum wages under the slogan "No wage below 3,000 Swiss francs". The campaign induced some—mainly low-wage—sectors such as the hospitality, retail, and the garment industry to include minimum wages in their collective wage bargaining agreements. The minimum wages in collective agreements effectively decreased the share of low-wage employees in these sectors (Oesch and Rieger, 2006).

The sector minimum wages however entail many exceptions for specific employee groups and employers covered by the relevant collective agreements (Berger and Lanz, 2020).[5] In most

[5] 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. The regular

sectors only employers covered by a collective agreement have to implement the sectoral minimum wage and the share of employees covered by a collective bargaining agreement is substantially lower than 50% (Baumberger, 2021).

The Swiss unions and some cantonal parliaments started in 2006 a second campaign for generally binding (regional) minimum wages to reduce the share of employees left uncovered by minimum wage clauses in collective agreements (Rieger and Gallusser, 2023). In liberal Switzerland important regulation decisions, that apply to everybody, such as the introduction of national or regional minimum wages are taken by popular vote, rather than by politicians or industrial relation partners. Although the popular vote on minimum wages on the national level failed in 2014, some attempts to introduce cantonal minimum levels were successful. The first minimum wage was implemented in 2018 in the canton of Neuchâtel. The canton Jura adopted a minimum wage in 2020, followed by Genève and Ticino at the turn of the years 2020/2021. In July 2022, cantonal minimum wages were introduced in Basel-Stadt. Table 1 presents a chronological overview of successful cantonal popular votes and their adoption dates.

The five cantons with minimum wages 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 generally exempted from cantonal minimum wages: apprentices, students in internships, and workers employed via work integration programs. Additionally, employees in the agricultural sector are either fully exempt (Ticino) or they have a minimum wage that is between 3-6 Francs lower than the cantonal hourly minimum wage (Neuchâtel and Genève). In the cantons Jura, Ticino, and Basel-Stadt the minimum wages do not apply for employees subject to collective minimum wages even if their wage is below the cantonal minimum wage (Rieger and Gallusser, 2023).

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 adoption in the canton of Genève. Ticino had the lowest initial minimum wage at around 19.5 Francs. Cantonal min-

hourly sector minimum wage in 2018 for example was 20.33 (Swiss) Francs. Small and seasonal establishments however could apply a lower minimum wage of 18.98 Francs and 19.58 Francs, respectively.

imum wages gradually increased over time, largely to account for inflation. To set the cantonal minimum wages in perspective, we also show the sectoral 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 above the the 5% percentile (except in 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 wage and cantonal minimum wage levels translate into differences in minimum wage intensities. To integrate average wage differences between cantons, we calculate the *Kaitz* index for each canton. For the minimum wages in the initial year, the *Kaitz* index values lie between 0.465 in Basel-Stadt, 0.509 in Genève, 0.536 in Neuchâtel, 0.547 in Jura, and 0.579 in Ticino. Although Ticino has the lowest absolute minimum wages, it has the strongest intensity.

Besides the high intensity 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.[6]

To demonstrate the effectiveness of Swiss minimum wages, we depict the evolution of the fractions of low-wage workers in cantons treated by minimum wages and in all remaining cantons on the basis of firm and household survey data, see Figure 2. We focus on two categories of low-wage workers: those earning below the minimum wage and those earning up to 15% more than the minimum wage. Based on the *Employment Structure Survey* (ESS) we find a gradual decrease

[6]The precise rules can be found in the federal law on the supplementary measures for posted employees and the control over minimum wages in regular employment contracts (*Entsendegesetz*, EntsG 823.20).

in the fraction of low wage workers in treated cantons from 6% in 2016 to about 3% in 2022. Similarly, based on the *Swiss Labor Force Survey* (SLFS) we find a drop of this fraction from 7% to 4%. A portion of this decrease is compensated by an increase of the share of workers in the category slightly above the minimum wage. We do not observe a decrease in the share of low-wage workers in the non-treated cantons. We also verify the effectiveness of minimum wages and regress a minimum wage treatment dummy on log wages of low-wage workers as proposed by Manning (2021)[7]. On the basis of the SLFS and ESS survey data, we estimate significantly positive wage semi-elasticities with respect to minimum wage dummies between 1.4% and 4.1% depending on survey and the inclusion of further controls (see Table A1).

3 Data

We study the impact of the staggered introduction of cantonal minimum wages on training using information on minimum wage adoption date and multiple survey data sources. For the cantons with minimum wages, we set our minimum wage treatment dummy to one from the first full policy year onward, while it equals zero in all other instances. The first *full* policy year is 2018 for Neuchâtel, 2020 for Jura, 2021 for Genève and Ticino, and 2023 for Basel-Stadt (Table 1).

Our primary data set is the SLFS, the most comprehensive source of information on labor market issues, based on a representative sample of the Swiss working age population.[8] 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. Ultimately, we use our data as repeated cross-section because we observe each individual only twice—in the same quarter of two consecutive years. The annual dataset contains

[7] Manning (2021) appeals to check the actual effectiveness of the minimum wage policy as *"Before considering the impact of the minimum wage on teen employment, it is important to verify that there is a detectable impact of the minimum wage on actual wages received by workers; failure to demonstrate this in any paper means there should be skepticism about any subsequent findings that the minimum wage has an employment effect."* (p. 8).

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

around 60,000 individuals. In addition to demographic variables, the SLFS data contain a rich set of information on education, employment, earnings, and most importantly for our research, on continuous training.

For our analysis, we exclude employees who are older than the normal retirement age (65 years), who 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 267,310 individual observations for the period 2016–2024. Our 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 throughout the whole period.[9] We define all employees in treated cantons during treatment as treatment group, which contains 20,363 individuals.

The SLFS survey offers only basic information on continuing training. To be able to explore changes in the characteristics of training, we additionally use the *Swiss Adult Education Survey* (SAES).[10] 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. The SAES is conducted only every 5 years and comprises a much smaller sample than the SLFS. We employ the last two SAES waves from 2016 and 2021. The two waves fit well to our setting: in 2016 there was no cantonal minimum wage in place, in 2021 four cantons had their minimum wages in place. The SAES survey always draws a new representative sample of the permanent resident population. Equivalently to SLFS, we limit the SAES to the currently employed working age population (15-65 years) that is not exempted from minimum wages. This leaves us with a pooled sample size of 15,785 individual observations of which 2,910 individuals are treated with minimum wages.

Table 2 shows descriptive statistics of all training measures from the SLFS and SAES sam-

[9] We did not consider the SLFS data prior to 2016 because the measurement of training variables has changed in this year.

[10] See *Mikrozensus Aus- und Weiterbildung* (MZB) at https://www.bfs.admin.ch/bfs/de/home/statistiken/bildung-wissenschaft/erhebungen/mzb.html.

ples. Training is self-reported in both surveys and relates to courses, private lessons, seminars, or conferences in the last four weeks prior to the interview (SLFS) or one year prior to the interview (SAES). If participants affirm this question, the measure of training incidence equals one and is zero otherwise. If respondents attended training, they are asked to recollect the number of training hours, which is our measure of training intensity. Training incidence and intensity are asked separately with respect to any course and specifically with respect to job-related courses in the SLFS. We do not know, however, in how many courses respondents participated. We construct four different training outcomes: training incidence dummy and training intensity in terms of training hours for all training measures and job-related training (cf. Table 2, SLFS columns).[11]

SAES also collects training incidence and intensity in training hours but allows us to explore much more details about training characteristics. We can differentiate between fully firm-financed training and training with workers' financial participation. We also can differentiate between the timing of training—exclusively during working hours vs. partly or fully outside working hours. In addition, we matching the training course content with the classification of economic activities of the employer we obtain training that is aligned with the economic activity of the employer, and the one which is not.[12] Finally, SAES also collects information upon informal training on-the-job. The training questions are referenced to the full past year prior the interview date. Thus to obtain equivalence with the SLFS training intensity measures, of which the reference period is last month, we divide the training intensity by 12.[13]

The SLFS and SAES surveys show that more than 85% of continuing training is job-related and that on average there are about 3.5 training hours per month. Our descriptive analysis in addition reveals that employees finance only a minor part of training and that more than half of the training takes place outside working hours. Finally, training content is frequently not aligned

[11] Given training is a crucial variable in our study, for our training measure in t we use information on training responses from both consecutive waves *within* the same year. For training incidence our measure then equals one if the respondent reports training in any of the two waves of year t. For training intensity we calculate average training hours reported in the two waves of year t.

[12] We use details on the training content (ISCED fields) and match them with the classification of main economic activities of the employer (NOGA 2008), see Table A4.

[13] As the period of retrospection is different in SLFS and SAES, we cannot obtain equivalent training incidence measures from SLFS and SAES. As a result the training incidence measures are slightly larger for the SAES survey but about the same in both surveys for the training intensity (Table 2).

with the main economic activity of the employer (Table 2).[14]

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 such as gender, age, migration background, job tenure, occupational level, firm size, and industry. In Table 3 we report statistical properties of the control variables. To align our analyses based on SLFS and SAES, we use the same set of control variables for both surveys.[15]

Finally, we use the ESS—a survey of Swiss establishments to measure changes in the workforce structure.[16] The survey is conducted bi-annually and surveys a representative sample of around 35,000 firms (∼80,000 establishments). 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 establishment-level information. Minimum wages may cause changes in the workforce. The ESS data allow 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 the ESS does not contain information on apprentices, we match it with Swiss administrative data on the number of currently trained apprentices for each establishment involved in apprenticeship training.[17] This data match allows us to create a dummy variable whether an establishment trains apprentices or not, and a count variable that measures the number of apprentices. Similarly to our analysis

[14]The sums of the *aligned* (0.245) and *not aligned* (0.408) incidence means in Table 2 exceed the overall training incidence of 0.473 because individuals may be counted in both when they take more courses, and of different types. We find a similar pattern for *during* and *outside* working hours and the two training sponsor training dummies. The training intensity sums for the three pairs are always somewhat smaller than the general measures because there are missing in training characteristics.

[15]There are very few differences in variables between both data sets: The migrant variable is a dummy in SLFS that equals one for people without the Swiss nationality, in SAES the dummy also equals one for 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 firm size is defined more broadly–from 50 to 249 employees.

[16]See https://www.bfs.admin.ch/bfs/en/home/statistics/work-income/surveys/ess.html.

[17]The registry data is referred to as SBG (*Statistik der beruflichen Grundbildung*), or *Vocational education and training statistics*, see https://www.bfs.admin.ch/bfs/en/home/statistics/education-science/diploma/upper-secondary/vocational-training.html.

based on SAES, ESS ends in 2022, when the minimum wage policy applies only in four cantons. Nevertheless we can make a number of relevant tests of potential minimum wage effects on the workforce structure. We report descriptive statistics of the ESS variables in our workforce structure analysis in the online appendix (Table A2).

4 Econometric approach

The gradual 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 main estimation strategy employs an event-study difference-in-differences estimator that accommodates heterogeneous treatment effects and staggered adoption of minimum wages across cantons (Callaway and Sant'Anna, 2021). The estimator relies on a doubly robust identification strategy (Sant'Anna and Zhao, 2020) meaning that treatment effects are consistently estimated as long as either the model for outcome pattern or the model for treatment assignment is correctly specified. Such approach improves the reliability of our estimates in observational data settings where model mis-specification is a clear concern, and it adds flexibility in handling treatment effect heterogeneity and staggered timing.

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 introduce minimum wages and $t - E_g = k$ is an indicator for being k years from the minimum wage introduction. Our main 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 information as well as 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 our standard errors ε_{it} on

canton-year level.

For for each group g and year relative to the minimum wage introduction k we derive a treatment effect on the treated $ATT_{g,k}$. We implement inverse probability weighting approach to make the control and treatment group observations more comparable on observable characteristics of individuals ($\mathbf{X}_{it}$). To obtain an overall ATT—over all groups and periods—we aggregate group-time estimates $ATT_{g,k}$ using weights $w_{g,k}$ that are proportional to the size of each treatment group, so that larger treated cohorts contribute more to the overall estimate (Callaway and Sant'Anna, 2021). In all regressions using SLFS and ESS data we report ATT. When estimating Equation 1, we exclude not-yet-treated periods of treated cantons from the control group to ensure credible comparisons.[18]

Throughout our analysis we also use variants of a two-way-fixed effect specification (e.g., as in Callaway and Sant'Anna, 2021, equation 3.2 or Bellmann et al., 2017, equation 2). The SAES dataset leaves no room for group-wise heterogeneity because only two years are available. As a result all treated cantons have equal treatment length and it is sufficient to use the standard diff-in-diffs method:

$$T_{it} = \gamma_0 \cdot D_{ct} + \mathbf{X}_{it}\beta + \alpha_c + \tau_t + \varepsilon_{it}, \tag{2}$$

where T_{it} is our training measure, D_{ct} is a minimum wage treatment dummy, α_c and τ_t represent the two-way fixed effects, and $\mathbf{X}_{it}$ is matrix of control variables. The estimated minimum wage effect is $\hat{\gamma}_0$. The specification Equation 2 is readily applicable when interacting treatment dummy with other variables or when the treatment is not binary.

In one of our subsequent analysis we focus on specific group of workers. Here we use a specification with an interaction term of the minimum wage dummy and a dummy marking individuals who are likely to be affected differently by minimum wages B_{ict} and explore heterogeneous effects by workers' characteristics. In the minimum wage-training literature a similar specification has been used previously for example in Grossberg and Sicilian (1999) or Arulampalam et al.

[18] We include not-yet-treated units in one of our sensitivity exercises.

(2004):

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

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

$$T_{it} = \gamma_0 \cdot MW_{ct} + \mathbf{X}_{it}\beta + \alpha_c + \tau_t + \varepsilon_{it}. \tag{4}$$

We use this specification in our reconciliation exercise where we specify the treatment intensity MW_{ct} as *Kaitz* index ($= w_{MW}/\bar{w}$), where w_{MW} is the valid minimum wage in canton c and year t, and $\bar{w}$ is an underlying median wage in year $t-1$.

In our sensitivity analyses we replace the canton fixed effects with individual fixed effects and estimate the models within an unbalanced panel framework. However, the SLFS sample observes the same individual only twice in two consecutive years.[19] The data structure therefore allows us to identify the effect only based on changes between $t, t+1$ for 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 estimation results including individual panel fixed effects.[20]

[19] 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.

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

5 Effects of minimum wages on training

5.1 Baseline effects

In Table 4 we report the 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 shows the results for job-related training only. The introduction of cantonal minimum wages increases the probability of receiving training on average by three percentage points and increases the training intensity on average by about half an hour per month. Given that the training incidence is at about 40%, the share of trained individuals increases by about 8%. On the intensive margin, minimum wages are associated with a 14% increase in training hours, considering the mean training intensity of 3.5 hours/month. The inclusion of sociodemographic and job-specific controls has no impact on the minimum wage ATT for training incidence and only slightly reduces the ATT for training intensity. More than 85% of all training is job-related (Table 2). 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 23 minutes instead of half an hour for all training, see Panel B in Table 4. The importance of job-related training in all training measures is further corroborated in Panel C: there is no minimum wage effect on other than job-related training incidence and only a very small positive effect on training intensity.[21] If we use the two-way fixed effects specification instead of the Callaway and Sant'Anna (2021) estimator, the effects are very similar (cf. Table A3). Given the role of the other than job-related training is minor, we focus in the subsequent analysis on measures based on all training.

The minimum wage effect on training may not evolve immediately. Employers may want to assess the consequences of the policy on their employees first, instead of changing their training effort immediately. Similarly, 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 (Equation 1). The event analysis

[21] The training intensity increase of 0.12 translate into seven more training minutes per month.

rejects the existence of pre-trends.[22] In the treated cantons, training intensity significantly increases already in the year of implementation and stay at the significantly higher level during the first five years after implementation. For training incidence, the effect is immediate, too, however not significant in the first two years. In the third year, there is an additional boost and the effect climbs to about five percentage points and remains there until the end of observation period.

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 efforts. In Neuchâtel, the training increases are only detectable three years after the minimum wage introduction. This lag leads to a zero ATT over the whole period. In Basel-Stadt, we see no change in any of the training measures. While the pooled event study rejects the existence of pre-trends, it is violated for some canton-groups, most notably for the smaller cantons of Neuchâtel and Jura for training incidence event graphs (Figure 4). Even if the Callaway and Sant'Anna (2021) estimator allows for flexible treatment effects and improves robustness, the identification still relies on a form of the parallel trends assumption conditional on covariates. As this assumption is not fulfilled for all canton-groups, the estimator may be biased in some cases. We interpret these effects with caution, accordingly.[23]

5.2 Training characteristics

In this part, we study the effect of minimum wages on training with different characteristics. We distinguish between training fully covered by employers, versus training partly or fully paid by employees. Furthermore, we differentiate between training taking place during vs. outside working hours, and by training content that is aligned with the main economic activity of the firm, vs. training content that goes beyond it. Finally, we look at informal workplace learning, such

[22] Figure 3 shows conditional event study estimates that use the full set of controls, e.g. Table 4, columns (3) and (6).

[23] Dube and Lindner (2024) argue that pre-trend testing is not always necessary or informative, especially if differences are unrelated to potential outcomes.

as learning from colleagues or learning-by-doing.[24] We use a TWFE specification (Equation 2) for the two waves available in SAES.[25] For comparability, we report in columns (1) and (2) of Table 5 the minimum wage effects on training incidence and intensity using equivalent samples from the years 2016 and 2021 from SLFS and SAES.

We find that the baseline effect in training incidence is driven by fully firm-financed training efforts. This also applies to a large extend for the training intensity effect (Table 5, columns (3) and (4)). The share of training measures that are partly or fully paid by the employee is generally low (Table 2). Training intensity of such training is unaffected by minimum wage treatment and for training incidence we even find a small reduction, pointing to a crowding-out effect (Table 5, column (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 of comparable size but not significant (Table 5, columns (5) and (6)). We aim to proxy specific vs. general training and group training measures by their content into fields directly aligned and not-aligned with the reported sector of economic activity. We find that after the introduction of minimum wages, mainly training that is not directly aligned with the economic sector increases and sector-aligned training remains unchanged (Table 5, columns (7) and (8)). We interpret this finding as an indication that mainly general training is positively affected by the minimum wage introduction. Finally, there is no minimum wage effect on the incidence and only a slight increase in the intensity of informal training at the workplace (Table 5, column (9)).

5.3 Effect heterogeneity

Prior studies often focus on sub-groups of workers who are either directly or more likely affected by minimum wages. In line with this literature, we analyze whether the training effect is concentrated on low-wage workers or recently hired employees. To replicate the previous papers with sub-groups of treated workers, we use an extension of a two-way fixed effects model (Equation 3) that includes interaction terms.

[24] This training is not part of the other training measures.

[25] 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.

We define two low-wage groups. The first group consists of employees who earn up to 15% more than the minimum wage.[26] The second group consists of employees who earn between 15% and 25% more than minimum wage earners. In Table 6 (Panel A) we report effects on training distinguished by wage groups and their interaction with minimum wage dummy. We find that there are considerable training effects for employees who earn 25% more than the minimum wage. After controlling for the two low-wage groups, the overall training incidence effect of minimum wages is about four percentage points and the overall training intensity effect is around 22 minutes more training per month. Employees in the two lowest wage groups have by two percentage points higher training incidence effect beyond the overall effect, albeit insignificant. The training intensity effect is about 0.56 higher for the lowest wage group and by 0.66 higher for the 15-25% group of low-wage earners. As the training effect is not concentrated on low-wage earners and employees with much higher wages still experience a positive training impact, we conclude that there are substantial ripple effects in training provision (Cengiz et al., 2019; Dube and Lindner, 2024).

We also find that training effects are not concentrated on job entrants. Table 6 (Panel B) shows that the training impact is evenly distributed over the three employee groups—overall, less than 1 year of tenure and 1-3 years of tenure. There is even a slight (albeit insignificant) negative minimum wage effect of job entrants—tenure less than one year. The additional training is thus mainly provided to retain employees. Initial training for job entrants does not seem to explain the employer training reaction to minimum wages.

5.4 Sensitivity analyses

Table 7 offers several variations of our baseline specification (cf. Table 4, Panel A, columns (3) and (6)). These variations include a different observation weighting, a reduced sample, different control groups and estimation methods. Our first sensitivity analysis controls the impact of the

[26]For the treated cantons always the valid minimum wage is used for the wage grouping. For the periods before their first treatment, we impose the minimum wage of the adoption year. For the 21 never-treated cantons, the average of the valid minimum wages is used. Before any treatment took place (2016-2017), we impose the first ever minimum wage level of Neuchâtel.

SLFS sampling weights on our results. The results remain robust if we drop the weights altogether or adjust them so that each canton has equal weight (cf. Table 7, columns (1) and (2)). The removal of either the retail or the hospitality sectors–sectors with the largest fractions of low-wage workers–hardly changes our main results (cf. Table 7, columns (3) and (4)). 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 (column (5)). Note however that the effect from the panel regression is the effect in the first adoption year, and is identified only by individuals from treated cantons for whom the minimum wage was introduced during the two observed years.[27] Thus, the panel specification strongly corroborates our repeated cross section results.

Our baseline estimates use a linear probability model for training incidence and assume a constant training intensity. Column (6) of Table 7 therefore reports the average marginal effects from nonlinear estimates—logit for training incidence and Tobit, left-censored at zero, for training intensity—leading to similar effect magnitudes as our baseline results. In column (7) we re-weight the control group using entropy weights so that the first three statistical moments of observable pretreatment control variables are equal to those of the treatment group (Hainmueller, 2012). Also this estimation approach delivers very similar estimates. Adding periods of treatment cantons before treatment into control groups does not change our main effects, either (column (8)). At last, in column (9) the control group includes only seven cantons (Vaud, Valais, Zürich, Schaffhausen, Aargau, Fribourg, and Solothurn) that are similar to the minimum wage cantons with respect to labor market tightness, measured by unemployment rate. The training incidence effect is slightly lower and the training intensity effect remains about the same.

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 drive training measures, we conduct a placebo exercise. Focusing on the subset of the 21 never-treated cantons, we impose placebo minimum wage treatments in five randomly picked cantons from the subset of 21 never-treated cantons over the 2018-2022 period. We repeat

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

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 randomization. The probability that the baseline effect on training incidence of 0.034 would appear by an accidental correlation is 0.04. The accidental probability of the treatment effect size of training intensity of 0.504 lies at 0.02. The potential for unobserved drivers of training that could generate effect sizes comparable to our baseline estimate therefore seems to be negligible.

6 Minimum wage effects beyond training?

This section explores whether there are changes in working conditions and the workforce structure that potentially confound our training effects of minimum wages. For this analysis we look at worker outcomes beyond training based on the SLFS data and workforce composition outcomes based on the firm-level ESS data.

Firms exposed to minimum wages may hire more labor market entrants or employer switchers. An increase in hiring activity however may lead to training increases solely driven by initial training. We first study changes 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 (see Table 8, Panel A, columns (1) and (2)). From the firm-level data we calculate shares of workers with short tenures—less than one year and between one and three years—and we do not find any economically meaningful minimum wage effects here either (see Table 8, Panel B, columns (1) and (2)).

Employers may reduce working hours or increase the share of part-time contracts to lower their labor costs. Using both surveys, we construct two part-time measures—a precise fraction of full-time equivalents and a dummy for employees who work less than 90% of full time. For none of these measures we find a minimum wage effect (cf. Table 8, columns (3) and (4)). Similarly, employees with a temporary contract may obtain less training, even if temporary contracts are quite rare in Switzerland. Using information on contract type, from both surveys, we do not find

any change in the incidence of temporary contracts after the introduction of minimum wages, compare Table 8, column (5).

Employers may prompt workers to more frequent overtime work to reduce their wage bill. Less overtime may open space for more training activities. We find support for neither, using dummy of self-reported frequent overtime (Table 8, Panel A, column (5)).

The ESS data offer further details on workforce characteristics on the firm level. Apprenticeships play a crucial role in the vocational education of the country (Muehlemann et al., 2013). A completed apprenticeship—on the upper secondary level—is the highest occupational certificate for the majority of Swiss workers. Apprentices have a paid work contract and they split their time between learning at a public occupational school and working at their training firms (Wolter and Ryan, 2011). Apprenticeships are exempted from minimum wage regulation and apprentice wages are substantially lower than the minimum wage level. Firms therefore may be tempted to substitute low-skilled workers by apprentices after the introduction of minimum wages. Conversely, firms may reduce the number of apprenticeships, or stop training altogether because the retention of apprentices after apprenticeship completion become more expensive and refraining from apprenticeship training can be another channel of labor costs savings. None of the mechanisms seems to be valid as neither the share of training firms, nor the share of apprentices in the workforce were significantly influenced by minimum wages (Table 8, Panel B, columns (6) and (7)).

Similarly, firms may adapt to the increased labor costs by hiring workers of different skill levels. As an adaptation channel, they could substitute lower-skilled with middle-skilled employees to increase productivity. In Table 8 (Panel B, columns (8) and (9)) we shows that firms did not change their shares of low-skilled workers, measured by education or occupation level. Switzerland is a small country with a substantial fraction of foreign workers. Minimum wages apply to any employees who habitually work in the canton. They, however, may allow employers to attract more well-trained workers from neighboring countries. This concern does not seem to be valid either. The fraction of foreign workers in the workforce—workers with temporary work permits and cross-border workers—remained unchanged (Table 8, Panel B, column (10)).

7 Reconciliation with earlier findings

To reconcile our results with some previous evidence, we reproduce estimation approaches used in earlier studies on minimum wage effects on training. Our paper uses the staggered introduction of minimum wages in some cantons to identify the training effect in comparison to a substantial number of never treated cantons. Most earlier papers do not have control regions with no minimum wage treatment, but they identify the effect via the variation in minimum wage treatment intensity during treatment (e.g., Leighton and Mincer, 1981; Neumark and Wascher, 2001; Acemoglu and Pischke, 2003; Fairris and Pedace, 2004; Baker, 2005). We use as our treatment intensity measure the *Kaitz* index as $w_{MW}/\bar{w}$, where the w_{MW} is the currently valid minimum wage level of a canton and $\bar{w}$ is the cantonal median wage in the previous year for a prespecified cluster. We use three clustering variants—canton-year, canton-year-occupation level, and canton-year-economic sector.

We first replace our minimum wage dummy by the *Kaitz* index, varying on canton-year, and repeat our baseline regressions to show whether the estimation approach leads to differences in the results. We use the standard diff-in-diffs specification (Equation 4) because the treatment via *Kaitz* index is non-binary. In this regression the treatment is still zero for never treated cantons or treated cantons before treatment but it is $w_{MW}/\bar{w}$ during treatment. The results in Table A5 are in line with our baseline results but rescaled to the different units of the maximum treatment.[28]

To reproduce the identification strategy that has been used in the previous literature, we implement the *Kaitz* index analysis on the subset of treated cantons *during* minimum wage treatment. We build two samples. The first sample uses data from the period 2021-2024 that includes the four cantons that implemented minimum wages until 2021. Accordingly, Basel-Stadt is not in the first sample but we can keep it longer. Our second sample considers only the years 2023-2024 but includes all five treated cantons. For the *Kaitz* index measured on the canton-year level, we find an effect of minimum wages on training only in one out of four specifications (see Table 9, columns (1) and (2)). The results are not very robust also because the variation of the

[28] If the *Kaitz* index is positive it ranges between 0.47 and 0.59.

Kaitz index for the canton-year clusters is low. For the *Kaitz* indices based on median wages by canton-year-occupation or canton-year-sector clusters, we obtain in most cases significantly negative minimum wage effect (see Table 9, columns (3) to (6)). For these regressions the variation in treatment intensity is larger. The interpretation of this training reaction is however different—clusters with relatively large minimum wage intensity train less than clusters with relatively small intensity. This does not contradict the fact that all clusters train more than before the minimum wage introduction. Our reconciliation results therefore demonstrate that there are positive effects of minimum wages when the control group contains units without treatment. Simultaneously, in line with some previous literature, the minimum wage effects become more ambiguous, and even can turn negative when based on variation in minimum wage intensity within treated regions during treatment.

8 Discussion

Our paper finds substantial and persistent positive training effects of the introduction of minimum wages in five Swiss cantons. The only other papers that find a positive training effect so far are Arulampalam et al. (2004) for the minimum wage introduction in the UK and partially also Acemoglu and Pischke (2003) after changes in the state and federal minimum wages in the US over the period 1987 until 1991. We discuss several potential reasons for the obvious disparity between our findings and the bulk of the existing literature that either shows insignificant or small negative training effects.

The Swiss cantonal minimum wages have the highest absolute levels worldwide and had a higher intensity upon introduction than in most other countries. The introduction of cantonal minimum wages in Switzerland therefore may have induced stronger effects than elsewhere (Harasztosi and Lindner, 2019; Manning, 2021). A relatively high minimum wage intensity also may explain the positive training impact of their introduction in the UK by Arulampalam et al. (2004).

In most countries minimum wages are introduced, or changed, by politicians or industrial

relations institutions. Their decisions may be responsive to regional or temporal labor market conditions and therefore indirectly related to inherent differences in training efforts. The introductions of Swiss cantonal minimum wages come very close to the concept of exogenous shock. Namely, the implementation of cantonal minimum wages in Switzerland entails number of random elements. Minimum wages can be introduced only after a successful popular vote. Inducing a popular vote can take several years and popular votes are far from always successful (Rieger and Gallusser, 2023)[29]. In addition, the lag between successful popular votes and implementation can differ between cantons.[30] There were no initiatives to carry out a popular vote in some cantons with above average shares in favor of the general minimum wage, such as Schaffhausen, Vaud, and Fribourg.[31] Finally, the popular vote is not binding and the cantonal parliaments may change the regulation before implementing it.[32] Lastly, it is quite unlikely that reverse causality determines our results, i.e. a change in the training efforts would drive the introduction of cantonal minimum wages.

In contrast to most previous studies, we assess the full effect on all employees in the treated cantons instead of studying the effect on selected sub-groups. We find a general increase in training incidence and intensity. We demonstrate substantial ripple effects even for employees who earn much more than the minimum wage. [33] The presence of ripple effects may have had a negative bias on the training effects in previous studies when measured on lower-wage subgroups, more likely affected by minimum wages (Neumark and Wascher, 2001; Cengiz et al., 2019, 2022).

[29]The popular votes on minimum wages in the cantons Solothurn and Basel-Landschaft were rejected in 2025, for example.

[30]Three minimum wage votes were swiftly implemented. In Neuchâtel and Ticino, six years elapsed between popular vote and implementation. In Neuchâtel the long lag was a consequence of appeals by employer associations against the vote. After the Federal Supreme Court unexpectedly decided that the appeals were repealed in July 2017, the minimum wage was promptly introduced in September 2017. Also the successful popular votes of June 2023 in the cities Zürich and Winterthur have not yet been implemented.

[31]"Mindestlohn selbst am Jurabogen chancenlos", *Neue Zürcher Zeitung*, 19.05.2014, Seite 9

[32]In Basel-Stadt, the cantonal parliament for example reduced the minimum wage from 23 to 21 Francs and exempted workers subject to collective agreements from the application of cantonal minimum wage rules divergent from the popular vote (Rieger and Gallusser, 2023).

[33]Berger and Lanz (2020) also find ripple effects on earnings in the restaurant industry after the introduction of minimum wages in the canton of Neuchâtel.

Besides measuring the effects on all employees, we also estimate dynamic training effects. Our data range allows us to estimate the immediate treatment effects and simultaneously the mid-term dynamic effects of the minimum wage introduction for up to five years. We find that the minimum wage effects are present already in the introduction year and that the effects remain roughly stable during the first few years in which the policy is in place. When we study group-wise effects, we also find that in the cantons of Neuchâtel and Basel-Stadt, the immediate effect was negative. In Neuchâtel the effect turned positive only after some years. Thus papers that concentrate at the immediate effect of minimum wages may underestimate the full effect on training (Clemens and Strain, 2021).

To shed light on the mechanisms underlying the positive training effects, we consider four potential explanations. First, minimum wages may induce firms to hire better qualified workers and provide them with initial training. Second, employers may increase the productivity of low-wage workers through additional training in order to retain them. Third, low-wage workers may invest in training to secure their higher-paid jobs. Fourth, the observed effects may reflect a compositional shift toward firms that provide more training. We show that the share of job entrants does not increase and that all workers, regardless of their tenure length, experience a higher training incidence and intensity after the introduction of minimum wages. Minimum wages therefore do not increase the hiring intensity.[34] Moreover, there is no evidence that the employee skill composition changes in response to minimum wages. We also find that the share of training financed by employees is small and unaffected by the introduction of minimum wages. Instead, the additional training induced by minimum wages is initiated and financed by employers.

We next assess whether the observed increase in training could be driven by confounding factors. In particular, we examine whether changes in workforce composition or work organization may have influenced training efforts (Dube et al., 2016). We find no evidence of changes in working time, overtime, or in the share of temporary, higher-skilled, or foreign workers. Moreover, minimum wages do not affect apprenticeship training, although apprentices in Switzerland are exempt from minimum wages and earn substantially less than the minimum wage.

[34]These findings are in accordance with Albagli et al. (2024), Dube et al. (2016), and Coviello et al. (2022)

Like Arulampalam et al. (2004), we measure the effects of the introduction of minimum wages instead of the effects of relatively small minimum wage changes. If we reduce our sample to cantons with minimum wages and calculate the training effects of differences in minimum wage intensity and their changes over time during treatment, the effect become insignificant or turns negative. Changes in minimum wage levels may be driven by endogenous factors such as economic prospects in the cantons that at the same time affect training investments (Clemens and Strain, 2021). In addition, the variation of minimum wages after their introduction is relatively small and the measures therefore may mainly pick up differences between sectors and occupations.

We find that the additional training efforts are not concentrated on job entrants and we therefore can infer that the impact of cantonal minimum wages on job mobility is low. We cannot exclude, however, that there is some additional regional mobility between cantons with and without minimum wages after implementation. Our paper observes training efforts on the employee level only. We therefore cannot directly analyze whether the increase in training also translates into an increase in training incidence and intensity on the employer level (as Fairris and Pedace, 2004; Bellmann et al., 2017). Related to this shortcoming, we also cannot identify whether the training increase is a consequence of a shift of employment to firms with higher training levels and job losses in firms with lower training levels (Acemoglu, 2001). Berger and Lanz (2020) however argue that restaurants in Neuchâtel hardly adjusted employment after the introduction of the cantonal minimum wages. The full training effects for all cantons also can be shown for each canton individually, except for Basel-Stadt. There seem to be mainly two reasons for the absence of a training effect in Basel-Stadt. First, the positive effect may evolve only with a time lag, like we observe in Neuchâtel. Second, the exemption of employees covered by collective bargaining, even if they earn less than the new cantonal minimum wage, lead to a very small share of employees affected by the minimum wage (Rieger and Gallusser, 2023). Employers therefore may not have seen the necessity to immediately react on the new minimum wage by increasing their training efforts.

Our findings suggest that the training increase is primarily driven by employers' investments

aimed at raising employee productivity. Previous studies show that productivity gains are larger for formal than for informal training (Zwick, 2005), and for investments in general rather than specific training (Becker, 1962; Simpson, 1984). Consistent with this evidence, we find that the additional training effort is concentrated in formal training and in training topics not directly aligned with the firm's main economic activity. Moreover, employers increasingly allow training to take place during regular working hours rather than employees' leisure time. This concession may signal employers' interest in retaining their workers.

Overall, our results are consistent with the hypothesis that labor market frictions generate rents that incentivize employers to invest in the training of retained low-wage workers in response to substantial increases in minimum wages (Acemoglu and Pischke, 2003). This interpretation is further supported by evidence that Swiss employers do not systematically share the returns to training-induced productivity gains with their employees, which is in line with Gerfin (2004), who shows that Swiss workers typically realize wage increases after training only when they change employers.

9 Conclusion

Our paper shows that employers invest in the productivity of their workers after the introduction of minimum wages. We argue that training investments pay off because employers can keep the underlying rents from retaining their low-wage employees (Levin-Waldman, 1996; Acemoglu and Pischke, 2003). Training therefore may be the central alternative to dismissing low-wage employees after minimum wage increases. Our results are in contrast to most existing theoretical and empirical papers that argued, on the basis of the standard human capital theory, that minimum wages reduce continuing training or at best have no impact (Neumark and Wascher, 2003).

We look at the consequences of the cantonal minimum wages on the provision of continuing training in Switzerland. We use the staggered minimum wage introduction in a handful of cantons as exogenous variations. We find that cantonal minimum wages lead to a positive training effect already in the year of introduction in most cantons and a long-lasting positive effect over

several years. The training effect is not concentrated on low-wage workers. We instead find pervasive ripple effects on the training level of employees who earn much more than minimum wages in the treated cantons. The training effect also is not concentrated on newly hired workers but also workers with tenure of more than one year receive additional training. The content of the additional training usually is not directly related to the main activity of the employer. The additional training is mainly provided in formal courses during working time and is fully paid for by the employers.

We argue that differences in minimum wage training effects 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. When we reproduce previous empirical approaches and reduce the analysis to minimum wage intensity in the treated regions during treatment, the training effects are more ambiguous and even turn significantly negative.

This paper contributes to the new chapter of minimum wage research that uses high-quality data on the introduction of recent minimum wages with substantially greater intensity than those studied in earlier work (Manning, 2021). Our analysis suggests that even in the liberal Swiss labor market, employers can extract rents from the employment of their low-wage workers and retain a large share of the returns to training. Our results therefore seem to be informative other settings in which employers possess rent-extraction power. We conclude that, following the introduction of binding minimum wages, employers prefer to increase the productivity of their existing low-wage workforce rather than incurring hiring costs to recruit better qualified workers.

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Figures and Tables

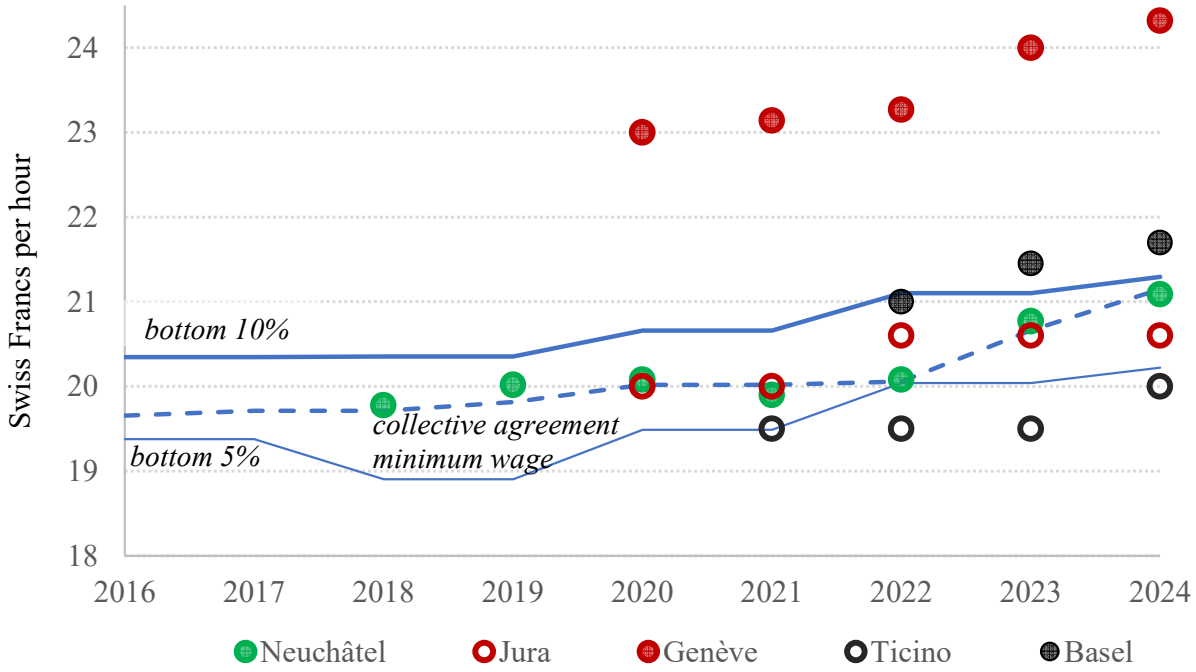

Figure 1: Cantonal minimum wages, hospitality sector minimum wages as per collective agreements, and 5% and 10% wage percentiles

Notes: Mandated hourly minimum wages in the five treated cantons (circles); regular hourly minimum wages from collective agreements in hospitality sector (dashed line); fifth and tenth percentiles in hospitality industry (thin and thick solid lines).
Sources: Own compilation based on: Minimum wage information of cantons, collective agreement documents 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.

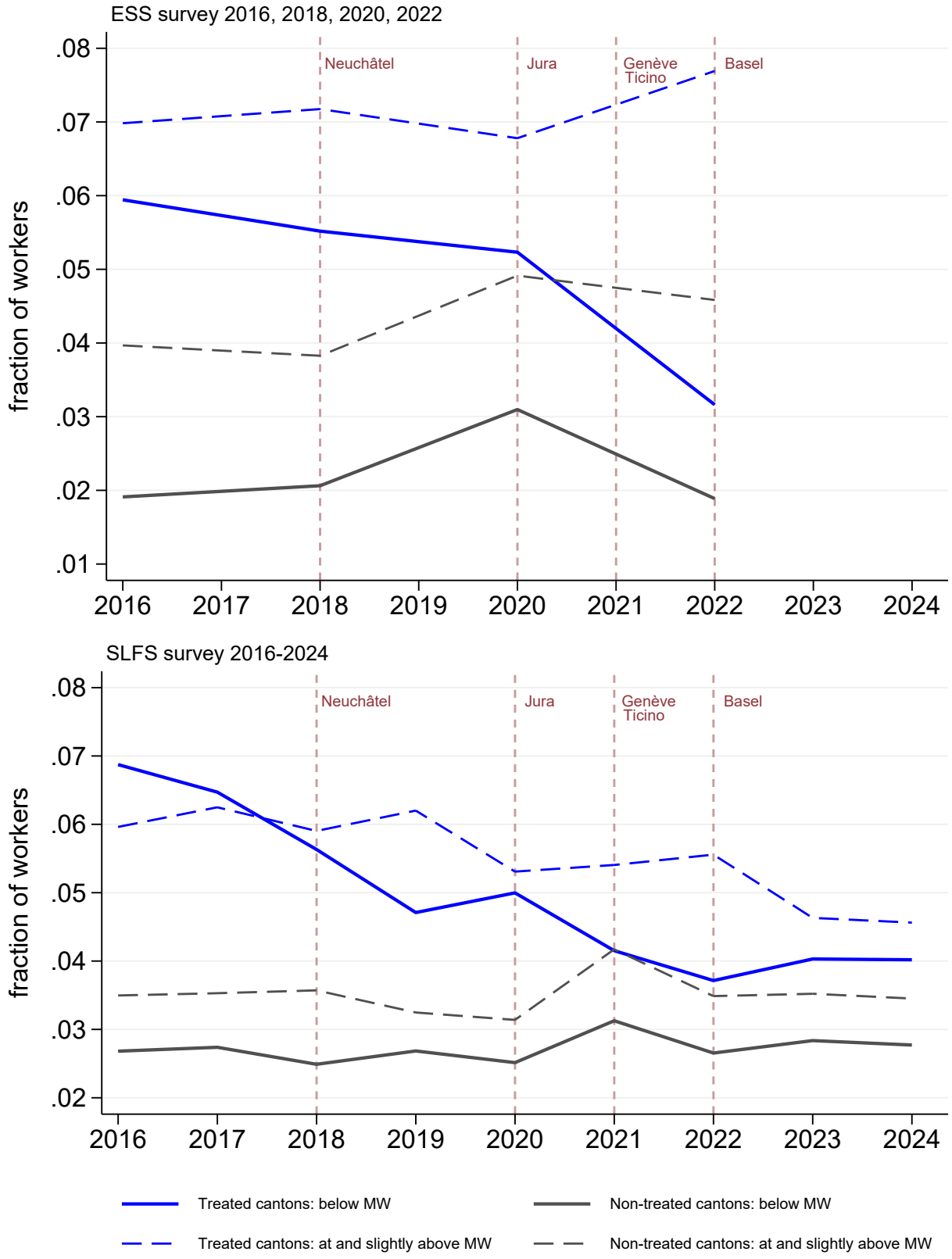

Figure 2: Minimum wages effectively reduced fractions of low-wage workers

Notes: Own depiction based on ESS 2016, 2018, 2020, 2022 (upper plot) and SLFS 2016-2024 (lower plot). 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 the first year of binding minimum wage regulation for canton(s) indicated right-top of the 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 numerical entries for odd years.

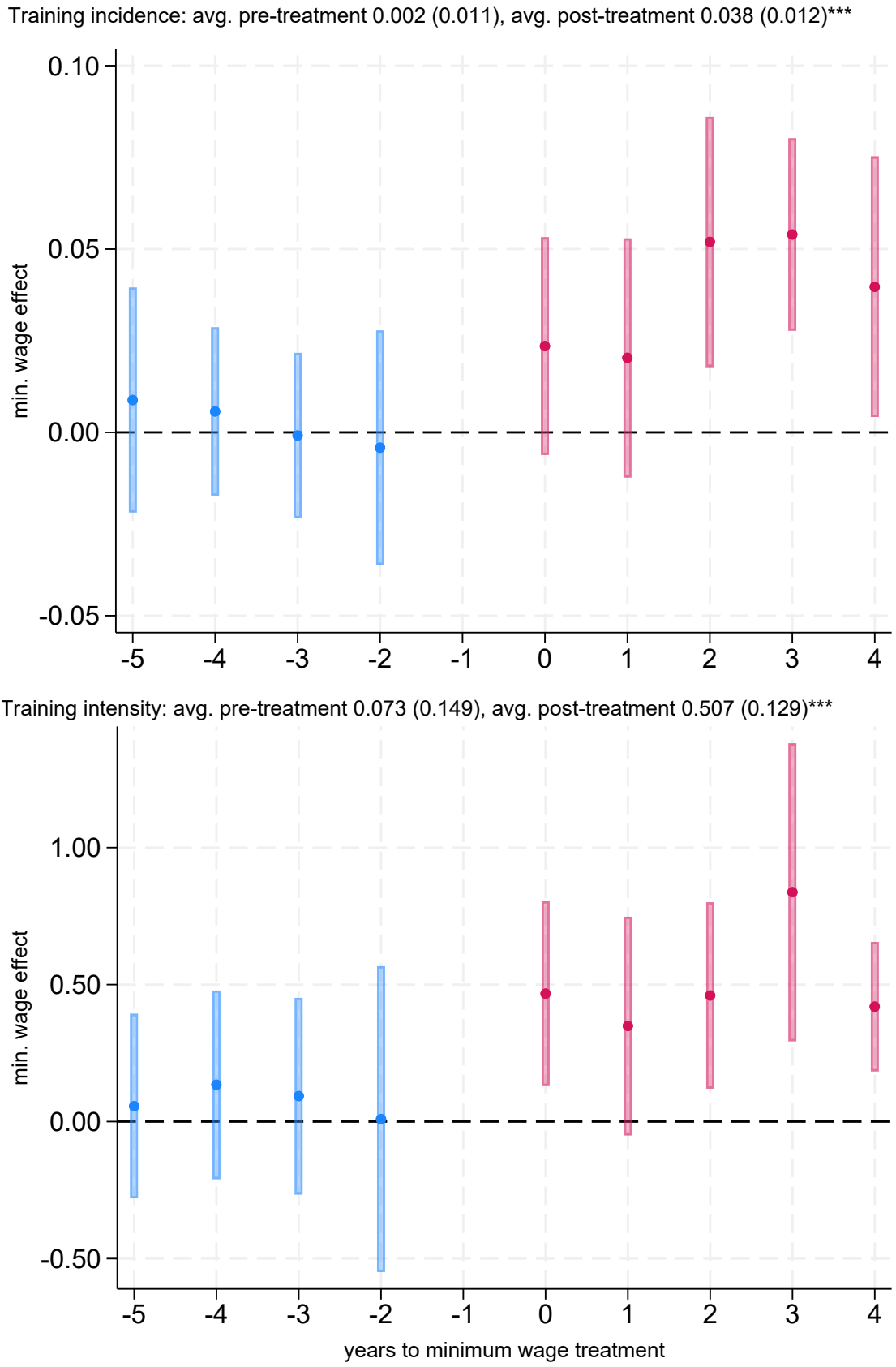

Figure 3: Dynamic treatment effects of minimum wage on training incidence and intensity

Notes: Dynamic minimum wage effects on training incidence and intensity estimated under the conditional parallel trends assumption, based on SLFS 2021-2024 using the Callaway and Sant'Anna (2021) approach with 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)).

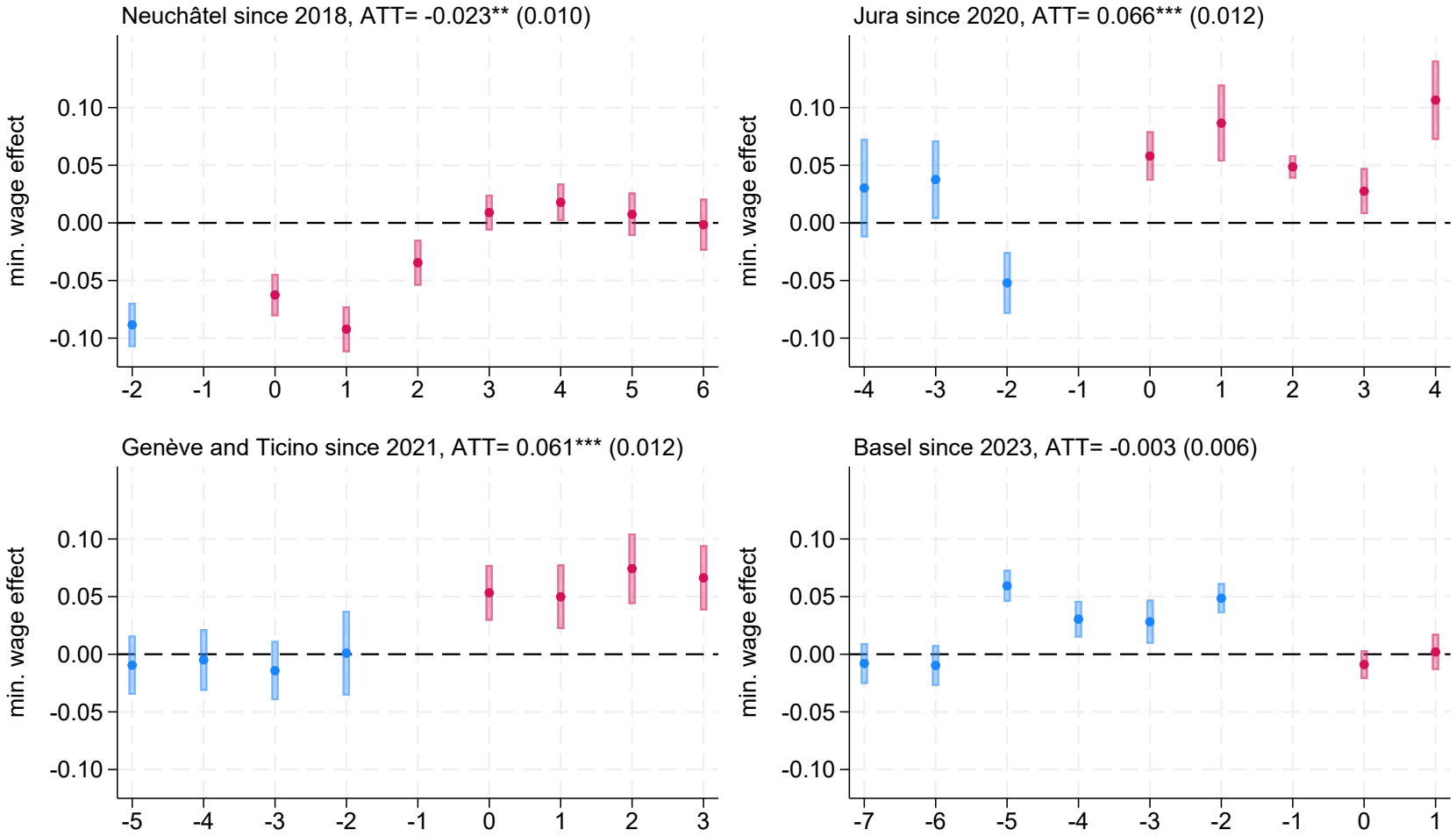

Figure 4: Group-time dynamic treatment effects of minimum wage on training incidence

Notes: Dynamic minimum wage effects on training incidence using the Callaway and Sant'Anna (2021) approach with 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: 2018, 2020, 2021, 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 level is indicated as *, **, ***, respectively.

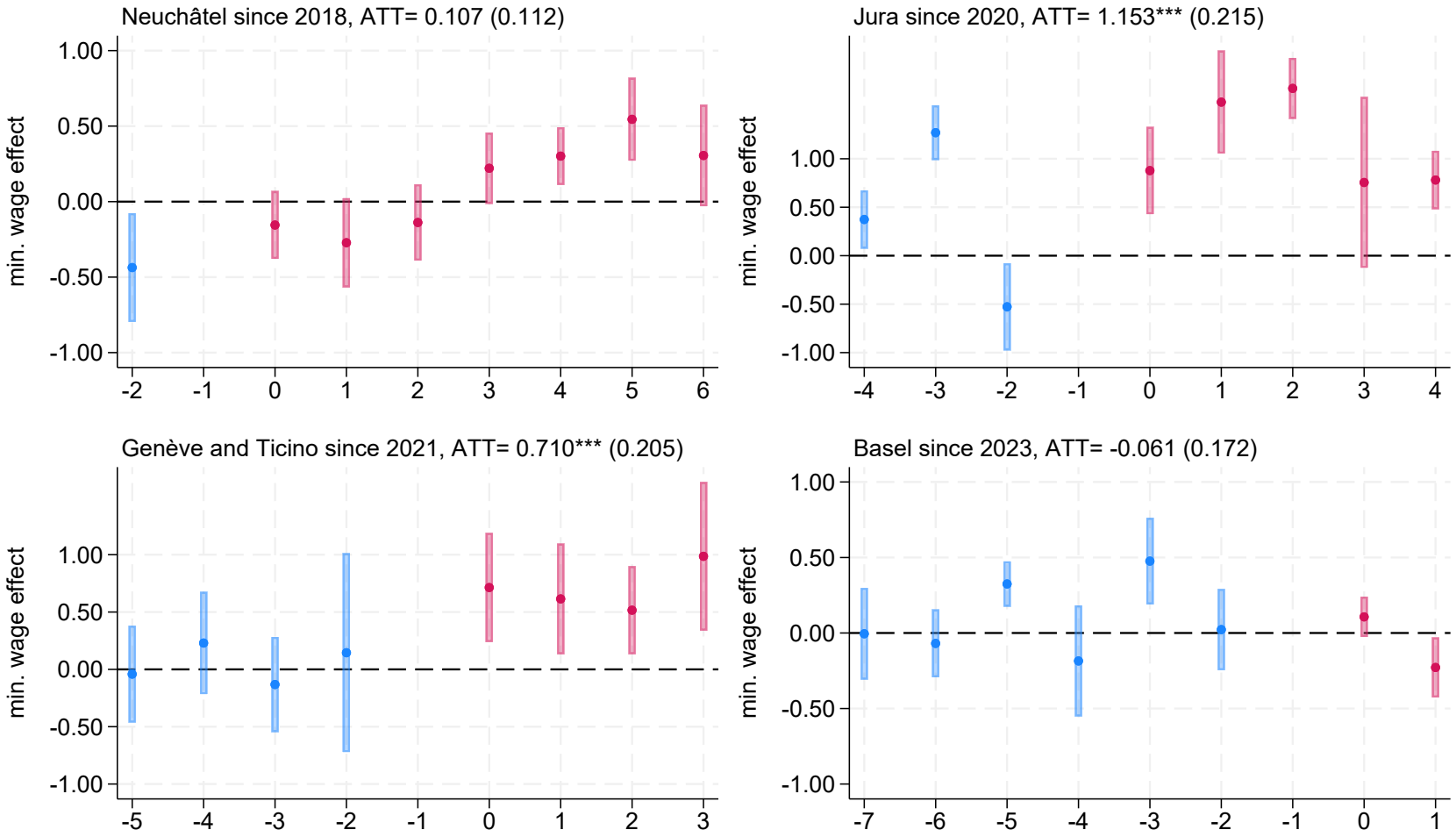

Figure 5: Group-time dynamic treatment effects of minimum wage on training intensity

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: 2018, 2020, 2021, 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 *, **, ***.

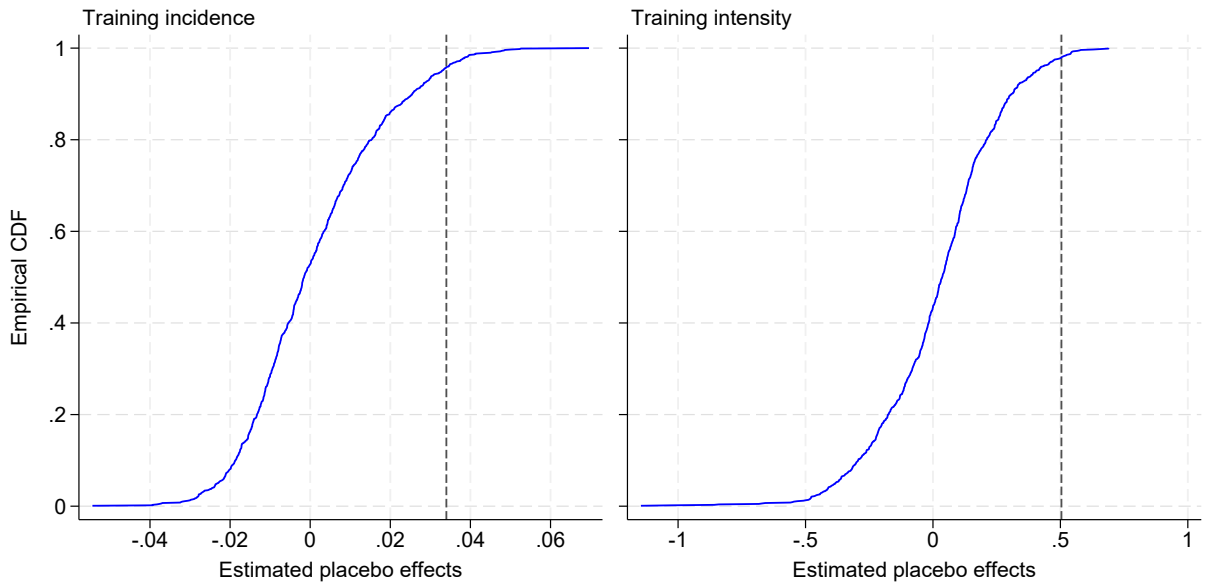

Figure 6: Distribution of minimum wage effects on training measures from 1'000 placebo randomizations

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 specification (6) for training intensity of Table 4 (Panel A). Specifications include canton and year fixed effects, and the full set of control variables. The vertical lines corresponds to the true effect estimate of 0.034 (training incidence) and 0.504 (training intensity). Placebo estimates use the doubly robust estimator with inverse probability weighting (Callaway and Sant'Anna, 2021).

Table 1: Popular vote and adoption dates of minimum wage policies across Swiss cantons

Canton	Minimum wage vote	"Yes" votes (%)	Minimum wage adoption
Neuchâtel	November 27, 2011	54.6	September 2017
Jura	March 3, 2013	54.2	January 2020
Ticino	June 14, 2015	54.7	January 2021
Genève	September 27, 2020	58.2	November 2020
Basel-Stadt	June 13, 2021	53.8	July 2022

Sources: Vote days and "yes" votes: https://c2d.ch/country/CH, adoption day Ticino: www4.ti.ch/dfe/de/usml/salariominimo/salario-minimo-cantonale/, adoption days all other cantons: Rieger and Gallusser (2023). Based on the minimum wage adoption date we set our minimum wage treatment dummy: It equals one from the first full year of the minimum wage policy—2018 for Neuchâtel, 2020 for Jura, 2021 for Genève and Ticino, and 2023 for Basel-Stadt.

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

	SLFS 2016-2024		SAES 2016, 2021	
	mean	(std. dev.)	mean	(std. dev.)
Dependent variable:				
Training incidence:				
All	0.395	(0.489)	0.473	(0.499)
Job-related	0.347	(0.476)	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)
Partly outside working hours			0.379	(0.485)
Training aligned with economic activity			0.245	(0.430)
Training not aligned with economic activity			0.408	(0.491)
Informal at work			0.303	(0.460)
Training intensity (hours/month):				
All	3.525	(8.517)	3.461	(12.292)
Job-related	3.072	(8.024)	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)
Partly outside working hours			2.438	(11.133)
Training aligned with economic activity			1.117	(6.807)
Training not aligned with economic activity			2.344	(9.438)
Informal at work			0.782	(5.941)
Observations	267,310		15,785	

Notes: Based on *Swiss Labor Force Survey* (SLFS) 2016-2024 and *Swiss Adult Education Survey* (SAES) 2016, 2021 estimation samples. Observations are weighted by their sampling weights. SLFS does not contain further details on training characteristics. All training incidence variables are dummies, where one stands for being in training in any of the last months prior to interview in any interview wave (SLFS), and being in training in the past 12 months prior to the interview (SAES). All training intensities are continuous measures standing for average training hours per month, in each survey based on reference period, with a span of zero to maximum 488 (SLFS) and 667 (SAES) hours. Even if these maximal numbers are unrealistically high we do not top-code them. The number of observations above 180 hours of training per month–what would mean a full training month–is less than 0.1% in both surveys and do not drive our results.

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

	SLFS 2016-2024		SAES 2016, 2021	
	mean	(std. dev.)	mean	(std. dev.)
Controls:				
Female	0.475	(0.499)	0.490	(0.500)
Age	41.746	(11.927)	41.323	(12.174)
Migrant	0.274	(0.446)	0.311	(0.463)
Education:				
Primary	0.094	(0.292)	0.098	(0.297)
Secondary	0.452	(0.498)	0.483	(0.500)
Job tenure:				
$<$ 1 year	0.159	(0.365)	0.085	(0.279)
1-3 years	0.263	(0.441)	0.312	(0.463)
4-7 years	0.205	(0.404)	0.217	(0.412)
Occupation level:				
Qualified man.	0.086	(0.280)		
Qualified non-man.	0.218	(0.413)		
Intermediary	0.339	(0.473)		
High	0.293	(0.455)	0.268	(0.443)
Firm size:				
1-19 employees	0.289	(0.453)	0.305	(0.461)
20-49 employees	0.177	(0.381)	0.156	(0.363)
50-99 employees	0.124	(0.330)	0.245	(0.430)
Observations	267,310		15,785	

Notes: Based on *Swiss Labor Force Survey* (SLFS) 2016-2024 and *Swiss Adult Education Survey* (SAES) 2016, 2021 estimation samples. Observations are weighted by their sampling weights. 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. All controls are 0/1 dummies with an exception age variable that spans 15-65 age range. In our regressions, we additionally control for 12 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 (min. wage)	0.031	0.032**	0.034***	0.548***	0.541***	0.504***
	(0.019)	(0.015)	(0.013)	(0.210)	(0.175)	(0.143)
Panel B: Job-related training						
ATT (min. wage)	0.028	0.029*	0.030**	0.439**	0.418***	0.385***
	(0.019)	(0.016)	(0.014)	(0.189)	(0.154)	(0.131)
Panel C: Other than job-related training						
ATT (min. wage)	0.002	0.004	0.004	0.108**	0.123**	0.119**
	(0.003)	(0.003)	(0.003)	(0.054)	(0.052)	(0.047)
year & canton FEs	yes	yes	yes	yes	yes	yes
Covariates:						
sociodemographic		yes	yes		yes	yes
job-specific			yes			yes

Notes: Using SLFS 2016-2024 and Callaway and Sant'Anna (2021) doubly robust estimator with inverse probability weights, reported are the ATT of minimum wage treatment ($N = 267,310$). Dependent variable: training incidence dummy (columns 1 to 3) or training hours (columns 4 to 6) in the month prior to the interview. Observations are weighted by their sampling weights. In Panel A, the dependent variable includes all training types, in Panel B it includes only job-related training, and in Panel C only other than 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 industry 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 5: Minimum wage effects on training with focus on training characteristics

	Equiv. estimates:		Financial participation:		Training timing:		Training topic:		Training
	SLFS	SAES	only firm	partly employee	only during working hours	partly outside working hours	aligned with econ. activity	not aligned with econ. activity	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	-0.016	0.025***	-0.029
	(0.009)	(0.016)	(0.010)	(0.007)	(0.009)	(0.014)	(0.011)	(0.008)	(0.018)
Adjusted R^2	0.123	0.070	0.110	0.037	0.089	0.099	0.156	0.134	0.080
Panel B: Training intensity									
min. wage	0.807***	0.857***	0.381**	-0.198	0.439**	0.440*	0.006	0.814***	0.255***
	(0.266)	(0.256)	(0.148)	(0.155)	(0.187)	(0.256)	(0.088)	(0.212)	(0.077)
Adjusted R^2	0.058	0.021	0.018	0.010	0.012	0.017	0.028	0.018	0.015

Notes: TWFE estimations (Equation 2) based on SAES 2016 & 2021, except column (1), where we use SLFS 2016 & 2021 in order to obtain estimates based on equivalent samples. The number of observations is 60,062 for the SLFS column and 15,785 for all other—SAES—columns. Dependent variable: training incidence dummy (Panel A) or training hours (Panel B). For SLFS column training in the month prior to the interview is considered. For SAES columns any training in the last 12 months is considered. SAES training intensity is rescaled to average monthly intensity. 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×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 for specific groups of workers

	training incidence		training intensity	
	(1)	(2)	(3)	(4)
Panel A: Low-wage groups				
min. wage	0.039***	0.038***	0.409***	0.374***
	(0.011)	(0.011)	(0.144)	(0.143)
below min. wage + 15%	-0.062***	-0.071***	-0.218**	-0.286**
	(0.004)	(0.005)	(0.110)	(0.110)
below min. wage + 15% × min. wage	0.021	0.022	0.527**	0.558***
	(0.013)	(0.013)	(0.209)	(0.210)
above 15-25% of min. wage		-0.067***		-0.576***
		(0.005)		(0.084)
above 15-25% of min. wage × min. wage		0.016		0.658*
		(0.017)		(0.354)
Adjusted R^2	0.116	0.117	0.047	0.047
Panel B: Short-tenure groups				
min. wage	0.045***	0.043***	0.515***	0.462***
	(0.011)	(0.012)	(0.148)	(0.143)
tenure $<$ 1 year	0.011***	0.011***	0.773***	0.770***
	(0.004)	(0.004)	(0.091)	(0.091)
tenure $<$ 1 year × min. wage	-0.010	-0.008	-0.293	-0.239
	(0.010)	(0.010)	(0.207)	(0.208)
tenure 1-3 years		0.006*		0.464***
		(0.003)		(0.054)
tenure 1-3 years × min. wage		0.007		0.176
		(0.008)		(0.158)
Adjusted R^2	0.115	0.115	0.047	0.047

Notes: Estimates stemming from group interaction specification (Equation 3) using SLFS 2016-2024 ($N = 267,310$). Dependent variable: training incidence dummy (columns (1) and (2)) or training hours (columns (3) and (4)), for all training. Panel A reports minimum wage interactions with low-wage bin groups. In Panel B minimum wage is interacted with short-tenure groups. 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. 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 7: Minimum wage effects on training incidence and intensity: Sensitivity analyses

	Weights		Excluding		Panel	Non-linear	Entropy	Including	Structurally
	no	adjusted	retail	hospitality	individual FE	model	balanced	not yet treated	similar
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Training incidence						*logit*			
ATT/min.wage	0.042***	0.032**	0.037**	0.034***	0.033***	0.042***	0.032*	0.034***	0.026*
	(0.009)	(0.014)	(0.014)	(0.013)	(0.011)	(0.011)	(0.018)	(0.013)	(0.014)
Panel B: Training intensity						*tobit*			
ATT/min.wage	0.402***	0.617***	0.508***	0.510***	0.252	0.567***	0.402***	0.517***	0.450***
	(0.098)	(0.156)	(0.157)	(0.143)	(0.173)	(0.145)	(0.104)	(0.142)	(0.166)
Observations	267,310	267,310	236,537	258,607	259,995	267,310	267,310	267,310	171,053

Notes: Estimates reported in columns (1) to (4), and (7) to (9) are based on Callaway and Sant'Anna (2021) doubly robust estimator with inverse probability weights using SLFS 2016-2024 data. All estimations include canton and year fixed effects and the full set of controls (as in columns (3) or (6) of Table 4). Column (5) reports individual panel fixed effects (FE) results. Column (6) reports, in the upper panel, an average marginal effect of minimum wage on the training incidence based on a logit regression of training incidence, and in the lower panel, it reports a Tobit estimate of the average marginal effect of minimum wages on number of training hours. In all columns except (1), (2), and (5) observations are weighted by their sampling weights. Column (1) and (5) do not use weights and column (2) adjust the sampling weights so that each canton has equal weight. In column (3) we exclude retail industry and in column (4) we exclude hospitality industry. The entropy balanced specification (column (7)), re-weights the control group using entropy weights so that the first three statistical moments of observable pretreatment control variables equal to those of the treatment group (Hainmueller, 2012). In column (8) the control group includes not yet treated periods of treated cantons. In column (9) the control group includes only seven cantons (Vaud, Valais, Zürich, Schaffhausen, Aargau, Fribourg and Solothurn) that are similar to the minimum wage cantons in labor market tightness, measured by unemployment rate. In all but column (5) the standard errors, in parentheses, are clustered on the canton×year level. The panel regressions, column (5), reports bootstrapped standard errors using 100 replications. Significance on 0.1, 0.05, and 0.01 is indicated as *, **, ***.

Table 8: Minimum wage effects on outcomes other than training

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Tenure		Part time		Temp.	Frequent	Employ-			
	in years	in days	in %	<90%	contract	overtime	ment			
Panel A: SLFS										
ATT (min. wage)	-0.213	-77.709	-0.720	0.007	0.004	-0.014	-0.005			
	(0.226)	(82.345)	(0.485)	(0.007)	(0.005)	(0.025)	(0.006)			
Means of d.v.	8.4	3057	83.22%	0.35	0.08	0.36	0.95			
Panel B: ESS										
	Tenure		Part time		Temp.	Apprentices		Skills of workforce		Foreign
	<1 year	1-3 years	in %	<90%	contract	any	number	primary	low-skilled	workers
ATT (min. wage)	-0.003	-0.013*	-0.654	0.006	-0.011	0.007	-0.004	-0.001	0.002	-0.005
	(0.007)	(0.008)	(0.857)	(0.009)	(0.009)	(0.015)	(0.169)	(0.010)	(0.013)	(0.008)
Means of d.v.	0.15	0.27	76.20%	0.49	0.05	0.18	1.24	0.16	0.06	0.12

Notes: Table reports ATT of minimum wage treatment based on Callaway and Sant'Anna (2021) doubly robust estimator with inverse probability weights using SLFS 2016-2024 (Panel A) and firm-level aggregated ESS 2016, 2018, 2020, 2022 (Panel B). Each estimate stems from a separate regression of a workforce structure outcome (see table headings for each Panel) on minimum wage treatment dummy.

For SLFS regressions (Panel A), all estimations include canton and year fixed effects and use full set of controls (as in columns (3) and (6) of Table 4). In the first two columns tenure is dropped from control variables. Number of observations is N = 267,310, but is smaller (N =267,287) in the first two columns due to missing values in tenure reports. Number of observations is larger (N =272,170) in column (5) because we include workers having all types of contracts, including seasonal, employment program, jobber, or internship contracts. In column (7) the N = 332,147, because it includes also unemployed. Standard errors, clustered on the canton×year level, are reported in parentheses.

For the ESS (Panel B) all regressions 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. For the estimates in the columns (6) and (7) we matched the ESS data with registry data of all firms involved in apprenticeship training in Switzerland (SBG). Dependent variable in column (6) is a dummy that equals one if firm has at least one apprentice and in column (7) it is the number current of apprentices in firm. In column (3) the dependent variable is the firm-level average % of full time equivalent across all workers. In all remaining columns dependent variable is a share of workers of particular type (cf. column heading of Panel B). 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 9: Minimum wage effects using variation in the treatment intensity during treatment

	canton-year		canton-year-occupation		canton-year-sector	
	2021-2024	2023-2024	2021-2024	2023-2024	2021-2024	2023-2024
	4 cantons	5 cantons	4 cantons	5 cantons	4 cantons	5 cantons
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Training incidence						
Kaitz index $(w_{\text{MW}}/\bar{w})$	0.032	0.417***	-0.652***	-0.692***	-0.326***	-0.316***
	(0.116)	(0.118)	(0.067)	(0.074)	(0.065)	(0.082)
Adjusted R^2	0.116	0.114	0.114	0.111	0.102	0.094
Panel B: Training intensity						
Kaitz index $(w_{\text{MW}}/\bar{w})$	-1.994	0.102	-3.991***	-4.850***	-2.292**	-1.663
	(3.629)	(2.244)	(0.532)	(0.939)	(0.797)	(1.038)
Adjusted R^2	0.025	0.022	0.024	0.021	0.022	0.019
Fixed effects			canton	canton	canton	canton
	year	year	year	year	year	year
Controls:	occupation	occupation			occupation	occupation
	sector	sector	sector	sector		

Notes: Estimations based on SLFS 2021-2024/2023-2024 periods and a subset of treated cantons during treatment (see table heading) using two-way fixed effects specification with treatment intensity (Equation 4). Observations are weighted by their sampling weights. $N = 15,862$ (odd columns) and $N = 9,717$ (even columns). Dependent variable is *Kaitz* index $(w_{\text{MW}}/\bar{w})$, where the w_{MW} is the valid minimum wage of each canton and the $\bar{w}$ is median wage in year $t-1$ in canton-year clusters (columns (1) and (2)), canton-year-occupation clusters (columns (3) and (4)), and canton-year-industry sector clusters (columns (5) and (6)). Due to the variation of the 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 *, **, ***.

Online Appendix

Table A1: Demonstration of raising wages of low-wage workers after adopting minimum wages

	Dependent variable: ln(hourly wage)		
	(1)	(2)	(3)
Panel A: Swiss Labour Force Survey 2016-2024			
Minimum wage	0.027*	0.035*	0.041**
	(0.016)	(0.019)	(0.019)
R squared	0.000	0.067	0.079
Observations	29,095	29,095	29,095
Panel B: Earnings Structure Survey 2016, 2018, 2020, 2022			
Minimum wage	0.026**	0.016**	0.014*
	(0.012)	(0.008)	(0.008)
R squared	0.001	0.017	0.054
Observations	675,022	675,022	675,022
year & canton FEs		yes	yes
Covariates			yes

Notes: Simple test á la Manning (2021) to show whether wages of low-wage workers actually increase after adoption of minimum wages using *Swiss Labor Force Survey* (Panel A) and *Earnings Structure Survey* (Panel B). Sample in both surveys consist only of workers with wages below 25 Swiss Francs. We regress natural logarithm of hourly wage on minimum wage treatment dummy and, depending on specification, on fixed effects and further control variables (see bottom rows of the table). Observations are weighted by their sampling weights. Reported are only wage semi-elasticities with respect to minimum wage. Control variables include gender, age, age squared, education dummies, tenure dummies, firm size dummies, occupation level dummies, and sector dummies. Standard errors, clustered on the canton×year level, are reported in parentheses. Significance level on 0.1, 0.05, and 0.01 is indicated as *, **, ***, respectively.

Table A2: Descriptive statistics of outcome and control variables in ESS sample

	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 *Earnings Structure Survey* (ESS) 2016, 2018, 2020, 2022 estimation sample. Original ESS data are on the employee level. We use and report establishment-level aggregated data (N = 319,733). Firms size 1-19 is a reference category. In our ESS regressions, we additionally control for 13 industry dummies (see Table 8, Panel B).

Table A3: Minimum wage effects on training incidence and intensity: TWFE

	training incidence			training intensity		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All training						
min. wage	0.039***	0.039***	0.043***	0.426***	0.431***	0.472***
	(0.011)	(0.011)	(0.011)	(0.140)	(0.143)	(0.143)
Adjusted R^2	0.016	0.083	0.116	0.011	0.036	0.047
Panel B: Job training						
min. wage	0.034***	0.034***	0.038***	0.353***	0.359***	0.397***
	(0.010)	(0.010)	(0.010)	(0.114)	(0.116)	(0.116)
Adjusted R^2	0.012	0.078	0.114	0.009	0.034	0.046
Panel C: Other than job training						
min. wage	0.005	0.005	0.005	0.073	0.072	0.075
	(0.003)	(0.003)	(0.003)	(0.050)	(0.050)	(0.050)
Adjusted R^2	0.007	0.013	0.014	0.002	0.005	0.005
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 (Equation 2). Estimations based on SLFS 2016-2024. 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 $N = 267,310$. Dependent variable: training incidence dummy (columns 1 to 3) or training intensity in hours (columns 4 to 6). In Panel A, the dependent variable includes all training types, in Panel B it includes only job-related training, and in Panel C it considers only other than 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 level on 0.1, 0.05, and 0.01 is indicated as *, **, ***, respectively.

Table A4: Alignment between main economic activity of the employer and employee field of training courses

General Classification of Economic Activities (NOGA 2008, 1-2 digits)	*Courses classifications* (ISCED fields, 1-2 digit)
A Agriculture, forestry and fishing	**8** Agriculture, Forestry, Fisheries and Veterinary
M71 Architectural and engineering activities; technical testing and analysis **C** Manufacturing **F** Construction	**7** Engineering, Manufacturing and Construction
H Transporting and storage	**104** Transport services
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 **M69** Legal and accounting activities **M70** Activities of head offices; management consultancy activities	**4** Business, Administration and Law
M72 Scientific research and development **M73** Advertising and market research **M74** Other professional, scientific and techn. activities **M75** Veterinary activities	**5** Natural Sciences, Mathematics and Statistics
P Education	**1** Education
Q Human health and social work activities	**9** Health and Welfare
R Arts, entertainment and recreation	**2** Arts and Humanities
S Other service activities	**10** Services (except **104**)

Notes: Based on codebook to *Swiss Adult Education Survey* (SAES) 2016, 2021. We used this alignment to distinguish between employee training aligned and not aligned with employer economic activity (see Table 5, columns (7) and (8)). All matched pairs between main economic activities of the employer and training fields of the employee in the table are counted into aligned training measures. The unmatched NOGA-ISCED pairs, or when the ISCED field of training courses is missing are accounted into non-aligned training measures, compare (Table 2, SAES columns). The *Nomenclature générale des activités économiques* (NOGA) is closely related to the classification of economic activities *Nomenclature statistique des Activités économiques dans la Communauté Européenne Rev. 2* (NACE) used in the European Union. The International Standard Classification of Education (ISCED) is widely used to classify education and training contents. The NOGA M-category did not have a clear equivalent at the 1 digit ISCED fields, thus we used the second digit division and aligned across three different ISCED fields categories. Two NOGA categories–G Wholesale and retail trade and I Accommodation and food service–have no clear equivalence in ISCED fields, thus training of the underlying workers is always not aligned with employer economic activity.

Table A5: 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						
Kaitz index $(0, w_{\text{MW}}/\bar{w})$	0.080***	0.082***	0.086***	0.927***	0.941***	0.977***
	(0.019)	(0.020)	(0.020)	(0.256)	(0.261)	(0.261)
Adjusted R^2	0.016	0.082	0.113	0.011	0.035	0.045
Panel B: Job training						
Kaitz index $(0, w_{\text{MW}}/\bar{w})$	0.069***	0.071***	0.074***	0.758***	0.773***	0.803***
	(0.018)	(0.018)	(0.019)	(0.211)	(0.214)	(0.214)
Adjusted R^2	0.012	0.077	0.110	0.009	0.034	0.044
Panel C: Other than job training						
Kaitz index $(0, w_{\text{MW}}/\bar{w})$	0.012**	0.012**	0.012**	0.169*	0.168*	0.174*
	(0.006)	(0.006)	(0.006)	(0.093)	(0.094)	(0.094)
Adjusted R^2	0.007	0.013	0.014	0.002	0.004	0.005
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 treatment intensity specification (Equation 4). Estimations are based on SLFS 2016-2024. Dependent variable: training incidence dummy (columns 1 to 3) or training intensity in hours (columns 4 to 6) in the month prior to the interview. *Kaitz* index: minimum wage treatment intensity index. It is equal to zero for no minimum wage treatment and it is the proportion of the cantonal minimum wage (w_{MW}) in median cantonal wage $(\bar{w})$ in $t-1$, during treatment. If the *Kaitz* index is positive it ranges between 0.47 and 0.59. 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 $N = 267,310$. 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 level on 0.1, 0.05, and 0.01 is indicated as *, **, ***, respectively.

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