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Dual Reform of Vocational Education and Early Labour Market Outcomes

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Dual Reform of Vocational Education and Early Labour Market Outcomes

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Dual Reform of Vocational Education and Early Labour Market Outcomes*

Abstract
Does it matter whether practical training in a vocational programme takes place in a school workshop, in a firm, or under an apprenticeship contract with the firm? We study the 2015/16 Slovak reform that added a dual track to the school-based and workplace-based tracks, using matched education and social security registers and a control function with a distance-based instrument. In the first year after graduation, dual graduates are employed for about 28 days longer and earn about 11% higher wages. The employment advantage shrinks after the first year; the wage premium does not. Workplace-based graduates, who train in firms without a contract, earn only about 2% more than school-based graduates, so the premium reflects the dual arrangement as a whole rather than workplace exposure alone. It survives the exclusion of graduates retained by a training firm, is flat across parental income, and leaves continuation into further education unchanged.

JEL classification
I26, I28, J21, J24

Keywords
apprenticeship, dual vocational education, workplace-based training, school-to-work transition, control function

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1 Introduction

How should young people learn an occupation: in a school workshop, inside a firm, or as apprentices bound to a firm by a contract? Governments that want to ease the transition from school to work have increasingly looked to the apprenticeship systems of Germany, Austria, and Switzerland, in which practical training takes place inside firms (Eichhorst et al., 2015). Apprenticeship is widely expected to serve students best, because it ties training to the needs of firms, and apprentices typically do well in the transition from school to work (Eichhorst et al., 2015; Fersterer et al., 2008; Cavaglia et al., 2020; Lüthi, 2025). Whether the advantage is causal, lasting, and due to the apprenticeship itself remains unsettled. Almost all of the evidence comes from countries where apprenticeship is entrenched across occupations, and it compares apprentices with school-trained peers, so that training inside a firm and the contractual tie to that firm move together (Parey, 2016; von Wachter and Bender, 2006).[2] For a country that wants to bring training closer to firms, the distinction matters: if exposure to a workplace is what pays, agreements between schools and firms suffice; if the contractual arrangement is what pays, apprenticeship itself is needed.

Slovakia's reform of 2015/16 offers a setting in which the two components can be contrasted. Before the reform, practical training took one of two forms. In the *school-based* track, students trained in school workshops. In the *workplace-based* track, the school placed its students in a firm for part of their practical training, under an agreement between the school and the firm; the student had no contract with the firm, and the law set no minimum share of training at the workplace. The reform added a *dual* track, which is an apprenticeship: the student signs an individual contract with a firm certified to train in the occupation, is paid by it, and spends at least half of practical training at the employer. Both firm-based tracks thus send students into firms, but on different terms. The three tracks have since coexisted within the same occupations, so comparing workplace-based with school-based students is informative about the return to time spent in a firm, and comparing dualists with workplace-based students about what the contract

[2]The exception closest to our setting is Hungary, where Horn (2016) compares apprentices in workplace-based training with school-based students; this comparison, too, does not separate workplace exposure from the apprenticeship contract (Section 3).

and its guarantees add. Many countries of Central and Eastern Europe introduced dual tracks after 2014, with little institutional tradition and, so far, little evidence on their effects.[3] We study the effects of the Slovak dual track with administrative data on every graduate of the programmes in which dual training was offered, following each graduation cohort for up to four years after leaving school.

The data link the education registry with social security records, combining each student's educational pathway with her employment and earnings after graduation. They include nationally standardised test scores at the end of grade nine, which control for ability before track entry, and the municipalities of residence and of every vocational school. Selection into the dual track is addressed with a commuting-distance instrument: the difference between the road travel time from the student's municipality of residence to the nearest school that took in dual students in her field in her year of entry and the travel time to the nearest vocational school offering that field. We estimate effects with a control function approach, report ordinary least squares alongside it, and compare both with a linear two-stage least squares benchmark. Because apprenticeships disproportionately attract students in the lower-middle range of the educational distribution (Cavaglia et al., 2020), we also ask whether the returns differ by family background.

Dual vocational education carries a substantial early labour market advantage. In the first year after graduation, dual graduates are employed for about one additional month, roughly 15% of the comparison-group mean, and earn wages that are about 11% higher than those of school-based and workplace-based graduates. The two advantages have different lifespans: the employment advantage shrinks after the first year, while the wage premium remains between 11% and 13% through the third year. The wage effect is positive in every graduation cohort we observe, including the cohort that graduated in 2021, when the COVID-19 pandemic made entry into the labour market most difficult. Controlling for the graduate's employment history and for the size and sector of the employer, both outcomes of the track, lowers the wage premium by two to four percentage points, so sorting by employer size and sector is one channel, but not the dominant one.

[3] Dual tracks were introduced in Hungary in 2015, in Bulgaria and Romania in 2016, and in Serbia in 2019 (Gessler and Peters, 2022).

The decomposition of the comparison group is our central result. Conditional on observables, the wage difference between workplace-based and school-based graduates is about 2% at every horizon and statistically significant at the 5% level only in the first two years, while the dual premium is present against each comparison track separately, although it is smaller and less precisely estimated against workplace-based graduates. Workplace exposure without the dual arrangement therefore does not account for the premium. But the bundle that comes with the dual track does: the individual contract, a legally guaranteed share of training at the employer, and a certified training firm that is typically larger than the average Slovak employer. Our design cannot separate the components of this bundle. Three further results narrow down the interpretation. The premium is not a retention effect: it falls by only two to three percentage points when we exclude the dual graduates whose first employer is a firm certified to train in their field and district, whereas the first-year employment advantage falls by a third to a half. It is similar across quartiles of parental income, so, conditional on entering the track, students from poorer families gain about as much as students from richer ones. Dual and non-dual graduates differ neither in enrolment in post-secondary education nor in the timing of entry. Altogether, these patterns are consistent with a combination of skill formation and better firm–worker matching as drivers behind the persistent wage premium of dual graduates.

We contribute to three strands of literature, which Section 3 reviews. We add to the evidence on the returns to apprenticeship an evaluation of a dual option in its early years, in a country without an apprenticeship tradition, and a comparison of firm-based training without a contract with school-based training at scale, a margin on which Horn (2016) for Hungary is the closest antecedent. We add to the small literature on reforms of vocational education itself a reform that changed neither what students learn nor for how long, but how and under which contract their practical training is organised. And, to our knowledge, we provide the first causal evaluation of the recent dual reforms introduced in Central and Eastern Europe.

Section 2 describes Slovak vocational education and the dual reform, and Section 3 places our research in the broader literature on apprenticeships. Sections 4 and 5 present the data and the empirical strategy. Section 6 reports the effects on employment and wages, the decomposition of the comparison group, heterogeneity by family background, and continuation in education.

Section 7 discusses the channels that can account for these results, and Section 8 concludes.

2 The Slovak education system and dual education policy

2.1 Vocational tracks and the 2015/16 reform

In Slovakia, around 65% of ninth-grade students make the transition to secondary vocational schools, while the remaining 35% continue in general secondary education (ÚHP and IVP, 2024). The choice between vocational and general tracks is made at age 15, upon completion of lower secondary education.[4] Secondary vocational education typically lasts three or four years and corresponds to upper-secondary attainment (ISCED 3). Three-year programmes conclude with the award of an occupational certificate. Four-year programmes always culminate in an occupational *maturita* (baccalaureate), which grants access to further study at ISCED levels 4, 5, or 6, and some of them additionally confer an occupational certificate.[5]

A large number of vocational occupations are offered. In the 2022/23 academic year, 440 approved occupations were available at secondary vocational schools, of which 364 had at least one enrolled student (Cedefop and ŠIOV/SNO, 2023). Enrolment is nevertheless concentrated: 98% of vocational students were enrolled in 226 occupations (ÚHP and IVP, 2024), and in 2025 the ten largest programmes accounted for 36% of full-time vocational students (CVTI SR, 2025).

Prior to the 2015/16 academic year, the vocational education track in Slovakia was organised either as school-based or workplace-based type, depending on the school and, in some cases, the occupation. In the 2015/16 academic year, Slovakia introduced a dual vocational track, which has since operated alongside the existing school-based and workplace-based tracks. Dual vocational education was established by the Act on VET 61/2015, and the first cohort of dual-track students enrolled in the 2015/16 academic year.[6] The initiative to introduce the dual vocational track was

[4]Admission to programmes leading to the *maturita*, both general and vocational, is based on school entrance exams, although top performers in the national grade-nine test can be exempted.

[5]Students who complete a three-year programme may subsequently enrol in a two-year programme leading to an occupational *maturita*. Figure A1, in the Online Appendix, shows the vocational pathways within the Slovak education system. Tables and figures whose numbers begin with A are in the Online Appendix.

[6]The full wording of the act and its successive amendments (in Slovak): https://www.slov-lex.sk/ezbierky/pravne-predpisy/SK/ZZ/2015/61/, last accessed in November 2026.

driven by firms and professional organisations in response to growing mismatches between the skills of vocational graduates and labour market requirements (Petanovitch et al., 2014). It is common for vocational schools to run all three tracks in parallel, even within one class.

Students in all three tracks receive the same weekly allocation of instructional time across general subjects (15–20 hours), occupation-specific theoretical subjects (4.5–6.5 hours), and practical training (9–14 hours), with total weekly instructional time between 32 and 36 hours.[7] Instruction in general and occupation-specific theoretical subjects always takes place at the vocational school. The key distinction across tracks lies in *how* practical training is organised for the same occupation.

In the school-based track, students complete practical training primarily in school laboratories and workshops. In the workplace-based track, part of the practical training takes place at an employer's workplace under a fixed-term agreement between the school and the firm. The law does not define how much of the practical training has to be delivered at the workplace, so its extent varies across schools and programmes. In the dual track, practical training is organised through an apprenticeship based on an individual contract between the student and a training firm certified to train in the occupation. At least half of it is guaranteed to take place at the employer's training workplace, and only the rest may be delivered in a school workshop.[8] Dual students receive from the firm an average gross pay (stipend) of around 300 euro a month and free lunch vouchers on firm training days (Table A1). Remuneration for productive work, for example for work beyond the mandatory training hours, is possible in both firm-based tracks. The tracks therefore differ in more than the place where practical training happens. Relative to the workplace-based track, the dual track adds an individual contract with pay and a guaranteed minimum share of training at a certified employer. Comparing workplace-based with school-based graduates shows whether some exposure to a workplace without these features pays, while comparing dual graduates with either group measures the return to the dual arrangement as a whole. We return to both comparisons in Section 6.2.

Owing to the dual nature of training, combining instruction at school and in a training firm

[7] Based on https://siov.sk/statne-vzdelavacie-programy/ (in Slovak), last accessed in November 2026.
[8] Act on VET 61/2015, § 10(2).

under an apprenticeship contract, students enrolled in this track in Slovakia were officially labelled as *dualists*.[9] The number of dualists gradually increased from fewer than 500 in 2015/16 to almost 10,000 in 2022/23. Nevertheless, they still represented only a small share (7.4%) of the 126,500 full-time upper-secondary vocational students (Cedefop and ŠIOV/SNO, 2023). For our analysis, we consider only graduates from the two types of secondary vocational schools that culminate in an occupational certificate (H-type and K-type in Figure A1), as the overwhelming majority of dualists are enrolled in these two school types. Dual places were spread unevenly across the country, partly mirroring population density.[10] What matters to our design, however, is not this aggregate geographic pattern but how far each student lived from a school with dual places in her own field, which we can directly measure (Section 5.2).

The Slovak dual vocational education system shares several features with the Germanic apprenticeship model (Table A1). The typical age of entry, programme duration, and time spent in firms are similar. The Slovak dual track, however, covers a much smaller share of vocational students, its pay is substantially lower, and far fewer graduates stay with their training firm.

2.2 Firms that train

Fewer than 1% of Slovak firms participate in the training of dualists. The register of employers certified to provide practical training in the dual system lists about 2,200 firms. We describe those that can be matched to company accounts from FinStat, a commercial database of Slovak financial statements (Table A2). They are far from a cross-section of the Slovak enterprise sector. The share of firms certified to train rises steeply with size: it is only 0.3% among firms with fewer than ten employees, but 14.1% among firms with fifty to two hundred and forty-nine employees and almost 60% among firms with a thousand employees or more (Table A3). Selection into the system is mainly on size. In terms of value added, certified firms resemble other firms within each size class: only among the smallest firms, with at most one employee, does the median certified firm report markedly higher value added. From two employees upwards the two medians are

[9] There exists a word *apprentice* (=*učeň*), but it is considered old-fashioned and has a more ambiguous meaning in the Slovak language. Thus, throughout our study, we use *dualist* rather than *apprentice* to refer to a vocational student with an apprenticeship contract.

[10] Figure A2 maps all vocational schools and the newly enrolled dual students by municipality.

close, and the sign of their difference changes from one size class to the next (Figure A3).[11]

Training provision is also uneven across sectors and places (Table A4). Every region has training firms in at least twenty-four of the thirty-one sectors distinguished by FinStat, but between a third and a half of a region's training firms are in its three largest sectors, while the leading sector differs across regions. The consequence is a thin grid: 15% of region-by-sector combinations contain no certified employer at all. The share rises to 69% at district level, which is the scale at which a fifteen-year-old actually chooses. Where a dualist trains is therefore not a random draw from the firm distribution. The dual contract places a student inside a larger employer than a comparable student would typically train with, which is part of what the dual track delivers. It also limits external validity: what we estimate is the effect of a system staffed by larger and financially sound firms, and it need not carry over to an expansion that brought in the small firms which today stay out.

2.3 Survey evidence

The dual system has been monitored since its introduction through feedback surveys of students, employers, schools and, in one wave, parents. Since 2024, a graduate tracing survey has also followed vocational school leavers about fifteen months after graduation; unlike the feedback surveys, which record students' plans and opinions, it records what graduates actually do after school.[12] Altogether the feedback surveys include the student cohorts whose labour market outcomes we observe, while the tracing survey follows later graduation cohorts, mainly those of 2022/23 and 2023/24. None of the surveys is representative in the sampling sense, so we use them descriptively (Table A5), to document how the participants describe the arrangement and to inform the interpretation in Section 7.

Three patterns stand out. First, dual students are satisfied with the track and view it as preparation for work: 92% in the 2018 wave and 98% in the 2020 wave consider dual study good

[11] Certified firms are also less likely to report zero value added: 0.06% against 0.82% of other firms.

[12] Feedback surveys commissioned by the State Vocational Education Institute for the reference years 2018, 2019 and 2020 (Centire and TRANSFER Slovensko, 2019b,a,c, 2020c,b,a); the external evaluation of the operational programme that funded the reform, whose wave of early 2022 also covered parents (Externé hodnotenie OP Ľudské zdroje, 2022); and the tracing survey (ŠIOV, 2025, 2026).

preparation for working life, and 91% in the 2022 wave would recommend it to others. Their stated motives are dominated by skill acquisition (62%) and by the stipend and other benefits received during the study (44%), with securing a job after graduation named only by 15%.

Second, the link to the training firm is real but far from guaranteed. Employers report having offered a job to 82% of their dual graduates in the 2018 wave, yet only 28% of their graduates actually started work with them; in the 2020 wave, 445 of 581 dual graduates received an offer and 257, or 44%, took it up. Between 40% and 65% of students hold a pre-contract on future employment alongside the training contract, and 49% of parents in 2022 expect their child to work for the training firm. The realised share is lower and imprecisely measured: 31% of dual contract holders in the 2024 tracing wave and 15% in the 2025 wave.[13]

Third, the 2025 tracing wave reports that the *same* share of non-dual graduates ended up employed at the firm where they did their practical training, and concludes that participation in the dual system makes no difference to the rate at which graduates move into employment with their practice firm (ŠIOV, 2026). Taken at face value, the *excess* retention attributable to the dual track, the dual minus non-dual difference, is approximately zero. The sample is small and not representative, so we do not lean on it. It nevertheless suggests that retention may contribute to the first-year employment advantage of dual graduates, but is unlikely to account for their wage premium, a point we develop in Section 7.

3 Related literature

Apprenticeship entails two distinct features, and both of them, or only one, may pay off in the labour market. Apprenticeship training may pay because it happens *inside a firm*, with real technology, real customers, and real workplace norms, or because of the arrangement that ties a particular student to a particular firm, with a contract, remuneration, and mutual commitment. Most of the literature compares apprentices with school-trained peers and therefore cannot separate the two. We review the evidence on the headline returns and on the two standing objections, namely that the effects are transitory and that they reflect mere retention, and return at the end to

[13] On 238 and 105 respondents respectively.

the rare evidence on firm-based training without an apprenticeship contract.

Early evidence for Germany, where most vocational students are apprentices, suggested that apprenticeship graduates avoid early non-employment better than graduates of school-based vocational education (Winkelmann, 1996). Studies that address selection with an instrumental variable approach find smaller and shorter-lived advantages. Instrumenting with the availability of apprenticeship places, Parey (2016) finds that former apprentices are less often unemployed, but that this advantage fades over time and that their wages do not differ significantly from those of graduates of full-time vocational schools; instrumenting with the father's vocational education, Riphahn and Zibrowius (2016) find no significant differences in early labour market outcomes between the dual and school-based tracks. Raw apprenticeship premia can thus be selection dressed as returns, and any evaluation stands or falls with its identification strategy.

Evidence from other established systems is more favourable, also under credible identification. Fersterer et al. (2008) use the length of apprenticeship completed in failed small Austrian firms as an instrument for its actual length and find returns of 3–4% to an additional year of apprenticeship training, with OLS and IV estimates that are statistically indistinguishable. Plug and Groot (1998) report a similar absence of selection for the Netherlands. In Switzerland, Lüthi (2025) compares dual and school-based graduates within occupations offered in both tracks: school-based graduates are more likely to enter higher education, but also more likely to be neither employed nor in education or training in the first three years after graduation. Swiss tertiary graduates with a dual secondary degree also find their first job faster and earn about 7% more in it (Oswald-Egg and Renold, 2021). Across 36 countries, labour market indicators are more favourable where the dual system is strong (Bolli et al., 2021). All these studies compare apprentices with school-trained peers, so workplace exposure and the apprenticeship arrangement move together.

The first standing objection is that the advantage is transitory. In Flanders, Neyt et al. (2020) find only an early advantage in the school-to-work transition that quickly fades away. By contrast, a large expansion of apprenticeship contracts in Brazil increased the probability of a permanent job by 7%, also five years after the apprenticeship (Corseuil et al., 2019), and in England the earnings of workers with an apprenticeship are 30–40% higher for men and 10–20% higher for women at age 23 and remain there at least until age 28 (Cavaglia et al., 2020). We address this

objection by following the outcomes of distinct graduation cohorts over different time horizons.

The second standing objection is that the advantage reflects the retention of apprentices by their training firms. The minority of German apprentices who are not retained initially experience a wage drop of 15%, which balances out within five years (von Wachter and Bender, 2006; Fitzenberger et al., 2015), and the gains from the 2003 Italian apprenticeship reform were driven primarily by large retaining firms (Albanese et al., 2021). In contrast, dual graduates in Madrid enjoy better employment prospects and higher wages than their school-based counterparts even after removing retained apprentices (Bentolila et al., 2023), and in France the advantage of apprentices in the transition to the first job survives the exclusion of retained apprentices for men (Bonnal et al., 2002). Realised retention in Slovakia is low by Germanic standards, a point we document in Section 2.3.

The advantage of apprenticeship may also depend on circumstances and on the content of the treatment. In a correspondence experiment, employers show no preference for apprentices over school-based vocational graduates, except in regions with high unemployment (Cahuc and Hervelin, 2024). In a field experiment in Uganda, firm-trained workers find employment faster, but the gains of workers trained in certified vocational institutes are larger and more persistent, both in employment and in earnings (Alfonsi et al., 2020). Dual training may also raise graduation rates and grades within vocational school (Mora et al., 2024) and ease the labour market integration of youth with a migration background (Zisler et al., 2025).

A small literature evaluates reforms of vocational education itself, though not reforms of how practical training is organised. Reforms that increased the general-education content of vocational programmes or moved students from vocational into general tracks produced no labour market gains from the additional general education (Oosterbeek and Webbink, 2007; Malamud and Pop-Eleches, 2010; Zilic, 2018). Updates of vocational curricula have been shown to raise firms' technology adoption (Schultheiss and Backes-Gellner, 2024) and the wages of the cohorts trained under the new curricula (Buehler et al., 2024; Salomons et al., 2026), and to affect the careers of incumbents trained under the old ones (Janssen and Mohrenweiser, 2018). The Slovak reform changed neither the content nor the length of vocational programmes, but how and under which contract their practical part is organised. For Central and Eastern Europe, where dual tracks

were introduced after 2014, the available evidence compares vocational with general graduates, as Hoidn and Šťastný (2023) do for the Czech Republic with PIAAC data, or workplace-based with school-based training in Hungary (Horn, 2016). Causal evidence on the new dual tracks is, to our knowledge, absent.

Direct evidence on firm-based training *without* an individual contract is scarce. The closest antecedent comes from Hungary, where Horn (2016) compares apprentices in workplace-based training with school-based vocational students within the same vocational track and finds no significant difference in employment about one year after graduation. Slovakia runs school-based, workplace-based, and dual training side by side within the same occupations, which allows us to probe this margin at scale and to compare both arrangements with the dual track.

4 Data

We use social security records from 2019 to 2023, matched with education registry data. We limit the sample to individuals with an upper secondary vocational education in three- or four-year programmes with occupational certification (H-type and K-type). We observe the graduation cohorts 2018/19 to 2021/22, that is, students who graduated in 2019 to 2022, the earliest of them for up to four years after graduation and the latest for one year. The 2018/19 cohort is the first cohort of four-year and the second cohort of three-year dual graduates.[14] The data have a panel format, so we can observe the labour market trajectory of each individual. Altogether, our data include information on 25,611 individuals who completed upper secondary education in occupations where dual education was provided for at least one student.[15] We consider this full set for the analysis of continuation into post-secondary education. For the early labour market outcome analysis we only consider a subset of 15,858 upper-secondary vocational graduates who did not continue in further studies. A graduate enters the sample for a given year after

[14]Dual students who entered three-year programmes in 2015/16 graduated in 2017/18, which is not among the cohorts we observe. Of the 2018/19 dual graduates, 247 completed a three-year and 273 a four-year programme.

[15]The occupations belong to 14 broad study fields, which enter the fixed effects and the instrument: metallurgy; mechanical engineering and metalworking; electrical engineering; technical chemistry of silicates; technical and applied chemistry; food processing; textiles and clothing; leather processing and footwear; woodworking; printing and media; construction; transport, post and telecommunications; agriculture; and trade and services.

graduation if she has at least one Slovak social insurance record in that year, as an employee, as a self-employed person, or in another insured status. Graduates without any record in a year, for example because they work abroad, including cross-border commuters, or are neither employed nor otherwise insured, are not observed in that year, so the employment outcomes are measured among graduates present in the Slovak social insurance system.

Our main outcomes are measures of early labour market success: annual employment duration in days and average monthly earnings. Additionally, we investigate continuation into post-secondary education, at ISCED level 4 or 5, with two measures: whether a graduate is enrolled in post-secondary education in each year after graduation, and whether she newly starts it in that year. The second measure distinguishes graduates who postpone further study from those who never take it up.

Our variable of interest is a dual vocational dummy that equals one for workers who graduated from a dual track. In our baseline specification the comparison group consists of all other types of vocational education: school-based and workplace-based. The data also identify which of the two a non-dual graduate attended, which allows us to split the comparison group and to compare the two non-dual tracks with each other; we exploit this in Section 6.2. Additionally, we include several sets of control variables to capture observable characteristics. The individual variables include age, gender, dummies for a child under six, native language and disability, and an ability proxy, the result of the national standardised test in mathematics at the end of grade nine (*Testovanie 9*). We also control for family background using the education and income of parents and an indicator that at least one parent was self-employed. From the school registry data, we have information on the school region, school provider, school size, learned occupation, and the actual study duration. We include fixed effects for the region (*kraj*) of the upper secondary school, and we control for the district unemployment rate, which varies over time, and for the share of marginalised Roma communities at the municipality level. In two extensions of the earnings regressions, we additionally include the graduate's employment history and the size and sector of the employer (Section 6.1).

We show statistical properties of our data by dual status in Table 1. Dual graduates differ from their school-based and workplace-based peers on several observables. They more often enrol

in four-year programmes, perform better on the standardised test at age 15, with a standardised difference of 0.38, and much more frequently complete their vocational studies on time. There are fewer women among dual graduates, mostly because of the fields in which dual places are offered,[16] and the parents of dualists earn more and are somewhat better educated. Selection into the dual track on observables is therefore positive, and we cannot exclude selection on unobservables either. In the next section, we propose an econometric procedure that addresses this problem.

Table 1: Descriptive statistics

	School-/workplace-based graduates		Dual graduates		Standardised
	mean	*(std. dev.)*	mean	*(std. dev.)*	difference
Outcomes					
Employment days	182	*(140)*	226	*(139)*	0.32
Average monthly wage (Eur)	987	*(325)*	1,149	*(312)*	0.51
Continuation in further educ.	0.33	*(0.47)*	0.30	*(0.46)*	−0.06
Control variables					
Three-year programme	0.61	*(0.49)*	0.34	*(0.47)*	−0.56
Testovanie 9	28.13	*(22.38)*	36.91	*(24.34)*	0.38
Years overdue	0.27	*(0.83)*	0.04	*(0.24)*	−0.38
Female	0.25	*(0.43)*	0.11	*(0.31)*	−0.37
Parental wage (annual, Eur)	17,933	*(12,559)*	22,655	*(13,958)*	0.36
Parental education					
Primary and below primary	0.13	*(0.33)*	0.07	*(0.25)*	−0.21
Lower secondary	0.45	*(0.50)*	0.42	*(0.49)*	−0.06
Upper secondary	0.33	*(0.47)*	0.39	*(0.49)*	0.13
Tertiary	0.09	*(0.29)*	0.12	*(0.33)*	0.10
Number of graduates	10,818–19,473		2,456–3,442		

Notes: Data used are matched social security-education registry data 2019 to 2023. We report here statistical properties of the maximum available sample for each variable. Depending on the specification, sample size for school-/workplace-based graduates varies between 10,818 and 19,473, the dualists' sample varies between 2,456 and 3,442 graduates. The standardised difference is the difference in means divided by the square root of the average of the two group variances. In our regressions we additionally control for school size (3 dummies), region (7 dummies), and study fields (13 dummies). In specification W3 of the wage regressions we further include 17 NACE industry dummies.

[16] Conditional on the study field and the other controls of the first stage, women are 1.9 percentage points less likely than men to be dualists, with a standard error of 1.8 points, against a raw difference of about ten points.

5 Empirical strategy

5.1 Baseline specification and the selection problem

The aim of our regression is the causal identification of the effect of dual vocational education on labour market outcomes. Our baseline specification is

$$Y_i = \alpha_0 + \beta_0 \cdot Dual_i + X_i\beta + Z_s\gamma + \theta_j + \tau_t + \varepsilon_{ijst}. \tag{1}$$

Our dependent variable, Y_i, stands for an early labour market outcome or an education attainment measure of individual i. The variable of interest is $Dual_i$, which is a dummy that equals one if individual i graduated from a dual vocational programme. In X_i, we control for individual and parental background characteristics, Z_s are vocational school and regional characteristics, θ_j are study field fixed effects, and τ_t stands for graduation cohort t fixed effects.

Given the specification in Equation 1, our treatment group is dual vocational graduates and, accordingly, our control group contains graduates from the other two types of vocational tracks that exist in parallel (school-based and workplace-based). Estimated by OLS, β_0 is the effect of the dual track under selection on observables. The rich set of controls, including the grade-nine test score and parental income and education, absorbs much of the observable selection documented in Table 1. The distributions of grade-nine test scores of dual and non-dual graduates overlap widely but do not coincide (Figures A4 and A5); the score itself is controlled for, but the difference suggests that selection extends to traits the test does not measure. The controls cannot absorb selection on unobservables such as motivation, and such selection is plausible: in the 2018 survey wave, 76% of dual students had signed their training contract before the start of their studies (Table A5), which may suggest higher motivation or interest.

5.2 A distance-based instrument and the control function

To address selection on unobservables, we follow the literature that instruments apprenticeship.[17] Our research takes inspiration from Bentolila et al. (2023), who instrument the apprenticeship dummy by the difference in travel time between the lower secondary school and the two nearest vocational schools offering the dual and the school-based track in the student's field of choice. Because we observe the students' municipality of residence, we measure travel time from there rather than from the lower secondary school. Our instrument is the difference, in minutes of road travel time, between the travel time from the student's municipality of residence to the closest vocational school with realised dual places in the student's study field and programme type (H or K) and the travel time to the closest vocational school offering that field and programme type. Travel times are computed along the road network between the municipality of residence and the municipality of each school. Dual places are measured in the year in which the student entered upper secondary school. The municipality of residence is the student's permanent residence as recorded in the education registry in her final year of upper secondary school, the only year in which we observe it for every cohort. A school has realised dual places in a field if students of that field were actually enrolled in its dual track. The difference is zero when the closest vocational school is itself a school with dual places in the student's field.

Using the distance-based instrument, we assume that a ninth grader continuing in vocational education will more likely enrol at a school closer to home. What gives the instrument its bite is not the overall scarcity of dual places but the sparseness of the *field-by-place* grid. Because the comparison is with the closest school with dual places in the student's own field, a student is shifted into or out of the dual track by whether her particular occupation happens to be trained dually nearby. The sectors of certified training firms give an impression of how thin this grid is: roughly seven in ten district-by-sector combinations contain no certified training employer at all, and a further quarter contain only one or two (Table A4). Two otherwise identical students living a short distance apart can therefore face very different travel times to a dual programme

[17] Instruments used include training firm dissolutions (Fersterer et al., 2008), business cycle fluctuations in apprenticeship offers (Parey, 2016), or the supply of certified journeypersons who train apprentices in firms (Gunderson and Krashinsky, 2015). Table A6 overviews the articles most relevant for our research design, paying special attention to the definition of the control group and to the means of addressing selection into apprenticeship.

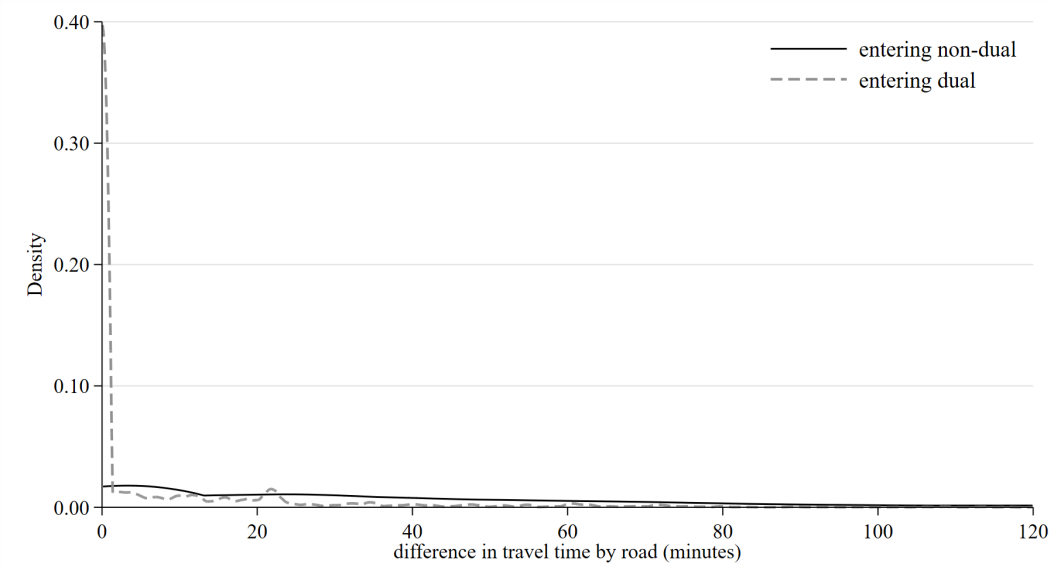


Figure 1: Distribution of the distance-based instrument

Notes: Density of the difference, in minutes, between the road travel time from the student's municipality of residence to the closest vocational school with realised dual places in the student's study field and programme type and the travel time to the closest vocational school offering that field and programme type. Dual places are those of the student's year of entry into upper secondary school; the municipality of residence is the permanent residence recorded in her final year. The difference is zero when the closest vocational school is itself a school with dual places in the student's field, and it increases with the additional travel time to a dual place. The sample is limited to vocational school graduates between 2019 and 2022. *Entering dual* (grey dashed) are students who went on to graduate from the dual vocational track; *entering non-dual* (black solid) are those who graduated from the school-based and workplace-based tracks. The horizontal axis is truncated at 120 minutes for legibility; beyond that point both densities are close to zero, and the instrument reaches at most 346 minutes.

in their field. Figure 1 plots the distribution of the instrument for dual and non-dual graduates. The density of dual graduates is by far the highest when the closest vocational school is also the closest school with dual places in the field of the learned occupation. The exclusion restriction requires that this travel time affects employment and earnings only through the track. The first step of our two-step procedure is

$$Dual_i = \delta \cdot dist_i + X_i\alpha + Z_s\delta + \theta_j + \tau_t + \nu_{ijst}, \tag{2}$$

where $Dual_i$ is a dummy variable that equals one if individual i graduated from a dual vocational programme and $dist_i$ is the individual-specific distance-based instrument.

Because the endogenous variable is binary, we use the control function (CF) approach (Wooldridge, 2015) and model the first stage (Equation 2) as a standard probit. The second

step is estimated as

$$Y_i = \alpha_0 + \beta_0 \cdot Dual_i + \gamma \cdot CF(Dual_i) + X_i\beta + Z_s\omega + \theta_j + \tau_t + \epsilon_{ijst}, \tag{3}$$

where, instead of predicted values from the first stage, the generalised residual from the first stage, $CF(Dual_i)$, is included in the second stage.[18] Our specification (Equation 3) delivers in $\hat{\beta}_0$ a causal effect of graduating from the dual track on outcome Y_i. Because the instrument shifts the dual dummy without shifting the outcome directly, the estimate applies to students whose track was decided by proximity to a dual school, and not necessarily to those who would have entered a dual programme in any case.

Beyond the pooled comparison, we estimate the same specification on two restricted samples, one with dual and school-based graduates only and one with dual and workplace-based graduates only, with the same instrument. We also compare workplace-based with school-based graduates directly, with workplace-based training as the treatment, and for this comparison we report OLS estimates only. Students apply for a dual place or to a school, but they do not choose between the workplace-based and the school-based variant of a programme, which is determined by the training agreements of the school. A distance-based instrument therefore has no individual choice to shift in this comparison, and its first stage is weak beyond the first year.[19] The comparison of the two non-dual tracks is thus conditional on observables, like our OLS estimates of the dual effect.

5.3 Identification checks

We report four sets of diagnostics in Table 2. First, the instrument is relevant in all samples that identify the dual effect. In the probit first stage that the control function uses, the excluded instrument has a $\chi^2(1)$ statistic of at least 13.9 in every sample in the first three years after graduation, comfortably above the conventional threshold of 10. In the fourth year, which rests on the earliest graduation cohort alone, it ranges from 7.0 to 15.4 and falls below 10 in the pooled and the dual versus school-based employment samples, so the fourth-year employment

[18] Practically, it is the first derivative of the probit log likelihood with respect to $\delta \cdot dist_i + X_i\alpha + Z_s\delta + \theta_j + \tau_t$.
[19] The probit χ^2 statistic of the instrument is below 6 in the wage sample from the second year on.

estimates should be read with particular caution. The F statistic from a linear probability model with the same regressors and the same sample as the probit is 70.6 in the first year in the pooled employment sample, and the Kleibergen–Paap F statistic of the linear 2SLS model with the full set of controls is 71.5. The probit, however, is the relevant first stage for our estimator. The instrument shifts enrolment by a large amount relative to its association with student characteristics. Ten additional minutes of travel time to the closest school with dual places in the student's field lower the probability of graduating from the dual track by 1.35 percentage points in the linear probability model and by 3.5 points on average in the probit (Panel A), in a first-year sample in which 17% of graduates are dualists. Conditional on the controls of the first stage, the same ten minutes are unrelated to the grade-nine test score, age, gender, disability, having a child under six, parental education, and having a self-employed parent (Table A7). The exception is parental income: annual parental income is lower by about 147 euro for every ten minutes, which is less than 1% of its mean. Parental income quartiles enter both stages of the estimation, and the premium is flat across them (Section 6.4). Unconditionally, that is, with no controls other than the year of the grade-nine test (column (3) of Table A7), students who live farther from a dual place in their field score lower and come from poorer families, which is why the instrument is used only conditionally on region, study field and programme type.

Second, a linear two-stage least squares (2SLS) benchmark with the same instrument and controls gives estimates of the same sign as the control function, with less precision (Panel B). For employment days the 2SLS estimates are larger, 76 days in the first year and 81 in the second. From the third year on, when only two cohorts and then one remain, they are too imprecise to show whether the advantage persists: the standard errors are 33 and 46 days. The Anderson–Rubin confidence sets, which remain valid when the instrument is weak, exclude zero in the first two years. For wages the 2SLS point estimates are between 6% and 11% in the first three years, but their standard errors are more than one and a half times those of the control function, and the Anderson–Rubin set for the first-year effect, -4% to 15%, includes zero.[20] The precision of the control function therefore rests on its parametric first stage. We interpret

[20] In the comparison of dual with school-based graduates, the first-year Anderson–Rubin test rejects a zero effect only at the 10% level ($p = 0.06$).

the wage results as robust in sign and size across estimators, with their statistical significance established by OLS and the control function. Panel B also reports the reduced form, the effect of the instrument on the outcome without the dual dummy; with a single instrument, the 2SLS estimate is the ratio of the reduced form to the corresponding linear first stage. Ten additional minutes of travel time lower employment significantly, by about one day, in each of the first two years, and insignificantly thereafter; the wage reduced form is about -0.1% per ten minutes in the first three years, significant at the 10% level only in the second, and -0.8% in the fourth.

Third, the test of endogeneity, that is, the test that the generalised residual enters the second stage with a zero coefficient, does not reject the exogeneity of the dual dummy in most specifications. The p-values lie between 0.40 and 0.74 for wages in specification W1 in the first three years and between 0.16 and 0.81 for employment days; only in the fourth year, on the smallest sample, does the test reject for wages at the 10% level. The CF and OLS estimates are therefore statistically hard to tell apart in most cells, and the control function buys robustness to selection on unobservables at the cost of precision rather than correcting a large measured bias. We report both estimators throughout rather than preferring one. Where the text quotes a single number, it is the control function estimate; OLS gives lower wage effects (9.5% against 11.1% in the first year) and larger employment effects (32 against 28 days).

Fourth, we re-estimate the wage specification with an alternative instrument built from the number of dual places a school *planned* to offer rather than the number it filled. This addresses the concern that filling rates may themselves reflect school or employer quality. The alternative instrument yields larger wage effects, between 26% and 29% in the first three years, with the same sign and significance pattern. Its probit first stage is weak, however, with χ^2 statistics of 7.9 and 6.2 in the first two years, 3.8 in the third, and a coefficient of the wrong sign in the fourth, so we present it as a robustness check and not as a preferred specification.

5.4 Threats to identification and what the estimates mean

The control function rests on the exclusion restriction that the travel-time difference affects employment and earnings only through the track. Two features of the instrument make this restriction more plausible than for a simple distance to the nearest dual school, and three threats

Table 2: Identification diagnostics

	Years since graduation			
	1	2	3	4
Panel A: probit first stage, $\chi^2(1)$ on the excluded instrument				
Dual vs all other tracks, employment sample	35.6	23.5	22.3	7.4
Dual vs all other tracks, wage sample	29.8	25.1	31.0	13.4
Dual vs school-based, employment sample	35.8	27.5	21.5	7.0
Dual vs school-based, wage sample	30.6	30.9	30.8	13.5
Dual vs workplace-based, employment sample	30.0	14.3	13.9	10.5
Dual vs workplace-based, wage sample	23.3	16.1	15.7	15.4
Memo: LPM F, probit regressors and sample	70.6	51.6	44.5	24.9
Memo: Kleibergen–Paap F, linear 2SLS	71.5	52.3	47.0	26.2
Memo: effect of 10 minutes on P(dual), LPM (pp)	-1.35	-1.11	-1.20	-1.54
Memo: effect of 10 minutes on P(dual), probit AME (pp)	-3.48	-2.78	-2.87	-3.17
Panel B: linear 2SLS benchmark, Anderson–Rubin sets and reduced form, dual vs all other tracks				
Employment days, 2SLS	76.1^{***}	80.8^{***}	16.9	69.3
	(21.56)	*(27.62)*	*(32.75)*	*(46.45)*
Anderson–Rubin 95% set	[36.5, 121.9]	[30.4, 142.2]	–	–
Reduced form, per 10 minutes	-1.02^{***}	-0.88^{***}	-0.20	-1.09
	(0.29)	*(0.28)*	*(0.40)*	*(0.70)*
Log wage, W1, 2SLS (in %)	5.6	11.1^{*}	9.1	36.8^{***}
	(4.53)	*(6.17)*	*(9.01)*	*(11.58)*
Anderson–Rubin 95% set (in %)	[−3.8, 14.6]	[−2.3, 23.0]	–	–
Reduced form, per 10 minutes (in %)	-0.09	-0.15^{*}	-0.14	-0.79^{***}
	(0.07)	*(0.08)*	*(0.14)*	*(0.23)*
Panel C: test of endogeneity in the CF model, dual vs all other tracks (p-value)				
Employment days	0.784	0.421	0.807	0.163
Log wage, W1	0.558	0.738	0.400	0.060
Log wage, W2	0.398	0.627	0.421	0.044
Log wage, W3	0.124	0.358	0.297	0.020
Panel D: alternative instrument based on planned dual places (specification W2)				
Log wage, CF (in %)	27.0^{***}	28.9^{***}	26.0^{**}	57.6^{**}
	(6.54)	*(9.52)*	*(10.96)*	*(26.21)*
Observations	7,255	4,789	2,277	662
Probit first stage, z on the instrument	-2.81	-2.49	-1.95	1.66

Notes: Data used are matched social security-education registry data 2019 to 2023. Panel A: $\chi^2(1)$ statistic of the excluded instrument in the probit first stage of the control function; all coefficients on the instrument are negative, and the probit drops observations whose treatment status the fixed effects predict perfectly. Memo rows, pooled employment sample: F statistic of the instrument in a linear probability model (LPM) with the probit's regressors and sample; Kleibergen–Paap F statistic of the 2SLS model of Panel B; effect of ten more minutes of travel time on the probability of graduating from the dual track, in percentage points, in the LPM and as the probit's average marginal effect (AME). Panel B, pooled sample: linear 2SLS with the instrument and controls of the control function; Anderson–Rubin 95% confidence set, valid whatever the strength of the instrument (controls partialled out, sample about 1% larger); reduced form per ten minutes, without the dual dummy, on the sample of the OLS columns of Table 3. Wage rows use specification W1 (Section 6.1). Panel C: p-value of the test that the generalised residual has a zero coefficient in the second stage (null: the dual dummy is exogenous). Panel D: Table 3, column (6), with the alternative instrument built from *planned* rather than filled dual places, and the z statistic of that instrument in the probit first stage. Standard errors, clustered on the school level, in parentheses. Significance on 0.1, 0.05, and 0.01 is indicated as *, **, ***.

remain.

The instrument is a *difference*: the travel time to the closest vocational school is subtracted from the travel time to the closest school with dual places in the student's field. General remoteness, which affects early labour market outcomes directly through local labour demand and commuting, is therefore differenced out: a student who lives far from any vocational school has an instrument value of zero if the closest school happens to train her field dually. What remains is the field-specific availability of dual places close to home, which varies across neighbouring municipalities and across fields within a municipality because of the thin place-by-field grid documented in Section 2.2. Furthermore, the regressions hold region fixed effects, the district unemployment rate in each year, and the municipality share of marginalised Roma communities constant, so the instrument has to operate within regions and conditional on the local labour market. Monotonicity is plausible by construction: a shorter distance to a dual place in her field cannot make a student less likely to enter the dual track, and the first-stage coefficient is negative in every sample and year (Table 2).

The first threat is that dual places are realised where certified training firms are, and certified firms are large (Section 2.2). A student living near such firms may face a better local labour market in her field irrespective of her track. The reduced form and a placebo test bear on this threat. The reduced form for employment is negative and significant in the first two years and insignificant thereafter (Table 2, Panel B). Among the graduates of the sample who did not enter the dual track, the instrument has no significant effect on wages in any of the first three years. It does predict fewer employment days in the first two years, by about two thirds of a day for every ten minutes of additional travel time, an effect significant at the 5% level that disappears in the third year. It is at least as large among the non-dual graduates who have few dual classmates, defined as those who graduated from a school, field and entry cohort in which fewer than one in ten students was a dualist, so spillovers from dual classmates are unlikely to drive it (Table A8).[21] Local labour demand may therefore contribute to the instrumented employment estimates, which we therefore interpret with caution, but the placebo detects no such effect on wages. The alternative instrument

[21] If this direct effect applied to all graduates, netting it out of the reduced form would lower the first-year 2SLS employment estimate from 76 to about 24 days, close to the control function estimate of 28 days.

built from the places schools planned to offer, which does not depend on which places were actually filled, also gives wage effects of the same sign and significance pattern.

The second threat is intergenerational. Parents who work in training firms may both steer their children into the dual track and help them into a job afterwards, and if such families live close to those firms and to the schools that partner with them, the instrument is correlated with family connections. The survey finding that a third of dual students have a family member who knows their workplace instructor (Table A5) shows that the concern is not hypothetical. We cannot test it directly, since we observe neither the identity of the training firm nor the employer of the parents. Two facts weigh against it as the explanation of our results. The instrument measures proximity to a school with dual places in the student's own field rather than proximity to training firms in general, so a parent's connection to a firm in another field does not move it. And connections of this kind are more common in families with higher parental income and employment, yet we show that the premium is flat across parental income quartiles (Section 6.4), which is not what a connections-driven premium would produce.[22]

The third threat is residential sorting: families who value apprenticeship could move close to a school with dual places. For the cohorts we observe, who were in grade nine between 2014/15 and 2018/19, this is unlikely, because the dual track did not exist before 2015/16 and its early geography was scattered and changing (Figure A2). It becomes more likely as the system matures, which is one reason why the stability of the premium within the earliest cohort (Figure 2) is reassuring. The registry also shows that few students move to another municipality between grade nine and the end of upper secondary school.[23]

Other two points concern the interpretation of the estimates rather than their validity. First, under the normality assumption built into the probit first stage, the control function estimates an

[22]Linking the apprenticeship contract register, which records the training firm, to the parents' employment records would allow a direct test.

[23]Residence is recorded in the final year of upper secondary school. Residential mobility during upper secondary education is, however, very low: linking earlier waves of the education registry, only 3% of graduates changed municipality after track entry and 5% since grade nine, equally among dual and non-dual students. The instrument computed at the entry-year residence, the grade-nine residence, or the location of the lower secondary school correlates with the baseline instrument at above 0.99 in all cases. We rely on the final-year residence because it is available for all cohorts, whereas the registry starts in 2016 and therefore misses the entry-year residence of the earliest entry cohort and the grade-nine residence of the two earliest ones.

average effect for the population of graduates; if that assumption fails, the estimate is best read as the effect for students whose track was decided by proximity.[24] Second, wages are observed only for employed graduates, and dual graduates are employed more in the first year. If the dual graduates drawn into employment by the track earn less than the average dual graduate, the first-year premium understates the effect on wage offers, and it overstates it if they earn more.[25] Throughout, standard errors are clustered at the school level, with about 250 clusters in the pooled first-year sample, which is the level at which dual places are organised.

6 Results

6.1 Employment and earnings

We report our baseline regression results in Table 3 for annual employment days and the log of average monthly wage in each of the first four years since graduation from the vocational school. Columns (1) and (2) give OLS and CF estimates for employment days. Columns (3) to (8) give the corresponding estimates for the log wage in three specifications. Specification W1, our baseline, includes only variables determined before the track was chosen. W2 adds the graduate's employment history since graduation, that is, cumulative employment days and the number of employers, and W3 adds the size and sector of the current employer. Both sets of variables are themselves outcomes of the track, so W2 and W3 decompose the premium rather than estimate it more cleanly. The OLS estimates use the estimation sample of the control function. Each cell comes from a separate regression on the sample of graduates observed in that year, so the number of observations declines mechanically with the horizon: only the earliest cohort is observed four years after graduation.

The early labour market advantage of the dual vocational track is substantial. In the first year after graduation, dual graduates are employed for 32 days longer according to OLS and for 28

[24] That is, students without a strong preference for or against the dual track whose field happened to be trained dually nearby or not. Because the CF and OLS estimates are close and the test of endogeneity does not reject, the distinction matters little in practice for our results.

[25] This selection cannot generate the later premium: the employment gap is gone by the second year in the control function specification, while the wage premium remains at 12% to 13%. Trimming bounds in the spirit of Lee (2009) could formalise this for the first year; we note it as a limitation of the first-year wage estimate.

Table 3: Effect of dual vocational education on employment and earnings

	Employment days		Log monthly wage (in %)					
			W1		W2		W3	
	OLS	CF	OLS	CF	OLS	CF	OLS	CF
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1st year	31.5***	28.0**	9.5***	11.1***	8.2***	10.4***	6.0***	9.6***
	(4.39)	*(13.46)*	*(1.29)*	*(2.80)*	*(1.20)*	*(2.76)*	*(1.07)*	*(2.63)*
Observations	11,258	11,298	8,121	8,170	8,121	8,170	8,121	8,170
R^2	0.19	0.19	0.26	0.26	0.29	0.29	0.35	0.35
2nd year	13.0***	2.7	10.3***	11.6***	8.8***	10.5***	6.2***	9.2**
	(4.41)	*(13.11)*	*(1.66)*	*(3.95)*	*(1.54)*	*(3.87)*	*(1.36)*	*(3.75)*
Observations	7,992	8,018	5,664	5,684	5,664	5,684	5,664	5,684
R^2	0.21	0.21	0.24	0.24	0.29	0.29	0.37	0.37
3rd year	8.8*	5.2	9.5***	13.3**	7.8***	11.3**	4.4**	9.0*
	(4.46)	*(15.32)*	*(2.06)*	*(5.21)*	*(1.95)*	*(5.12)*	*(1.75)*	*(5.14)*
Observations	4,610	4,625	3,123	3,133	3,123	3,133	3,123	3,133
R^2	0.24	0.24	0.21	0.21	0.26	0.26	0.35	0.34
4th year	−1.0	30.9	14.3***	25.6***	13.0***	24.2***	9.9***	23.6***
	(7.40)	*(26.28)*	*(3.00)*	*(7.11)*	*(2.99)*	*(6.67)*	*(2.63)*	*(7.21)*
Observations	1,704	2,017	1,136	1,309	1,136	1,309	1,136	1,309
R^2	0.28	0.27	0.22	0.20	0.25	0.23	0.36	0.34
Employment history	no	no	no	no	yes	yes	yes	yes
Firm variables	no	no	no	no	no	no	yes	yes

Notes: Data used are matched social security-education registry data 2019 to 2023. Each cell reports the coefficient on the dual vocational dummy from a separate regression. Columns (1) and (2) report effects on annual employment days; columns (3) to (8) report effects on the log of the average monthly wage, expressed in percent. The (CF) columns use the distance-based measure to instrument the dual education dummy through a probit first stage. Control variables include gender, age, disability dummy, child under 6 years, language of instruction, percentile from grade 9 standardised test in mathematics, year of graduation from elementary school, region of upper secondary school (fixed effects), school provider dummies, school size, study field, level of completed study, cohort fixed effects, parents' highest attained education, parents' income quartile, district unemployment rate, and municipality share of marginalised Roma communities. Specification W1 uses these controls only; W2 adds the graduate's cumulative employment days and number of employers in the year; W3 adds the size and the economic sector of the current employer (fixed effects). The probit first stage includes the same controls except age, the child dummy, the cohort fixed effects, the district unemployment rate and the municipality share of marginalised Roma communities, which enter the outcome equation only because they are realised after entry into upper secondary education: age and the child dummy refer to the outcome year, the graduation cohort also reflects delays in completing school, and the unemployment rate is measured in the outcome year. OLS is estimated on the sample of the control function's probit first stage, which drops observations whose treatment status is perfectly predicted by a fixed effect, so the OLS and CF columns differ slightly in the number of observations. The χ^2 statistic of the excluded instrument in the first stage is reported in Table 2. Standard errors, clustered on the school level, are reported in parentheses. Significance on 0.1, 0.05, and 0.01 is indicated as *, **, ***.

days longer according to the control function, which is roughly one additional month or about 15% of the control group mean, and they earn 9.5% (OLS) to 11.1% (CF) higher wages. Relative to the comparison-group mean wage of 987 euro, the first-year premium corresponds to roughly 95 to 110 euro per month. The employment advantage then shrinks. In OLS it falls by more than half by the second year, and in the CF specification it is statistically indistinguishable from zero from the second year on.[26] The wage premium does not fade. In the control function it is 12% in the second year and 13% in the third, and is estimated at 26% in the fourth year. The fourth-year estimate rests on the 2018/19 graduation cohort alone, observed between September 2022 and August 2023, and on 1,309 observations. The same rise appears in OLS and in that cohort's own estimates by year since graduation (Figure 2), so it reflects the specificity of that cohort and year rather than the estimator, and we treat it as indicative rather than as evidence of a widening gap.

The CF estimates are larger than OLS for wages at every horizon and smaller for employment days in the first three years, but the two estimators are statistically hard to separate: the test of endogeneity reported in Table 2 (Panel C) does not reject in most of these cells. Selection into the dual track on unobservables therefore does not appear to be the dominant force behind the raw gap, at least among the students whose track was decided by proximity to a dual school.

Columns (5) to (8) add the post-treatment controls. The employment history (W2) lowers the estimated premium by about one to two percentage points, and the size and sector of the employer (W3) by a further one to two points, so that the control function premium falls from 11% to 10% in the first year and from 13% to 9% in the third. Part of the observed advantage therefore reflects the sorting of dual graduates by employer size and sector rather than a pay difference between comparable workers in firms of the same size and sector. Because the employer and the employment history are themselves outcomes of the track, these columns decompose the premium rather than estimate it more cleanly. The attenuation is moderate, which means that sorting is one channel among several rather than the whole story.[27]

[26] The pattern is the same for the log of one plus employment days: the control function estimate is 0.23 (standard error 0.10) in the first year and indistinguishable from zero in the following three years (Table A9). Because the logarithm weighs differences at low numbers of days more heavily, the first-year effect is larger in relative terms in this specification.

[27] The wage premium is robust to replacing the region fixed effects by district fixed effects: in specification W1 the control function estimates are 10.0%, 9.0% and 15.5% in the first three years, against 11.1%, 11.6% and 13.3% with

In Figure 2, we depict the control function estimates of the dual effect by graduation cohort and years since graduation for the log wage and employment days, estimated separately for each cohort-by-year cell. The wage effect is positive in every cell and statistically significant at the 10% level in seven of the ten (at the 5% level in five), ranging from 4% to 24%. The 2019 cohort, the only one observed for four years, holds a stable premium of 11 to 13% over its first three years, before the elevated fourth-year estimate discussed above. The 2021 cohort, which graduated into the most difficult phase of the COVID-19 labour market for entrants, is the weakest at entry, with an insignificant 4% in its first year, and recovers to around 11% in its second. The 2020 cohort, by contrast, starts strong at 14% and its subsequent estimates, while positive, are noisier. The employment-day effects are imprecisely estimated in every cohort once the control function is applied, which is consistent with the shrinking of the pooled employment result.

6.2 Workplace exposure and the dual arrangement

The pooled comparison group mixes two arrangements. School-based students do their practical training in school workshops. Workplace-based students do part of it inside a firm, but without an individual contract with that firm and without a guaranteed minimum share of training at the employer. If the dual advantage came simply from being present in a real workplace, it should appear in the workplace-based track as well. Table 4 separates the three comparisons (wage contrasts plotted in Figure A6).

In the first year, the dual premium is present against each comparison track. In the CF specification, dual graduates earn about 10% more than school-based graduates and about 8% more than workplace-based graduates, and they are employed 30 and 26 days longer respectively, although the latter difference is imprecisely estimated. Against workplace-based graduates the wage premium is smaller and less precisely estimated: in the control function it is significant at the 5% level in the first and fourth years only. The two first-year wage contrasts differ by about two percentage points, in line with the small direct difference between the two non-dual tracks.

The direct comparison of the two non-dual tracks in column (5) is the most informative about workplace exposure. Conditional on observables, workplace-based graduates earn 1.7% more than

region fixed effects (Table A9).

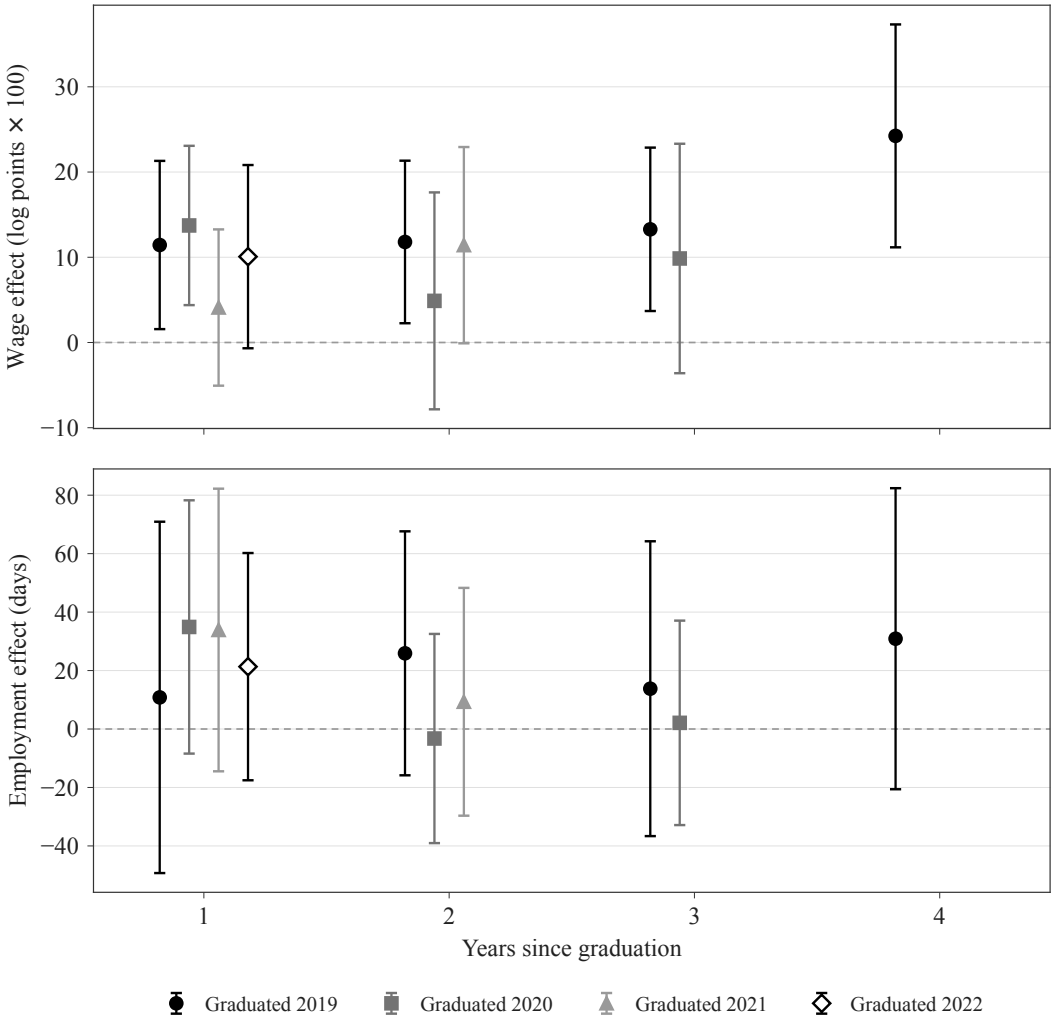


Figure 2: Effects of dual education on log wage and employment days by cohort and years since graduation

Notes: The upper panel plots control function estimates of the effect of dual education on the average log monthly wage separately for each graduation cohort and each year since graduation, using the specification of Table 3, column (6). The lower panel plots the equivalent set of estimates for annual employment days, using the specification of column (2). Whiskers are 95% confidence intervals based on standard errors clustered on the school level. Cohorts are observed only until August 2023, so the number of available years declines across cohorts; all feasible cohort-by-year cells are shown.

Table 4: Decomposing the comparison group: dual, workplace-based and school-based graduates

	Dual vs school-based		Dual vs workplace-based		Workplace- vs school-based
	OLS	CF	OLS	CF	OLS
	(1)	(2)	(3)	(4)	(5)
Panel A: annual employment days					
1st year	35.0***	30.0***	26.5***	26.1*	10.1***
	(4.61)	*(11.55)*	*(4.66)*	*(13.39)*	*(2.82)*
Observations	8,338	8,383	4,821	4,886	9,475
2nd year	13.3***	3.7	17.0***	31.5**	4.0
	(4.66)	*(13.46)*	*(4.79)*	*(14.22)*	*(3.76)*
Observations	6,247	6,272	2,896	2,930	6,972
3rd year	8.9*	5.7	13.9**	25.6	−1.8
	(4.78)	*(15.85)*	*(6.04)*	*(17.28)*	*(4.52)*
Observations	3,637	3,651	1,578	1,604	4,120
4th year	−2.5	10.8	11.7	70.7**	−1.0
	(7.53)	*(23.02)*	*(11.47)*	*(28.86)*	*(5.65)*
Observations	1,450	1,679	474	563	1,873
Panel B: log average monthly wage, specification W1 (in %)					
1st year	10.0***	10.1***	7.9***	8.1***	1.7**
	(1.39)	*(2.52)*	*(1.35)*	*(3.01)*	*(0.84)*
Observations	6,009	6,063	3,725	3,786	6,567
2nd year	11.3***	11.4***	7.8***	7.9*	2.3**
	(1.83)	*(3.75)*	*(1.67)*	*(4.57)*	*(1.17)*
Observations	4,415	4,435	2,225	2,254	4,746
3rd year	10.8***	12.9**	5.7**	8.6*	2.0
	(2.21)	*(5.42)*	*(2.16)*	*(4.67)*	*(1.58)*
Observations	2,434	2,443	1,183	1,206	2,677
4th year	15.5***	23.1***	7.4*	18.8**	1.7
	(3.07)	*(6.31)*	*(4.23)*	*(8.13)*	*(2.63)*
Observations	966	1,086	330	386	1,191

Notes: Data used are matched social security-education registry data 2019 to 2023. Each pair of columns restricts the estimation sample to two of the three tracks. In columns (1) and (2) the sample contains dual and school-based graduates, in columns (3) and (4) dual and workplace-based graduates, and in column (5) workplace-based and school-based graduates, with workplace-based training as the treatment. The CF columns use the travel-time difference to the closest school with realised dual places in the student's field as the instrument. The comparison of the two non-dual tracks is estimated by OLS only, because students do not choose between these two tracks and the distance-based first stage is weak (Section 5.2). Control variables are as in Table 3; Panel B uses specification W1. OLS is estimated on the sample of the control function's first stage. Standard errors, clustered on the school level, are reported in parentheses. Significance on 0.1, 0.05, and 0.01 is indicated as *, **, ***.

school-based graduates in the first year and 2.3%, 2.0%, and 1.7% more in the following three years; the first two differences are significant at the 5% level, the last two are not. In every year the estimates are precise enough to rule out a wage gain from workplace-based training as large as half of the dual premium over school-based graduates (Figure A6).[28] The return to workplace exposure without the dual arrangement is thus small, so mere presence in a firm cannot account for the dual premium. What accounts for it is the bundle that comes with the dual contract: more intensive workplace learning due to a guaranteed minimum share of training at the employer, the contract itself with its pay and defined role, and a certified training firm, which is typically larger (Section 2.2). Our design cannot separate these components, and we cannot measure how much time workplace-based students actually spend at the employer.

Two implications follow from the decomposition exercise. Substantively, it weighs against exposure to a workplace as such as the source of the premium, which narrows the set of mechanisms that can plausibly account for our results, as we discuss in Section 7. It also supports the use of the pooled comparison group in Table 3: since the two comparison tracks perform similarly, pooling them costs little but gains precision.

6.3 Excluding graduates retained by a training firm

We do not observe the training firm of a dual graduate, so retention cannot be measured directly. We therefore use a proxy that errs on the side of counting too many graduates as retained: a graduate is a *stayer* if her first employer after graduation is a firm certified to provide practical training in her field and district. For a dual graduate this captures her own training firm, and any other certified firm in the same field and district; for a school-based or workplace-based graduate, who never trained under a contract, it measures the background rate of starting at such a firm. Dual graduates reach a certified firm far more often: 26% of them start there, against 2% of their peers, a dual-specific difference of about 21 percentage points conditional on occupation, district and cohort.[29]

[28] The employment gap between the two tracks is 10 days in the first year and indistinguishable from zero thereafter.

[29] On broader definitions the gap is similar: 43% of dual graduates against 3% of their peers start at a firm certified in their field anywhere in the country, and 47% against 9% at a firm certified in any field.

Table 5 re-estimates both outcomes after dropping the dual stayers, in the baseline specification. The wage premium changes little: in the control function specification it falls from 11.1% to 8.6% in the first year and from 13.3% to 10.7% in the third, and in OLS from 9.5% to 7.2% and from 9.5% to 7.8%. Excluding all stayers, including the comparison-group graduates who start at a certified firm, gives 7.8% and 14.3%. Dropping the dual stayers removes exactly the dual graduates whose wages could reflect continued employment at the training firm, so the premium among the remaining dual graduates is not generated by retention. The first-year employment advantage, by contrast, falls by a third to a half, from 28 to 18 days in the control function specification and from 32 to 17 days in OLS, so a large part of it comes from dual graduates who start work at a certified firm in their field and district. Excluding stayers mechanically lowers the employment gap, since stayers are by definition employed. The wage comparison has no such problem, because wages are observed only for employed graduates in both groups. Because the proxy counts as stayers also graduates who merely started at some certified firm in their field and district, it removes more dual graduates than were actually retained. This makes the wage test conservative, and it makes the fall in the employment advantage an upper bound on what retention by the training firm itself explains.[30]

6.4 Heterogeneity by family background

Apprenticeships disproportionately attract students in the lower-middle range of the educational distribution (Cavaglia et al., 2020), which makes the distribution of the returns a question of first-order policy interest. Table 6 interacts the dual dummy with the quartile of parental income.

The premium for graduates from the lowest-income quartile is 7.3% in the first year, and none of the three interaction terms with the higher quartiles is individually significant at the 5% level in the first three years; only the top-quartile term in the first year is significant at the 10% level. A joint significance test of the three interactions does not reject at conventional levels in any of the four years.

The dual premium is therefore broadly flat across family backgrounds. Students from poorer families gain about as much as students from richer ones, so the dual track does not appear to

[30] A direct measure would require the apprenticeship contract register, which is not yet available.

Table 5: Employment and wages when dual graduates retained by a training firm are excluded

	1st year		3rd year	
	OLS	CF	OLS	CF
	(1)	(2)	(3)	(4)
Panel A: annual employment days				
Full sample	31.5***	28.0**	8.7*	5.2
	(4.38)	*(13.46)*	*(4.46)*	*(15.32)*
Observations	11,298	11,298	4,625	4,625
Excluding dual stayers	17.0***	17.7	4.2	−4.1
	(5.00)	*(15.03)*	*(5.26)*	*(17.62)*
Observations	10,778	10,778	4,486	4,486
Excluding all stayers	18.2***	18.0	4.9	9.7
	(5.00)	*(14.71)*	*(5.60)*	*(19.03)*
Observations	10,603	10,603	3,834	3,834
Panel B: log average monthly wage, specification W1 (in %)				
Full sample	9.5***	11.1***	9.5***	13.3**
	(1.29)	*(2.80)*	*(2.06)*	*(5.21)*
Observations	8,170	8,170	3,133	3,133
Excluding dual stayers	7.2***	8.6**	7.8***	10.7*
	(1.35)	*(3.55)*	*(2.25)*	*(6.28)*
Observations	7,663	7,663	3,006	3,006
Excluding all stayers	7.5***	7.8**	8.1***	14.3**
	(1.35)	*(3.47)*	*(2.27)*	*(5.67)*
Observations	7,495	7,495	2,754	2,754

Notes: Data used are matched social security-education registry data 2019 to 2023. Each cell reports the coefficient on the dual vocational dummy from a separate regression in the baseline specification of Table 3: annual employment days in Panel A and the log average monthly wage in specification W1, in percent, in Panel B. OLS is estimated on the full sample, so it can differ slightly from the OLS columns of Table 3, which use the sample of the probit first stage. The first row of each panel uses the full sample. The second row excludes dual graduates whose first employer after graduation is a firm certified to provide practical training in the dual system in the graduate's field and district (*stayers*); school-based and workplace-based graduates are kept whether or not they start at such a firm. The third row excludes all stayers, whatever their track. Graduates whose district of residence is unknown cannot be classified and are excluded from the second and third rows. Certified firms are identified from the register of employers certified to train (RZOVP), which records the period in which each certificate was valid. Standard errors, clustered on the school level, are reported in parentheses. Significance on 0.1, 0.05, and 0.01 is indicated as *, **, ***.

Table 6: Dual education effect on log wage by parental income quartile

	Years since graduation			
	1	2	3	4
Dual education, 1st (lowest) quartile	7.3**	9.1*	11.3*	27.7**
	(3.37)	*(4.83)*	*(6.75)*	*(11.26)*
× 2nd quartile	4.8	6.5	−5.5	−8.7
	(3.83)	*(5.16)*	*(7.82)*	*(13.67)*
× 3rd quartile	1.4	0.2	−1.4	−11.7
	(3.93)	*(5.10)*	*(6.29)*	*(13.48)*
× 4th quartile	8.3*	1.9	5.7	32.1
	(4.34)	*(6.27)*	*(7.87)*	*(21.60)*
Joint test of interactions (p)	0.248	0.600	0.675	0.141
Observations	8,170	5,684	3,133	1,309

Notes: Data used are matched social security-education registry data 2019 to 2023. The dependent variable is the log of the average monthly wage; coefficients are expressed in percent. The specification is the control function model of Table 3, column (6), with the dual dummy interacted with the quartile of parental income; all four resulting endogenous terms are instrumented. The first row reports the effect for graduates from the lowest parental income quartile, the following rows the increment for each higher quartile. The joint test is a χ^2 test with three degrees of freedom of the hypothesis that all three interaction terms are zero. Standard errors, clustered on the school level, are reported in parentheses. Significance on 0.1, 0.05, and 0.01 is indicated as *, **, ***.

widen the earnings gap inherited from the family, even though selection into the track is positive on parental income (Table 1). This is a modest but useful result for the mobility debate: dual education does not by itself reduce inequality of opportunity. Even if access is unequal, it does not amplify the dual gains.

6.5 Continuing in education

Lüthi (2025) shows that in Switzerland, graduates from school-based vocational education are more likely to continue with further education. In our sample of H- and K-type graduates, about one in three continues in post-secondary education, either at ISCED level 4 or directly at ISCED level 5 (Table 1), which is close to the 34% that national graduate statistics report for these programmes.[31] There is no significant difference between dualists and non-dualists (Table 7). The CF estimate for the first year is -0.006 with a standard error of 0.054, and neither the OLS nor the CF estimate is significant in any of the four years, with the exception of a negative OLS

[31] Including the four-year *maturita* programmes without an occupational certificate (M-type), which our sample excludes, the national share is 47% (Inštitút sociálnej politiky, 2026).

coefficient in the third year, significant at the 5% level.

Table 7: Effect of dual vocational education on continuing in further education

	Enrolled in further education		Newly starting further education	
	OLS	CF	OLS	CF
	(1)	(2)	(3)	(4)
1st year	−0.012	−0.006		
	(0.015)	*(0.054)*		
Observations	18,047	18,047		
R^2	0.13	0.13		
2nd year	−0.015	0.023	−0.006	−0.018
	(0.019)	*(0.070)*	*(0.016)*	*(0.070)*
Observations	13,269	13,269	13,269	13,269
R^2	0.13	0.13	0.12	0.12
3rd year	−0.056**	−0.025	0.000	−0.074
	(0.025)	*(0.084)*	*(0.014)*	*(0.065)*
Observations	8,108	8,108	8,108	8,108
R^2	0.15	0.15	0.05	0.04
4th year	−0.031	0.056	0.005	−0.043
	(0.038)	*(0.129)*	*(0.020)*	*(0.058)*
Observations	3,883	3,883	3,883	3,883
R^2	0.15	0.14	0.09	0.08

Notes: Data used are matched social security-education registry data 2019 to 2023. In columns (1) and (2) the dependent variable is a dummy equal to one if the graduate is enrolled in further education in the year after graduation indicated by the row. In columns (3) and (4) the dependent variable is a dummy equal to one if the graduate *starts* further education in that year, that is, later than immediately after graduation; it is reported from the second year onwards, because in the first year it equals enrolment. The (CF) columns use the distance-based measure to instrument the dual education dummy. Control variables are as in Table 3, excluding the labour market variables. Standard errors, clustered on the school level, are reported in parentheses. Significance on 0.1, 0.05, and 0.01 is indicated as *, **, ***.

Because a null on enrolment could hide a difference in timing rather than in level, columns (3) and (4) look at newly starting further education in each year, which distinguishes graduates who postpone study from those who never take it up. Here too there is no systematic difference: none of the six estimates, OLS or control function, is statistically significant. Dual graduates earn more without either continuing in education at different rates or delaying entry into it.

7 Interpreting the dual advantage

The effects documented in Section 6 admit several interpretations. Table 8 lists channels through which the dual track could generate them in its rows and six facts from our estimates in its

columns, and marks whether each fact is consistent with each channel or weighs against it. We discuss the channels in turn, keeping in view that part of the measured advantage may reflect residual selection (Section 5.4).

The comparison of workplace-based with school-based graduates (Table 4 and Figure A6) is the most informative fact for this discussion. Any mechanism that operates purely through exposure to a workplace, its technology, its norms, or the informal contacts made on the shop floor should also have raised the wages of workplace-based graduates relative to school-based ones. Conditional on observables, it raised them by only about 2%. This weighs against exposure alone as the source of the dual premium, though, as Section 6.2 notes, it does not isolate the effect of the contract from the greater time at the firm and the larger, certified employer that come with it.

The most straightforward interpretation is skill formation: the dual track builds more, and more labour-market-relevant, human capital, because dualists spend at least half of their practical training inside a producing firm, close to the technology and routines that employers actually use (Eichhorst et al., 2015; Harhoff and Kane, 1997), and because the contract commits both sides to the training. Part of this human capital is general and part firm-specific (Becker, 1964), and under labour market frictions firms finance even general training (Acemoglu and Pischke, 1998, 1999), which leaves the worker with a portable productivity advantage. Skill formation predicts a persistent wage premium; it does not by itself predict the shrinking of the employment advantage.

A second interpretation is firm–worker matching. An apprenticeship is an extended two-sided audition, so that the post-apprenticeship match is formed under far less uncertainty than a cold-start hire (Jovanovic, 1979). Young workers normally learn where their productivity is highest by sampling jobs, which accounts for much of early wage growth (Johnson, 1978; Miller, 1984; Topel and Ward, 1992; Neal, 1999). The dual track moves part of this search into a structured, paid training relationship, so that dualists enter work faster and start out in better matches. Two facts fit this channel: the premium attenuates once firm characteristics enter the regression, so part of it operates through *where* dualists work; and certified training firms are a selected group of employers, concentrated among the largest firms (Section 2.2). Because the size of the first employer has lasting consequences for young workers' careers (Arellano-Bover, 2024), and dual systems improve the quality of the first job match (Oswald-Egg and Renold, 2021), this early

sorting persists.

The long-standing objection to both interpretations is retention, the possibility that the early advantage is mechanical: training firms retain their own apprentices, so dualists step straight into a job while their peers search (von Wachter and Bender, 2006; Fitzenberger et al., 2015; Albanese et al., 2021). Retention is the most natural explanation for the first-year employment advantage, but an unlikely one for the wage premium. The employment advantage shrinks while the premium does not, which a retention account, tied to tenure at the training firm, does not predict; and a firm that knows more about its apprentices than outside employers do can retain them at wages below their productivity (Acemoglu and Pischke, 1998). Studies that strip out retained apprentices still find a premium, at least for men (Bentolila et al., 2023; Bonnal et al., 2002), and we find one too: dropping the dual graduates whose first employer is a certified training firm lowers the premium by two to three percentage points, while it cuts the first-year employment advantage by a third to a half (Table 5). Retention is also low in Slovakia, 15% to 31% of dual graduates in the graduate tracing surveys, with no excess over non-dual graduates in the 2025 wave (ŠIOV, 2026).

Signalling is a fourth candidate. The dual credential may signal productivity, motivation, and workplace readiness at hiring (Spence, 1973), but under employer learning the return to an easily observed signal erodes as employers accumulate direct information about a worker's productivity (Altonji and Pierret, 2001). A premium that remains stable over the first three years is hard to reconcile with pure signalling, unless employers learn slowly, and fits better with skill formation and match quality.

Networks and workplace socialisation are a fifth. Time spent inside a firm gives access to referrals and to the weak ties that ease labour market entry (Granovetter, 1973), and to workplace norms whose familiarity may help disadvantaged youth (Zisler et al., 2025). The surveys give both some substance: a third of dual students have a family member who knows their workplace instructor and a quarter report being treated as a full employee (Table A5). Workplace-based students, however, also spend part of their training in firms, though typically less of it and without a legal minimum, and they earn only about 2% more than school-based graduates. A pure network or socialisation story therefore carries little of the premium, unless what matters is the employee-like status and the training intensity that the contract brings rather than presence in a workplace as

such.

Two interpretations remain. The absence of any difference in continuing education argues against one in which the dual track mainly reshapes educational trajectories or option values, as school-based training does in Switzerland (Lüthi, 2025). The other is residual selection: part of the pattern could still reflect selection on characteristics that the control function does not absorb, above all unobserved motivation and family connections to training firms. Section 5.4 discusses this threat, and the fact that bears on it here is the flat profile across parental income quartiles (Table 6), which a connections-driven story would not predict.

Table 8 confronts each channel with the six facts. Matching and skill formation are the only channels that no fact weighs against, and residual selection is weakened, though not eliminated, by the flat profile across parental income. Our results therefore point to occupation-specific skill formation and better firm–worker matching operating through the dual arrangement, rather than to retention, workplace exposure, or signalling alone.

Table 8: Mechanisms and the six facts

	Empirical fact					
Channel	(1) Employment advantage shrinks	(2) Wage premium persists	(3) Firm controls attenuate	(4) Small workplace-based gain	(5) No effect on further education	(6) Flat across parental income
Skill formation	·	✓	✓	✓[a]	·	·
Matching and experimentation	✓	✓	✓	✓	·	·
Retention by the training firm[b]	✓	×	·	·	·	·
Signalling	✓	×	·	✓	·	·
Networks and socialisation	✓	·	✓	×	·	·
Changed educational pathways	·	·	·	·	×	·
Residual selection	·	✓	·	·	·	×

Notes: The six facts are: (1) the employment advantage of about one month in the first year shrinks afterwards, by more than half in OLS and to zero in the control function (Table 3); (2) the wage premium of 11% to 13% persists through the third year (Table 3); (3) the premium attenuates by two to four percentage points once the employment history and the size and sector of the employer are controlled for (Table 3); (4) workplace-based graduates earn only about 2% more than school-based graduates conditional on observables (Table 4); (5) there is no effect on continuation into further education, in level or timing (Table 7); (6) the premium is flat across parental income quartiles (Table 6). Cell entries: ✓ = the channel predicts or is consistent with the fact; × = the fact weighs against the channel; · = the fact is not informative about the channel. [a] Consistent only if the relevant skills require the employee-like role and the training intensity of the dual track rather than mere presence in a firm, which is what fact (4) suggests. [b] The retention channel is additionally bounded by its low incidence, between 15% and 31% of dual graduates according to the graduate tracing surveys (Section 2.3), and by Table 5: excluding dual graduates who start at a certified training firm cuts the first-year employment advantage by a third to a half but lowers the premium by only two to three percentage points.

8 Conclusion

This paper presents evidence on how the organisation of practical training in vocational education shapes early labour market outcomes, exploiting the introduction of a dual vocational track in Slovak upper secondary vocational education. Graduates of the dual track accumulate more employment days and earn higher wages in the first year after graduation than graduates of the school-based and workplace-based tracks, and their wage premium of 11% to 13% persists into the third year while the employment advantage shrinks. The persistence of the wage effect suggests that the advantage does not reflect short-lived transition effects alone, and it is more consistent with enhanced occupation-specific skill formation and improved firm–worker matching than with retention by the training firm.

The decomposition of the comparison group sharpens this interpretation and carries, in our view, the clearest policy content. Workplace-based graduates earn only about 2% more than school-based graduates, even though the former spend part of their practical training in firms. Exposure to a workplace without the dual arrangement pays little. What pays is the dual arrangement as a whole: the individual contract with its pay and employee-like status, a guaranteed minimum share of training at the employer, and certified training firms that are typically large. For a country deciding how to organise practical training, this suggests that moving students into firms is not sufficient by itself. It also raises the question whether the workplace-based track has a rationale of its own, or whether its students would be better served by either of the two alternatives.

That question has a cost side. The dual track is publicly supported through direct payments and tax relief for training firms, while schools keep their per-student funding, whereas workplace-based training carries no comparable support. An early cost-benefit simulation of the Slovak dual system suggests that it pays off in the medium run (ÚHP and IVP, 2024), but it says little about deadweight: because certified firms are among the largest in the country, some of them would plausibly have trained young workers anyway. Our estimates speak to the returns for students, not to the net returns for public budgets.

Several limitations point to further research. The most important is retention at the training firm, which we can only proxy, because the identity of the training firm is not in our data. The premium survives the exclusion of dual graduates who start at a certified training firm, which cuts the first-year employment advantage by a third to a half, but linking apprenticeship contracts to subsequent employment records would allow a direct decomposition of the premium by retention status and a test of the intergenerational threat to our instrument. Separating the components of the dual arrangement, the contract, the intensity of workplace training, and the quality of the training firm, would require independent variation in each, which the Slovak reform does not provide. Finally, our fourth-year results rest on a single cohort, and longer panels will show whether the wage premium persists beyond the early career.

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Online Appendix to
"Dual Reform of Vocational Education and Early Labour Market Outcomes"

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November 2026

This Online Appendix accompanies the paper and is intended for online publication only. Tables and figures whose numbers begin with A are in this appendix; all other tables, figures and sections referred to here are in the paper.

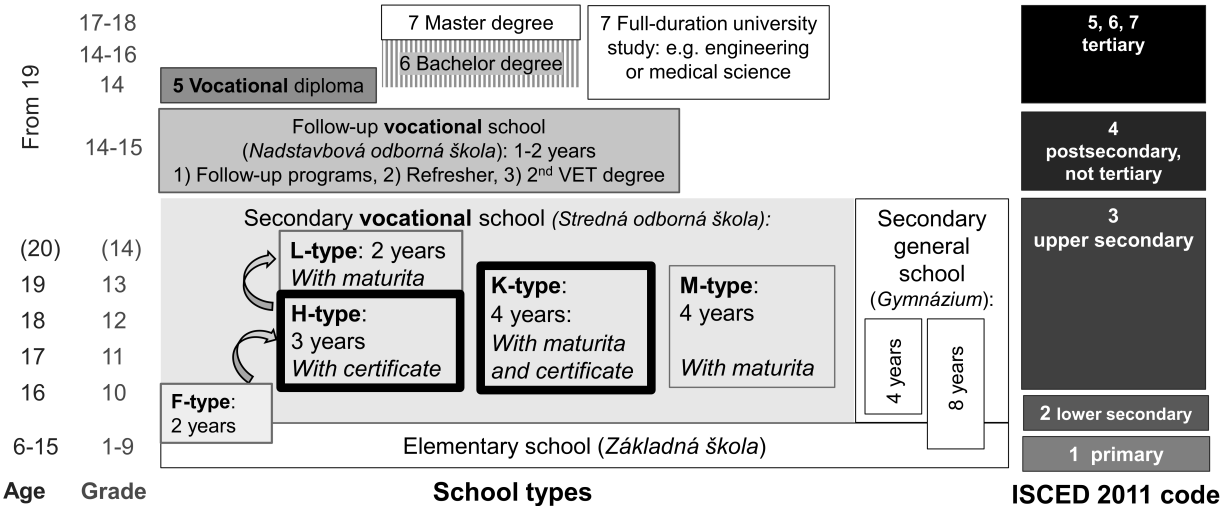


Figure A1: Vocational schools within Slovak education system

Notes: Shades of grey-filled boxes indicate vocational type of education. We bold-outline vocational school types where the dual policy reform mostly applies (H-type and K-type). Only graduates from these two types of vocational schools are entering our analysis. From the figure we exclude schools for children with special needs and vocational schools with music, art, or sport specializations. We indicate arrows of upward progression only for the F-type and the H-type graduates. Anyone who achieved *maturita* at the secondary vocational school, or graduates of *Gymnázium*s (i. e. graduates with a general *maturita*) is eligible to continue with further education on any of the ISCED 4, 5 and 6 levels. All 4-year secondary schools with *maturita* take +1 more year if they are bilingual (not depicted).

Source: Own depiction based on ÚHP and IVP (2024) and National Qualification of Education (https://www.minedu.sk/data/files/3772.pdf, last accessed in November 2026).

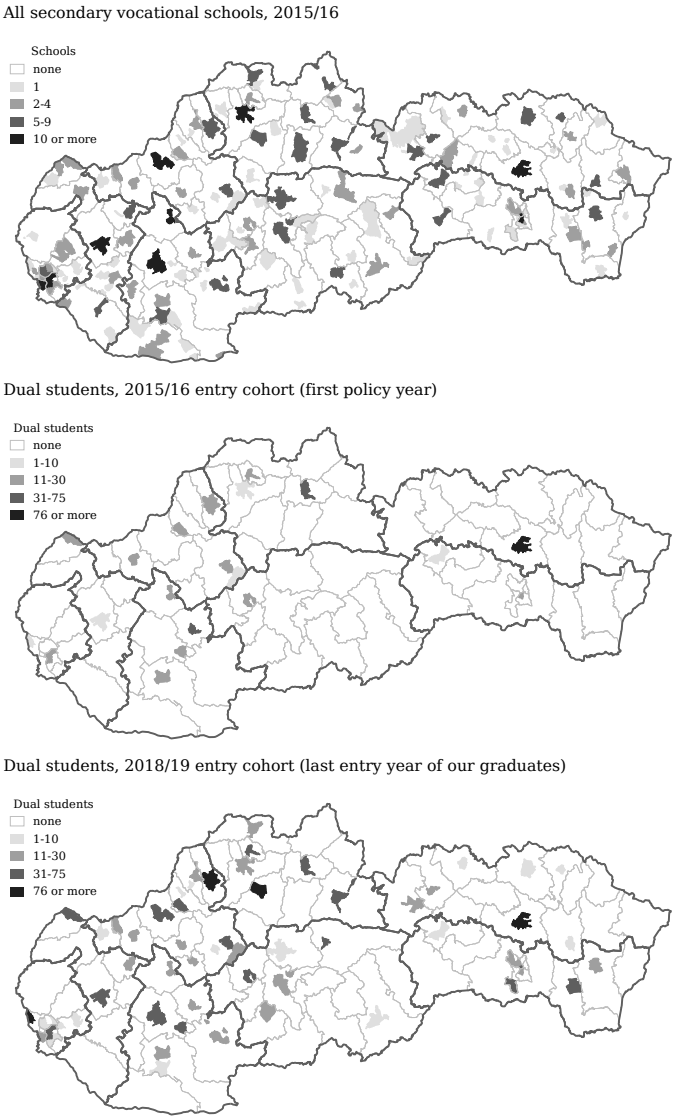


Figure A2: Vocational schools and dual places by municipality

Notes: The upper panel shades each municipality by the number of secondary vocational schools in operation in 2015/16 (1; 2 to 4; 5 to 9; 10 or more), from the 2015 school statistics of the Slovak Centre of Scientific and Technical Information: 458 schools in 150 municipalities. Gymnáziá, conservatories and special schools are excluded, and each part of a joint school (*spojená škola*) counts as a separate school. The two lower panels shade municipalities by the number of newly enrolled dual students at their schools in the 2015/16 entry cohort, the first policy year, and in the 2018/19 entry cohort, the last year in which the graduates we observe entered vocational school (1 to 10; 11 to 30; 31 to 75; 76 or more). Enrolment counts come from the Education Policy Institute (data as of 15 September of each year; for 2015/16, from the second- and third-year enrolment on 15 September 2017); they measure realised dual places, which can fall short of the places that schools planned to offer. Of the 2,927 municipalities, 20 had dual enrolment in 2015/16 and 60 in 2018/19. Thin grey lines are the borders of the 79 districts (*okresy*), the scale at which a fifteen-year-old chooses a school; thicker lines are the borders of the eight regions (*kraje*); municipality borders are not drawn.

Table A1: Comparison of apprenticeships in Germanic countries with Slovak dual VET system

	Austria	Germany	Switzerland	Slovakia
Typical age at entry	15	16	15	15
Typical number of years of pre-VET education	9	10	9	9
Study length	2-4 years	2-3.5 years	3-4 years	3-4 years
Share of VET students in all upper secondary	69%	47%	61%	68%
Share of apprentices (dualists) in VET	45%	88%	80%	20%
Number of available occupations	230	320	240	270
Days per week in training firms	4	3-4	3-4	2.5-3
Average monthly gross pay: 1st year	650 Eur	900 Eur	724 Francs	300 Eur
Average monthly gross pay: 3rd year	1100 Eur	1300 Eur	1300 Francs	300 Eur
Share of retained	70%	72%	46%	15-30%
Share of training firms	14.3%	19.4%	18%	0.8%

Sources: Austria: Dornmayr (2023), Schmid and Petanovitsch (2025), Cedefop and ibw Austria – Research & Development in VET (2023), https://www.ams.at/arbeitsuchende/topicliste/lehrlingsausbildung; Germany: Muehlemann et al. (2010), Bundesinstitut für Berufsbildung (2022); Switzerland: Statistik der beruflichen Grundbildung (SBG), Gehret et al. (2025), Lüthi (2025); Slovakia: Own data, ÚHP and IVP (2024), https://www.minedu.sk/ziak-v-systeme-dualneho-vzdelavania/ (in Slovak, last accessed in November 2026). The numerical information for all countries varies between 2021–2025 due to variety of sources. The share of VET students/apprentices is based for all countries on OECD (2025) (Table B3.2.). For Slovakia, the share of apprentices refers to the OECD category of combined school- and work-based programmes, in which between 25% and 90% of the curriculum is delivered in the workplace; it therefore includes practical training at an employer outside the dual system, such as the workplace-based track, and exceeds the dual share of 7.4% reported in Section 2.1. The Slovak share of retained dualists refers to the share of dual graduates subsequently employed by their training firm, as reported by graduates themselves in the national graduate tracing survey. It should be read as an order of magnitude rather than a point estimate: the 2025 wave gives 15% on 105 dual respondents and the 2024 pilot wave 31% on 238, and both reports admit that they are not representative. Employer surveys use different denominators and place the figure somewhat higher. Section 2.3 discusses the resulting range. The three comparator countries are recorded on their own national definitions and are not harmonised with the Slovak figure.

Table A2: Firms certified to train dualists

	Number of firms	Share within panel
Firm size		
0–9 employees	501	29.7%
10–49 employees	560	33.2%
50–249 employees	369	21.9%
250 employees and more	216	12.8%
Size unknown	42	2.5%
Five largest sectors		
Tourism and gastronomy	290	17.2%
Sales and maintenance of vehicles	165	9.8%
Metalworking and metallurgy	149	8.8%
Construction	147	8.7%
Law, consulting and accounting	121	7.2%
Other sectors (26 categories)	816	48.3%
Region		
Bratislavský	265	15.7%
Prešovský	257	15.2%
Košický	243	14.4%
Trenčiansky	227	13.4%
Žilinský	204	12.1%
Nitriansky	183	10.8%
Trnavský	178	10.5%
Banskobystrický	130	7.7%
Legal form		
Limited liability company	1,404	83.2%
Joint stock company	227	13.4%
Other legal forms	57	3.4%
Financial health		
FinStat score: high prosperity	660	39.1%
FinStat score: average prosperity	951	56.3%
FinStat score: low prosperity	63	3.7%
Altman Z-score: low prosperity zone	296	17.5%
In bankruptcy or restructuring proceedings	36	2.1%
Recorded tax or social security arrears	163	9.7%
Payment orders on record	237	14.0%

Notes: The 2023 register of employers certified to provide practical training in the dual system (RZOVP) lists 2,225 unique employers by their identification number (IČO); of these, 1,688 could be matched to 2023 company accounts from FinStat (https://finstat.sk), a commercial database of Slovak company data based on the public register of financial statements. Unmatched entries are mostly sole traders and public or non-profit institutions, which do not file company accounts. Sectors are the 31 industry categories of FinStat's own classification, which do not correspond one-to-one to NACE sections. Sector labels, size classes, the FinStat prosperity score, and the Altman Z-score classification are as constructed by FinStat; shares are based on the 1,688 matched firms, with missing categories omitted, so panels need not sum to exactly 100%. The register is a snapshot, so firms whose certification lapsed before 2023 are not covered, and the register records the certification to train, not whether a firm trained a dual student in a given year or not.

Table A3: Employers certified to train and all other Slovak firms

	Certified to train dualists	All other Slovak firms	Certified share of the size class
0–9 employees	501	195,085	0.3%
10–49 employees	561	12,240	4.4%
50–249 employees	370	2,253	14.1%
250–999 employees	156	385	28.8%
1,000 employees and more	61	43	58.7%
All firms with a known size	1,649	210,006	0.8%
Firms reporting zero value added	0.06%	0.82%	

Notes: Firms certified to provide practical training in the dual system, compared with every other firm in the Slovak enterprise register, within employee size class. Counts are as reported in the underlying comparison, which covers 1,691 certified employers and 284,329 other firms; the 42 certified and 74,323 other firms whose size is not recorded are excluded from the size rows but included in the zero-added-value row. The counts differ by three from Table A2 because the two extracts were drawn on slightly different dates. The *certified* share uses all firms of the size class as the denominator, including firms that report no employees. With only firms that have employees in the denominator, as in Austrian statistics, the share among firms with one to nine employees is 0.4% rather than 0.3%. The last row reports the share of firms in each group whose value added is zero. Value added is not observed for firms that do not file company accounts, chiefly sole traders or public and non-profit bodies.

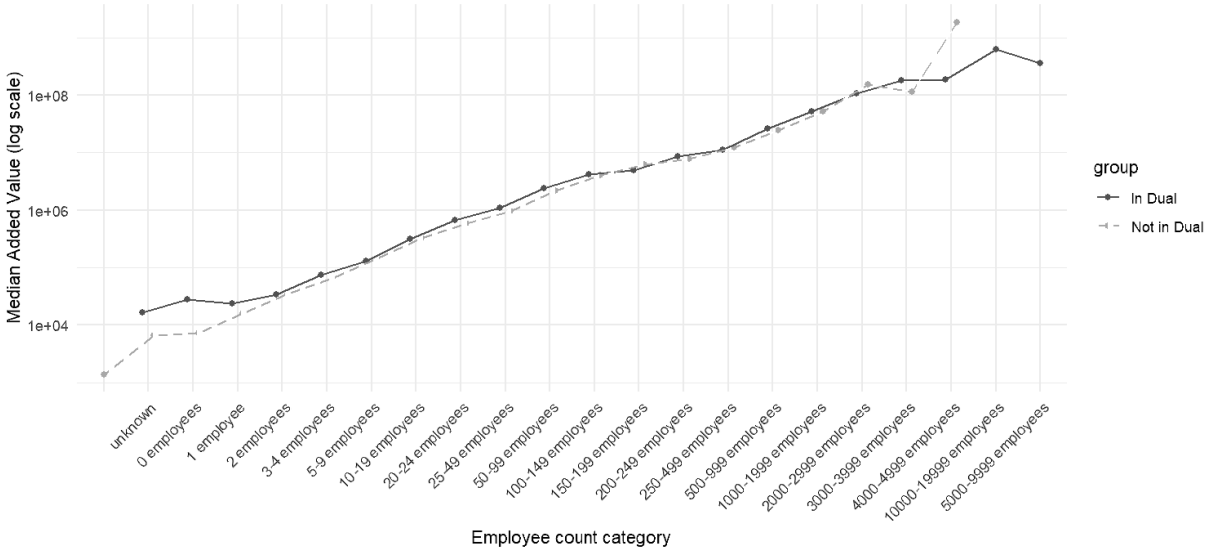


Figure A3: Median value added by firm size, dual training firms and all other firms

Notes: Median value added in Euro, on a logarithmic scale, by employee size class, for firms certified to train dual students (black, solid) and for all other Slovak firms (grey, dashed). Firms with zero value added are excluded. Among firms with at most one employee the median training firm reports clearly higher value added; from two employees upwards the two medians are close, and the sign of their difference changes across size classes. Above two thousand employees at least one of the two groups has fewer than ten firms in each class, so the medians are noisy. We use the figure only qualitatively: it reproduces, in greyscale, the chart of the underlying comparison of FinStat company accounts, whose medians are not available in tabular form.

Table A4: Training firms: regions and leading sectors

Region	Training firms	Sectors represented	Share in the top three	Leading sector
Banskobystrický	130	27	35%	Law and consulting
Bratislavský	265	29	42%	Tourism and gastronomy
Košický	243	30	44%	Tourism and gastronomy
Nitriansky	183	24	39%	Construction
Prešovský	258	26	47%	Tourism and gastronomy
Trenčiansky	227	24	34%	Metalworking
Trnavský	179	26	44%	Tourism and gastronomy
Žilinský	205	26	34%	Vehicle sales and repair
How thin the place-by-sector grid is				
Region × sector cells with no training firm	15% of 248 cells (8 regions × 31 sectors)			
cells with one or two firms only	30%			
District × sector cells with no training firm	69% of 2,387 cells (77 districts × 31 sectors)			
cells with one or two firms only	24%			

Notes: The 1,691 employers certified to provide practical training in the dual system, cross-tabulated by region and by district against the 31 sectors of the FinStat industry classification; one firm with no recorded region is omitted from the regional rows. "Sectors represented" counts how many of the 31 sectors contain at least one certified employer in that region. "Share in the top three" is the proportion of the region's training firms in its three largest sectors. The lower panel reports how many place-by-sector cells are empty. Firm sectors are only a proxy for the study fields that define our instrument, but the panel conveys why it has bite: because the instrument compares the closest vocational school with the closest school with dual places *in the student's own field*, what generates variation is the sparseness of the grid rather than the overall number of training firms. At district level, which is the scale at which a fifteen-year-old actually chooses, roughly seven in ten sector-by-place combinations contain no certified employer at all.

Table A5: What students, parents and training firms report about the dual system

Panel A: feedback surveys

	Reference year of the survey		
	2018	2020	2022
Students in the dual track (number of respondents)	(424)	(1,149)	(142)
Considers dual study good preparation for working life	92%	98%	
Would recommend the dual track to other students	84%		91%
Own expectations of the dual track were met			73%
Training contract signed before the start of studies	76%		
Holds a pre-contract on future employment	40%	46%	65%
Plans to work for the training firm after graduation		44%	
Feels part of the team, own work is meaningful	30%		
Treated by others as a full employee	23%		
Feels given menial work	14%		
Offered further training at the firm beyond practical instruction	17%		
Somebody in the family knows the workplace instructor	33%		
Entered the dual track to acquire skills needed later		62%	
Entered the dual track for the stipend and other benefits		44%	
Entered the dual track to secure a job after school		15%	
Parents of dual students (number of respondents)			(73)
Expects the child to work for the training firm			49%
Training firms (number of respondents)	(41)	(158)	
Would recommend joining the dual system to other employers	88%	97%	
Signs a pre-contract on future employment with students	32%	15%	
Share of own dual graduates offered a job	82%	77%	
Share of own dual graduates who started with the firm	28%	44%	

Panel B: graduate tracing survey, about 15 months after leaving school

	2024 wave	2025 wave
Dual contract holders employed by the training firm	31%	15%
Number of dual contract holders	(238)	(105)
Non-dual graduates employed by their practice firm		same share

Notes: Panel A: the 2018 and 2020 columns are feedback surveys commissioned by the State Vocational Education Institute (ŠIOV) under the national project *Duálne vzdelávanie a zvýšenie atraktivity a kvality OVP* and carried out by Centire and TRANSFER Slovensko; that series covers reference years 2018 to 2020 and ended with the project in September 2021. The 2022 column comes from the external evaluation of the operational programme, fielded in February and March 2022, which is the only wave to survey parents; students and parents in that wave were reached through fifteen selected schools, so those figures are illustrative rather than national. Numbers of respondents are in parentheses, and blank cells indicate items not fielded in that wave. The 2020 recommendation figure of training firms refers to firms that joined the system recently. The training-firm rows on offers and take-up are computed by the survey authors from the reported number of dual graduates, the number offered a job, and the number who started: in the 2020 wave, 445 of 581 graduates received an offer and 257 started. Panel B: the graduate tracing survey follows vocational school leavers about fifteen months after graduation (ŠIOV, 2025, 2026); the 2025 wave reports no difference between dual and non-dual graduates in the share employed by the firm of their practical training. All surveys are self-reported and none is representative in the sampling sense; the tracing reports say so explicitly. We use them descriptively, to document how the participants describe the arrangement and to inform the interpretation of the mechanisms, not as evidence of effects.

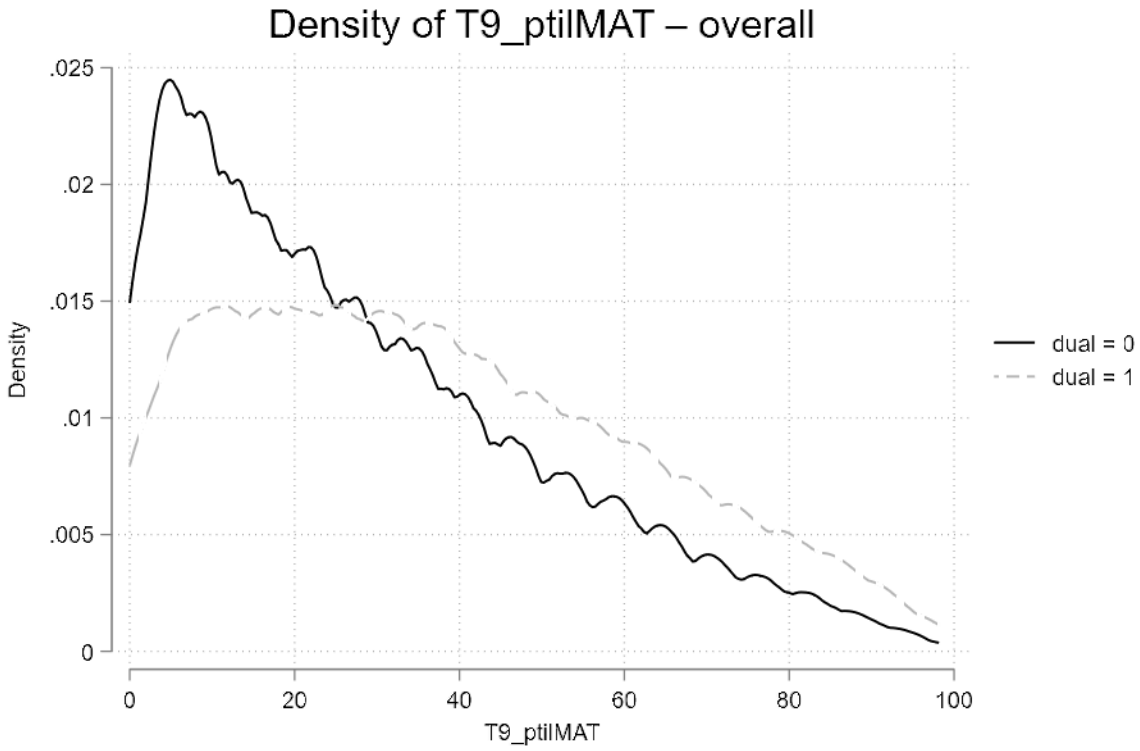


Figure A4: Distribution of pre-treatment ability among vocational school graduates

Notes: Density in grade percentiles of *Testovanie 9* in mathematics of vocational school graduates between 2019–2022. Grey dashed are graduates from the dual vocational track, black solid are graduates from the school-based and workplace-based vocational tracks. The standardised mean difference between the two groups is 0.38 (Table 1).

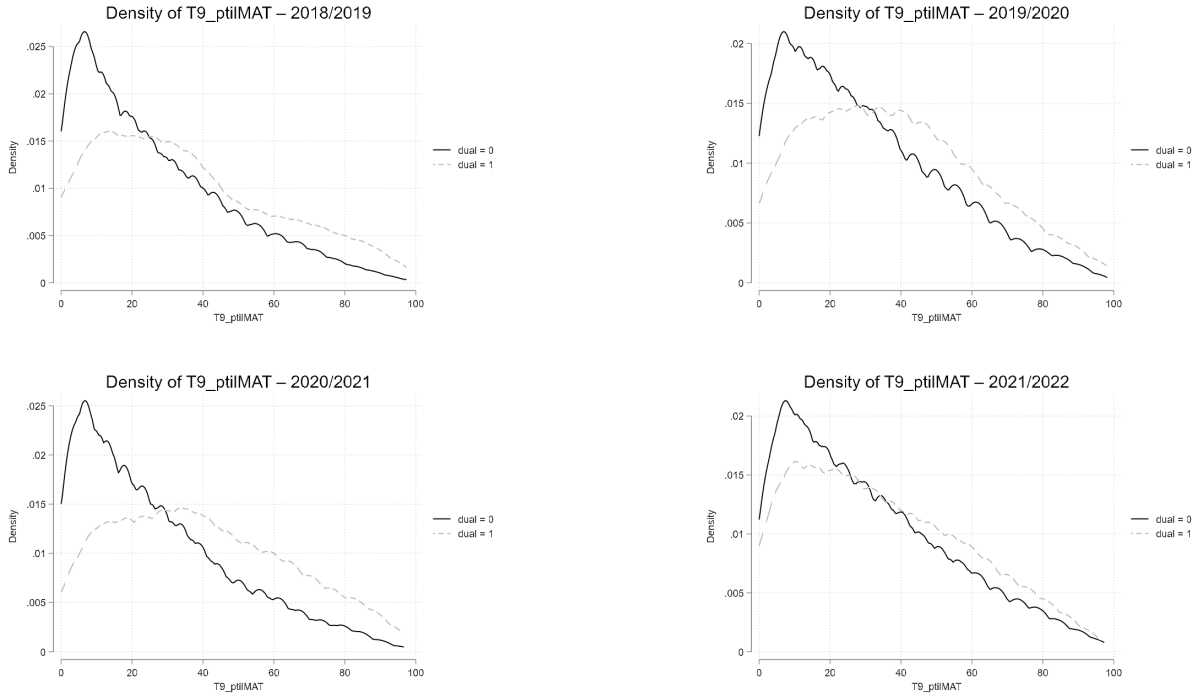


Figure A5: Distribution of pre-treatment ability by graduation cohort

Notes: Density in grade percentiles of *Testovanie 9* in mathematics of vocational school graduates, separately for the 2018/19, 2019/20, 2020/21 and 2021/22 graduation cohorts (panels from top left to bottom right). Grey dashed are graduates from the dual vocational track, black solid are graduates from the school-based and workplace-based vocational tracks. The panels give the cohort-by-cohort counterpart of Figure A4.

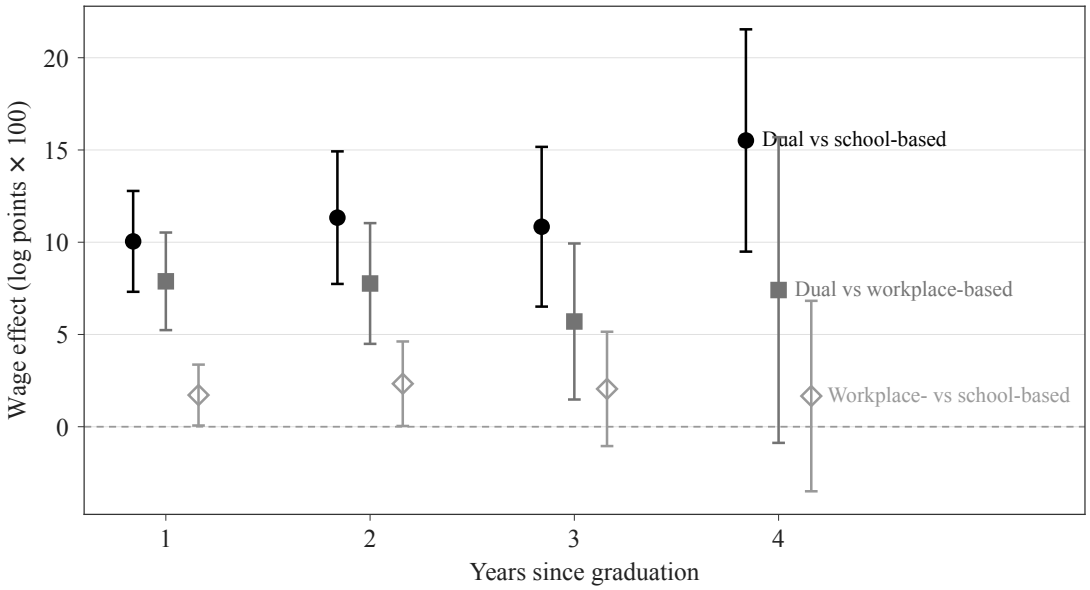


Figure A6: Wage effects across the three track comparisons

Notes: OLS estimates of the wage effect in specification W1 of Table 3, from the pairwise track comparisons of Table 4: dual versus school-based graduates, dual versus workplace-based graduates, and workplace-based versus school-based graduates. Whiskers are 95% confidence intervals based on standard errors clustered on the school level. The OLS estimates are plotted so that the three contrasts are directly comparable; the control function estimates for the two comparisons involving dual graduates are reported in Table 4.

Table A6: Main features of the literature on labour market effects of apprenticeship using IV approach

Dataset: sample, country, year, N	Treatment [control group]	Instrumental variable	Outcomes and main effects OLS [IV]
Fersterer et al. (2008) **Austrian** Social Security Data; males apprentices trained in small firms (<10 workers); 1975-1998, $N = 47,881$	Effect identification via: Length of apprenticeship	Firm failed during apprenticeship	Wage return of additional apprenticeship year is $\sim 5\%$ [IV wage returns of similar magnitude]
Gunderson and Krashinsky (2015) **Canadian** census of 2006; excluding: self-employed, individuals on transfer payment, younger than 18, older than 60, individuals still in school, part-time employees $N_{\text{men}} = 2,983,355$; $N_{\text{women}} = 2,160,065$	Workers with a registered apprenticeship certificate [workers with no degree, high-school diploma, trade certificate, or community college]	Fraction of certified journeypersons (can supervise apprentices) to all workers at least five years older than census respondent in each provincial labour market	For men: wage returns to being an apprentice compared to the alternative pathways combined $\sim 8\%$ [IV $\sim 10\%$] For women: negative wage returns for apprenticeship compared to the alternative pathways combined $\sim -5\%$ [same for IV]
Parey (2016) Employment histories for **German** employees (IABS); only West German males without *Abitur* and any tertiary degree 1964-1975, $N = 242,014$	Individuals with vocational degree incl. apprenticeship [degree from full-time vocational school]	Availability of apprenticeships at age 16 normalised by the number of young people in 141 regional markets	Reduction in unemployment [IV effect 3 times larger than OLS]; increase in wages [IV effect of similar magnitude, but insignificant]

Table A6: Main features of the literature on labour market effects of apprenticeship using IV approach, continued

Dataset: sample country, year, N	Treatment [control group]	Instrumental variable	Outcomes and main effects: OLS [IV]
Brébion (2017) **France**: Génération survey: representative survey of students in years 1998, 2001, 2004 and 2007, $N = 99,861$ (cumulatively in all years) **Germany**: German Socio-Economic Panel (SOEP), all students leaving school in years 1992-2013, $N = 3,489$	Apprentices [full-time students], separately at secondary and higher education levels	Regional rate of apprenticeship as number of apprentices in secondary (resp. higher) education relatively to the number of full time vocational students at the same level, three years before the school exit	Reduction in unemployment [IV effect for France comparable, for Germany insignificant] Smaller first wage (Germany), slightly larger first wage (France) [IV: no effect on first wage in both countries]
Oswald-Egg and Renold (2021) **Swiss** graduate survey of all graduates from formal institutions of higher education in years 2011, 2013, 2015 and 2017, only graduates from fields in which work experience or an internship is not required, younger than 30 $N = 13,490$	Higher education graduates with previous vocational work experience [higher education graduates with academic diploma only]	Vocational education enrollment rates of the higher education graduates' pre-study region of origin at the time when they had to decide their upper secondary education pathway	Significantly larger wage [IV: significantly lower wage]
Bentolila et al. (2023) Academic records of all students enrolled in tertiary VET in **Madrid (Spain)** between 2011 and 2016, who graduated over 2013 to 2016 and were younger than 30, matched with longitudinal social security records with their employment histories, incl. only fields offering dual track with a minimum of 5 graduates in both the dual and school-based tracks; $N = 7,266$	Dual vocational graduates [school-based VET graduates] in the same field and year	Difference in travel time between lower secondary school and two nearest VET schools offering dual and school-based tracks in their field of choice	Positive effect on days of work, larger earnings, small effect on job quality indicators [IV effects stronger for days of work and earnings, no effect on job-quality]

Notes: Papers are ordered chronologically. N indicates the largest sample size used in the paper for effect identification. The table is not an exhaustive list of the literature on *returns to apprenticeship* that uses instrumental variables; it illustrates the breadth of instruments used to address selection into apprenticeship.

Table A7: Balance of pre-determined characteristics with respect to the instrument

	First year, first-stage controls		First year,	Third year,
	per 10 minutes	in SD	test year only	first-stage controls
	(1)	(2)	(3)	(4)
Testovanie 9, mathematics percentile	−0.009 [0.16]	−0.0004	−0.623 [8.39]	−0.054 [0.65]
Age (years)	0.0022 [1.09]	0.0028	−0.0100 [4.25]	0.0030 [1.39]
Female	0.0012 [1.07]	0.0031	0.0081 [4.25]	0.0001 [0.08]
Disability	−0.0002 [0.25]	−0.0007	−0.0003 [0.32]	−0.0018 [1.48]
Child under six	0.0005 [0.88]	0.0026	0.0031 [4.32]	−0.0007 [0.84]
Parental education (four categories)	0.0016 [0.74]	0.0020	−0.0164 [6.27]	0.0019 [0.71]
Self-employed parent	0.0009 [1.05]	0.0026	0.0002 [0.23]	0.0003 [0.23]
Parental annual wage (euro)	−147.4 [5.60]	−0.0116	−394.8 [10.28]	−153.0 [4.89]
Memo: P(dual), LPM (pp)	−1.35 [8.40]	−0.0356		−1.20 [6.67]
Memo: P(dual), probit AME (pp)	−3.48 [6.35]	−0.0918		−2.87 [5.07]

Notes: Each entry is the coefficient on the instrument, scaled to ten additional minutes of travel time to the closest school with dual places in the student's field, from an OLS regression of the characteristic on the instrument; absolute t statistics based on standard errors clustered on the school level are in brackets. Columns (1), (2) and (4) include the controls of the probit first stage other than the characteristic itself; for the three parental characteristics, all parental controls are left out. Column (3) includes only the year of the grade-nine test. Column (2) expresses column (1) in standard deviations of the characteristic. For comparison, the memo rows report the first stage in the pooled employment sample: the change in the probability of graduating from the dual track, in percentage points, from the linear probability model and as the average marginal effect of the probit; column (2) expresses it in standard deviations of the dual indicator (0.379, with 17% dualists). The first-year samples have 11,298 graduates (12,404 for the parental wage and self-employment, 11,336 for parental education) and the third-year samples 4,625 (5,061). Parental wage is the annual wage of the parents in euro.

Table A8: Placebo: the instrument and the outcomes of non-dual graduates

	Years since graduation		
	1	2	3
Panel A: annual employment days, per 10 minutes of travel time			
All non-dual graduates	−0.70**	−0.64**	0.06
	(0.29)	*(0.29)*	*(0.41)*
Observations	9,280	6,806	3,991
Cells with dual take-up below 10%	−0.79**	−0.64**	0.07
	(0.31)	*(0.32)*	*(0.45)*
Observations	7,429	5,748	3,415
Panel B: log average monthly wage, specification W1, per 10 minutes (in %)			
All non-dual graduates	−0.01	−0.07	−0.02
	(0.07)	*(0.08)*	*(0.14)*
Observations	6,461	4,660	2,612
Cells with dual take-up below 10%	−0.03	−0.06	−0.03
	(0.08)	*(0.09)*	*(0.15)*
Observations	5,041	3,860	2,184

Notes: Coefficient on the instrument, scaled to ten additional minutes of travel time to the closest school with dual places in the student's field, from OLS regressions of the outcome on the instrument, estimated on graduates who did not enter the dual track, that is, graduates of the school-based and workplace-based tracks. The regressions include the controls of Table 3, with fixed effects for region, study field, programme type, year of the grade-nine test and graduation cohort; the wage regressions use specification W1, as in the rest of the paper. A cell is a school, study field, programme type and entry cohort; the second row of each panel keeps the cells in which fewer than 10% of students were dualists, where spillovers from dual classmates can play little role. Standard errors, clustered on the school level, are reported in parentheses. Significance on 0.1, 0.05, and 0.01 is indicated as *, **, ***.

Table A9: Robustness: employment days in logs and district fixed effects

	Years since graduation			
	1	2	3	4
Panel A: log of one plus annual employment days, dual vs all other tracks				
OLS	0.229***	0.066*	0.040	−0.046
	(0.033)	*(0.035)*	*(0.038)*	*(0.057)*
Observations	11,258	7,992	4,610	1,704
CF	0.233**	−0.025	0.011	0.086
	(0.101)	*(0.133)*	*(0.143)*	*(0.179)*
Observations	11,298	8,018	4,625	2,017
Panel B: district instead of region fixed effects, control function				
Dual vs all other tracks, employment days	15.6	2.0	17.1	
	(12.19)	*(12.86)*	*(17.87)*	
Dual vs all other tracks, log wage W1 (in %)	10.0***	9.0**	15.5***	
	(2.74)	*(3.85)*	*(4.48)*	
Dual vs school-based, employment days	12.1	−2.2	12.8	
	(10.76)	*(12.47)*	*(17.97)*	
Dual vs school-based, log wage W1 (in %)	10.9***	10.5***	16.1***	
	(2.83)	*(3.80)*	*(4.20)*	

Notes: Panel A: the dependent variable is the log of one plus annual employment days; specification and samples as in Table 3, columns (1) and (2). Panel B: control function estimates in which the eight region fixed effects are replaced by fixed effects for the 79 districts; the estimates with region fixed effects are in Table 3 and Table 4, and the samples are the same. The district estimates cover the first three years; the fourth rests on a single graduation cohort. With district fixed effects the F statistic of the excluded instrument in the linear first stage lies between 30 and 68. Standard errors, clustered on the school level, are reported in parentheses. Significance on 0.1, 0.05, and 0.01 is indicated as *, **, ***.