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Abstract

Workplace-based training has been praised for its effectiveness in smoothing the school to work transition. Apprentices have been shown to have lower initial unemployment probabilities as compared to other secondary-school graduates. There are but a handful of studies that can convincingly show that the effect of apprenticeship training on labor market outcomes is causal. This study provides additional support for the argument that workplace-based practical training increases initial employment probabilities. Using a unique individual panel database which includes, among others, extensive controls for individual skills, school attainment and parental background, it is shown that Hungarian students in the lowest, non-college bound vocational training track have about a 10-15% higher probability of employment after leaving school, as opposed to graduates of the same track, who carried out their practical training within the school. This effect seems to be stable across industries. The data also shows that apprentices, when employed, earn the same amount of money, but are more likely to receive long-term contracts compared to non-apprentices. Moreover, apprentices who move to another industry, are less likely to receive long-term contracts compared to "stayers", but are more likely to receive long term contracts compared to non-apprentices. These results suggests that it is not the increased specific skills of apprentices, but rather the increased screening and maybe the signaling effect of apprenticeship training that smoothes the school to work transition.

Keywords

School to work transition, apprenticeship training, causal inference, unemployment, vocational training.

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Daniel Horn
Max Weber Fellow, 2012-2013

Introduction

Workplace-based training has long been praised for its effectiveness in preparing non-college bound youth for the labor market. In particular the "dual" vocational education and training (VET) systems at the secondary level, combining school-based vocational education with employer-provided, workplace-based (apprentice) training, have sustained a positive track record in smoothing the school to work transition process, lowering the unemployment rate, and increasing the quality of work (Rosenbaum et al. 1990; Müller and Shavit 1998; Shavit and Müller 2000; Ryan 2001; Breen 2005; Wolbers 2007; Wolter and Ryan 2011; Noelke and Horn 2011; Piopiunik and Ryan 2012). Nevertheless, existing empirical research provides little information about the causal mechanisms that make the mixed school and workplace-based education effective. In particular, the mechanisms that explain why apprentices find their first job more quickly than non-apprentices are empirically not well tested.

This paper improves on existing literature in two ways. It adds empirical support to the positive causal link between workplace-based training and early labor market outcomes, and provides tests on the potential reasons why apprenticeship training leads to smoother school to work transition. Note that this study looks at the supply side of the market, rather than the demand side. The question is thus not why firms provide apprenticeship training, but whether apprentices are better off, and if yes, why?

Causal relation

There are at least four problems in the way of determining the causal effects of apprenticeship training on the individual level labor market outcomes (see Wolter and Ryan 2011). 1) It is hard to implement the counterfactual. What are the foregone choices for students entering apprenticeship training? Which group of students/workers would be the "control group"? While this question is inherently an empirical one, many previous studies were able to consider only the differences between the various school tracks (e.g. Breen 2005; Wolbers 2007; Rosenbaum et al. 1990; Müller and Shavit 1998; Shavit and Müller 2000). This comparison, however, is problematic, since 2) the allocation of young people to upper-secondary programs is not random (see e.g. Bertschy, Cattaneo, and Wolter 2009), which increases the probability of omitted variable bias, and thus makes estimations unreliable. The problem is not only that students are selected or self-selected into the different programs (tracks), but also that curricular or quality differences between school programs make it hard to establish whether the different types of schools or the differences in school-based vs. workplace-based training drives the results. Moreover, 3) the effects of apprenticeship training could differ between occupations. Some occupations might be learnt in school, while practical skills – acquired in firms – might be essential in another. Also, since the distribution of training provision varies between occupations, the lack of such information can easily bias the results. And finally 4) it could be argued that the usual outcome of unemployment or income does not cover all possible fields where apprenticeship might benefit or harm the students.

Research questions

The first aim of this paper is to eliminate most of the above concerns when testing the effects of workplace-based training on labor market entrance by using a new panel database, the Hungarian Life Course Survey (HLCS). The study compares two groups of students within one secondary level program. It is possible to look at the vocational training track, and compare those who have taken workplace-based training (apprentices) with those who were enrolled only in school-based training. Because of the institutional set-up of the Hungarian VET system (see below) non-college bound students, who enter the "lowest" vocational training track, could either carry out their compulsory practical training at firms or within the school. Hence the "treatment" and the "control" groups within the system are quite obvious: both groups have received the same general training (the first two years in the vocational training program) and they might even go to the same school; the only difference is the place where practical training takes place. Although the allocation of students between training places might not be random, the HLCS offers an exceptionally wide variety of controls, which reduces the omitted variable bias concern. Moreover, the HLCS is a panel database, which rules out the problem of inverse causality. The database also includes information on the types of qualification that students have acquired, which allows for an industry (proxy for occupation) control at the individual level. Finally the database is rich enough to test the effect of apprenticeship training on several labor market outcomes including, besides the usual unemployment probability, the net earning, the length of contract, and post-secondary training.

Hungary is also a good country in which to study the effects of apprenticeship training. The Hungarian VET is not a dual-system per se. In fact, the system is very much school-based, with relatively few links to the labor market (Kis et al. 2008). The system has been one of the most decentralized ones in the OECD (OECD 2004). So, if having practice at a private firm is indeed beneficial, Hungarian apprentices can really profit from this experience. Also, the outcomes of the Hungarian VET system are around the OECD average. The youth unemployment relative to adults ratio, the "neither employed nor in education or training" ratio, and the share of upper-secondary vocational students are all around the middle (Piopiunik and Ryan 2012), which suggests that Hungarian VET is most likely not an outlier and that the conclusions might also be generalizable to other VET systems.

The second aim of the paper is to provide some empirical results on the potential reasons for the decreased unemployment of apprentices. Why do students benefit from apprenticeship training? To put it simply, there are three main lines of argument (Acemoglu and Pischke 1998; Plug and Groot 1998; Wolter and Ryan 2011). The first is a human capital argument: apprentices find their initial job more quickly because of their improved skills, which facilitates faster adoption to the new workplace, as well as higher productivity right from the start. Skills learnt at the workplace can either be specific to the firm, or technologically general (cf. Acemoglu and Pischke 1998), meaning that although skills acquired at the firm are specific to the given technology, they can also be useful in other firms using the same technology. The second is a screening argument: graduates with workplace-based training are already screened by employers and, thus, the risk of hiring someone with unfavorable characteristics is smaller than for graduates with school-based training. Or similarly, training firms select their future employees already when they hire apprentices; that is, they equate this period of VET training with the usual probation period. And the third is a signaling argument: apprentices carry a signal that informs the future employer about their unobservable characteristics, even if the firm is not their training firm.

While all these arguments predict a lower initial level of unemployment for apprentices, there are differences in the prediction of other outcomes. The human capital argument predicts higher wages for the increased productivity of apprentices, while the pure screening or signaling argument does not. If we believe that apprenticeship training increases the specific or technologically general skills of the trainees, then firms should reward this by increasing their wages. Note that this is a simplified argument, since firms might consider the training to be an allowance for the apprentice, and thus cut their starting salary accordingly, which decreases the wage differences between employed apprentices and non-apprentices.

Both the screening and the signaling argument put forward a higher ratio of long-term contracts for apprentices, but the human capital argument does not. If firms use apprenticeship training as the probation period, they are more likely to offer apprentices long-term contracts after they hire them, since they have already done the screening. Similarly, if apprenticeship training carries an important signal, firms are more likely to offer long-term contracts to apprentices, since the risk involved in their employment is smaller.

To separate the effect of signaling and screening I separate those who stayed at the same firm, where they were trained, and those who moved to a different firm after the training period was over. The signaling argument would predict "stayers" should receive long-term contracts just as likely as the "movers", since both groups carry the same signal, but both groups should be in a better position than non-apprentices. However the screening argument would put forward that only "stayers" benefit from training, while "movers" are in the same position as non-apprentices.

Note that there are limitations to apprenticeship training (see Ryan 2011). The benefits of apprenticeship might differ not only across occupations but also across students. Some might prefer the theoretical approach, while others the practical, and we know little about the distribution of these groups. Also, employers might utilize apprentices as "cheap labor", i.e. consider them as a source and not as an investment (Ryan 2011; Mohrenweiser and Zwick 2009; Wolter and Ryan 2011), which suggests that apprentices might not profit from workplace-based training in terms of human capital. Moreover, it is very hard to strike the right balance between academic and practical training, or between general and specific skills. The over-abundance of either – in the case of VET – might be considered harmful either in the short run (no specific skills), or in the long run (no general skills). In relation to this, the immediate benefits of apprenticeship training – such as the smoother school to work transition – might be counterbalanced by long run disadvantages (Plug and Groot 1998; Ryan 2001). Hence, it is not at all obvious that apprenticeship is indeed beneficial for all, even in the short run.

Previous research

There are but a handful of empirical studies that offer analysis of the causal effects of apprenticeship training on individual level labor market outcomes (see Wolter and Ryan 2011). These analyses almost exclusively predict that apprentices benefit from workplace-based training, in that their initial employment probability is higher, but their methods, additional tests, and conclusions differ.

Bonnal et al. (2002) show for France that apprentices have a better chance of finding a job immediately after graduation, but this effect is mainly driven by the "stayers", i.e. those that stay at the firm that provided the training. Female apprentice "movers" have the same (or lower) employment probability than non-apprentice vocational students, while male "movers" also have lower employment probability than "stayers", but similar or higher than non-apprentices. The authors argue that this finding could be due to three distinct reasons, among which they are unable to discriminate: a) apprentices might lack the general human capital, as opposed to non-apprentice VET students, and thus finding a job at a new firm is harder/not-easier; b) "movers" might be negatively selected, as those who are not hired by the training firm might have some unobserved negative trait; and similarly c) there might be a negative signaling effect associated with moving to another firm, even if "movers" are not different from "stayers" in other respects. Nevertheless, all these considerations point more towards the screening and the signaling model than the human capital model.

Other studies that look at the causal link worry less about the reasons for increased employment opportunity. Bertschy, Cattaneo and Wolter (2009) also find that full-time vocational students are less likely to finish education successfully, as opposed to apprentices, and hence less likely to find an adequate job 1 ¾ years after the modal student finished education. But their focus is on another important point involved in this topic. Looking at the Swiss training system, they emphasize that self-selection into educational tracks is very important. In fact, students with higher PISA literacy scores are less likely to drop out, and less likely to enroll in a vocational field with a higher intellectual level; but the level of literacy does not have a direct effect on the probability of finding an adequate job, yet only though the vocational track choice.

Noelke and Horn (2011) study Hungary after the transition, when the number of apprenticeship training places has dropped significantly. Using the fact that the decrease in training places was different in the different counties, they conclude that apprentices are less likely to be unemployed after they enter the labor market; this effect fades out some time after entry into the labor market. The authors find no differences in the quality of job acquired in the labor market. Parey (2009) also uses variation in the supply of apprenticeship places in local labor markets as an exogenous predictor for individuals' choice between firm-based apprenticeship training and fully school-based vocational program, to identify the returns to apprenticeship training. Similarly to the above listed papers, he shows that apprenticeship training leads to substantially lower unemployment rates, which fade out over time.

The HLCS data

The Hungarian Life Course Survey (HLCS) is an individual panel survey conducted annually. The original sample of 10,022 respondents was chosen in 2006 from the population of 108,932 eighth grade students with valid test scores from the National Assessment of Basic Competencies (NABC). The NABC measures the literacy and numeracy of all 6th, 8th and 10th grade students in every year, starting from 2006 (OECD 2010). The NABC also contains a set of family background variables, such as parental education or employment status. The first HLCS survey wave was completed during the winter of the school-year 2006/7, and subsequent waves have been fielded on a yearly basis. Currently there are 6 waves available with fairly large response rates. The sample appreciation, on average, is around 5% (see Table 1).

The HLCS database contains detailed information on skills (literacy and numeracy in 8th grade as well as class marks in each year), ethnicity, school trajectory, family background –including parental education and income –, and many other dimensions. The main blocks are family and financial situation, parents' work history, studies/school results, track change/dropout, labor market, and data on partner/child. Although students with special educational needs (SEN) are overrepresented in the data, propensity weights are used to control for the oversampling, and the imminent sample attrition, in the estimations. The HLCS database also has a fully representative subsample (7,218 of the 10,022 students in 2006/07). This subsample is used for robustness checks, and for analyses where weights could not be utilized.

To adjust for sample attrition, propensity weights, which were designed to adjust for non-response and for the oversampling of low-status students in the initial sample, were recalculated for each wave. The same stratifying procedures were used as in the initial sample. The three strata are: 1) 3 settlement types: the capital and big cities, other cities, villages 2) 7 NUTS-2 regions[1] 3) Reading literacy test scores (30 equal groups from the NABC 2006 reading literacy distribution).

The most important variables of interest in this paper are the school track, the apprenticeship status, and the labor market outcome. School track is defined as the student's school track in the fourth wave of the study (see "Hungarian VET system" below), the year when the median student was finishing the last year of compulsory schooling. Vocational students could either do their practical training within school in class, or in a school workshop, or could go to a private firm, either with the help of the school (usually in groups), or by organizing the training by themselves. I have labeled the former two as school-based and the latter two as workplace-based training. Anyone, who did workplace-based training in the 4th wave or in the 5th wave of the study, is considered an apprentice. The four types of labor market outcomes – employed, unemployed, studying and other – are considered in the last (available) wave of the study, and are self-declared. The main reason for this is that the vast majority of students in the 5th wave (2010/11) were still in school, even among the vocational training students (see Table 2). By the school year 2011/12 the majority of vocational training graduates have entered the labor market (as employed or unemployed) and only a little less than a quarter of them are still in school (e.g. in further training). Besides labor market outcomes, net income and the length of employment contract are also used as outcome measures.

Other variables that are used are the standardized test score (literacy and numeracy), class mark averages (1- fail to 5- excellent), gender (0 male, 1 female), SEN status, Roma ethnicity, and parental education, and are all from the first wave of the study. Additional controls are the class mark average from the 4th wave, whether the student was in the 12th grade in the fourth wave (a proxy for repeating class) and whether s/he applied to her/his 9th grade school in the first place (proxy for motivation) (see Table 3).

[1] The NUTS classification (Nomenclature of territorial units for statistics) is a hierarchical system for dividing up the economic territory of the EU.

The Hungarian VET system

The Hungarian education system resembles that of the post-Soviet systems (see figure A1 in the appendix). Most students choose between three tracks at the end of their 8th grade[2]: an academic track (*gimnázium*), and two vocational tracks. The vocational secondary track (*szakközépiskola*) mixes academic and vocational training and allows for tertiary entrance after graduation, while the vocational training track (*szakiskola*) is a "dead-end", but either school-based or workplace-based vocational practical training is compulsory. Table 4 shows the transition between 8th grade and 9th grade for the cohort included in the HLCS data. Little more than 35% of the cohort enters academic secondary tracks, with around 8% already in the early-selective academic tracks. The other two-thirds of students go to vocational tracks. A large majority of vocational students (over 40% of the cohort) enter the vocational secondary, while around 20% find themselves in vocational training tracks. The remaining less than 5% of students are either dropouts, repeaters, or students with special educational needs (SEN) enrolled in special vocational training tracks.

The vocational training (VT) tracks are considered to be the lowest ranked in the hierarchy of tracks. Hermann (2013) has shown that vocational training tracks are also of worse quality: students suffer substantial losses in literacy and numeracy between grades 8 and 10 as opposed to students in the other two tracks. So comparing VT apprentices with non-VT students would bring up several methodological problems. Nevertheless the question remains: can workplace-based training improve the labor market prospects of non-college bound VT students?

Selection into apprenticeship

Before addressing the effectiveness of the apprenticeship training it is essential to understand the selection into apprenticeship. The system of education, including the vocational schools, is highly decentralized in Hungary. All schools that offer training have to state the profession for which they are training, based on which students can choose schools. Most professions are included in the National Training Register (Országos Képzési Jegyzék - OKJ). The practical training of students in vocational training schools cannot start earlier than grade 10, or the age of 16. There is no centralized process for the allocation of students to training places. It is the duty of the school to provide a training place for each student, although the student can search for a training place for her/himself at any business entity (firm). There is only anecdotal evidence about the process of apprenticeship selection, and thus endogeneity cannot be ruled out: students, who would more likely be employed, are also more likely to get an apprenticeship position. It is not unlikely that apprentices have different personal traits than non-apprentices, but it is also highly likely that the local labor market (the demand side), as well as the occupation of the trainee (the supply side), has an effect on the probability of employment.

Table 6 shows the association of the observable personal traits on the difference between apprentices and non-apprentices. The most unique controls are the standardized test scores, which are measured before students enter the secondary tracks. Note that these test scores cannot be used for the secondary level entrance.[3] In addition, the 8th grade class marks – which are given by the teachers, and which are used for secondary entrance – and the 12th grade class marks are used. Parental background effects are proxied by parental education. Roma ethnicity, and SEN status, as well as grade repetition, are controlled for. Motivation is measured by the variable of "9th grade track is first choice", assuming that those who were accepted on the track of their first choice are more motivated. The month when the survey was taken is also controlled for in all estimations and is not shown.

The only covariates that are significant in the first estimation (Table 6 column 1) are the reading test score and the proxy for grade repetition. Both suggest that higher skilled students are more likely to enter into apprenticeships. Within industry estimations (column 2) do not show the skill differences between apprentices and non-apprentices, suggesting that there are some occupations that attract better students. The most restricted, within school estimations (column 3), show that people with lowly educated parents are more likely to have practical training at private firms. The results also

[2] About 8% of each cohort enters the so called early-selective academic tracks after 4th or after 6th grade, thus students are already enrolled here at the end of their 8th grade. More on this see Horn (2013).

[3] It is used to make schools accountable and to provide feedback for the teachers (see OECD 2010).

suggest that students who get higher class marks at the end of the first semester of the last year are also more likely to become an apprentice. Whether more motivated students (who do better in class) are more likely to become apprentices, or whether teachers give higher grades to those who could get outside-school practice, is unknown.

Both the AIC and the BIC statistics are smaller for the within industry model than the first model, but it is the smallest for the within school estimation, suggesting that both the industry and the school effects are very important. Also, the adjusted count R^2 for the first estimation is only 0.04, meaning that the personal traits can only help to predict the outcome in 4% of the cases, suggesting that the observed personal traits are not very important in selection into apprenticeship.

So it seems that while on the national level there are very small but observable differences between the average personal traits of apprentices and non-apprentices, these observable differences seem to diminish within school and within occupation.

Does workplace-based training increase labor market outcomes?

The base model is a multinomial logit model with all four possible outcomes – employed, unemployed, studying, and other – on the left hand side. Due to the fact that the right hand side variables are measures before the left hand side variable, reverse causality is unlikely. In order to minimize omitted variable bias all controls presented in Table 3 are included group-by-group in Table 7. In the first estimation (1) only apprentice is included, in the second (2) measures of skills (test scores and class marks) are also controlled for, while in the third (3) the social background characteristics and other controls are included. Note that apprenticeship training is significant in all three estimations, and show, that those VT students who had carried out practical training at a private firm, as opposed to doing practical training in school, have around 1.5 times higher odds of being employed, as opposed to being unemployed. This effect is unchanged by any of the personal traits that are included in the model. On the other hand skills, as well as social background, matter. It seems that class marks matter more than skills, measured by standardized test scores 6 years earlier, in the probability of being employed. Parental education also plays a role, but only if the parents have a low educational background. There are important gender differences, and those who did not repeat a class up to grade 12 are also more likely to get a job.

The baseline uncontrolled average probability of being employed for a VT student in 2011 is 44%. Apprentices, however have a 47% chance, while school-based trained students have a 39,5% chance of being employed. The chances of being unemployed is the reverse: apprentices have a 21% chance, while the others have a 26,5% chance. There are no differences in the uncontrolled average baseline probabilities of the other two outcomes between the two groups (study: 24%, other: 9%). Using the above model (Table 7, full model), to predict the probabilities, yields very similar results. The average predicted a probability for apprentices is 47,5%, while for the school-trained it is 38,7%. The respective predicted probabilities at means are 48,9% and 39,2%, thus the sample distribution is not highly skewed. The marginal effect of being trained at a private firm is 9,6% at the mean. This effect is very similar for the top of the range students (high class mark averages, high literacy and numeracy, and parents with secondary general or tertiary schooling) as well as for the lowest (low class mark averages, low literacy and numeracy, and parents' education primary or below). While the marginal effect for the first type is 8,2%, for the second it is 10,8%, and both are highly statistically significant.

The effectiveness of workplace-based training can depend very much on the type of the industry. The HLCS contains information on the type of the OKJ qualification for vocational graduates, although the number of missing cases is high (see Table 5). Of the 1,471 VT students only 964 has this information in the dataset. Table 8 shows the same multinomial logit model with industry fixed effects added.[4]

The main conclusion does not change even if industry fixed effects are controlled for: apprentices have a 1,7 times higher odds to be employed vs. being unemployed in 2011 spring than

[4] Note that due to the large missing values of industry codes I recalculated the sample weights with the inverse ratio of having a qualification using the original sampling strata, and hence the larger weighted number of observations.

those with only school-based vocational training practice. Table 9 shows the predicted probabilities and marginal effects of apprenticeship for a student with qualifications in the different industries at the population mean and at the industry means. While the probability of being employed differs a lot between industries, the effect of workplace-based training remains stable across industries.

The non-difference of apprentice effect in the different industries is also underlined if workplace-based training and industry product term interaction are included in the model. Since interaction terms in non-linear models are problematic (Ai and Norton 2003), I have estimated linear probability models[5] on the probability of being employed (1) vs. unemployed, studying or other (0) with industry and apprentice interactions (Table A1 in the Appendix).[6] The results show that although the effect of apprenticeship training is statistically significant only in mechanics, services, and agriculture, the effects do not significantly differ between any two industries, except industry and mechanics on the 10% level (Table A2).

Robustness checks

Although reverse causality is not likely in the base model, school fixed effects estimation below can further diminish the problem of omitted variable bias. Other checks will also highlight that the results are not driven by the model specification, or by the measured outcome.

Table 10 adds school fixed effects to the base model as well as to the industry fixed effect model. Looking at differences within schools is an especially strong test of the effect of apprenticeship training, since it controls for both local labor market effects as well as potential differences between school quality. Note that the HLCS has not used schools as sampling units, thus the fact that some students are from the same school is chance only. In fact the 1471 VT students are from 295 VT schools, providing, on average, about 5 students per school for the test. There are only 16 schools with only one student in the sample. Taking missing values as well as the variance of the outcome measure within school into account,[7] and the fact that the representative subsample should be used due to problems of weighting in fixed-effects logistic regressions, little less than 100 schools are left for the non-linear analysis.

Also since the multinomial logit model with a large number of fixed effects has not yet been fully developed (see Pforr 2011), I have estimated linear probability models as well as logit models with fixed effects for this robustness check. Moreover, since fixed effect logit models in Stata cannot deal with within group weights, the representative subsample had to be utilized (Table10).

Nevertheless, the effect of apprenticeship training remained significant in both non-linear and linear specifications without industry fixed effects. Also the size of the effect – both the average marginal effect (11-12%) and the odds ratio (1.8) – is very similar to that of the base model. Moreover, while the apprentice parameters in the models, where both industry and school fixed effect were included, have lost their significance, their size has not changed.[8] I believe this is a very strong test of the effect of apprenticeship training on employment.

Tests in Table 12 ensure that the observed effects are not due to the definition of the apprentice variable. The same multinomial logit model is used as in the base model but apprenticeship training is split into two years: those who were trained in the 4th wave, and those who were trained in the 5th wave (Table 11).

Tables 12 and 13 highlight that apprenticeship training has a strong effect on the probability of being employed, even for those who had training after finishing compulsory schooling. Students

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[5] Note that the critique of Horrace and Oaxca (2006) that linear probability models are inherently biased might be less important here, since most of the independent variables are dummies, thus out of sample prediction is less likely (and see also Angrist and Pischke 2008).

[6] Estimating the same models on the probability of being employed (=1) vs. unemployed (=0) offers substantively the same results.

[7] Fixed-effect logit regressions identify the effect only from schools, where both apprenticed and non-apprenticed students were present.

[8] Note that including the school as well as the industry fixed-effects look at only those schools where there are apprentices as well as non-apprentices present, and apprentices do training in multiple industries.

enrolled in workplace-based training in the 5th wave of the study (after the median student finished compulsory education) have on average an 8% higher chance of being employed in the next year, *ceteris paribus* the effect of workplace-based training in the 4th wave and 5th wave employment status. This effect is also constant across industries.

The third robustness check uses another set of outcome variables. The HLCS also asks students about their employment status during the last academic year. That is, students in the 6th wave of the study, in 2012 spring, were asked whether they had had any regular job during the months between September 2010 (the start of the school year) and August 2011, and students in the 5th wave were asked whether they had a regular job between September 2009 and August 2010. The data is for each month in between. Figure 1 depicts the predicted probability for a male, non-Roma, non-SEN student with average class marks and test scores, parents with vocational education, who has not repeated class up until 12th grade, and applied for his track in the first place in 9th grade, and filled out the survey in May 2012. The dependent variable is 1, if the student had a regular job, and 0 otherwise.

It seems that apprentices are much more likely to find a regular job right after the end of the school year. The gap between the average employment probability of apprentices and non-apprentices grows during the summer months, and does not decline afterwards. This indicates that apprentice VT students have a smoother transition into the labor market than the non-apprentice VT students. The effect is also quite sizeable. It is around 14% in August 2011 (decreasing to 10% in 2010 May), while the average employment probability is around 40%.

The same pattern is observable within almost all of the industries (figure 2). The employment probability gap between apprentices and non-apprentices increase to around 11-20% after the end of school, for the three months, and then it either decreases slightly (as in mechanics and transport-environment), or stays at the same level, but remains statistically significant and large.

Whether this effect is due to the superior specific skills that apprentices gained while being trained at the firm, or due to the increased screening or the increased signaling effect, is not clear from these figures. While screening and signaling would predict an immediate and large difference between the groups – because training firms hire the best candidates right away – which should fade away by time, the human capital argument would suggest a steady but continuous increase in the gap, which should only fade away after a good amount of time, when others also gain the specific skills. The increase in the first three months supports the human capital argument, but frictions in the labor market (e.g. summer break at firms) could also explain why the screening or signaling takes time to “kick in”. Also the decline (or non-increase) in the differences after the third month would underline the screening and the signaling argument, but proponents of the human capital argument could argue that the still remaining 10%+ gap in employment chances could well be the exact reward for superior employer skills.

In order to see whether the signaling or the human capital argument comes closer to reality, other outcomes should be studied.

Other measures of labor market success

The HLCS allows for two other types of labor market outcome measure: net earnings and the type of employer contract (long-term vs. fix-term). The HLCS asks for the average monthly net earnings, and the average net wage received from the main job of the respondent. If data for the first question was missing I imputed it with data from the second. Data only for 14 of the total of 511 employed VT students was missing (2,4% of cases). The uncontrolled mean net earnings for the apprentices were almost exactly the same as for the non-apprentices: 85 thousand Hungarian forints (~280 Euro). Table 14 shows the model where the net earning is regressed on the same controls as in the base model. The difference between apprentices and non-apprentices remains insignificant, even after controls are included.

However, as noted above, firms might consider the training to be an allowance for the apprentice, and thus cut their starting salary accordingly. This would also decrease the wage differences between employed apprentices and non-apprentices, even if apprentices have increased skills. Luckily the HLCS have asked about the reservation wage of the students. Using this information to impute the net wages of the non-employed – and assuming that students correctly judge their own skills, and hence their reservation wage corresponds to this – I tested the difference between apprentices and non-apprentices. Apparently, there seems to be no difference at all.

Another technique to correct for the selection bias is the Heckman (1979) sample selection method.[9] Table 13 column 3 and 4 show the Heckman correction for the model in column 1. Although the selection corrected results are somewhat larger, neither of the estimates shows significant effects of apprenticeship on net earnings.

The no-difference in net earnings between apprentices and non-apprentices suggests that employment differences are not due to increased skills but due to something else.

Table 15 regresses the dummy of a long-term contract (vs. fix-term-contract) on the controls of the base model. Apparently, apprentices are more likely to get long-term contracts, as opposed to fix-term contracts, than school-based trained students. While 73% of employed apprentice students have long-term contracts in spring 2012, the respective figure for non-apprentices is only 62%. Even after controlling for the individual characteristics, as in the base model, the chance of an average apprentice getting a long-term contract is significantly higher. The average marginal effect is around 16% (table 15, columns 1-2). The effects are substantively the same, even if industry fixed effects are included (table 15, columns 3-4).[10]

These results suggest that the screening or the signaling effect is more important in getting the first job than skills. If apprenticeship students had superior skills compared to non-apprentices, firms would most likely offer them a higher amount to compensate for higher productivity. On the other hand, if screening or signaling did not matter, the chance for non-apprentices to get a fix-term contract should be just as high as for apprentices. This latter result suggests that firms use apprenticeship training as some sort of a substitute for the probation period, or they use it as a signal about the quality of the apprentices.

To separate these two effects, the “stayers” and the “movers” should be separated. If screening is more important that signaling, “stayers”, i.e. those who get their first job at the firm where they were apprentices, might drive the results, as in case of France (Bonnal, Mendes, and Sofer 2002), if however signaling is more important, there should be no difference between these two groups of students.

“Stayers” and “movers”

Unfortunately the HLCS does not contain direct information about the exact firm of the apprenticeship. Nevertheless the type of firm[11] during the apprenticeship, as well as the type of first job, is surveyed, but only after the 5th wave. That is, the effect of “moving” can only be estimated for those who had workplace-based training in the 5th wave. Moreover, since these firm categories are very broad, this is a better proxy for “moving” than for “staying”, since it is likely that if the industry of the training firm and the employer is not the same, people have moved; however its converse does not mean that apprentices have stayed where they were trained.[12] Table A3 in the appendix shows the number of students within the different apprenticeship/employer type categories. Naturally this variable is only available for those who were apprentices in the 5th wave and got a job in the 6th wave.

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[9] This is a textbook case for the Heckmann selection bias: only the earnings of the employed are observed, and since non-apprentices are less likely to be employed thus the observed mean earning of the non-apprentices are likely to be higher than the unobserved wage offers, the effect of apprenticeship training on observed earnings is likely to be downwardly biased.

[10] I have also estimated a Heckman probit correction model, with no significant sign for selection bias. Not shown here.

[11] Agriculture, forestry and fishing; Mining and quarrying; Processing; Electricity, gas, steam and air conditioning; Water supply, wastewater collection and treatment, waste management; Construction Trade, automotive services; Transportation, warehousing; Hotels and restaurants, catering; Information, communication; Financial and insurance activities; Real estate transactions; Professional, scientific and technical activities; Administrative and support service activities; Administration and defense, compulsory social security; Education; Human health and social work; Arts, entertainment and recreation; Other services; Households as employers, producers, and service; Organizations outside Hungary; Other.

[12] But if we assume technologically specific skills these categories are useful.

Thus employment probabilities cannot be analyzed, only the effects in terms of net earnings and long-term contracts. The reference group is non-apprentices, with a job in the 6th grade.

The results seem to underline that screening has an important effect: “stayers” have a 22-23% higher chance, or 2.8-3.1 times higher odds, of receiving a long-term contract as opposed to either “movers” or to non-apprenticeship students who are employed in the 6th wave. The advantage of “movers”, as opposed to non-apprentices, is less obvious. It is non-significant in the linear, but significant in the logit specification, and the size of the effect is also much smaller, but still sizeable; around 10-15% higher probability or 1.5-2 times higher odds. Nevertheless, movers do have a non-negative or positive advantage, which suggests that signaling also matters for finding the first job. On the other hand, differences in net-earning – again – are not significant, which downplays the importance of skills (table 16).

Conclusion

Although workplace-based training has long been praised for its effectiveness in preparing non-college bound youth for the labor market, there are but a handful of studies that could convincingly show that the observed association between apprenticeship training and higher initial employment probability is causal. This analysis shows that vocational training program graduates, who have done their practical training at private firms (apprentices), are around 10-15% more likely to be employed after they finish education, than those who had their practical training in schools. This effect is net of individual skills, school attainment, parental background, motivation, gender and ethnicity, and robust to the inclusion of school fixed effects. The effect is also very similar across industries, and is likely to remain significant and large during the first year in the labor market.

On the other hand, there seems to be no difference between the net earnings of apprenticed and non-apprenticed students after they are employed, which suggests that there are no significant differences in specific skills between these two groups. However, the difference between the two groups in getting a long-term contract with their employer is significant and sizeable. Apprentices are 16-20% more likely to sign a long-term contract as opposed to the non-apprentices, which suggests that firms might use the training period as a probation period (screening), or that an apprenticeship is a good signal on the labor market. Comparing those who might have stayed at the same firm where they were trained, with those who moved to another type of sector, shows that “stayers” are more likely to get long term contracts, but not more likely to earn more money, which suggests that screening plays an important role in apprenticeship training. On the other hand “movers” also have a higher probability of getting a long term contract as opposed to non-apprentices, which implies that their apprenticeship training might also have a signaling function.

All in all, this study argues that the positive effect of workplace-based training on initial employment probability is causal, but it is more likely to be due to the increased screening, and maybe due to the increased signals, that it offers for firms, and not due to increased productivity.

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Table 1. Basic statistics of the HLCS database

wave	School year	Date of the survey	Median school grade	Number of students (with oversampling SEN students)	Number of students (representative sub-sample)
1	2006/07	2006 fall	9	10022 (100%)	7218 (100%)
2	2007/08	2007 fall	10	9300 (92,8%)	6716 (93%)
3	2008/09	2008 fall	11	8825 (88,1%)	6397 (88,6%)
4	2009/10	2009 fall	12	8333 (83,1%)	6071 (84,1%)
5	2010/11	2011 spring	13 (LM entry, post-secondary vocational or tertiary)	7662 (76,4%)	5587 (77,4%)
6	2011/12	2012 spring	14 (LM entry, post-secondary vocational or tertiary)	6974 (69,5%)	5111 (70,81%)

Table 2: Labor market outcomes in the 5th and 6th wave

	5th wave					6th wave				
	work	unempl.	study	other	**Total**	work	unempl.	study	other	**Total**
academic (8-yr)	56	54	2525	96	**2731**	155	48	2429	39	**2671**
%	2,05	1,98	92,46	3,52	**100**	5,8	1,8	90,94	1,46	**100**
academic (6-yr)	212	28	4358	121	**4719**	255	96	4214	151	**4716**
%	4,49	0,59	92,35	2,56	**100**	5,41	2,04	89,36	3,2	**100**
academic (4-yr)	838	775	23360	837	**25810**	2795	1331	20538	1278	**25942**
%	3,25	3	90,51	3,24	**100**	10,77	5,13	79,17	4,93	**100**
voc. sec.	1642	1676	30517	909	**34744**	7220	4975	19633	2568	**34396**
%	4,73	4,82	87,83	2,62	**100**	20,99	14,46	57,08	7,47	**100**
voc. tr.	1647	2066	11306	664	**15683**	6794	3581	3642	1430	**15447**
%	10,5	13,17	72,09	4,23	**100**	43,98	23,18	23,58	9,26	**100**
spec. voc. tr.	191	369	2441	130	**3131**	736	517	1558	281	**3092**
%	6,1	11,79	77,96	4,15	**100**	23,8	16,72	50,39	9,09	**100**
Missing	2797	4462	12127	2804	**22190**	6716	4807	7623	3173	**22319**
%	12,6	20,11	54,65	12,64	**100**	30,09	21,54	34,15	14,22	**100**
Total	7383	9430	86634	5561	**109008**	24671	15355	59637	8920	**108583**
%	6,77	8,65	79,47	5,1	**100**	22,72	14,14	54,92	8,21	**100**

Note: the Table contains the weighted number of students

Table 3: Descriptive statistics – students in the 6th wave of HLCS

Full sample						
Variable	*obs.*	*weighted obs.*	*mean*	*s.d.*	*min.*	*max.*
math test score (std.)	6453	103298	-0.02	1.04	-3.16	3.08
reading test score (std.)	7002	108583	-0.10	1.03	-3.78	2.87
8th grade class mark average	6754	104920	3.87	0.73	1	5
12th grade class mark average	5463	87557	3.70	0.68	2	5
female	5367	86074	0.49	0.50	0	1
SEN student	7001	108573	0.06	0.25	0	2
Roma	7002	108583	0.06	0.24	0	1
parents' ed.: below primary	6992	108484	0.01	0.10	0	1
parents' ed.: primary	6992	108484	0.11	0.31	0	1
parents' ed.: secondary	6992	108484	0.35	0.48	0	1
parents' ed.: tertiary	6992	108484	0.25	0.43	0	1
12th grader in 4th wave	5357	86358	0.85	0.35	0	1
9th grade track is first choice	6369	97572	0.77	0.42	0	1
Vocational training students only						
Variable	*obs.*	*weighted obs.*	*mean*	*s.d.*	*min.*	*max.*
math test score (std.)	1087	14180	-0.83	0.68	-2.74	2.10
reading test score (std.)	1217	15447	-0.92	0.68	-3.78	1.21
8th grade class mark average	1170	14883	3.18	0.53	1	5
12th grade class mark average	1217	15447	3.32	0.58	2	5
female	1194	15143	0.35	0.48	0	1
SEN student	1216	15437	0.10	0.32	0	2
Roma	1217	15447	0.09	0.29	0	1
parents' ed.: below primary	1214	15412	0.02	0.15	0	1
parents' ed.: primary	1214	15412	0.20	0.40	0	1
parents' ed.: secondary	1214	15412	0.25	0.43	0	1
parents' ed.: tertiary	1214	15412	0.05	0.22	0	1
12th grader in 4th wave	1217	15447	0.78	0.41	0	1
9th grade track is first choice	1196	15210	0.73	0.44	0	1

Table 4: Transition from 8th to 9th grade (from 2006 to 2007)

		primary	ac. (8-yr)	ac. (6-yr)	Missing	Total
9th grade	**primary school**	454	17	15	33	**519**
	%	0,42	0,02	0,01	0,03	**0,47**
	academic (8-yr)	318	2945	45	92	**3400**
	%	0,29	2,7	0,04	0,08	**3,11**
	academic (6-yr)	450	72	4884	197	**5603**
	%	0,41	0,07	4,47	0,18	**5,13**
	academic (4-yr)	27895	256	264	773	**29188**
	%	25,53	0,23	0,24	0,71	**26,71**
	voc. sec.	42546	274	270	1644	**44734**
	%	38,94	0,25	0,25	1,5	**40,94**
	voc. tr.	20693	83	22	739	**21537**
	%	18,94	0,08	0,02	0,68	**19,71**
	spec. voc. tr.	2103	18	0	143	**2264**
	%	1,92	0,02	0	0,13	**2,07**
	Missing	1794	41	60	124	**2019**
	%	1,64	0,04	0,05	0,11	**1,85**
	Total	96253	3706	5560	3745	**109264**
	%	88,09	3,39	5,09	3,43	**100**

HLCS data, own calculations

Note: sample weighted to represent the whole 2006, 8th grade cohort

Table 5.: Number and percentage of VT students in school-based and workplace-based training by industry

	Unweighted				Weighted			
Industry	school-based	work-based	missing	**Total**	school-based	work-based	missing	**Total**
social services	3	6	0	**9**	24	81	0	**105**
%	33,33	66,67	0	**100**	22,86	77,14	0	**100**
mechanics	108	112	4	**224**	1210	1341	41	**2592**
%	48,21	50	1,79	**100**	46,68	51,74	1,58	**100**
industry	124	106	2	**232**	1356	1193	10	**2559**
%	53,45	45,69	0,86	**100**	52,99	46,62	0,39	**100**
transport-environment	13	19	0	**32**	108	230	0	**338**
%	40,63	59,38	0	**100**	31,95	68,05	0	**100**
services	121	267	7	**395**	1374	3160	88	**4622**
%	30,63	67,59	1,77	**100**	29,73	68,37	1,9	**100**
agriculture	43	29	0	**72**	462	398	0	**860**
%	59,72	40,28	0	**100**	53,72	46,28	0	**100**
missing	178	296	33	**507**	1483	2628	260	**4371**
%	35,11	58,38	6,51	**100**	33,93	60,12	5,95	**100**
Total	590	835	46	**1471**	6017	9031	399	**15447**
%	40,11	56,76	3,13	**100**	38,95	58,46	2,58	**100**

Table 6: Selection into apprenticeship

	(1)	(2)	(3)
8th grade class mark avg.	1.369	1.228	0.961
	(0.266)	(0.311)	(0.292)
12th grade class mark avg. (1st semester)	1.023	1.030	1.519*
	(0.164)	(0.198)	(0.361)
math test score (std.), 8th grade	0.779	0.854	0.858
	(0.119)	(0.156)	(0.200)
reading test score (std.), 8th grade	1.555***	1.445*	1.045
	(0.240)	(0.274)	(0.239)
parents' ed.: primary or below	1.335	1.166	2.093**
	(0.314)	(0.351)	(0.740)
parents' ed.: secondary or higher	1.124	1.095	1.311
	(0.241)	(0.288)	(0.411)
SEN student	2.307	3.021	1.303e+06
	(1.987)	(2.635)	(1.335e+09)
Roma	1.125	0.890	1.748
	(0.352)	(0.343)	(0.942)
9th grade track is first choice	1.103	1.315	1.056
	(0.221)	(0.335)	(0.311)
12th grader in 2009	1.580**	1.540	0.996
	(0.356)	(0.406)	(0.325)
female	1.005	0.534**	0.784
	(0.200)	(0.156)	(0.247)
Constant	0.369		
	(0.293)		
Industry FE	n	y	n
School FE	n	n	y
Observations	575	404	313
AIC	780.923	512.427	262.265
BIC	846.238	568.447	314.712

OR reported, se in parentheses, *** p<0.01, ** p<0.05, * p<0.1,
Month of survey is controlled for

Note: Representative subsample is used, because fixed-effect logistic regressions cannot deal with within group weighting.

Table 7: Base model: multinomial logit model, odds of being employed, studying or other wrt. being unemployed

	(1)			(2)			(3)		
VARIABLES	work	study or trainee	other	work	study or trainee	other	work	study or trainee	other
apprentice	1.489***	1.149***	1.226***	1.457***	0.975	1.179**	1.581***	1.097*	1.161*
	(0.0632)	(0.0553)	(0.0790)	(0.0670)	(0.0509)	(0.0819)	(0.0769)	(0.0604)	(0.0966)
8th grade class mark avg.				1.186***	1.367***	1.373***	1.251***	1.350***	1.411***
				(0.0553)	(0.0724)	(0.0975)	(0.0635)	(0.0769)	(0.123)
12th grade class mark avg. (1st semester)				1.136***	1.636***	1.597***	1.094**	1.656***	1.403***
				(0.0473)	(0.0771)	(0.0995)	(0.0481)	(0.0814)	(0.104)
math test score (std.), 8th grade				1.170***	1.062	0.720***	0.951	0.952	0.995
				(0.0442)	(0.0459)	(0.0427)	(0.0390)	(0.0444)	(0.0723)
reading test score (std.), 8th grade				0.724***	1.023	0.897*	0.807***	1.074	0.680***
				(0.0281)	(0.0457)	(0.0528)	(0.0335)	(0.0510)	(0.0488)
parents' ed.: primary or below							0.540***	0.484***	0.694***
							(0.0322)	(0.0348)	(0.0688)
parents' ed.: secondary or higher							1.014	1.289***	1.532***
							(0.0585)	(0.0828)	(0.158)
SEN student							0.749*	0.802	2.06e-07
							(0.113)	(0.149)	(0.000105)
Roma							0.821**	0.999	3.277***
							(0.0691)	(0.0995)	(0.373)
9th grade track is first choice							1.014	1.009	1.079
							(0.0541)	(0.0617)	(0.0965)
12th grader in 2009							1.941***	0.616***	0.774***
							(0.121)	(0.0390)	(0.0760)
female							0.552***	1.022	10.46***
							(0.0300)	(0.0621)	(1.162)
Constant	1.487***	0.923**	0.349***	0.501***	0.0786***	0.0190***	0.399***	0.140***	0.00610***
	(0.0481)	(0.0333)	(0.0171)	(0.0921)	(0.0167)	(0.00542)	(0.0827)	(0.0327)	(0.00226)
Net number of observations	1204	1204	1204	1040	1040	1040	970	970	970
Weighted number of observations	15,048	15,048	15,048	13,392	13,392	13,392	12,672	12,672	12,672

Standard error in parentheses, *** p<0.01, ** p<0.05, * p<0.1, ORs reported, reference category is *unemployed*. The month of the survey in 2011 is controlled

Table 8: Multinomial logit model with industry fixed effects, odds of being employed, studying or other wrt. being unemployed

	weighted			representative subsample		
VARIABLES	work	study-trainee	other	work	study-trainee	other
apprentice	1.775***	0.985	1.363***	2.060***	0.924	1.196
	(0.0823)	(0.0482)	(0.130)	(0.430)	(0.202)	(0.487)
social services	0.306***	0.841	0.167***	1.78e-07	0.765	4.85e-08
	(0.0674)	(0.136)	(0.0511)	(0.000189)	(0.636)	(0.000117)
mechanics	1.583***	1.359***	0.813	1.867**	1.110	0.611
	(0.103)	(0.0973)	(0.164)	(0.549)	(0.361)	(0.494)
industry	1.266***	1.157**	1.094	1.558	1.072	0.683
	(0.0799)	(0.0796)	(0.153)	(0.444)	(0.332)	(0.404)
transport-environment	2.882***	2.156***	2.75e-07	1.762	2.143	9.56e-07
	(0.496)	(0.403)	(0.000221)	(1.097)	(1.393)	(0.000967)
services (reference)						
agriculture	1.267**	2.000***	2.334***	2.182	4.092***	2.406
	(0.123)	(0.194)	(0.340)	(1.058)	(1.904)	(1.691)
Constant	0.375***	0.613**	0.426**	1.207	1.278	5.376
	(0.0785)	(0.133)	(0.177)	(1.177)	(1.295)	(9.385)
Net number of observations	681	681	681	681	681	681
Weighted number of observations	15,824	15,824	15,824	803	803	803

Standard error in parentheses, *** p<0.01, ** p<0.05, * p<0.1, ORs reported, reference category is *unemployed*. Controls not shown: class marks, test scores, parents education, SEN, Roma, female, 9th grade track choice, 12th grader in 4th wave

Table 9: Predicted probabilities and marginal effects for the different industries at the mean.

	Predicted probability		Marginal effect	Marginal effect
	school-based training	workplace-based training	workplace-based training	workplace-based training
	at population mean		at population mean	at industry mean
social services	0,148	0,236	0,092	0,139
mechanics	0,404	0,546	0,144	0,145
industry	0,371	0,511	0,142	0,145
transport-environment	0,480	0,624	0,143	0,146
services	0,336	0,473	0,139	0,141
agriculture	0,291	0,420	0,132	0,145

Table 10: Robustness check with school fixed effects

	employed=1, unemployed, studying or other=0			
VARIABLES	linear		logit	
apprentice	0.123**	0.113	1.876**	1.841
	(0.0577)	(0.0789)	(0.557)	(0.766)
Constant	0.566***	0.336		
	(0.218)	(0.476)		
school FE	y	y	y	y
industry FE	n	y	n	y
Observations	573	405	397	243
R-squared	0.500	0.568		
Number of schools	215	178	96	67

+Note that weights varying within category cannot be used for FE panel logit, thus the representative subsample is utilized for all models in this Table

Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Controls not shown: class marks, test scores, parents education, SEN, Roma, female, 9th grade track choice, 12th grader in 4th wave, month of survey in 6th wave

Table 11: Number of apprentices in the 4th and 5th wave

		Apprentice 5th wave			
		No	Yes	missing	Total
apprentice 4th wave	No	205	120	252	577
	Yes	62	388	369	819
	missing	13	16	46	75
	Total	280	524	667	1471

Note that in the subsequent estimation, students with missing apprentice data in the 5th wave were coded as 0 (not-apprentices), since they are not in school, and hence not asked this question.

Table 12: Multinomial logit model with and without industry fixed effects, odds of being employed, studying or other wrt. being unemployed

	(1)			(2)		
VARIABLES	work	study-trainee	other	work	study-trainee	other
employed in 5th wave	2.391***	0.0394***	1.054	2.391***	2.39e-08	7.08e-08
	(0.193)	(0.0135)	(0.161)	(0.364)	(1.57e-05)	(9.34e-05)
apprentice in 5th wave	1.881***	1.784***	0.921	1.780***	1.270***	1.306**
	(0.103)	(0.110)	(0.0926)	(0.0903)	(0.0681)	(0.137)
apprentice in 4th wave	1.193***	0.762***	1.107	1.369***	0.776***	1.079
	(0.0613)	(0.0453)	(0.0977)	(0.0699)	(0.0419)	(0.113)
industry FE	n	n	n	y	y	y
Constant	0.278***	0.133***	0.00574***	0.0648***	0.594*	0.0880***
	(0.0584)	(0.0319)	(0.00213)	(0.0196)	(0.158)	(0.0444)
Net number of observations	972	972	972	679	679	679
Weighted number of observations	12,708	12,708	12,708	15,771	15,771	15,771

Standard error in parentheses, ORs reported, weighted regressions

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Controls not shown: class marks, test scores, parents education, SEN, Roma, female, 9th grade track choice, 12th grader in 4th wave, month of survey in 6th wave

Table 13: Marginal effect of apprenticeship on being employed.

Marginal effect	**Model 1 (Table 8)**		**Model 2 (Table 8)**	
	apprentice in 5th wave	apprentice in 4th wave	apprentice in 5th wave	apprentice in 4th wave
Main effect	0,077	0,080		
social services			0,036	0,038
mechanics			0,090	0,109
industry			0,088	0,103
transport-environment			0,090	0,123
services			0,084	0,094
agriculture			0,067	0,087

Note: marginal effect is calculated for a non-employed, non-apprentice, male, non-Roma, non-SEN student with average class marks and test scores, parent with vocational education, who has not repeated class till 12th grade and applied for his track in the first place in 9th grade.

Table 14. Other labor market outcomes – net earnings

			Heckman correction	
	net earning			1st stage
VARIABLES		w/ reservation wage	net earning	employed
apprentice	1,201	948.2	8,286	0.240**
	(4,447)	(3,022)	(5,109)	(0.0965)
8th grade class marks	6,799	4,618	7,137	0.0750
	(5,353)	(3,736)	(5,995)	(0.112)
class mark (grade) average, 1st semester	5,786	5,751**	912.3	-0.0380
	(3,940)	(2,470)	(4,294)	(0.0794)
math test score (std.)	1,461	1,382	-762.7	-0.00914
	(3,115)	(2,801)	(3,944)	(0.0755)
reading test score (std.)	3,086	-1,548	-3,675	-0.0979
	(2,856)	(2,300)	(4,082)	(0.0785)
parents' ed.: primary or below	4,031	-5,310	-3,549	-0.134
	(5,508)	(3,795)	(7,045)	(0.130)
parents' ed.: secondary or higher	1,365	5,845	159.7	-0.121
	(5,129)	(3,665)	(5,855)	(0.109)
SEN student	-16,775*	-5,754	-17,495*	-0.0607
	(8,828)	(5,355)	(10,505)	(0.216)
Roma	-15,917**	-1,290	-22,221***	-0.229
	(7,784)	(4,992)	(8,373)	(0.151)
current track is first choice	2,840	3,900	2,535	0.0302
	(4,113)	(3,324)	(5,253)	(0.103)
12th grader	6,082	2,644	28,669***	0.492***
	(5,800)	(3,666)	(7,497)	(0.119)
female, NABC 2006	-20,504***	-11,739***	-39,102***	-0.562***
	(4,720)	(3,299)	(7,618)	(0.106)
6.fho	6,280	5,407	5,754	0.0621
	(4,339)	(3,342)	(5,749)	(0.108)
7.fho	9,971	3,219	4,769	0.0120
	(6,115)	(3,618)	(6,432)	(0.124)
8.fho	1,006	1,594	-3,758	-0.152
	(5,848)	(6,847)	(12,421)	(0.228)
Constant	40,084*	44,574***	-11,576	-0.709
	(23,501)	(15,652)	(28,613)	(0.451)
athrho				2.435***
				(0.393)
lnsigma				10.91***
				(0.128)
Observations	414	891	955	955
R-squared	0.087	0.051		

Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Table 15. Other labor market outcomes – long term contract

VARIABLES	Linear long-term contract++	Logit+	Linear long-term contract++	Logit+
apprentice	0.162***	2.080***	0.209***	2.864***
	(0.0593)	(0.128)	(0.0721)	(0.241)
8th grade class mark avg.	-0.0734	0.709***	-0.0440	0.794***
	(0.0636)	(0.0466)	(0.0831)	(0.0658)
12th grade class mark avg. (1st semester)	0.0192	1.097*	0.0238	1.130*
	(0.0464)	(0.0610)	(0.0575)	(0.0822)
math test score (std.), 8th grade	0.0533	1.288***	0.0580	1.316***
	(0.0479)	(0.0694)	(0.0533)	(0.0909)
reading test score (std.), 8th grade	-0.0311	0.871***	-0.0476	0.793***
	(0.0457)	(0.0455)	(0.0517)	(0.0531)
parents' ed.: primary or below	0.00932	1.043	0.0241	1.151
	(0.0749)	(0.0858)	(0.0915)	(0.126)
parents' ed.: secondary or higher	-0.00625	0.965	0.0798	1.542***
	(0.0627)	(0.0676)	(0.0686)	(0.139)
SEN student	-0.0447	0.827	-0.0812	0.629**
	(0.153)	(0.164)	(0.177)	(0.135)
Roma	-0.260**	0.327***	-0.267*	0.286***
	(0.104)	(0.0369)	(0.140)	(0.0439)
9th grade track is first choice	0.0204	1.093	-0.0368	0.808**
	(0.0601)	(0.0719)	(0.0652)	(0.0718)
12th grader in 2009	0.0372	1.182*	0.0965	1.585***
	(0.0843)	(0.103)	(0.0966)	(0.177)
female	0.0184	1.090	0.0104	1.035
	(0.0679)	(0.0795)	(0.0880)	(0.109)
Industry FE	n	n	y	y
Constant	0.749***	3.150***	0.543*	0.458
	(0.255)	(0.841)	0.209***	(0.271)
Net number of observations	428	428	291	291
Weighted number of observations		5,693		4,011
R-squared	0.060		0.138	

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

+ ORs reported, population weighted++ Long term contract =1 fix term contract=0,

Table 16. Stayers vs. movers and non-apprenticeship employed.

	net earning		long-term contract			
VARIABLES			linear		Logit+	
mover	1,589	2,426	0.102	0.150*	1.545***	2.020***
	(5,469)	(6,220)	(0.0759)	(0.0846)	(0.115)	(0.149)
stayer	7,977	9,352	0.225***	0.236**	2.889***	3.110***
	(6,144)	(6,823)	(0.0852)	(0.0919)	(0.278)	(0.259)
Industry FE	n	y	n	y	n	y
Constant	35,787	46,038*	0.737***	0.635**	2.919***	0.562
	(23,933)	(23,482)	(0.257)	(0.318)	(0.817)	(0.249)
Net number of observations	425	284	438	292	438	292
Weighted number of observations					5,828	6,734
R-squared	0.100	0.093	0.054	0.132		

Robust Standard errors in parentheses, +ORs reported, weighted regressions

*** p<0.01, ** p<0.05, * p<0.1

Controls not shown: class marks, test scores, parents education, SEN, Roma, female, 9th grade track choice, 12th grader in 4th wave, month of survey in 6th wave

Figure 1. Predicted probability of VT students having a regular job

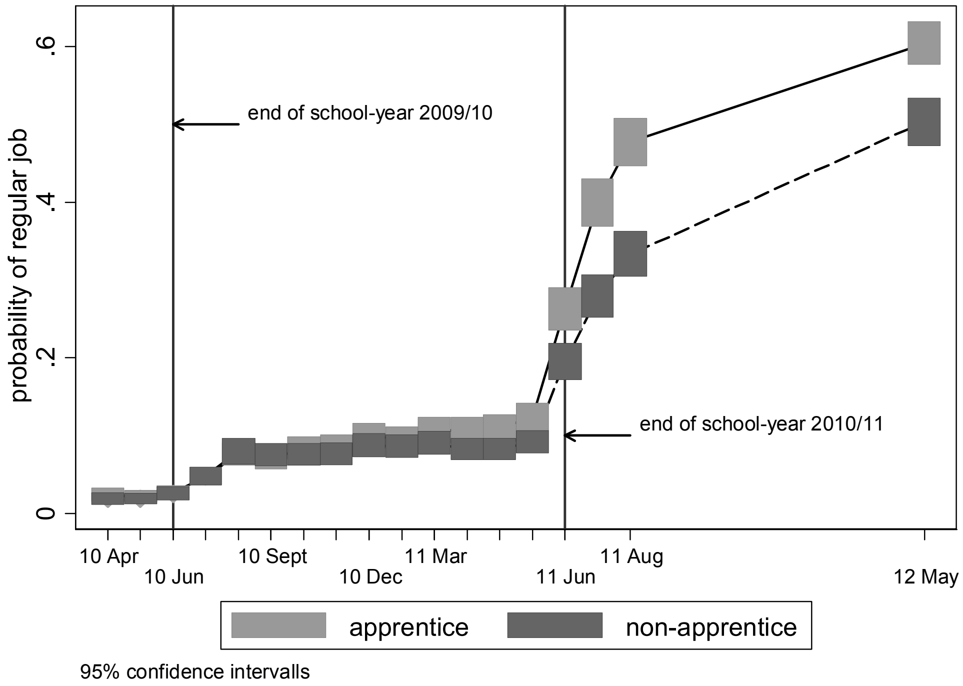
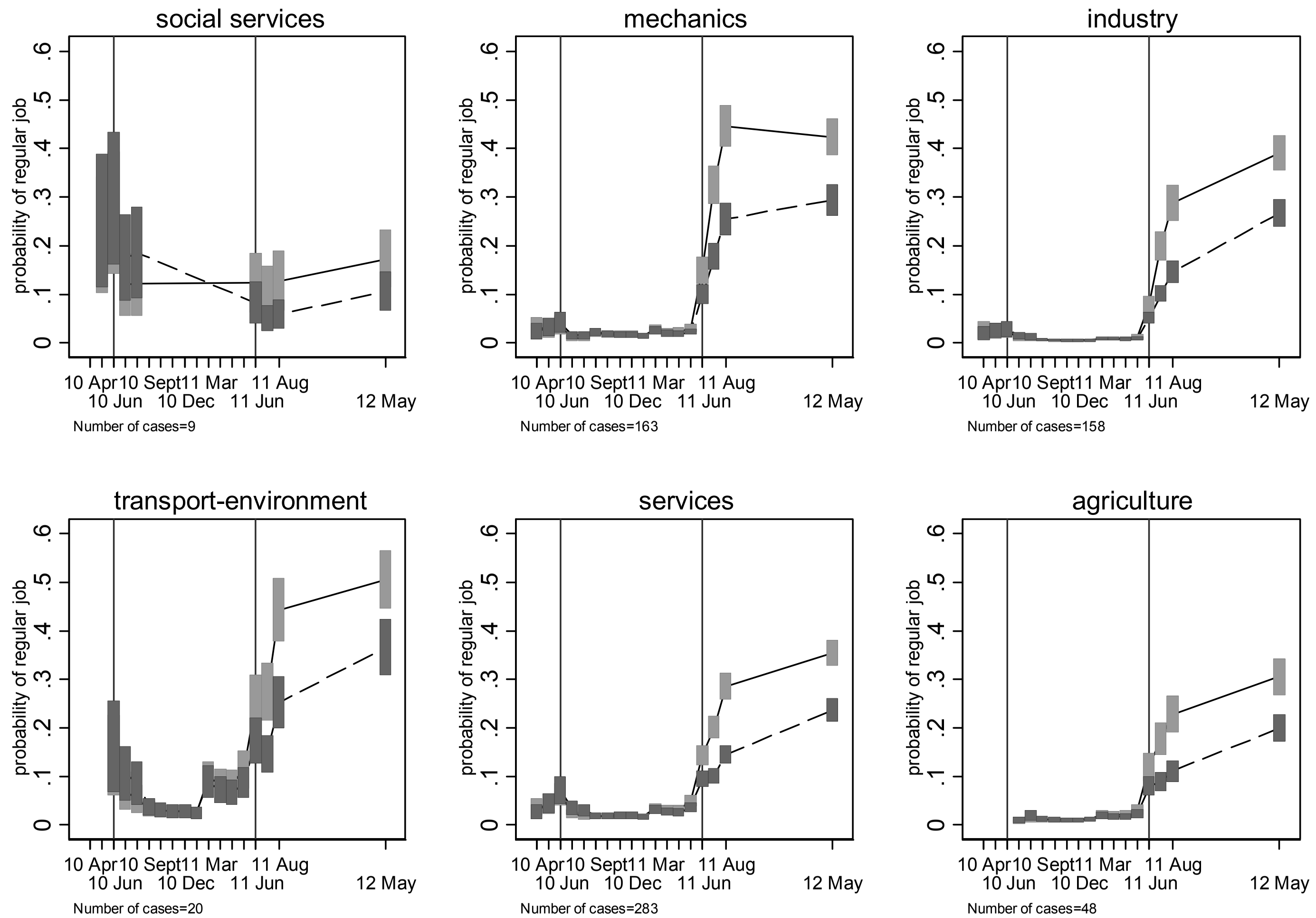


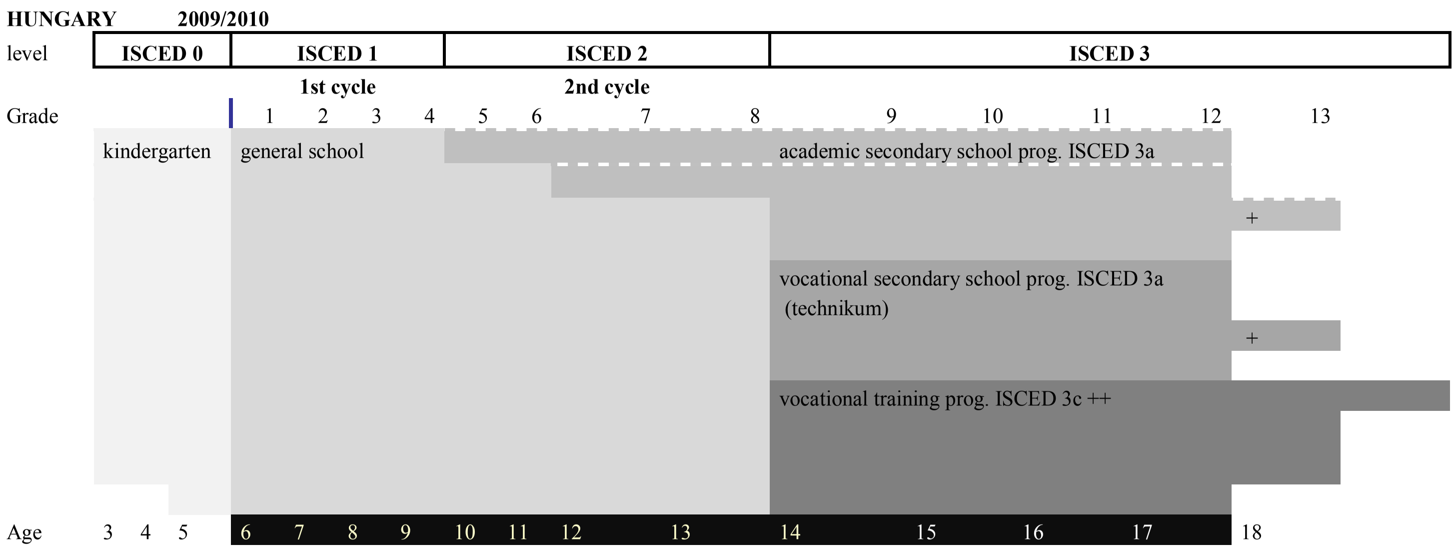
Figure 2. Predicted probability of VT students having a regular job, by types of industry



– apprentice - - non-apprentice

Appendix A

Figure A.1 The Hungarian compulsory education system



compulsory education until the age of 18 applies for the 1st graders in 1998 and later (previously and from September 2012: until the age of 16)

vocational secondary school programs curriculum includes vocational subjects and many students progress to PS voc to get a VQ

+ : some schools offer an extra grade teaching a foreign language before secondary school educ. (i.e. between grade 8 and 9)

++: some programs are also available for elementary school drop-outs

ISCED	English	national language	share
0	kindergarten	óvoda	
1,2a	general school	általános iskola	100%
3a	academic secondary school prog.	gimnázium	
3a	vocational secondary school prog.	szakközépiskola	
3c	vocational training prog.	szakiskola	

Table A1: Linear probability model with industry and apprenticeship interactions, AME of being employed vs. being unemployed, studying or other (weighted)

	(1)
VARIABLES	employed=1
Interactions	
Social services * apprentice=0	-0.0527
	(0.374)
Social services * apprentice=1	-0.123
	(0.116)
Mechanics * apprentice=0	-0.0801
	(0.0931)
Mechanics * apprentice=1	0.138
	(0.0904)
industry * apprentice=0	ref.
industry * apprentice=1	-0.00618
	(0.0938)
transport-environment * apprentice=0	0.153
	(0.232)
transport-environment * apprentice=1	0.142
	(0.179)
services * apprentice=0	-0.0965
	(0.0954)
services * apprentice=1	0.0322
	(0.0776)
agriculture * apprentice=0	-0.220*
	(0.114)
agriculture * apprentice=1	0.0675
	(0.137)
Constant	0.286
	(0.211)
Observations	681
R-squared	0.109

Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1
Controls not shown: class marks, test scores, parents education, SEN, Roma, female, 9th grade track choice, 12th grader in 4th wave, month of survey

Table A2: Does the effect of apprenticeship training differ between industries?
Significance (p-values) of F-tests, comparing the effects of apprenticeships between industries as estimated in table A1. Diagonal elements show the p-value of apprenticeship training within industries.

	social services	**mechanics**	**industry**	**transport-environment**	**services**	**agriculture**
social services	0,85					
mechanics	0,46	*0,02*				
industry	0,87	*0,09*	0,95			
transport-environment	0,90	0,43	0,99	0,97		
services	0,61	0,45	0,27	0,63	*0,09*	
agriculture	0,38	0,69	0,10	0,34	0,35	*0,06*

Table A3: Types of training and employer firms and the number of individuals

			1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	mis	total
5th wave trainer	1	Agriculture, forestry and fishing	**1**	0	1	0	1	0	1	0	0	0	0	0	0	0	0	0	0	0	1	0	0	0	0	12	17
	2	Mining and quarrying	0	**0**	1	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1
	3	Processing	6	0	**29**	2	0	2	1	0	2	0	0	0	0	0	1	1	0	0	2	0	0	0	0	56	102
	4	Electricity, gas, steam and air conditioning	1	0	0	**7**	1	2	0	1	0	0	0	0	0	0	2	0	0	0	1	0	0	0	1	14	30
	5	Water supply, wastewater collection and treatment, waste management	1	0	2	0	**0**	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	2	5
	6	Construction	10	0	17	1	1	**45**	1	1	1	1	0	0	1	1	4	0	1	0	5	0	0	0	0	81	171
	7	Trade, automotive services	1	0	15	0	0	4	**20**	2	7	0	0	0	0	0	3	0	0	0	8	0	0	0	0	70	130
	8	Transportation, warehousing	0	0	3	0	0	0	3	**1**	0	0	0	0	0	0	0	0	0	0	2	0	0	0	0	7	16
	9	Hotels and restaurants, catreing	2	0	10	0	0	1	17	0	**64**	0	0	0	0	0	0	0	1	2	5	0	0	0	0	129	231
	10	Information, communication																									
	11	Financial and insurance activities	0	0	0	0	0	0	0	0	0	0	**0**	0	0	0	0	0	0	0	0	0	0	0	0	4	4
	12	Real estate transactions																									
	13	Professional, scientific and technical activities	0	0	0	0	0	0	0	0	0	0	0	0	**0**	0	0	0	0	0	0	0	0	0	0	1	1
	14	Administrative and support service activities	0	0	0	0	0	0	0	0	0	0	0	0	0	**0**	0	0	0	0	0	0	0	0	0	2	2
	15	Administration and defense, compulsory social security	0	0	0	0	0	0	0	0	0	0	0	0	0	0	**3**	0	0	0	0	0	0	0	0	1	4
	16	Education	0	0	3	0	0	0	2	0	0	0	0	0	0	0	1	**0**	0	0	0	0	0	0	0	9	15
	17	Human health and social work	0	0	1	0	0	0	0	0	0	0	0	0	0	0	0	0	**2**	0	0	0	0	0	0	21	24
	18	Arts, entertainment and recreation	0	0	0	0	0	0	0	0	0	0	0	0	0	0	1	1	0	**0**	0	0	0	0	0	1	3
	19	Other services	2	0	7	0	3	3	5	2	8	0	0	0	0	1	3	0	1	0	**19**	0	0	0	1	70	125
	20	Households as employers, producers, and service	0	0	1	0	0	2	0	0	0	0	0	0	0	0	0	0	0	0	0	**0**	0	0	0	0	3
	21	Organizations outside Hungary																									
	22	Other																									
	23	Don't know	0	0	0	0	0	0	0	0	1	0	0	0	0	0	1	0	0	0	0	0	0	0	0	6	8
		missing	211	2	443	24	46	154	345	71	213	33	40	1	12	25	154	23	54	58	226	11	1	4	12	6,967	9,13
		total	235	2	533	34	52	213	395	78	296	34	40	1	13	27	173	25	59	60	269	11	1	4	14	7,453	10,022

Appendix B

The official list of OKJ qualifications contains 21 larger categories. I have grouped these into 6 broad categories (industries) in order to increase the number of cases within each category, but still facilitate relevant comparison between the groups.

New categories (industries)	Original categories in the national training register
Social Services	Health
	Social services
	Education
	Art, culture, communication
Mechanics	Engineering
	Electrical-engineering, electronics
	Informatics
Industry	Chemical industry
	Architecture
	Light industry
	Wood industry
	Printing industry
Transportation-environment	Transportation
	Environment and water-management
Services	Business and economics
	Management
	Trade, marketing and administration
	Catering, tourism
	Other Services
Agriculture	Agriculture
	Food industry