THE EFFECTIVENESS OF WORKPLACE-BASED TRAINING IN A NON-DUAL EDUCATION SYSTEM

Evidence on the school-to-work transition of Hungarian apprentices

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Abstract

Although apprenticeship training has been praised for its effectiveness in smoothing the school-to-work transition of non-college bound students in dual education systems, there is a lack of evidence in non-dual systems and the mechanism behind this effect is also unclear. Using a unique individual-level panel database, which includes an extensive set of controls, the study shows that Hungarian students of the non-college bound vocational training track with workplace-based training, have about 10-15% higher probability of initial employment, compared to similar graduates from the same track, who were trained in school. This effect seems to be stable across industries, and robust to specification checks. Tests using alternative outcomes – such as net earning, or length of employment contract – suggest that this positive effect is due to the screening of apprentices and not to their increased specific skills. This observation puts a question mark on the general efficacy of apprenticeship training in non-dual systems.

keywords: workplace-based training, non-dual systems, screening, school-to-work transition

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1. Introduction[1]

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; Piopiunik and Ryan 2012). Nevertheless, existing empirical research provides little information about the causal mechanisms that make the workplace-based education effective in non-dual systems. The mechanisms that explain why students, who are trained in firms, find their first job more quickly than non-apprentices are theoretically available but are empirically not well tested. This question is especially interesting in non-dual systems, where the coordinating mechanisms between the actors in the labor market and in the education system are not well developed, and thus evidence from a non-dual system where workplace-based training exists could provide important policy input on how and why workplace-based training would be effective if extended in systems with no or little apprenticeship training.

The paper improves on the existing literature in two ways. Firstly, there has been only a handful of studies that tried to show the causal effect of workplace-based training on labor market outcomes (Bonnal, Mendes, and Sofer 2002; Bertschy, Cattaneo, and Wolter 2009; Parey 2009; Noelke and Horn 2011) and all but one of them used evidence from dual education systems. Secondly, the reasons behind the improved labor market outcomes of apprentices are even less researched. This paper backs up these causal finding by adding more robust empirical support to the assumed positive causal link, and it also provides some

[1] Uncommon abbreviations in the text: Hungarian Life Course Survey (HLCS), National Assessment of Basic Competencies (NABC), vocational education and training (VET), vocational training track (VT), special education needs (SEN)

empirical evidence about the potential mechanisms that could cause the lower unemployment rate of apprentice students. While this paper supports the conclusion of previous research in that workplace-based training increases initial labor market prospects, the paper argues that this positive effect is mainly due to the screening of apprentices by the firms, and the skill-enhancing effect of apprentice training is less evident.

2. Research questions

The first aim of this paper is to demonstrate the positive effects of workplace-based training on labor market entrance by using a new individual panel database, the Hungarian Life Course Survey (HLCS). While the analyses below are not *per-se* causal, my aim is to convince the reader that controlling for a wide variety of observable individual characteristics and track and occupation and school effects tackles all important endogeneity concerns.

In Hungary every student in the "lowest" non-college bound vocational training track had to do at least two years of practical training, which could either be done in the school or at a firm (see the description of the Hungarian VET system below).[2] This study compares these two groups of students. Hence the "treatment" and the "control" groups are quite obvious: both groups have received exactly the same general training (the first two years in the vocational training program), and – provided they aim for the same occupational qualification – they have to fulfill the same requirements; the only difference between the groups 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 individual controls, which reduces the omitted variable bias concern. The database also includes information on the types of qualification that students have acquired, and on the schools of the

[2] Note that the newly enacted law on Vocational Education (2011/CLXXXVII) has changed the system extensively, but these changes fall outside the time horizon covered by this study.

students, which allows for within industry and within school analysis, that further strengthens the reliability of the estimates. Moreover, the HLCS is a panel database, which rules out the problem of reverse causality.

The second aim of the paper is to provide some empirical evidence on the potential reasons for the decreased unemployment of apprentices. Why could students benefit from workplace-based training? The literature on the relation of educational attainment and socioeconomic outcomes is vast (see Bills 2003 for a comprehensive review). Of the possible theoretical links the human capital (Becker 1994) and the screening theory (Stiglitz 1975; Acemoglu and Pischke 1999) comes closest to providing plausible but distinct explanations. Bills (2003) lists eight possible theoretical links on the association of educational attainment and socioeconomic attainment: besides the human capital and screening theories, signaling, control, cultural capital, institutional and chartering theories and the credentialist explanation are discussed in the review.

To put it simply, a human capital explanation of this link would state that students trained in firms find their initial job more quickly because of their improved specific skills, which facilitate 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. Note, however, that the other side of this argument it is less obvious: whether training in schools is less effective in providing the same specific skills, or whether schools provide different sets of skills (cf. general skills) that are useful outside the industry as well. While the former would clearly suggest the superiority of apprentice training, the latter would cast a doubt on that.

The screening argument decreases the importance of skill-differences and presses that 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 to put it differently, training firms select their future employees first from among their apprentices and then from the labor market; that is, they equate this period of vocational training with the usual probation period. This means that *even if* apprentices and non-apprentices are perfectly similar – i.e. the selection into apprenticeship is random *and* workplace-based training is not superior to school-based training – apprentices still have a higher chance of being employed right after education is over. To give an example: let's assume there are 80 available jobs for 100 students, so each has 80% chance to be employed. Also assume that of the 100 students 60 have been trained at the workplace and 40 in school. Of the 60 apprentices 48 are kept at their training firm (i.e. 80%) and the other 12 are out on the labor market with the other 40 non-apprentice. The remaining 32 jobs are randomly allocated among the 52 students on the market, which means they only have 62% chance of getting a job compared to the 80% of the apprentices. In short, even if selection into apprenticeship training and into employment is random, apprentices have 18% higher chance of getting a job, since they are selected first.[3] Although this is not strictly a casual effect - as in the human capital argument, where workplace-based training improves the employability of apprentices – a naïve comparison of the two groups would still suggest the superiority of apprenticeship training.[4]

While these two arguments are both plausible explanations for the increased initial employment of apprentices, the other six are less suitable for this problem.

[3] I have tried to use numbers that are close to reality, but of course the baseline employment probability is unknown.

[4] Note moreover that if, for instance, screening improves the employment chances of different social groups differently, workplace-based training still could be considered socially beneficial. More on this, see below.

The signaling argument would claim that apprentices carry a signal that informs the future employer about their unobservable characteristics (e.g. about their superior productivity), even if the firm is not their training firm. So while screening is demand driven, signals come from the supply side. There are two problems with this argument, as for the current paper is concerned. First, it is not possible to separate the predictions based on this argument and the ones based on the human capital argument. Both predict that apprentices have superior productivity, and hence the firm's responses should be the same. Secondly, the signaling argument assumes that the higher productivity of apprentices comes from before the apprenticeship training takes place (e.g. born with or gained during general schooling) and thus the selection into apprenticeship provides the signal. This paper argues that even if selection into apprenticeship is not random, all the important variables are controlled for. Thus if there are any difference in labor market outcomes between apprentices and non-apprentices it is not due to the pre-apprenticeship differences in individual characteristics.

The other theories reviewed by Bills (2003) all base their argument on class differences or on the elite's ability to control access to occupations or positions, and as such are less likely to be important for students with similar background and in a similar school type and especially within similar occupations.

The human capital and the screening arguments put forward different policy conclusions. If workplace-based training improves skills, its effects should be long lasting, which also suggests that workplace-based training should and could be extended and subsidized by the government. However, the assumed positive effects of screening most likely fade out quickly – as turnover of employees increases - and its benefits may not be possible to replicate, had the apprenticeship training been extended.[5] Although there might be some societal benefits of

[5] Although one could argue that in this way employers would not have to rely on school grades as signals.

screening, in that it might provide relatively bigger chance for disadvantaged students, who otherwise had bigger problems of getting a job, but it would most likely still offer smaller societal benefits and hence demand smaller governmental support.

While both the human capital and the screening arguments predict a lower initial level of unemployment for apprentices as compared to non-apprentices, there are differences in the prediction of other outcomes. The human capital argument predicts higher wages for the higher productivity of apprentices, but the pure screening argument does not. If apprenticeship training increases the specific or technologically general skills of the trainees, then firms should reward this by increasing their wages as compared to non-apprentices. On the other hand, if firms use apprenticeship training as a screening device, the offered wage of permanently employed apprentices and permanently employed non-apprentices should be the same, since their productivity is not different. A counter argument to this reasoning would be that firms might consider the training to be an allowance for the apprentice, and thus cut their starting salary accordingly, which decreases the initial wage differences between employed apprentices and non-apprentices. However, one might point out that non-apprentices should also be trained after they are employed, and thus firms should lower their salaries even more.

The screening argument puts forward a higher ratio of permanent contracts for apprentices. If firms use apprenticeship training as the probation period, they are more likely to offer apprentices permanent contracts after they hire them, since they have already done the screening. But firms might consider offering long-term contracts to any worker with high productivity; hence looking at the pure difference between apprentices and non-apprentices would be uninformative in separating the two mechanisms. However, if screening is the sole mechanism that helps to decrease the initial unemployment rate of apprentices, we should see a marked difference between apprentice “stayers” and “movers”. In other words, the screening theory predicts that those apprentices, who stay at the same firm, where they were trained, are more likely to receive long-term contracts as opposed to those, who moved to a different firm after the training period was over. So the screening argument would put forward that only “stayers” benefit from training, while “movers” are in the same position as non-apprentices. Conversely, the human capital argument would predict that “stayers” are as likely to receive long-term contracts as the “movers”, since both groups are of higher productivity and thus both groups are in a better position than non-apprentices.

3. Previous research on causal effects

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 review by 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 foci, methods, additional tests, and conclusions differ.

A study by Bonnal, Mendes and Sofer (2002) comes closest to the approach and focus of this paper. Bonnal et al. (2002) look at the French dual system and compare apprenticeship and vocational school graduates. They try to take the selection to apprenticeship into account by simultaneous maximum likelihood estimation, where they estimate the apprenticeship choice together with the other regressions on the exit from schooling, on the exit from unemployment and on staying in one’s training firm. Although their data is also an individual panel with detailed employment record for one and a half years after graduation, they can only control for the father’s employment situation and the region, and not for school achievement or ability. Their results show 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 firm other than their training 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 than the human capital model.

A similarly designed study is Bertschy, Cattaneo and Wolter (2009), who look at the Swiss dual system. They also use a panel which is connected to the PISA 2000 Swiss database, which provides standardized test scores to proxy student achievement as well as socio-economic status and other controls, and they also use simultaneously estimated equations to take selection into account. However, since the vast majority of the Swiss vocational students (over 90%) are in the dual apprenticeship training, they compare apprentices, who taken up training with “higher intellectual level”, with the others. Also their utilized outcome is not employment, but employment in “adequate job” that matches the graduates’ qualifications. Initially they find a significant difference between these two groups, which disappears after they take selection into tracks into account. 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 more likely to enroll in a vocational field with a higher intellectual level. The level of literacy does not have a direct effect on the probability of finding an adequate job, but only though the vocational track choice.

The only paper using data from a non-dual system is the one by Noelke and Horn (2011), which also uses Hungarian data, but its approach and time of investigation is different. Noelke and Horn 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 estimate a difference-in-difference model. They conclude that apprentices are less likely to be unemployed after they enter the labor market, but 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. Note that these latter findings are also more in line with the screening than with the human capital argument.

Parey (2009) also uses variation in the supply of apprenticeship places in local German 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 current paper backs up these studies, in finding a positive effect of workplace-based training on employment chances, but it further develops on the potential reasons of this causal link.

4. A non-dual system - the Hungarian VET system

While most of the studies that have addressed the question of the effectiveness of apprenticeship training are based on countries with dual systems, the Hungarian

VET is not a dual-system, which allows for a within track comparison of workplace-based and school-based training. As a non-dual system this country study should especially be important for countries with less experience in apprentice training. While in the dual systems – such as Germany, Austria, Switzerland or Denmark – the industry/business and the education sectors cooperate closely in the coordination of the vocational segment of education in non-dual systems this cooperation is less developed. Thus findings from a non-dual system, where the workplace-based training is still widely utilized, could be informative for those countries where apprenticeship training is less widely spread but its development is considered.

Also, as van de Werfhorst and co-authors (van de Werfhorst 2011b; Bol and van de Werfhorst 2011; van de Werfhorst 2011a) have pointed out different theories might explain better the education-labor market link in different countries or in different labor market settings. Specifically van de Werfhorst (2011b) argues that in dual systems the human capital theory is more adequate, since there is a stronger match between the skills acquired in education and skills needed on the workplace. In countries with less evident link between labor market and education, other indicators of skills – such as general literacy or numeracy – are more important, and thus educational attainment is less important. Although in his comparative studies (van de Werfhorst 2011b; Bol and van de Werfhorst 2011) Hungary is considered as a strongly vocationally oriented country, and as such is grouped with the dual systems, I argue that since the Hungarian system is highly decentralized with very weak links to the labor market Hungary has a non-dual education system. This feature – being non-dual but still large share of apprenticeship training within a large vocational sector – allows for an important test of mechanisms that might hold for other non-dual systems, had the share of vocational education (and specifically the workplace-based training) been extended.

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:[6] an academic track (*gimnázium*), and two vocational tracks. The vocational secondary track

[6] 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).

(*szakközépiskola*) mixes academic and vocational training and allows for tertiary entrance after graduation, while the vocational training track (*szakiskola*) is non-college bound, but vocational practical training, either in the form of school-based or workplace-based training, is compulsory. In 9th grade a little more than 35% of the cohort is in academic secondary tracks. Another 60% of students go to vocational tracks: a large majority of them (over 40% of the full cohort) enter the vocational secondary, while around 20% enter the vocational training track. The remaining less than 5% of students are dropouts, repeaters, or those students with special educational needs (SEN), who cannot be integrated with the others and thus enrolled in special vocational training. While both the academic and the vocational secondary tracks offers general training for four years - and the vocational secondary offers pre-vocational training, with usually one or two optional years of vocational practical training after the school-leaving exam – the vocational training track has so far offered only two years of general training[7] with two additional years of practical training.

This paper focuses on the 20%, who are enrolled in the vocational training (VT) track. This track is considered to be the lowest ranked in the hierarchy of tracks (but still above no-education). This paper compares VT students who have done practical training at a private firm with those, who have done practical training within schools.

Although the selection into workplace-based training might not be random, there is no central procedure that allocates students in one group or in another. In fact, the organization of the system is overly 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). It is the duty of the school to provide practical training for the student. The school can either organize the training within its boundaries (e.g. by hiring vocational teachers) or can “outsource” the training to a private firm, which can be done in groups or individually as well. The student

[7] The system has changed only recently, where general training has been reduced immensely and practical training introduced in the first two years as well.

can also organize training for her/himself at a private firm. In all of these cases a tripartite contract must be signed between the firm, the school and the student.

Firms also have (small) incentives to train students. All firms have to pay a contribution towards vocational training (a tax), which is 1,5% of the sum of the gross wages of the firm. Firm with less than 50 employees can use 60%, while larger firms 33% of this amount to train their workers, including training apprentices. Apprenticeship students have to be paid at least 20% of the minimum wage while in training,[8] which amount is deductible from the contribution towards vocational training. Some further costs, such as the foregone earning of the trainers at the firm or some material costs can also be deducted.

So Hungary is an ideal place to test the pure effect of workplace-based training in non-dual systems: not high but existing incentives for firms to train, basically non-existent compensation for apprentices and two ideal groups to compare, both of which receive the same general training but differ in their place of practical training. The only open question is, how VT students are allocated between workplace-based and school-based training. After the introduction of the HLCS data, I will address this question.

5. 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 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

[8] This amount is close to nothing. The minimum wage in 2010 was 73500HUF that is approximately 260-270 EUR/month. Correspondingly, the average amount the apprenticeship students received in our data was 15361 HUF (~55 EUR) a month with a standard deviation of 5691 HUF.

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 annual sample attrition rate, on average, is only around 5% (see Table 1).

(Table 1 around here)

The HLCS database contains detailed information on achievement (standardized literacy and numeracy scores in 8th grade from the NABC data as well as teacher given class marks in each year), ethnicity, school trajectory, family background – including parental education and employment –, 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, as well as for the imminent sample attrition. The following strata were used during the data collection, and in estimating the weights: 1) 3 settlement types: the capital and big cities, other cities, villages 2) 7 NUTS-2 regions[9] 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 4th wave of the study, the year when the median student was finishing the last year of compulsory schooling. All students in the analysis were enrolled in the vocational training track in the 4th wave. Vocational training 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.

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

Anyone, who did workplace-based training in the 4th wave or in the 5th wave of the study (the year after finishing compulsory education), is considered an apprentice.[10] The four types of labor market outcomes – employed, unemployed, studying and other[11] – are considered in the last (available) wave of the study, and are self-declared. The main reason for using the 6th wave and not the 5th wave, which is the one after the compulsory education ends, is that the vast majority of students in the 5th wave were still in education, even among the vocational training students (see Table 2). By the 6th wave the majority of vocational training graduates have entered the labor market (as employed or unemployed) and less than a quarter of them are still in school (e.g. in further training or switching to tracks leading to the school leaving exam).

(Table 2 around here)

Besides labor market outcomes, net earnings and the length of employment contract (fixed-term vs. long-term) are also used as outcome measures to test the human capital and the screening hypotheses.

Other variables that are used are the standardized test score (literacy and numeracy) in 8th grade,[12] class mark averages (1- fail to 5- excellent) in 8th and in 12th grade, gender, SEN status, Roma ethnicity, parental education and occupation. All control variables are from the first wave of the study, unless otherwise noted. Additional controls are a proxy for grade repetition (whether the student was in the 12th grade in the 4th wave of the study) and a proxy for motivation (whether her/his 9th grade school was her/his first choice). The size of the training firm (small: 1 to 12 employed, medium: 13 to 100 employed and large: over 100

[10] Although students could have done workplace-based training in the 3rd wave of the HLCS study, this information is unfortunately not available.

[11] The four possible options within the other category are: disabled, on maternity-leave, caring for family and other reasons.

[12] Note that these test scores cannot be used for the secondary level entrance, but are used to make schools accountable and to provide feedback for the teachers (see OECD 2010).

employed) is also used in some estimations. The month of survey is controlled in all estimations and is not shown. See Table 3 for descriptive statistics.

(Table 3 around here)

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 HLCS also contains information on the type of the qualification for vocational graduates, although the number of missing cases is high (see Table 4). Of the 1,471 VT students only 964 has this information in the dataset. 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 (see Table A1. In the appendix).

(Table 4 around here)

6. Selection into apprenticeship

Before addressing the effectiveness of the apprenticeship training it is essential to understand, which student chooses workplace-based and which chooses school-based training. 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 at the end of the education, 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.

In Table 5 linear probability models are used to assess the strength of association between personal traits and training provisions. The fit of the linear models can be interpreted more straightforwardly than the fit of the non-linear models and within groups weights cannot be used in fixed-effect logit models.[13]

Covariates that are significant in the first estimation (Table 5 column 1) suggest that higher skilled students are more likely to enter apprenticeship training. The within industry estimation (column 2) as well as the within school estimation (column 3) do not show these strong skill differences between apprentices and non-apprentices, suggesting that the (self)selection into occupations or more likely into schools might drive the results. That is, there might be some occupations and/or schools that attract better students (see also Bertschy, Cattaneo, and Wolter 2009). The base (column 1) and the within school estimations (column 3) also show that people with less educated parents are more likely to have practical training at private firms. The results in the most restrictive within school and within occupation model (column 4) however highlight that none of the individual traits matter if occupational differences and school and/or local labor market effects are taken into account.

(Table 5 around here)

Individual traits explain only 4% of the variation of the base model, but industry effects add an additional 4%, suggesting that students in different occupations have different chances of getting into apprenticeship (see also table 4). The inclusion of school fixed effects in the model increases the fit tremendously. The school fixed effect model explains is almost 50% of the variance, which is further increased to 73% when industry fixed effects are added. This is of course not surprising, given that the sample was not representative on the school level and thus there are on average less than 4 students per school in the sample, which further decreases when within industry effects are considered within schools.

Nevertheless, 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

[13] Using fixed effect logit models on a representative subsample of the HLCS provides substantially the same results (see Author 2013).

observable differences seem to fade away within school and within industry, suggesting that the industry and especially the school (and the local labor market) matters much more than individual traits.

7. 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. Due to the fact that the independent variables are measured before the dependent 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 6 and 7. In the first estimation (Table 6 block 1) only the apprentice variable is included, in the second (block 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 1.5-1.6 times higher odds of being employed, as opposed to being unemployed. The size of this effect is unchanged by any of the personal traits that are included in the model. On the other hand social background, gender and grade repetition matters in getting a job. It seems that students with employed fathers have much higher odds of being employed; whether this effect materializes through socialization or though social networks is not obvious. Also men are more likely to be employed and women are more likely to fall into the other category (e.g. maternity leave). Students, who have not repeated grades until 12th grade, are also more likely to be employed in the 6th wave of the study. Note, however, that none of the school achievement variables – neither the standardized test scores, nor the teacher given class marks – seem to be relevant in employment, although students with higher class marks are more likely to study than to be unemployed.

Table 7 adds further controls to the base model. Table 7 block 1 is the same as Table 6 block 3 to facilitate comparison of models. Table 7 block 2 shows the same multinomial logit model with industry fixed effects added,[14] while dummies of the training firm size are used in block 3 instead of the apprenticeship dummy. The main conclusion does not change even if these controls are added: apprentices have significantly higher odds to be employed vs. being unemployed than those with only school-based vocational practice, although the effect increases slightly.

Although it seems that apprentices in all sizes of firms have higher odds to be employed than non-apprentices, the significance of the general effect seems to be driven by the medium sized firms. Apprentices in firms with employees between 13 and 100 have almost 4 times higher odds of being employed than non-apprentices, while the corresponding odds of smaller and larger firms are 1.5 and 1.7, respectively, but non-significantly different from zero on conventional levels. This result is suggestive of the mechanism, and discussed in detail below.

(Tables 6 and 7 around here)

Table 8 shows the predicted probabilities and marginal effects of apprenticeship training. The baseline uncontrolled average probability of being employed for a VT student in 2011 is 44%. Apprentices, however have a 47.1% chance, while school-based trained students have a 39.6% 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 base model (Table 6 block 3) to predict the probabilities at the population means similar but somewhat higher percentages are gained. The predicted a probability of being employed for apprentices is 52.3%, while for the

[14] 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.

school-trained it is 41.4%; the marginal effect of being trained at a private firm is thus 10.9 % at the mean. In other words, the average apprentice has about 11% higher chance of being employed after graduation than the average non-apprentice. This effect is somewhat lower for the top of the distribution students (high class mark averages, high literacy and numeracy, and parents with secondary general or tertiary schooling) and higher for the lower status lower skilled students (low class mark averages, low literacy and numeracy, and parents' education primary or below). While the former group has 8.7% higher probability of being employed the latter has 11.5%. The marginal effects are also larger for apprentices, who were trained in mid-sized firms. The average marginal effect here is 19%, but bottom of the distribution students benefit more (25.7%) than top of the distribution students (14.5%). This result suggests that apprentice training might be more beneficial for the lower status children, although note that students in the sample – who attend vocational training schools – are already from the bottom of the social distribution, and thus the top of the distribution students might be a specially selected bunch.

Although the probability of being employed differs a lot between industries, the effect of workplace-based training remains stable across industries (see table 8). The average apprentice has about 15% higher chance of being employed as compared to a non-apprentice with similar occupational qualifications. This effect is also very stable for the top as well as for the bottom of the distribution students, suggesting that there are important compositional differences between occupations.

(Table 8 around here)

8. Robustness checks

Although reverse causality is not likely in the base model, robustness checks could further underline that the results are not driven by the model specification, by omitted variables or by the time of the measured outcome.

Firstly, apprentice and non-apprentice students were matched with propensity score matching (nearest neighbor matching) using all variables in the base model (Table 6 block 3) as well as using industry fixed effects. For propensity score matching the four category outcome had to be transformed to binary: employment chances are compared to the joint chance of the other three categories. The results – not presented here in detail – underline that average treatment effects are highly significant and a bit larger than in the multinomial logit models: apprenticeship students on average are 16.5-17% more likely to be employed, which increases to a 19% average treatment effect within industry.[15] Hence it is not the functional form specification that drives the results.

The second robustness check 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 qualities. 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. There are 16 schools with only one student in the sample. The average school has 3.7 students in the sample, which further decreases to 3.1 if industry fixed effects are included. Taking missing values as well as the variance of the outcome measure within school into account, and the fact that a representative subsample should be used due to problems of weighting in fixed-effects logistic regressions, little less than 100 schools would be left for a

[15] It differs a bit across industries: mechanics: 23,8%, industry: 11,5%; transport-environment: 30,8%, services: 19,7%; agriculture: 21,9%. These effects are not significantly different from each other.

non-linear analysis.[16] Also since the multinomial logit model with a large number of fixed effects has not yet been fully developed (see Pforr 2011), linear probability models were estimated for this robustness check.[17] The four category outcome of employed, unemployed, study or other was transformed into a binary as in the propensity score matching test (employed vs. everyone else).

The effect of apprenticeship training halves within schools, and loses its significance if school fixed-effects are included in the linear model (Table 9). Apparently, the average apprenticeship student does not have a greater chance of being employed than the average non-apprentice if they went to the same school. However this effect is driven by the size of the training firm. Apprentices, who were trained in small or in large firms, have exactly the same probability to be employed as non-apprentices, who went to their school. Students in mid-sized firms on the other hand enjoy a considerable 15-20% higher probability, even if they are from the same school and their individual traits as well as vocational qualifications are the same. This finding is especially interesting in light of the potential mechanisms that could explain the superior effectiveness of the apprenticeship training. Either medium sized firms train students better than their smaller *and* their larger competitors, or they screen students more effectively than the others.

(Table 9 around here)

The third robustness check sheds a bit of light on the firm-size puzzle. Figures 1 and 2 use 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

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

[17] Non-linear robustness checks, with similar results are in Author (2013).

(the start of the previous 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 marginal effect of apprenticeship training 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, filled out the survey in May 2012 and have qualification from the “average” industry. The dependent variable is 1, if the student had a regular job and 0 otherwise. Note that the outcome in May 2012 is the variable that was used as in the estimations above.

It seems that apprentices are much more likely to find a regular job right after the end of the school year. The marginal effect of apprenticeship training increases dramatically after the end of school during the summer months, and declines afterwards. This indicates that apprentice VT students have a quicker transition to the labor market than the non-apprentice VT students, but also that their advantage slowly evaporates.[18] The initial effect is also quite sizeable: it is around 19% in August 2011, decreasing to 14% in 2010 May.

Figure 2 underlines that students trained in mid-sized firms are the ones, who really benefit from apprenticeship training. Apprentice students, who were trained in small firms, do not enjoy a higher probability of being employed, not even right after the school. Although the size of the effect is around 11%, which remains constant through the year, it is non-significantly different from zero on conventional levels. Conversely, apprentices in large firms seem to be hired right after graduation, and their advantage over non-apprentices are as large as 25% at the end of the summer, but this advantage drops rapidly, and loses its significance by the beginning of the next summer. Students in mid-sized firms, however, keep

[18] See also a study by Plug and Groot (1998) showing that there are no long run differences in labor market outcomes between tracks.

their significant advantage during the whole observation period, although the size of the effect also drops mildly.

(Figure 1 and 2 around here)

Whether this effect is due to the superior specific skills that apprentices gained while being trained at the mid-sized firms, or due to the screening of these firms, is not entirely clear from these figures. Note however that the screening argument would predict an immediate and large difference between the groups – because training firms hire the best candidates right after the training – which effect should converge by time, as firms hire new employees. The human capital argument, on the other hand, 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 necessary for employment. The figures, thus, support the screening rather than the human capital arguments. Also it is likely that small firms rely less on institutionalized screening processes, as they are more likely to use their social networks to recruit apprentices and thus rely less on this "probation period" and more on other information channels (e.g. take relatives or friends as apprentices in the first place). Conversely, medium or large firms are more likely to have institutionalized mechanisms for selecting apprentices and new employees. Moreover, turnover at a large firm is probably larger, at least in sheer numbers, which suggests that the potential advantage that an apprentice can gain from being selected early diminishes quickly as the firm hires new workforce.

In order to see whether the screening or the human capital argument comes closer to reality, other outcomes can also be studied.

9. Other measures of labor market success

9.1 Full sample

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 for only 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 (~314 Euro). Table 10 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. Although one might argue that non-apprentices have to be trained as well, and hence the firm might consider cutting their salary too. Also 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, so the effect of apprentice training on observed earnings is likely to be downwardly biased. 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 – the difference between apprentices and non-apprentices can be more properly tested. Apparently, there seems to be no difference between the earnings of the two groups at all. Even if the apprentices are split into three groups by the size of their training firm, differences between earnings between the four groups are not-significant, or are significant only on the 10% level, and are very small.[19] The results are especially convincingly zero if we look at the permanently employed. Students with long-term contracts seem to receive the same amount of money independent of their training place. This no-difference in net earnings between apprentices and non-apprentices suggests that employment differences are not due to skill differences between the groups but due to something else.

Table 11 regresses the dummy of a long-term (permanent) 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 11, column 1). The effects are substantively the same even if industry fixed effects are included (table 11, columns 2). This effect is driven by the small and the medium sized firms. Large firms are not more likely to offer long-term contracts to apprentices than to non-apprentices.

These results suggest that the screening might be 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 did not matter, the chance for non-apprentices to get a long-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, as a screening device.

[19] Note that earnings are in Hungarian Forints and are monthly. At the time of the survey the conversion rate between Euro and Forint was around 270 HUF/EUR, which means that a difference of 10.000 HUF would be a difference of around 37 EUR per month.

However, firms might offer long-term contracts to any high productivity individuals, and thus compensate for high productivity. If apprentices are of higher skills, they are of higher productivity and thus we observe their higher likelihood of getting permanent contracts.

9.1 "Stayers" and "movers"

To separate the two effects more precisely, the apprentice "stayers" and "movers" should be separated. If screening is more important than special skills, "stayers", i.e. those who get their first job at the firm where they were trained, might be more likely employed – as in the case of France (Bonnal, Mendes, and Sofer 2002) – and more likely to receive a permanent contract than "movers", who switch to another firm, after the training is over. If, however, increased skills are more important, there should be no difference between these two groups of students. Also, if only screening matters, "movers" should not be in a better position than non-apprentices, but if workplace-based training increased their skills, "movers" should dominate the non-apprentice group.

Unfortunately the HLCS does not contain information about the exact firm of the apprenticeship. Only the type of the industry of the firm[20] during the apprenticeship, as well as the type of the industry of the first job, is surveyed, and only from 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". It is highly 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 at the firm where they were trained. Nevertheless, even if some of the

[20] 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.

observed “stayers” are in fact “movers” (in that they switch firms within industry) the estimated differences will be downwardly biased.

Table A2 in the appendix shows the number of students within the different apprenticeship/employer type categories. Naturally, since this variable is only available for those who were apprentices in the 5^{th} wave and got a job in the 6^{th} wave, employment probabilities cannot be analyzed, but the effects in terms of net earnings and long-term contracts can be estimated.

As a first step differences in individual traits between the three groups (stayers, movers and non-apprentices) are shown in Table 12. The multinomial logit estimation underlines that there are only minor differences between the three groups. The few differences are significant on the 10% level only. Nevertheless it is still possible that there are unobserved differences between the groups, which might drive the results: e.g. stayers might be in general more loyal, which the firm recognizes and rewards it with long-term contracts.

Assuming that these unobserved traits are non-existent or small, the results in table 13 seem to underline that screening has an important effect: “stayers” have a much higher chance of receiving a long-term contract as opposed to either “movers” or to employed non-apprenticeship students. On the other hand the advantage of “movers”, as opposed to non-apprentices, is not significant. Also differences in net-earning – again – are not significant, which downplays the importance of skills.

10. Conclusion

Although workplace-based training has long been praised for its effectiveness in preparing non-college bound youth for the labor market, there are only a few studies that look at this question in a non-dual education setting and only a handful of studies try to show that the observed association between apprenticeship training and higher initial employment probability is causal. What is more, the mechanism behind the assumed causal effect is not at all clear. This paper underlines the causal finding of the dual systems that workplace-based training improves initial employment chances of apprentices in the non-dual setting of Hungary, but argues that the observed effect are due to the increased screening of firms and not due to the increased specific skills of apprentices.

In particular, the results of this study show that Hungarian vocational training graduates, who have done their practical training at private firms, are around 10-15% more likely to be employed after they finish education, than those who had their practical training in schools and are otherwise similar to the workplace-based group. The effect is net of individual skills, school attainment, parental background, motivation, gender and ethnicity, and robust to the inclusion of industry fixed effects, and for school fixed effects but only for students trained in mid-sized firms. Also results show that the significant marginal effect of apprenticeship training declines rapidly for students trained at large firms, while this decline is less marked in medium or small firm trained apprentices, suggesting that large turnover could eliminate the positive effects of apprenticeship training more quickly.

There seems to be no difference between the net earnings of employed apprenticeship and non-apprenticeship students. 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 non-apprentices. All of these findings suggest that there are no significant skill differences between these two groups, but firms might use the training period as a probation period, as a screening device.

Comparing those who have stayed at the same industry 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. On the other hand “movers” are not significantly more likely to get a long term contract as opposed to non-apprentices. This also implies that screening plays an important role in apprenticeship training.

While this study underlines most of the findings of the literature by arguing that the positive effect of workplace-based training on initial employment probability is causal, the argument that this effect is due to screening and not to increased skills puts forward a less favorable policy consequence for apprenticeship training in non-dual education systems. If, indeed, the effect is solely due to screening, its observed positive effects might fade out quicker and its benefits may not be possible to replicate, had the apprenticeship training been extended. Thus policy should seriously reconsider its benefits and hence its support.

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Table 1. The predominant form of organization of VET

	School-based	Dual system
Bulgaria	X	
Czech Republic		X
Estonia	X	
Hungary		X
Latvia	X	
Lithuania	X	
Poland	X	x
Romania		
Slovakia	X	x
Slovenia	x	X

source: Kogan et al. (2008, p. 22)

Table 2. Training enterprises as % of all enterprises

	2005	2010
United Kingdom	90	80
Norway	86	n.a.
Denmark	85	n.a.
Austria	81	87
Sweden	78	87
Finland	77	74
Netherlands	75	79
France	74	76
Slovenia	**73**	**68**
Czech Republic	**72**	**72**
Luxembourg	72	71
Germany (until 1990 former territory of the FRG)	69	73
Estonia	**67**	**68**
Ireland	67	n.a.
Belgium	63	78
Slovakia	**60**	**69**
European Union (27 countries)	60	66
Croatia	n.a.	57
Cyprus	51	72
Hungary	**49**	**49**
Spain	47	75
Lithuania	**46**	**52**
Malta	46	54
Portugal	44	65
Romania	**40**	**24**
Latvia	**36**	**40**
Poland	**35**	**22**
Italy	32	56

Bulgaria	**29**	**31**
Greece	21	28

source: Eurostat 2013, table: trng_cvts02

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%)

Note: LM = Labor Market
* The sample was selected from a population of 108932 students taking the NABC test, from whom 37027 students have indicated to be available for such a panel study. Of the initial 10000 sample 1484 were unsuccessful for various reasons (the most populous reasons are: refuse to answer: 726, not available during the survey period: 143, moved: 131, four unsuccessful approaches: 143) and thus additional sample units from the given sampling unit was approached (more on this see Kézdi, Molnár, and Medgyesi 2007, in Hungarian)

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

	5th wave						6th wave					
	work	unempl.	study	other	missing	Total	work	unempl.	study	other	missing	Total
academic	70	54	1717	62	172	2075	187	95	1419	85	289	2075
%	3,37	2,6	82,75	2,99	8,29	100	9,01	4,58	68,39	4,1	13,93	100
voc.sec.	106	115	2037	62	158	2478	452	303	1219	161	343	2478
%	4,28	4,64	82,2	2,5	6,38	100	18,24	12,23	49,19	6,5	13,84	100
voc.tr.	148	189	958	62	114	1471	541	290	286	123	231	1471
%	10,06	12,85	65,13	4,21	7,75	100	36,78	19,71	19,44	8,36	15,7	100
spec.voc.tr.	23	34	191	12	26	286	60	45	108	25	48	286
%	8,04	11,89	66,78	4,2	9,09	100	20,98	15,73	37,76	8,74	16,78	100
missing	252	418	906	246	1890	3712	508	408	515	262	2019	3712
%	6,79	11,26	24,41	6,63	50,92	100	13,69	10,99	13,87	7,06	54,39	100
Total	599	810	5809	444	2360	10022	1748	1141	3547	656	2930	10022
%	5,98	8,08	57,96	4,43	23,55	100	17,44	11,38	35,39	6,55	29,24	100

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

		Vocational training students only				
Variable	obs.	weighted obs.	mean	s.d.	min.	max.
apprentice, 4th or 5th wave	1183	15048	0.60	0.49	0	1
math test score (std.), 8th grade	1087	14180	-0.83	0.68	-2.74	2.10
reading test score (std.), 8th grade	1217	15447	-0.92	0.68	-3.78	1.21
class mark average, 8th grade	1170	14883	3.18	0.53	1	5
class mark average, 12th grade	1217	15447	3.32	0.58	2	5
female	1194	15143	0.35	0.48	0	1
SEN student	1216	15437	0.09	0.29	0	1
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.: vocational	1214	15412	0.48	0.49	0	1
parents' ed.: secondary	1214	15412	0.25	0.43	0	1
parents' ed.: tertiary	1214	15412	0.05	0.22	0	1
father employed, 4th wave	1215	15424	0.52	0.50	0	1
father unemployed, 4th wave	1215	15424	0.23	0.42	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

note: all data are available for 964 students, corresponding to a weighted number of 12649 students.

Table 4.: Number and percentage of VT students in school- and workplace-based training by industry

Industry	school-based	work-based	missing	Total
social services	3	6	0	9
%	33,33	66,67	0	100
mechanics	108	112	4	224
%	48,21	50	1,79	100
industry	124	106	2	232
%	53,45	45,69	0,86	100
transport-environment	13	19	0	32
%	40,63	59,38	0	100
services	121	267	7	395
%	30,63	67,59	1,77	100
agriculture	43	29	0	72
%	59,72	40,28	0	100
missing	178	296	33	507
%	35,11	58,38	6,51	100
Total	590	835	46	1471
%	40,11	56,76	3,13	100

Table 5: Selection into apprenticeship – linear probability models

	(1)	(2)	(3)	(4)
class mark average, 8th grade	0.0914**	0.0321	0.0289	-0.0428
	(0.0413)	(0.0505)	(0.0444)	(0.0794)
class mark average, 12th grade	-0.0124	-0.0118	0.0611	0.0925
	(0.0334)	(0.0396)	(0.0377)	(0.0626)
math test score (std.), 8th grade	-0.0141	-0.00424	-0.0280	-0.0182
	(0.0318)	(0.0378)	(0.0318)	(0.0530)
reading test score (std.), 8th grade	0.0624**	0.0337	0.0263	-0.0380
	(0.0308)	(0.0406)	(0.0338)	(0.0652)
parents' ed.: primary or below	0.0866*	0.0689	0.134***	0.0927
	(0.0482)	(0.0625)	(0.0513)	(0.0912)
parents' ed.: secondary or higher	0.0151	0.0260	0.00147	0.0226
	(0.0437)	(0.0521)	(0.0456)	(0.0782)
father employed, 4th wave	-0.0553	-0.0292	-0.0711	-0.105
	(0.0437)	(0.0526)	(0.0487)	(0.0877)
father unemployed, 4th wave	-0.0270	-0.00995	-0.0469	0.00422
	(0.0508)	(0.0667)	(0.0539)	(0.0911)
SEN student	0.0294	0.0210	0.0795	0.0878
	(0.103)	(0.0935)	(0.103)	(0.172)
Roma	-0.0429	-0.0581	-0.0223	0.103
	(0.0647)	(0.0862)	(0.0655)	(0.119)
9th grade track is first choice	0.0323	0.0775	0.0531	0.104
	(0.0426)	(0.0549)	(0.0441)	(0.0743)
12th grader in 4th wave	0.142***	0.151***	0.0481	0.0822
	(0.0476)	(0.0547)	(0.0483)	(0.0768)
female	0.0159	-0.0680	-0.0230	0.00427
	(0.0406)	(0.0581)	(0.0421)	(0.0856)
Constant	0.270	0.412**	0.267	0.305
	(0.168)	(0.209)	(0.171)	(0.319)
Observations	968	679	961	679
R-squared	0.0394	0.089	0.484	0.733
Industry FE	no	yes	no	yes
School FE	no	no	yes	yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
month of survey is controlled for

Table 6: Effects of apprenticeship training, base model - multinomial logit model, odds of being employed, studying or other wrt. being unemployed

	(1)			(2)			(3 – the base model)		
	work	study	other	work	study	other	work	study	other
apprentice, 4^{th} or 5^{th} wave	1.489**	1.149	1.226	1.457*	0.975	1.179	1.648**	1.106	1.211
	(0.264)	(0.232)	(0.319)	(0.283)	(0.216)	(0.324)	(0.335)	(0.253)	(0.401)
class mark average, 8th grade				1.186	1.367	1.373	1.210	1.347	1.384
				(0.258)	(0.325)	(0.438)	(0.265)	(0.312)	(0.534)
class mark average, 12th grade				1.136	1.636**	1.597*	1.056	1.618**	1.344
				(0.197)	(0.323)	(0.386)	(0.188)	(0.320)	(0.436)
math test score (std.), 8th grade				1.170	1.062	0.720	0.963	0.972	0.974
				(0.221)	(0.216)	(0.184)	(0.184)	(0.203)	(0.272)
reading test score (std.), 8th grade				0.724*	1.023	0.897	0.797	1.064	0.681
				(0.128)	(0.209)	(0.233)	(0.144)	(0.217)	(0.203)
parents' ed.: primary or below							0.624*	0.529**	0.789
							(0.167)	(0.157)	(0.317)
parents' ed.: secondary or higher							0.985	1.243	1.534
							(0.235)	(0.341)	(0.712)
father employed, 4th wave							1.841**	1.247	1.707
							(0.443)	(0.343)	(0.693)
father unemployed, 4th wave							0.927	0.685	0.839
							(0.254)	(0.216)	(0.365)
SEN student							0.807	0.851	8.19e-08***
							(0.413)	(0.490)	(5.52e-08)
Roma							0.877	1.071	3.538***
							(0.289)	(0.433)	(1.525)
9th grade track is first choice							1.015	1.017	1.084
							(0.231)	(0.259)	(0.406)
12th grader in 4th wave							1.851**	0.603*	0.730
							(0.483)	(0.161)	(0.279)
female							0.539***	0.987	10.18***
							(0.124)	(0.257)	(4.456)
Constant	1.487***	0.923	0.349***	0.501	0.0786***	0.0190***	0.372	0.154*	0.00588***
	(0.193)	(0.137)	(0.0640)	(0.415)	(0.0754)	(0.0207)	(0.328)	(0.158)	(0.00820)
Observations	1,183	1,183	1,183	1,025	1,025	1,025	964	964	964

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

Table 7: Effects of apprenticeship training, industry FE - multinomial logit model, odds of being employed, studying or other wrt. being unemployed

	(1 – the base model)			(2)			(3)		
VARIABLES	work	study	other	work	study	other	work	study	other
apprentice, 4th or 5th wave	1.648**	1.106	1.211	1.826**	0.978	1.329			
	(0.335)	(0.253)	(0.401)	(0.479)	(0.275)	(0.594)			
apprentice firm size, small (1-12)+							1.496	0.790	0.983
							(0.477)	(0.276)	(0.637)
apprentice firm size, medium (13-100)+							3.926***	2.275*	5.083**
							(1.663)	(1.039)	(3.246)
apprentice firm size, large (100+)+							1.703	0.937	0.799
							(0.597)	(0.381)	(0.573)
Constant	0.372	0.154*	0.00588***	0.103	0.563	0.0572	0.0739*	0.457	0.0450
	(0.328)	(0.158)	(0.00820)	(0.153)	(0.863)	(0.148)	(0.112)	(0.705)	(0.109)
Industry FE	no	no	no	yes	yes	yes	yes	yes	yes
Observations	964	964	964	679	679	679	670	670	670

Standard error in parentheses, *** p<0.01, ** p<0.05, * p<0.1, ORs reported, reference category is *unemployed*. The month of the survey is controlled for. All variables - as in Table 6 block 3 - are included in the models and are not shown. +Reference category is the non-apprentice

Table 8: Predicted probabilities and marginal effects

	Predicted probability		Marginal effect		
	school-based training	workplace-based training	workplace-based training		
	at population mean		at population mean	For a low achiever low status student*	For a high achiever, high status student**
uncontrolled	39.6	47.1	-	-	-
base model	41.4	52.3	10.9	11.5	8.7
with industry FE	35.6	50.4	14.8	14.9	13.4
at a medium size firm	34.9	53.9	19.0	25.7	14.5
	within industry				
	at population mean		at population mean	at industry mean	
social services	13.9	23.0	9.4	14.6	
mechanics	39.8	54.9	15.2	14.8	
industry	36.7	51.6	15.0	15.2	
transport-environment	44.9	60.3	15.3	15.3	
services	33.7	48.2	14.7	15.3	
agriculture	28.0	41.5	13.8	15.3	

* class mark averages =2, standardized test scores=-1, highest parental education= primary or below, non-SEN, non-Roma, non-repeater, current track is first choice, male
** class mark averages =5, standardized test scores=1, highest parental education= secondary or above, non-SEN, non-Roma, non-repeater, current track is first choice, male

Table 9: Robustness check with industry and school fixed effects – linear probability models

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
apprentice, 4th or 5th wave	0.0986***	0.127***	0.0560	0.0570				
	(0.0380)	(0.0464)	(0.0477)	(0.0629)				
apprentice firm size, small (1-12)					0.0744	0.0993*	-0.0117	0.0170
					(0.0477)	(0.0567)	(0.0576)	(0.0729)
apprentice firm size, medium (13-100)					0.182***	0.183***	0.190***	0.156*
					(0.0518)	(0.0675)	(0.0629)	(0.0877)
apprentice firm size, large (100+)					0.0559	0.120*	0.0131	0.0467
					(0.0544)	(0.0642)	(0.0696)	(0.0842)
Constant	0.365**	0.0342	0.442**	0.110	0.363**	-0.0176	0.441**	0.0614
	(0.164)	(0.236)	(0.183)	(0.259)	(0.166)	(0.236)	(0.186)	(0.268)
Observations	964	675	957	670	936	666	929	661
R-squared	0.093	0.105	0.440	0.498	0.099	0.109	0.451	0.498
Industry FE	no	yes	no	yes	no	yes	no	yes
School FE	no	no	yes	yes	no	no	yes	yes

Dependent variable is employed=1, unemployed, studying or other =0, all variables as in the base model (Table 6 block 3) are controlled for
Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1

Table 10. Other labor market outcomes: net earnings – linear models

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
									long-term employed	
VARIABLES	net earning		with reservation wage		net earning		with reservation wage		net earning	
apprentice, 4th or 5th wave	1,058	2,148	1,260	4,234					-1,807	
	(4,449)	(5,415)	(3,027)	(3,817)					(6,262)	
apprentice firm size, small (1-12)					-701.7	-2,264	774.8	1,532		-1,183
					(4,786)	(6,423)	(3,515)	(4,517)		(7,793)
apprentice firm size, medium (13-100)					-2,409	435.6	-1,698	172.3		-2,788
					(4,632)	(6,948)	(3,466)	(5,042)		(7,608)
apprentice firm size, large (100+)					9,980	11,268	7,215	11,939*		-2,729
					(9,168)	(11,789)	(5,868)	(7,216)		(7,280)
Constant	44,557*	61,191**	45,785***	58,918***	64,999***	69,071**	54,060***	59,146***	112,274***	112,413***
	(24,060)	(28,073)	(16,047)	(18,620)	(19,483)	(27,426)	(15,358)	(18,796)	(26,616)	(26,989)
Observations	414	283	889	620	407	280	864	612	196	194
R-squared	0.093	0.094	0.062	0.061	0.106	0.103	0.060	0.068	0.090	0.091
Industry FE	no	yes	no	yes	no	yes	no	yes	yes	yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
all variables as in the base model (Table 6 block 3) are controlled for

Table 11. Other labor market outcomes: long term contract – linear probability models

	(1)	(2)	(3)	(4)
apprentice, 4th or 5th wave	0.161***	0.200***		
	(0.0593)	(0.0725)		
apprentice firm size, small (1-12)			0.149**	0.276***
			(0.0735)	(0.0871)
apprentice firm size, medium (13-100)			0.225***	0.268***
			(0.0717)	(0.0891)
apprentice firm size, large (100+)			0.126	0.0699
			(0.0817)	(0.0934)
Constant	0.772***	0.443	0.858***	0.538
	(0.262)	(0.464)	(0.261)	(0.492)
Observations	428	291	419	288
R-squared	0.060	0.142	0.070	0.161
Industry FE	no	yes	no	yes

Robust standard errors in parentheses

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

all variables as in the base model (Table 6 block 3) are controlled for

Table 12. Stayers vs. movers vs. non-apprentices– multinomial logit

VARIABLES	(1) stayer	(3) non-apprentice
class mark average, 8th grade	0.976	1.440
	(0.254)	(0.385)
class mark average, 12th grade	1.420	0.790
	(0.323)	(0.186)
math test score (std.), 8th grade	1.165	1.475*
	(0.258)	(0.333)
reading test score (std.), 8th grade	0.850	0.693*
	(0.180)	(0.153)
parents' ed.: primary or below	0.567	0.509*
	(0.221)	(0.207)
parents' ed.: secondary or higher	0.631*	0.745
	(0.173)	(0.207)
father employed, 4th wave	0.692	1.040
	(0.207)	(0.328)
father unemployed, 4th wave	0.593	1.074
	(0.223)	(0.413)
SEN student	2.418	1.123
	(1.732)	(0.945)
Roma	1.090	1.378
	(0.639)	(0.820)
9th grade track is first choice	1.143	0.664
	(0.331)	(0.185)
12th grader in 4th wave	0.663	1.137
	(0.218)	(0.403)
female	0.680	1.412
	(0.205)	(0.412)
Constant	0.691	0.651
	(0.760)	(0.734)
Observations	412	412

reference is 'mover'

ORs reported, standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 13. Stayers and movers vs. non-apprentice employed – linear probability models

	(1)	(2)	(3)	(4)
VARIABLES	net earning		long-term contract	
mover	-219.7	582.4	0.0931	0.0943
	(6,733)	(6,456)	(0.0891)	(0.0920)
stayer	5,862	7,243	0.235***	0.225**
	(6,004)	(7,377)	(0.0841)	(0.0918)
Constant	31,870	30,489	0.638*	0.648*
	(25,692)	(27,000)	(0.349)	(0.361)
Observations	243	241	249	247
R-squared	0.129	0.135	0.122	0.142
Industry FE	no	yes	no	yes

Robust standard errors in parentheses

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

all variables as in the base model (Table 6 block 3) are controlled for

Figure 1. Marginal effect of apprentice students having a regular job

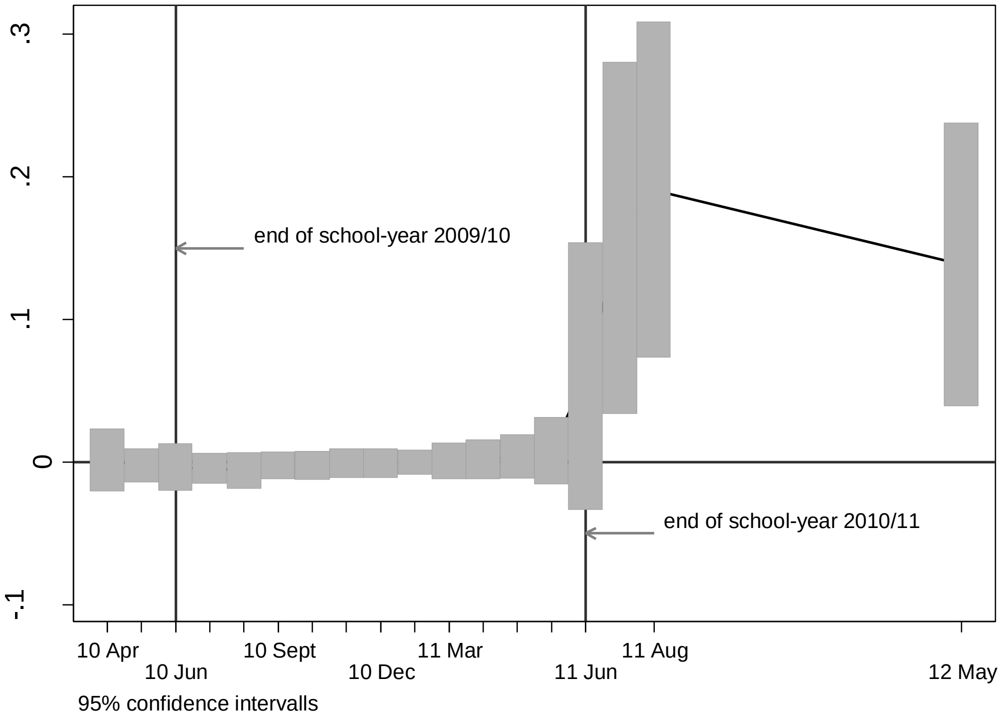
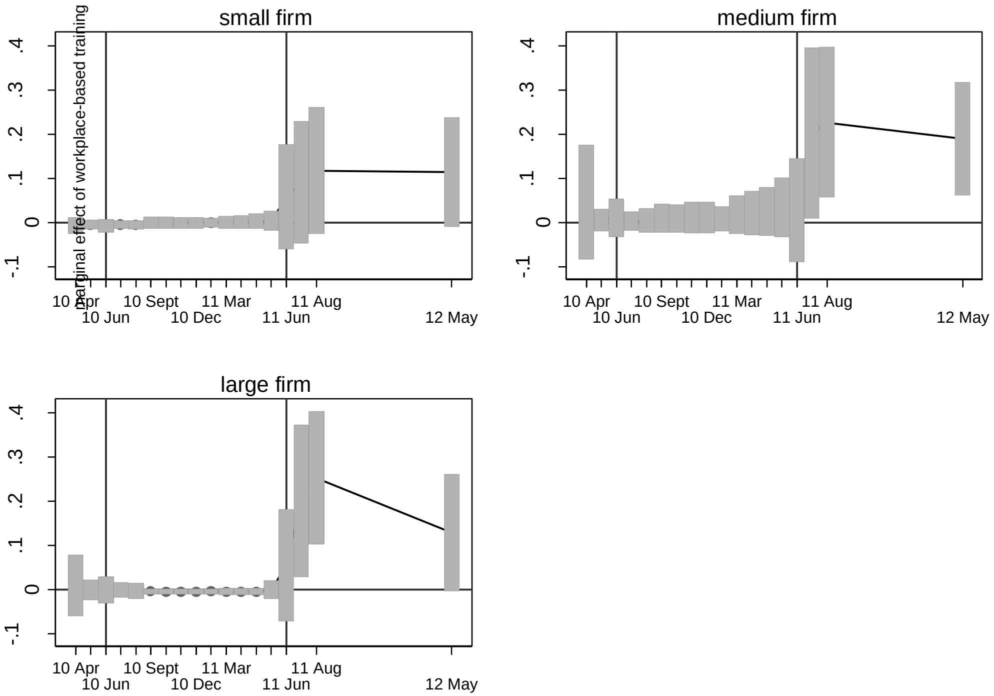
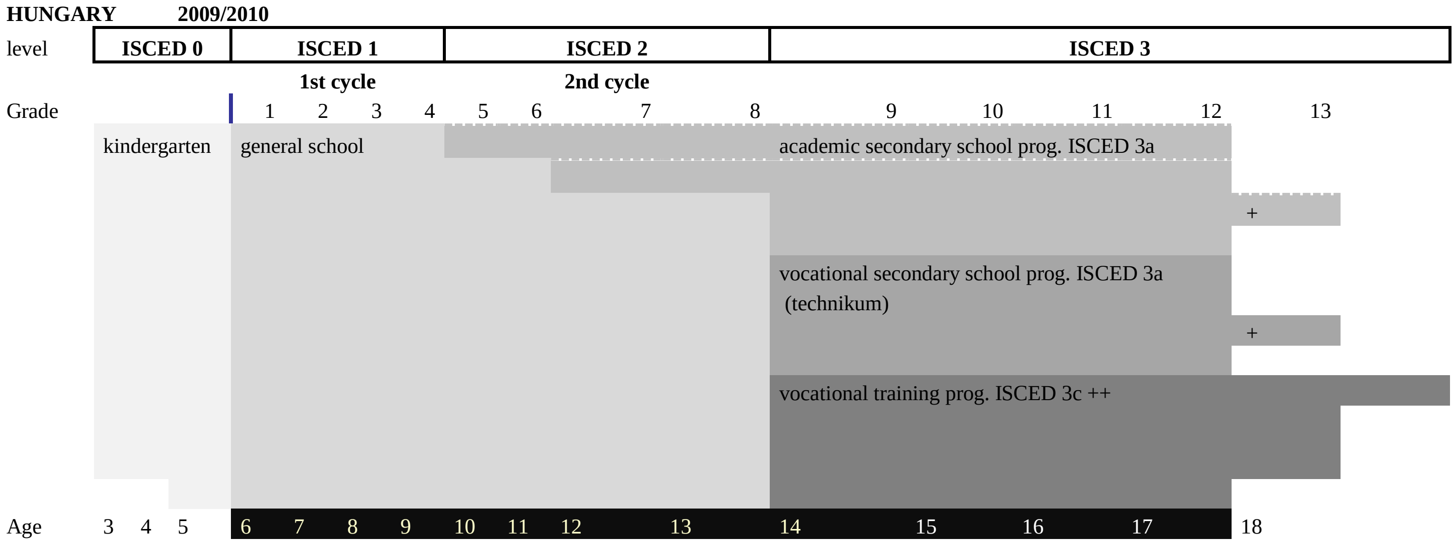


Figure 2. Marginal effect of apprentice students having a regular job in different firm sizes



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: Old and new categories of the national training register (OKJ)

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

Table A2: 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