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Do Human Arts Really Offer a Lower Return to Education?*

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Abstract

Is the wage gap between majors in human arts and other fields caused by the education? If the educational choice is endogenous, the wage gap may instead be caused by selection. We document that individuals' educational choice is correlated with that of older students and by the concentration of women in their high school. Conditional on high school fixed effects, these characteristics are unlikely to affect post-university wages and are plausible instruments for the educational choice. Our 2SLS estimates reveal that the gap in returns to education is negligible, implying that the wage gap is attributable to selection.

Keywords: Returns to education, human arts, instruments

JEL Classification codes: I21, J24

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1 Introduction

The notion that education is a key determinant of individual productivity has a long and distinguished history in economics, going back at least to the work of Mincer (1958), Houthakker (1959) and Miller (1960). At the conceptual level one may distinguish between three dimensions of a formal education which hold the potential to affect individual productivity: The quantity of education, the quality of education, and the subject matter studied.

While the quantity of education can be measured by years of schooling, the quality of education is harder to account for. Still, one may attempt to gauge the impact from quality, by adding reasonable proxies to otherwise standard wage regressions, such as test score results. Alternatively, one may try to infer the impact from quality by including characteristics of the school attended in earnings regressions (e.g. pupil/teacher ratios and school size). As is well known, standard theories would predict a positive impact from both of these dimensions of education on individual productivity (Becker, 1967), as well as on macroeconomic outcomes (e.g. Lucas, 1988). This proposition has been tested (and debated) intensely over the years.[1]

The third dimension of human capital accumulation, which has received considerably less attention by academic researchers, is what we focus on in the present study. The issue is whether the particular field of study, or the contents of the curriculum, has a separate impact on individual productivity. Existing studies, surveyed below, suggests this is the case. A typical finding is that the labor market pay-off from pursuing an education within the humanities is substantially smaller than that associated with most other types of education. For example, OLS estimates for Denmark, reported below, suggests the wage rate earned by individuals with a tertiary education within the humanities is 22% lower than that associated with other tertiary degrees.[2]

[1] See Card (1999, 2001) for a review of the literature which attempts to estimate the causal impact from an additional year of schooling on individual wages; Card and Krueger (1996) review the literature on the impact from school quality on labor market outcomes at the level of the individual. Bills and Klenow (2000) provide an analysis of the education/growth nexus at the aggregate level; Hendricks (2002) examines the contribution from quality differences in human capital in accounting for cross-country wage differences.

[2] We refer to the groups under consideration as having obtained a "tertiary" education. Note, however, that all individuals in our sample below attained a master degree. Hence, the number of years of schooling for all individuals in our sample is rather homogenous.

These findings could suggest that some types of education provide the individual with more productive human capital than others. At the same time, large wage premia across different fields of study are somewhat puzzling. If wage differentials (of considerable magnitude) appear one would *a priori* expect changes in the distribution of students across fields of study; a process that would continue (in theory) until wages are equalized.

An alternative explanation for the above mentioned findings is that existing OLS estimates are not identifying the impact of different types of education on wages. Instead, the results may be attributed to a lack of control for differences in relative cognitive abilities, or, "comparative advantages" in intellectual pursuits. It seems plausible that comparative intellectual advantages matter when the individual chooses which *type* of education to pursue. That is, a relatively mathematically skilled student may be more partial to an education where mathematics is used intensively, compared to a gifted student with comparative advantages in verbal abilities. Moreover, some types of ability do seem to yield a higher labor market pay-off than others. For example, Dougherty (2003) finds that numeracy has a strong positive impact on individual wages, whereas literacy has a much smaller (and often insignificant) impact.[3] Accordingly, if relative cognitive abilities determine the type of education, the individual pursues *and* affects the final wage, existing return estimates to the *type* of education may be biased.

The Danish educational system is well suited for studying the returns to different types of education. The reason is that university degrees in Denmark are highly specialized. For example, if one chooses to study economics then this is the subject matter pursued throughout the entire time at the university; both during the undergraduate and the graduate level. Intellectual excursions into other fields only occur to a very modest extent, in contrast to what may be the case under e.g. a US-type system. Consequently, examining the labor market performance of Danes holding different types of tertiary education is likely to convey information about the extent of human

[3]See also Bishop (1992) and Joensen and Nielsen (2009) who find that greater skills in mathematics goes along with higher individual wages. Interestingly, similar results are obtained in the aggregate data. Hanushek and Woessmann (2009) document that the link between average test scores in mathematics and science is more strongly related to *aggregate* growth than test scores in reading; when all three types of test scores are included in the regression the latter turns insignificant.

capital production within different fields of study. In addition, Danish universities are publicly funded which reduces the scope for marked quality differences.

Accordingly, the present paper contributes to the literature by attempting to elicit information about the causal effect of *the field of study* on individual productivity, as it manifests itself in individual wages. The data set underlying the empirical analysis covers the part of the Danish population which completed high school during the period 1981-1990.[4] Narrowing the focus to the group of individuals which subsequently graduated from a tertiary education, and ended up in wage-employment, we examine whether returns to education differ systematically across previous field of study.

As a first pass, we examine the relative labor market performance of individuals who chose to study within the broad fields of human arts and other types of tertiary educations, using the standard wage equation. Conditional on standard determinants of wages an OLS regression reveals that individuals who pursued an education within the human arts fared much worse, as noted above, than individuals with other majors.

Still, OLS estimates are unlikely to capture the causal effect of the type of education on individual productivity, unless relative cognitive abilities are controlled for. Accordingly, we subsequently try to control for comparative intellectual advantages by invoking individual-level information about academic specialization in the Danish high school system. In addition, we are able to utilize information about the high school attended, high school GPA, as well as other individual control variables. Upon including such controls in the wage equation we find a considerable reduction in return differences. Still, a negative and significant difference persist; the wage difference between human arts majors and others is reduced from 22% to 16%.

Ultimately it is hard to rule out that other – unobserved – factors could simultaneously impact on the choice of education type as well as productivity. As a result we try to make additional headway by employing an instrumental variables (IV) approach to the issue at hand.

[4] When we refer to the Danish high school in this paper, we mean the ordinary high school ("gymnasium"). The Danish high school is of a three year duration.

To identify the impact of the field of study on wages we begin by studying the educational choice itself. That is, the choice of which type of tertiary education to pursue. Specifically, we model the choice of field of study as a function of (relative) academic abilities, and variables thought to capture the observed academic tastes of the individuals' high school peer group. While the former turns out to be linked to final wages, the latter determinants should not affect the productivity of the individual, once we carefully control for high school fixed effects (perhaps reflecting variation in teacher quality etc.), the curriculum studied by the individual in high school as well as the academic achievements of the individual when graduating from high school. As a consequence, peer group characteristics serve as *instruments* for the individuals' choice of field of study.

As documented below, student choices are indeed interdependent. Specifically, we find that there is a high correlation between the ultimate education choices of seniors and the ultimate educational choice of (the two years younger) freshmen.[5] Similarly, the fraction of female high school students in the cohort greatly influences the educational choice of the individual student.

We interpret these findings as reflecting the influence from student interaction about the attractiveness of various fields of study. That is, it reflects the consequences of informational updating. The type of information conveyed is unlikely only to be about labor market earnings; raw labor market earnings are relatively easy to observe. However, it is considerably harder to assess the broader "quality-of-life" pay-off to a specific education. For instance, what is the associated status, work environment and so forth? We hypothesize that student interaction serves to convey this kind of information. In addition, we conjecture that students with (revealed or hypothesized) preferences for particular fields of study likely hold an informational advantage *within* their preferred area. Accordingly, if an individual is more exposed to a peer group with preferences for the human arts, the more likely it will be that new information about the "quality-of-life" aspects of a working life with a human arts degree is brought forward. This new information may affect the educational choice. Note that since the hypothesis emphasizes information *updating*, it does not

[5] As explained in Section 3, there is a good reason why the interdependence should appear between seniors and first year students (rather than between seniors and second year students) during the period we study.

follow that more information about, say, the humanities necessarily will *increase* the probability that one would choose an education within this field of study.[6]

The link between the fraction of seniors with revealed preference for the humanities (judged by their ultimate choice of education type) and the educational choices of younger students, amends itself readily to this interpretation. The link between the fraction of females and the ultimate educational choice of the individual is perhaps somewhat more controversial. Nevertheless, a similar interpretation is viable. To begin with, female and male students may have a different *composition* of their abilities. In an influential study, Benhow and Stanley (1980) examined nearly 10,000 mathematically. gifted boys and girls, at the ages of 12 to 14. Their main empirical finding was a significant sex difference in mathematical reasoning as measured by the SAT-M, in favor of the boys. This observed difference could not be ascribed to differential course-taking accounts.[7] Moreover, 20 years later Benbow et al. (2000) revisited the sample and studied the educational and career outcomes of the students; they document a significant difference *in education choices*, with boys (now around 33) more likely to have chosen an education within the natural sciences; girls were more likely to pursue an education within the humanities. Admittedly, it seems hard to assess whether (or the extent to which) these findings have a "genetic" or cultural origin. But either way it would appear that women are more partial to the humanities, compared to men. Indeed, we obtain a similar correlation in our empirical analysis below; females are themselves more likely to enter into the humanities. Hence, if females are partial to the humanities, they may well hold more information about the consequences of choosing this field of study. The ensuing interaction and dissemination of information about the non-pecuniary returns to an education in the humanities may then influence the choice of education of the individual student.

In sum, we argue that the high school specific fractions of female students, and seniors choosing an education with the human arts, are viable instruments for the choice of which type of tertiary education individuals pursue. With these instruments in hand, we proceed to estimate the impact

[6] Section 3 contains a more detailed discussion of this issue.

[7] See also Guiso et al. (2008) and Machin and Pekkarinen (2008) for a discussion of gender specific test scores in math and reading.

of choosing an education in the humanities by 2SLS.

Our 2SLS estimates differ substantially from the OLS counterparts. After instrumenting we find *no difference* in the impact from the education choice on wages. Hence, we are led to the following conclusions: Relative cognitive abilities do have a substantial impact on wages, and comparative intellectual abilities do seem to matter for the choice of which education to pursue. However, the impact from the education *per se* on wages is independent of the field of study. In other words, returns to education do not differ across fields.

Naturally, one may question our identification strategy. In particular, one could argue that the first stage correlation between the educational choices of different high school specific groups is simply picking up (unobserved) school quality in various dimensions. Since such quality differences may influence productivity and wages this reasoning would suggest that our instruments are invalid.

We believe such concerns can be put to rest in the present case, for a number of reasons. First, Danish high schools are (generally) publicly funded, from a regional source. Hence, the type of local "neighborhood effects", known to be operative in e.g. the US, whereby high income municipalities can provide better funding for educational facilities, are not operative in Denmark. Second, all Danish high schools follow the same curriculum and students attend the same (centrally devised) written exams. Third, in our analysis we are able to control for the identity of the high school the individual have attended. That is, we include high school fixed effects (149 in all). If a specific high school happens to deliver high quality teaching in some particular field, a high school fixed effect picks it up. Finally, although we are confident in the excludability of our instruments in the second stage, for the reasons stated above, we nevertheless test the exclusion restriction directly and find support for this hypothesis as well.

The paper proceeds as follows. The next section provides a brief review of the related literature. Section 3 presents a simple model illustrating our identification strategy. Section 4 describes the econometric approach, and Section 5 describes the institutional features of the Danish educational system as well as the data used in our empirical analysis. Section 6 presents our main results, and section 7 provides various robustness checks. Finally, section 8 concludes.

2 Related Literature

While the literature on the return to schooling is vast, only a relatively limited number of studies have attempted to come to grips with the return to *type* of education.

James et al. (1989) is the earliest contribution (to our knowledge) which provides evidence of differences in human capital remuneration, by field of study. Specifically, they add dummy variables to an annual earnings equation capturing college majors. Their sample includes earnings and various individual specific characteristics (including the college attended) of 1241 males, drawn from the *National longitudinal study of the high school class of 1972* (NLS72). They find very large differences in the "return" to college major. For instance, a student who chose his major in the humanities, instead of engineering, should expect 45% lower annual earnings in 1985, *ceteris paribus*; a truly remarkable return difference. Indeed, as James et al. concludes (p. 251): "*[...] while sending your child to Harvard appears to be a good investment, sending him to your local state university to major in Engineering, to take lots of math, and preferably to attain a high GPA, is an even better investment.*" On *a priori* grounds, however, their estimates may not reflect a causal impact on productivity for two reasons. First, their labor market data concerns annual earnings. As a result, some of the observed difference may be attributed to differences in number of hours worked in different occupations. Second, the choice of major is treated as exogenous.[8]

Blundell et al. (2000) draw on the UK *National Child Development Survey,* which contain data on family background of children born in 1958 (between March 3 and 9), their educational choice (including the subject studied) along with labor market data on hourly wages. The wage is observed for the year 1991, when the subjects were 33 years old. In contrast to previous studies, Blundell et al. (2000) also attempt to deal with the endogeneity problem by invoking matching methods to identify the impact from higher education on hourly wages. Specifically, individuals

[8]Daymont and Andrisani (1984) also contain information about fields of study; but their focus is on showing that the gender gap in wage regressions shrink, once the choice of major is accounted for. Other studies that investigates earnings differential across majors include Dolton and Makepeace (1990), Grogger and Eide (1995) and Loury and Garman (1995). A common feature of these studies is that they also (in contrast to the present study) treat the choice of type of education as exogenous.

with a higher education were compared with individuals who could have taken a degree (based on previous educational performance) but chose not to, while sharing various observable characteristics (like ability, family background etc.). Naturally, this only resolves the endogeneity problem if all relevant individual specific characteristics are controlled for. If unobservable characteristics matter for wages and choice of education the estimates remain biased.[9] In line with previous studies, Blundell et al. also detect differences in labor market rewards across fields of study. For example, chemistry and biology exhibits the lowest return, whereas economics, accountancy and law the highest. In many cases, however, the effects from educational type is not very precisely estimated, presumably because of a rather limited sample size.

Bratti and Mancini (2003) also examine data from the UK. Like Blundell et al. (2000) they invoke matching methods. In addition, they also consider the problem that selection may take place over unobservable variables. In ensuring identification they rely on a multinomial-logit-OLS (MLO) set up, where choice of education is estimated and then the impact of education type on wages. As in the present paper they invoke an IV methodology. In Bratti and Mancini the exclusion restriction is that choices made in previous education (specifically: A-level curriculum) and the age of the student does not matter for wages directly, controlling for type of degree and standard Mincer-type controls. While their OLS results suggest that graduates from economics and business subject did better than the rest, their MLO results lead to no clear-cut ranking of subjects; the pecking order appears to change over time. One may argue, however, that their data are not optimal. The reason is that the data source (University Statistical Research data) does not include information about salaries. Since the authors do have access to fairly detailed information about occupation, they can construct salaries for individuals. This is done by using data from the New Earnings Survey; individuals salaries are computed as (p. 9) "the average gross weekly pay of individuals employed full time (in the same occupation) in the year following the questioner". Hence, by construction there is no within-occupation variation in earnings in their sample. As a consequence,

[9] This approach is similar to the OLS wage regressions reported below; like the National Child Development Survey our data contain very rich socio-economic background information of the individuals pursuing a higher education, which we control for alongside more standard variables like work experience etc.

their results are likely to speak to the impact from the type of education on occupations, rather than on wages *per se*; potentially valuable information pertaining to *differences* in wages across individuals with different educational backgrounds *in similar occupations* cannot be used for the purpose of identification.

Finally, Arcidiacono (2004) examine the return to college major, by modelling the educational decision explicitly. Arcidiacono, like James et al. (1989), rely in the NLS72 data set, implying the return estimates speak to earnings, rather than wages *per se.* The study documents that selection is indeed taking place. Moreover, controlling for selection, he still finds considerable return differences across majors; as in James et al. students majoring in e.g. the natural sciences fare better in the labor market.

3 Some Theoretical Considerations

In this section we develop a simple conceptual framework which motivates our identification strategy. Suppose individuals derive utility from wage income, y, and "quality of life" more broadly, q. The latter variable is thought to capture, in a parsimonious way, factors such as status, work environment and job satisfaction associated with being employed using education of type $i = H$(uman arts), O(ther). Without loss we assume wage income is observable, whereas q is something individuals hold expectations about. Utility is separable in the two arguments (y, q), and the expected level of utility for an individual (the index of whom is suppressed in the interest of brevity) is therefore

$$E\left[U\left(y,q\right)\right] = u\left(y\right) + \int v\left(q\right) f\left(q\right) dq,$$

where $f(\cdot)$ is the density function for q.[10]

We assume $f(\cdot)$ supports a given variance σ^2 and mean μ; both may be specific to either type of education: $\left(\mu_i, \sigma_i^2\right)$, $i = H, O$. Importantly, both σ_i^2 and μ_i are thought to reflect the

[10] See e.g. Fershtman et al. (1996) for an analysis of the allocation of talent in a society where individuals derive utility from consumption and social status. In the present case, however, we define "q" more broadly to include other aspects of final employment that individuals may value.

individuals' *perception* of the moments of the distribution of q. We treat both as known with subjective certainty in the derivations below, but both may vary from one individual to the next. In this sense we capture, in a simple way, differences in the information set of individuals at the time of optimization. Accordingly, these are the parameters which may be influenced by student-to-student interaction.

The felicity functions $u(\cdot)$ and $v(\cdot)$ exhibit positive and diminishing marginal utility: $u_y > 0$, $u_{yy} < 0$, $v_q > 0$, $v_{qq} < 0$. If we Taylor approximate v around the mean, μ, we obtain:

$$v(q) \simeq v(\mu) + v_q(\mu)(q-\mu) + \frac{v_{qq}(\mu)}{2}(q-\mu)^2 .$$

Evaluating expected utility we obtain, after some rearrangements, a simple representation of the preferences, which depends on income, expected quality of life and the variance of the latter[11]

$$E[U(y,q)] \approx u(y) + v(\mu) + \frac{v_{qq}(\mu)}{2}\sigma^2 .$$

Now, suppose an individual with these preferences are to choose between two alternative types of education: H and O. Realistically, the individual undoubtedly will have different aptitude to the two types of education. That is, different relative ability levels, which manifests itself in different wages. To capture this we may define the levels of income in final occupation as $y_H \equiv y(\alpha_H)$, and $y_O \equiv y(\alpha_O)$.[12] The parameter α_i captures ability, and we expect the *relative* level of ability (α_H/α_O) to differ across individuals, reflecting variation in comparative cognitive ability. Hence, some students may have a comparative cognitive advantage in the humanities, implying $\alpha_H > \alpha_O \Rightarrow y_H > y_O$. For others, of course, it may be the other way round. The pertinent characteristic of α_i is that it is predetermined at the time of optimization; it may have

[11] See the Appendix for derivations.

[12] Of course, we could easily admit wages to be affected explicitly by years of schooling etc. Say, by assuming $y_i = \alpha_i e^{\rho_i u_i}$, where ρ_i is the (potentially) field-specific return to a year of (field specific) education, u_i. Similarly, at the cost of some more notation, we could allow both dimensions of ability (α_H, α_O) to affect wages in either form of occupation; say $y_i = e^{\rho_i u_i}\Pi\alpha_i^{\beta_i}$. In general, then, we would allow the return to these abilities to differ; $\beta_H \neq \beta_O$. Finally, we abstract from "absolute" ability. This too could be introduced, perhaps defined as an average of the two components (α_H, α_O).

been determined earlier in life, or simply at birth.

Next, one may suppose the perceived mean and variance of q in the two potential endeavours of life differ. For simplicity, suppose only the latter differs. If so the individual will prefer H to O iff

$$u\left(\left[y\left(\alpha_H\right)\right]\right)+\frac{v_{qq}\left(\mu\right)}{2}\sigma_H^2>u\left[y\left(\alpha_O\right)\right]+\frac{v_{qq}\left(\mu\right)}{2}\sigma_O^2.$$

Hence, individuals with high ability in H will be more likely to choose this type of education. However, greater uncertainty with respect to q (i.e., σ_H^2) may persuade the individual to do otherwise.

Accordingly, uncertainty as to the non-pecuniary consequences of the educational choice may impact on what the individual decides, as a consequence of risk aversion. We hypothesize that *some* of the uncertainty may be resolved by interacting with fellow students. In particular, if the individual is exposed to students with information about q this will lower σ^2.

Naturally, the interaction could affect perceived μ as well. As a consequence of these multiple channels of influence, *the net impact* on the inequality from "more information" is ambiguous. For instance, if the result of the interaction is simply to lower σ_H^2 (say) then interaction should make it more likely that the individual chooses H. Alternatively, suppose the student-to-student interaction reveal information about μ. Naturally, if the information update implies $\mu_H' > \mu_H$ (with μ_H' being the revised mean), it should also make it more likely that the individuals chooses H. But if $\mu_H' < \mu_H$, the converse is true.

An important point, however, is that neither μ nor σ^2 matters to wages, y; they only affect the educational choice. Accordingly, factors that lead to changes in $\left(\mu_i, \sigma_i^2\right)$ may be useful in identifying the impact of the educational choice itself. We hypothesize that student-to-student interaction, and thus the characteristics of the individuals' peer group, may serve this purpose.

Our empirical results indicate that the educational choices of students are indeed interdependent. For instance, we find that the ultimate choice of tertiary education of freshmen is influenced by the occurrence of seniors choosing an education within the humanities. It is important to stress that seniors and freshmen (in Denmark they are two years apart) are not paired up arbitrarily. During the period we study students were to choose their academic specialization in high school *after* the first year.[13] It seems plausible that high school specialization could give rise to a tendency to academic path dependence; early specialization affecting the ultimate form of specialization. Hence, if fellow students were to have an impact on individual's choice of ultimate tertiary education, a major influence would be possible after one year of high school studies. It should be observed that this empirical link is *not* driven by high school specific effects, like teacher quality, as documented below. It cannot be accounted for by student performance, or academic preferences as revealed by the mode of specialization.

We also document that the frequency of female fellow students impacts the educational decision of the individual high school student. As noted above, and documented below: In practice female students tend to choose a human arts education more frequently than male students. Insofar as this reflect preferences for human arts, it is plausible that female students are able to convey updated information about q_H to their fellow students.

Admittedly, our empirical analysis cannot pinpoint exactly why there appears to be interdependence in the educational choices of students. However, the simple framework above offers a potential explanation. It seems plausible that the interaction between students affect σ^2, and perhaps μ. As a consequence, educational choices are influenced.

From the perspective of identification the key point is that there is little reason to expect that the student interaction during high school influences productivity (and thus wages) directly once we carefully control for high school fixed effect, and various student specific characteristics, including test score results. Rather, in keeping with the theory above, we hypothesize the interaction influences expectations about the non-pecuniary rewards to pursuing an education in the humanities, and outside the humanities, which impacts on the choice of education. To be sure, we also test this assumption; data does not allow us to reject it.

[13] The institutional details about the high school system during the period under examination are laid out in Section 5.2.1.

4 Econometric Strategy

In estimating the relative return to field of study we specify a wage equation that includes the individual's choice of educational type. The wage earned by individual i is denoted by y_i. S_i measures the education type (major in human arts or other types) chosen by individual i and is the endogenous variable of interest. S_i equals one for having a masters degree within human arts and zero otherwise; the return estimate of human arts is therefore *relative* to other majors. This indicator is used because we restrict our self to include tertiary educations of about equal duration, and because the Danish educational system is such that one specializes in one topic only at the university.[14]

Our wage regression is

$$\log(y_i) = \alpha + \rho \cdot S_i + \mathbf{x}_i\boldsymbol{\beta} + d_{i,c} + d_{i,t} + d_{i,s} + u_i. \tag{1}$$

The parameter ρ captures the relative return on a degree within the humanities; it is the key parameter of interest. The vector $\mathbf{x}_i$ consists of observed background variables to be described below; this set includes standard controls in wage regressions. The variables $d_{i,j}$ are fixed effects which we introduce to try to control for ability; both the absolute level and comparative advantages. We expect these fixed effects to affect wages, and the choice of educational type, S_i.

The fixed effects are $d_{i,c}$ for high school **c**urriculum, which should capture the individual's own assessment of the costs of acquiring specific skill types. We describe this variable in greater detail below. The variable $d_{i,t}$ controls for **t**ime effects. More precisely, this is the year of graduation from high school. Finally, $d_{i,s}$ is included to control for the attended high **s**chool and thereby potential quality differences in skills formation.

Ultimately we will treat S_i – the indicators for educational type – as endogenous. In order to obtain consistent estimates for ρ we therefore employ a two-step procedure. The first step involves

[14] We have also attempted to examine a finer division of studies. Unfortunately, we have not be able to disentangle the returns to education in this more disaggregated setting; our instruments prove to be weak under this setup. A possible interpretation is that we need a description of relative abilities in more dimensions than the two dimensional "verbal" versus "mathematical" ability division that we apply below.

fitting the following equation for educational choice

$$S_i = \theta + \mathbf{z}_i\gamma + \mathbf{x}_i\delta + d_{i,c} + d_{i,t} + d_{i,s} + \varepsilon_i, \tag{2}$$

We estimate (2) as a linear probability model, and the notation for the controls are the same as above. Hence, the *only* new entry is $\mathbf{z}_i$; determinants of educational choice which do not matter to wages themselves. That is, our instruments for S_i. From a theoretical point of view, we consider variables which have an impact on the individual's expectations about the value of each type of education. Empirically, our instruments have to satisfy the two requirements that (A) they are orthogonal to u_i and (B) they are highly correlated with the choice of education type, S_i.

Having estimated equation (2) we subsequently construct the fitted values from the regression, $\hat{S}_i$. The second step of our two-step approach involves estimating equation (1) with $\hat{S}_i$ entering the right hand side in place of S_i. As we control for all the determinants of S_i, except for $\mathbf{z}_i$, this provides us with 2SLS estimates for the return on schooling.[15]

5 Data

The data we use in our empirical analysis is a data set covering the Danish population of individuals graduating from Danish high schools during the period 1981-1990. The data are administered and maintained by Statistics Denmark that has gathered the data from three administrative registers: the Integrated Database for Labor Market Research (IDA), the Danish Income Registry and the Danish Student Registry.

For each individual, we have complete data on educational and labor market histories along with detailed information on other socioeconomic characteristics. The educational data comprise

[15] We would prefer to use a set of dummy variables that allows for high school quality to vary over graduation years and high school branches, i.e., $d_{i,c\times t\times s}$. This is, however, not possible since γ is not identified as there is one dummy variable per observation of the instumental variables as these are measured for clusters of students at the high school level for different graduation years and high school branches. Alternatively, dummy variables based on the interaction between specific high schools and graduation years, i.e., $d_{i,t\times s}$, would control for time varying high school quality. The number of dummies under this specification – equal to 1,306 dummy variables – represents around 60 percent of the number of clusters in the empirical analysis, again resulting in collinearity problems.

detailed codes for the type of education attended (level, subject, and educational institution) and the year for completing the education. The labor market data contain the *hourly* wages; measured as the annual labor income divided by total hours worked.

5.1 Sampling of Data

In this study we focus on individuals that satisfy the following three criteria: (i) graduated from high school between 1981 and 1990, (ii) proceeded to obtain a masters degree, and (iii) was wage-employed in all years over the period 1999-2003; in the regressions below we use the average wage over these five years as dependent variable.

We confine attention to high school graduates from the period 1981-1990 since this period was characterized by a particularly useful institutional setting, which allows us to proxy comparative intellectual abilities. After 1990 the high school system changes. We describe the nature of the institutional setting in some detail below.

Using 1999 as the "first year" is a choice made for practical reasons. The last high school cohort in our sample graduated in 1990. In Denmark it is not uncommon for students to take a sabbatical before beginning their university studies. Moreover, few students manage to complete their studies within a 5 year window that is the planned course of study. Hence, in order to include all cohorts in the sample 1999 is a reasonable starting point. However, the exact choice of "initial year" is in the end not crucial, as demonstrated in Section 7.

We average over the five year period to even out potential yearly fluctuations in wages. After all, the null is that the choice of education matters to permanent income; averaging should increase the signal-to-noise in the dependent variable. Still, in Section 7 we demonstrate that using time averages is not critical to the results.

While our main regressions concern all wage earners (including public employees), we show in Section 7 that confining attention to private employees only does not change the results appreciably.

5.2 Explanatory Variables

5.2.1 High School Information

Figure 1 show graphically how a student would proceed through the Danish educational system, from lower secondary school to tertiary education, during the period 1981-1990. Individuals usually enter the Danish high school immediately after completing lower secondary school, and graduates after three years.

When applying to a high school for admission, the student was required to specify an over-all track to follow: "mathematical" or "language". After completing the first year, students then self-selected into various "branches" available for each track, as illustrated in Figure 1. Under the math track students could choose between math/physics, math/natural sciences, math/social sciences, or math/music, while under the languages track students could choose between languages/social sciences, languages/music, modern languages, or classical languages.[16] Hence, individuals were grouped into eight distinct branches. During this institutional arrangement the curriculum was determined after strictly defined course packages, implying that knowing the track and branch provide fairly precise information about the curriculum the students completed.

<Figure1 around here>

The information about which *branch* the individual pursued in high school appear in (1) and (2) as the curriculum fixed effect (i.e., $d_{i,c}$) to control for relative cognitive abilities directly. Hence, the basic ideas is that the choice of "branch" provides information about the individual students' *relative* abilities; a math/social science major was likely not quite as mathematically inclined as a math-physics student; at least the level of math taught was objectively speaking higher in the math-physics branch compared to the math-social science branch.

The "branch based system" was in place until 1990; from 1991 onwards students were given much greater autonomy with regards to course packages. Hence, the reason why we only sample

[16] In the last years of the sample a few experimental branches was allowed at some high schools; e.g., Math-English and Math-Chemistry. Only very few students pursued these branches; they are excluded from our sample.

high school graduates up until 1990 is precisely because it marks the end of the branch based system.

Eventually we do not have to rely on being able to fully control for relative ability, since we pursue an IV approach. However, as will be seen: branch choices hold considerable explanatory power vis-a-vis post university wages, suggesting that relative abilities across subjects indeed matter.

In order to control for "absolute ability", we use the high school GPA, which enters into $\mathbf{x}_i$. The GPA is a weighted average of the grades at the final exam at each course. The quality of the courses as well as the GPA is comparable across high schools since all students within the same cohort face identical written exams; all exams and major written assignments are evaluated by the student's own teacher as well as external examiners; high school teachers from other high schools. The external examiners are assigned by the Danish Ministry of Education.

Completed high school is a general admission requirement for tertiary educations, as suggested by Figure 1. We have information on which of the 149 high schools individuals attended. This information enters as the high school fixed effect, (i.e., $d_{i,s}$) and serve as controls for high school quality. Moreover, we have information on year of graduation from high school, which enters as the graduation year fixed effect, i.e., $d_{i,t}$. This dummy captures information on experience in equation (1).

5.2.2 Tertiary Education

As mentioned above, we focus exclusively on individuals who ultimately obtain a master's degree. The reason is that we want to avoid any selection bias in our results due to the choice of education length. Moreover, we partition the type of tertiary education into two bins: human arts vs "others". This information enters in the regressions as individual choice of education type, i.e., in S_i.

5.2.3 Other Explanatory Variables

We apply detailed individual information not related to high school attendance as explanatory variables, i.e., variables that enter in $\mathbf{x}_i$. These are gender and parental education. Gender is included to control for the gender wage gap in (1), whereas it enters (2) to control for gender differences in relative abilities or preferences (more on this below). Parental education is controlled for by including a set of indicators for each parent regarding both the length and type of their education.

Table 1 displays selective descriptive statistics for the samples. The sampling unit is the individual, and the table presents the distribution on type of tertiary education, the distribution of students on high school branches, their high school grade point average, and their gender.

<Table 1 around here>

Some aspects of the data are worth remarking on. Almost 85% chose the math track in high school, while only 15% chose the language track; the largest high school branch is math/physics. Recall, these statistics are all conditional on progressing to a tertiary education and being wage employed for an unbroken period of 5 years. As regards subsequent choice of education type, social sciences attract the most students, whereas human arts the least. Moreover, slightly more than 60% consist of men. The high school GPA is 8.8, which is above average as expected.[17]

5.3 Instruments

Our identification strategy is based on the idea that co-students influence the information set on which the individual base his or her final choice of a tertiary education. We do not doubt that individuals own abilities and interest are central. However, it would seem plausible that fellow students influence the individuals' choice of education. This influence can take many subtle forms, including providing students with a sense of what a certain type of education implies in terms of job satisfaction given the individuals ability and interest. Such information could affect the individual's expectations about the consequences of obtaining an education. More concretely, we apply two instruments.

[17] A numerical grading system is used in Denmark. The possible grades were at the time: 0, 3, 5, 6, 7, 8, 9, 10, 11 and 13; 6 were the lowest passing grade, and 8 was given for the average performance.

Our main instrument is a measure of older student's educational choice at the high-school. The instrument is constructed as follows: First, the shares of individuals with tertiary education are constructed for education types; the shares are determined for the group of individuals within the same high school and high school track. Second, the shares are lagged *two* years, to capture the influence from seniors on freshmen. As explained in Section 3, we lag the variable by two years because when a cohort graduates from high school freshman students have to choose the branch they want to follow within the track they decided on before entry (see Figure 1). If the education choice of older students are to have an impact on younger students it is exactly between graduating students and freshmen students choosing branch.

Our alternative instrument exploits what appears to be a sex bias in educational attainment; men and women appear to have different academic tastes, which in turn may be related to different aptitudes towards mathematical reasoning (Benbow and Stanley, 1980, and Guiso et al., 2008). It may also be a consequence of culture, albeit Benbow and Stanley are unable to explain the difference in test scores based on course choices, which conceivably would be affected by culture as well.[18] Moreover, even focusing on mathematically. gifted men and women, females tend to favour an education within human arts (Benbow et al., 2000). We document a similar "bias" below, in our sample of Danish students.

Whatever is the source of this difference among the sexes it might well matter to the educational choices of the individual. A greater occurrence of women in the high school will, *ceteris paribus*, imply a greater scope for interaction with individuals with relatively strong preferences for, and thus presumably also more knowledge about, the humanities. As a consequence the expectations about future (non-pecuniary) rewards to jobs offered in this dimension may be affected, and therefore the educational choice itself. Specifically, the instrument is the share of women instrument is constructed as the share of women in the same high school track as the individual.

[18] Guiso et al. (2008) show that the gender "gab" in math test scores, which generally favors men, tend to recede as societies develop greater over-all equity between the sexes; this suggests an important cultural dimension to the math test score gap. However, the reading gap, which favors women, does not disappear with gender equality. Hence, *relative* differences in math/reading across sexes appear to be a robust feature across countries.

Table 1 also displays mean and standard deviations for the instruments. The moments are determined for the 2,171 clusters in the dataset, i.e., the instruments are determined after high school attended, high school track, and graduation year. The measures are normalized by the national value of the instruments, i.e., the instruments are measured as *the deviation from the national value.* This measure captures the notion that, e.g., an "unusually" large share of the cohort two years earlier entering into a particular type of education will provide prospect students with better information to make the educational choice. The mean of both instruments is close to zero reflecting that the high schools of students that eventually ventured on to take a tertiary education are not "special" in this sense. At the same time one may observe that there is considerable variation in the shares. It is this variation we will draw on in order to obtain identification.

6 Main Results

We begin the presentation of our results by focusing on wage differences between the two educational choices. Subsequently, the results for the determinants of educational choice are discussed and finally the results for the returns to education types are reported.

6.1 Wage differences

In Table 2, we report the results from the standard wage regression. That is, the endogeneity problem is ignored.

<Table2 around here>

To recapitulate: These regressions are performed for persons with a tertiary education, who are wage-employed in all years over the period 1999-2003. In the first model (raw log wage differences), only indicators of the choice of education type are included in order to study raw log wage differences between education types. The "raw" wage gaps (column 1) reveal that human arts graduates have 22% lower wages than other graduates (1-exp[-0.2452]).

In Models 2-5, more information is introduced into the log wage regression to study how the estimated wage difference changes. In Model 2 we introduce a gender dummy in the regression that enters negatively and significantly with a parameter corresponding to women earning an average wage that is 14% lower than the average wage for men. In Model 3 we introduce high school GPA and find that the average wage increases by around 2.5% per grade point. Model 4 introduces curriculum fixed effect or the choice of high school branch that proxies for relative talent. It is evident that those who studied at the math-physics branch in high school earned the highest wages compared to any other branch. A high school curriculum in classical languages or language music led to the lowest wages that on average were 11% lower that math-physics. In general, those who chose the language track tend to earn lower wages than those that chose the math track. This suggests that mathematical abilities are valued more in the labor market than linguistic abilities. The final Model 5 includes all above mentioned explanatory variables. In addition we also include dummies for graduation year from high school, information for education length and type of parents, as well as high school fixed effects. High school fixed effects come in addition to, for example, the effect of parental education, but may comprise, e.g., teacher quality etc.

Over-all, when control variables are progressively added we observe that the relative difference in returns across fields of study shrinks from -22% to -16%, but remains statistically (as well as economically) significant.

6.2 Educational choice

The results for the linear probability model of the choice of education type are presented in Table 3a, that reports coefficients and their associated standard errors.

<Table3a around here>

The variables of particular interest are those from which we obtain identification; the two sets of instrumental variables, as described above. The first instrument, recall, is the fraction of students choosing human arts out of the total number of students that complete a tertiary education within the students' high school and track. This instrument is lagged two years, as discussed in Section

5.3. The second instrument is the fraction of female students out of the total number of high-school students in the high-school track.

What we have in mind when using the instruments is that fellow students influence the individuals' choice of education type through a better information set. More specifically, we imagine that individuals are better informed about the education choice of human arts for high values of the two fractions that are used as instruments. More information can either increase or decrease the probability of choosing human arts. In other words, we expect that the instruments would influence the probability in the same direction within each high school track. Consequently, we allow for different effects for individuals that have followed the language track and the math track, respectively, to get a more flexible formulation of the instruments impact on the probability of choosing human arts.

Generally, the instruments do a good job in explaining the choice of education type judged from their significance. In Model 6, which is based on the first instrument only, it is seen that a larger fraction of older students choosing human arts increases the probability with a point estimate of 0.23 in the language track and reduces the probability by 0.10 in the math track.

In Model 7, we include the alternative instrument in addition to the first instrument. It is evident that a better informed student in the language track has a higher likelihood of choosing human arts, whereas a better informed student in the math track has a lower likelihood. That is, the point estimates for the instruments are positive for students in the language track and negative for students in the math track. These results are consistent with our expectations regarding better informed students.

Notice that we also include the fraction of high school students choosing human arts lagged *one* year only. Here the results are much weaker; economically and statistically. Indeed, in the context of the language track the fraction of high school students choosing human arts lagged one year is insignificant; for the math track it is just significant at the 5% level. These results are consistent with the notion that a major influence on high school students future education choice occurs around the time when the individuals were choosing their high school branch.

This leaves us with Model 8, which includes instruments that are significant at the 1 percent level only. The first instrument enters for both high-school tracks, whereas the alternative instrument of female students out of the total number of high-school students only enters in the math track. It is also interesting to observe that the alternative instrument is insignificant in the language track. One possible explanation is that female students are highly concentrated in this track – with an average share of about 80 percent – implying that additional female students do not reveal much new information about working environment for human arts majors. As a result, the alternative instrument enters insignificantly in explaining the educational choice.

The F-test of excluded instruments is a test for weak instruments. The results presented in Table 3a are estimated using clustering that allows for dependence in residuals within clusters. There are 2,171 clusters in the dataset divided after high-school attended, high-school track and graduation year. Unfortunately, there exist no critical test value for weak instruments under clustering. The usual F-test is the Cragg-Donald F-statistic with critical values that follow Stock et al. (2002) and Stock and Yogo (2005) which both require independent residuals. In the absence of critical F-values, we apply the rule of thumb that instruments are strong if the first-stage F is larger than ten (see Staiger and Stock, 1997). It is evident that the F-tests in Models 6 and 8 exceed this value and we consider the instruments to be strong.

Concerning the proxy for relative ability, as measured by curriculum fixed effects, we find that math-physics high school students (students who tend to obtain high wages post graduate – see Table 2) have the lowest probability for studying human arts. All other high school branches give a significantly higher probability. It is much more likely that the education choice is human arts, when the high school branch is within the language track.

Besides instrument variables, and the proxies for relative ability, a number of the other variables are worth commenting on. Being a woman increases the probability of choosing human arts by 2.5%. This result is consistent with Benhow et al. (2000) who find a sex differences in educational choices between sexes, conditional on cognitive abilities. Moreover, a higher GPA, which we use as a proxy for absolute ability, reduces the probability of choosing human arts.[19]

6.3 Returns to Education

Next, we turn to the second stage regression of the 2SLS estimation. The results are presented in Table 3b

<Table 3b around here>

We report the coefficients from the OLS regression of Table 2, Model 5 for comparison. Models 6-8 present the 2nd stage regressions corresponding to the 1st stage regressions presented in Table 3a above. The results are striking. After instrumenting the education choice, the pronounced wage differences reported in Table 2 *disappear*. The human arts dummy now enters with a slightly *positive* sign. More importantly, the estimate is now economically as well as statistically insignificant. In Model 6, the point estimate is 0.0005, whereas the point estimate is 0.0170 in Model 8. These results are dramatically different from the (statistically significant) OLS estimate (= -0.18). Hence, the 2SLS approach suggests that the returns to human arts and other tertiary education types are of similar magnitudes.[20]

We present the Hansen J test for overidentification, where the null-hypothesis is whether the instruments can be excluded from the second stage of the 2SLS regression. The p-values in Models 6-8 are all above 10%, implying that we cannot reject the exclusion restriction.

We also report endogeneity tests; the null is that the endogenous regressor can be treated as exogenous. The null-hypothesis is rejected at the 10% level for Model 6 and at the 5% level for Models 7 and 8. In other words, we conclude that the choice of education type is indeed endogenous.

Finally, we report the Anderson-Rubin F-test for the joint test of whether the endogenous

[19] In principle, high school GPA should enter the regression in a more flexible formulation to capture the impact of ability on educational choice and wages in a flexible way and to allow for nonlinear effects of GPA on education choice due to different admission requirements. However, when using more flexible formulations, e.g., a second or third order polynomial, the extra terms enter insignificantly.

[20] We have also estimated Model 8 using a number of alternative estimation techniques. The alternative estimation techniques are: the GMM IV using one moment restrictions, the LIML IV estimator, the IV Fuller estimator and the bias corrected 2SLS. These estimators produce similar results, namely that the coefficient on the relative returns to human arts studies virtually equal zero with point estimates around 0.02.

regressor is insignificant in the structural equation *and* that the overidentifying restrictions are valid. These tests suggest that we cannot reject the null-hypothesis for any model.

As a result, we are led to the conclusion that the relative wage pattern observed in the (raw) data is largely caused by selection into education types based on observed and especially unobserved variables. The fact that human arts majors earn wages much lower than the average academic employee is not caused by their field of study but rather the composition of their ability endowments and the returns to these endowments in the labor market. Simply put, human arts majors are particularly negatively selected in terms of the market values of their ability endowments.

7 Robustness

In this section we investigate the robustness of the main result of insignificant differences in the return to education across tertiary education types. Robustness is tested in two dimensions: Using private employees only and using alternative time span for calculating the average wage used in the structural regression.

7.1 Private Employees

We perform the OLS and 2SLS regressions for private employees being in the sample for all years during the period 1999-2003. The main result presented above in Table 3a and b is robust to this change. This is evident from Table 4, where the OLS estimate of the wage difference between human arts majors and other majors points to a large difference of almost 20%, whereas the 2SLS regressions in Models 6-8 suggest that there is no significant difference in returns to education. Actually, the point estimates in Models 6-8 suggest that human arts majors earn a higher return that other majors with a positive point estimate of around 5% that, however, is insignificant. Again the tests for overidentified restrictions, endogeneity of the educational choice, and weak instrument robust inference all point to sound specifications for our preferred Model 8. Hence, the 2SLS regressions result in statistically insignificant differences in the return to education type for the subsample of private employees.

<Table 4 around here>

7.2 Average Wages

We also investigate the robustness of the results to changes in the time span for calculating the average wage rate. As argued in Section 5.1 above, we apply average wages of individuals to reduce the transitory element in the wage income. In Table 5 we present Model 8 estimated on the sample of individuals that are wage employees in all years over the period starting in 1999 and ending at alternative years, e.g., from 1999 to 2000. We carry out this robustness check for all end years in 1999-2003 such that average wages are calculated over a 5 year period - the baseline estimation of Table 3 -, 4 years and so on all the way to 1 year.

<Table5 around here>

As is clear from the table, the point estimate of interest is quite insensitive to change in the average wage. This is the case for both OLS and 2SLS estimates, suggesting that the restriction that individuals are in the sample as wage-employees in all years over the period 1999-2003 is not paramount.

To investigate this issue further, we estimate the wage difference using OLS and the returns to education using 2SLS for Model 8 using single year wages for all years over the period 1996-2003. The results are depicted in Figure 2. In the upper part of the figure, the point estimates using OLS and 2SLS are presented. Whereas the OLS point estimates are statistically and economically significant (and negative) the 2SLS point estimates are around zero until 2000 after which the estimates drop to -0.10. Hence, the results reported in the last section are valid for each year over the period 1996-2000.

<Figure2 around here>

For 2001-03, the main result can no longer be established. The reason is that statistical significance of the instruments becomes weaker as time goes by, which can be seen in the lower graph of Figure 2. Here, the p-value of the t-statistic on the main instrument in the math track (senior's impact on freshmen) is displayed. The instrument becomes insignificant from 2001 and forward. In other words, we are left with weak instruments.

This result can be understood from the development in the distribution of the number of years between high-school and university graduation for the wage-workers in the sample. In 1996, around 20,000 wage-workers enter the analysis, increasing to 31,000 in 2001. The share of wage-workers graduating from university at least ten years after high-school graduation has increased considerable over this period. In 1996, the 85% percentile had ten years between graduation from high-school and university; in 2001 this was the case for the 73% percentile. Moreover, 40% of the wage-workers entering into the sample between 1996 and 2001 graduated from university ten years or later after completion of high-school. This means that the distribution of the number of years between high-school and university graduation becomes more and more right-skewed as time goes by. Therefore, it seems plausible that factors outside high-school become relatively more important for the educational choice; factors that we do not measure in our data and as a consequence the instruments end up weak for the later years.

Support for the above explanation is provided when the parameters for 2001-03 are estimated using samples of individuals that were wage-workers in 1999 - our base year - as well as the specific year under investigation. Under this restriction, the main result is re-established after 2000, i.e., the instruments enter as strong instruments in the linear probability model for the choice of education and there is no significant difference in the returns to education across fields of study.

8 Conclusion

In this paper we have essentially examined the efficiency of human capital production across different types of education by exploiting Danish register data. If some fields of study are more efficient in producing human capital, this should manifest itself in a superior labor market performance of its graduates. Baseline OLS regressions reveal that students of human arts fare the worst in the

Danish labor market with an hourly wage rate around 20% below that of graduates within other majors.

One may suspect, however, that the partial correlation between the type of education and wages does not convey accurate information about human capital production. If the selection into educational types is nonrandom the OLS estimates will be biased. Our analysis confirms that selection seems to be at work. Socioeconomic circumstances, ability, as well as relative cognitive abilities, measured by high school course work, influence the choice of education type.

Consequently, we invoke instruments for education type to address the selection problem. Our two instruments are based on the influence from other students on individuals' choice of education type. Strikingly, once education type is instrumented, we find *no* difference in the return across fields of study. This result suggests that there is no type of education, which provides the individual with more productive human capital than others. Accordingly, the relatively poor performance of human arts majors in the Danish labor market is due to selection according to relative cognitive ability, rather than to low human capital production at universities.

The present analysis raises new questions worth exploring in future research. First, wage differences seem to be related to relative cognitive abilities; mathematics appear to be important, for example. But why is that? Is it because such abilities are relatively scarce in the population or because they are particularly productive? If the latter is the case, then it would be useful to try and discern why such abilities are in high demand. Further motivation for pursuing this question is found at the macro level where test scores in math and natural sciences seem to be a stronger linear predictor of aggregate growth than test scores for reading (Hanushek and Woessmann, 2009).

Second, how are relative abilities formed? As relative abilities determine education choices as well as wages, it would be useful to know whether these cognitive traits have a genetic origin, or are acquired during primary and secondary schooling. If the former is the case, education policies cannot be invoked to influence them; and conversely if relative talents are acquired.

A Deriving Expected Utility

The second order Taylor approximation

$$v(q) \approx v(\mu) + v_q(\mu)(q-\mu) + \frac{v_{qq}(\mu)}{2}(q-\mu)^2$$

Observe that $(q-\mu)^2 = q^2 + \mu^2 - 2q\mu$. Hence

$$v(q) \simeq v(\mu) + v_q(\mu)(q-\mu) + \frac{v_{qq}(\mu)}{2}q^2 + \frac{v_{qq}(\mu)}{2}\mu^2 - \mu q v_{qq}(\mu)$$

Inserted into the utility function we obtain

$$\begin{aligned} E[U(y,q)] &\approx u(y) + v(\mu) + v_q(\mu)\int qf(q)\,dq - \mu v_q(\mu) \\ &\quad + \frac{v_{qq}(\mu)}{2}\int q^2 f(q)\,dq + \frac{v_{qq}(\mu)}{2}\mu^2 - \mu v_{qq}(\mu)\int qf(q)\,dq. \end{aligned}$$

A useful result regarding means and variances is that $\int q^2 f(q)\,dq = \mu^2 + \sigma^2$. Using it in the expression above we get

$$E[U(y,q)] \approx u(y) + v(\mu) + \frac{v_{qq}(\mu)}{2}\sigma^2$$

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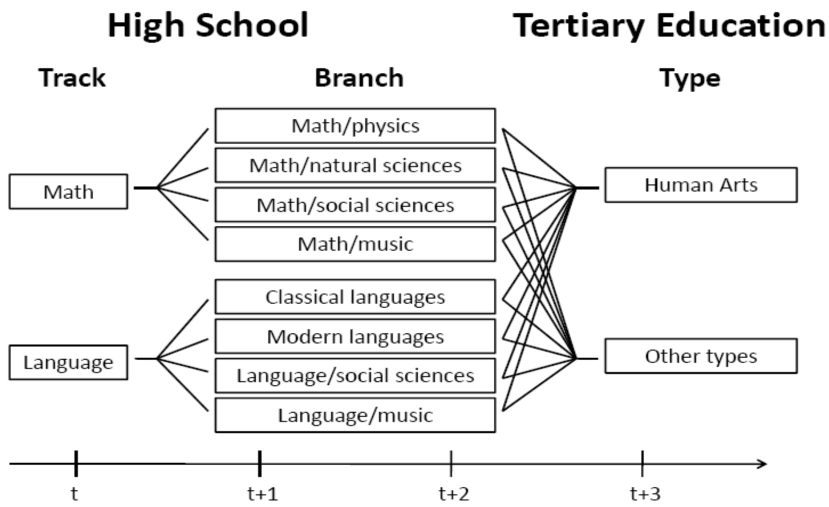
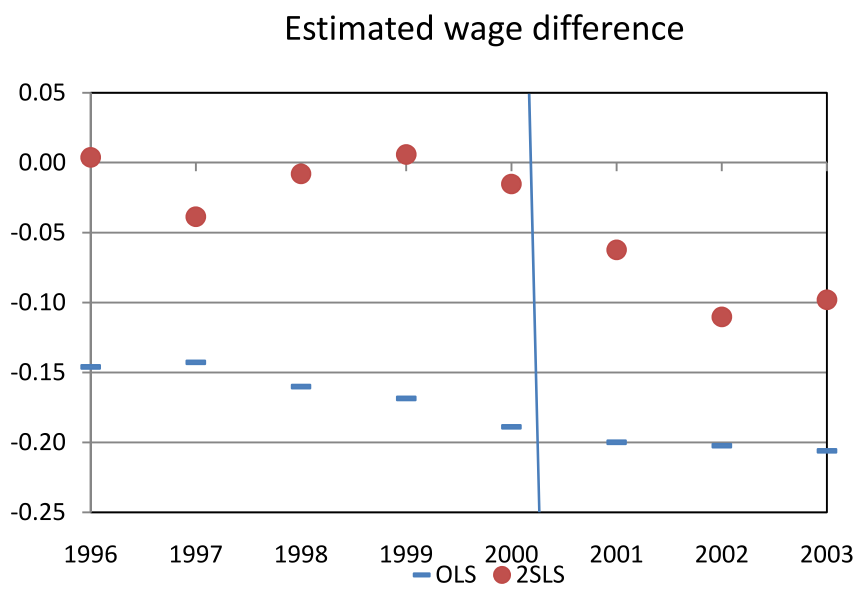
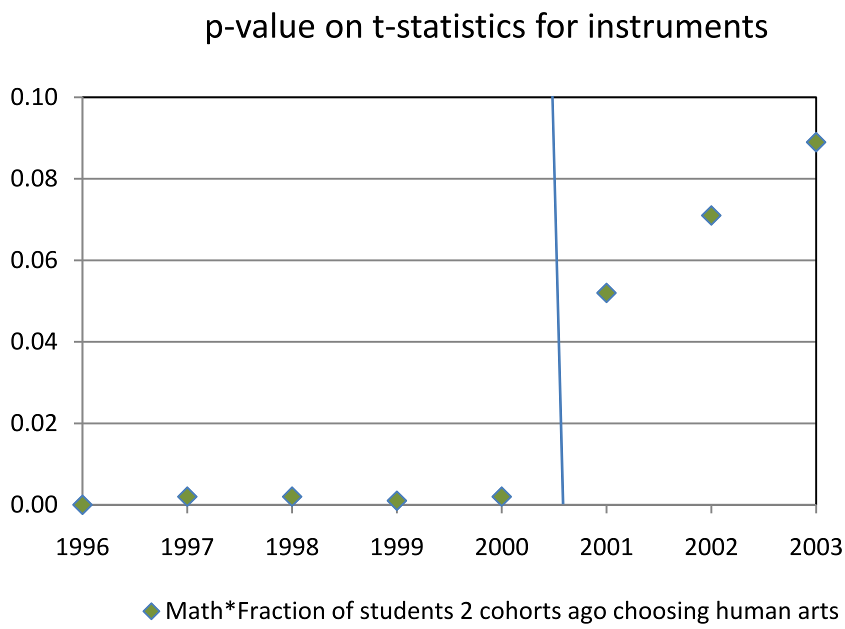


Figure 1: A sketch of the Danish High School System during the period 1981-90.

Figure 2: OLS and 2SLS log wage regressions, yearly wage rates





Note: Upper graph: Estimated models for the single year t is Model 5 for OLS and Model 8 for 2SLS. Explanatory variable is log to the yearly wage rate, i.e., log(yi,t) for t belonging to the period 1996-2003. Lower graph: information on other instruments than the "Math*Fraction of students 2 cohorts ago choosing human arts" is suppressed because p-values are <0.00001.

Table 1: Descriptive Statistics for Individuals in **Private and Public Employment** (1999-2003)

	Mean	Std. Dev.	Minimum	Maximum
log (wage rate)	5.620	.320	4.625	8.235
Subsequent education type				
Human arts share	.083			
Other educational types	.917			
High school branch				
Math-Music	.054			
Math-Physics	.438			
Math-Natural Sciences	.199			
Math-Social Sciences	.161			
Modern Languages	.064			
Classical Languages	.004			
Language-Social Sciences	.070			
Language-Music	.010			
High school GPA	8.813	.842	6	11.9
Women	.383			
# individuals	20,716			
# high schools	149			
# high school graduation years	10			

INSTRUMENTS	Mean	Std. Dev.	Minimum	Maximum
Fraction of students in human arts (language track, 2 cohorts ago)	.001	.122	-.470	.630
Fraction of students in human arts (math track, 2 cohorts ago)	.001	.048	-.091	.506
Fraction of women in language track	.092	.122	-.218	.411
Fraction of women in math track	-.078	.087	-.450	.248
# clusters	2,172			

Table 2: OLS log wage regressions, **private sector employees** only, no instruments, Average wages 1999-2003

	Raw log wage gap		**Model 2**		**Model 3**		**Model 4**		**Model 5**	
	Coeff.	Std. Err.	Coeff.	Std. Err.	Coeff.	Std. Err.	Coeff.	Std. Err.	Coeff.	Std. Err.
Human arts	**-.2452**	.0071	**-.2120**	.0069	**-.2122**	.0069	**-.1933**	.0074	**-.1801**	.0071
Woman			**-.1546**	.0039	**-.1578**	.0039	**-.1367**	.0040	**-.1365**	.0039
High school grade					**.0250**	.0022			**.0342**	.0022
Highschool branch (Math-Physics - reference)							**-.1004**	.0086	**-.0394**	.0091
Math-Music							**-.0770**	.0052	**-.0764**	.0051
Math-Natural Sciences							**-.0484**	.0055	**-.0245**	.0054
Math-Social Sciences							**-.0435**	.0084	**-.0716**	.0083
Modern Language							**-.1232**	.0315	**-.1519**	.0309
Classical Language							**-.0858**	.0078	**-.0807**	.0077
Language-Social Sciences							**-.1200**	.0191	**-.1209**	.0188
Language-Music										
Graduation year dummies	NO		NO		NO		NO		YES	
Parental education	NO		NO		NO		NO		YES	
High school fixed effects	NO		NO		NO		NO		YES	
R-squared	0.0544		0.1207		0.1259		0.1362		0.2087	
# observations	20,716		20,716		20,716		20,716		20,716	

Note: All regressions include graduation year dummies, parental education and high school fixed effects Bold = significant at 1% level; italics = significant at 5% level.

Table 3a: Linear Probability Model of Choice of Tertiary Educational Type, Human Arts versus Other (**Public and Private Employees** in 1999-2003)

	Model 6		**Model 7**		**Model 8**	
	Effect	Std. Err.	Effect	Std. Err.	Effect	Std. Err.
Woman	**.0252**	.0041	**.0259**	.0041	**.0261**	.0041
High school grade	**-.0076**	.0021	**-.0075**	.0021	**-.0075**	.0021
***Highschool branch* (Math-Physics - reference)**						
Math-Music	**.0539**	.0100	**.0529**	.0101	**.0534**	.0100
Math-Natural Sciences	**.0124**	.0039	**.0124**	.0039	**.0122**	.0039
Math-Social Sciences	**.0454**	.0050	**.0454**	.0050	**.0453**	.0050
Modern Languages	**.3311**	.0140	**.3344**	.0301	**.3474**	.0148
Classical Languages	**.4701**	.0597	**.4713**	.0668	**.4866**	.0596
Language-Social Sciences	**.1933**	.0114	**.1949**	.0303	**.2089**	.0121
Language-Music	**.4565**	.0316	**.4581**	.0411	**.4726**	.0318
Instruments by high school track						
Language*Fraction of students 2 cohort ago choosing human arts	**.2291**	.0442	**.2131**	.0448	**.2222**	.0442
Language*Fraction of students one cohorts ago choosing human arts			.0312	.0483		
Language*Fraction of women in track			.0601	.1181		
Math*Fraction of students 2 cohorts ago choosing human arts	**-.0969**	.0324	**-.0945**	.0331	**-.0900**	.0325
Math*Fraction of students one cohorts ago choosing human arts			*-.0846*	.0351		
Math*Fraction of women in track			**-.1173**	.0307	**-.1198**	.0306
Clusters	2,172		2,172		2,172	
Cluster-robust F-test	18.36		9.38		17.44	
Observations	20,716		20,716		20,716	

Note: All regressions include graduation year dummies, parental education and high school fixed effects Bold = significant at 1% level; italics = significant at 5% level.

Table 3b: OLS and 2SLS log wage regressions **private and public** sector employees, average wages 1999-2003

	Model 5		**Model 6**		**Model 7**		**Model 8**	
	Coeff.	Std. Err.	Coeff.	Std. Err.	Coeff.	Std. Err.	Coeff.	Std. Err.
Human arts	**-.1801**	.0071	.0005	.1034	.0262	.0963	.0170	.0989
Woman	**-.1365**	.0039	**-.1410**	.0046	**-.1417**	.0045	**-.1415**	.0046
High school grade	**.0342**	.0022	**.0357**	.0025	**.0359**	.0025	**.0358**	.0025
Highschool branch								
Math-Music	**-.0394**	.0091	**-.0492**	.0105	**-.0506**	.0103	**-.0501**	.0104
Math-Natural Sciences	**-.0764**	.0051	**-.0786**	.0054	**-.0789**	.0054	**-.0788**	.0054
Math-Social Sciences	**-.0245**	.0054	**-.0327**	.0074	**-.0339**	.0072	**-.0335**	.0072
Modern Language	**-.0716**	.0083	**-.1316**	.0356	**-.1401**	.0334	**-.1370**	.0343
Classical Language	**-.1519**	.0309	**-.2382**	.0545	**-.2505**	.0520	**-.2461**	.0530
Language-Social Sciences	**-.0807**	.0077	**-.1154**	.0215	**-.1204**	.0204	**-.1186**	.0209
Language-Music	-**.1209**	.0188	**-.2053**	.0509	**-.2179**	.0478	**-.2130**	.0490
			Test val	P-value	Test val	P-value	Test val	P-value
Endogeneity test			3.136	0.0766	5.494	0.0267	3.984	0.0459
Hansen J test			0.183	0.6686	1.322	0.9326	0.347	0.8409
Anderson-Rubin F-test			0.09	0.9132	0.23	0.9654	0.12	0.9468
# observations	20,716		20,716		20,716		20,716	

Note: All regressions include graduation year dummies, parental education and high school fixed effects Bold = significant at 1% level; italics = significant at 5% level.

Table 4: OLS and 2SLS log wage regressions **private** sector employees (average wages 1999-2003)

	Model 5		Model 6		Model 7		Model 8	
	Coeff.	Std. Err.	Coeff.	Std. Err.	Coeff.	Std. Err.	Coeff.	Std. Err.
Human arts	**-.1956**	.0111	.0368	.1635	.0532	.1422	.0597	.1494
Instruments								
Language*Fraction of students 2 cohort ago choosing human arts			**.2785**	.0702	**.2465**	.0727	**.2697**	.0699
Language*Fraction of students one cohorts ago choosing human arts					.0621	.0735		
Language*Fraction of women in track					.1599	.1787		
Math*Fraction of students 2 cohorts ago choosing human arts			-.0311	.0352	-.0264	.0356		
Math*Fraction of students one cohorts ago choosing human arts					-.0668	.0383		
Math*Fraction of women in track					**-.1333**	.0353	**-.1405**	.0357
Clusters			1942		1942		1942	
Cluster-robust F-test			8.44		6.29		16.45	
			Test val	P-value	Test val	P-value	Test val	P-value
Endogeneity test			2.109	0.1464	3.356	0.0670	3.192	0.0740
Hansen J test			0.095	0.7585	1.506	0.9124	0.089	0.7657
Anderson-Rubin F-test			0.07	0.9313	0.28	0.9460	0.13	0.8754
# observations	12,304		12,304		12,304		12,304	

Note: All regressions include graduation year dummies, parental education and high school fixed effects. Bold = significant at 1% level; italics = significant at 5% level.

Table 5: OLS and 2SLS log wage regressions **private and public** sector employees, average wages (Model 8)

	1999-2003		**1999-2002**		**1999-2001**		**1999-2000**		**1999**	
	Coeff.	S. E.	Coeff.	S. E.	Coeff.	S. E.	Coeff.	S. E.	Coeff.	S. E.
Human arts – OLS	**-.1806**	.0071	**-.1807**	.0069	**-.1844**	.0065	**-.1778**	.0062	**-.1686**	.0060
2SLS	.0170	.0989	-.0126	.1008	.0039	.0902	.0136	.0861	.0059	.0851
Clusters	2,172		2,188		2,200		2,213		2,229	
Cluster-robust F-test	17.44		16.57		15.52		17.81		17.82	
	Test val	P-val	Test val	P-val	Test val	P-val	Test val	P-val	Test val	P-val
Endogeneity test	3.984	0.046	2.63	0.105	4.551	0.033	5.153	0.023	4.223	0.0399
Hansen J test	0.347	0.841	1.04	0.606	3.086	0.379	1.282	0.734	5.354	0.1477
Anderson-Rubin F-test	0.12	0.947	0.34	0.794	0.77	0.543	0.33	0.859	1.35	0.2495
Human arts share	.0826		.0842		.0906		.0930		.0976	
Women Share	.3827		.3830		.3878		.3962		.4056	
# observations	20,716		21,876		23,477		25,520		28,026	

Note: All regressions include graduation year dummies, parental education and high school fixed effects. Bold = significant at 1% level; italics = significant at 5% level.