Door Opener or Waste of Time?

The Effects of Student Internships on Labor Market Outcomes

Nils Saniter* — DIW Berlin and IZA Bonn

Mathias Schumann† — Universität Hamburg

Thomas Siedler‡ — Universität Hamburg and IZA Bonn

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Abstract

This paper studies the causal effect of student internship experience on wages later in life. We use variation in the introduction and abolition of mandatory internships at German universities as an instrument for completing an internship while studying. Employing longitudinal data from graduate surveys, we find positive and significant wage returns of about six percent in both OLS and IV regressions. The positive returns are particularly pronounced for individuals and areas of study that are characterized by a weak labor market orientation, and for graduates in humanities and social sciences.

Keywords: internships, skill development, higher education, labor market returns, instrumental variable

JEL Classification: I23, J01, J31

*DIW Berlin, Mohrenstrasse 58, 10117 Berlin, Germany. Email: nsaniter@diw.de.

†Universität Hamburg, Von-Melle-Park 5, 20146 Hamburg, Germany. Email: mathias.schumann@wiso.uni-hamburg.de

‡Universität Hamburg, Von-Melle-Park 5, 20146 Hamburg, Germany. Email: thomas.siedler@wiso.uni-hamburg.de.

I. Introduction

Internships have become a widespread phenomenon among university students in many countries throughout North America and Europe. Callanan and Benzing (2004), for example, argue that internships in the US have become increasingly popular as a way to bridge the transition from education to work, with three out of four college students completing an internship in 2004, compared to fewer than 40 percent of students in 1980. In Germany, 55 percent of students who are currently enrolled in a university report having completed an internship during the past twelve months (Krawietz et al., 2006). By the time students finish their studies, nearly 80 percent report at least one absence from university to complete an internship (Scarletti, 2009).

What motivates students to complete internships while enrolled at university? First and foremost, students expect internships to pay off after graduation when they enter the labor market. Indeed, when asked for their main motivation for undertaking an internship, most cite the desire to get to know the work environment and gather practical work experience. Many also hope that an internship will help them to find employment later. The desire to earn money as an intern appears to be only a secondary motivator (Krawietz et al., 2006).

The surge in popularity of internships in higher education is not only a consequence of individual choices; it is also the result of universities emphasizing the importance of internships as part of the broader educational experience. Following the policy changes implemented as part of the Bologna Reform, graduates' employability has become a central objective of higher education across Europe (Teichler, 2011). Universities have been called upon to prepare their graduates better for the transition to work by focusing on competencies that are relevant to the job market. Internships have been identified as an effective means of building these competencies (Wolter and Banscherus, 2012; Teichler, 2011). As a consequence, many universities urge students to complete internships or even make internships an integral part of the curriculum.

Internships are believed to help students build work-relevant skills, gain specific knowledge of their future occupations, develop a clearer self-concept, and confirm or redirect individual career goals (Brooks et al., 1995). Most of the skills acquired during internship are general and transferable (Busby, 2003). These attributes may then translate into various favorable outcomes for the transition into the labor market and early career success, for example, shorter job search duration, lower probability of unemployment, more stable job positions, better job match, and increased earnings. However, internships also produce costs due to the investment of time, effort and sometimes even money. Interns have to accept educational opportunity costs and might enter the labor market later than non-interns. Considering the fact that most internships are poorly paid or not paid at all, it is not surprising that some debate has arisen about the potential downside effects of internships, namely the allegation that firms exploit highly qualified students as cheap workers (Wolter and Banscherus, 2012). The overall effect of internships on individual labor market outcomes is unclear, and empirical research is needed to provide a basis for sound conclusions.

Based on economic theory, we anticipate student internships to have positive wage returns. Human capital theory (Becker, 1993; Mincer, 1974) predicts that the additional knowledge, skills and competencies accumulated as an intern result in higher pay if the time spent on an internship is more rewarding than the time spent studying.[1] Signaling theories point out that employers' hiring decisions are made under uncertainty since the productivity of potential workers is unknown. Job seekers may therefore use internships and positive reference letters provided to them upon completion of the internship to signal high ability, which may result in improved job matching and higher earnings (Spence, 1973; Akerlof, 1970; Schnedler, 2004). Screening theory predicts that firms use such signals to more accurately assess workers' hidden productivity (Stiglitz, 1975). Social capital theory (Bourdieu, 1986; Coleman, 1988) also foresees positive labor market returns of internships because of the opportunity they provide to establish relationships with co-workers and potential employers. These social ties might lead to better jobs after graduation (Granovetter, 1995).

For the empirical investigation, we use longitudinal data from graduate surveys conducted by the German Centre for Research on Higher Education and Science Studies (DZHW) that provide information on student internships and wages later in life. In order to account for the endogeneity of students' decisions to undertake an internship, we employ a two-stage least squares (2SLS) approach and instrument internship completion with mandatory internships. Exogenous variation comes from the introduction and abolition of mandatory internships at the university and area of study level. The first-stage regressions suggest that the presence of mandatory internships has a large and significant impact on the likelihood of acquiring internship

[1] We suspect that this is likely given that most students do their internships between terms and therefore do not miss lectures or classes.

experience during the course of studies. In fact, students have a 56 percentage points (80 percent) higher likelihood of completing an internship during the course of their studies if the internship is mandatory. Internship experience causes wages to rise by around six percent and are very precisely estimated, both in OLS and IV regressions. The positive returns are particularly pronounced for individuals and areas of study with a weak labor market orientation[2], and for humanities and social science graduates. For example, student internship experience increases wages by 11 percent for graduates in humanities and social sciences, compared to wage returns of just below six percent among graduates in science, mathematics and engineering. Across other subgroups of the population, however, we do not detect heterogeneous treatment effects.

We provide arguments and comprehensive evidence that mandatory internships are as good as randomly assigned, conditional on certain pre-determined explanatory variables, such as area of study and university fixed effects. To support the credibility of the findings, several aspects are addressed: (1) the risk of self-selection into study programs with mandatory internships; (2) variation over time in requirements to complete an internship as a major source of exogenous variation; (3) the impact of potential confounders, that is, simultaneity in the introduction or abolition of mandatory internships with other changes at the level of area of study (e.g., changes in the structure of the study program, access to IT services, skills training, availability of career counseling); and (4) differences in the quality of universities and study programs. Importantly, detailed evidence from various student surveys document that the question of whether internships are mandatory plays no role in students' choices of university or field of study. We also implement a novel econometric technique for evaluating robustness of results to omitted variable bias (Oster, 2014). Consistent with the OLS and IV findings, the bias-adjusted estimations suggest that the positive returns of internship experience on wages do not suffer from omitted variable bias.

The remainder of this paper is structured as follows: section II discusses the relevant literature, section III describes the data, and section IV lays out the empirical strategy. Section V presents the main results for the effects of internship experience on wages. Section VI discusses several aspects of identification and section VII inspects whether the effects differ for various subgroups of the population. Section VIII sheds light on labor market transitions. Various

[2] Measured by a self-assessment of how important labor market considerations were when choosing the study subject (for individuals) and classified by the occupational specificity of study degrees (for areas of study), respectively. See variables description in section III.

robustness checks are presented in section IX, and section X discusses some limitations. Section XI concludes.

II. Literature

Despite the prevalence of student internships and their significance for vocational exploration, the empirical literature on causal effects of internship experience remains scant. Several studies draw conclusions based on opinion polls among interns about the perceived benefits of their work experiences (Beck and Halim, 2008; Cook et al., 2004; Shoenfelt et al., 2013; Krawietz et al., 2006). Another strand of literature compares treatment and control groups, but does not account for potential self-selection into the treatment group. Some studies have found internships to be positively correlated with interns' self-crystallization of interests and values (Taylor, 1988) and self-efficacy (Brooks et al., 1995). Moreover, interns are reported to be more likely to adopt employer-oriented values (Pedro, 1984), to acquire job relevant competencies (Garavan and Murphy, 2001), and to possess interpersonal skills that are typically not part of the study curriculum (Crebert et al., 2004). Studies also report positive correlations of internships with shorter job search duration (Gault et al., 2000), higher job stability (Richards, 1984), more and better quality job offers (Taylor, 1988), a higher chance of choosing a career-oriented job (Callanan and Benzing, 2004), and wage increases (Gault et al., 2000; Reimer and Schröder, 2006; Scarletti, 2009).

To our knowledge the only papers that aim at estimating causal effects of internship experience are Nunley et al. (2014) and Klein and Weiss (2011). Nunley et al. (2014) conduct a résumé-audit study in the US and randomly assign three-month internship experience to fictitious job seekers. They find that applicants with internship experience receive about 14 percent more interview requests than those who were not assigned an internship. The effects are larger for non-business degree holders than for business degree holders. Due to the set-up of their field experiment, however, the authors are not able to study labor market behavior and wage effects later in life. Klein and Weiss (2011) study wage effects of mandatory internships among university graduates in Germany. The authors employ matching estimation methods and find no positive effects on wages. Similar to our study, the authors argue that the introduction of mandatory internships is independent of unobservable characteristics. However, the scope of interpreting their results is limited because they use cross-sectional data and do not utilize changes in the occurrence of mandatory internships over time, which makes their identification less robust and less credible. Further, their findings are based on relative small sample sizes.

III. Data, Variables, and Descriptive Statistics

We use longitudinal data from surveys among university graduates conducted by the German Centre for Research on Higher Education and Science Studies (DZHW). See Rehn et al. (2011), for a thorough description of the survey and data. Recent studies that have also used DZHW data are, for example, Parey and Waldinger (2011) and Grave and Goerlitz (2012). Each survey is a random sample of the student population at German universities. We employ information from three different cohorts that comprise persons who graduated in the years 2001, 2005, and 2009, respectively. For each cohort, an *initial survey* was conducted around one year after graduation from university. Around five to six years later, a *follow-up survey* was conducted. For the cohorts 2001 and 2005, data are available for both waves, the initial and the follow-up survey. For the 2009 cohort, only the first wave is available. Figure 1 visualizes the timing of the data collection.

In the initial survey, students were asked whether they did a voluntary and/or mandatory internship during the course of their studies. We use this information to generate the key dummy variable for whether students did an internship and the instrument dummy variable for whether the study regulations included a mandatory internship. Further information was collected on details of the area of study and universities as well as on the graduates' opinions about their university studies. The surveys also include comprehensive demographic, socioeconomic and educational information. In particular, the survey collects various proxy variables for students' intelligence, ability, and labor market orientation, and information on the parental background. The outcome variable, gross monthly wages, is self-reported for the job at the time of the interview and measured in euros adjusted to 2005 prices.

Throughout the analysis, we mainly work with a sample which comprises all available waves for the three graduate cohorts as indicated by the shaded areas in Figure 1. This sample helps increasing the precision of the estimates, which will become particularly relevant when studying heterogeneous effects in section VII. We borrow the idea of pooling the data from Parey and Waldinger (2011). In section IX, we also distinguish between short-term (e.g., one year after graduating from university) and longer-term labor market effects (e.g., five to six years after graduating), using subsamples from of all available data.

A typical feature of some university subjects and degrees is that they imply an obligatory second phase of education. For example, prospective teachers take a first state exam upon completing their university studies and then have to complete a 1.5 year practical training phase in the classroom before taking a second state exam, which then enables them to work as a teacher. Similar obligatory second educational phases of varying duration exist for, i.e., lawyers, clerics and medical doctors in Germany. During this period, individuals are outside the regular labor market. For this reason, we exclude all individuals from our sample who finished university with a state exam (lawyers, clerics, pharmacists, teachers, and physicians) or reported having to complete an obligatory second phase of education. Furthermore, we exclude graduates who finished university with a bachelor's degree because of small sample size issues. Bachelor graduates were only interviewed in 2009. Moreover, Bachelor's degrees imply a shorter duration of study than other university degrees (Diplom, Magister, Master) and are less accepted by employers in Germany. Finally, we keep only observations that have non-missing values for all relevant variables. This results in a sample size of 13,976 graduates, with 19,736 person-wave observations. 6,790 graduates are observed in both the initial survey and the follow-up survey.

Table 1 reports summary statistics for the overall sample (column 1) and differentiated by first and second wave observations (columns 2 and 3) and by internship experience (columns 4 and 5). The numbers in column 1 in Table 1 show that the average year of birth is 1977, 53 percent are female, nearly one in three graduates completed an apprenticeship before studying, and the final high school grade is 2.2 (on a scale 1-5 with 1 signifying "excellent" and 5 "failing"). Further, many students come from highly educated families, with 37 percent of mothers and 50 percent of fathers having graduated from an upper secondary school. Columns 4 and 5 indicate that students who did an internship express a stronger labor market orientation in their self-assessment when asked "To what extent did labor market considerations play a role when choosing your area of studies?" (on a scale 1-5 with 1 signifying "not at all" and 5 "very much"). It is important to point out that labor market orientation refers to a point in time prior to entering university and can therefore be considered to be a pre-determined variable. With respect to the outcome variable—log monthly wages—the unconditional means show that students who did an internship during the course of their studies receive quite similar wages to their fellow graduates.

IV. Estimation Method

To estimate the effect of internship experience while attending university on wages later in life, we use a 2SLS setup and instrument internship experience with the presence of mandatory internships. The two main equations are:

$$
\begin{aligned}
Log(Wage) = & \beta_0 + \beta_1 Internship + \beta_2 Female + \beta_3 GradCohort + \beta_4 BIRTHYEAR + \\
& \beta_5 AREA + \beta_6 UNIVERSITY + X\gamma + \epsilon \qquad (1) \\
Internship = & \alpha_0 + \alpha_1 Mandatory + \alpha_2 Female + \alpha_3 GradCohort + \alpha_4 BIRTHYEAR + \\
& \alpha_5 AREA + \alpha_6 UNIVERSITY + X\gamma + \varepsilon, \qquad (2)
\end{aligned}
$$

where $log(Wage)$ is the logarithm of wages, $BIRTHYEAR$ is a 22×1 vector that comprises indicators for year of birth, $AREA$ is a 53×1 vector that comprises fixed effects for students' area of study, and $UNIVERSITY$ is a 262×1 vector that comprises university fixed effects.[3] $Female$ is a dummy variable indicating gender and the vector $GradCohort$ contains dummy variables for the graduation cohorts.[4] Depending on the particular specification, the vector X contains different sets of additional explanatory variables. In equation (1), the variable $Internship$ equals one if the student did an internship while studying, and zero otherwise. In the first-stage equation (2), the dichotomous variable $Mandatory$ equals one if an internship was mandatory during the course of studies, and zero otherwise.

We present results for two different specifications. In our baseline model, we control for individuals' year of birth fixed effects, area of study, and university fixed effects, a female and a survey wave dummy, dummy variables for the graduation cohorts, as well as a dummy variable for graduating from a university of applied sciences. We call this the parsimonious model. In the second specification—called the full model—we add several predetermined variables that are likely to be good proxy variables for students' intelligence, ability, and labor market orientation. We control for students' final high school grade (*high school grade*), whether they completed an apprenticeship before studying (*apprenticeship*), the self-reported influence of labor market

[3] Note that for $AREA$, the data only allow us to observe the areas of study, which are referred to as *Studienbereiche* in the nomenclature of the Federal Statistical Office (2014), but not the exact subject. For example, we can observe whether someone studied Romance philology, but not whether the subject was French, Italian, Spanish, or Portuguese.

[4] We also control for a survey dummy variable, capturing whether the outcome is measured in the initial survey or the follow-up survey.

aspects on their choice of what career to pursue and thus what to study at the university (*labor market orientation*), as well as a full set of dummy variables for mother's and father's highest general educational degree (four groups each).[5]

V. Results

The OLS and IV results for equation (1) are presented in Table 2. Each column shows the estimated coefficients and standard errors from a different regression. The first two columns present results for the OLS regressions, and columns 3 and 4 show the IV estimates. The standard errors are clustered at the individual level, accounting for the fact that for 6,790 graduates we use repeated observations. In the robustness section IX, we also present results when clustering at the level of departments, where departments are defined as unique combinations of area of study and university.

All regressions in Table 2 show a positive and significant relationship between internship experience while attending university and wages later in life. The OLS coefficients for both samples suggest that a student who gained labor market experience through an internship during the course of his or her studies has six percent higher wages later in life.[6] The estimated coefficients are statistically significant at the one percent level. Importantly, the IV estimates also point to a causal positive and significant relationship between internship experience and graduates' labor market wages, with estimated effects of around six percent. Taken as a whole, the results in Table 2 do not suggest a large and significant bias in the OLS estimates, conditional on a broad set of explanatory variables.[7]

Table 2 also shows the estimated effects for other selected explanatory variables. Female graduates have around 17 percent lower wages than male graduates. These results are broadly consistent with previous findings for Germany (Machin and Puhani, 2003; Leuze and Strauß, 2009). Moreover, the estimates for the variable *apprenticeship* reveals that graduates who com-

[5]Mincer type wage equations typically control for age and age^2 to proxy work experience. Age variables have been omitted from the baseline specification because they are likely to be outcome variables themselves. This is because internship experience might delay labor market entry due to the extra time working rather than attending university. We experimented with the inclusion of age variables and found that this leaves our results unchanged.

[6]Throughout the article, we interpret the coefficients in the log-linear wage models in terms of percentage points, obtaining the percentage changes using the formula $(exp(\widehat{coef.}) - 1)$.

[7]The results from a Durbin-Wu Hausman test of endogeneity also suggest that internship experience while attending university is unlikely to be an endogenous variable, with a p-value of 0.79 in the full model. Moreover, in the robustness section IX we explore the sensitivity of our estimates to omitted variable bias by studying coefficient movements and movements in R-squared values when including additional controls in the spirit of Oster (2014). These analyses suggest that endogeneity bias is unlikely to affect our estimates.

pleted an apprenticeship before studying have around eight percent higher wages. In the IV regressions, the magnitude of the estimate is quite similar to the effect of internship experience during studying. Note, however, that the majority of apprenticeships in Germany last around three years, whereas student internships last on average twelve weeks (Scarletti, 2009). A comparison of these two estimates underlines the economic relevance of the positive wage returns of internships.

Is a six percent increase in wages due to internship experience a small or rather large effect? To answer this question, a comparison with the empirical literature on causal wage returns of education is helpful. For the US, Angrist and Krueger (1991) and Acemoglu and Angrist (2000) report causal wage returns to schooling of around 6-10 percent, and Oreopoulos (2007) estimates returns of around 13 percent. For Germany, the returns to schooling estimates vary between zero and ten percent (Becker and Siebern-Thomas, 2007; Pischke and Wachter, 2008; Saniter, 2012). A comparison with this literature therefore suggests that the returns to internship experience are roughly comparable to the wage returns of one more year of schooling and therefore quite significant in size.[8]

First-stage results based on equation (2) are presented in Table 3. We again report estimates for the parsimonious and the full model, respectively. The estimated coefficient for the instrumental variable *Mandatory internship* is always positive and precisely estimated at the 0.1 percent significance level. The estimates suggest that a compulsory student internship increases the likelihood of internship experience by around 56 percentage points. The corresponding F-statistics of 41 and the partial correlation coefficient of 0.55 also point toward a strong first-stage relationship. In line with the summary statistics in Table 1, the first-stage estimates show a negative relationship between studying at a university of applied sciences and having completed an apprenticeship before studying and the likelihood of doing an internship during the course of studies.[9]

[8] Note, however, that the local average treatment effects are estimated for different groups. The literature on causal returns to schooling estimates returns for individuals with low levels of schooling who are forced to acquiring more education because of an increase in compulsory years of schooling. In this study, we estimate wage returns of internship experience for university graduates.

[9] Students at universities of applied sciences are less likely to complete an internship while being enrolled at university, but are more likely to do a "practical semester" during the course of their studies than students at university. In unreported regressions, we examined this issue and found that the results are not affected by controlling for practical semesters.

VI. Aspects of Identification

This section provides arguments and detailed evidence that support the credibility of our results for causal interpretation. Four aspects are addressed: (1) the possibility of self-selection into study programs with mandatory internships; (2) variation over time in the occurrence of mandatory internships as a major source of exogenous variation; (3) the impact of potential confounders, that is, simultaneity in the introduction or abolition of mandatory internships with other changes at the level of university and the area of study; and (4) differences in the quality of universities and study programs.

1. Self-Selection into Study Programs with Mandatory Internships

Our identification approach crucially hinges on the assumption that individuals do not select themselves systematically into study programs with mandatory internships based on unobservable characteristics. Put differently, the instrument must provide variation that is exogenous given the control variables. This assumption would be violated if, for example, more ambitious students were more likely to choose subjects with mandatory internships, and if they were also more successful in the labor market later in life. Moreover, ambition would have to be an omitted variable that is not sufficiently captured by the pre-determined observables such as high school degree, labor market orientation and parents' educational background, all of which are included in the full model specification. We believe that it is very unlikely that students choose their subjects and universities based on whether internships are mandatory. Instead, the quality and reputation of the study programs and universities are likely to be the most important choice determinants (Hoyt and Brown, 1999; Parey and Waldinger, 2011).[10] Several German national newspapers such as *Handelsblatt*, *Die Zeit*, and *Der Spiegel* regularly publish university rankings by subjects and institutions, and this information is widely circulated. Hachmeister and Hennings (2007) report that the majority of high school students in the final grade in Germany know and consult these rankings. However, none of these published rankings includes information on internships. Moreover, gathering information from university websites as to whether or not internships are mandatory is rather difficult, and unlike in the U.S., German universities do not distribute brochures or college catalogues to prospective students (Hoyt and Brown, 1999).

[10] Proximity to the nearest university also plays a role (Spiess and Wrohlich, 2010).

Further evidence comes from surveys about the field of study and university choices. Table A.2 in the appendix summarizes studies that ask students in Germany about factors that influence these choices. The table provides an overview of the type of survey, sample size, the relevant question, key findings, and whether (compulsory) internships played a role in student university choice. Most surveys elicit students' reasons for their university choice on a 5-point Likert scale (1 = "very important"; 5 = "not important at all"). For example, the representative surveys among first-year students conducted by the DZHW ask "How important are the following reasons for your choice of study?" on aspects such as reputation of the university, accessibility of the campus from home, quality of the academic program, etc. (Heine et al., 2005, 2009). Whether an internship is mandatory−or whether universities have good connections with firms that enable students to more easily find internships during the course of their studies−was not among the items listed in the surveys. Indeed, none of the studies we have found on this topic for Germany lists mandatory internships as a relevant aspect of study choice (Hachmeister and Hennings, 2007; Hachmeister et al., 2007; Bartl and Korb, 2009; Institut für Marktforschung GmbH, 2014). Neither do student surveys in the U.S. and Canada provide evidence that internship availability seems to play any role in students' choices (Pryor et al., 2012; Canadian Undergraduate Survey Consortium, 2004; Hoyt and Brown, 1999). This suggests that methodologists, educators and researchers do not think that the question of whether an internship is mandatory during the course of university studies is a relevant aspect in students' choice of university or field of study.

In summary, we therefore believe and argue that students' self-selection into fields of study with mandatory internships is very unlikely to bias the present estimates.

2. Variation Over Time in the Occurrence of Mandatory Internships

In this section, we shed some light on the introduction and abolition of mandatory internships across cohorts, universities, and areas of study, which is the main source of exogenous variation. The data allow us to identify the existence of mandatory internships for individuals who report having chosen a certain subject in a certain area of study at a certain university. We also know the cohort to which they belong. However, single observations do not reveal whether there was a change in the occurrence of mandatory internships for earlier or later cohorts. In order to capture potential status changes, we therefore refer to departments as the smallest institutional units available, where departments are defined to be unique combinations of universities and areas of study. We then calculate the proportion of students in a department reporting a mandatory internship, separately for each cohort. If, from one cohort to the next, the majority of reports in one department changes from the non-existence to the existence of mandatory internship, then we consider this department to have introduced mandatory internships. If the change occurs in reverse direction, then we think of the department as having abolished mandatory internships. In the same fashion, this procedure also allows us to detect departments that have not changed their status. Table 4 sorts department and observations into distinct groups that result from the outlined procedure. As the reports within the combinations of department × cohort (*cells* hereafter) are rarely univocal, we have to define the (non-)existence of mandatory internships along the lines of thresholds. The 50/50 threshold defines cells as having mandatory internships if more than half of all graduates report that an internship was mandatory, and zero otherwise. Alternative thresholds are 60/40 and 70/30, which are more restrictive in the sense that they determine the status of cells only if the majority is more pronounced. That is, assignment is only established if the proportions exceed the 60 (70) percent level or stay below the 40 (30) percent level. When choosing the optimal threshold, one therefore faces a trade-off between maintaining a high number of observations (best 50/50) and precisely assigning departments into the different groups (best 70/30).[11]

Column 1 of Table 4 defines the different groups, distinguishing between departments which introduced or abolished mandatory internship (Introducer, Abolisher), changed back and forth (Introducer & Abolisher), or made no changes (Stayer). Since not all departments are included in all surveys 2001, 2005, and 2009, we have an unbalanced panel data set. This is reflected by a final category (Uncertain) that comprises all departments that cannot be observed in all three surveys. The sample comprises 283 different universities and 54 different areas of study, out of which a total of 1,494 departments can be distinguished.[12] For the 50/50 threshold, column 2 of Table 4 shows that there are 102 departments that introduced mandatory internships

[11] We are aware that this approach involves some measurement error as we only observe departments and not their actual study regulations, which would be more precise. However, we believe that this is the best we can do to evaluate the variation in mandatory internships over time, since no such information at the department level is available from external data sources. In the robustness section, we use all three thresholds to generate alternative instrumental variables to evaluate the robustness of the main findings. However, none of the alternative instruments captures the exposure to mandatory internships as precisely as students' own reports.

[12] Note that one university typically covers less than 54 areas of study, for which the number of departments is less than 283 × 54.

between 2001 and 2009. Conversely, 95 departments abolished mandatory internships, 23 both introduced and abolished them, and 99 did not change their status. For 1,175 there is missing information for at least one period. The corresponding numbers in column 3 do not count departments but person-wave observations. They suggest that around 15 percent out of all observations ($1,411 + 1,212 + 419 = 3,042$ out of 19,736) belong to a department in which variation occurred over time. If we disregard the departments for which there is uncertainty about status changes due to the fact that they were not surveyed in all years, this share increases to 52 percent (3,042 out of $19,736 - 13,884 = 5,852$) indicating that more than half of the departments have changed the status of mandatory internships between the 2001 and 2009 cohorts. The alternative thresholds 60/40 and 70/30 in columns 4-6 in Table 4 also suggest that the majority of departments experienced changes in mandatory internships over time. Hence, there is considerable variation in mandatory internships at the department level over time that contributes to the identification of our IV estimates.

3. Impact of Potential Confounders

If the introduction or elimination of mandatory internships coincides with other changes at the level of the area of study that could in turn affect wages, this would pose a major threat to our identification strategy. For instance, if the introduction of mandatory internships coincides with improvements in career counseling at the departmental level, estimates of internship experience would likely be upward biased. In order to assess the influence of such potential confounders, we make use of items in the questionnaires that elicit the respondent's evaluation of various aspects of studying. More specifically, we examine twelve different quality indicators of the area of study and/or university that may have an independent effect on wages, thereby potentially biasing the main results.

The twelve indicators cover the following four areas: (1) overall quality of education, (2) educational media and infrastructure, (3) training, and (4) career counseling. Respondents can rate items in each of the categories on a five-point scale, from "very bad" (1) to "very good" (5).[13] We test whether changes in the quality indicators across cohorts coincide with

[13]Figure A.1 in the appendix displays the distribution of the twelve variables. The figure shows that there are considerable differences in how graduates evaluate the quality of their studies. For example, around 50 percent of the graduates rate the structure of the degree program and the modernity of methods taught as very good or good (panel A). In contrast, fewer than 15 percent of graduates assign this positive rating to the provision of career orientation (panel D).

the introduction and elimination of mandatory internships by estimating regressions similar to first-stage equation (2), specifically:

$$EduQual_j = \alpha_0 + \alpha_1 Mandatory + \alpha_2 Female + \alpha_3 GradCohort + \alpha_4 BIRTHYEAR + \alpha_5 AREA + \alpha_6 UNIVERSITY + X\gamma + \varepsilon \quad (3)$$

where the outcome variable $EduQual_j$ measures the jth variable of educational quality with $j = 1, ..., 12$ and is coded one if respondents tick a four ("good") or five ("very good"), and zero otherwise. $Mandatory$ indicates whether internships were mandatory or not. In line with the methodology described above, we use graduates' self-reported information and the 50/50, 60/40 and 70/30 thresholds for identifying changes in the occurrence of mandatory internships at the level of departments over time. The estimate of interest is the parameter $\hat{\alpha}_1$, which indicates whether changes in mandatory internships across cohorts are significantly correlated with changes in educational quality. For each variant of equation (3), we only report the estimates from the full specification.[14]

Table 5 reports the estimates of α_1 from equation (3) for the twelve different educational outcomes. Each combination of estimated coefficient and standard error in parentheses comes from a different regression, and columns 1-4 report the findings for using different definitions for whether internships were mandatory. Positive coefficients imply that the presence of mandatory internships coincides with improvements in the quality indicators, and negative coefficients indicate a deterioration. The overwhelming majority of the estimated coefficients in Table 5 are close to zero and not statistically significant at the 5 percent level. The only estimates that are statistically significant at the 5 percent level are for the outcome variable *Up-to-date education* in columns 1 and 4. In unreported regressions, we also estimated logit models, which yields a very similar picture. Overall, given that 46 out of the 48 estimated coefficients in Table 5 are small in magnitude and not statistically significant different from zero, we believe that it is rather unlikely that our main findings are biased by other educational changes at the departmental over time that coincide with changes in the occurrence of mandatory internships.

[14]Results from the parsimonious specification are very similar to the full model and are available from the authors upon request.

4. Differences in Quality of Universities and Study Programs

One potential concern may be that the quality and reputation of the university and/or the study program are correlated with the availability of mandatory university-organized internships, and with graduates' labor market outcomes later in life. If good universities offer, on average, more programs with mandatory internships and if their graduates are also more successful in finding high quality jobs, then instrumental variable estimates can be upward biased. To account for this, the regressions control for a maximum set of university and area of study fixed effects. As a result, differences between universities and differences between areas of study at a given university are controlled for. To further mitigate this concern, one robustness check in Section IX involves the inclusion of 1,494 dummy variables that represent unique combinations of university and area of study (i.e., departments). Another sensitivity analysis includes dummies for combinations of area of study and *type* of university (e.g. university or university of applied sciences). The estimates from both robustness exercises are not significantly different from the main results in Table 2. Differences in the quality of universities and their areas of study therefore do not pose a threat to our identification strategy.

VII. Heterogeneous Effects

This section studies the heterogeneity of treatment effects across subgroups of the population. Panel A of Table 6 reports the impact of internship experience separately for women and men. Difference in returns to internship experience may exist in a similar way as, for example, college degree returns are higher for females than for males (Jacobson et al., 2005; Jepsen et al., 2014). Along the coefficients and standard errors for OLS and IV models we also report the relevant p-values from interacted models. Panel B investigates heterogeneity in treatment effects by parents' education. The sample is divided by whether or not one of the parents has an upper secondary school degree. Students with highly educated parents might benefit from their social networks, irrespective of their own labor market experience. Hence, a student internship might be more rewarding for students without these intergenerational networks. In panel C in Table 6, separate effects are estimated for graduates by their final high school grade, since students with good and very good grades are likely to have other unobservable characteristics (e.g., high motivation, intelligence, social skills) that might make them benefit more from an internship than students with lower grades. Further, due to their abilities, they might be more likely to participate in an internship of high quality and prestige, an aspect that we cannot observe. The estimates in panel D show heterogeneity of treatment effects across students' labor market orientation. Students for whom labor market aspects played a critical role in their choice of what to study might be more ambitious and motivated during their internships than students with lower levels of labor market orientation, potentially leading to higher returns. Alternatively, internships might be particularly beneficial for students who have not given much thought to labor market aspects. An internship experience might help them to gain a clearer self-concept and develop better career plans. Panel E in Table 6 reports separate treatment effects according to whether the area of study has a strong or weak labor market orientation. Following Scarletti (2009), we sort graduates' areas of study into those with a *strong* labor market orientation when they lead to a particular profession. Examples are medicine and architecture, since nearly all medical students become doctors and most students of architecture work as architects later in life. In contrast, study areas with a *weak* labor market orientation do not necessarily lead to a particular profession. They teach more general skills that qualify graduates for a wide range of different jobs. Examples are history, philosophy, and languages.[15] Finally, panel F shows the impact of internships separately by field of study. Three groups are distinguished: (1) science, mathematics, engineering; (2) business and economics; (3) humanities and social sciences.

The estimates in panels A, B and C in Table 6 do not point toward heterogeneous treatment effects of internship experience by gender, parental background, or high school performance. In contrast, the point estimates in panels D and E suggest that internships are particularly beneficial for students with lower levels of labor market orientation. For example, the IV estimates in Panel D suggest returns of around 11 percent for students for whom labor market aspects did not play an important role in their choice of what to study compared to only 2 percent for those who took labor market aspects strongly into consideration. The difference of nine percentage points is statistically significant from zero at the 5 percent level, as indicated by the p-value of 0.016 from the interacted model. In line with this finding, the estimates in panel E in Table 6 also point toward higher returns of internship experience for graduates in areas of study with a weak labor market orientation, with the difference being statistically significant at the 10 percent

[15]See Table A.1 in the appendix for a complete classification of areas of study into weak and strong labor market orientation.

level (p-value of 0.051 from the interacted model). The heterogeneous effects by field of study in panel F are also consistent with those in panels D and E. The estimates suggest that graduates from humanities and social sciences (without economics) have larger internship wage returns compared to those who studied science, mathematics, engineering, business or economics.

We conclude that those who benefit most from internship experience are individuals with a weaker labor market orientation or who study fields with a weaker labor market orientation. One explanation for this is that internships help students to develop a better understanding of their future occupation and a clearer concept of their own preferences. Moreover, for graduates in subjects with a weak labor market orientation, internships can help to establish contacts with potential employers, which may facilitate the screening of candidates when the subject of studies is not a strong signal.[16]

VIII. Transition to the Labor Market

In this section, we examine how internship experience affects the transition to the labor market, specifically during the first years after graduating from university. Before turning to regression techniques, we begin with descriptive evidence from the DZHW surveys that collect information on how former students found their first job. 16.6 percent of all graduates say that they found their job through connections from a previous internship or through connections made while working on their final master (or diploma) thesis in a firm.[17] We also know whether or not students completed an internship after graduating from university. Taking the subsample of individuals that completed an internship while attending university but gathered no additional internship experience afterwards, allows us to relate the information on how they found their first job to their university internship. In this group of students, 17.3 percent report that they found their job through internship connections. This figure suggests that part of the positive internship returns is likely to be driven by attractive job offers from the very firm with which they completed an internship while studying.

Next, we use calendar information in the surveys to construct binary activity indicators

[16] In unreported regressions, we also distinguished between students who graduated from a university versus a university of applied sciences. Studies at universities of applied sciences are more practically oriented and the treatment effect of internship experience might therefore differ by the type of university degree. The regression results did not point toward heterogeneous effects.

[17] Note that it is not possible to distinguish between connections through an internship and connections while working on the final thesis because both are elicited together in one item of the questionnaire.

for every month during the first five years after graduation. Monthly information is available for employment, unemployment, and full-time employment. Figure 2 graphically displays the estimated coefficients of internship experience for these activities from OLS and IV regressions. The vertical bars represent the 95 percent confidence intervals. Panel A in Figure 2 displays the effects of internship experience on the probability to be employed. While there are no significant effects during the first two years, later years exhibit positive coefficients, though significant at the five percent level only during the third year. Panel B reports estimates on the likelihood to be unemployed. The graph reveals that internship experience decreases the risk of being unemployed during the first year. However, in later years, this effect levels off to nearly zero and becomes insignificant in most regressions. Panel C in Figure 2 shows the results for being in full-time employment. This indicator is only defined for employed individuals in the respective month. The graph shows a higher propensity to be in full-time employment in most months, with significant point estimates mainly between 20 and 35 months after entering the labor market.

Overall, the descriptive evidence from the surveys and the findings in Figure 2 suggest that internships have positive wage effects by helping to find the first job and by more favorable employment outcomes during early years of graduates' labor market career.

IX. Robustness Checks

In this section, we first discuss alternative instrumental variable estimations to evaluate the robustness of the main findings in Table 2. Then, we present sensitivity checks with respect to sample attrition, clustering, and additional explanatory variables. Thereafter, we explore the sensitivity of our estimates to omitted variable bias in the spirit of Oster (2014).

Table 7 presents results from five alternative instrumental variable estimations, together with the corresponding first-stage estimates and F-statistics. The first alternative instrument IV_{50} is an indicator variable equal to one if the majority of students in a certain cell (defined as cohort $\times$ department) say that an internship was mandatory, and zero otherwise. This instrument measures the strength of students' exposure to mandatory internships at the departmental level.[18] Similarly, the instruments IV_{60} and IV_{70} are dichotomous variables equal to one if the majority of students of a given cohort in a certain department (e.g., 60 or 70 percent, respectively) report

[18]For a similar approach, see Parey and Waldinger, 2011, who use exposure to scholarships of the Erasmus program to instrument study stays abroad.

that a student internship was mandatory, and zero if fewer than 40 or 30 percent, respectively, do so. The fourth instrument IV_{Ratio_1} measures the proportion of graduates for whom an internship was mandatory. Similar to the first three instruments, it is defined for cells that are constructed from combinations of cohorts × departments. The fifth instrument IV_{Ratio_2} also measures the proportion of graduates with a mandatory internship, but it is based on groups that are constructed from combinations of university *starting years* × departments. Using the year when individuals entered university rather than the graduation year may improve the precision of the instrument in the sense that it is more likely to capture different study regulations. In most cases, study regulations are imposed on students at the beginning of their studies. It is important to point out that the various definitions used are likely to introduce measurement error in the instrument. However, this is not a major concern for identification if the first-stage is strong enough. The point estimates in Table 7 suggest positive returns on internship experience of between 11-15 percent, with nearly all point estimates being statistically significant at the 5 percent level. Note, however, that the first-stage relationships are less precisely estimated than in our main instrumental variable regression, with the F-statistics ranging between 17 and 28. Taken together, the estimates in Table 7 strongly support the main findings in Table 2, suggesting that student internships have a positive causal impact on wages after graduating from university.

In unreported regressions, we also estimated heterogenous effects similar to those in Table 6 using these five alternative instrumental variables. All alternative IV estimates suggest that internship returns are larger in magnitude for students with a weak labor market orientation.[19] Further, when distinguishing by the labor market orientation of the study subject, most of the internship returns are higher for graduates who study a field with a weak rather than a strong labor market orientation.[20] Similar to the results in Panels A-D in Table 6, none of the alternative IV regressions suggest heterogenous effects by gender, parental background or high school performance.

Table 8 reports the results of further sensitivity analyses based on the full model specification similar to the regressions in Table 2. First, we consider the fact that certain departments might

[19] The differences in labor market returns between graduates with a high and a low labor market orientation (as in Panel D, Table 6) is statistically significant at the 5 percent level in two IV regressions, and statistically significant at the 10 percent level in three IV regressions.

[20] Note, however, that the differences in returns are never statistically significant at conventional significance levels.

differ in educational quality, connections to firms, or degree of support provided to students in finding high-quality jobs. To control for these potential differences, panel A in Table 8 reports the estimates when controlling for a maximum set of 1,494 department fixed effects. These fixed effects are added to the full model specification, which already comprises area of study and university fixed effects. Hence, there might be the risk that this model is overspecified. It turns out that the coefficient for internship experience decreases slightly, suggesting positive returns of around five to six percent.

Second, the regressions always control for whether students studied at a university or a university of applied sciences. However, there might be differences in labor market returns for the same area of study across the two types of universities. For example, studying economics might differ in terms of quality or labor market returns between universities and universities of applied sciences. To address this concern, the regressions in panel B in Table 8 additionally include fixed effects for interactions between area of study and type of university. Reassuringly, the estimates do not change notably.

Third, there is the risk that the returns on internship experience are confounded by other forms of practical work experience. For instance, 48 percent of graduates report paid employment during the course of their studies that was related to their degree. Moreover, the requirement to complete an internship might affect whether students pursue other forms of work experience, which might be substitutes or complements for internships. The regressions in panel C Table 8 include a dummy variable for whether graduates worked during the course of their studies. The point estimates for internship experience remain largely unaffected, pointing towards positive internship returns of around six percent.

Fourth, panel D reports the results when clustering at the department, rather than at the individual level. The standard errors are nearly identical to those in Table 2 and the overall conclusions do not change.

Sample attrition might be a problem, as only 34 percent of individuals participating in the initial survey were also interviewed in the follow-up survey. To address this concern, panel E reports estimates of internship experiences on wages only measured at the time of the initial survey, i.e., around one year after graduation, and panel F reports longer term effects on wage measured around five to six years after graduating from university. Here, clustering of the standard errors is at the university level. Both IV estimates point toward positive effects of internship experience on wages of around six percent. We therefore argue that the main findings are unlikely to be biased by selected sample attrition and we note that the differences in wages in the short- and medium term are not very large (1.4 percentage points). In unreported regressions, we also estimated the potential problem of selected attrition by estimating linear probability (and probit) models on graduates' likelihood of participating in the second wave. We found no empirical evidence for differences in attrition rates between treatment and comparison group.

Finally, we investigate the robustness of our results to omitted variable bias in the spirit of Oster (2014). The author developed a new econometric method investigating how robust estimates are to omitted variable bias by studying coefficient movements and movements in R-squared values when including additional controls. This framework builds on previous work by Altonji et al. (2005) and makes it possible to compute bounding values for the treatment effect. Oster (2014) derives the following bias-adjusted coefficient for the treatment effect:

$$\beta_1^{\star\prime} = \tilde{\beta}_1 - \tilde{\delta}\frac{(\dot{\beta}_1 - \tilde{\beta}_1)(R_{max} - \tilde{R})}{(\tilde{R} - \dot{R})}, \tag{4}$$

for $\tilde{\delta} = 1$.[21] $\tilde{\delta}$ denotes the coefficient of proportionality, which captures the explanatory power of unobserved variables as a proportion of the explanatory power of observed variables. R_{max} denotes the R^2 of a hypothetical regression if one would observe all relevant (observed and unobserved) factors for the outcome variable. The bias-adjusted coefficient depends on estimated parameters ($\dot{\beta}_1, \tilde{\beta}_1, \dot{R}, \tilde{R}$) and chosen values for $\tilde{\delta}$ and R_{max}. In our case, the estimated coefficient $\dot{\beta}_1$ and the R-squared $\dot{R}$ come from an OLS regression of equation (5), and $\tilde{\beta}_1$ and $\tilde{R}$ stem from estimating equation (6).

$$Log(Wage) = \beta_0 + \beta_1 Internship + \beta_2 Female + \beta_3 UNIVERSITY + \epsilon \tag{5}$$

$$\begin{aligned} Log(Wage) = &\beta_0 + \beta_1 Internship + \beta_2 Female + \beta_3 GradCohort + \beta_4 BIRTHYEAR + \\ &\beta_5 AREA + \beta_6 UNIVERSITY + X\gamma + \epsilon. \end{aligned} \tag{6}$$

In equation (6), the vector X contains all the pre-determined variables from the full model. To identify $\beta_1^{\star\prime}$, one also needs assumptions for $\tilde{\delta}$ and R_{max}. Oster (2014) argues that $\tilde{\delta} \in [0, 1]$ is a useful bound. This is because control variables are deliberately chosen by researchers and it

[21] See Oster (2014) for the estimation of $\beta_1^{\star\prime}$ with values for $\tilde{\delta} \neq 1$.

is therefore unlikely that unobservables have a stronger impact on the outcome variable than observables. We present results for $\tilde{\delta} = 1$ and $\tilde{\delta} = 2$. The latter is assuming that the influence of unobservables on the outcome variable is double the size of the impact of observable variables. It is plausible to assume that $R_{max} < 1$, as some idiosyncratic component in the variation of the outcome variable is likely, which cannot be explained entirely by the (observed and unobserved) explanatory variables. Oster (2014) argues that $R_{max} = min\{2.2\tilde{R}, 1\}$ is a useful bound. This yields the identified set $[\tilde{\beta}_1, \beta_1^{\star\prime}(min\{2.2\tilde{R}, 1\}, 1)]$ for the treatment effect. If this set excludes zero, the result from the controlled regression (i.e., full model, equation 6) can be considered as robust to omitted variable bias.

The results of coefficient stability to omitted variable bias are shown in Table 9. Most importantly, the table reports the identified set for the treatment effect of internship experience $[\tilde{\beta}_1, \beta_1^{\star\prime}(min\{2.2\tilde{R}, 1\}, 1)]$. The identified set for the treatment internship experience if $\tilde{\delta} = 1$ is [0.061, 0.063], and the identified set assuming that $\tilde{\delta} = 2$ is [0.061, 0.065]. Hence, the bias-adjusted coefficients $\beta_1^{\star\prime}$ do not change considerably relative to $\tilde{\beta}_1$, the identified sets do not include zero, and an omitted variable bias problem is therefore unlikely. Further, Oster (2014) also suggests examining whether the bounds of the identified set are within the confidence interval of $\tilde{\beta}_1$. Table 9 shows that this is true for $\tilde{\delta} = 1$ and $\tilde{\delta} = 2$.

In summary, the results of the Oster (2014) method in Table 9 suggest that the positive wage returns of internship experience are very unlikely to be biased by omitted variables. These findings are entirely consistent with the OLS and IV estimates. Further, the findings also support the discussion on potential self-selection and the empirical evidence on confounders in section VI above.

X. Discussion

This section provides a discussion of the limitations of our study. If one trusts the validity of the instruments, the empirical findings suggest causal wage returns of student internship of at least six percent in the short and medium term. Due to data limitations, however, we cannot estimate longer term effects as individuals' wages are not observed 10-20 years after graduating from university. Moreover, data constraints prevent us from investigating whether the duration of the internship matters.[22] For instance, if the positive impact is mainly driven by practical learning experience on the job−rather than by signalling effects−longer internship experience is likely to be associated with higher wage returns. Further, we have no information on the timing of the internship, and the data also contains no information on the size, sector, and reputation of the firm or institution at which the internship took place. Hence, potential heterogenous effects by firm characteristics cannot be explored.

Despite these caveats, to the best of our knowledge, this is the first study to aim at estimating causal wage returns to internship experience. As such, this study complements the large empirical literature on the returns of schooling by estimating local average treatment effects of job experience among highly educated individuals. Whether returns vary by internship length and how important firm characteristics are for a successful transition into the labor market after graduating from university is left for future research.

XI. Conclusions

This study provides new evidence on the causal effects of student internships on wages for university graduates. The estimates from instrumental variable regressions suggest that work experience gained through student internships increases wages by around six percent. The empirical findings further suggest that graduates who completed an internship face a lower risk of unemployment during the first year of their careers. The positive returns are similar in magnitude for female and male graduates. There is also no empirical evidence of heterogeneous effects by students' socio-economic background and ability, proxied by their parents' educational attainments and students' average final high school grade, respectively. However, we do find significant differences in treatment effects with respect to the labor market orientation of students and the areas of study. Highest returns are estimated for a weak labor market orientation and humanities and social sciences, which is in line with the notion of internships serving as a means of vocational exploration and screening.

The present findings are of interest for university students, policy makers, and educators alike. Student internship experience can be regarded as a "door opener" to the labor market in terms of wages. This is relevant against the background of a contradictory debate in higher

[22] Scarletti (2009) reports that the average internship length among university students in Bavaria is around 12 weeks.

education that has gained momentum in recent decades: on the one hand, institutions of higher education are expected to incorporate labor market demands into their study curricula. On the other hand, they should guarantee freedom and independence in academic research and teaching. Our study suggests that university education—combined with practical learning through internships—might be one way of bringing these two aspects together.

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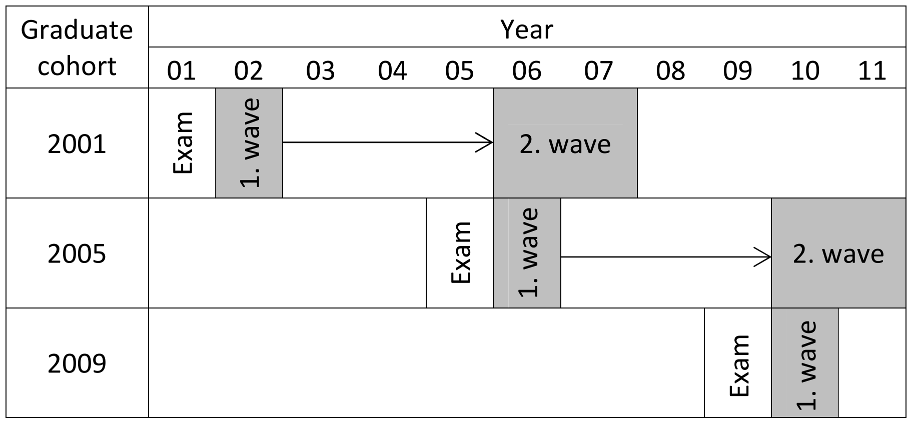
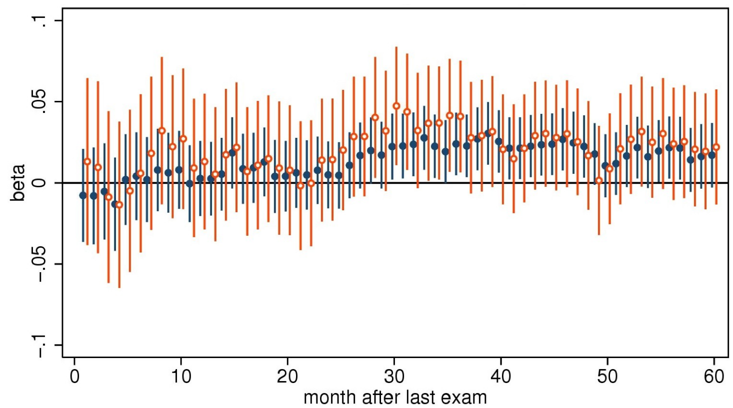
Figures and Tables

Figure 1: DZHW Panel Survey of Graduates

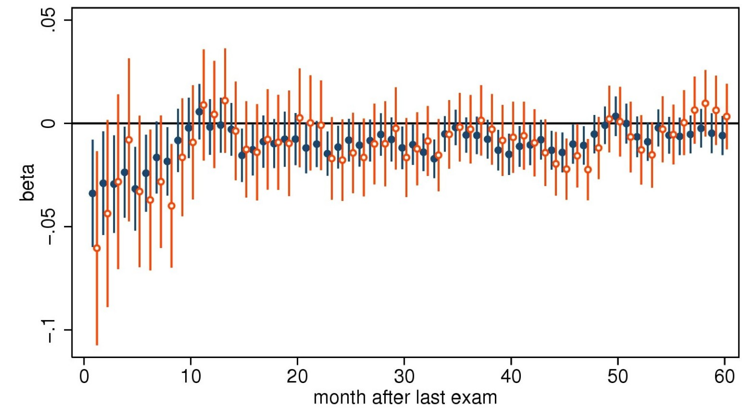


Note: Adopted from Rehn et al. (2011), p. 367. This study employs data from graduate surveys conducted by the DZHW. It includes random samples of university graduates who passed their last exam in 2001, 2005, and 2009. For the cohorts 2001 and 2005, we utilize an *initial survey* one year after graduation (first wave) and a *follow-up survey* about five to six years after graduation (second wave). For the cohort 2009, only the first wave is available. For the analysis, we use a pooled sample. It comprises all second-wave observations of the cohorts 2001 and 2005 and all first-wave observations of 2001, 2005, and 2009.

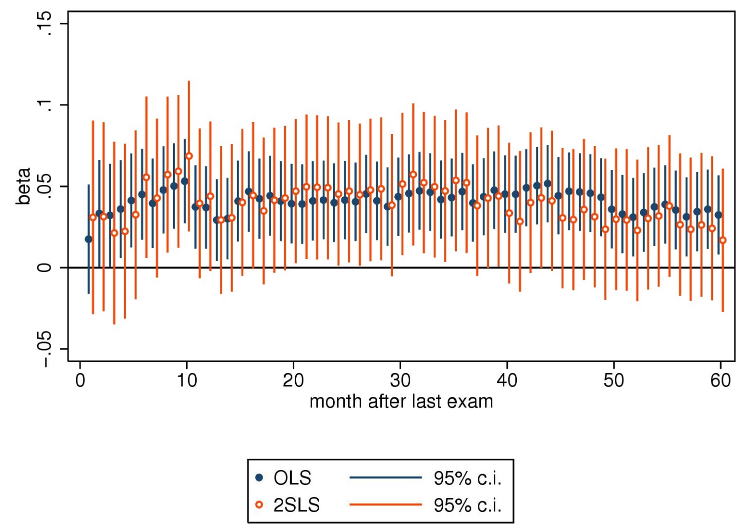
Figure 2: Transmission Variables over Time



(A) Employed



(B) Unemployed



(C) Full-time employed

Note: Estimates from OLS and 2SLS regressions for the effect of internship experience on binary variables indicating monthly status activity. Each circle represents the coefficient for one particular month. Vertical spikes stand for the 95% confidence intervals. All models control for gender, year of birth FE, area of study FE, university FE, degree type FE, university type, high school degree type mother and father FE, apprenticeship, high school grade, degree of labor market orientation and a dummy for the 2nd wave.

Table 1: Sample Means

	All	1 year after graduation	5-6 years after graduation	Internship No	Internship Yes
	(1)	(2)	(3)	(4)	(5)
Panel A. Explanatory variables					
Internship	0.66	0.66	0.67	0.00	1.00
Voluntary internship	0.41	0.41	0.41	0.00	0.63
Mandatory internship	0.48	0.48	0.48	0.05	0.70
Paid employment during studies	0.48	0.48	0.48	0.43	0.51
Year of birth	1977	1977	1976	1976	1977
Female	0.53	0.53	0.54	0.48	0.56
Apprenticeship	0.30	0.30	0.31	0.42	0.24
High school grade	2.23	2.24	2.22	2.26	2.22
Labor market orientation[a]	2.91	2.92	2.88	2.81	2.95
University of applied sciences	0.41	0.41	0.40	0.59	0.31
Mother has upper secondary school degree	0.37	0.38	0.36	0.32	0.40
— intermediary —	0.36	0.36	0.35	0.36	0.35
— lower —	0.26	0.25	0.28	0.31	0.24
— no —	0.01	0.01	0.01	0.01	0.01
Father has upper secondary school degree	0.50	0.51	0.50	0.44	0.54
— intermediary —	0.23	0.23	0.23	0.26	0.22
— lower —	0.25	0.25	0.27	0.29	0.23
— no —	0.01	0.01	0.01	0.01	0.01
Panel B. Outcome variable					
Log wages	7.71	7.54	8.05	7.72	7.71
Employed	0.81	0.78	0.86	0.82	0.80
Unemployed	0.03	0.03	0.02	0.03	0.03
Full-time employed	0.70	0.65	0.79	0.70	0.69
Share of observations in 2nd wave	0.34	0.00	1.00	0.34	0.35
Number of individuals	13,976	12,946	6,790	4,720	9,256
Number of observations	19,736	12,946	6,790	6,631	13,105

Note: DZHW graduate surveys 2001, 2005, and 2009. Column (1) presents variable means for the estimation sample according to Figure 1. Column (2) only includes observations from the first wave (1 year after graduation). Column (3) only includes observations from the second wave (5-6 years after graduation). Columns (4) and (5) divide the sample by treatment status. [a] The variable "labor market orientation" measures how important labor market aspects were with respect to study choice, measured on a five-point scale with 5 indicating "very important" and 1 "unimportant".

Table 2: The Effect of Student Internship Experience on Log Wages

	OLS		IV	
	Parsim.	Full	Parsim.	Full
	(1)	(2)	(3)	(4)
Internship	0.061***	0.061***	0.064**	0.065***
	(0.010)	(0.010)	(0.020)	(0.019)
Female	-0.165***	-0.170***	-0.165***	-0.170***
	(0.012)	(0.012)	(0.012)	(0.012)
University of applied sciences	-0.031	-0.040	-0.031	-0.040
	(0.037)	(0.036)	(0.037)	(0.036)
Dummy 2nd wave	0.514***	0.514***	0.514***	0.514***
	(0.013)	(0.013)	(0.013)	(0.013)
Apprenticeship		0.078***		0.078***
		(0.014)		(0.014)
High school grade		-0.030***		-0.030***
		(0.009)		(0.009)
Labor market orientation		0.037***		0.037***
		(0.005)		(0.005)
Cohort FE	Yes	Yes	Yes	Yes
Birth year FE	Yes	Yes	Yes	Yes
Area of study FE	Yes	Yes	Yes	Yes
Degree type FE	Yes	Yes	Yes	Yes
University FE	Yes	Yes	Yes	Yes
Parental schooling FE	No	Yes	No	Yes
Adj. R2	0.326	0.332	0.325	0.332
Number of observations	19,736	19,736	19,736	19,736

Note: The dependent variable is log(wage). Standard errors (in parentheses) clustered on the individual level. $^{+}$ p<0.10, * p<0.05, ** p<0.01, *** p<0.001.

Table 3: First-Stage Results

	Parsim. (1)	Full (2)
Mandatory internship	0.565***	0.559***
	(0.014)	(0.014)
Female	0.022**	0.027***
	(0.008)	(0.007)
University of applied sciences	-0.093***	-0.080***
	(0.022)	(0.021)
Dummy 2nd wave	-0.002	-0.001
	(0.003)	(0.003)
Apprenticeship		-0.085***
		(0.011)
High school grade		-0.009
		(0.006)
Labor market orientation		0.019***
		(0.003)
Cohort FE	Yes	Yes
Birth year FE	Yes	Yes
Area of study FE	Yes	Yes
Degree type FE	Yes	Yes
University FE	Yes	Yes
Parental schooling FE	No	Yes
F-statistic[a]	41.84	41.24
Partial correlation coefficient[a]	0.551	0.548
Adjusted R^2	0.460	0.467
Number of observations	19,736	19,736

Note: The dependent variable is equal to one if a graduate completed an internship during the course of studies, and zero otherwise. Standard errors (in parentheses) clustered on the individual level. [a] Relates to the instrument variable "Mandatory internship".[+] p<0.10, * p<0.05, ** p<0.01, *** p<0.001.

Table 4: Variation in Mandatory Internships over Time, by Department

		Threshold 50/50		Threshold 60/40		Threshold 70/30	
		Departments	Obs.	Departments	Obs.	Departments	Obs.
Row	(1)	(2)	(3)	(4)	(5)	(6)	(7)
1	Introducer	102	1,411	80	1,094	54	719
2	Abolisher	95	1,212	69	880	42	407
3	Introducer & abolisher	23	419	8	131	8	131
4	Stayer	99	2,810	82	2,384	67	2,108
5	Uncertain	1,175	13,884	1,255	15,247	1,323	16,371
	Number of observations	1,494	19,736	1,494	19,736	1,494	19,736
	Share of changers	69%	52%	66%	47%	61%	37%

Note: Departments are constructed as unique combinations of areas of study and universities. Departments in rows 1 and 2 are defined as introducing or abolishing mandatory internships, respectively, across the cohorts 2001, 2005, and 2009. Using a binary denotation for the existence of mandatory internships at these three points in time, introducers are, for example, $[0, 0, 1]$ (introduction between 2005 and 2009) or $[0, 1, 1]$ (introduction between 2001 and 2005). Abolishers are, for example, $[1, 0, 0]$ or $[1, 1, 0]$. Row 3 comprises all departments that both introduced and abolished mandatory internships over time, specifically $[1, 0, 1]$ and $[0, 1, 0]$. In row 4, departments maintained the status over the years ($[0, 0, 0]$ and $[1, 1, 1]$). Row 5 results from the unbalanced sample. It collects all observations with missing entries for one of the three points in time (e.g., $[-, 0, 1]$). The share of changers is calculated by taking the sum of rows 1 through 3 divided by the sum of rows 1 through 4.

Table 5: Estimates of Introducing Mandatory Internships on Quality Indicators

	Self-reported (1)	Threshold 50/50 (2)	Threshold 60/40 (3)	Threshold 70/30 (4)
Overall quality of education:				
Structure of the study program	-0.005	-0.014	0.004	-0.009
	(0.011)	(0.017)	(0.019)	(0.023)
State-of-the-art methods taught	0.001	-0.004	0.005	0.008
	(0.012)	(0.015)	(0.015)	(0.020)
Up-to-date education[a]	0.024*	0.014	0.030	0.044*
	(0.012)	(0.018)	(0.018)	(0.022)
Educational media and infrastructure:				
Availability of literature in the library	-0.009	-0.006	-0.01	0.005
	(0.011)	(0.016)	(0.018)	(0.022)
Access to IT services (internet, databases)	0.003	0.012	0.014	0.015
	(0.012)	(0.014)	(0.02)	(0.023)
Use of electronic communication devices	0.015	-0.010	-0.001	0.018
	(0.012)	(0.017)	(0.021)	(0.025)
Training:				
Oral presentation training	0.004	0.007	0.011	0.004
	(0.011)	(0.018)	(0.02)	(0.024)
Writing skills training	-0.001	-0.005	0.000	0.028
	(0.011)	(0.014)	(0.017)	(0.02)
Training in foreign languages[b]	0.005	0.003	0.011	0.007
	(0.011)	(0.017)	(0.021)	(0.025)
Career Counseling:				
Help in finding a job and starting a career	0.002	-0.013	-0.004	-0.001
	(0.008)	(0.011)	(0.012)	(0.015)
Availability of career counseling	0.009	-0.018	0.004	0.016
	(0.01)	(0.012)	(0.015)	(0.02)
Provision of career orientation events	-0.004	-0.009	-0.004	-0.010
	(0.007)	(0.011)	(0.012)	(0.015)
Number of observations	18,220	18,220	15,467	12,886

Note: Estimates from OLS regressions based on different definitions of the treatment. Standard errors (in parentheses) clustered on the individual level. All models control for gender, year of birth FE, area of study FE, university FE, degree type FE, university type, high school degree type mother and father FE, apprenticeship, high school grade, degree of labor market orientation and a dummy for the 2nd wave. [a] The variable measures the actuality of education with respect to current job requirements. [b] The variable measures subject- or job-specific training in foreign languages. Note that departments in which 40-60 percent (or 30-70 percent) of graduates say that an internship was mandatory are excluded from the regressions, resulting in smaller sample sizes in columns 3 and 4. $^{+}$ $p<0.10$, * $p<0.05$, ** $p<0.01$, *** $p<0.001$.

Table 6: Heterogeneous Effects

	OLS	IV	Number of observations
Panel A: Gender			
Women	0.075***	0.057+	10,523
	(0.017)	(0.032)	
Men	0.053***	0.079***	9,213
	(0.013)	(0.023)	
P-value of interaction (internship×women)	0.220	0.978	19,736
Panel B: Parental background			
Parents with 'high' levels of schooling	0.066***	0.067*	11,294
	(0.016)	(0.028)	
Parents with 'low' levels of schooling	0.045**	0.044	8,442
	(0.016)	(0.029)	
P-value of interaction (internship×highly educated parents)	0.340	0.329	19,736
Panel C: High school performance			
High school grade $\geq$ median	0.052***	0.043+	12,051
	(0.013)	(0.023)	
High school grade $<$ median	0.070***	0.118**	7,685
	(0.020)	(0.041)	
P-value of interaction (internship×high grade)	0.090	0.108	19,736
Panel D: Labor market orientation of student			
LM orientation $\geq$ median	0.049***	0.022	12,385
	(0.013)	(0.026)	
LM orientation $<$ median	0.065***	0.107***	7,351
	(0.019)	(0.032)	
P-value of interaction (internship×weak LM orientation)	0.040	0.016	19,736
Panel E: Labor market orientation of study subject [a]			
Strong LM orientation	0.047***	0.048*	14,743
	(0.011)	(0.019)	
Weak LM orientation	0.104***	0.142**	4,993
	(0.031)	(0.067)	
P-value of interaction (internship×weak LM orientation)	0.040	0.051	19,736
Panel F: Field of study subject			
Science, mathematics, engineering	0.048**	0.055*	10,125
	(0.015)	(0.022)	
Business and economics	0.056**	0.032	3,921
	(0.021)	(0.050)	
Humanities and social sciences	0.088***	0.112*	5,445
	(0.026)	(0.051)	
P-value of interaction (internship×BE)	0.228	0.449	19,491[b]
P-value of interaction (internship×HSS)	0.069	0.088	

Note: All models control for gender, year of birth FE, area of study FE, university FE, degree type FE, university type, high school degree type mother and father FE, apprenticeship, high school grade, degree of labor market orientation and a dummy for the 2nd wave. [a] See Table A.1 in the appendix for a classification of areas of studies into weak and strong labor market orientation. [b] 245 observations are outside of the study field categorization. + p<0.10, * p<0.05, ** p<0.01, *** p<0.001.

Table 7: Robustness Checks I : Alternative Instruments

	IV_{50}	IV_{60}	IV_{70}	IV_{Ratio_1}	IV_{Ratio_2}
	(1)	(2)	(3)	(4)	(5)
Internship	0.101^{+}	0.130^{*}	0.155^{*}	0.116^{*}	0.124^{***}
	(0.056)	(0.060)	(0.065)	(0.050)	(0.036)
First-stage estimate	0.268^{***}	0.339^{***}	0.388^{***}	0.473^{***}	0.505^{***}
	(0.016)	(0.018)	(0.023)	(0.023)	(0.018)
F-statistic^{a}	17.029	19.152	16.764	20.842	27.597
Number of observations	19,736	16,791	13,978	19,736	19,736

Note: All models control for gender, year of birth FE, area of study FE, university FE, degree type FE, university type, high school degree type mother and father FE, apprenticeship, high school grade, degree of labor market orientation and a dummy for the 2nd wave. ^{a} Relates to the regressor of the corresponding instrument in the first-stage estimation. Note that areas of study in which 40-60 percent (or 30-70 percent) of graduates say that an internship was mandatory are excluded from the regressions, resulting in smaller sample sizes in columns 2 and 3. $^{+}$ p<0.10, * p<0.05, ** p<0.01, *** p<0.001.

Table 8: Robustness Checks II: Specification and Sample Selection

	OLS	IV	Number of observations
Panel A: Department fixed effects			
Internship	0.055***	0.044*	19,736
	(0.011)	(0.022)	
Panel B: Area of study-university type fixed effects			
Internship	0.056***	0.055**	19,736
	(0.010)	(0.020)	
Panel C: Employed during studies			
Internship	0.054***	0.062**	19,700
	(0.010)	(0.019)	
Panel D: S.e clustered on department level			
Internship	0.061***	0.065***	19,736
	(0.011)	(0.019)	
Panel E: Short term wages			
Internship	0.058***	0.069**	12,946
	(0.014)	(0.023)	
Panel F: Longer term wages			
Internship	0.061***	0.055*	6,790
	(0.015)	(0.025)	

Note: All models control for gender, year of birth FE, area of study FE, university FE, degree type FE, university type, high school degree type mother and father FE, apprenticeship, high school grade, degree of labor market orientation and a dummy for the 2nd wave. Exceptions: The regression in panel A omits area of study FE and university FE due to the newly introduced interaction fixed effects between the two. Likewise, panel B omits area of study FE and the dummy indicating the university type due to the newly introduced interaction fixed effects between the two. Panel E uses wage information only from the initial survey conducted around one year after graduation. Panel F uses wage information only from the follow-up survey conducted around 5-6 years after graduation. Standard errors (in parentheses) clustered on the individual level in panels A, B, and C. They are clustered on the department level in panel D and on the level of universities in panels E and F. $^{+}$ $p<0.10$, * $p<0.05$, ** $p<0.01$, *** $p<0.001$.

Table 9: Robustness Check III: Sensitivity to omitted variable bias

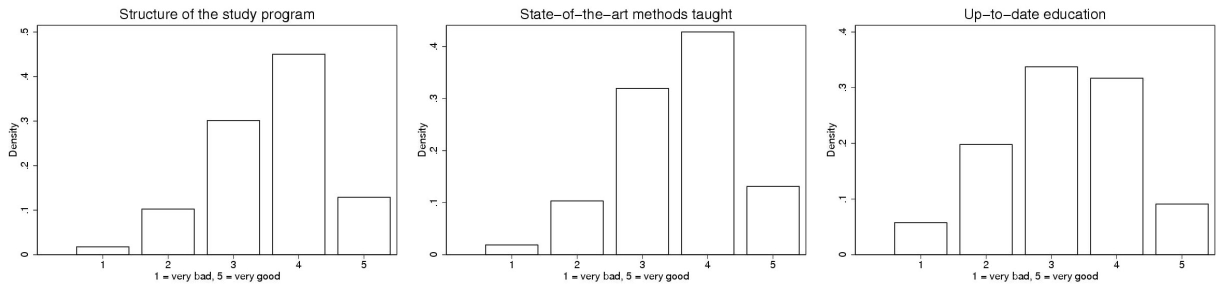
Description	Estimate (Stand. error)
Baseline effect $\dot{\beta}_1$	0.060
	(0.011)
R-squared $\dot{R}$	0.139
Controlled effect $\tilde{\beta}_1$	0.061
	(0.010)
R-squared $\tilde{R}$	0.345
Bias-adjusted coefficient $\beta_1^{*'}$ for $\tilde{\delta} = 1$	0.063
Bias-adjusted coefficient $\beta_1^{*'}$ for $\tilde{\delta} = 2$	0.065
Identified set $[\tilde{\beta}_1, \beta_1^{\star\prime}(min\{2.2\tilde{R}, 1\}, 1)]$ for $\tilde{\delta} = 1$	[0.061, 0.063]
Identified set $[\tilde{\beta}_1, \beta_1^{\star\prime}(min\{2.2\tilde{R}, 1\}, 1)]$ for $\tilde{\delta} = 2$	[0.061, 0.065]
Confidence Interval$_{95,\tilde{\beta}_1}$	[0.041, 0.081]
Zero excluded in identified set? ($\tilde{\delta} = 1$)	yes
Zero excluded in identified set? ($\tilde{\delta} = 2$)	yes
$\beta_1^{*'}$ within 95-confidence interval? ($\tilde{\delta} = 1$)	yes
$\beta_1^{*'}$ within 95-confidence interval? ($\tilde{\delta} = 2$)	yes

Note: Based on an econometric method developed by Oster (2014). $\dot{\beta}_1$ is the coefficient and $\dot{R}$ the R-squared from an OLS regression of equation (5). $\tilde{\beta}_1$ and $\tilde{R}$ stem from an OLS regression of equation (6). $\beta_1^{\star\prime}$ is defined by equation (4). Standard errors (in parentheses) clustered on the individual level.

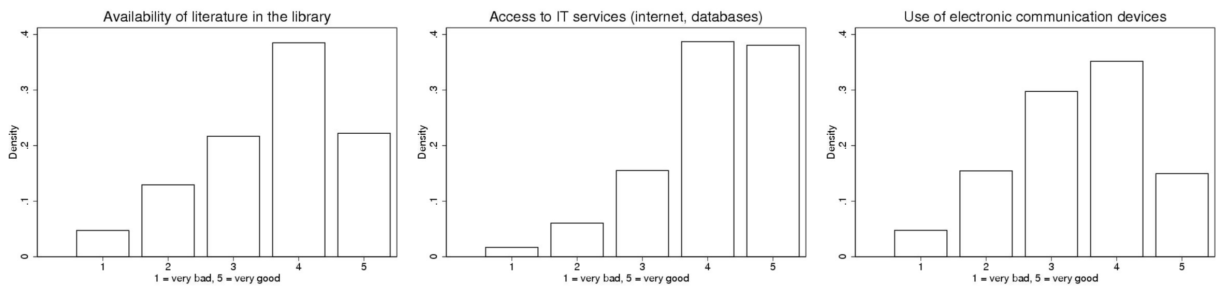
A. Appendix (For Online Publication)

Figure A.1: Students' Evaluation of Study Related Aspects

(A) Overall Quality of Education

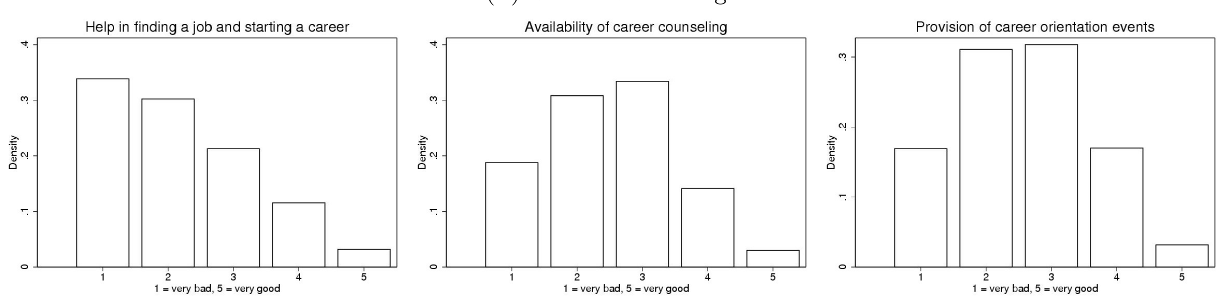


(B) Educational Media and Infrastructure



(C) Training

(D) Career Counseling



Note: The corresponding questionnaire item reads "How do you evaluate the following aspects of your completed studies?" Respondents are then asked to answer on a scale from 1 ("very bad") to 5 ("very good"). Data is taken from the first wave ($N = 12,964$).

Table A.1: Classification of Areas of Study into Strong and Weak Labor Market Orientation

Strong LM orientation	Weak LM orientation
administrative studies	ancient/classic philology, modern Greek
agricultural sciences	area studies
architecture and interior design	arts, general art history
biology	catholic theology/religious education
chemical science	composition and design
civil engineering	cultural studies/cultural sciences
computer science	English studies, American studies
dentistry/dental medicine	extra-European linguistic and cultural studies
economics	film studies
electrical engineering	fine arts
engineering management	comparative literary and linguistic sciences
food and beverage technology	general cultural studies
forestry, forest and wood management	general economic and social science
general engineering	general linguistics and philology
geomatic/geospatial engineering	geography
geosciences (without geography)	German philology and studies
healthcare science	history
human medicine	library science, documentation, communication
jurisprudence/law	music, musicology
landscape conservation, - architecture	education
mathematics, natural sciences	performing arts, theater studies
mechanical engineering, process engineering	philosophy
mining and metallurgy	political sciences
nautical science / navigation	protestant theology/religious education
pharmacy	psychology
physics, astronomy	Romance philology and studies
social pedagogy	Slavonic, Baltic, Finno-Ugrian studies
spatial planning	social sciences
teletraffic engineering	special education
trophology, nutritional and domestic science	sport science
veterinary medicine	

Note: Based on Scarletti (2009).

Table A.2: Overview of Survey Evidence on Students' Reasons for the Choice of University and Study Program in Germany

Author(s)	Survey and sample	Question	Type of question	Main results	Question on internship?
Heine et al. (2005)	Representative survey of first-year students in Germany in the winter term 2004/05; 8,200 students.	How important are the following reasons for your choice of study?	5-point Likert scale (1 very important; 5 not important) on 20 different potential reasons/aspects with respect to the university and city.	Percent who answer that it is (very) important (1 & 2 on the scale): • Interest in content (91%) • Affinity/Ability (88%) • Many occupational choices later on (67%) • To be able to work independently (64%)	No
Hachmeister and Hennings (2007)	Survey of students in the middle of their studies.	How important were the following aspects for the choice of your university?	6-point Likert scale (1 very important; 6 not important at all).	Percent who answer that it is (very) important (1 & 2 on the scale): • To study preferred field of study (66%) • Good reputation of the university and professors (59%) • Proximity to home (58%) • Interesting city (51%)	No
Hachmeister and Hennings (2007); Hachmeister et al. (2007)	Survey among high school students in final grade; around 3,600 pupils.	How important were the following aspects for the choice of your university?	4-point Likert scale (1 applies very much; 4 does not apply at all).	Percent who answer that it is (very) important (1 & 2 on the scale): • Interest in content (100%) • Good facilities (90%) • Atmosphere in the city (89%) • Services for students (82%)	No
Bartl and Korb (2009)	Online survey among first-year students at the Martin-Luther University Halle-Wittenberg in the winter term 2008/09; around 800 students.	How important were the following reasons in your choosing this university?	5-point Likert scale (1 very important; 5 not important).	Average value of the 5-point scale: • Interest in content (1.79) • No need to pay tuition fees (1.96) • Good life conditions (2.23) • Proximity to home (2.27) • University has a good reputation (2.48)	No
Heine et al. (2009)	Representative survey among first-year students in Germany in the winter term 2007/08; 8,342 students.	How important are the following reasons for your choice of study?	5-point Likert scale (1 very important; 5 not important) on different reasons/aspects with respect to the university and city.	Percent who answer that it is (very) important (1 & 2 on the scale): • Interest in study program content (83%) • Proximity to home (66%) • Good reputation of the university (60%) • Good facilities (54%)	No
Institut für Markt-forschung GmbH (2014)	Representative online survey in 2013 among young people aged 16-24 who aim at studying at university; 500 individuals.	What is the most important aspect in the choice of the university?	5-point Likert scale (1 very important; 5 not important) on long list of aspects.	Percent who answer that it is very important (1 on the scale): • I do not want to be away too far from home (29%) • I will choose the university with the best study program (28%) • I want to live in a city that also has good recreational oportunities (23%) • I will choose a university with a good reputation (13%)	No