Oxford Economic Papers, 2024, **76**, 495–513
https://doi.org/10.1093/oep/gpad015
Advance access publication 29 April 2023
Article

OXFORD

Work experience, information revelation, and study effort

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Abstract

Firms screen graduates using grade thresholds, which can turn into students' targeted learning outcomes or reference points in the model of study effort choice. Variability in the usage of grade thresholds implies students' uncertainty about the value of grades. Work experience from internships can reduce this uncertainty and, in turn, affect the choice of study effort. We theoretically show that a reduction in uncertainty induces more effort from less able students but, in contrast, less effort from more able students. Consistent with the theory, we empirically find that students returning from year-long internships have a flatter grade-ability profile.

JEL classifications: I20, D81

1. Introduction

Most students leave post-secondary education with some work experience gained from internships, co-op programmes, or work placements related to their field of study.[1] Such work experience can impact upon educational outcomes and choices through different channels like, most notably, human capital formation and ability signalling. Recently, Arcidiacono *et al.* (2016) demonstrated that work experience can also help students resolve the problem of imperfect information about their productivity and labour market expectations. Specifically, they show for university dropouts how the informational channel of work experience impacts upon their decision to return to education. In this article, we argue that this informational channel can also affect the intensive margin of educational choice through its impact upon study effort.

The approach of this article is based on two stylized facts that characterize university graduate labour markets. The first is that employers use crude measures of academic attainment to screen job candidates, who are typically required to meet a certain minimum grade threshold.[2] In the USA, the threshold is based on the cumulative grade point average (GPA), with median threshold in most industries set at 3.0 out of 4.0 (NACE, 2015a, 2018). In the

[1] See National Association of Colleges and Employers (NACE) (2015b, 2016) for evidence from the USA and European Commission (2012) for evidence from Europe.

[2] Although employers may have access to academic transcripts with detailed information, it can be very costly for them to extract and comparatively evaluate this information across students from different universities (Feng and Graetz, 2017). Furthermore, Zubrickas (2015) shows in the signalling model that coarse grading can be teachers' favourite strategy as it maximizes students' expected effort.

UK, universities use a degree classification system with grade averages mapped onto four main degree classes. Historically, a common minimum requirement for a job offer at a high pay sector has been the second of these classes, called 'Upper Second Class' or '2:1', and degrees in the top two classes have been viewed as 'good' degrees. Naylor *et al.* (2016) find that the 'good' degree premium increases from about 5% 1 year after university to about 8% 6 years after university; also see Feng and Graetz (2017) for more evidence.

The second stylized fact is the uncertainty around screening measures to be used by employers. In the USA, the proportion of employers using minimum GPA as a screening device varies both across time and across industries. In the period 2010–2019, between 67% and 78% of employers screened their applicants by means of a minimum GPA threshold, which in some industries could be as high as 3.5 (Koeppel, 2006; NACE, 2015a, 2018). In the UK during 2004–2012, the proportion of employers requiring a 'good' degree increased from 52% to 76% (Vasagar, 2012), but fell back to about two-thirds by 2017 (Coughlan, 2018; Borrett, 2019). Consequently, diversity and variation in screening practices among employers imply students' uncertainty about the signalling value of grades.

In this article, we argue that these two stylized facts about the job application screening process can have important implications for student study effort. First, students can view the minimum grade threshold as the targeted learning outcome or reference point with failure to achieve it resulting in significant welfare loss. This observation then suggests the relevance of reference dependence with loss aversion for modelling students' preferences (Tversky and Kahneman, 1991). Secondly, with uncertainty surrounding the minimum grade threshold the arrival of new information about the labour market can affect students' expectations and their choice of study effort, which we capture by modelling the reference point as a distributional, expectations-based variable (Köszegi and Rabin, 2006).[3]

In the theoretical part of the article, we present a model which is an extension of the commonly used model of student effort choice with reference dependence and loss aversion. We demonstrate that reduced uncertainty about the reference point (in the sense of second-order stochastic dominance) increases the low-ability student's effort but reduces the high-ability student's effort. Due to reduced uncertainty, the high-ability student no longer needs to put more effort in an attempt to minimize the risk of falling behind the reference point. In contrast, this risk increases for the lower ability student or rather the probability of surpassing the reference point decreases, thus, prompting the student to put more effort. In formulating the empirical hypothesis, we assume that work experience with potential employers makes the student more informed about the reference point, thus, yielding asymmetric predictions about the educational effects of internships.

The empirical setting is the final year of undergraduate economics studies in the University of Bath. This university has a long-standing work placement programme that offers students the option to take up year-long internships between the penultimate and final years of study. An important feature of this programme is that many students return to their final year with job offers that are explicitly conditional on minimum academic attainment. We contrast the academic performance of placement students in their final year against that of non-placement students. Measuring ability with the grade average of the penultimate year, we find that work experience has a positive effect on the performance of lower ability students but a negative effect on higher ability students as predicted by the model. We estimate that the pattern of internship effects is the same whether internship participation is modelled as endogenous or exogenous. Importantly, our findings of asymmetric effects of internships are consistent with related findings from studies on the performance effects of information revelation through feedback in higher education (Azmat *et al.*, 2019; Dobrescu *et al.*, 2021; also see Kuhnen and Tymula, 2012).

[3] See Zafar (2011) for evidence about students' consistently updating their grade expectations upon the arrival of new information and accordingly adjusting their study effort.

Lastly, understanding the educational implications of internships goes beyond academic interest as the further integration of work and education is the object of active government policy. In 18 of the 48 countries in the European Higher Education Area, governments 'reported providing incentives to some or all higher education institutions to increase the number of available internships' (European Commission, 2015, p. 202). In the UK, in particular, the landmark 1997 Dearing Report on higher education recommended 'that the Government, with immediate effect, works with representative employer and professional organizations to encourage employers to offer more work experience opportunities for students' (Dearing, 1997, p. 137). The expected educational benefits of work internships lie, at least in part, behind this recommendation as '[t]he educational value of such work is enhanced if the student is encouraged and helped by the institution to reflect on the work experience, to make linkages with theory learned in other settings and, thereby, to learn from it' (Dearing, 1997, p. 117).

The article proceeds as follows. The next section discusses the related literature. Section 3 develops the theoretical model and its empirical implications. Section 4 discusses the institutional setting of the data of the empirical analysis. Section 5 presents the empirical evidence, Section 6 discusses implications and further results, and Section 7 concludes.

2. Related literature

Recent advances in the internship literature have provided the first evidence that internships have positive causal effects on various labour market outcomes. Résumé audit studies by Nunley *et al.* (2016) in the USA and Baert *et al.* (2021) in Belgium found positive internship effects on the probability of call-backs. Margaryan *et al.* (2022) found that internships in German universities have positive effect on earnings in the first 5 years after graduation. On the mechanisms generating these effects, Nunley *et al.* (2016) suggest signalling as the likely mechanism. In their study, the gap between 3-month summer internships and employment was at least 4.5 years, which is a long period of time for skills accumulated during such short internships to remain salient. In Baert *et al.* (2021), however, the gap between internships and employment is much shorter, which cannot rule out a human capital mechanism. Nunley *et al.* (2016) also highlight the importance of grades as they find that the internship effect on call-backs is 2.7 times larger when a high-grade average is included in a résumé.

In contrast to the aforementioned studies, this article is concerned with educational effects of internships. Our approach is consistent with the literature that views educational choices as the outcome of updating beliefs in the face of experience in higher education and in the labour market. For example, in Altonji *et al.* (2012), the choice of major is the outcome of updating beliefs about one's own ability after the first 2 years of university studies. In Arcidiacono *et al.* (2016), the choice to continue with employment or to return to college depends on beliefs about one's own productivity as they are updated on the basis of work experience. In our article, work experience reduces uncertainty about the signalling value of grades in the graduate labour market with students accordingly adjusting their choice of study effort. Conceptually, our article is closest to Arcidiacono *et al.* (2016) with the difference that they study the extensive margin of the educational effect of work experience whereas we study the intensive margin.

Although the focus of this article is on an informational mechanism, our model can also incorporate the human capital mechanism that underpins much of the research on the educational effects of internships and work experience. Developmental psychologists and educationists point to the benefits of learning by doing (Wilson, 1987; Linn, 2004). In terms of skills, interns are expected to acquire both subject-specific cognitive skills and non-cognitive skills. The subject-specific skills may impact upon academic outcomes directly, while non-cognitive skills may have indirect benefits by improving the level and quality of effort [see Almlund *et al.* (2011) for a review of the evidence on educational benefits of non-cognitive skills]. These views are not without empirical support. For internships lasting longer than one semester, participation tends to be weakly but positively correlated with subsequent academic performance. Studies of engineering programmes in the USA find that participation in a co-op programme is associated with a GPA increase of between 0.06 and 0.15 (Blair *et al.*, 2004; Schuurman *et al.*, 2008). In the UK, Gomez *et al.* (2004), Surridge (2009), and Mansfield (2011) find that year-long work placements are associated with 3–4 percentage points higher final year grade averages, which may yet be upwards biased because of unaccounted endogeneity concerns. Our finding of the asymmetric educational effects of internships across the student ability spectrum hints that the previously reported average differences in grades may hide richer learning dynamics related to work experience.[4]

Our work also relates to the strand of literature on the role of relative rank feedback for performance in higher education. Dobrescu *et al.* (2021) demonstrate that while relative performance feedback is, on average, performance enhancing, its effect may be negative for higher ability students, which the authors heuristically attribute to a ceiling effect (Bandiera *et al.*, 2015). Asymmetric effects of feedback across the ability spectrum are also observed by Azmat *et al.* (2019) and experimentally demonstrated by Kuhnen and Tymula (2012). Our contribution to this literature is two-fold. First, we demonstrate that information revelation through internships can have a similar effect on performance as information revelation through relative performance feedback, which possibly suggests a common principle at work. Secondly, we propose reference dependence and loss aversion as a new explanation of the asymmetric performance effects of information revelation.

Lastly, our article is an application of the utility theory of reference dependence and loss aversion (Tversky and Kahneman, 1991) in the domain of education. In general, this theory is motivated by empirical evidence about the effects of contextual circumstances on the individual perception of utility. These effects are typically found to take the form of loss aversion with respect to some reference point, determined by the decision maker's current position and expectations, as well as by social norms and comparisons. An important modification of the theory of reference dependence is provided by Köszegi and Rabin (2006) who suggest that rational expectations can serve as the reference point for reference-dependent choices. Since its inception, the theory of reference dependence has found application in many practical settings such as, e.g. investment portfolio choice (Benartzi and Thaler, 1995; Berkelaar *et al.*, 2004), supply of labour and effort provision (Abeler *et al.*, 2011; Crawford and Meng, 2011), professional golf (Pope and Schweitzer, 2011), migration and remittances (Seshan and Zubrickas, 2017; Antoniades *et al.*, 2018), and employment (Martin, 2017; Dickson and Fongoni, 2019). Similar to other realms of economic behaviour, the perceived utility of grades is likely to depend on contextual circumstances such as their expected signalling value in the job market, thus, calling for the application of the utility theory of reference dependence, which is attempted in this article.

3. Model and hypotheses

A student needs to make a decision about an educational outcome, measured here by an exam score. Achieving exam score $z \in [0, 1]$ comes at an effort cost of $C(z, n)$, where $n > 0$ is a scalar measure of skills relevant to the exam, which we refer to as 'ability'. The cost function $C(z, n)$ is increasing and convex in z, $C_z > 0$, $C_{zz} > 0$,[5] and the marginal cost of exam score is decreasing in ability, $C_{zn} < 0$. We assume that $C(0, n) = 0$, $C_z(0, n) = 0$, and $\lim_{z\to 1} C(z, n) = \infty$.

[4] There is a strand of literature on the educational effects of work experience that is contemporaneous to studies. Due to the time resource constraint, work experience is found to have a negative effect on academic performance but the effect is not homogeneous across different student groups (Stinebrickner and Stinebrickner, 2003; Darolia, 2014).

[5] Arcidiacono (2004) finds evidence supporting the convexity of the cost function.

The utility of exam score z depends on its value relative to the grade threshold to be used for screening in the labour market, which we refer to as reference point $\overline{z}$. We let reference point $\overline{z}$ be uncertain at the moment of the student's choice of effort but we assume that the student believes it is distributed according to function $F(\overline{z})$ over [0, 1]. For a given reference point $\overline{z}$, the utility of exam score z is given by increasing function $U(z-\overline{z})$ and we assume loss aversion which we model by that U satisfies the properties of a 'universal gain–loss function' in Köszegi and Rabin (2006). Specifically, the student experiences a negative utility if his exam score is below the reference point, $U=\lambda(z-\overline{z})$ for $z<\overline{z}$, and a positive utility otherwise, $U=z-\overline{z}$ for $z\geq\overline{z}$. Because of loss aversion we require $\lambda>1$ or, in words, utility loss from falling short of the reference point resonates more than utility gain from exceeding it.

We express the student's total utility as

$$W(z,n)=\int_0^1 U(z-\overline{z})\mathrm{d}F(\overline{z})-C(z,n), \tag{1}$$

which, given the functional form of U, we can rewrite as

$$W(z,n)=\int_0^z (z-\overline{z})\mathrm{d}F(\overline{z})+\int_z^1 \lambda(z-\overline{z})\mathrm{d}F(\overline{z})-C(z,n). \tag{2}$$

Let $z^*=\mathrm{argmax}_z W(z,n)$ denote the student's optimal score level, which is found from the first-order condition obtained using the Leibniz integral rule

$$F(z^*)+\lambda(1-F(z^*))-C_z(z^*,n)=0. \tag{3}$$

Modelling assumptions imply that the solution z^* is unique and interior, which also implies that the second-order condition ($W_{zz}<0$) is satisfied.

By the Implicit Function Theorem applied to condition (3), we find that the internal derivative $\mathrm{d}z^*/\mathrm{d}n$ satisfies

$$\frac{\mathrm{d}z^*}{\mathrm{d}n}=\frac{C_{zn}}{W_{zz}}>0. \tag{4}$$

Hence, the abler the student, the higher the score he aims for. From a different perspective, an improvement to ability n leads to a better educational outcome.

Next, we are interested in the effect of uncertainty about the reference point on educational outcomes. We can show that.

Proposition 1 Let F and G be distributions for the reference point $\overline{z}$, and let z^* and $z^{\#}$ be the respective optimal scores. If $F(z^*)\gtreqless G(z^*)$, then $z^*\lesseqgtr z^{\#}$.

Proof. Rewrite first-order condition (3) as $\lambda=(\lambda-1)F(z^*)+C_z(z^*,n)$. If $F(z^*)>G(z^*)$, then for this expression to hold under distribution G, the score needs to be increased because of $C_{zz}>0$, thus, $z^*<z^{\#}$; and *vice versa* for $F(z^*)\leq G(z^*)$. ■

Now suppose that in Proposition 1 the distribution F is a mean-preserving spread of G. Also, let the graphs of F and G cross only once in the interior as illustrated in the left diagram of Figure 1. Then, there is threshold z' such that $F(\overline{z})\gtreqless G(\overline{z})$ for $\overline{z}\lesseqgtr z'$. By Proposition 1, we have that under distribution G the optimal score schedule $z^{\#}$ is flatter in ability than

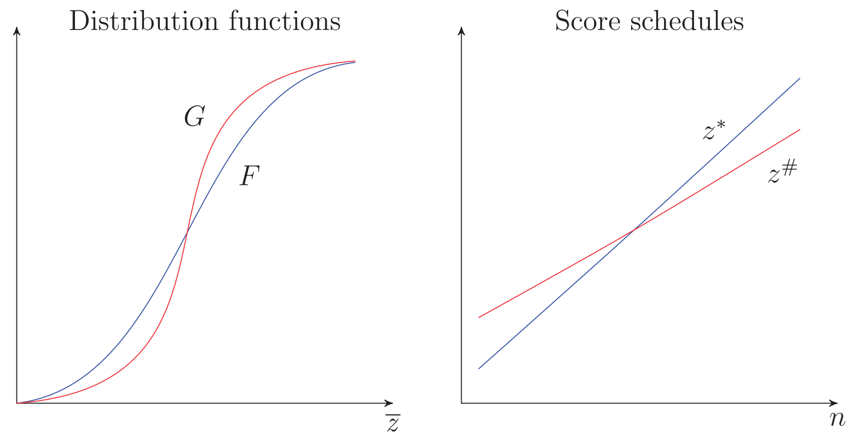

Figure 1. Uncertainty about reference point and optimal score schedules.

Notes: The left diagram shows the graphs of distributions F (blue) and G (red), where F is the mean-preserving spread of G. The right-hand diagram illustrates the optimal score schedules z^* (blue) and $z^\#$ (red) for different levels of ability under distributions F and G, respectively.

the optimal score schedule z^* under distribution F as shown in the right diagram of Figure 1.

For intuition behind the results illustrated by Figure 1, consider an able student. When uncertain about the reference point, the student will increase his studying effort as a precautionary measure against a high reference point. But with more certainty, the student's need for precautionary measures weakens and so will his learning effort. Now consider a less able student with a score below the expected reference point. With much uncertainty about the reference point, there is a chance that the student's score is above the reference point. However, with less uncertainty this chance diminishes and, therefore, the student responds with more studying effort.

When work experience reveals new information about the signal value of grades, a student can form more precise expectations about the grade threshold used in the job market. In the next section, we discuss such information revelation mechanisms in the context of our empirical application. If such mechanisms are present, then Proposition 1 implies

Hypothesis 1 (Information channel): The effect of work experience on academic performance is positive for less able students and negative for more able students.

Students' ability or skill set may not stay constant during an internship. If work experience develops skills complementary to the skills accumulated in school (in the sense of Cunha and Heckman, 2007), the effect on academic performance will be positive across the ability distribution. This potential mechanism is captured by Equation (4) which shows that higher ability results in higher target exam score. Put differently, with human capital formation the educational effect of work experience is symmetric across the student ability spectrum. The asymmetric effects predicted by the informational mechanism therefore cannot be predicted by a human capital mechanism. At the same time, finding evidence in favour of Hypothesis 1 would not be evidence against the human capital channel but rather of loss aversion and reference dependence.[6]

[6] At an extreme, if the positive effect of work experience is very large, it can cause a shift up of the ability–score schedule in Figure 1, so that there are no negative effects in the upper part of the distribution. However, all the evidence suggests that this possibility is not empirically relevant—see the estimates discussed in Section 5.

An alternative, though closely related, approach to modelling the role of information revelation for educational outcomes is offered by Azmat and Iriberri (2010), which is further extended by Dobrescu *et al.* (2021). This alternative model features peer effects, presented in the form of competitive preferences, which can also be interpreted as reference dependence. However, their model cannot generate the asymmetric effect of information revelation that is behind our Hypothesis 1, for which loss aversion would be needed. Nevertheless, as already discussed earlier, Dobrescu *et al.* (2021) report the performance effects of information revelation that are in line with Hypothesis 1, and which they attribute to a ceiling effect (Bandiera *et al.*, 2015).

4. Institutional context and data

The typical duration of full-time undergraduate study in England is 3 or 4 years depending on whether a student opts to take a year off their studies to pursue an internship. These are intra-curricular internships, also known as work placements, taken after the second year of study and last at least 40 weeks.[7] Upon completion of the placement year, students return to university for the third and final year of their studies. Placement take-up varies by subject and over time, with subjects such as business studies and computer science having participation rates around 30%, while in humanities there are almost no undergraduate students on placements [Higher Education Statistics Agency (HESA), 1995–2016].

Our sample consists of economics students who entered the Economics Department at the University of Bath between 1998 and 2004. This was a time of relative stability in UK higher education, compared with the years that followed. Cohorts in this sample paid the relatively low tuition fees of £1,000 per year and did not face the steep escalation of tuition fees that started in 2006 and increased nine-fold by 2012. Nationally, during that period the percentage of economics students on year-long internships almost doubled from 4% to 8%, despite the increasing number of students. Because the supply side of the placement market is highly concentrated, most placement students originated from just a handful of universities. For economics, in the period 1996–2008, on average 81% of all placement students annually originated from just four universities and 93% from only six universities (HESA, 1997–2008). The University of Bath is one of this handful of universities. Economics students from this university had an average placement take-up rate around 77% and accounted for 24% of all economics placements in the UK annually.

Summary statistics of our sample of students are presented in Table 1. Placement students had significantly higher grade averages than non-placement students in all 3 years of study. Though small, these differences translate to a 21.5 percentage point differential in the proportion obtaining a first or upper second-class degree, which is consistent with reference-dependent behaviour. With respect to other characteristics, females and younger students were significantly more likely to go on placement, while students enrolled in the Economics and Politics degree were significantly less likely. Compared with the UK national population of economics students (as reported in Naylor and Smith, 2004), in our sample students have similar gender composition and proportion graduating from private schools but have substantially higher entry grades and tend to be younger.

The model in Section 3 predicts that reduced uncertainty about the reference point generates asymmetric study effort responses from students. Hypothesis 1 also draws on the assumption that work experience from internships can serve as an informational channel for reducing uncertainty. In the context of our sample, one informational channel works through job offers from the internship employers. The administration in the university of our sample started collecting data on returning interns with jobs offers in 2020, for the cohort completing their internships in 2019–2020. Table 2 presents the available data for the

[7] In the article, we use the terms *internship* and *placement* synonymously.

Table 1. Summary statistics by internship participation

Student characteristics	Non-interns	Interns	Difference
Entry age	18.89	18.69	0.20***
	(0.74)	(0.52)	
Female	0.23	0.33	−0.10**
	(0.42)	(0.47)	
Ethnicity (non-White British = 1)	0.13	0.15	−0.02
	(0.33)	(0.36)	
Disability	0.03	0.04	−0.01
	(0.18)	(0.20)	
Private secondary school	0.25	0.27	−0.02
	(0.43)	(0.44)	
Entry score	337.14	344.42	−7.28***
	(18.74)	(17.26)	
Internship intent (upon entry)	0.21	0.53	−0.32***
	(0.41)	(0.50)	
Year 1 grade (average)	59.39	62.33	−2.94***
	(6.80)	(6.16)	
Year 2 grade (average)	57.56	60.28	−2.72***
	(6.68)	(6.21)	
Final year grade (average)	60.16	63.70	−3.54***
	(5.82)	(4.41)	
First or upper second class degree	0.61	0.83	−0.21***
	(0.49)	(0.380)	
Degree programme:			
BSc Economics	0.54	0.60	−0.06
	(0.50)	(0.49)	
BSc Economics and Politics	0.30	0.18	0.12***
	(0.46)	(0.38)	
BSc Econ. and Intl. Development	0.17	0.22	−0.05
	(0.37)	(0.42)	
Sample size, obs./percentage	149/24.4%	462/75.6%	

Notes: Means are reported with standard deviations in parentheses. *Entry score* is given by UCAS (Universities and Colleges Admissions Service) points, only for entry years 2002–2004, 260 observations. *** indicates difference is significantly different from zero at the 0.01 level and ** at 0.05.
Source: University of Bath and authors' calculations.

Table 2. Internship outcomes for economics students

Year	Did the internship end with a job offer? (%)			Number of responses
	Yes	Maybe	No	
2019/20	23	–	77	137
2020/21	26	37	37	130
2021/22	22	40	38	173

Notes: The table reports economics students' responses from the annual student survey conducted by the placement office of the University of Bath to the question whether the internship ended with a job offer. There was no 'Maybe' option in the 2019–2020 survey. Data are available starting from academic year 2019–2020.
Source: University of Bath.

years 2019–2020 to 2021–2022. Despite COVID uncertainty, at the end of their internship, 22–26% of interns from the same department as our sample were already holding offers from their internship employer and 37–40% were waiting a decision. For the most recent cohort (2021–2022) we conducted a follow-up survey and found that 74% of interns holding offers had offers conditional on specific academic results (other than successfully graduating). To the extent that these offers are accepted and the job search ends, the distribution of grade thresholds collapses to a single point and uncertainty disappears. A sizeable fraction of interns therefore begin the final year of their studies with no uncertainty around grade thresholds. For those who have not received offers, year-long internships with potential employers offer ample time for information about screening practices to be revealed and therefore for uncertainty to be reduced.

5. Empirical evidence

An empirical implication of the asymmetric effects prediction is that after the internship stage, there is compression in the grade distribution of interns compared with that of non-interns. This is because the model predicts that among interns, average grades will fall for the more able and increase for the less able. In contrast, the differential in average grades is not predicted to change among non-interns. This prediction can be tested with a difference-in-difference (DiD) approach. We split our sample first into high-ability and low-ability according to whether Year 2 average is above or below the median and then into interns and non-interns. We compute mean grades $Y^a_{s,t}$ for $a = H, L$ (high or low ability), $s = I, NI$ (intern or non-intern), and $t = 1, 2, 3$ for year of study. Using the mean grades for each sub-sample we construct the DiD for each year of study, $\text{DiD}_t = (Y^H_{I,t} - Y^L_{I,t}) - (Y^H_{NI,t} - Y^L_{NI,t})$. The results are presented in Figure 2. In Years 1 and 2, we find small and statistically insignificant differences: $\text{DiD}_1 = 0.08$ (s.e. 1.16) and $\text{DiD}_2 = -0.49$ (s.e. 0.77). After the internship stage however we find that $\text{DiD}_3 = -2.34$ (s.e. 0.85), a sizable and statistically significant difference. Consistent with the model's predictions, the difference between the high- and low-ability sub-samples is reduced among interns compared with non-interns. We turn next to regression methods.

We can test Hypothesis 1 using the following specification:

$$\text{Finalyearavg}_i = \beta_0 + \delta\text{Internship}_i + \beta_1\text{Ability}_i + \gamma\text{Internship} \times \text{Ability}_i + \theta'\text{X}_i + \epsilon_i, \quad (5)$$

where i is the student index, Finalyearavg is the final year grade average, Internship is a dummy variable taking the value of 1 if the student took up an internship and 0 otherwise; Ability is measured by Year 2 grade average; X is a vector of student characteristics that

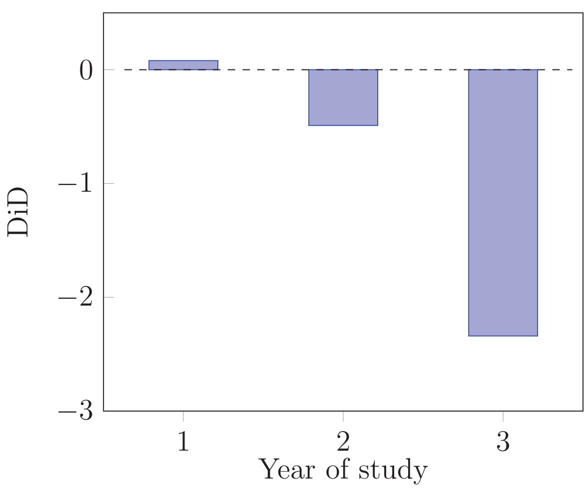

Figure 2. DiD in performance across the ability spectrum for interns and non-interns.
Note: DiDs are estimated controlling only for entry year fixed effects.

include gender, entry age, ethnicity, disability, degree programme, private secondary school attendance, parent occupation, and entry year fixed effects; and ϵ represents unobserved factors affecting academic performance. If Hypothesis 1 is correct, the coefficient on the interaction term between Internship and Ability has to be negative, $\gamma < 0$. In other words, the schedule of grades against ability has to be flatter for students returning from internships compared with students not going on internships.

Our main findings are reported in Table 3. Its first column shows the ordinary least squares (OLS) estimates of a regression using the whole sample. The negative interaction term implies that in the ability spectrum the grade schedule is flatter for interns than for non-interns, as predicted by our model. These estimates imply that interns with a relatively low-grade average of 50 in the second year do better than non-interns by 3.74 points, while on the other end of the distribution, internship effects become negative for those interns with second year average of 69.7 or higher.

Before we consider the potential endogeneity of internship participation, it is worth noting that the pattern of differential slopes between interns and non-interns we observe in the final year is specific to this year, that is, the year after the internship stage. In Column (4) of Table 3, we present estimates from a regression that uses Year 2 average as regressand (instead of final year) and measures Ability with Year 1 average (instead of Year 2). For before the internship stage, as the estimates in Column (4) show, we find no statistical difference in the slopes of interns and non-interns [and the slope of Ability is virtually identical to the regression in Column (1)]. The asymmetry in the slopes between interns and non-interns is therefore specific to internship participation. This is further supported in the next subsection, where using instrumental variable methods, we find no evidence of bias for our OLS estimate of the interaction term in the final year regression.

5.1 Endogeneity of internships

We instrument internship by an early expression of intent to pursue a year-long internship, recorded in students' university applications. This is similar to Margaryan *et al.* (2022), which uses the choice of programmes offering internships as instrument. The instrument used here has two advantages to that used by Margaryan *et al.* (2022). First, in our context, academic entry requirements are invariant to the expression of internship intent (and as a consequence students can and do change their intentions before the internship stage). Secondly, with our data we can also test instrument validity using a placebo test.

Table 3. Internship effects on educational outcomes

	(1)	(2)	(3)	(4)
Internship	1.91***	−0.76	3.06***	0.82
	(0.37)	(1.56)	(0.76)	(0.47)
Ability	0.65***	0.67***	0.87***	0.66***
	(0.06)	(0.11)	(0.12)	(0.06)
Internship × Ability	−0.19***	−0.19	−0.42***	0.08
	(0.05)	(0.14)	(0.12)	(0.07)
R^2	0.53		0.50	0.57
Obs.	603	603	487	603

Notes: The dependent variable is final year grade average for (1)–(3) and Year 2 grade average for (4). Ability is measured as the preceding academic year grade average, i.e. Year 2 for (1)–(3) and Year 1 for (4). Columns (1) and (4) report OLS coefficients, Column (2) 2SLS coefficients, and Column (3) OLS coefficients only for a subsample of interns and those non-interns who expressed internship intention upon university application. Control variables are listed in the text. Ability in the interaction term is measured as deviations from its mean. For Column (2), the minimum eigenvalue statistic is 16.3236 and the critical value for size of nominal 5% Wald test is 7.03 (at the 10% level of significance). First-stage and reduced-form regressions for Column (2) results are presented in Supplementary Appendix Table A1. *** indicates significance at the 0.01 level.
Source: Authors' calculations.

The intention to pursue an internship even before entering university suggests a strong labour market orientation, so our identification strategy rests upon the relationship of such orientation to academic skills. Surprisingly little is known about this relationship. We may expect that labour market orientation can motivate greater study effort, but whether it is related to academic skills or not is an empirical question. The closest to empirical evidence to this question is the Nelson and Sandberg (2017) study of students in a Swedish university. They find that labour market orientation becomes stronger as students progress in their studies, but it is either very weakly or not at all correlated to different approaches to study (e.g. surface versus deep approach). Our tests provide additional evidence that labour market orientation is unrelated to academic skills.

We start by examining the relationship of our instrument to the observables. The first two columns of Table 4 present results from regressions of the instrument, denoted as early internship intent (EII), on student characteristics. The regression in the first column is on the full sample and the regression in the second column is on the subsample for which we have university entrance exam grades. The results are very similar for both, showing that very little is correlated with the EII indicator. Enrolment in the BSc Economics and International Development programme (one of the three programmes in the Department) is the only characteristic strongly (positively) correlated with EII, possibly because of its more focused professional orientation. Crucially, EII is not correlated with prior academic performance (as measured by university entrance exams) and there is no evidence of a systematic relationship with parental social class or private secondary school attendance. In particular, at 0.0001 (s.e. 0.0021), the coefficient of university entrance exams is not only statistically insignificant but implies that an increase of an additional full grade (e.g. from B to A) is associated with a trivial 0.2 of 1 percentage point higher probability of an early preference towards internships.

Next, we repeat a similar exercise as in the beginning of this section, only this time we calculate the DiD across students with the instrument switched on (EII $= 1$) and off (EII $= 0$). If the instrument is related to the causes of the grade compression observed among interns, then we should find a shift in the DiD for the final year of study. We compute mean grades $Y^a_{r,t}$ for $a = H, L$ (high or low ability), $r = E, NE$ (EII $= 1$ or EII $= 0$), and $t = 1, 2, 3$ for year of study and then we construct the DiD for each year of study, $\text{DiD}_t = (Y^H_{E,t} - Y^L_{E,t}) - (Y^H_{NE,t} - Y^L_{NE,t})$. The results are presented in Figure 3. Our estimates for the DiDs are -1.37 (s.e. 0.81), -1.58 (s.e. 0.60), and -1.35 (s.e. 0.70) for Years 1, 2, and 3, respectively, showing minimal variation across the three years of study. This does not support any relation of the instrument to the shift in the grade distributions of the high- and low-ability students that we observe in the final year.

Crucially for the validity of the instrument, our data allow us to provide a strong test of the claim that it is not correlated with unobservables. Following Angrist and Pischke (2009, p. 130), because we observe the Year 2 grade average which is realized before internships and therefore is uncontaminated by the potentially endogenous internship participation indicator, we can test the validity of the instrument in a placebo regression on the instrument and its interactions. If the EII were correlated with the unobservables affecting academic performance, then its coefficient would be significant in this placebo regression. The results are shown in Column (3) of Table 4. The coefficient of the instrument is estimated at -0.08 (s.e. 0.35) and the coefficient of its interaction with Year 1 average is 0.02 (s.e. 0.06). The estimated effects of the instrument are individually and jointly statistically insignificant (and economically trivial), failing to reject the validity of the instrument.

We turn then to the estimated internship effects that account for potential endogeneity, presented in Column (2) of Table 3. Note that if internship is endogenous, the internship interaction term is also endogenous and therefore we instrument both variables. For the interaction term, we follow the recommendation of Wooldridge (2010, pp. 942–943) and use the product of Year 2 average with the predicted probability of internship, as estimated by

Table 4. Instrument validity tests

	EII	EII	Year 2 avg.
EII			−0.08
			(0.35)
EII × Year 1 average			0.02
			(0.06)
Year 1 average			0.72***
			(0.04)
Female	0.08*	0.11	0.38
	(0.05)	(0.08)	(0.39)
Ethnicity (non-White British = 1)	0.03	0.07	−1.32**
	(0.06)	(0.09)	(0.55)
Disability	0.13	0.25	1.12
	(0.10)	(0.15)	(0.96)
Entry age	−0.04	0.0001	−0.09
	(0.03)	(0.0543)	(0.30)
Entry score		0.0001	
		(0.0021)	
Private secondary school	−0.05	−0.02	−0.86**
	(0.04)	(0.07)	(0.41)
Degree programme:			
BSc Economics and Politics	0.07	−0.06	1.25***
	(0.05)	(0.09)	(0.40)
BSc Econ. and Intl. Development	0.21***	0.18**	0.97**
	(0.05)	(0.08)	(0.46)
Parent occupation:			
Professional	−0.11	−0.10	−0.61
	(0.10)	(0.14)	(0.77)
Managerial and technical	−0.09	−0.14	−0.14
	(0.10)	(0.14)	(0.72)
Skilled manual	0.001	−0.04	−0.06
	(0.122)	(0.19)	(0.90)
Skilled non-manual	−0.08	−0.10	−0.46
	(0.11)	(0.15)	(0.87)
Partly skilled	−0.22*	−0.27	−0.62
	(0.13)	(0.20)	(1.17)
Unskilled and routine	0.007	−0.03	−0.50
	(0.219)	(0.27)	(1.53)
R^2	0.069	0.079	0.569
Obs.	603	259	603

Notes: The dependent variable is indicated at the top of each column. All regressions include entry year fixed effects. Year 1 average is in deviation from its mean when interacted with EII. BSc Economics is the reference category for degree programme and no response is the reference for parent occupation. *** indicates difference is significantly different from zero at the 0.01 level and ** at 0.05.
Source: Authors' calculations.

probit (first stage and reduced form regressions are presented in Supplementary Appendix Table A1). The internship coefficient is now estimated at −0.76 (s.e. 1.56), showing that the OLS estimate is biased upwards and therefore implying a positive correlation between the unobserved factors affecting internship take-up and academic performance. Our focus however is on the slopes of the final year grade schedule of the interns and non-interns, which remain virtually unchanged. The 2SLS estimate of the interaction term is identical to the OLS estimate to two decimal places, though not statistically significant since the standard error is now three times larger. We therefore find no evidence that the OLS estimate of the slope is biased, at least for the compliers, which we

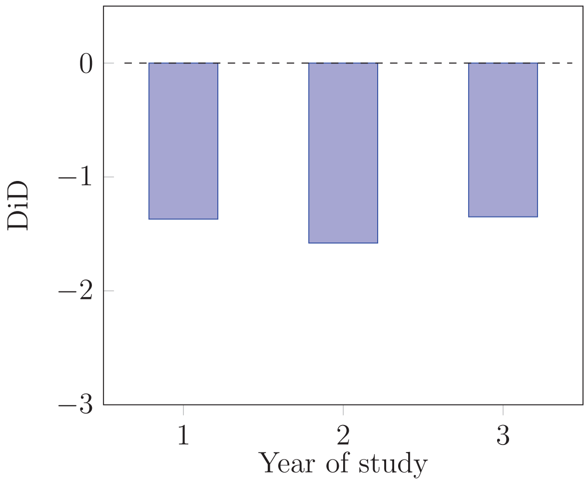

Figure 3. DiD in performance across the ability spectrum for student with and without EII.
Note: DiDs are estimated controlling only for entry year fixed effects.

estimate at 20.4% of the sample. Therefore, the 2SLS results confirm that the slope of the intern schedule is flatter.[8]

A possible objection to the above as a test of Hypothesis 1 is that since we do not observe the information set of students before and after internships, it is possible that the reduction in uncertainty is due to information revealed before interns embark on their internships. Although we do not observe such information, our data allow us to rule out that the flatter intern schedule is due to information revealed before internships. By limiting our sample to all interns plus those non-interns who had early expressions of intent to take-up internships, we in effect define a sub-sample that includes all students who entered the internship labour market, regardless of whether they eventually succeeded in securing an internship or not. In other words, we in effect define the sub-sample of students that offered their services, though some of them were not hired. If information about grade thresholds was revealed in the process of searching for an internship and not during the internship employment, then the grade schedules of the interns and non-interns in this sub-sample would be the same. The results are presented in Column (3) of Table 3. The interaction term is negative and significant, again confirming that the slope of the grade schedule of interns is flatter than that of non-interns. We conclude then that the pattern of internship effects across the grade distribution, with positive effects in the lower part and negative effects in upper part, is consistent with the predictions of our theoretical model and that the source of the differential slopes is internship employment.

5.2 Non-parametric analysis

Another potential issue is that our estimates may not really reflect different slopes of linear schedules but are perhaps due to considerable non-linearities in these schedules. We address this question by non-parametrically estimating internship effects across the distribution of the outcome. The most direct approach is to estimate internship effects at different quantiles of the final year grade average using unconditional quantile treatment effects (QTEs). They are unconditional in the sense that they do not refer to quantiles of conditional distributions

8 The 2SLS interaction term estimate because small in absolute value relative to the coefficient of internship (as with the OLS estimate), impacts mainly the tails of the grade distribution. For example, the final year average of those interns with Year 2 averages in the 70–73 interval is predicted to fall below 70, i.e. below the threshold for a first-class degree.

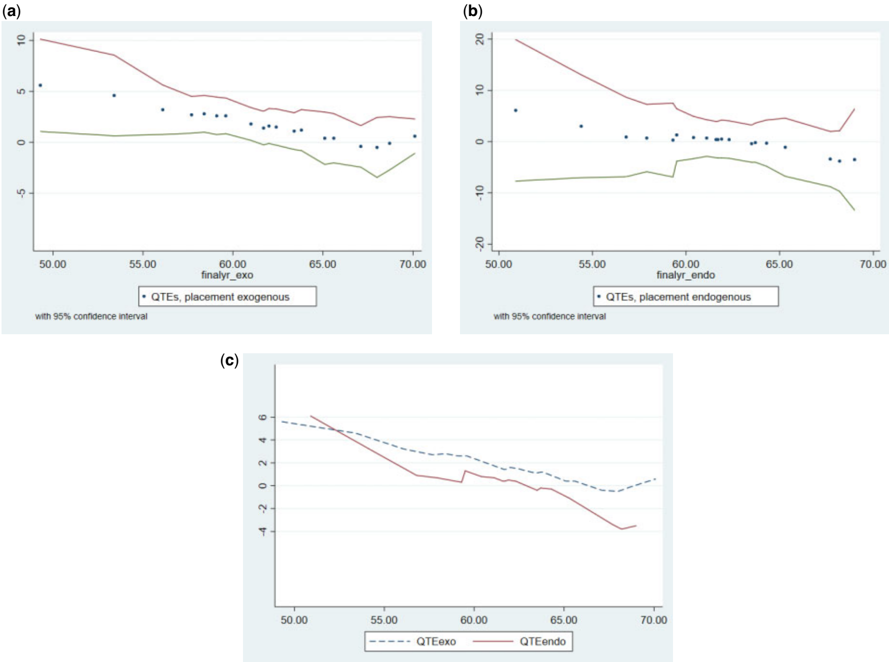

Figure 4. Internship QTEs: (a) QTEs on final year average, placement exogenous, with 95% confidence intervals; (b) QTEs on final year average, placement endogenous, with 95% confidence intervals; and (c) QTEs on final year average with placement exogenous versus endogenous.

Note: Estimation used Stata commands in Frölich and Melly (2010).

but to quantiles of the (marginal) outcome distributions, with and without treatment. The estimation of QTEs depends on whether treatment is considered exogenous or endogenous. Firpo (2007) developed an estimator for unconditional QTEs under treatment exogeneity and Frölich and Melly (2013) developed an unconditional QTEs estimator under treatment endogeneity when an instrument is available.

Figure 4(a) presents QTEs under internship exogeneity using the Firpo (2007) estimator and Figure 4(b) shows QTEs with endogenous placement instrumented with the EII indicator using the Frölich and Melly (2013) estimator. Both figures show QTEs and their 95% confidence intervals, as estimated at 5-percentile intervals, starting at the 5th and up to the 95th percentile. In both figures, the horizontal axis measures final year grade averages as estimated without treatment.[9] Figure 4(c) shows the two sets of QTEs together for comparison. For most of the grade distribution, the effects decrease monotonically and close to linearly. Exceptions are at the very top of the distribution where both sets show that negative effects bottom out, and a small notch in the middle of the distribution of the endogenously estimated QTEs. Overall, however, the departures from linearity are very small, consistent with our specification.

6. Discussion and further results

In this section, we discuss the interpretation of our results and explore how student background may be related to informational frictions. The stylized facts we began with referred

[9] Because treatment effects under exogeneity apply to the whole sample whereas under endogeneity they apply only to the subsample of compliers, the grade distributions without treatment are different. The complier distribution is more concentrated around the centre, but differences are generally small.

to the labour market for university graduates, but similar features may be observed in other labour markets. For example, apprenticeships in Germany require only secondary education. In such market for apprentices, Protsch and Solga (2015) found that employers use academic thresholds in their selection of applicants and that these thresholds were uncertain and varied in the range of 2.5–3.0 (with 1 as the top grade). On the other hand, there are markets for university graduates where the conditions necessary for our model do not apply. For example, in the Netherlands employers do not rely on academic results (other than graduation with a specific degree), which is also common knowledge among students (Allen and van der Velden, 2007; Di Stasio and van de Werfhorst, 2016).

One feature of the internships analysed in this article is that although intra-curricular, they are not a requirement for graduation and they cannot substitute for academic credit. In that sense they are similar to internships undertaken in co-op programmes, which also last about 40 weeks, but their duration can be split into two or three segments. These types of internships are different from internships that comprise a requirement for degrees in fields such as education or medicine or from semester-long internships that may substitute for academic credit. The differentiation between these two types of internships likely reflects differences in the proximity of their skill content to the skill content of the corresponding academic degree. In general, skills developed in the latter type of internships are closely related to students' subject-specific academic skills, unlike the former type where skill proximity can be limited. The dynamics of effort and skill acquisition can get more complicated in internships with skill content in close proximity to an academic subject. On the one hand, the 'skills beget skills and abilities beget abilities' (Cunha and Heckman, 2007, p. 35) property of skills accumulation may benefit disproportionately the more able students and outweigh the information revelation mechanism presented in this article. On the other hand, if there are ceilings in the potential benefits of the more able students, and if the substitutability between skills accumulated in internships and in university is high, we may observe a compression in the grade distribution of interns due to factors other than information revelation.

As suggested earlier, the long duration of the internships analysed here can be crucial for the information on grade thresholds to be revealed. During the 40 weeks to 1 year duration, much of an intern's actual or potential workplace productivity is revealed to the employer, often resulting in job offers at the end of the internship. The job offers in return, whether conditional on academic results or not, reveal whether a grade threshold matters and if so what it is. This process is less likely to take place during university holiday internships lasting 1–3 months because there is less likelihood that an intern's workplace productivity is revealed and therefore less likelihood of a job offer at the end of the internship. For example, it is common for year-long internships to have a one-month handover period during which interns of the outgoing cohort train the interns of the incoming cohort. Following the handover period, these interns can take up projects lasting several months, an opportunity that interns in shorter internships do not have. But there is serious need for systematic data collection and analysis to allow rigorous comparisons of different types of internships.

We turn now to the relationship between student family background and informational frictions in the labour market. Information through social networks is one way that family background can impact upon the information available to a student (on social networks and labour markets, see e.g. Calvó-Armengol and Jackson, 2004). We explore the possibilities that information may flow through private school attendance or through parental occupation. Private school attendance may link into wealth-based networks whereas parental occupation may link to more direct employment-based networks. We test these possibilities by allowing the ability-internship interaction to vary by (i) private school attendance and (ii) by parental occupation. The parental occupation variable here takes the value 1 if a parent's occupation is in the professional or managerial and technical categories and 0 otherwise. Results are presented in Table 5. The first column presents results from a regression

Table 5. Student background and internship effects

X:	Private school	Parental occupation
Internship	2.07***	2.03***
	(0.42)	(0.63)
Internship × *X*	−0.68	−0.17
	(0.85)	(0.78)
Ability	0.65***	0.64***
	(0.06)	(0.06)
Internship × Ability	−0.20***	−0.28***
	(0.06)	(0.07)
Internship × Ability × *X*	0.05	0.14**
	(0.06)	(0.07)
R^2	0.53	0.53
Obs.	603	603

Notes: The dependent variable is final year grade average. The table reports results from OLS regressions with the same controls as Table 3, plus two additional interaction terms denoted with × *X*. Ability in the interaction terms is measured as deviations from its mean. *** indicates significance at the 0.01 level. ** indicates significance at the 0.05 level.
Source: Authors' calculations.

with the same controls as the regression in Table 3 and with added interactions for the internship indicator and the internship–ability interaction. Our interest is in the coefficient of the triple interaction term between internship, ability, and private school or parental occupation. Estimates of these coefficients are presented in the bottom row of the table. A positive coefficient implies that private school or parental occupation moderate the process of information revelation. To put it differently, a positive coefficient implies that the intern grade schedule is less flat than the non-intern schedule for the sub-sample of the interaction term. We find that both interaction terms are positive but only the interaction term with parental occupation is statistically significant. This suggests less informational frictions for those interns with parents in the professional or managerial and technical occupations. This is consistent with these parents making use of their professional networks to inform their offspring about employer selection processes.

7. Conclusion

The educational effects of internships and, more generally, work experience are typically examined from the perspective of the human capital formation theory. In this article, we argue that work experience can also affect academic performance through an informational channel in ways that are different from human capital explanations. Work experience gained at intra-curricular year-long internships provides information about employers' most up-to-date practices in evaluating academic outcomes. This information can influence students' expectations of the value of grades in the labour market and, in turn, their studying effort choice. Drawing on the utility theory of reference dependence and loss aversion, we show that a reduction in uncertainty about grade values asymmetrically affects academic performance with less able students increasing their studying effort and abler students decreasing it. We provide robust empirical support for this theoretical prediction. Our empirical analysis, however, is quite specific in terms of the sample's location in the population ability distribution, its academic context, and the relationship between academic and workplace skills. This specificity has methodological advantages in that it holds key parameters constant, but it is limiting in terms of offering a comparative perspective and generalizations. Larger samples from populations with more diverse academic backgrounds are needed to gain a better understanding of how student work experience informs educational choices.

Supplementary material

Supplementary material is available on the OUP website. These are the data and replication files and the Online Appendix.

Funding

This work was supported by the Public and Environmental Economics Research Group of the University of Bath to purchase HESA data.

Acknowledgements

We are very grateful to Blaise Melly for his advice and to two anonymous referees for their comments. We also acknowledge helpful comments by Andy Snell and participants in the Bath-Bristol Applied Economics Workshop and the EALE annual conference 2020.

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