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How Home Exams and Peers Affect College Grades in Unprecedented Times

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ABSTRACT

How Home Exams and Peers Affect College Grades in Unprecedented Times*

Leveraging administrative data from the University of Iceland, which cover more than 60% of the undergraduate population in the country, we examine how home exams and peer networks shape grades around the COVID-19 crisis. Using difference-in-difference models with a rich set of fixed effects, we find that home exams taken during university closures raised grades by about 0.5 points (≈7%) relative to invigilated in-person exams outside the pandemic period. Access to a larger share of high-school peers leads to an average grade increase of up to two-fifths of a point, and exposure to higher-quality peers yielded additional, but smaller gains. Interactions between peer-network measures and the COVID/home-exam indicators are near zero, providing no evidence that peer networks amplified home-exam gains during the pandemic.

JEL Classification: I21, I23, J24, D85, J16

Keywords: academic performance, online education, COVID-19, networks, academic dishonesty, Iceland

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1 Introduction

Although technological progress in general has rapidly transformed most industries, the education sector has usually been slow to adjust. In recent years, however, universities have expanded their use of distance learning tools, e-learning platforms, and massive open online courses (MOOCs). The COVID-19 pandemic sharply accelerated this transition, forcing institutions to reorganize instruction and assessment. This paper provides new empirical evidence on the separate impacts of the pandemic outbreak and unsupervised take-home exams on college students' grades. We also examine how these effects vary with the share and quality of students' secondary school peer networks within department and course, and whether there are systematic gender differences in responses.

Our setting is the University of Iceland (UoI) in Reykjavík, which offers an ideal environment to address these questions. First, UoI enrolls around 62% of the country's undergraduates and charges no tuition fee, only a low flat registration fee of about \$500 (less than 9% of average monthly earnings in 2022). This yields a representative sample of students without the selection issues, which often complicate studies in larger higher-education systems (e.g., Fairlie and London, 2012; Figlio et al., 2013; Bettinger et al., 2023; Aucejo et al., 2020; Rodríguez-Planas, 2022; Kofoed et al., 2024; Altindag et al., 2025). Second, we use comprehensive administrative data that cover all undergraduate students from Fall 2018 to Fall 2022, including personal characteristics and complete transcripts. By linking these records with information on high school attended and high school grades, we construct secondary-school-based network measures at the department and course level. Iceland has a small number of high schools, many of which feed a large fraction of their graduates into UoI, making such networks salient. Third, while all students faced the same national health system and pandemic restrictions, departments differed in the timing and intensity of online teaching and the use of take-home exams both before and after the onset of COVID-19. We exploit this 'local' variation estimating difference-in-differences models with rich sets of fixed effects.

To the best of our knowledge, this is the first paper to study the effects of unproctored take-home exams on college performance by leveraging the pandemic as a source of exogenous variation in assessment practices in a setting with near-universal coverage of a national undergraduate population and detailed high-school network data. Our empirical analysis exploits the variation in exam format across courses and semesters within departments, jointly with variation in the size and quality of high-school-based networks among students enrolled in the same course. We guide the interpretation of our results using a simple conceptual framework in which students choose effort and collaboration with peers to maximize grades, taking changes in the environment induced by the pandemic as given.

We highlight three main findings. First, home exams during campus closure due to COVID-19 increased grades by roughly half a point, or about 7% relative to the mean. We interpret this as reflecting lower opportunity costs of studying when employment opportunities and alternative activities were restricted. Second, having a larger share of high-school peers in the same department raises grades by up to two-fifths of a point, with smaller but positive effects from course-level networks. We also find a small positive impact of higher-quality networks (peers with higher high school marks), consistent with the role of positive role models. Third, we do not find additional grade gains from high-school networks on home exams during the COVID-19 period, suggesting that illicit collaboration within these networks played a limited role in unsupervised home exams. Gender differences are modest. Men's grades are largely unaffected by COVID-19 restrictions, whereas women experience a small grade improvement of about one-tenth of a point, consistent with the idea that women were better able to organize their study time and work independently during lockdown.

Our work contributes to three strands of literature. First, we add to the research on online learning in higher education. The expansion of e-learning platforms and online degrees has raised questions about their efficacy and cost-effectiveness (Deming et al., 2015), with mixed and context dependent evidence. (See Escueta et al. (2020) for a recent review). For example, Cacault et al. (2021) study a randomized field experiment at the University of Geneva and find that live-streamed lectures lower performance for low-ability students but raise it for high-ability students in compulsory economics and management courses. Other work finds modest positive or negative effects of technology-based interventions and online instruction, often in specific institutional contexts (e.g., Fairlie and London, 2012; Bettinger et al., 2017; Xu and Jaggars, 2013; Figlio et al., 2013; Bowen et al., 2014; Bettinger et al., 2023). We complement this literature by focusing on online assessment rather than instruction and by exploiting quasi-experimental variation in exam formats generated by the pandemic.

Second, we contribute to the growing literature on COVID-19 and college education. Rodríguez-Planas (2022) find that the immediate effect of the pandemic on academic performance at Queens College (CUNY) was positive, particularly for lower-performing lower-income students. Other studies report negative impacts. Aucejo et al. (2020) document adverse effects on graduation timing, grades, and post-college labor outcomes among Arizona State University undergraduates. Kofoed et al. (2024) show that West Point cadets randomized into an online version of an introductory economics course perform worse than those taught in person. Altindag et al. (2025) find that in-person instruction raised grades and reduced withdrawals at a medium-sized US university following the abrupt move online in Spring 2020. Similarly, Bird et al. (2022) and De Paola et al. (2023) report negative effects on course and credit completion in Virginia's Community College and at the University of Calabria (Italy), respectively. By contrast, Binelli et al. (2024) find positive effects on earned credits and GPA at a large university in northern Italy, attributing them to increased study time and access to recorded lectures.

Our contribution is to exploit Iceland's unique administrative data and institutional setting: our data cover all UoI programs before and after COVID-19, with substantial exogenous variation in online teaching and take-home exams within and across departments and over time. The extensive coverage of the Icelandic college population mitigates concerns about college selection and allows us to study a broad cross-section of students, not only those in selective programs or vulnerable socioeconomic groups, who may have responded differently to the pandemic along various dimensions, including mental health (e.g., Etheridge and Spantig, 2020; Proto and Zhang, 2021; Adams-Prassl et al., 2022; Quintana-Domeque and Zeng, 2024; Foliano et al., 2024).

Third, we speak to the literature on peer effects in higher education (see, e.g., Sacerdote, 2011; Bramoullé et al., 2020; De Giorgi et al., 2022). Identification in this literature often relies on roommates and dormmates, or random assignment to classes or sections. These strategies are not available in our context: admission to UoI departments does not depend on high school grades, students are not randomly assigned to classes, and most undergraduates live off-campus, with dorms housing only about 11% of students, and assignment is not random.[1] We therefore approximate latent peer groups using secondary-school cohorts. About 85% of the undergraduates in our sample come from just 16 high schools in Reykjavík, making the UoI community highly interconnected. Survey evidence shows that students who started during the pandemic formed fewer new ties and relied more heavily on secondary-school networks (Sigurdardottir et al., 2023), and that secondary-school background is the single most important predictor of students' social networks (Torfason et al., 2021). As Epple and Romano (2011) emphasize, such measures capture both direct and indirect peer effects. We construct high-school-based network measures at the department and course levels, using either the share of high school peers (network size) or their average high school grades (network quality). Section 3 discusses how we address potential correlations between program assignments and grades. Importantly, this peer environment shifted exogenously for all students when UoI closed due to the COVID-19 outbreak (see, among others, Agostinelli et al., 2022).

A central question in this context is whether stronger secondary-school networks facilitated illicit collaboration in take-home exams during the pandemic, a behavior we refer to

[1] Admission not based on high school marks, ruling out regression-discontinuity designs. Classes are typically small enough that all enrolled students attend, so there is no quasi-random allocation to sections.

as "CPR" ("Collaboration under Pandemic Restrictions"). While we have neither objective nor self-reported measures of cheating (see Bilen and Matros, 2021; Carrell et al., 2008), our effort-and-learning model guides how to interpret the interaction between network measures, exam formats, and the COVID-19 period. Intuitively, the incentive to cheat in home exams should rise with the expected gains from accessing a larger or higher-quality peer network when assessments are unproctored, relative to invigilated in-person tests.[2] Our finding that high-school networks raise grades, but not disproportionately so during COVID-19 when exams were taken at home, suggests that such illicit collaboration remained limited in our setting.

The remainder of the paper is organized as follows. Section 2 presents the model that underpins our analysis. Section 3 describes the research design, and Section 4 the data. Section 5 reports the main results, and Section 6 concludes. The Online Appendix contains supplementary material and additional evidence.

2 A Model of College Performance with Effort and Cheating

We start with a model in which students choose how much effort to put into studying and whether to cheat on exams to maximize their grades. Studying and cheating on exams serve as input and grades as output. The cost of effort is the disutility of studying in terms of sacrificed leisure and labor income (with part-time employment being a choice frequently made by college students in Iceland), while the opportunity cost of cheating is the expected loss due to detection. At the optimum, a student cannot reduce the total cost of effort and cheating for a given grade by substituting one of these two inputs for the other. The optimal combination of studying and cheating depends on the marginal costs of effort and cheating, as well as several parameters that we discuss below.

We assume that undergraduates dislike effort *per se* and prefer to earn their grades with minimal effort. Students can enjoy learning, but at some point they begin to dislike increasing the time devoted to, or the intensity of, studying. The disutility of effort is given by the function $p(e; \Psi)$, with $p' > 0$, $p'' > 0$, so that the marginal disutility of effort increases in effort, e. We collect all other factors that influence p into the parameter Ψ. This includes the opportunity cost of studying in terms of the sacrificed utility of doing other activities, such as leisure (including sports and going clubbing) or working to earn money, and the effects of increased availability of online academic material and exam anxiety (and more generally mental distress) on the difficulty of learning. Networks may also affect the

[2] Chan and Ahn (2023) find that cheating in online exams was either limited or ineffective at boosting scores; Newton (2023); Newton and Essex (2024) argue instead that cheating rose substantially in unproctored online exams, especially during the pandemic.

disutility of effort by making studying more enjoyable and allowing members to engage in legitimate human-capital-enhancing activities. We will refer to p as the *disutility of effort function*.[3]

The benefit of studying is measured by the grade obtained in each exam. The grade is a function of individual effort and (collective) collaboration during exams, i.e., cheating c. That is, $g(e + c; \Phi)$, where $g' > 0$ and $g'' < 0$, giving concavity. We refer to g as the *reward function*. Legitimate, even encouraged, collaboration among students when preparing for an exam is expected to increase the productivity of effort and is embedded in Φ. Cheating c, instead, enters g as a separate determinant. The parameter Φ measures factors that affect grades, keeping effort and cheating constant, such as the size and quality of networks, which affect the effectiveness of studying. It also includes the college marking system that could have become more lenient during the COVID-19 crisis, and other factors, such as the student's mental health, which might also have changed in the pandemic period.

In addition to effort, students make a decision about their academic integrity, which is captured by c. Suppose they choose to cheat during an exam. In that case, we assume that dishonesty is detected with probability q and, if caught, students suffer a penalty P in utility terms (for a similar setup in the labor market context, see Shapiro and Stiglitz (1984)).[4] The parameter P includes the stigma of having been found cheating and the possible loss of future earnings if the academic offense is recorded on the student's transcripts, visible to employers. The term also has a subjective element so that a conscientious student is endowed with a higher P, and thus would cheat less and study more. The probability of detection is a convex increasing function in the level of c, i.e., $q(c; P, \Gamma)$, where $q' > 0$, $q'' > 0$, and Γ captures other factors that affect the probability of being caught, such as whether exams are on-site and in-person or taken at home and unsupervised. We call q the *penalty function*.

Each student decides on the effort e and the level of cheating during an exam c to maximize their grade. The first-order condition with respect to effort sets the marginal benefit of increased effort in terms of a higher grade equal to the marginal disutility of studying:

$$g'(e + c; \Phi) = p'(e; \Psi). \tag{1}$$

The student also sets the marginal benefit of cheating on exams equal to the marginal cost, which depends on the penalty imposed on a cheater, P:

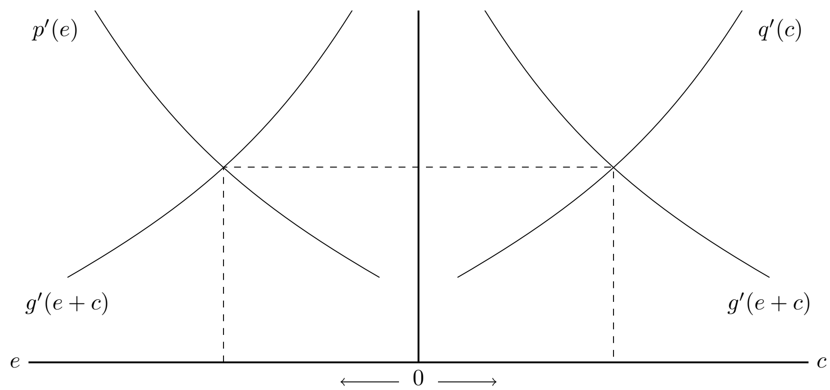
$$g'(e + c; \Phi) = q'(c; P, \Gamma). \tag{2}$$

[3] The underpinnings of the function p can be traced back to Solow (1979).

[4] We can allow different probabilities of detection depending on whether the exam is in class or at home, with $q^{class} > q^{home}$. Still, the main insights of our analysis will not change. In our basic setup, without loss of generality, we implicitly assume $q^{class} = 1$ and label q^{home} as q.

The solution is shown in Figure 1, where the marginal disutility of studying is equal to the marginal cost of cheating.[5] A student can both study hard and cheat to attain a given grade. On the one hand, studying is demanding and gives disutility while, on the other hand, cheating is hazardous since the student may be caught flouting the rules and be punished. At an optimum, the marginal costs in terms of disutility and in terms of the risk of illicit collaboration are equalized.[6]

Figure 1. The First Order Conditions for the Student's Problem



Notes: The figure shows the optimal level of effort, e, and cheating, c, for a student studying for a course.

It follows from equations (1) and (2) that the marginal rate of substitution between effort and cheating depends on the ratio of the second derivatives of the penalty function and the disutility-of-effort function:

$$\frac{de}{dc} = \frac{q''(c; P, \Gamma)}{p''(e; \Psi)}.$$

A given increase in cheating, dc, will then cause the effort to increase more if the slope of the marginal penalty curve, q'', is greater than the slope of the marginal disutility-of-effort curve, p''.

2.1 COVID-19 and its Interplay with Networks

The COVID-19 pandemic and subsequent lockdown restrictions affected all three relationships: the opportunity cost of studying, the importance of networks in studying and cheating on exams, and the probability of being caught cheating. The effects are captured by the disutility-of-effort function, p, the reward function, g, and the penalty function, q.

[5] The solution represents a maximum because the Hessian matrix of second derivatives is negative definite.

[6] Corner solutions are possible. For example, a conscientious and diligent student (with a high value of P) may find it optimal not to cheat at all. In this case, the marginal cost of cheating intersects the marginal benefit of cheating at $c = 0$, while the marginal-benefit-of-effort function in the left-hand panel of Figure 1 is shifted upwards to the left generating an equilibrium where e is high.

First, the opportunity cost of studying is reduced when people face an explicit limitation on social interactions through lockdown as well as not being able to do paid work on the side. This is captured by the parameter Ψ in the disutility-of-effort function $p(e;\Psi)$. The pandemic would then induce students to study more, thereby increasing their effort. But the pandemic, by limiting social interactions, might also have harmed students' mental health and well-being, which in turn may have an adverse effect on effort and effectiveness in studying. This is captured by Φ in the reward function $g(e+c;\Phi)$, which weakens the effect of effort on the measured performance.

Second, the existence of networks generally benefits students. Teamwork in preparation for exams raises the effectiveness of effort and may eventually raise grades. This is captured by Φ in g. Breaching university rules and ethics by cheating during exams, c, instead has a direct impact on grades. Networks may also increase effort through the term Ψ in the function p by making studying more enjoyable.[7]

Third, the pandemic made universities move teaching online and allowed students to take their exams at home. Unsupervised take-home exams opened the door to cheating, both by individual students using books and internet searches, and by collaborating with peers. This is captured by the parameter Γ in the penalty function $q(c;P,\Gamma)$. University authorities may also have become more lenient in dealing with students caught cheating, lowering the penalty P for those caught breaking university rules.

2.2 How COVID-19 and Networks Affect Students' Performance

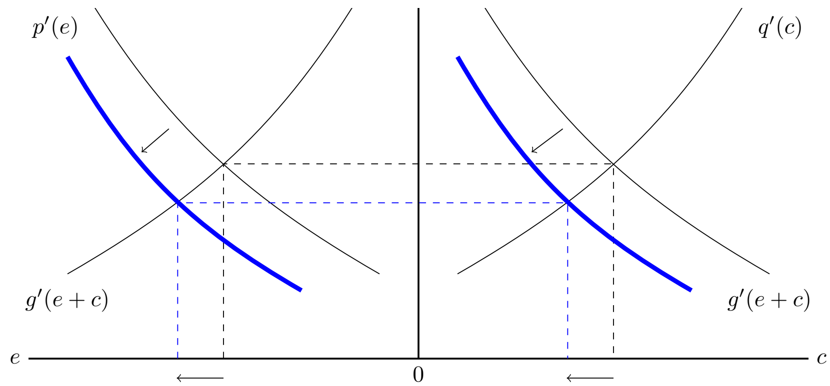
We begin by tracing the direct effects of the pandemic on effort and cheating. The first effect is the reduction in the opportunity cost of studying during the pandemic. This implies that the marginal-disutility curve in the left-hand side panel of Figure 2 shifts to the left, making the student study more. In the right-hand side panel of the same figure, the marginal benefit of academic dishonesty is lowered as a result, which leads the student to cheat less in home exams. Not having much else to do, students may decide to study more. Having studied more, they could be better prepared for exams and have lower incentives to cheat. The net effect on grades is ambiguous, while the student's knowledge is unambiguously higher.[8]

However, students may feel depressed during the pandemic. This would make studying more challenging and possibly reverse the effect on effort by shifting the disutility-of-effort

[7] There are several channels through which this can occur. Carlana and La Ferrara (2025) find that online tutoring programs improved students' aspirations, socio-emotional skills, and psychological well-being during school closures, thus creating study networks between tutors and students. Although these could also have occurred at UoI, we cannot identify them with our data.

[8] The net effect on grades cannot be easily determined, since it depends on the slope of the three curves in our framework, For example, if the penalty curve is steep, cheating will be reduced less and the effect on g becomes positive.

Figure 2. Effects of Reduced Opportunity Cost of Studying during COVID-19



Notes: The figure shows the effect of COVID-19 on effort and academic dishonesty through a lower disutility of effort.

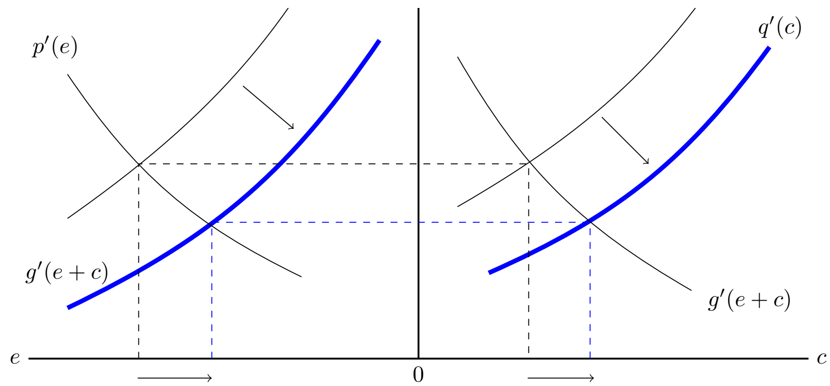
curve upward and the reward function downward. The students would then study less and, possibly, cheat more.

There is also the effect of the COVID-19 outbreak, which led universities to move teaching online and allow students to take their exams unproctored at home. These changes may make cheating easier, something that works through the term Γ in the probability of detection function, q. Anecdotal evidence suggests that the penalties for cheating at UoI were small. The effect is to shift the marginal-cost-of-cheating curve in the right panel of Figure 4 to the right, generating more academic offenses. This reduces the marginal benefit of studying in the left panel of the same figure, making students study less. The effect of a reduction in the penalty for cheating, P, would be identical.[9] As in the case of Figure 2, the net effect on g is ambiguous as it depends on the slope of the three key curves of the model. For example, if the disutility-of-effort function is steep, effort will be reduced less and the net effect on grades becomes positive.

We now turn to the effect of peer networks. These exist independently of the lockdown restrictions, but may have become more important during the pandemic crisis. Stronger networks may benefit students, so that their members can perform better for a given level of effort, through both encouraged teamwork during term times and illicit collaboration under pandemic restrictions (CPR) in take-home assessments. These elements are captured by Φ in the reward function g. Their effect is to increase both effort and cheating, as shown in

[9] In the case of a linear disutility-of-effort function, $p'' = 0$, the fall in effort will completely offset the increased cheating, leaving the grade g unchanged. Cheating will not only increase because it is easier but also because effort is reduced. In this case, the marginal disutility curve is horizontal, which pins down the level of the marginal benefit of the sum of studying and cheating. With increasing marginal disutility of studying, $p'' > 0$, the curve becomes upward sloping. Here decreased effort e lowers the marginal disutility of effort and the crowding out of effort is incomplete, generating a higher level of $e + c$ when the penalty of cheating is reduced, hence a higher grade $g(e + c; \Phi)$.

Figure 3. The Effect of COVID-19 on Cheating when Detection is Harder



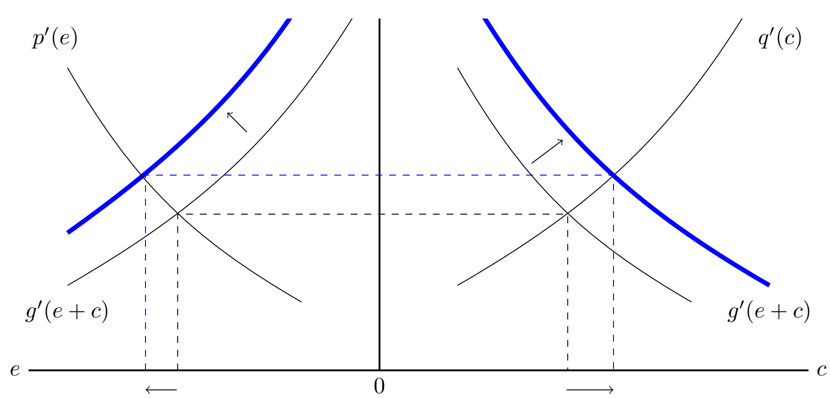
Notes: The figure shows the effect of COVID-19 on academic dishonesty and effort when take-home exams make detection of cheating more difficult.

Figure 3. The effect is to increase grades.

Networks may also reduce the disutility of effort through Ψ in $p(e; \Psi)$ by making studying more enjoyable, which would shift the disutility of effort curve downward, leading students to increase their effort even further while reducing the incentives to cheat on exams. At the same time, the marginal benefit of academic dishonesty in exams is reduced, causing students to commit fewer offenses.

This effect is further strengthened if the exam-marking policy, captured by Φ, becomes more lenient during the pandemic. If a more lenient grading environment leaves the marginal utility of studying, g', unchanged — i.e., it does not change the slope of the utility of studying curve — this will affect neither e nor c; whereas, if good exams were rewarded more than before, the incentive to both cheat and study would be enhanced, the marginal utility of studying would be increased, generating a higher grade for both reasons.

Figure 4. Effects of Networks on Academic Performance and Dishonesty



Notes: The figure shows the effect of networks on effort and cheating when networks both increase productivity in studying and make cheating easier.

Taking stock of the key insights from the model, COVID-19 could have raised grades by reducing the opportunity cost of studying and making cheating easier, as CPR detection would have been harder and penalties associated with misbehavior during home exams might have been reduced. The degree to which higher grades can be attributed to increased effort or increased cheating depends on the relative size of these two effects. Lower grades could instead be the outcome if students' health and concentration deteriorated in response to the pandemic, something that would have curtailed effort. Networks are expected to increase grades by increasing the productivity of studying and easing CPR, as well as reducing the disutility of studying. These effects could have been enhanced during the pandemic crisis, when teaching was online and online exams were unsupervised and taken at home. We shall take these predictions to the data.

3 Research Design

This section links the model developed in previous section to our empirical strategy. We begin with a simple grade production function for each student i: $g_i = g(e_i, \Theta_i)$, where e is individual effort and Θ is a vector of all parameters described in Section 2, which enter the reward, penalty, and disutility-of-effort functions, as well as those affecting the value of peers. We take this to the data using the following specification:

$$G_{imdt} = \alpha + \lambda C_t + \gamma H_{imdt} + \beta(C_t \times H_{imdt}) + \mathbf{X}_{imdt}\boldsymbol{\xi} + \mu_i + \delta_m + \rho_d + \eta_t + \{FEs\} + \varepsilon_{imdt}, \quad (3)$$

where G_{imdt} is the sample analogue of g in the model and denotes the grade for individual i, sitting the exam in course m, in department d, and semester t. C_t is a dummy variable that takes value 1 for the academic terms under COVID-19 restrictions and 0 otherwise; H_{imdt} is another indicator variable that takes value 1 for take-home (unproctored) exams, and 0 for on-site (in-class and invigilated) exams. While C is invariant across departments, courses, and students, H varies for each student i not only over time t (semesters), but also across courses, m, both within a given department and across departments, d, as some departments have take-home exams regardless of the pandemic restrictions.

The vector of control variables $\mathbf{X}_{imdt}$ includes the student's age and the age squared. Other measures available in the data are time-invariant and are thus excluded since model (3) accounts for individual fixed effects, μ_i. These time-invariant factors are country of origin, secondary school attended, and gender. Secondary school will be central to the construction of the network measures described below; interactions of secondary-school fixed effects, φ_s, with other determinants of performance will also be accounted for. For foreignborn students who attended secondary schools outside Iceland, the definition of high school (and hence networks) overlaps with country of origin. Finally, gender is relevant given observed performance gaps (Bertrand, 2020) and differences in the propensity to lie or cheat (e.g., Erat and Gneezy, 2012), motivating separate analyses by gender, after presenting our benchmark results from the pooled sample of women and men.

We saturate the model with individual fixed effects, μ_i, course fixed effects, δ_m, department fixed effects, ρ_d, and semester fixed effects, η_t. These absorb unobserved heterogeneity in students (a proxy of the time-invariant of e), instructors (a proxy of instructor-specific teaching styles, since courses are always taught in one section by one lecturer), departments (a proxy of departmental pedagogy and evaluation norms, which may change only relatively slowly over time), and aggregate temporal shifts in performance.[10] Additional fixed effects included in $\{FEs\}$ are described below.

The parameters of interest are λ, γ, and β. Based on Section 2, we expect $\lambda > 0$: the pandemic plausibly reduced the opportunity cost of studying. We expect $\gamma > 0$ as well, because take-home exams may encourage cheating, which is less detectable when assessments are unsupervised.[11] Finally, $\beta > 0$ since the pandemic could amplify the effect of take-home exams by compounding lower opportunity costs of studying with easier flouting of exam rules. β can be interpreted as a difference-in-difference parameter, whereby the impacts of take-home exams, H_{imdt}, on grades, G_{imdt}, are compared before/after and during COVID-19 restrictions, C_t. Conditional on the predetermined variables included in $\mathbf{X}$, the full set of fixed effects and the exogenous temporal diffusion of C across departments and courses, the residual difference-in-difference variation in the impact of H on G relies on the timing of take-home exams within cohort, department, course, and individual student, which is as good as random.[12]

[10] The within-student, cross-course variation that separately identifies δ_m and ρ_d comes from the fact that students in the same department can enroll in different courses, some of which are offered by other departments. Similarly, some courses can be part of the curriculum in more than one department. Thus, students are not restricted to taking courses within a single program or department, and various courses are used across majors.

[11] The sign of γ is theoretically ambiguous: If COVID-19 led to more cheating because detection was harder (Figure 4) or via illicit CPR (Figure 3), we expect γ to be positive. For a similar prediction, see Chan and Ahn (2023). Of course, γ could pick up effects other than remote learning and take-home exams, such as students' lower test anxiety and lecturers' greater leniency in marking summative assessments. We should also underline that the "experimental" dimension induced by COVID-19, with its forced modes of teaching delivery and exam assessment, is likely to identify a different parameter from those estimated by, say, Figlio et al. (2013) or Cacault et al. (2021), but more similar to those pinned down by Aucejo et al. (2020), Rodríguez-Planas (2022), or De Paola et al. (2023).

[12] Identification of γ stems from courses that switched mode of evaluation (from in-class to take-home and vice versa) before and after the lockdown restrictions, while identification of β arises from switches during the pandemic semesters. There was no room for anticipation by students or lecturers.

Network measures — The model in Section 2 highlights the role of peer networks in shaping effort, learning, and potential cheating. Instead of modeling unobserved network formation (e.g. Carrell et al., 2013; Battaglini et al., 2022), we follow the conventional approach of proxying latent social networks using observable characteristics (e.g., Sacerdote, 2011; Patacchini et al., 2017; Lavy and Sand, 2019) — in our case, secondary school origin. We augment equation (3) with two separate network measures. The first is measured at the course level and is given by

$$N_{imdt}^{\{m\}} = \frac{S_{s,m,t,-i}}{S_{m,t,-i}}, \tag{4}$$

where $S_{s,m,t,-i}$ is the number of undergraduates from the same secondary school s as focal student i, enrolled in the same course m and in the same semester t (excluding i), and $S_{m,t,-i}$ is the total course enrollment, again leaving student i out. $N_{imdt}^{\{m\}}$ varies between 0 (no peers present) and 1 (all peers from the same high school as the focal student).

The second measure is defined analogously, but now at the department level, d, i.e.,:

$$N_{idt}^{\{d\}} = \frac{S_{s,d,t,-i}}{S_{d,t,-i}}, \tag{5}$$

which is the share of high school peers studying in the same department and the same semester, irrespective of year of study. $N^{\{d\}}$, therefore, picks up peers, say, enrolled in the final year in a given department, while i might be a first-year student.

In the specifications that include either $N^{\{m\}}$ or $N^{\{d\}}$, the corresponding parameters $\theta^{\{m\}}$ and $\theta^{\{d\}}$ are expected to be positive. This follows from the model's prediction that networks raise grades by making studying more enjoyable for a given level of effort, by making effort more productive through collaboration, and by facilitating cheating during exams, especially in the case of take-home assignments when communication among students is close to impossible to observe without appropriate home detection technology.[13]

In line with social identity theory (e.g., Jenkins, 2014), both measures reflect the extent of shared identity, i.e., students who attended the same secondary school share a distinctive *esprit de corps*, even if they are not close friends. We have no prior expectation about whether course-based networks, $\theta^{\{m\}}$, should matter more or less than department-based networks, $\theta^{\{d\}}$. Although the extent of collaboration could be greater among secondary-school peers who sit in the same course, social identity could express itself more intensely when students take a broader departmental perspective.

Both $N^{\{m\}}$ or $N^{\{d\}}$ identify peer *quantity*: the larger the share of peers from the same school, the greater the potential influence on grades. However, peer *quality* may also mat-

[13]We expect a positive impact even if the direct effect of collaboration during term times *per se* (e.g., studying together and sharing notes) may reduce individual effort.

ter. To capture this, we construct analogous measures based on the mean secondary-school graduation marks of a student's peers in the same course or in the same department, $M^{\{m\}}$ and $M^{\{d\}}$, respectively (with corresponding parameters $\zeta^{\{m\}}$ and $\zeta^{\{d\}}$). High-ability peers could boost performance through positive spillovers or discourage lower-ability students; we therefore let the data determine the sign and magnitude of these effects.

Identification Issues and Fixed-Effects Structure — A classic challenge in estimating the θ's and ζ's effects is sorting (Manski, 1993). observing students in a program in a given department is the result of choices on the part of the students themselves, their parents and high schools. For example, graduates from one high school may overwhelmingly enroll in medicine, while those from another may tend to cluster in law. Likewise, the best graduates from one school may predominantly go to chemistry, while the best from another would go to economics (and specialize in macroeconomics rather than econometrics). These patterns are likely to reflect secondary schools' specialization and reputation, as well as expectations among pupils and parents. This sorting is likely correlated with grades and could suggest the existence of peer effects where none exists. To mitigate this concern, equation (3) includes a rich structure of fixed effects — department, course, and cohort (or semester) — and exploits year-to-year variation in the distribution and quality of peers within departments and courses.

Students from the same cohort and department may have different treatment exposure, since treatment also depends on high school. This variation allows us to account for additional forms of sorting contained in $\{FEs\}$. Because individual fixed effects μ_i absorb φ_s, we cannot include secondary-school fixed effects directly. Instead, we allow sorting into departments to vary by school using department-by-high-school fixed effects, $\rho_d \times \varphi_s$, which compare students from the same high school within the same department across cohorts. We also include cohort-by-high-school fixed effects, $\eta_t \times \varphi_s$, which account for changes in the composition of high-school cohorts over time, and department-by-cohort fixed effects, $\rho_d \times \eta_t$, absorbing time-varying departmental shocks. With all these fixed effects, identification relies on residual variation akin to a triple-difference design, where the differencing levels are cross-semester, cross-high-school origin, and cross-department.[14]

Interpreting Network Interactions as CPR — Section 2 argues that COVID-19 restrictions may have increased opportunities for cheating in unsupervised take-home exams. We cannot observe cheating directly, but network interactions with C_t and H_{imdt} provide an

[14] We do not perform the same exercise adding further interactions with course fixed effects because these are highly collinear with those that include department fixed effects. Replacing department with course fixed effects interactions does not change our main results.

indirect way to detect it. Specifically, we interpret positive coefficients on $C{\times}N$ and $C{\times}H{\times}N$ (and analogously for M) as indication that students benefit more from their networks *during* the pandemic, when assessments were unproctored. We interchangeably refer to such behavior as academy dishonesty or collaboration under pandemic restrictions (CPR).

This interpretation relies on three assumptions. First, cheating leads to higher grades. A positive triple interaction is consistent with this. Second, cheating is easier on unproctored, online exams than on invigilated, in-class exams. This is supported by substantial evidence (e.g., Harmon and Lambrinos, 2008; Cluskey Jr. et al., 2011; McGee, 2013; Holden et al., 2021). Third, cheating gains increase with network size/quality. This requires a significant positive deviation of the triple interaction coefficients compared with their double interaction counterparts. Thus, improved performance during COVID-19 is not enough: CPR requires that grades rise more for students with larger or better networks when those exams are take-home rather than invigilated.[15]

4 Data

Sample Selection within Our Institutional Background — The data come from the administrative records of the University of Iceland and span nine academic semesters for the universe of undergraduate students at the University, starting in the Fall of 2018 through to the Fall of 2022. The timing of the data provides complete coverage of the period leading up to, during, and following the COVID-19 crisis.

Compared to the rest of the world, Iceland was not severely affected by the lockdown restrictions. Except for a complete closing of the University during the first wave of the pandemic between mid-March and early May 2020, the UoI campus remained at least partially open for most of the time from May 2020 to June 2021. Although subject to strict regulations, teaching in small groups was possible and departments could, for example, prioritize splitting first-year classes into smaller groups, thereby offering them the possibility of some on-site teaching. Many students were, however, reluctant to attend in-person lectures and participate in other activities on campus. Most instructions were moved online in the Spring and Fall of 2020 when many departments administered take-home exams, although individual lecturers were free to plan in-class exams. This was hard to fully anticipate. Spring 2021 consisted mainly of online teaching, but Fall 2021 included a mix of online and on-site teaching.

[15] This parallels Chan and Ahn (2023)'s insight that students who underperform in in-person exams have stronger incentives to cheat online. Alternative identification strategies, such as those in Lavy (2024), are feasible in our data but left for future work.

The distribution of home exams by semester was as follows: 93% in Spring 2020, 55% in Fall 2020, 56% in Spring 2021, 22% in Fall 2021, and 18% in Spring 2022. Before Spring 2020 and in Fall 2022, the home exam rate was around 10%. Thus, both the temporal distribution and the within-department distribution of take-home exams have been essentially unpredictable from the students' viewpoint. This means that students could not be in a position to choose their courses strategically based on exam mode (in-class versus take-home). The exam mode is not available pre-pandemic and for Fall 2022, but all exams are assumed on-site for these semesters.

The original administrative records contain roughly 406,000 student-semester-course observations for nearly 21,300 undergraduates. Case-wide deletion of observations based on missing values in key variables (most notably university grades, which are often missing when students choose not to sit an exam without penalty) leads to a working sample of 184,354 student-semester observations from 17,791 undergraduates (6,337 males and 11,454 females). Appendix Table A.1 summarizes the selection criteria used to construct the final estimation sample from the original UoI records. Because secondary-school marks are not available for all students, the high-performance indicator and the peer-quality network measure are defined on smaller subsamples (see below). The results reported in the next section are unchanged when the analysis is restricted to these subsamples.

Descriptive Statistics — Table 1 provides the summary statistics of the main variables for the final sample and by gender. Our outcome variable is grade, G_{imdt}, which is measured for each individual in each course on a scale from zero to ten. The overall mean grade is 7.3 (s.d.=1.94), with women performing better than men, 7.4 versus 7.1 on average, with the difference of 0.3 being statistically significantly different from zero (p-value<0.0001). This is also reflected in the gender difference of the high-performance indicator. [16]

Because of the time window covered by the data, almost 39% of the student-semester-course observations under analysis refer to exams that students took during the period of the restrictions induced by the pandemic. This is C_t in equation (3). Another 22% of the whole sample, on and off COVID-19, involves take-home exams, i.e., H_{imdt}. Gender differences in C_t and H_{imdt} are small and statistically insignificant. The overall mean of the network measure defined at the course level, $N_{imdt}^{\{m\}}$, is 0.087, that is, 8.7% of the individuals in a course

[16] An additional 16,884 student-semester-course observations are graded on a pass/fail basis. We can impute numerical grades using departmental and semester-specific averages for these cases. Specifically, students who pass are assigned the average pass grade for their department during that semester, while students who fail are assigned the average fail grade for their department during that semester. The new overall mean grade for this larger sample is 7.2 (s.d.=2.03), 7.0 for men and 7.3 for women. The analyses of this larger sample deliver similar results to those discussed in Section 5, and are therefore not reported. They are available from the authors.

Table 1. Summary Statistics

Variable	All	Women	Men	Gender gap
G_{imdt} (grade)^a	7.306	7.413	7.116	0.297***
	(1.938)	(1.831)	(2.010)	
H_{imdt} (take-home exam)^b	0.217	0.216	0.218	–0.002
C_t (COVID-19)^b	0.385	0.388	0.380	0.008
$N_{imdt}^{\{m\}}$ (course network, quantity)^c	0.087	0.083	0.095	–0.012*
	(0.114)	(0.108)	(0.125)	
$N_{idt}^{\{d\}}$ (department network, quantity)^c	0.071	0.069	0.076	–0.007
	(0.065)	(0.061)	(0.070)	
$M_{imdt}^{\{m\}}$ (course network, quality)^{a,d}	7.546	7.534	7.570	–0.036***
	(0.666)	(0.657)	(0.681)	
$M_{idt}^{\{d\}}$ (department network, quality)^{a,e}	7.550	7.548	7.554	–0.006
	(0.549)	(0.540)	(0.563)	
High-performance student^b	0.525	0.565	0.460	0.105***
Age (years)	24.829	24.966	24.587	0.379***
	(6.286)	(6.482)	(5.915)	
Foreign^b	0.079	0.083	0.072	0.011**
Faculty:				
Health Sciences^b	0.299	0.372	0.169	0.203***
Social Sciences^b	0.247	0.233	0.272	–0.039**
Humanities^b	0.131	0.131	0.130	0.001
Education Sciences^b	0.106	0.122	0.079	0.043**
Engineering and Natural Sciences^b	0.217	0.142	0.350	–0.208***
Student-Semester-Course Observations	184,354	117,715	66,639	

Notes: The table reports means (standard deviations in parentheses). 'Gender gap' refers to (women–men) differences. ^{a} ranges from 0 to 10 (included). ^{b} dummy variable {0,1}. ^{c} ranges from 0 to 1 (included). ^{d} The observations are 164,293, 104,810, and 59,483 for all, women, and men, respectively. ^{e} The observations are 122,459, 78,128, and 44,331 for all, women, and men, respectively. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

are in the student's network, whereas the mean of $N_{idt}^{\{d\}}$ is 0.071, meaning that, on average, 7.1% of the students in a given department come from the same high school as the focal student.[17] Peers' average high-school marks, our measure of peers' quality, whether within

[17]For students who attended a foreign high school, their country of origin identifies their high school. Thus, for the proportion to be 1 for a student who went to a foreign secondary school, everyone in a given course (or department) at semester t must be of the same country of origin as the focal (foreign) undergraduate student. The proportion is 0 if no other student in the same course (or department) at time t is from the same foreign country as student i.

the same course, $M^{\{m\}}$, or within the same department, $M^{\{d\}}$, are essentially identical at 7.55 out of a maximum score of 10. However, the dispersion is smaller for the latter (0.55 versus 0.67), perhaps reflecting a larger sample size. Men display larger average network quantity and quality along the course margin, $N^{\{m\}}$ and $M^{\{m\}}$, while gender differentials at the departmental level, $N^{\{d\}}$ and $M^{\{d\}}$, are negligible.

The students in the sample are on average 25 years old, a higher age than what is observed among undergraduates in other advanced economies. This is in part due to the fact that Icelanders finish high school when they are 19–20 years old on average, and many start their university studies after traveling abroad or after working for some time while others work while studying at college. UoI is organized into five separate Schools. Health Sciences is the largest, with about 30% of the observations, while the School of Education and the School of Humanities are the smallest, covering around 11% and 13% of the sample, respectively. Social Sciences, as well as Engineering and Natural Sciences are similar in size and account for almost half of the sample jointly. The University is female-dominated (particularly in Health Sciences and Education), although males are about 2.5 times more likely than females to be enrolled in the School of Engineering and Natural Sciences. About 8% of the sample comprises foreign individuals, that is, students who were educated in a foreign secondary school.[18]

Since a distinctive feature of the university experience during the pandemic was a greater reliance on distance learning and take-home exams, Appendix Table A.3 presents the summary statistics by exam mode. This gives us a better understanding of the results discussed in the next section. The table distinguishes the observations assessed with traditional invigilated on-site exams ($H_{imdt} = 0$) from the cases with unproctored take-home assessments ($H_{imdt} = 1$). Although hard to document empirically, we collected qualitative anecdotal evidence, which suggests that teaching methods are comparable between assessment types and were not substantively altered during the pandemic, with the obvious exception of almost universal online delivery.

Except for gender, along which we have balance between the two subsamples, Appendix Table A.3 detects statistically significant differences along all the other observable char-

[18] Appendix Table A.2 show that the selection criteria used to construct our final sample are unlikely to bias our results. The largest differences emerge in terms of grades (higher average grade in our final sample as opposed to the original UoI records) and network quality (peers in the final sample have higher secondary school marks on average). This is not surprising because our analysis needs to observe individuals with valid G. Most of the other differences are either negligible (both statistically and quantitatively) or hard to reconcile with a sample selection driven by a substantially greater exposure to COVID-19 and remote exam incidence rates. However, we cannot exclude the possibility that the individuals who dropped out of our final sample could have faced pandemic-driven psychological challenges that led to withdrawal or poor exam performance. However, this remains just a possibility, which we cannot test with our data.

acteristics. This is not surprising for two reasons. First, as the table shows, 93% of the student-semester-course observations in the take-home subsample refer to the COVID-19 period (as opposed to 23% in the other subsample). Second, health sciences, as well as engineering and natural sciences, are overrepresented in the subsample taking on-site exams, which are likely to reflect historical differentials in summative assessment practices across UoI faculties. On average, students evaluated at home are one year older and slightly more likely to be foreigners. Take-home-exam cohorts also show that both network quantity and network quality measures are smaller (i.e., they have fewer peers and peers with lower prior academic performance), while their raw grades are roughly four-tenths of a point higher, suggesting that unadjusted differences partly pick up compositional effects, which will be accounted for in estimation.

5 Results

5.1 Main Estimates with Network Quantity

Table 2 reports our benchmark results. The first column of the table shows the estimates on C_t, H_{imdt}, and their interaction, partialling out the impact of the controls $\mathbf{X}_{imdt}$, as well as the course, department, student, and time fixed effects. In line with the predictions of Section 2, we find that home exams led to an average grade increase of 0.25 ($\gamma > 0$) and that students' academic performance *improved* slightly by 0.07 points during the COVID-19 pandemic ($\lambda > 0$). Participating in take-home exams during the lockdown restriction period led to an additional mean grade increase of 0.17 points ($\beta > 0$). When aggregating these three effects, the total increase in average grade in G driven by home exams during the COVID-19 crisis as opposed to invigilated exams on-site (in-person) off the pandemic is approximately 0.49 points, a 6.8% premium over the baseline mean grade of 7.3.

Undergraduates may have faced isolation, psychological distress, and declines in mental well-being during the pandemic, yet their academic performance does not appear to have deteriorated (for similar evidence, see Chan and Ahn, 2023).[19] Our interpretation follows the model of Section 2. The higher grades observed during COVID-19 ($\lambda > 0$) may reflect a reduction in the opportunity cost of studying (Figure 2). In the model, this lowers the marginal benefit of cheating and encourages greater study effort. Although we lack direct evidence on cheating, improved performance is consistent with students working harder, and — crucially — our network results below provide no indication of illicit collaboration. A

[19] Although many studies document adverse effects of pandemic restrictions, Hansen et al. (2024) show that suicides among high school students in the United States fell at the onset of COVID-19 and rose upon returning to in-person schooling, likely due to changes in bullying exposure. While this mechanism is unlikely in our setting, it highlights that the pandemic could have had unexpected positive effects.

Table 2. Effects on Grades with Network Quantity, All Undergraduates

		(a)	(b)	(c)	(d)	(e)	(f)	(g)
H_{imdt}	$[\gamma]$	0.248***	0.248***	0.248***	0.249***	0.248***	0.260***	0.304***
		(0.036)	(0.036)	(0.036)	(0.036)	(0.036)	(0.042)	(0.047)
C_t	$[\lambda]$	0.071***	0.071***	0.072***	0.074***	0.072***	0.069***	0.060**
		(0.019)	(0.019)	(0.019)	(0.020)	(0.021)	(0.020)	(0.021)
$C_t \times H_{imdt}$	$[\beta]$	0.174***	0.174***	0.174***	0.173***	0.174***	0.172***	0.178***
		(0.037)	(0.037)	(0.037)	(0.037)	(0.037)	(0.044)	(0.050)
$N_{imdt}^{\{m\}}$	$[\theta^{\{m\}}]$		0.126*		0.135*		0.136*	
			(0.061)		(0.066)		(0.064)	
$N_{idt}^{\{d\}}$	$[\theta^{\{d\}}]$			0.418*		0.419*		0.405*
				(0.203)		(0.205)		(0.201)
$C_t \times N_{imdt}^{\{m\}}$					−0.023		0.020	
					(0.069)		(0.056)	
$C_t \times N_{idt}^{\{d\}}$						−0.005		0.023
						(0.105)		(0.120)
$C_t \times H_{imdt} \times N_{imdt}^{\{m\}}$							0.037	
							(0.061)	
$C_t \times H_{imdt} \times N_{idt}^{\{d\}}$								0.053
								(0.068)
R^2		0.285	0.285	0.285	0.285	0.285	0.286	0.286
Observations		184,354	184,354	184,354	184,354	184,354	184,354	184,354

Notes: The variables on the left-hand side of the table are defined in equation (3). In brackets, we show the corresponding parameters as mentioned in the text. All regressions include age and age squared, as well as course, department, semester, department×high school, semester×high school, department×semester, and student fixed effects. Specifications (d) and (f) also include the term $H_{imdt} \times N_{imdt}^{\{m\}}$ and specifications (e) and (g) also include the term $H_{imdt} \times N_{idt}^{\{d\}}$; their estimates are not reported for simplicity. Standard errors clustered at the student level are in parentheses. 'Observations' refers to the number of student-semester-course observations.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

complementary explanation is a change in assessment practices. Lecturers across UoI could have adopted a more lenient assessment process during the pandemic, even though anecdotal evidence suggests exams themselves were not made easier.[20] Higher grades in home exams ($\gamma > 0$) are consistent with the predictions in Figure 3 that home exams lower the risk of detection, allowing students to collaborate when solving exam questions.

A plausible explanation of the positive β (interaction) coefficient is the abrupt change in exam setting in the 2020 Spring semester: many home exams during COVID were broadly adapted from previous in-person exams, whereas pre-pandemic home exams were purpose built for that format. The ability to use notes and textbooks on exams originally designed

[20]There is also no indication that students selected into easier elective courses when $C_t = 1$. Course choice is limited in the early years of study at UoI, making this channel implausible.

for invigilated settings effectively increased the marginal benefit of cheating (Figure 1), raising grades as long as the marginal-cost-of-studying curve is not too flat.[21] Several other channels could also account for this positive interaction effect. First, assessment design may have shifted toward less time-pressured or more open-ended questions, disproportionately benefiting take-home formats. Second, instructors may have applied more lenient grading in take-home contexts, either because of monitoring challenges or a general relaxation of standards during remote instruction. However, as mentioned already, anecdotal evidence does not support this mechanism. Third, strategic grade inflation, motivated by institutional expectations of flexibility and equity, may have affected take-home exams more than other assessments. Fourth, take-home exams during the pandemic may have reduced test anxiety, thereby improving performance. Any combination of these factors may explain why grades on home exams were systematically higher during the pandemic than in other periods.

In columns (b) and (c), we examine the role of network quantity, by adding $N^{\{m\}}$ and $N^{\{d\}}$, respectively. Relative to column (a), the estimates of γ, λ, and β are unchanged. Both network measures are positively and significantly associated with grades ($p < 0.05$). For $N^{\{m\}}$, moving from a course with no secondary-school peers to one in which all course-mates share the focal student's high-school background raises grades by about one-eighth of a point, even after controlling for individual fixed effects. For the department-level network measure, the $\theta^{\{d\}}$ estimate is 0.42, which corresponds to an average grade increase of two-fifths of a point.[22] Overall, social identity among high-school peers appears to matter more through departmental connectedness than through course-level ties, although both channels exert small positive effects on performance.[23] We return to this point in subsection 5.2.

Before turning to the remaining estimates, it is worth stressing that we tested whether network effects are stronger for first-year students, who might initially rely more heavily on familiar high-school connections before forming new networks. This could be especially true for students who started university at the outbreak of the pandemic (Sigurdardottir et al., 2023). The results (not shown) provide no support for this hypothesis: the benefits of secondary-school networks are similar across all years of study, regardless of students' entry cohort.

Columns (d) and (e) of Table 2 examine whether collaboration under pandemic restrictions (CPR) operated through interactions between C_t and the network measures, $N^{\{m\}}$

[21] If the marginal costs were flat, students would simply have studied less, anticipating access to notes and textbooks at home.

[22] Note that a one-standard-deviation increase in N, which roughly corresponds to a doubling of the network size for both definitions, leads to a grade increase of approximately 0.02 points for $N^{\{m\}}$ and 0.04 for and $N^{\{d\}}$, respectively.

[23] As mentioned in Section 3, alternative specifications with different sets of $\{FEs\}$ yields broadly similar estimates of γ, λ, β, $\theta^{\{m\}}$, and $\theta^{\{d\}}$.

and $N^{\{d\}}$. In both cases, all other coefficients remain stable. The interaction terms are small and statistically indistinguishable from zero, as are the unreported $H \times N$ interactions. Thus, while larger networks generally improve academic performance (i.e., both $\theta^{\{m\}}$ and $\theta^{\{d\}}$ remain positive and significant), network size does *not* differentially affect grades when students take unproctored home exams or during the pandemic.

To strengthen this evidence, we add the triple interactions of $C \times H \times N^m$ and $C \times H \times N^d$ in the final two columns. Once again, all previous results hold, and the triple interaction estimates are statistically insignificant and economically negligible. These findings make it unlikely that CPR among high-school peers, either within courses or within departments, played a meaningful role. In short, students with larger peer networks earned slightly higher grades overall but did not gain an additional advantage during the COVID-19 take-home exam period. Consistent with Figure 2, lower opportunity costs of studying during COVID-19 were compounded by a lower marginal benefit of cheating, even though detection risk was minimal. The responses highlighted in Figures 3 and 4, where networks would amplify cheating incentives, seem less plausible. This evidence aligns with Chan and Ahn (2023), who finds that performance on unproctored exams can improve without cheating, possibly because students experience lower anxiety when taking uninvigilated online exams at home, a view compatible with our model in Section 2.

Our results also echo the arguments of Gächter and Schulz (2016), who emphasize the role of cultural values in shaping the prevalence of rule violations (PRV, an index based on country-specific information about corruption, tax evasion, and fraudulent politics). Countries with low PRV, such as Sweden and the United Kingdom, exhibit stronger intrinsic honesty. Although Iceland is not included in their sample, its cultural and institutional similarity to these countries suggests that strong intrinsic honesty may also characterize Icelandic undergraduates. In a small, tightly connected country with low PRV, the expected costs of being caught cheating may be high even when formal detection probabilities are low.

We close with two robustness exercises. First, to validate our network measures, we construct 'random' networks by matching each student to peers drawn randomly from other high schools, either in the same course or the same department. These random networks have no significant effect on grades and do not interact significantly with home-exam or pandemic indicators (the results are not reported for brevity, but are available from the authors). This supports the interpretation that our measured networks capture meaningful peer links rather than mechanical groupings.

Second, to probe further for potential CPR, we build student cells defined by high school, department, and semester, compute the within-cell k standard deviation of grades, $\mathrm{SD}(G)_k$, and regress $\mathrm{SD}(G)_k$ on the COVID-19 indicator, C_t. Widespread CPR would be expected to compress grades and reduce $\mathrm{SD}(G)_k$. We find no evidence of such compression. Although alternative mechanisms (e.g., COVID-era grading policies) could mask CPR, the absence of a detectable reduction in dispersion provides little support for systematic network-driven misconduct during take-home exams.[24]

5.2 The Role Played by Network Quality

We investigate the role of network quality by incorporating $M^{\{m\}}$ and $M^{\{d\}}$ into two variants of equation (3). In the first variant, we replicate the analysis in Table 2 but replace network quantity with network quality measures. For ease of exposition, we omit the estimates from the last two columns of Table 2, as the additional interaction terms are always insignificant. In the second variant, we include both quantity and quality measures simultaneously, along with their interaction.

Table 3. Effects on Grades with Network Quality, All Undergraduates

		(a)	(b)	(c)	(d)
H_{imdt}	$[\gamma]$	0.264***	0.259***	0.263***	0.262***
		(0.044)	(0.038)	(0.044)	(0.038)
C_t	$[\lambda]$	0.031	0.081***	0.032	0.082***
		(0.028)	(0.020)	(0.030)	(0.028)
$C_t \times H_{imdt}$	$[\beta]$	0.188***	0.160***	0.191***	0.163***
		(0.045)	(0.039)	(0.045)	(0.039)
$M^{\{m\}}_{imdt}$	$[\zeta^{\{m\}}]$	0.042***		0.037***	
		(0.010)		(0.011)	
$M^{\{d\}}_{idt}$	$[\zeta^{\{d\}}]$		0.019		0.026
			(0.015)		(0.016)
$C_t \times M^{\{m\}}_{imdt}$				0.014	
				(0.013)	
$C_t \times M^{\{d\}}_{idt}$					–0.019
					(0.014)
R^2		0.296	0.293	0.296	0.293
Observations		122,459	164,293	122,459	164,293

Notes: The variables on the left-hand side of the table are defined in equation (3). In brackets, we show the corresponding parameters as mentioned in the text. All regressions include age and age squared, as well as course, department, semester, department×high school, semester×high school, department×semester, and student fixed effects. 'Observations' refers to the number of student-semester-course observations.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3 reports the results of the first exercise. Across all specifications, the γ and

[24] The absence of compression is also inconsistent with widespread, uniform grade leniency. Further research is needed to fully assess this issue.

β estimates closely match their counterparts in Table 2 in both magnitude and statistical significance. In particular, the estimated 0.16–0.19 grade boost for home exams during pandemic restrictions (as evidenced by the positive estimates of β) confirms our earlier finding that the pandemic likely reduced the opportunity cost of studying. A notable difference emerges for the direct COVID-19 effect, λ, which becomes slightly larger when we use the department-based quality network measure (columns (b) and (d)), whereas it becomes smaller and statistically insignificant when $M^{\{m\}}$ is used.

Turning to the network estimates, we find that having secondary school peers in the same course with one additional average high-school mark (which corresponds to a 1.5 standard deviation increase) yields a small but statistically significant 0.04-point improvement in grades ($p < 0.001$, column (a)). By contrast, a one-unit increase in $M^{\{d\}}$ produces a small and statistically insignificant effect (column (b)).[25] These results indicate that course-level, rather than department-level, peer quality is more salient for academic performance. Finally, the last two columns of Table 3 — and the insignificant $C \times H \times M$ effects (not shown) — suggest that illicit collaboration among peers taking home exams was negligible during the pandemic restriction period.

The second exercise estimates the joint effects of network quantity and quality, along with their interactions. The results are in Table A.4. The estimates of γ, λ, and β are consistent with those reported in Tables 2 and 3. When considering both measures together (column (a)), the magnitude of $\zeta^{\{m\}}$ (quality) mirrors that of $\zeta^{\{d\}}$ (quantity), but only the former is statistically significant ($p < 0.001$). Thus, course-level peer quality appears more important for performance than the number of peers. Column (b) shows that neither department-level quantity nor quality exerts any significant influence on G.

Columns (c) and (d) add quantity–quality interactions. These terms are small and statistically insignificant, indicating no complementarity between peer quality and peer quantity in grade production. Consistent with the estimates in the first two columns, the number of peers — whether at the department or course level — has no detectable impact on grades. The quality of peers, however, does matter: an additional high-school mark among peers in the same course (column (c)) or department (column (d)) is associated with a modest 0.04-point increase in grades ($p < 0.05$).

The final two columns assess whether CPR played a role or not. We find no evidence of increased collaboration during the pandemic: all network-specific interactions with the COVID-19 indicator, C_t, are small and statistically indistinguishable from zero. Including

[25] Our analysis cannot reliably control for ordinal peer ability rank effects (e.g., Elsner and Isphording, 2017; Murphy and Weinhardt, 2020). Notably, Bertoni and Nisticò (2023) show that the effect of mean peer ability becomes much larger once rank is included as a control.

these interactions does not materially alter the ζ^m and ζ^d estimates, both of which remain small but statistically significant. Nor does it materially alter the peer-quality estimates.

Piecing together the evidence from Tables 2, 3, and A.4, we conclude that students gained additional marks from take-home exams during the pandemic, reinforcing the already positive effect of home exams off pandemic. This likely reflects lower opportunity costs of studying under pandemic restrictions, consistent with the theoretical predictions of the model in Section 2 (see Figure 2). We find no evidence of a grade penalty associated with COVID-19, although the positive impact is generally modest and sometimes statistically insignificant. Sharing courses and departments with more and/or better high-school peers yields small but meaningful grades, which may reflect the influence of positive role models. However, these connections did not become more valuable during the pandemic, suggesting that academically dishonest collaboration was absent or very limited.

5.3 Heterogeneity by Prior Academic Performance

We next conduct an exercise that helps to understand the mechanisms behind the results in the previous subsection. Specifically, we distinguish between high-performing and low-performing students, and then between men and women. The rationale is that home exams, online instruction, and peer networks may affect these groups differently. Low-performing students may be more dependent on in-person teaching and supervision, and similar considerations may apply to men, who on average underperform women and may face greater challenges in maintaining self-discipline in online learning environments. Conversely, these same groups may benefit more from supportive peer networks during the pandemic.

We begin by separating students whose high-school marks are above the sample median from those whose marks are below. The corresponding estimates are reported in Table 4. Four findings stand out. First, students with lower prior performance experience larger gains from both take-home exams and the COVID-19-induced shift to remote assessment. The γ estimate on H_{imdt} increases from about 0.25–0.28 among higher-performance undergraduates to roughly 0.29–0.35 among their lower ability counterparts. The β estimate on the interaction term $C_t{\times}H_{imdt}$ rises five-fold in columns (a)–(e), from about 0.05–0.06 to 0.32–0.33, and it doubles in the remaining columns (f)–(g), from 0.12–0.14 to 0.27–0.31. Taken together, these differences imply that weaker students enjoyed an additional one-third of a grade premium during the pandemic, consistent with lower opportunity cost of studying and reduced effectiveness of conventional monitoring in take-home settings. However, this pattern could also reflect differential exposure to grade leniency. Specifically, lower-performing students may have had more scope for upward grade movements due to ceiling and floor effects, or may have benefited disproportionately from relaxed grading practices aimed at minimizing academic disruption during the pandemic. These possibilities suggest that the heterogeneous effects we observe need not reflect differences in misconduct or effort alone but could also arise from differential susceptibility to institutional responses during the crisis.

Second, for specifications (d)–(g) (i.e., once N and $C \times N$ are included), C_t is insignificant for high ability students, but turns statistically significant ($p < 0.01$) for low-ability students, adding at least one-tenth of a grade point to their average grade. This pattern again suggests that students with weaker prior achievement were better positioned to translate the "extra" study time generated by lockdown measures into higher grades. This distinction is central to the broader interpretation of our findings. If the primary driver were increased leniency or policy-induced shifts in grading standards, then the observed premium would reflect institutional responses rather than differences in student adaptation or the format of take-home exams. While our evidence does not allow us to fully disentangle these channels, this possibility warrants further research.

Third, the network coefficients $\theta^{\{m\}}$ and $\theta^{\{d\}}$ are much larger for the low-performance sample. For example, moving from a department where no one is part of the focal student's network to a department where everyone is leads to a rise of approximately one full grade point for low performance students, compared with a statistically insignificant gain of three-fifth of a grade for the high ability group (column (c) in Table 4). When the network measures are interacted with C and $C \times H$, high ability students do not seem to gain much. Only in column (g) does a larger departmental level network translate into a two-fifth of-a point increase in G during the pandemic. Overall, peer networks for high-ability undergraduates do not seem to have had a substantially different impact on grades during the pandemic and generally did not play much of a differential role when exams were unproctored and taken at home.

The pattern is markedly different for low-ability students. Although they generally benefit from larger course-level networks, this advantage was fully offset during the lockdown restrictions, likely because remate instruction curtailed the in-person interactions through which these networks operate (columns (d) and (f)). A similar pattern is found for department-level networks, but in this case the COVID-19 penalty is not large enough to eliminate the substantial network premium, $\theta^{\{d\}}$. This means that, off-pandemic, a low-ability student would gain 1.37 grade points when moving from a department with no connected peers to one in which all peers are part of their high-school network (columns (e) and (g)). On pandemic, however, the gain shrinks to about 0.4 points (=1.37–0.96).

The triple interactions $C_t \times H_{imdt} \times N$ in columns (f) and (g) remain statistically insignificant for both high- and low-performance groups, reinforcing the view that larger peer networks enhance performance primarily through legitimate collaboration and role-model

Table 4. Effects on Grades with Network Quantity, by Performance Level

		(a)	(b)	(c)	(d)	(e)	(f)	(g)
A. High-performance students								
H_{imdt}	$[\gamma]$	0.248***	0.248***	0.249***	0.277***	0.330***	0.220*	0.259**
		(0.070)	(0.070)	(0.070)	(0.072)	(0.073)	(0.088)	(0.095)
C_t	$[\lambda]$	0.031	0.031	0.032	0.031	–0.000	0.028	–0.006
		(0.039)	(0.039)	(0.039)	(0.041)	(0.043)	(0.041)	(0.043)
$C_t \times H_{imdt}$	$[\beta]$	0.054	0.055	0.053	0.055	0.057	0.118	0.136
		(0.072)	(0.072)	(0.072)	(0.072)	(0.072)	(0.091)	(0.099)
$N^{\{m\}}_{imdt}$	$[\theta^{\{m\}}]$		0.029		0.074		0.061	
			(0.120)		(0.129)		(0.130)	
$N^{\{d\}}_{idt}$	$[\theta^{\{d\}}]$			0.629		0.610		0.580
				(0.374)		(0.379)		(0.380)
$C_t \times N^{\{m\}}_{imdt}$					0.003		0.040	
					(0.134)		(0.137)	
$C_t \times N^{\{d\}}_{idt}$						0.347		0.411*
						(0.192)		(0.200)
$C_t \times H_{imdt} \times N^{\{m\}}_{imdt}$							–0.764	
							(0.666)	
$C_t \times H_{imdt} \times N^{\{d\}}_{idt}$								–0.982
								(0.839)
R^2		0.319	0.319	0.319	0.319	0.319	0.319	0.319
Observations		47,490	47,490	47,490	47,490	47,490	47,490	47,490
B. Low-performance students								
H_{imdt}	$[\gamma]$	0.292***	0.292***	0.291***	0.312***	0.318***	0.353***	0.322**
		(0.075)	(0.075)	(0.075)	(0.077)	(0.078)	(0.089)	(0.103)
C_t	$[\lambda]$	0.053	0.053	0.054	0.116**	0.128**	0.122**	0.129**
		(0.038)	(0.038)	(0.038)	(0.043)	(0.045)	(0.043)	(0.046)
$C_t \times H_{imdt}$	$[\beta]$	0.333***	0.332***	0.333***	0.315***	0.315***	0.265**	0.310**
		(0.077)	(0.077)	(0.077)	(0.078)	(0.078)	(0.095)	(0.109)
$N^{\{m\}}_{imdt}$	$[\theta^{\{m\}}]$		0.176		0.454*		0.476*	
			(0.184)		(0.194)		(0.196)	
$N^{\{d\}}_{idt}$	$[\theta^{\{d\}}]$			0.964*		1.372**		1.375**
				(0.491)		(0.500)		(0.502)
$C_t \times N^{\{m\}}_{imdt}$					–0.770**		–0.843***	
					(0.235)		(0.248)	
$C_t \times N^{\{d\}}_{idt}$						–0.955**		–0.961**
						(0.303)		(0.317)
$C_t \times H_{imdt} \times N^{\{m\}}_{imdt}$							0.748	
							(0.819)	
$C_t \times H_{imdt} \times N^{\{d\}}_{idt}$								0.081
								(1.185)
R^2		0.333	0.333	0.333	0.333	0.333	0.333	0.333
Observations		43,008	43,008	43,008	43,008	43,008	43,008	43,008

Notes: 'High performance' identifies students whose high school mark is above the median; 'Low performance' identifies students whose high school mark is below (or at) the median. 'Observations' refers to the number of student-semester-course observations. For other details, see the notes to Table 2.
$^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

effects rather than through illicit coordination during remote, unproctored exams. However, our earlier conclusion remains: on or off pandemic, lower-ability undergraduates gain more from networks than higher-ability students.

Taken together, the evidence in Table 4 sheds light on the mechanisms outlined in Section 2. Students with weaker prior knowledge (proxied by lower high school marks) appear to (i) benefit disproportionately when assessment constraints are relaxed and unproctored exams become widespread, and (ii) rely more heavily on familiar peer structures when the opportunity cost of effort falls, as during the lockdown period. In contrast, stronger students exhibit muted responses, consistent with a lower marginal return to additional study time and reduced dependence on high-school networks once at university. This heterogeneity has potential implications for the way UoI (and possibly similar institutions) might support lower-ability students, for example by facilitating early social integration with familiar peers.

5.4 Are There Gender Differences?

A large body of work shows that, over the past 25 years, women tend to outperform men in higher education across a range of indicators, including enrollment, completion rates, and grades (e.g., Goldin et al., 2006; Fortin et al., 2015; Blau and Kahn, 2017; Francesconi and Parey, 2018). At the same time, several studies have documented that the mental-health burden of pandemic restrictions fell disproportionately on women in the general population, though not necessarily among university students (e.g., Etheridge and Spantig, 2020; Proto and Quintana-Domeque, 2021; Adams-Prassl et al., 2022). Table 5 reports our gender-specific estimates. For brevity, the coefficients on $H \times N$ and $C \times H \times N$ are omitted, as they are all small and statistically insignificant.

Both women and men benefited from take-home exams, gaining 0.23 and 0.26 grade points (column (a) and (c), respectively). However, only female undergraduates experienced an additional premium of about one-tenth of a point during the period affected by COVID-19 restrictions. Although women may have faced higher psychological costs from the restrictions, female students may also have been better able to deploy or adapt the skills required for distance learning and online exams, such as sustained motivation, effective time management, flexibility, and conscientiousness. Both genders earned statistically significantly higher grades in take-home exams during the pandemic, with an average increase of 0.18 points for women and 0.17 points for men.

For men, both θ^m and θ^d are small and statistically insignificant. For women, θ^m is likewise insignificant, but departmental-level network quantity does matter, suggesting that women benefited substantially from having a greater number of high-school peers within their department. This departmental network premium amounts to roughly two-thirds of a grade point and, as column (d) indicates, did not intensify during the pandemic.

Taken together, the estimates imply that women's grade gains reflect two components: an larger take-home-exam premium (0.5 points vs. 0.43 for men) and a sizable, pandemic-invariant department-level network premium. For a woman with the average baseline grade of 7.4, these combined effects imply a total increase of approximately 1.15 points, a substantial 15% improvement in performance. The corresponding gain for men is 0.43 points (6%), which is statistically significantly smaller.

These gender-specific results are broadly confirmed when combining network quality and

Table 5. Effects on Grades for Male and Female Undergraduates

		Male undergraduates		Female undergraduates	
		(a)	(b)	(c)	(d)
H_{imdt}	$[\gamma]$	0.264***	0.265***	0.231***	0.231***
		(0.067)	(0.067)	(0.042)	(0.042)
C_t	$[\lambda]$	0.030	0.047	0.101***	0.085***
		(0.039)	(0.040)	(0.023)	(0.024)
$C_t \times H_{imdt}$	$[\beta]$	0.166*	0.162*	0.181***	0.186***
		(0.069)	(0.069)	(0.043)	(0.043)
$N_{imdt}^{\{m\}}$	$[\theta^{\{m\}}]$	0.150		0.160	
		(0.118)		(0.085)	
$N_{idt}^{\{d\}}$	$[\theta^{\{d\}}]$		0.053		0.638**
			(0.368)		(0.245)
$C_t \times N_{imdt}^{\{m\}}$		0.010		-0.034	
		(0.118)		(0.085)	
$C_t \times N_{idt}^{\{d\}}$			0.206		0.121
			(0.182)		(0.127)
R^2		0.271	0.271	0.323	0.325
Observations		66,639	66,639	117,715	117,715

Notes: The variables on the left-hand side of the table are defined in equation (3). All regressions include age and age squared, as well as course, department, semester, department×high school, semester×high school, department×semester, and student fixed effects. 'Observations' refers to the number of student-semester-course observations. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

network quantity measures (see Appendix Tables A.5 and A.6). Men gain about one-tenth of a point from sharing a course with higher-achieving secondary-school peers, while women benefit only when such "star peers" are in the same department. Consistent with earlier findings, women experience sizable gains from larger within-department networks (about two-thirds of a point), whereas men benefit more modestly from larger course-level network (about one-sixth of a point). The absence of interactions between exam type and networks, and the lack of complementarity between peer quality and quantity, reinforce the conclusion that CPR effects and illicit coordination during the pandemic are unlikely drivers of performance, irrespective of gender.

Because women appear to benefit primarily from larger peer groups and men from exposure to high-quality peers, we further refine our network measures by distinguishing own-sex from opposite-sex peers. The estimates are reported in Tables A.7–A.10. They indicate that women gain from denser female networks in their departments and from male peers in their courses, while men benefit from larger female course networks but face penalties when surrounded by many male peers. Both genders benefit from high-quality own-sex peers at close range, underscoring the importance of role-model effects.[26]

6 Conclusion

Using rich administrative data from the University of Iceland, covering most of the undergraduate population in the country, this paper shows that unproctored home exams during COVID-19 increased student grades by about half a point, a roughly 7% premium, on top of the usual positive return to take-home exams already present off-pandemic. Despite widespread disruption, student performance did not deteriorate during the pandemic. A simple effort-and-shirking framework suggests that the reduced opportunity costs of studying during lockdown likely drove these gains.

Peer networks also matter. Having more high-school peers in the same department or course raised grades, particularly for lower-ability students, and exposure to higher-quality peers generated additional benefits consistent with social-learning mechanisms. These effects existed both before and after the pandemic and do not appear to reflect illicit collaboration; rather, they point to peer networks as productive academic assets. Universities could leverage this by fostering structured peer-support groups, especially for students at risk of underperforming. Of course, cheating on take-home online exams could have occurred in different ways, e.g., using unauthorized notes, searching material on the internet, and collaborating with people other than high-school peers, which we cannot explore in our setting.

Heterogeneity by prior achievement is more salient than gender. Students with below-median high-school marks experienced substantially larger gains from take-home exams and from the COVID-19 period: the COVID×home-exam interaction is roughly one-third of a grade for low-ability students, while it is small for high-ability students, and the direct COVID effect is positive only in the low-ability group. Network effects are likewise stronger for lower-ability students off-pandemic, particularly at the department level, while lockdown attenuated these gains; triple interactions remain insignificant. Together, these patterns

[26] Tables A.7 and A.8 show that women's department-level network premium is driven entirely by male peers, yielding gains of 0.7–0.8 points. Male peers in the same course also raise women's grades by 0.58 points, whereas female peers have no significant effect. For men, department-level networks are largely irrelevant, but course-level exposure matters: more male peers reduces grades by 0.33–0.45 points, while more female peers increases them by roughly 0.8 points. Finally, results based on network quality measures (Tables A.9 and A.10) show no department-level effects for either gender. At the course level, men gain about one-tenth of a point from higher-quality male peers, and women benefit, though to a smaller degree, from high-quality female peers.

are consistent with reduced opportunity costs of study time and with networks operating primarily through legitimate support rather than illicit collaboration. By contrast, gender differences are modest: women obtain a small additional premium during COVID and benefit more from department-level network size, while men faced mixed effects depending on the gender composition of their peers. Crucially, we find no gender-specific evidence that the pandemic widened achievement gaps or that peer networks were used for misconduct during remote assessment.

Overall, the experience of UoI undergraduates contrasts with that of their counterparts in some U.S. institutions where online learning hampered performance. Our findings align with recent evidence that unproctored online exams can be an informative and reliable measure of learning (Chan and Ahn, 2023), although the rise of generative AI raises new challenges for remote assessment design going forward (e.g., de Winter, 2024; Susnjak and McIntosh, 2024). Understanding how grading practices may have shifted during COVID-19, whether effects persisted after returning to in-person teaching, and how remote learning shaped later cohorts' outcomes remains an important avenue for future research.

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A Supplementary Material for Online Appendix

A.1 Sample Selection

The table below documents the selection criteria we used to obtain our final estimating sample from the original administrative records of the University of Iceland.

Table A.1. Sample Selection

Reason for sample selection	Observations deleted	Observations
Original sample		*406,551*
Course withdrawal	105,157	
		301,394
Final exam absence (illness)	8,376	
		293,018
Final exam absence (other reasons)	20,601	
		272,417
Missing high school	10,737	
		261,680
Missing gender	253	
		261,427
Course does not have final exam	60,144	
		201,283
Missing university grade	16,929	
		184,354

Notes: The term 'Observations' refers to the number of student-semester-course observations. The value in italics is the size of the original sample in the administrative records of the University of Iceland. The value in bold is the size of the sample used in our estimation analysis.

There are also 20,061 cases for which secondary school grades are missing and would be needed to construct the quality network measure at the departmental level, $M^{\{d\}}$. This leaves us with 164,293 observations for the analysis. Finally, there are 41,834 student-semester-course observations for which secondary school grades are missing and would be needed to construct the quality network measure at the course level, $M^{\{m\}}$. This selection yields a new sample of 122,459 student-semester observations.

A.2 Comparing Our Selected Sample to the Original Sample

Table A.2 documents how the sample characteristics change when moving from the 406,551 original observations in the raw files to the 184,354 observations that form our estimating sample. The table displays means (and standard deviations) for the key variables in our main analysis, reported separately for all enrollments ('Original sample') and for our selected subsample ('Final sample').

Table A.2. Summary Statistics: Comparing Our Selected Sample to the Original Sample

	Original sample (N=406,551)	Final sample (N=184,354)	Difference
Male	0.364	0.361	0.003
G_{imdt} (grade)	4.410 (3.948)	7.306 (1.938)	-2.896^{**}
H_{imdt} (take-home exam)	0.235	0.217	$0.018^{\dagger}$
C_t (COVID-19)	0.398	0.385	$0.013^{\dagger}$
$N_{imdt}^{\{m\}}$ (course network, quantity)	0.080 (0.104)	0.087 (0.114)	$-0.007^{\dagger}$
$N_{idt}^{\{d\}}$ (department network, quantity)	0.069 (0.060)	0.071 (0.065)	-0.002
$M_{imdt}^{\{m\}}$ (course network, quality)[a]	6.898 (2.181)	7.546 (0.666)	-0.648^{**}
$M_{idt}^{\{d\}}$ (department network, quality)[b]	6.767 (2.372)	7.550 (0.549)	-0.783^{**}
Age	26.336 (7.818)	24.829 (6.286)	1.507^{**}
Foreign	0.080	0.079	0.001
Faculty:			
Health Sciences	0.258	0.299	-0.041^{**}
Social Sciences	0.230	0.247	$-0.017^{\dagger}$
Humanities	0.161	0.131	0.030^{*}
Education Sciences	0.129	0.106	0.023^{*}
Engineering and Natural Sciences	0.222	0.217	0.005

Notes: Standard deviations (for continuous variables) in parentheses. For all other details, see the notes to Table 1.

[a] Observations are 272,213 and 122,459 for the original and final samples respectively. Grades for missing high-school marks coded as 0.

[b] Observations are 363,728 and 164,293, for the original and final samples respectively. Grades for missing high-school marks coded as 0.

$^{\dagger}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.01$.

A.3 Summary Statistics by Exam Mode

Table A.3. Summary Statistics by Exam Mode

	On-site exam (N=144,408)	Take-home exam (N=39,946)	Difference
Male	0.361	0.363	−0.002
	(0.480)	(0.481)	
G_{imdt} (grade)	7.215	7.633	−0.418***
	(1.965)	(1.796)	
C_t (COVID-19)	0.234	0.932	−0.698***
	(0.423)	(0.252)	
$N^{\{m\}}_{imdt}$ (course network, quantity)	0.090	0.075	0.016***
	(0.121)	(0.087)	
$N^{\{d\}}_{idt}$ (department network, quantity)	0.076	0.067	0.009***
	(0.070)	(0.059)	
$M^{\{m\}}_{imdt}$ (course network, quality)	7.569	7.467	0.102***
	(0.666)	(0.662)	
$M^{\{d\}}_{idt}$ (department network, quality)	7.571	7.473	0.098***
	(0.545)	(0.555)	
Age (years)	24.610	25.620	−1.010***
	(6.131)	(6.756)	
Foreign	0.075	0.094	−0.019***
	(0.264)	(0.291)	
Faculty:			
Health Sciences	0.335	0.167	0.168***
	(0.472)	(0.373)	
Social Sciences	0.211	0.377	−0.166***
	(0.408)	(0.485)	
Humanities	0.123	0.159	−0.036***
	(0.329)	(0.366)	
Education Sciences	0.096	0.141	−0.045***
	(0.295)	(0.348)	
Engineering and Natural Sciences	0.234	0.156	0.078***
	(0.423)	(0.363)	

Notes: For details, see the notes to Table 1 in the text.
$^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

A.4 Other Results

Table A.4. Effects on Grades with Network Quality and Quantity, All Undergraduate Students

		(a)	(b)	(c)	(d)	(e)	(f)
H_{imdt}	$[\gamma]$	0.264***	0.259***	0.264***	0.259***	0.262***	0.258***
		(0.044)	(0.038)	(0.044)	(0.038)	(0.045)	(0.039)
C_t	$[\lambda]$	0.032	0.081***	0.031	0.081***	0.030	0.069***
		(0.028)	(0.020)	(0.028)	(0.020)	(0.029)	(0.022)
$C_t \times H_{imdt}$	$[\beta]$	0.188**	0.160***	0.188***	0.160***	0.187***	0.161***
		(0.045)	(0.039)	(0.045)	(0.039)	(0.045)	(0.039)
$M_{imdt}^{\{m\}}$	$[\zeta^{\{m\}}]$	0.042***		0.038**		0.040***	
		(0.010)		(0.013)		(0.013)	
$M_{idt}^{\{d\}}$	$[\zeta^{\{d\}}]$		0.018		0.043*		0.042*
			(0.015)		(0.021)		(0.020)
$N_{imdt}^{\{m\}}$	$[\theta^{\{m\}}]$	0.045		–0.040		0.032	
		(0.099)		(0.867)		(0.091)	
$N_{idt}^{\{d\}}$	$[\theta^{\{d\}}]$		0.166		0.409		0.231
			(0.225)		(1.229)		(0.215)
$N_{imdt}^{\{m\}} \times M_{imdt}^{\{m\}}$				0.057			
				(0.109)			
$N_{idt}^{\{d\}} \times M_{idt}^{\{d\}}$					–0.108		
					(0.295)		
$C_t \times M_{imdt}^{\{m\}}$						–0.010	
						(0.012)	
$C_t \times M_{idt}^{\{d\}}$							–0.019
							(0.016)
$C_t \times N_{imdt}^{\{m\}}$						–0.011	
						(0.037)	
$C_t \times N_{idt}^{\{d\}}$							–0.019
							(0.104)
R^2		0.296	0.293	0.297	0.293	0.297	0.295
Observations		122,459	164,293	122,459	164,293	122,459	164,293

Notes: The variables on the left-hand side of the table are defined in equation (3). In brackets, we show the corresponding parameters as mentioned in the text. All regressions include age and age squared, as well as course, department, semester, department×high school, semester×high school, department×semester, and student fixed effects. 'Observations' refers to the number of student-semester-course observations.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.5. Effects on Grades with Network Quality and Quantity, Female Students

	(a)	(b)	(c)	(d)	(e)	(f)	(g)	(h)
H_{imdt}	0.235***	0.234***	0.233***	0.232***	0.235***	0.232***	0.233***	0.234***
	(0.051)	(0.043)	(0.044)	(0.041)	(0.054)	(0.044)	(0.051)	(0.043)
C_t	0.086*	0.112***	0.086*	0.110***	0.077*	0.108***	0.079*	0.111***
	(0.037)	(0.022)	(0.038)	(0.024)	(0.036)	(0.029)	(0.039)	(0.032)
$C_t \times H_{imdt}$	0.200***	0.174***	0.202***	0.175***	0.200**	0.177***	0.202***	0.173*
	(0.053)	(0.045)	(0.054)	(0.044)	(0.056)	(0.043)	(0.053)	(0.044)
$M_{imdt}^{\{m\}}$	0.015		0.014		0.012		0.013	
	(0.103)		(0.099)		(0.104)		(0.101)	
$M_{idt}^{\{d\}}$		0.037*		0.043*		0.039*		0.041*
		(0.018)		(0.018)		(0.018)		(0.018)
$N_{imdt}^{\{m\}}$			0.020		0.021		0.021	
			(0.036)		(0.039)		(0.037)	
$N_{idt}^{\{d\}}$				0.651**		0.645**		0.632***
				(0.164)		(0.162)		(0.157)
$N_{imdt}^{\{m\}} \times M_{imdt}^{\{m\}}$					0.026			
					(0.069)			
$N_{idt}^{\{d\}} \times M_{idt}^{\{d\}}$						0.024		
						(0.075)		
$C_t \times M_{imdt}^{\{m\}}$							–0.015	
							(0.018)	
$C_t \times M_{idt}^{\{d\}}$								–0.023
								(0.018)
$C_t \times N_{imdt}^{\{m\}}$							–0.025	
							(0.040)	
$C_t \times N_{idt}^{\{d\}}$								–0.011
								(0.035)
R^2	0.338	0.337	0.338	0.337	0.339	0.337	0.340	0.338
Observations	78,128	104,810	78,128	104,810	78,128	104,810	78,128	104,810

Notes: The variables on the left-hand side of the table are defined in equation (3). In brackets, we show the corresponding parameters as mentioned in the text. All regressions include age and age squared, as well as course, department, semester, department×high school, semester×high school, department×semester, and student fixed effects. 'Observations' refers to the number of student-semester-course observations.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.6. Effects on Grades with Network Quality and Quantity, Male Students

	(a)	(b)	(c)	(d)	(e)	(f)	(g)	(h)
H_{imdt}	0.302***	0.285***	0.303***	0.286***	0.301***	0.284***	0.302***	0.284***
	(0.082)	(0.071)	(0.083)	(0.074)	(0.080)	(0.071)	(0.084)	(0.070)
C_t	0.002	0.023	0.003	0.022	0.004	0.023	0.003	0.024
	(0.056)	(0.039)	(0.055)	(0.039)	(0.058)	(0.040)	(0.055)	(0.039)
$C_t \times H_{imdt}$	0.177*	0.149*	0.175*	0.148*	0.177*	0.149*	0.175*	0.151*
	(0.084)	(0.073)	(0.082)	(0.072)	(0.085)	(0.072)	(0.085)	(0.074)
$M_{imdt}^{\{m\}}$	0.084***		0.086***		0.082***		0.083***	
	(0.017)		(0.019)		(0.021)		(0.018)	
$M_{idt}^{\{d\}}$		0.005		0.006		0.009		0.007
		(0.028)		(0.029)		(0.028)		(0.030)
$N_{imdt}^{\{m\}}$			0.154*		0.155*		0.156*	
			(0.063)		(0.066)		(0.068)	
$N_{idt}^{\{d\}}$				0.027		0.025		0.028
				(0.090)		(0.094)		(0.092)
$N_{imdt}^{\{m\}} \times M_{imdt}^{\{m\}}$					0.107			
					(0.144)			
$N_{idt}^{\{d\}} \times M_{idt}^{\{d\}}$						–0.208		
						(0.243)		
$C_t \times M_{imdt}^{\{m\}}$							0.009	
							(0.022)	
$C_t \times M_{idt}^{\{d\}}$								0.013
								(0.029)
$C_t \times N_{imdt}^{\{m\}}$							0.015	
							(0.059)	
$C_t \times N_{idt}^{\{d\}}$								–0.014
								(0.083)
R^2	0.276	0.271	0.259	0.256	0.260	0.259	0.259	0.257
Observations	44,331	59,483	44,331	59,483	44,331	59,483	44,331	59,483

Notes: The variables on the left-hand side of the table are defined in equation (3). In brackets, we show the corresponding parameters as mentioned in the text. All regressions include age and age squared, as well as course, department, semester, department×high school, semester×high school, department×semester, and student fixed effects. 'Observations' refers to the number of student-semester-course observations.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.7. Effects on Grades for Female Undergraduates with Gender-Specific Network Measures

		(a)	(b)	(c)	(d)
H_{imdt}	[γ]	0.229***	0.232***	0.231***	0.231***
		(0.042)	(0.042)	(0.042)	(0.042)
C_t	[λ]	0.099***	0.095***	0.099***	0.095***
		(0.022)	(0.021)	(0.022)	(0.021)
$C_t \times H_{imdt}$	[β]	0.183***	0.183***	0.183***	0.183***
		(0.043)	(0.043)	(0.043)	(0.043)
$N^{\{m\}}$ (own-sex peers)		–0.033		–0.098	
		(0.108)		(0.110)	
$N^{\{d\}}$ (own-sex peers)			0.787**		0.688*
			(0.291)		(0.330)
$N^{\{m\}}$ (opposite-sex peers)				0.578***	
				(0.155)	
$N^{\{d\}}$ (opposite-sex peers)					0.462
					(0.479)
R^2		0.322	0.323	0.322	0.323
Observations		117,442	117,715	117,442	117,715

Notes: The variables on the left-hand side of the table are defined in equation (3). All regressions include age and age squared, as well as course, department, semester, department×high school, semester×high school, department×semester, and student fixed effects. The term 'Observations' refers to the number of student-semester-course observations.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.8. Effects on Grades for Male Undergraduates with Gender-Specific Network Measures

		(a)	(b)	(c)	(d)
H_{imdt}	[γ]	0.262***	0.262***	0.263***	0.262***
		(0.067)	(0.067)	(0.067)	(0.067)
C_t	[λ]	0.029	0.031	0.027	0.031
		(0.038)	(0.037)	(0.038)	(0.037)
$C_t \times H_{imdt}$	[β]	0.165*	0.166*	0.166*	0.165*
		(0.069)	(0.069)	(0.069)	(0.069)
$N^{\{m\}}$ (own-sex peers)		–0.334*		–0.451*	
		(0.167)		(0.169)	
$N^{\{d\}}$ (own-sex peers)			0.659		0.832
			(0.586)		(0.600)
$N^{\{m\}}$ (opposite-sex peers)				0.826***	
				(0.182)	
$N^{\{d\}}$ (opposite-sex peers)					–0.557
					(0.569)
R^2		0.269	0.270	0.271	0.270
Observations		66,499	66,639	66,499	66,639

Notes: The variables on the left-hand side of the table are defined in equation (3). All regressions include age and age squared, as well as course, department, semester, department×high school, semester×high school, department×semester, and student fixed effects. The term 'Observations' refers to the number of student-semester-course observations.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.9. Effects on Grades for Female Undergraduates with Gender-Specific Quality Network Measures

	(a)	(b)	(c)	(d)
H_{imdt}	0.301***	0.242***	0.276***	0.293***
	(0.076)	(0.061)	(0.055)	(0.075)
C_t	0.112**	0.124***	0.110**	0.156***
	(0.032)	(0.030)	(0.033)	(0.037)
$C_t \times H_{imdt}$	0.168**	0.189**	0.180**	0.166**
	(0.053)	(0.063)	(0.058)	(0.051)
$M^{\{m\}}$ (own-sex peers)	0.030*		0.041*	
	(0.015)		(0.019)	
$M^{\{d\}}$ (own-sex peers)		–0.037		0.023
		(0.023)		(0.031)
$M^{\{m\}}$ (opposite-sex peers)			–0.008	
			(0.022)	
$M^{\{d\}}$ (opposite-sex peers)				–0.001
				(0.021)
R^2	0.354	0.356	0.349	0.349
Observations	75,919	76,747	75,919	76,747

Notes: The variables on the left-hand side of the table are defined in equation (3). All regressions include age and age squared, as well as course, department, semester, department×high school, semester×high school, department×semester, and student fixed effects. The term 'Observations' refers to the number of student-semester observations.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.10. Effects on Grades for Male Undergraduates with Gender-Specific Quality Network Measures

	(a)	(b)	(c)	(d)
H_{imdt}	0.276**	0.268**	0.265**	0.279**
	(0.071)	(0.071)	(0.071)	(0.071)
C_t	0.036	–0.026	0.034	–0.021
	(0.053)	(0.047)	(0.053)	(0.047)
$C_t \times H_{imdt}$	0.202*	0.222*	0.204*	0.221*
	(0.094)	(0.094)	(0.095)	(0.093)
$M^{\{m\}}$ (own-sex peers)	0.105***		0.104***	
	(0.023)		(0.029)	
$M^{\{d\}}$ (own-sex peers)		0.034		0.020
		(0.035)		(0.039)
$M^{\{m\}}$ (opposite-sex peers)			0.047	
			(0.029)	
$M^{\{d\}}$ (opposite-sex peers)				0.021
				(0.031)
R^2	0.287	0.292	0.290	0.294
Observations	43,146	43,402	43,146	43,402

Notes: The variables on the left-hand side of the table are defined in equation (3). All regressions include age and age squared, as well as course, department, semester, department×high school, semester×high school, department×semester, and student fixed effects. The term 'Observations' refers to the number of student-semester observations.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$