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Abstract

We evaluate whether large capital grants matter for student outcomes using the timing of grants allocated to Further Education colleges in England for identification. Large capital grants improve facilities and student progression. Improvements in progression are partially explained by changes in student intake – grants increase pre-attainment scores and reduce the share of students that are disadvantaged. However, progression still improves conditional on student intake, suggesting that capital grants have a direct effect on learning outcomes. There are also some effects on enrolment to higher education and on labour market outcomes.

J.E.L. codes: I21; I22; I28

Keywords: capital expenditure; student outcomes; further education colleges.

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1 Introduction

The debate on the relationship between school resources and student outcomes is an old and controversial one (for reviews see, for example, Hanushek, 1989, 1997 and Gibbons and McNally, 2013), although there is less evidence on the effect of capital expenditure. This paper provides new evidence by studying the effect of capital expenditure in Further Education (FE) Colleges in England. These colleges provide post-compulsory schooling education, similar to US Community Colleges. About half of school leavers in England attend FE colleges, though they are generally considered the poor relation of schools and universities, enrolling lower achieving students and with less resources per student (Britton et al. 2019).[1] Capital investment projects in these colleges have the potential to improve educational outcomes for large numbers of disadvantaged students and thus to facilitate social mobility. These colleges also have an important role to play in providing the intermediate and higher technical skills, widely regarded as being in short supply in Britain.

Capital grants account for about 10% of the total FE budget but annual capital spending has declined in recent years – from almost £1 billion a year between 2010 and 2015 to £404 million in 2016/17 (Association of Colleges, 2018). A recent review of post-18 education (Independent Panel Report, 2019) argues that colleges must make substantially more capital expenditure if they are to provide high-quality learning opportunities for their students. As part of their response to such reviews, the government have recently committed to large-scale capital investment to refurbish colleges.[2]

Against this background, we aim to evaluate the impact of previous large-scale college investment programmes on student outcomes. We focus on "young" learners, typically between 16 and 20 years old when first entering college.[3] This is one of the only studies to evaluate the effect of capital expenditure in post-secondary education and to our knowledge is the first study in this literature to make use of longitudinal administrative data on individual-level outcomes with linked data on capital expenditure at the institutional level.

We focus on several educational outcomes: enrolment in a course in college that leads to an upper-secondary level vocational or academic qualification; achievement on these courses; and progression from further education to higher education after college.[4] We also look at labour market outcomes (earnings

[1] The legal school leaving age in England is 16, at which point students complete the national GCSE exams. These are standardised exams, and must include English, math and science. However, most students undertake post-compulsory education between age 16 and 18 and since 2015 all are obliged to undertake some form of education or training up until age 18.

[2] In the 2020 budget, the UK government committed to £1.5 billion over five years to bring the facilities of colleges everywhere in England up to a good level: https://www.gov.uk/government/publications/budget-2020-documents/budget-2020.

[3] We do not consider adult learners, who enter college for retraining or on-the-job training. The kind of learning they undertake tends to be of lower intensity and shorter duration (see Aucejo et al, 2020), which makes educational outcomes hard to measure. In addition, we have no prior attainment data for these adult learners, which means for older learners we cannot control for changes in student intake when considering the impact of investment.

[4] Progression from further education to higher education in the UK is comparable to moving from a two-year degree (associate degree) to a four-year degree (bachelor's) in the US.

and employment). Our empirical strategy takes advantage of two features of the data available and the way the capital expenditure program was implemented. First, we have a rich dataset on student-level outcomes and characteristics, which allows us to control for prior attainment to purge some of the potentially confounding effect of selection on ability. Second, we use the fact that not all capital expenditure programmes are undertaken at the same time to select a suitable group of control students. We select treatment and control groups of colleges that invest in similar large capital expenditure projects (at different times) and examine outcomes for students attending these colleges. We study the impact of capital expenditure programmes undertaken by FE colleges in England over the period from 2006 to 2009. We show results as an event study, which highlights the absence of pre-trends.

We find that large capital grants increase student enrolment on upper-secondary level courses that lead to "good" qualifications (i.e. at Level 3 in the English nomenclature). This matters because less than half of young learners progress to these courses (Hupkau et al. 2017). Level 3 qualifications are associated with positive earnings returns (McIntosh, 2006) and are a pre-requisite to enter university. Conditional on enrolment, large capital grants have no effect on achievement. This is still a good outcome because it suggests more students achieving Level 3 outcomes – enrolments go up with no effect on achievement rates. These effects persist after controlling for the composition of the student intake. Capital expenditure also has a positive effect on enrolment to higher education and on employment, but little or no effect on earnings, at least over the time-period of our data.

There are several reasons why capital expenditure may have these effects. First, substantial capital expenditure on new equipment, laboratories or workshops may improve learning on courses that rely on specific and costly assets (for instance, engineering). Second, better buildings may improve the learning experience. Safe, clean, and appealing learning environments - with no overcrowding, good lighting, heating - could improve concentration and lead to greater student and teacher morale and effort. On the other hand, large capital expenditure projects may be disruptive and positive effects may take time to materialise. As we show, these positive effects also partly reflect changes in the composition of student intake – improving outcomes for colleges that receive grants but not necessarily for the whole system.

The earlier literature on the relationship between capital investment and student achievement used cross-sectional variation in school facility expenditures to measure effectiveness. This literature found mixed results (Hanushek, 1989; Hedges et al. 1994). More recent studies continue to find mixed results using quasi-experimental designs to study the relationship between capital investment in schools and student outcomes, although this is still under-researched compared to the effects of general school expenditure. Much of this literature is from the US and measures capital expenditure at the school district level. Descriptive evidence on how school capital expenditure has evolved in the US is recent (Biasi et al. 2021). All the literature focuses on schools, and test scores are usually pooled across several age groups. Several studies make use of the fact that school capital projects in the US are primarily financed locally through bonds that are repaid by property taxes. Because these bonds need to be approved by voters, these papers can use a regression discontinuity design to compare districts where bond measures narrowly pass to districts where they narrowly fail (Barron (2021), Cellini et al. (2010), Hong and Zimmer (2016), Kogan et al. (2017), Rauscher (2020) and Martorell et al. (2016)). Only Martorell et al. (2016) directly link bond passage to a measure of facility quality, and they find only modest changes in facility conditions. These studies tend to find small effects of district-wide capital expenditure on student achievement in reading or maths, which take some years to emerge. Another branch of this literature investigates the impact of upgrading an entire school district's capital stock. This mainly consists of constructing new schools, involving bigger capital outlays than those studies investigating the effect of bond measures. This literature uses a difference-in-differences setting and includes Goncalves (2015) and Conlin and Thompson (2017) for Ohio, Neilson and Zimmerman (2014) for New Haven, and Lafortune and Schonholzer (2018) for Los Angeles. Evidence for Ohio shows a large disruption effect when building works are undertaken with only small subsequent effects on achievement. The study for New Haven finds large effects for reading proficiency but not for maths whereas the study for Los Angeles finds effects for both maths and English. In both New Haven and Los Angeles, positive effects take time to emerge fully.

There are few studies in the UK that investigate the impact of capital expenditure on student outcomes. Zhang (2014) studies the impact on student outcomes of a large-scale English school improvement programme (Building Schools for the Future) planned to be implemented between 2003 and 2020, but eventually cancelled in 2010. Using a difference-in-differences design, he compares changes in student performance in schools that implemented capital expenditure programmes earlier to those that implemented it later. He finds positive effects of capital programmes on disadvantaged students and no effects for advantaged students.

We make several significant contributions relative to this literature. First, we focus on the effect of capital expenditure for students in post-secondary education - in colleges rather than in schools. The institutions we study - FE Colleges in England - resemble US Community Colleges.[5] They are large institutions catering both for young people and adults, though we look only at young people so we can observe their educational trajectories before and after they attend FE colleges. The focus on this type of college and age group is very different from nearly all the existing literature on the effects of capital expenditure, where the focus is on schools (usually for multiple year groups). A recent exception is Chakrabarti et al (2020), who show that state funding (appropriations) in higher education in the US increases student attainment, particularly in the two-year, Community College sector. The authors identify the causal effects of state appropriations using a shift-share instrument that predicts institutional

[5] Like Community Colleges, these are institutions in which young people (or adults) can pursue vocational education and may eventually transfer to universities (more like four-year colleges) if they want to pursue degree-level education. Unlike Community Colleges, Further Education Colleges enrol students when they are younger (typically at age 16/17) and many students complete their upper secondary education within them. They have a wide range of provision from low-level vocational through to upper secondary through to tertiary qualifications.

level changes from institutional initial levels, interacted with state level changes. Whilst there is no explicit estimation of the attainment impact of capital grants, the authors show that capital appropriations rise with state appropriations, which suggests they may play a role, though the authors attribute the attainment impacts more to increases in academic support, instructional expenditures, and reductions in student teacher ratios. One existing study commissioned by the UK government (Business Innovation and Skills, 2012) estimates the association between capital expenditure and institutional-level outcomes in FE in England. This report finds a small positive association between capital expenditure and student numbers but a negligible association with measures of achievement or student retention. We go beyond this analysis, using micro-level data and a research design aimed at causal inference.

We also build and improve on the studies of capital expenditure in other educational settings that use quasi-experimental designs based on the timing of grant allocations for identification. Unlike literature for the US, we observe capital expenditure at the level of the institution (rather than the district). This is an advantage because we can be sure that students attending colleges receive a given treatment (i.e. it is not dispersed among several institutions). Also, although we focus on major capital projects, this is not the same as construction of new sites, which has been an important component of several studies referenced above. We can also measure outcomes at individual level and follow these students over time in administrative data as they progress in education and into the labour market. To the best of our knowledge, this is the first study in this literature to use such data. We overcome potential problems with fixed effects designs that estimate causal effects from only the timing of events in a panel design (Athey and Imbens, 2021; de Chaisemartin and D'Haultfoeuille, 2020; Goodman-Bacon, 2021; Sun and Abraham, 2020) by using only future-treated colleges as controls for treated colleges, by stacking each cohort of treated colleges with a cohort of these future-treated colleges and by considering treatment effects in a fixed time window either side of grant approval (similar to Martorell et al 2016).

The rest of the paper is organised as follows. Section 2 describes the institutional context. Section 3 describes the data. Section 4 outlines the identification challenges and presents the research design. Section 5 presents the results and Section 6 concludes.

2 Institutional Background

2.1 Further education and qualifications in England

All students take national exams (GCSEs) at age 16 – the end of compulsory schooling, also known as the end of Key Stage 4 (KS4).[6] Upon completing KS4, students decide whether to pursue further education at school, Sixth Form college or FE college. The majority who do academic qualifications (about 45 per cent of a cohort) do so in school or Sixth Form college and study for two years before

[6] Most students take 8 to 10 General Certificate of Education (GCSE) subjects. Some students make also take a small number of exams equivalent to GCSEs.

sitting for A-level qualifications. Those pursuing a vocational route typically go to FE college (about 50 per cent of a cohort). Between the age of 16 and 18, about 70 per cent of students attending FE college are full-time and studying programmes that range from the very general 'Preparation for Life and Work' to more occupationally focussed such as Health, Public Services and Care and Engineering and Manufacturing Technologies (Hupkau and Ventura, 2017). Many qualifications are available, most of which are classified by difficulty level – for example A-levels are Level 3, a GCSE pass is Level 2.

Although some students with good GCSEs choose to go to FE colleges, those with poor GCSEs have little choice. Almost all students with poor GCSEs study at an FE college. Around 60 percent of younger FE-learners undertake low-level vocational qualifications (Level 2 or below). Another 30 per cent do higher-level vocational qualifications (Level 3) and might combine their vocational programmes with A-level study. Level 3 qualifications are pre-requisites to enter university and are also associated with positive labour market returns (see, for instance, McIntosh 2006). There are few tertiary-level courses (Level 4). Progression from low to high levels is problematic, with only about 45 per cent of those studying a Level 2 qualification at age 17 progressing to Level 3 by age 20 (Hupkau et al. 2017). Successful FE-outcomes include enabling students to progress to Level 3, as well as achievement at that level. This informs the outcome measures considered below. We also investigate whether attending a college receiving significant capital expenditure increases enrolment in higher education (typically at university) or has positive effects on employment and earnings.

2.2 Capital expenditure program and matching grants for FE Colleges

FE colleges fund capital expenditure through own means, debt and grants given by central government. Between 2001 and 2010, the Learning and Skills Council (LSC) ran a bid-based programme to fund FE capital expenditure. In 2010, funding responsibilities moved to the Skills Funding Agency (SFA), which ran several capital funding rounds between 2010 and 2014. The LSC scheme encouraged colleges to draw up property strategies and offered funding based on an affordability calculation in which grants topped-up the money colleges could raise from loans, property sales and their own cash. The LSC provided a 35% grant in most cases but, as their own budget increased, contributed a higher percentage for some bigger projects. Each project was evaluated based on educational, property and financial considerations. SFA staff used similar evaluation criteria, but rather than invite individual bids as and when ready, they had a single set of deadlines and scored submitted projects against each other. The aim of the early SFA grants from 2010 was to support smaller projects but the overall budget and hence the size of grants increased between 2013 and 2015.

Figure 1 shows the evolution of total capital project expenditures by FE colleges and the share of government grant contributions to these projects. Government contributions were stable at around 35% up until 2005, after which both the total value of approved projects as well as the government's contributions to these projects rose, up to the year 2009 (the year funding responsibilities were transferred to the SFA). In 2007, a total of 1.1 billion pounds in projects were approved, of which more than 70% were funded by government grants. Many projects approved over this period are of relatively small size and cover mostly health and safety or minor refurbishments of existing buildings. The larger grants involve mainly major refurbishments and the construction of new buildings or campuses. Our analysis focuses on large capital projects only – those in the third and fourth quartile of the grant distribution - because we want to exclude regular maintenance that might not be expected to have much effect on the student experience (and hence on outcomes). As Table 1 shows, when dividing grants into quartiles based on the value of the grant received, projects in the fourth quartile of grants represent 66% of all projects in terms of value, and 80% of the value of total grants approved over the period of analysis. The top 50% of grants (third and fourth quartile) represent 86% of all projects in terms of value, and 93% of the value of all grants.

3 Data

We use college-level datasets on central government grants for capital projects and college accounts, combined with administrative datasets measuring student educational and labour market outcomes.

3.1 Government grants for capital projects

We use data on central government grants provided to fund college capital projects from 2001 to 2015.[7] This covers all grants approved during this period by the LSC (2001 to 2010) and the SFA (2011 to 2015). For each grant we know the project type, approval date, grant amount and total project value.[8] While we do not observe the date construction started, we can observe the associated capital expenditure and changes in fixed asset values from the college's cash flow statement and balance sheet.[9]

3.2 College accounts

We use annual college accounts data, which provide a detailed financial picture.[10] This includes cash flow statements, profit and loss accounts and balance sheet data. The cash flow statements detail capital expenditure on physical assets such as buildings (new or refurbished) and fixed equipment (installations, workshops, and laboratories). The balance sheet data provides the value of fixed assets, a proxy for the value of estates and facilities. It also includes data on the number of teachers employed, expenditure by category (e.g., teachers, administration), and size of the estate (in square meters). The longitudinal nature

[7] This data was provided to us by the Department for Education and the former Department for Business, Innovation and Skills (BIS).
[8] For some grants we also know the completion date of the project. Information on the type of project includes, for example, whether a new building was constructed, existing facilities upgraded or new equipment purchased.
[9] Section 5.1 shows that capital expenditure rises significantly in the year of grant approval, stays high for the next two years before returning to pre-grant approval level.
[10] FE colleges must submit annual financial statements in a harmonized format to the authority responsible for publicly funded learning at FE college (LSC from 2001-2010, SFA from 2011-2016 and Education and Skills Funding Agency from 2017 onwards). See Education and Skills Funding Agency (2017) for more details.

of the college accounts data means we observe capital expenditure and fixed assets in years before and after grant receipt.

3.3 Student-level data

We use data on student educational and labour market outcomes from 2003 to 2017 – the exact years vary by outcome as described below - taken from the Longitudinal Education Outcomes database (LEO). Using a unique person identifier, LEO allows the linking of data from the National Pupil Database (NPD), the Individual Learner Record (ILR), the Higher Education Statistical Agency (HESA), the UK tax authority (Her Majesty's Revenue and Customs, HMRC) and the Department for Work and Pension (DWP).

3.3.1 Prior attainment (KS4) and student characteristics from the NPD

The NPD covers all pupils in England. It provides us with data on results for KS4 exams taken at age 16 – the end of compulsory schooling.[11] As students enter FE college after compulsory schooling, we use this to measure prior attainment. We also use NPD data on student characteristics – gender, age, ethnicity, language spoken at home, and eligibility for free school meals during compulsory education - a measure of economic disadvantage.

3.3.2 Student Learning Outcomes (Levels 3 and 4)

The ILR is a student-level dataset providing information on all state funded learning undertaken by students over the age of 16 in FE Colleges and other FE providers. For each student, we have detailed information on qualifications enrolled in, including qualification level, subject area, date of and duration of enrolment, whether the qualification was achieved and at which provider (FE college or other).[12] This allows us to link student-level outcomes to grant and capital expenditure data at the college level.

We focus on enrolment and outcomes for Level 3 and 4 qualifications, as well as for apprenticeships. As discussed in Section 2, a typical young learner enters FE college holding Level 1 or Level 2 qualifications – as a reminder, Level 2 is commensurate with passing national GCSE (or KS4) exams at age 16. Thus, enrolling and achieving a qualification at Level 3 or above reflects the extent to which FE colleges allow students to build on and extend prior skills. Learning at higher levels can take place purely at the FE college or as part of an apprenticeship, which is why we also look at the latter to measure student progression.

3.3.3 Higher education (Level 5 and above)

HESA data provides detailed records of students enrolled in publicly funded UK higher education institutions (universities). The data includes type of degree individuals enrol in and when, majors chosen, and qualifications achieved. We look at overall enrolment in higher education at any level – including

[11] The KS4 score is the total score in all General Certificate of Education (GCSE) exams or exams equivalent to GCSEs taken at age 16. These exams are externally set and marked and for most students the score is for 8 to 10 GCSE subjects.
[12] Further education qualifications can be taken at FE colleges but also other public or private providers. FE colleges provide about XX% of all further education for young people in England.

foundation degrees (level 5) and bachelor's degrees (level 6), and whether a student has completed a bachelor's degree (conditional on enrolling).

3.3.4 Labour market data

HMRC and DWP provides individual labour market data from administrative tax and employment records and contains information on employment spells, including duration, and on annual earnings. We focus on the probability of being in sustained employment (employed for more than 90 days) and annual earnings in the two years after leaving the FE college.

4 Econometric Strategy

Our focus is on estimating the effect of college capital grants on student outcomes. Regressing student outcomes on capital grants will be misleading if unobserved confounders influence capital grant decisions and college performance. There are at least two concerns. First, colleges are primarily state funded and capital expenditures are dependent on grants from central government. These grants could be targeted to address under-performance or to 'reward' success. In the first case, this could downward bias estimates of the effect of grants on outcomes. In the second case, the bias could go in the opposite direction. Second, even if government funds are not selectively targeted, the allocation of capital grants might be non-random, if better managed colleges have more success when bidding for government funds.[13]

To deal with these problems, we estimate treatment effects in the years after grant approval using a difference-in-differences strategy. We use the timing of grant approval to define treatment and control colleges. The possibility that effects on student outcomes might take time to emerge precludes the use of a traditional event study using a simple treatment-indicator for the post grant period. An alternative would be to use grant-cohort treatment dummies – which take the value one for the set of colleges receiving a grant in a given year – interacted with a set of pre- and post-treatment year dummies. The small number of treated colleges per year, and the resulting need to pool across years, precludes this approach.

Instead, we proceed as follows. All colleges that receive a grant before a given cut-off year are defined as treated colleges. We want to estimate the effects of grants over several years following treatment to allow for impacts to take time to emerge. We need to account for this when constructing the control groups to ensure that no control colleges are treated in the time-period over which treatment effects are estimated. We do this by constructing cohorts of treated and control colleges.[14] For a given year, the cohort comprises treated colleges – defined as all colleges that receive a grant in that year – and control colleges – defined as all colleges that do not receive a grant until at least a fixed number of years later. We use four years for reasons discussed below. For each cohort, we estimate treatment effects by

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[13] Even if capital expenditure is uncorrelated with unobservable college characteristics, only the most productive uses of capital may be approved. In this case we estimate the effect of expenditure in its most productive uses, not the effects of random capital projects spending.

[14] Eyles and Machin (2019) use a similar approach to define the control group in the difference-in-difference setting in their study of the effect of schools converting to academies, which have more autonomy in several dimensions of school management, on student outcomes.

comparing outcomes of treated and control colleges in the relevant years following the year of grant approval that defines that cohort. This method of constructing treatment and controls limits the period over which we can look for an impact of grants, because after the fixed number of years used to identify control colleges those controls start receiving grant approvals. Although treatment effects are allowed to vary by the number of years since grant approval, we assume that treatment effects do not differ across cohorts and pool cohorts accordingly.

Identification of the effect of the grants comes from the timing of grants, rather than whether a college receives a grant. The identifying assumption is that, in terms of unobservable characteristics that would affect the trend in student outcomes, colleges that receive grants later are similar to colleges that receive grants earlier, thus providing valid counterfactuals.[15] Although this identifying assumption cannot be tested, we provide evidence that pre-trends for observable characteristics are similar between treatment and controls. We also show that the treatment and control colleges (early and later grant recipients) are generally comparable in terms of observable characteristics.

The data and the institutional arrangements in our context suggest that, if anything, any lack of comparability between early (treatment) and later (control) grant recipients will tend to bias estimates towards zero. This is because, the approval process got slightly more stringent after 2009 - the period corresponding to the control group - as less money became available for grants. Colleges successful in later control rounds may therefore be those with stronger leadership, which in turn leads to relatively good student outcomes in the pre-2009 period. Another factor is that treatment group colleges undertaking capital expenditure projects early on would have had to service project-related debt repayments, so may have had fewer resources for teaching than the control colleges.

Other potential biases from designs, like ours, that identify effects from the timing of policy interventions have received attention in recent years (Athey and Imbens, 2021; de Chaisemartin and D'Haultfoeuille, 2020; Goodman-Bacon, 2021; Sun and Abraham, 2020). There are two main concerns in this literature. Firstly, if treatment effects are heterogeneous over the policy roll-out period, then a standard two-way, within-groups fixed effects design ('staggered' difference-in-difference, with panel unit and time fixed effects) estimates an average of these treatment effects which is weighted towards the middle of the period. This is because the time-varying treatment indicators (i.e., the treatment x post-policy dummies) are switched on for different lengths depending on whether they are treated early or later, and least squares coefficient estimates are weighted by the variance of this time varying treatment. The second potential problem is that in the standard two-way fixed effects design, early treated units provide controls for the later-treated units. If treatment initiates a change in trends (rather than the step change assumed by difference-in-difference methods), then the early treated units may be on different trends by the time the later-treated units are treated, violating the 'parallel trends' assumption underlying

[15] Despite the change in the grant-awarding body described in Section 2.3, discussions with the relevant officials suggests that grants were allocated using similar criteria throughout the period.

basic difference-in-difference designs. Our design circumvents the first of these problems because treatment effects are estimated using a sample of colleges spanning a fixed time window around each grant treatment cohort group, so the dummies indicating grant approval are switched on for the same number of periods for each treatment cohort. The design also sidesteps the second problem, by using only future-treated colleges that have not yet received grants as controls for each grant treated cohort, and there is no a-priori reason to expect these future-treated colleges to be on different trends at the time of grant approval for earlier-treated colleges. Therefore, our estimates will be unbiased, assuming the timing of grant allocation is uncorrelated with time varying unobservables.

4.1 Difference-in-Difference/Event-Study Estimator

The most general form of the equation used to estimate the impact of grants on student outcomes is as follows:

$$Y_{ict} = \beta_0 + \sum_{\tau=-PRE}^{POST} \beta_\tau (D_{t=g+\tau} \cdot Treat_c) + \sum_{\tau=-PRE}^{POST} \delta_\tau D_{t=g+\tau} + \mathbf{x}_{ict}'\boldsymbol{\beta}_{\mathbf{x}} + \gamma_c + \lambda_g + \mu_t + \varepsilon_{ict} \quad (1)$$

Data is at student level and outcome variables Y_{ict} are for student (*i*) attending college (c) at time (t). Variation in treatment and timing of treatment is at the college-by-year level. $Treat_c$ is a treatment indicator defined at the college level and is set equal to one for all treated colleges, zero for control colleges. For each cohort (g) we define grant-cohort-by-year dummies, $D_{t=g+\tau}$ which are indicators that year $t = g+\tau$. These indicate the number of years τ pre- or post-treatment a cohort of colleges is at time *t*. *PRE* and *POST* define the number of years for which we estimate effects pre-and post-treatment, respectively. Vector $\mathbf{x}_{ict}$ represents a vector of time-varying college and time-invariant individual level control variables. We also control for fixed effects for the college (γ_c), grant-cohort year (λ_g) and the academic year the individual is observed in the college (μ_t). Finally, ε_{ict} is the error term.

As discussed above, the cohorts are constructed to ensure that control colleges never get treated during the time window following treatment (defined by *POST*). Similar to Martorell et al. (2016), we stack the data for each grant cohort and its matched control group, which means that some colleges will act as a control in more than one cohort (e.g. a college first receiving a grant in year *t* acts as a control for each college in cohorts *g = t – (POST+1)* and earlier). Given this, we cluster errors at college level. In some specifications, to look at the effects of grant awards on college-by-year level inputs, we estimate a college by year level version of this specification, i.e. equation (1) without the *i* subscripts.

Given the specification of equation (1), the coefficients β_τ on the interactions $D_{t=g+\tau} \cdot Treat_c$ provide estimates of the effects of grants on student outcomes Y_{ict}, by comparing mean outcomes for students in treated colleges before and after the grant approval year, with mean outcomes for students in the relevant control colleges at the same time.

Estimating equation (1) using our data requires us to make several decisions that are specific to our setting. The most important concerns the length of the window over which β_τ is estimated by including the relevant interactions. To allow for as much time as possible for effects to occur post treatment, we consider only three pre-treatment observations and set *PRE*=3. Our student outcome data start in 2003, so the first cohort of treatment and control colleges is defined for 2006. Although we have grant approval data till 2015, few colleges receive approvals in that year, so colleges with grant approval in 2014 are the last we can use to define control colleges for the final cohort. Given this, we set *POST*=4 and look for treatment effects for students attending college in the four years following treatment. This implies the first cohort is comprised of colleges treated in 2006 and control colleges that only have grants approved sometime 2011 onwards. Data from treatment and control colleges for 2007 to 2010 is used to estimate treatment effects for those attending college in the four years following approval in 2006, before the control group receives grants starting in 2011.[16] The last cohort of treatment and control colleges is comprised of colleges treated in 2009 and control colleges that receive grant approvals in 2014. For that cohort, we use data from treatment and control colleges for 2010 to 2013 to estimate treatment effects for those attending college in the four years following approval in 2009, before the control group receives grants in 2014.[17] In total, we construct four cohorts for colleges treated in years between 2006 and 2009, inclusive.

With these choices made, we can provide some concrete examples of how the coefficients on the interaction terms are constructed. So, for example, $D_{t,g-3} = 1$ in 2003, for colleges in grant cohort $g = 2006$ - which includes treatment colleges receiving grant approval in 2006 plus control colleges receiving grants from 2011 onwards. Similarly, $D_{t,g-3} = 1$ in 2004, 2005 and 2006 for colleges in grant cohorts g=2007, 2008 and 2009, respectively, and is zero otherwise. This dummy captures differences in outcomes for those attending treated colleges three years before treatment, with two other dummies capturing effects for those attending two and one year before treatment. Four post-treatment dummies capture the effect of receiving grants for those attending college in the four years following treatment. So, for example, dummy $D_{t=g+1} = 1$ in 2007, 2008, 2009 and 2010 for colleges in grant cohort g = 2006, 2007, 2008 and 2009, respectively and captures the effect for those attending college in the first year following treatment. Taken together, this set of eight dummies can be used to estimate effects for those attending college in a window of eight years, around the year of grant approval for treated colleges.

[16] Robustness checks – available on request – look at effects up to t+6. We can only do this for outcomes that are measured annually in the ILR – enrolment in a Level 3 qualification, enrolment in a Level 4 or above qualification, enrolment in an apprenticeship, and achievement of the latter three types of qualifications, conditional on enrolment. Changing the time window over which we look for effects also changes the control group available for each cohort as discussed in the text.

[17] As discussed above, we cluster errors at the college level because the way we match grant-cohorts to controls means that individual colleges act as controls in more than one grant-cohort (e.g. a college first receiving a grant in 2014 acts as a control for each college in grant-cohorts receiving grants in 2006, 2007, 2008 and 2009;).

Students (i) are allocated to college-year observations if they are enrolled in that college (c) in that year (t).[18] Students can appear multiple times in the estimation sample, either because they attend more than one college in different years, or because they are in a control college, which itself is repeated multiple times as a control for different grant cohorts. Ninety three percent of students only appear once in the treatment group colleges. The dependent variable Y_{ict} is one of several education and employment outcomes for student i, observed at some point after they entered college c in year t. As discussed in the results section, the time at which the outcome is observed - and hence the interpretation of the treatment effect - varies by outcome. We only have one time-varying college characteristics in vector $\mathbf{x}_{ict}$ - the log of the number of full-time equivalent students at the college. We include several time-invariant individual level control variables in $\mathbf{x}_{ict}$ - gender, ethnicity, a measure of socio-economic status (whether the student had ever been eligible to receive free school meals during compulsory education), the standardised score in GCSE exams (undertaken by all students at age 16), and a set of dummies indicating the year in which the student took these exams.[19] Estimating specifications with and without $\mathbf{x}_{ict}$ gives us the effect of grants with and without controlling for any composition effect that might arise from grant receipt.

We use equation (1) with various restrictions. For graphical analysis, we show the effects over the full range of pre- and post-grant years, in an event study style analysis with $\tau = -1$ as the baseline year (i.e., we omit $D_{t,g-1}$). The estimates of the effects β_τ on $(D_{t=g+\tau} \cdot Treat_c)$ for $\tau < 0$ capture differences in trends in outcomes between treatment and control colleges for those attending college pre-date grant approval. Our main estimates, combine the pre-approval years $\tau \in \{-3, -2, -1\}$ into a baseline group and omit the corresponding dummies ($D_{t,g-3}, D_{t,g-2}$ and $D_{t,g-1}$) . This improves estimate precision, by reducing the number of coefficients estimated. It also reduces the sensitivity of the estimates to the variability in a college's outcomes in the single year corresponding to $\tau = -1$.

4.2 IV Approach

The setup described above estimates the effect of capital expenditure projects on student outcomes but not the effect of physical capital *stock* – i.e., the buildings and facilities funded by these grants. Estimating the effect of capital stock introduces an additional problem because unobserved college-level factors - such as the quality of the leadership and managerial organisation - are likely to influence internal capital decisions and student performance. This problem is analogous to that of the effect of unobserved firm heterogeneity in estimating the coefficient on capital stock in firm production functions. The likely impact is an upward bias in estimates, if good management is associated with higher capital stock and better

[18] For outcomes that are observed after leaving the college, such as progression to higher education, we match students to a college-year observation based on their year of first enrolling in that college.

[19] Free School Meal (FSM) is a statutory benefit available to school-aged children from families who receive other qualifying benefits, including various income support and out-of-work benefits. We standardize GSCE scores for each cohort to have mean equal to zero, standard deviation equal to one.

educational outcomes. We use information on capital grants to help address these concerns over the endogeneity of capital stock.

To do this, we estimate regressions of student outcomes on a measure of physical capital - the value of fixed assets as recorded in the balance sheets of the college accounts - using the timing of grants as an instrumental variable (IV) for the value of fixed assets. This IV approach requires the further restriction that grants only affect outcomes via the induced capital expenditure and associated increase in the value of fixed assets, i.e., there were no other changes in college revenues or expenditure due to receiving the grant that would potentially affect college outcomes. It also requires the value of fixed assets – an accounting concept – to be a good measure of physical capital stock.

We use the specification in equation (1) for the first stage continuing to combine pre-approval years $\tau \in \{-3,-2,-1\}$ in to one baseline group:

$$lnAssets_{ict} = \beta_0 + \sum_{\tau=0}^{4} \beta_\tau (D_{t=g+\tau} \cdot Treat_c) + \sum_{\tau=0}^{4} \delta_\tau D_{t=g+\tau} + \mathbf{x}_{ict}{}'\boldsymbol{\beta}_{\mathbf{x}} + \gamma_c + \lambda_g + \mu_t + \varepsilon_{ict} \quad (2)$$

where $lnAssets_{ict}$ is the log of the value of fixed assets for student (i) attending college (c) in year (t). This will be identical for every student enrolled at college c in year t. Everything else is defined as in equation (1). The second stage equation is:

$$Y_{ict} = \beta_0 + \beta_{2SLS}\widehat{lnAssets}_{ict} + \sum_{\tau=0}^{4} \delta_\tau D_{t=g+\tau} + \mathbf{x}_{ict}{}'\boldsymbol{\beta}_{\mathbf{x}} + \gamma_c + \lambda_g + \mu_t + \varepsilon_{ict}. \quad (3)$$

5 Results

5.1 Sample selection and descriptive statistics

As discussed in the introduction, we focus on younger learners, for which we have prior attainment data. These individuals are typically between 16 and 20 years old when first entering college. The first cohort of FE-learners for which we have prior attainment data completes compulsory education at age 16 in the academic year 2001/02; the last completes compulsory education in 2013/14.[20]

We drop the small number of colleges that do not receive a grant between 2001 and 2015.[21] We exclude colleges that receive multiple grants in different years, except where these two years are consecutive, in which case we combine the grants into one, as if approved in the earlier year.[22] We consider only large grants in the main analysis (i.e., colleges that applied for and received grants for new buildings, extensions and major redevelopments, rather than minor refurbishment). We define large grants as those in the fourth quartile of the grant distribution ('Q4 grants'). We provide additional results including grants in the third quartile ('Q3+Q4 grants'). As discussed above, Q3+Q4 grants represent over 90% of the value of all grants.

[20] This limits the time window over which we can study the effects of capital grants on student outcomes.
[21] More than 95% of all FE colleges in our dataset, which covers all English FE colleges, has at some point received a grant between 2002 and 2015, the time period the grant data spans.
[22] After combining grants in the same year and those in successive years, there are a total of 69 grants in the fourth quartile (Q4), and a total of 87 grants in the third and fourth quartile (Q3+Q4) over the period from 2006 to 2009. By restricting the sample to those college that only receive one grant over a time window of eight years, we can study the effect of 38 grants (Q4) and 61 grants (Q3+Q4).

Tables 2 and 3 show summary statistics at baseline for treatment and control groups when looking at Q4 grants and Q4+Q3 grants, respectively. These baseline characteristics are measured three years before grant receipt for treatment and control colleges, e.g., for colleges in grant cohort $g = 2006$, characteristics are measured in 2003 for both treatment and control colleges.[23] The control group is generally larger than the treatment group (in terms of total income, fixed assets, capital expenditure and number of students) but the mean values are rarely statistically different from each other. In other respects, the samples look similar at baseline. Pre-trends in the main variables of interest are considered in the relevant sub-section.

5.2 The Effects of Grants on Capital Expenditure and Fixed Assets

We start by checking that capital grants increase capital expenditure rather than simply substituting for other funding. Grants need to increase capital expenditure – and thus improve college buildings and facilities – for there to be any effect on enrolments and student outcomes. We also look at the impact of capital grants on the value of fixed assets, which we use as a measure of physical capital. Although the relationship between capital expenditure and the value of fixed assets is mechanical in the accounting data – capital expenditure, minus depreciation, determines the change in the value of fixed assets – looking at both gives an overall picture of the timing of capital projects.

Figures 2 and 3 show results from estimating a college-level equivalent of equation (1) for log capital expenditure (Panel A) and log total fixed assets (Panel B) for Q4 and Q4+Q3 grants, respectively.[24] Panel A in Figure 2 shows that grant approval increases capital expenditure in the year of approval and for the next three years, by between 0.75 to 2.13 log points for Q4 grants. Figure 3 shows an increase of between 0.78 and 1.69 log points over the same time-period for Q4+Q3 grants. Both figures show capital expenditures falling to pre-grant approval levels four years after approval.[25] These estimates imply big increases – annual capital expenditure is around five-times higher than pre-grant approval levels in each of the three years following grant approval. Consistent with these effects on capital expenditure, Panel B in

Figure 2 and Figure 3 shows fixed assets increase in the first two years after grant approval before stabilising at a new, higher level. The corresponding increase in the average absolute value of fixed assets for treated colleges four years after grant approval is around £34 million for Q4 grants, around £26 million for Q4+Q3 grants. There is no evidence of pre-trends in capital expenditure or fixed assets in the three years preceding grant approval.

If grants do not fully fund capital projects these increases in capital expenditure could imply decreases in operating expenditure. If this happens, then we may under-estimate the impact of capital grants given what is known about the positive relationship between spending and student outcomes in England

[23] For colleges that are used as controls in multiple grant cohorts, we only include the value of baseline characteristics for the first grant cohort in which they are used as controls.

[24] For college-level regressions we drop all student-level covariates from $\mathbf{x}_{ict}$ so it only includes the college-level covariate log number of students enrolled in year t at college c.

[25] See Table 14 and Table 15 for the full set of results from the event study regressions.

(Gibbons and McNally, 2013). Panel C of Figures 2 and 3 suggests this is not the case – operating expenditure is unaffected by grant approval.

The profile of increases in capital expenditure, and the evolution of fixed assets suggest that construction is completed up to three years after grant approval.[26] This has implications for student outcomes as for some, but not all, projects the earliest we might expect to see positive effects is for the cohort enrolling three years after approval. The whole literature on capital expenditure suggests that effects take time to emerge and are usually not contemporaneous with the commencement of capital projects. It is also possible that there might be some negative effects due to disruption during the initial construction phase (as found, for example, by Goncalves (2015) and Conlin and Thompson (2017) for construction projects in Ohio). In practice, as shown below, we find no evidence of anticipation or disruption effects of this type.

5.3 The Effect of Grants on Enrolment

The most immediate effect of capital grants may be on enrolments. Students can enrol in colleges at any time. For younger learners (our focus) the inflow at age 16-17 is large as this is when students commence post-compulsory-school education. This is a time when students consider the options available to them and many change institutions. Changes in the number or the composition of enrolments, for example towards better qualified students, are of direct interest and may also have implications for other student outcomes.

Panel (A) of

Figure 2 shows estimates of a college-level equivalent of equation (1) – and the associated 95% confidence intervals - for Q4 grants where the outcome variable is the number of fulltime-equivalent (FTE) students enrolled in year τ in college c (and we drop the number of students in the college as a control variable**)**. Panel A of Figure 4 shows the corresponding estimates and confidence intervals for Q4+Q3 grants. Neither figure shows significant increases in the number of FTE students enrolled following grant approval nor any evidence of significant pre-trends.

Turning to the composition of enrolments we look at three outcomes that indicate whether the academic and socio-economic background of the student intake is changing in response to capital expenditure projects. For academic achievement we use the standardised KS4 score of students enrolling in year τ in college c to consider the composition of the intake in terms of the exams taken at the end of compulsory education. We also look at the percentage of the student intake that have ever been eligible

[26] We do not have data on the completion dates for all projects. For those projects where we observe completion dates, the average duration is 1.6 years for projects that received a Q3 grant, and two years for projects that received a Q4 grant. We do not see further increases in fixed asset values in year three after grant approval, even though capital expenditure remain higher than during the pre-approval period. This can be explained by the fact that colleges will start amortising the newly completed building in the completion year (t+3), and this will reduce the value of fixed assets accordingly. If capital expenditures are just equal to the value of depreciation and amortization, the value of fixed assets will remain stable.

for free school meals (FSM) during compulsory education as an indicator of deprivation and the percentage of student intake that are white British.

Figure 4 and Figure 5 show full event study estimates for these outcomes for Q4 and Q4+Q3 grants, respectively, based on student-level regression estimates of equation (1). Both figures suggest that pre-trends in these three outcomes were the same for treatment and control colleges prior to grant approval but that intake does change post-grant approval. Tables 3 and 4 present the corresponding regression results – where we use the absence of pre-trends to combine pre-treatment years as described above.

For Q4 grants, results in Table 4 confirm the impression from the full event study. Three years after grant approval the student intake has significantly higher KS4 scores: an increase of 0.15 of a standard deviation in year three, 0.20 in year four. This appears to be consistent with the timing of increases in capital expenditure and fixed assets following grant approval. The socio-economic composition of students also changes as students in treatment colleges are 2.67 percentage points less likely to have been recipients of free school meals in year three, and 4.42 percentage points less likely in year four. There is no effect on the ethnic composition of the intake.

For Q4+Q3 grants, results in Table 5 show smaller effects on prior attainment. Three years after grant approval, treatment colleges enrol students with 0.07 of a standard deviation higher KS4 scores (not significant), increasing to 0.12 of a standard deviation higher after four years (significant). For these grants there is no effect on the probability of students enrolling having been eligible for free school meals nor on ethnic composition. The results for FSM for Q4+Q3 show the benefits of pooling baseline years to reduce the undue influence of t-1 when estimating treatment effects.

To summarise, colleges that receive grants do not increase the number of students enrolled. But these colleges see quite large and significant effects on the educational attainment of their intake. These effects are stronger for the largest grants and the timing is consistent with the effect on capital expenditures and assets. Colleges with the largest grants also reduce the proportion of socially disadvantaged students in their intake. There is no evidence for changes in ethnic composition.

5.4 The Effects of Grant Approval on Student Learning Outcomes

We use several different indicators for student educational outcomes, which we divide into two groups – learning outcomes while at college (discussed in this section) and educational outcomes post-college (discussed in Sections 5.5 and 5.6). The first group may be immediately affected by completion of a capital project, either because they measure enrolment in a course leading to a qualification or because they measure achievement in qualifications which are undertaken at college and usually completed within one or two years. This group includes whether a student enrols in at least one aim at Level 3 and whether, conditional on enrolment, they achieve a Level 3 aim; and similarly for enrolment and achievement of Level 4 or an apprenticeship. Level 4 and apprenticeship qualifications may take two years to complete but we can still see an immediate impact given that second year students may benefit from improved facilities. The qualifications we consider usually involve one to two years of study, which means that for most students we should observe whether they completed. For these outcomes we match students to the college in which they are currently enrolled. That means the coefficients on the post-approval dummies capture the effect on outcomes for the cohort of students who are observed in the college in a given year following grant approval.

Results for estimates of equation (1), combining all pre-approval periods in the baseline category, are shown in Table 6 and Table 7 for Q4 and Q4+Q3 grants, respectively. Table 5 shows that students who attend a treatment college in year t+3 after grant approval are 6.28 percentage points more likely to enrol in at least one Level 3 qualification, and those attending in year t+4 after approval are 8.24 percentage points more likely. The second specification, that reports results when controlling for demographic characteristics and prior attainment, shows this effect is partially driven by changes in the intake composition. Coefficients in years t+3 and t+4 remain significant but are reduced to 4.16 and 4.84 percentage points, respectively. There is no significant effect on achievement of Level 3 courses conditional on enrolment. This is a still a good outcome because it suggests more students achieving Level 3 outcomes – enrolments go up with no effect on achievement rates. As discussed previously, enabling students to progress to Level 3 is important because this is a pre-requisite for university and is also associated with positive labour market returns. Results for these outcomes are similar in magnitude and significance for the larger sample of Q4+Q3 grants. The magnitude of the effect on enrolment is large. It is close to the effect of marginally achieving a good grade in the KS4 (GCSE) English exam, which has been shown to affect the probability of enrolling in a level 3 qualification by between 6.4 and 9 percentage points (Machin et al. 2020). It is also close to the effect of moving from a low value-added FE college to a high value-added FE college, reported in Aucejo et al. (2020). They find that a one SD increase in college quality increases the likelihood of obtaining a Level 3 qualification by 4.4 percentage points, or 10.5% compared to the sample mean.

We next look at enrolment in qualifications at Level 4 and above (i.e., tertiary education) at FE college. Few students take these qualifications at FE colleges (only about 2.5% of students in our sample) and these estimates do not reflect Higher Education outcomes, which are considered in the next section. For Q4 grants, as was the case for Level 3, effects on enrolments do not emerge immediately and are only evident four years after grant approval. In contrast to Level 3, these effects are generally not significant at standard level once controlling for changes in student characteristics. Results are similar for Q4+Q3 grants, although never significant, whether controlling for student characteristics or not.

Moving to apprenticeships, results indicate that, if anything, students at treated colleges are less likely to enrol in an apprenticeship in years three and four after grant approval, although these coefficients are imprecisely estimated and not significant at standard levels.[27] There is no effect on achievement of

[27] In England, FE Colleges are not major providers of education for apprentices. Off-the-job training for apprentices is mainly catered for by private education providers.

apprenticeships conditional on enrolment. We also show a graphical representation of results based on the full event study specification in equation (1) for enrolment in Level 3 or Level 4+ qualifications. Results are shown in Panels (A) and (B) of Figure 7.

To sum up, capital grants tend to increase enrolment in higher level learning at FE colleges, driven by increases in enrolment in qualifications at Level 3.

5.5 The Effect of Grant Approval on Higher Education Entry

The second group of educational outcomes – for outcomes post-college - can only be affected in the medium-term. We have shown that capital grants have a significant effect on enrolment in qualifications at Level 3, which are prerequisites for further study. We therefore look at whether students who attended treated colleges are more likely to enter higher education (HE) and whether, conditional on enrolment they complete a degree. For these outcomes we match students to colleges based on the year in which they first attend the FE college. That means the coefficients on the post-approval dummies pick up whether a student first attending in a given year post-grant approval are observed achieving these outcomes in any year after first enrolling in the FE college.

We observe higher education enrolment and achievement until 2015/16 for all our cohorts of school leavers, although for HE outcomes we only consider those who complete compulsory schooling in 2009/10 or before to allow sufficient time for them to progress to HE when looking at the t+4 coefficients. Table 8 shows that students first attending treatment colleges in t+3 after grant approval are 5.15 percentage points more likely to enrol in a foundation degree (Level 5) or bachelor's degree (Level 6) at some point post-FE compared to those attending control colleges. The effect is similar in t+4 (4.53 percentage points), but no longer significant. Controlling for student characteristics halves the coefficients, without changing the significance of the t+3 coefficient. These effects are large considering that only about 28% of the students in our sample progress to HE. The magnitude is within the same ballpark as effects shown by Machin et al. (2020) for achieving a good grade in GCSE (KS4) English.[28] Panel C of Figure 6 and Figure 7 show full event study estimates of the effect of grant approval on whether the individual enters higher education, for Q4 and Q4+Q3 grants, respectively. Like the earlier results, there are no pre-trends for this outcome variable. These effects are relatively large compared to, for instance, the effect of moving from a lower ranked FE college in terms of value-added to one that is ranked highly: Aucejo et al (2021) find that a one SD increase in FE college quality increases the likelihood of later attending university by nearly 4 percentage points, or 10% compared to the sample mean.

As shown in Columns (3) and (4) of Table 8 and Table 9, there is no effect on degree completion conditional on enrolment. As with earlier outcomes, this suggests students who enrol in HE because of

[28] Machin et al. (2020) find that marginally achieving a C grade in GCSE English increases the probability of commencing tertiary education by 2.5 to 4 percentage points

attending a college that undertook a large capital expenditure project are just as likely as their peers to complete their courses. This leaves the achievement rate, conditional on enrolment unchanged.

5.6 The Effect of Grant Approval on Labour Market Outcomes

Results so far show that capital grants have a positive impact on academic progression. We next consider whether this also translates into improved employment probabilities and earnings later in life. The data allows us to observe employment and earnings for all students for at least four years after they first enrol in an FE college (which is sufficient time to observe a student completing a typical 1–2-year course in the college and then transitioning to the labour market). We consider two outcome variables, measured one or two years after a student graduates: Whether students were employed for more than 90 days and annual earnings. Note that the earnings estimates are difficult to interpret because the composition of the sample is directly influenced by capital grants (e.g., the capital grant enables a proportion of students to remain in education, and this might negatively influence the composition of students with positive earnings). The results are shown in Table 12 and Table 13 for the sample of Q4 grants and Q4+Q3 grants, respectively.

For employment, coefficients are generally positive and significant only in year t+4 - in all specifications when Q4 is the treatment group (Table 12) and in those specifications with fewer controls when Q4+Q3 is the treatment group (Table 13). Students who attend treated FE colleges t+4 years after the capital grant was approved are 1-5 percentage points more likely to be in sustained employment one or two years after leaving college. The point estimate is towards the lower end of this range when controls are included for the student KS4 achievement, prior work experience and demographics. The estimates are higher when considering the largest grants only (i.e., Q4 rather than Q4+Q3). Compared to the effects of moving from a lower ranked FE college in terms of value-added to one that is higher ranked, these are relatively large effects. For instance, Aucejo et al. (2021) find that increasing college quality by one SD increases the likelihood of being in sustained employment for young people after FE college attendance by only about 1.7 percentage points.

For earnings, coefficients are small and insignificant in all years up to t+4. In year t+4, the coefficients are comparable for both treatment groups when limited controls are included but only significant for the Q4+Q3 sample. The point estimates suggest that capital grants lead to higher annual earnings of approximately £300 for students who attended t+4 years after the capital grant was approved and subsequently gained employment. The point estimates are lower when full controls are included, especially in the Q4 sample.

5.7 The Direct Effect of Physical Capital – an IV approach

The previous sections showed the effects of grants on student outcomes. These effects are, we have argued, plausibly causal given our research design. The more generalizable parameter is the effect of a school's physical capital on student outcomes, which, as discussed above, is likely to be endogenous to student outcomes. To estimate this parameter, we use a similar research design, but regress student outcomes on the school's physical capital stock (the value of fixed assets - buildings and installations - recorded in the college accounts), using the grants as an instrument for capital. The specification is set out in Section 4.2.

The results of the second stage - equation 2 - are shown in Tables 9 and 10 for Q4 and Q4+Q3, respectively. We consider the following outcomes: whether a student is enrolled in a Level 3 course; whether they progress to higher education; and whether they are employed for more than 90 days 2 years after leaving college.[29]

In all cases, the IV approach increases the point estimates, and the size of estimated coefficients is similar across the two samples. The IV approach suggests that increasing fixed assets (arising from the capital grants) increases the probability of enrolment in a Level 3 course by around 3-4 percentage points (only significant for Q4+Q3) and the probability of enrolling in higher education by around 2 percentage points (significant for both Q4 and Q4+Q3). There is no significant effect on the probability of employment. The IV estimates control for $\mathbf{x}_{ict}$ and are similar in magnitude as comparable results in the event study.

6 Conclusion

Little is known about the effects of capital investments on student outcomes. This paper fills this gap by considering whether large capital projects in England led to improvements in educational achievement and progression.

We find that capital projects take about 3 years to complete (as shown by the evolution of fixed assets after investments take place) and that changes in student outcomes take place at that time or the year after. FE Colleges see a marked change in student composition after the completion of capital projects – they attract students with higher prior achievement and a higher proportion of "non-poor" students (i.e. who did not receive free school meals when in school). Even controlling for the change in composition, more students progress to a good upper secondary qualification (i.e. Level 3) or to higher education (at degree-level) in a university. After investment, students also achieve sustained employment with higher probability. Achievement does not change conditional on enrolment, suggesting that marginal students are not more likely to drop out. Effects are usually larger for the largest grants.

[29] Results for all outcomes are available in on request.

These results show that capital investment in college infrastructure has a visible effect on student outcomes within a reasonable timeframe. Investing in capital infrastructure can benefit many cohorts of students and is best considered a long-term investment. However, these results are reassuring for policy makers who may be more concerned about short-term returns as they show that for large capital projects, the benefits materialise as soon as the project is complete.

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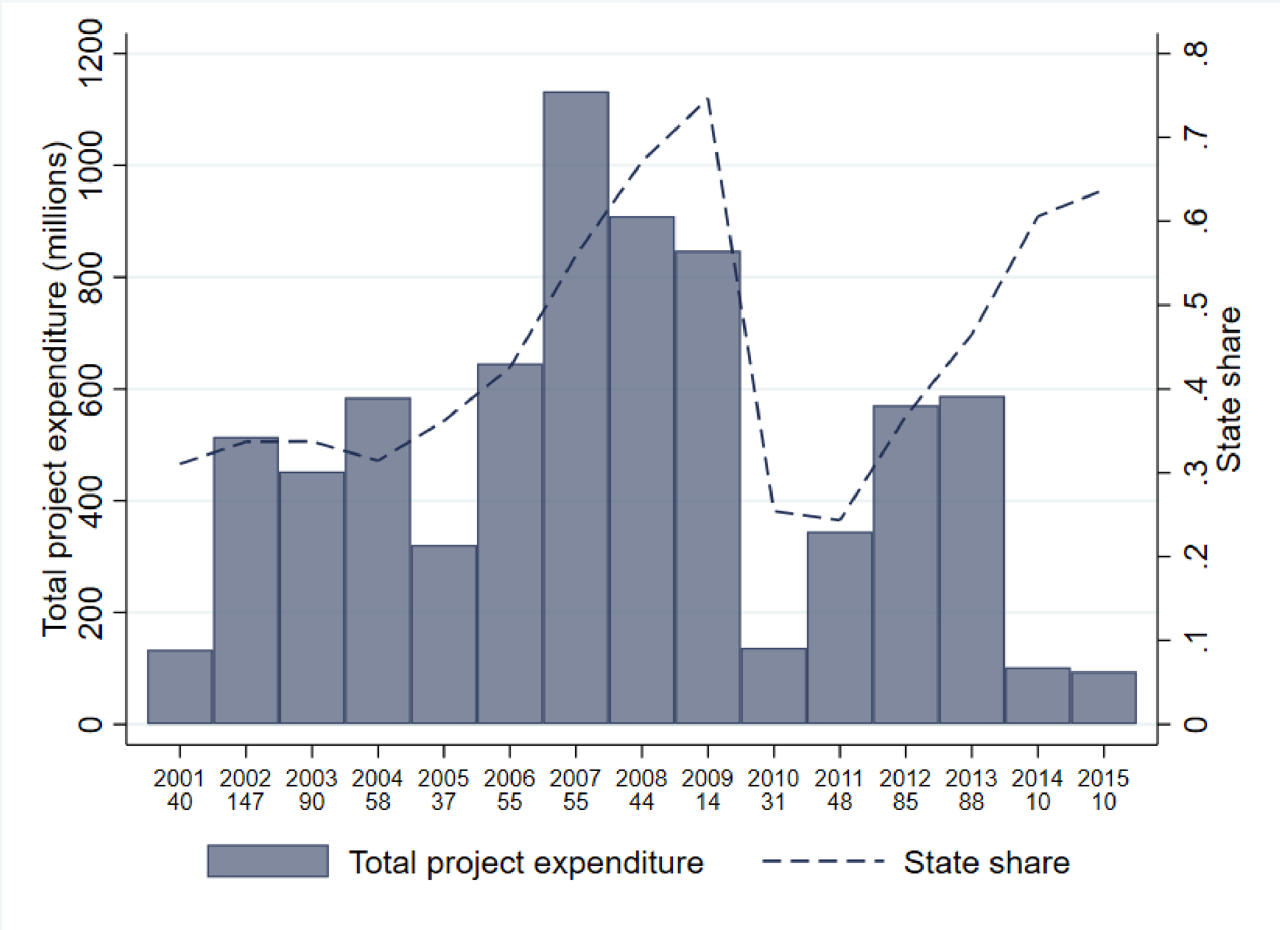
Figures

Figure 1: Evolution of grant values and government contributions

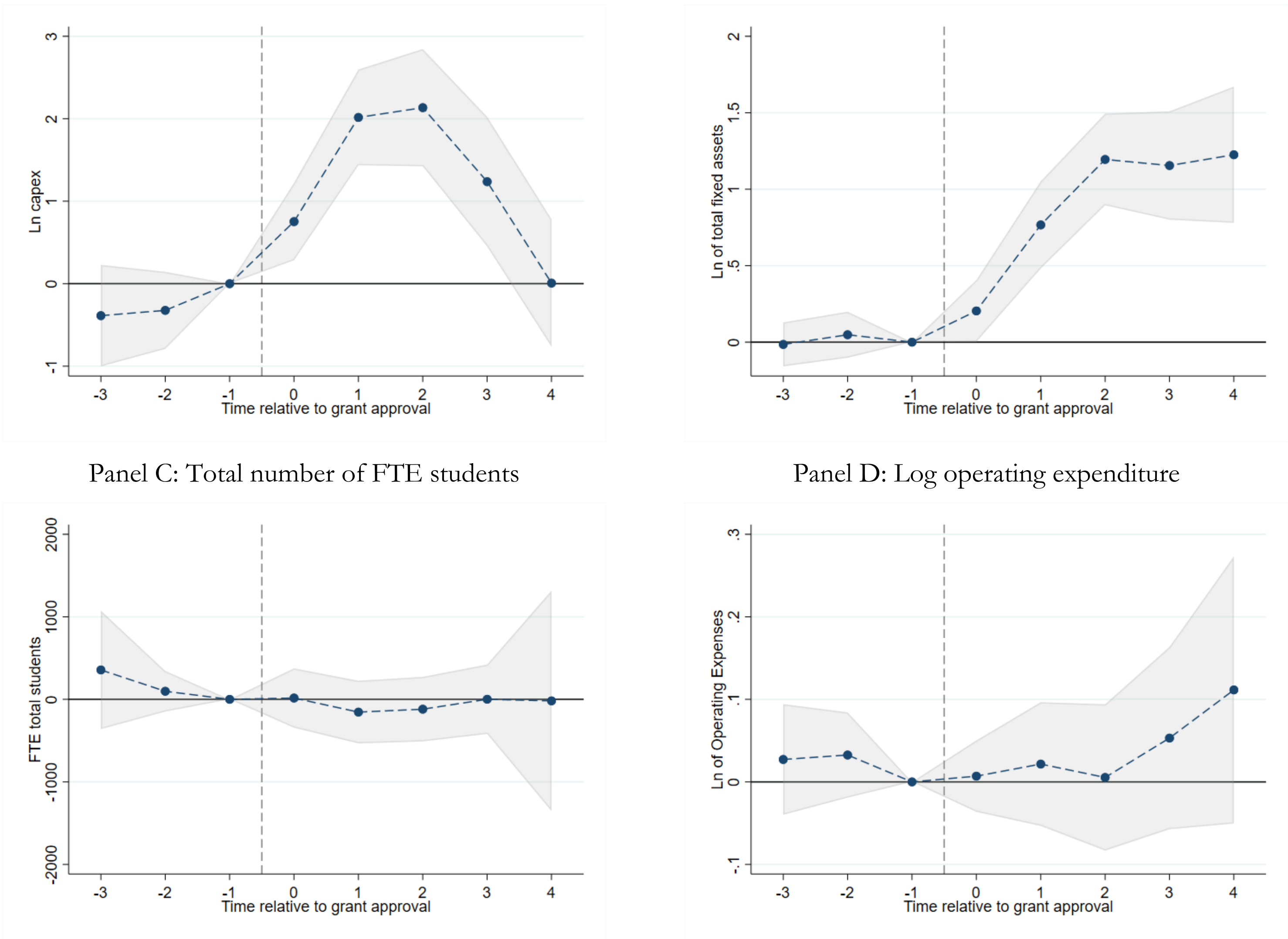


Source: LSC (2001 to 2010) and the SFA (2011 to 2015). Notes: The figure shows the total value of all projects approved by the government for grant funding and the total value of government grants as a share of the total value of project approved each year. The total numbers of projects are indicated at the bottom of the horizontal axis, below the approval year.

Figure 2: Project timing and college-level inputs – Q4 grants

Panel A: Log capital expenditure

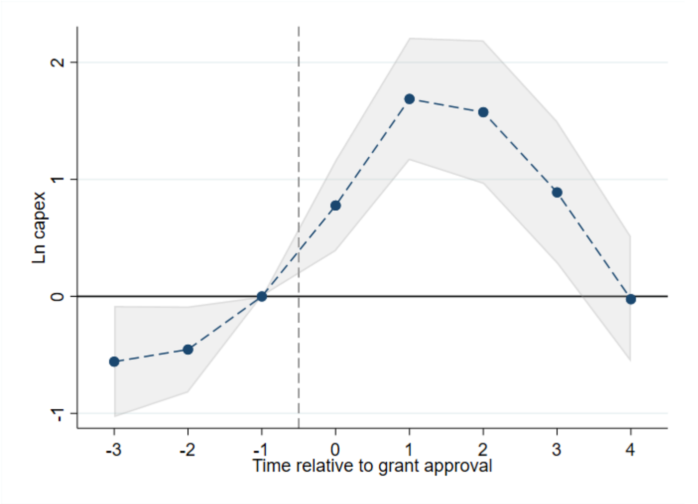
Panel B: Log fixed assets



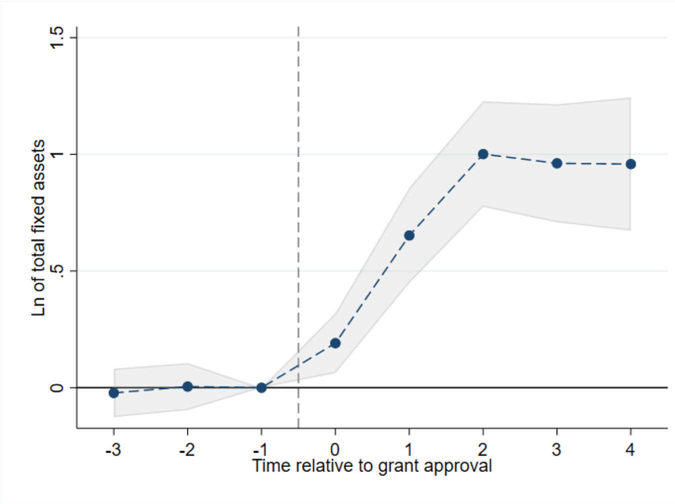
Source: LCS, SFA and ILR. Notes: The figures show point estimates (βτ) and their 90% confidence intervals (grey shaded areas) of regressions of Equation (1) for the sample of colleges that received a grant in the fourth quartile of the grant distribution between 2006 and 2009. The omitted period is τ=-1. Outcomes are measured at the college-year level.

Figure 3: Project timing and college-level inputs – Q3+Q4 grants

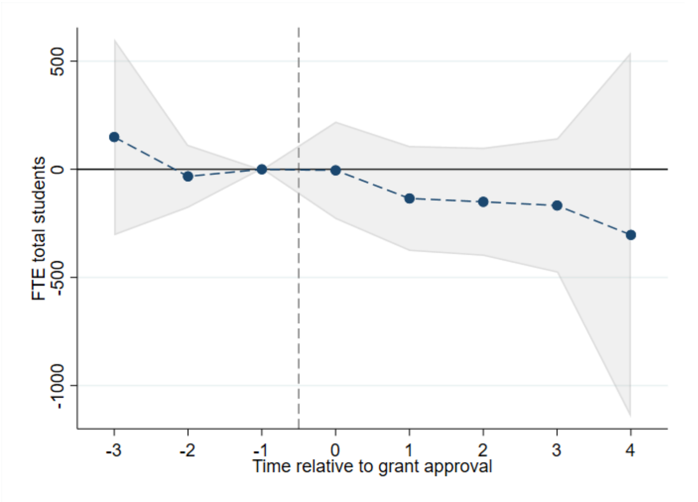
Panel A: Log capital expenditure



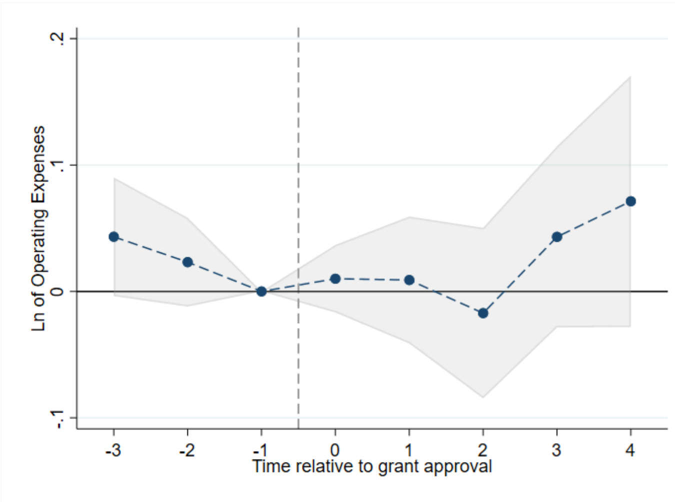
Panel B: Log fixed assets



Panel C: Total number of FTE students



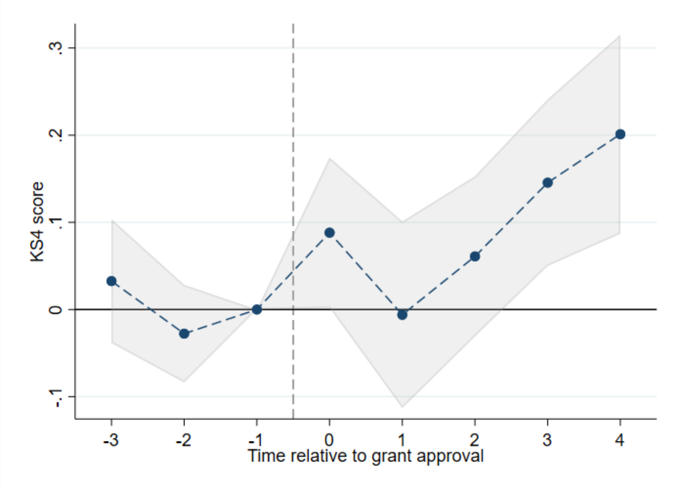
Panel D: Log operating expenditure



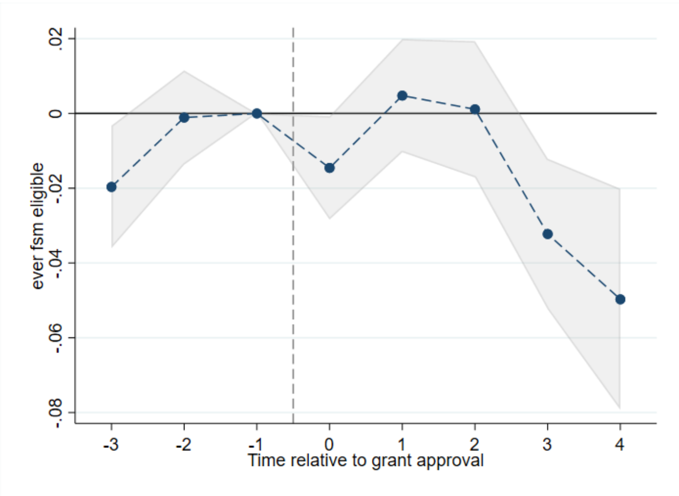
Source: LCS, SFA and ILR. Notes: The figures show point estimates (β_τ) and their 90% confidence intervals (grey shaded areas) of regressions of Equation (1) for the sample of colleges that received a grant in the third and fourth quartile of the grant distribution between 2006 and 2009. The omitted period is τ=-1. Outcomes are measured at the college-year level.

Figure 4: Project timing and student intake characteristics – Q4 grants

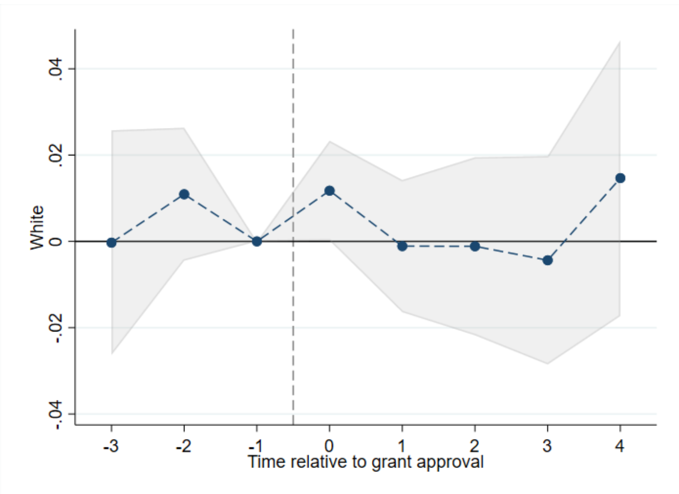
Panel A: Key Stage 4



Panel B: Ever FSM



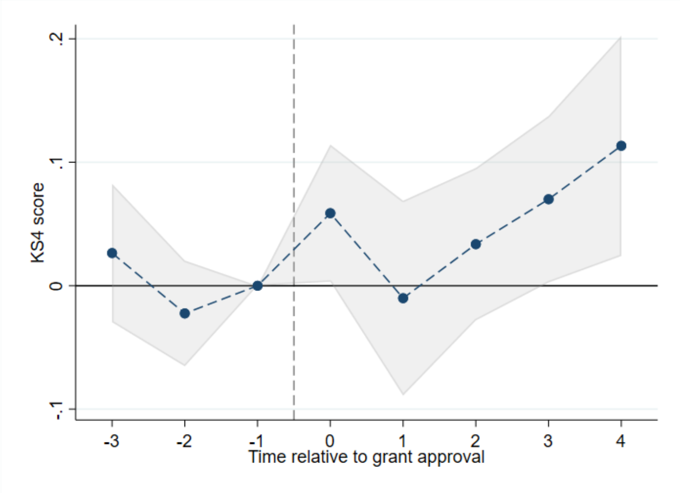
Panel C: White



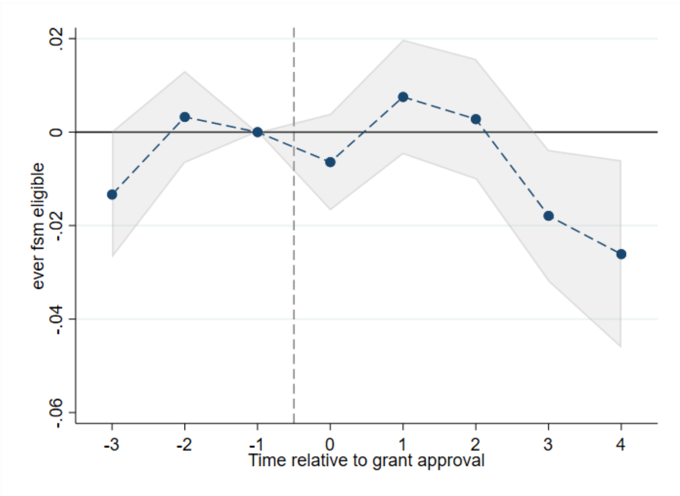
Source: LCS, SFA, ILR and NPD. Notes: The figures show point estimates (β_τ) and their 90% confidence intervals (grey shaded areas) of regressions of Equation (1) for the sample of students that attended colleges that received a grant in the fourth quartile of the grant distribution between 2006 and 2009. The omitted period is τ=-1. Outcomes are measured at the individual level.

Figure 5: Project timing and student intake characteristics – Q3+Q4 grants

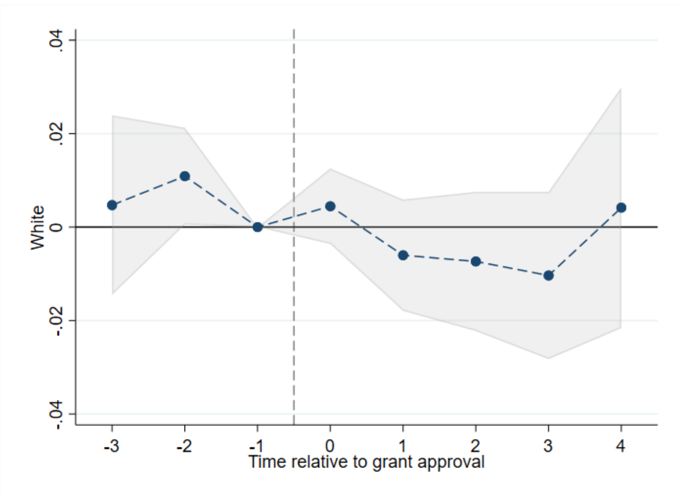
Panel A: Key Stage 4



Panel B: Ever FSM



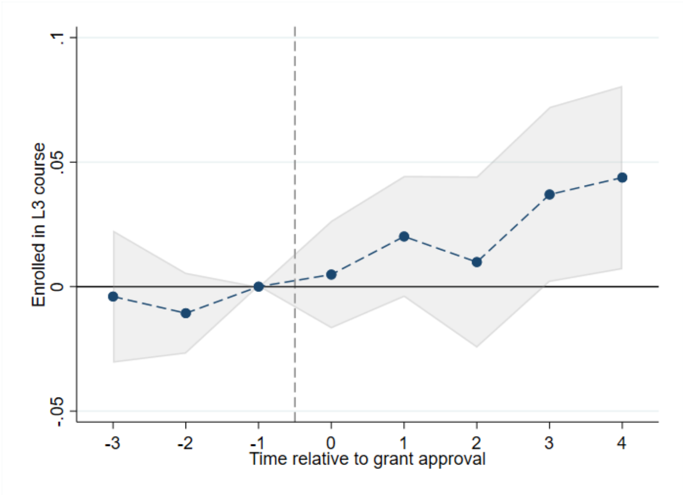
Panel C: White



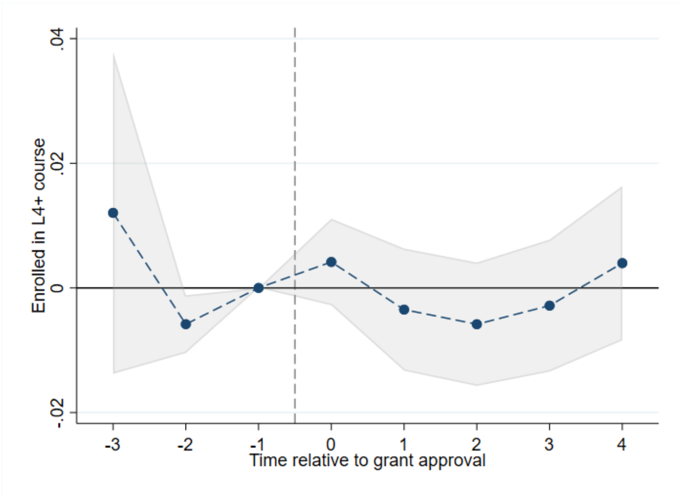
Source: LCS, SFA, ILR and NPD. Notes: The figures show point estimates (β_{τ}) and their 90% confidence intervals (grey shaded areas) of regressions of Equation (1) for the sample of students that attended colleges that received a grant in the third and fourth quartile of the grant distribution between 2006 and 2009. The omitted period is τ=-1. Outcomes are measured at the individual level.

Figure 6: Project timing and educational outcomes – Q4 grants

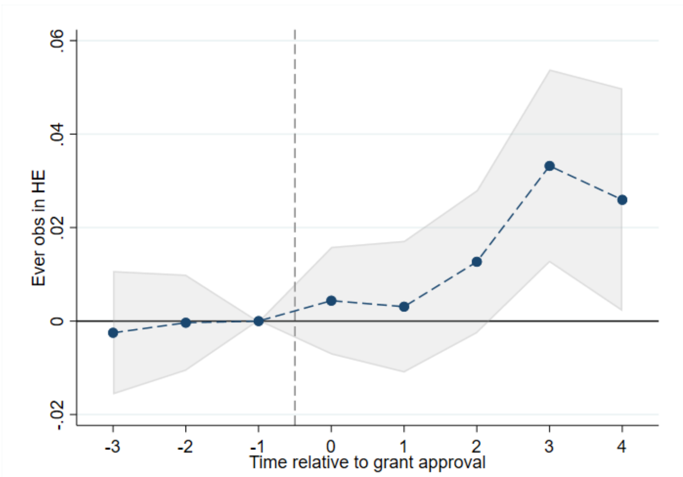
Panel A: Enrolled in a Level 3 Qualification



Panel B: Enrolled in a Level 4 (or above) qualification



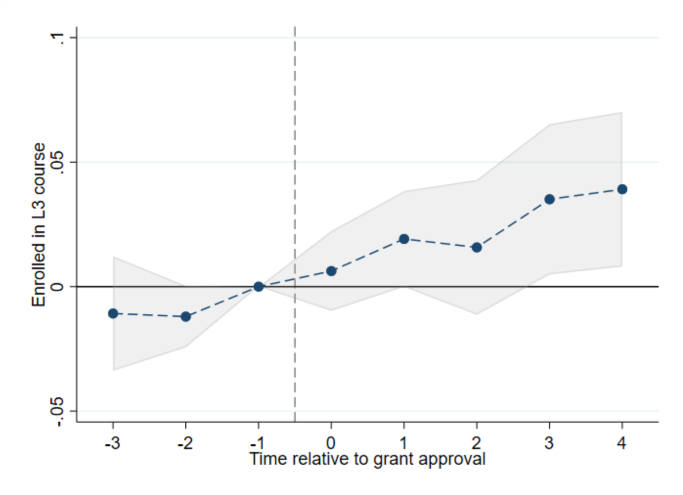
Panel C: Progressed to Higher Education



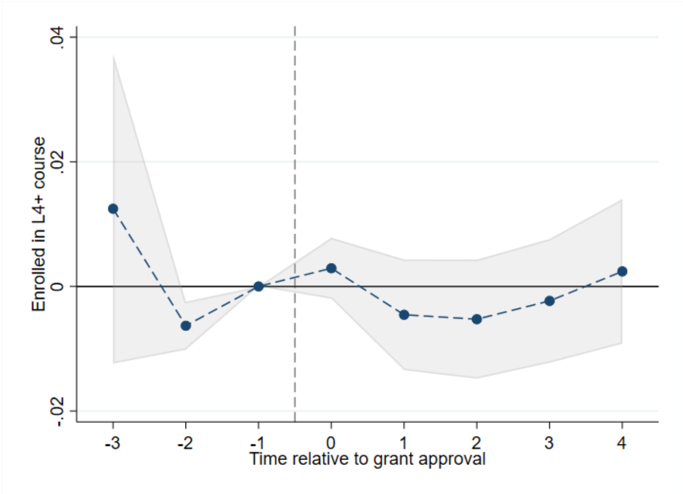
Source: LCS, SFA, ILR and NPD. Notes: The figures show point estimates (β_τ) and their 90% confidence intervals (grey shaded areas) of regressions of Equation (1) for the sample of students that attended colleges that received a grant in the fourth quartile of the grant distribution between 2006 and 2009. The omitted period is τ=-1. Outcomes are measured at the individual level.

Figure 7: Project timing and educational outcomes – Q3+Q4 grants

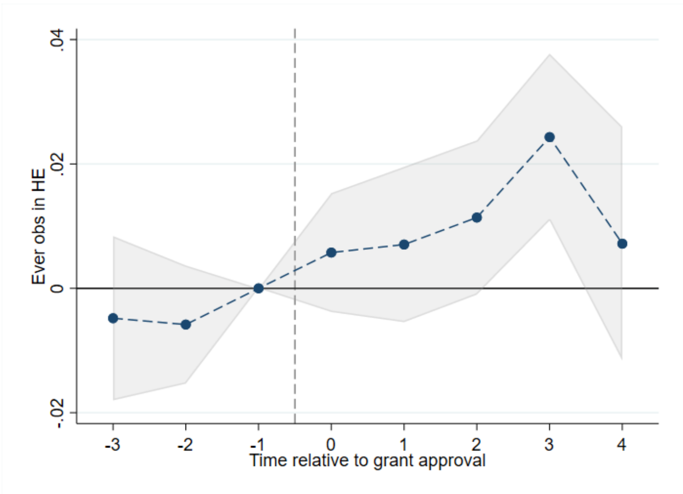
Panel A: Enrolled in a Level 3 Qualification



Panel B: Enrolled in a Level 4 (or above) qualification



Panel C: Progressed to Higher Education



Source: LCS, SFA, ILR and NPD. Notes: The figures show point estimates (β_τ) and their 90% confidence intervals (grey shaded areas) of regressions of Equation (1) for the sample of students that attended colleges that received a grant in the third and fourth quartile of the grant distribution between 2006 and 2009. The omitted period is τ=-1. Outcomes are measured at the individual level.

Tables

Table 1: Distribution of project values and grant values from 2001 to 2015 by grant quartile

Grant quartile	Total project value	Total grant value	Share of Total Project Value	Share of Total Grant Value	Number of Projects
1	240822.1	49055.73	3%	1%	142
2	804356.2	203685	11%	6%	146
3	1448611	450485.1	20%	13%	136
4	4882187	2838277	66%	80%	141
Total	7375976	3541503	100%	100%	565

Source: LSC (2001 to 2010) and the SFA (2011 to 2015). Column 2 shows the sum of the values of all capital projects approved by the LSC or the SFA over the period from 2001 to 2015. Column 3 shows the sum of the values of all grants approved for these projects. Column 4 shows the ratio of the total value of projects in each grant quartile over the total value of all projects approved over the period. Column 5 shows the ratio of the total value of grants in each grant quartile over the total value of all grants approved over the period. Column 5 shows the number of grants/projects in each grant quartile

Table 2: Control/treatment balancing, Q4 grants

	(1)		(2)		(3)
Variable	Control mean (sd)		Treatment mean (sd)		Difference (1)-(2)
Total income ('000)	31467.87	(20439.96)	23809.87	(29523.98)	7658.00
Total fixed assets ('000)	33433.13	(24788.59)	17524.00	(16958.40)	15909.13*
Total capital expenditure ('000)	4511.53	(6117.44)	1325.35	(1749.43)	3186.19
Greater London	0.20	(0.41)	0.13	(0.34)	0.07
Number of unique students	14064.13	(8548.11)	11918.00	(11546.18)	2146.13
FTE students	6095.21	(3286.20)	4358.70	(3331.66)	1736.52
% of students white British	0.69	(0.27)	0.76	(0.23)	-0.07
% of students aged 19 and below	0.25	(0.08)	0.21	(0.07)	0.04
% of students aged 25 and above	0.59	(0.10)	0.64	(0.08)	-0.05
% of students reporting disability	0.06	(0.03)	0.06	(0.04)	0.00
% of students attending part-time	0.70	(0.11)	0.72	(0.09)	-0.01
% of students disadvantaged	0.43	(0.22)	0.38	(0.22)	0.05
Average GLH per student	196.93	(55.67)	171.57	(52.99)	25.36
% of students who achieve aim	0.70	(0.14)	0.72	(0.06)	-0.02
% of all L2 aims achieved	0.51	(0.14)	0.48	(0.10)	0.03
% grant funding for 1st grant received	0.40	(0.23)	0.61	(0.18)	-0.21**
Number of colleges	15		23		

Notes: Significance levels: * 5%, ** 1%, *** 0.1%. All variables are measured three years before grant receipt for treatment colleges, and in the same year for control colleges. Sample of colleges which received grants in the fourth quartile (Q4) of the grant distribution at some point over the period (treated colleges: between 2006-2009, control colleges: in or after 2011).

Table 3: Control/treatment balancing, Q3+Q4 grants

Variable	(1) Control mean (sd)		(2) Treatment mean (sd)		(3) Difference (1)-(2)
Total income ('000)	25036.86	(16951.29)	22468.25	(25519.11)	2568.61
Total fixed assets ('000)	26658.59	(19633.54)	18139.88	(15516.76)	8518.71
Total capital expenditure ('000)	3438.10	(4817.00)	1340.63	(1529.88)	2097.48*
Greater London	0.21	(0.41)	0.16	(0.37)	0.05
Number of unique students	12249.41	(7684.69)	11456.34	(10019.31)	793.07
FTE students	4878.18	(2873.55)	4205.63	(2962.49)	672.56
% of students white British	0.75	(0.24)	0.78	(0.22)	-0.02
% of students aged 19 and below	0.25	(0.09)	0.20	(0.08)	0.04
% of students aged 25 and above	0.60	(0.11)	0.65	(0.09)	-0.04
% of students reporting disability	0.05	(0.04)	0.07	(0.10)	-0.02
% of students attending part-time	0.71	(0.11)	0.72	(0.10)	-0.02
% of students disadvantaged	0.37	(0.21)	0.37	(0.23)	0.00
Average GLH per student	188.45	(65.06)	170.09	(50.87)	18.36
% of students who achieve aim	0.71	(0.11)	0.71	(0.07)	0.00
% of all L2 aims achieved	0.48	(0.14)	0.46	(0.12)	0.02
% grant funding for 1st grant received	0.38	(0.20)	0.51	(0.24)	-0.13*
Number of colleges	29		32		

Source: LCS, SFA and ILR. Notes: Significance levels: * 5%, ** 1%, *** 0.1%. All variables are measured three years before grant receipt for treatment colleges, and in the same year for control colleges. Sample of colleges which received grants in the third quartile or fourth quartile (Q3+Q4) of the grant distribution at some point over the period (treated colleges: between 2006-2009, control colleges: in or after 2011).

Table 4: Intake composition, Q4 grants

	KS 4 score (standardised)	Ever FSM	Ethnicity: white
Years relative to project approval			
Omitted periods: t-3 to t-1	.	.	.
t	.0888	-.00897	.00829
	(.0494)	(.00862)	(.00921)
t+1	-.00482	.0104	-.00496
	(.0584)	(.00877)	(.0132)
t+2	.0621	.00658	-.00479
	(.0516)	(.0111)	(.0162)
t+3	.147*	-.0267*	-.0081
	(.0567)	(.0128)	(.0171)
t+4	.202**	-.0442*	.0109
	(.0697)	(.0189)	(.0209)
College FE	yes	yes	yes
Grant cohort FE	yes	yes	yes
KS4 cohort FE	yes	yes	yes
Clustered FE (college level)	yes	yes	yes
Mean dep variable	-0.371	0.279	0.752
Nb. of Observations	632,213	632,213	632,213
Nb. Of Colleges	38	38	38

Note: Source: LSC, SFA, ILR and NPD. Notes: The table shows estimates from equation (1), combining the pre-approval years $\tau \in \{-3, -2, -1\}$ into a baseline group and omit the corresponding dummies ($D_{t,g-3}$, $D_{t,g-2}$ and $D_{t,g-1}$). Significance levels: * 5%, ** 1%. Sample of individuals who attended a college which received a grant in the fourth quartile (Q4) over the sample period (treated colleges: between 2006-2009, control colleges: in or after 2011).

Table 5: Intake composition, Q3+Q4 grants

	KS 4 score (standardised)	Ever FSM	Ethnicity: white
Years relative to project approval			
Omitted periods: t-3 to t-1	.	.	.
t	.0603	-.00432	-.000566
	(.0329)	(.00613)	(.00619)
t+1	-.00827	.00957	-.0111
	(.0453)	(.00682)	(.00919)
t+2	.0354	.0048	-.0124
	(.0353)	(.00755)	(.0111)
t+3	.0718	-.0159	-.0154
	(.0396)	(.00838)	(.0118)
t+4	.115*	-.0241	-.000883
	(.0525)	(.0123)	(.0158)
College FE	yes	yes	yes
Grant cohort FE	yes	yes	yes
KS4 cohort FE	yes	yes	yes
Clustered FE (college level)	yes	yes	yes
Mean dep variable	-0.384	0.255	0.766
Nb. of Observations	971,806	971,806	971,806
Nb. Of Colleges	61	61	61

Source: LSC, SFA, ILR and NPD. Notes: The table shows estimates from equation (1), combining the pre-approval years $\tau \in \{-3, -2, -1\}$ into a baseline group and omit the corresponding dummies ($D_{t,g-3}$, $D_{t,g-2}$ and $D_{t,g-1}$). Significance levels: * 5%, ** 1%. Sample of individuals who attended a college which received a grant in the third or fourth quartile (Q3+Q4) over the sample period (treated colleges: between 2006-2009, control colleges: in or after 2011).

Table 6: Further education enrolment and outcomes, ILR data, Q4 grants

	Enrolled in at least one L3 qualification		Achieved at least one L3 qualification (conditional on enrolment)		Studied towards at least one Level 4 or above qualification		Enrolled in at least one apprenticeship		Achieved at least one apprenticeship (conditional on enrolment)	
	Spec 1	Spec 2	Spec 1	Spec 2	Spec 1	Spec 2	Spec 1	Spec 2	Spec 1	Spec 2
Years relative to project approval										
t	.0237	.00929	-.0145	-.0152	.00361	.00247	.00727	.00657	-.00268	-.00163
	(.0194)	(.0146)	(.0396)	(.038)	(.00312)	(.00317)	(.0057)	(.00571)	(.0356)	(.036)
t+1	.0288	.0248	-.0254	-.028	.00175	.0015	.00232	.00231	.0204	.0194
	(.017)	(.0155)	(.0362)	(.0344)	(.00403)	(.00404)	(.00905)	(.00904)	(.0463)	(.0464)
t+2	.0241	.0144	.0345	.029	.0006	-.000063	.00578	.00582	.00707	.00789
	(.0225)	(.0204)	(.0483)	(.0471)	(.00475)	(.00467)	(.0098)	(.00962)	(.0736)	(.0737)
t+3	.0628*	.0416*	.0509	.0444	.00528	.0036	-.0146	-.0153	.019	.0224
	(.0245)	(.0195)	(.0621)	(.0602)	(.00517)	(.00506)	(.0148)	(.0148)	(.0392)	(.0388)
t+4	.0824**	.0484*	.07	.0644	.013*	.0102	-.0124	-.0145	-.0313	-.0285
	(.0274)	(.0205)	(.0568)	(.0539)	(.00641)	(.00589)	(.0131)	(.0132)	(.045)	(.0446)
College FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Grant cohort FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
KS4 cohort FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Demographics	no	yes	no	yes	no	yes	no	yes	no	yes
KS4 points	no	yes	no	yes	no	yes	no	yes	no	yes
Clustered FE (college)	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Mean dep variable	0.435	0.435	0.701	0.701	0.025	0.025	0.076	0.076	0.292	0.292
Nb. of Observations	1,073,494	1,073,494	367,096	367,096	1,073,494	1,073,494	1,110,535	1,110,535	61,097	61,097
Nb. Of Colleges	38	38	38	38	38	38	38	38	38	38

Source: LSC, SFA, ILR and NPD. Notes: The table shows estimates from equation (1), combining the pre-approval years $\tau \in \{-3, -2, -1\}$ into a baseline group and omit the corresponding dummies ($D_{t,g-3}, D_{t,g-2}$ and $D_{t,g-1}$). Significance levels: * 5%, ** 1%. Sample of individuals who attended a college which received a grant in the fourth quartile (Q4) over the sample period (treated colleges: between 2006-2009, control colleges: in or after 2011).

Table 7: Further education enrolment and outcomes, ILR data, Q3+Q4 grants

	Enrolled in at least one L3 qualification		Achieved at least one L3 qualification (conditional on enrolment)		Studied towards at least one Level 4 or above qualification		Enrolled in at least one apprenticeship		Achieved at least one apprenticeship (conditional on enrolment)	
	Spec 1	Spec 2	Spec 1	Spec 2	Spec 1	Spec 2	Spec 1	Spec 2	Spec 1	Spec 2
Years relative to project approval										
t	.0247	.0127	-.0152	-.016	.00158	.000823	.00515	.0051	.00982	.0106
	(.0135)	(.0103)	(.0292)	(.0279)	(.00262)	(.00261)	(.00442)	(.00439)	(.0247)	(.0249)
t+1	.0296*	.0257*	-.0316	-.0341	.000433	.000244	.00476	.00514	.0199	.0203
	(.0125)	(.0107)	(.0289)	(.0276)	(.00342)	(.00338)	(.00636)	(.00635)	(.0309)	(.0312)
t+2	.0295	.0222	.0101	.00642	.000631	.000268	.00801	.0086	.0178	.0186
	(.0173)	(.0152)	(.0367)	(.0355)	(.0042)	(.00412)	(.00757)	(.00747)	(.0488)	(.0492)
t+3	.0533**	.0415*	.0253	.0202	.00445	.00373	-.00836	-.00827	.0214	.0221
	(.0194)	(.0162)	(.0436)	(.042)	(.00471)	(.00463)	(.0105)	(.0105)	(.0317)	(.0318)
t+4	.0654**	.0455**	.0408	.0367	.0101	.00873	-.0147	-.0156	-.0192	-.0177
	(.0205)	(.0167)	(.0416)	(.0393)	(.00549)	(.00528)	(.0117)	(.0117)	(.0359)	(.0359)
College FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Grant cohort FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
KS4 cohort FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Demographics	no	yes	no	yes	no	yes	no	yes	no	yes
KS4 points	no	yes	no	yes	no	yes	no	yes	no	yes
Clustered FE (college)	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Mean dep variable	0.428	0. 428	0.698	0.698	0.219	0.219	0.076	0.076	0.271	0.271
Nb. of Observations	1,624,572	1,624,572	538,626	538,626	1,624,572	1,624,572	1,689,574	1,689,574	94,655	94,655
Nb. Of Colleges	61	61	61	61	61	61	61	61	61	61

Source: LSC, SFA, ILR and NPD. Notes: The table shows estimates from equation (1), combining the pre-approval years $\tau \in \{-3, -2, -1\}$ into a baseline group and omit the corresponding dummies ($D_{t,g-3}, D_{t,g-2}$ and $D_{t,g-1}$). Significance levels: * 5%, ** 1%. Sample of individuals who attended a college which received a grant in the third or fourth quartile (Q3+Q4) over the sample period (treated colleges: between 2006-2009, control colleges: in or after 2011).

Table 8: Higher education enrolment and outcomes, Q4 grants

	Progressed to Higher Education		Completed degree (conditional on enrolment)	
	Spec 1	Spec 2	Spec 1	Spec 2
Years relative to project approval				
t	.0182	.00523	-.0065	-.00726
	(.0124)	(.00638)	(.0123)	(.0125)
t+1	.00433	.00387	-.0158	-.0165
	(.0122)	(.0077)	(.0142)	(.0147)
t+2	.0241	.0135	.00277	.00226
	(.0147)	(.00852)	(.0188)	(.0175)
t+3	.0515*	.034**	.0123	.00488
	(.0215)	(.0113)	(.0263)	(.0219)
t+4	.0453	.0267	.0222	.021
	(.0246)	(.0137)	(.0331)	(.0289)
College FE	yes	yes	yes	yes
Grant cohort FE	yes	yes	yes	yes
KS4 cohort FE	yes	yes	yes	yes
Demographics	no	yes	no	yes
KS4 points	no	yes	no	yes
Clustered FE (college level)	yes	yes	yes	yes
Mean dep variable	0.279	0.279	0.676	0.676
Nb. of Observations	517,966	517,966	121,282	121,282
Nb. Of Colleges	38	38	38	38

Source: LSC, SFA, ILR, NPD and HESA. Notes: The table shows estimates from equation (1), combining the pre-approval years $\tau \in \{-3, -2, -1\}$ into a baseline group and omit the corresponding dummies ($D_{t,g-3}, D_{t,g-2}$ and $D_{t,g-1}$). Significance levels: * 5%, ** 1%. Sample of individuals who attended a college which received a grant in the fourth quartile (Q4) over the sample period (treated colleges: between 2006-2009, control colleges: in or after 2011).

Table 9: Higher education enrolment and outcomes, Q3+Q4 grants

	Progressed to Higher Education		Completed degree (conditional on enrolment)	
	Spec 1	Spec 2	Spec 1	Spec 2
Years relative to project approval				
t	.0196	.00899	.00612	.00277
	(.0101)	(.00661)	(.0117)	(.0107)
t+1	.0122	.0104	-.00456	-.00838
	(.0112)	(.00803)	(.012)	(.0113)
t+2	.0217	.0146	.0153	.012
	(.0109)	(.00802)	(.0152)	(.014)
t+3	.0351**	.0276**	.0069	.00187
	(.0124)	(.00807)	(.0163)	(.0145)
t+4	.0211	.0104	.0191	.0193
	(.0154)	(.0106)	(.0209)	(.0186)
College FE	yes	yes	yes	yes
Grant cohort FE	yes	yes	yes	yes
KS4 cohort FE	yes	yes	yes	yes
Demographics	yes	yes	yes	yes
KS4 points	no	yes	no	yes
Clustered FE (college level)	yes	yes	yes	yes
Mean dep variable	0.267	0.267	0.670	0.670
Nb. of Observations	822,802	822,802	184,654	184,654
Nb. Of Colleges	61	61	61	61

Source: LSC, SFA, ILR, NPD and HESA. Notes: The table shows estimates from equation (1), combining the pre-approval years $\tau \in \{-3,-2,-1\}$ into a baseline group and omit the corresponding dummies ($D_{t,g-3}$, $D_{t,g-2}$ and $D_{t,g-1}$). Significance levels: * 5%, ** 1%. Sample of individuals who attended a college which received a grant in the third or fourth quartile (Q3+Q4) over the sample period (treated colleges: between 2006-2009, control colleges: in or after 2011).

Table 10: OLS and 2SLS regressions for effect of total fixed assets on student outcomes, Q4 grants

	Enrolled in a Level 3 course		Progressed to HE		Employed (more than 90 days) 2 years after leaving college	
	OLS	2SLS	OLS	2SLS	OLS	2SLS
Log total fixed assets	.0149	.0328	.00696	.0209*	.000233	.0104
	(.00818)	(.0194)	(.00609)	(.0101)	(.00265)	(.00574)
College FE	yes	yes	yes	yes	yes	yes
Grant cohort FE	yes	yes	yes	yes	yes	yes
KS4 cohort FE	yes	yes	yes	yes	yes	yes
Demographics	yes	yes	yes	yes	yes	yes
KS4 points	yes	yes	yes	yes	yes	yes
prior work experience	-	-	-	-	yes	yes
Clustered FE (college level)	yes	yes	yes	yes	yes	yes
Mean dep var	0.435		0.279		0.605	
Nb. of Observations	1,073,494	1,073,494	517,966	517,966	568,423	568,423
Nb. Of Colleges	38	38	38	38	38	38

Source: LSC, SFA, ILR, NPD and HESA. Notes: The table shows estimates from equation (3). Significance: *5%; **1%; ***0.1%. Results only reported when there is evidence of a significant effect in the reduced form regressions. Instruments are dummies for year since project start. First stage regression coefficients for Column 2 (from Equation 2) are as follows:

t: 0.296 (0.161)
t+1: 0.78*** (0.193)
t+2: 1.07*** (0.201)
t+3: 1.06*** (0.211)
t+4: 1.09*** (0.22)
F-Stat: 6.222 (Kleibergen-Paap Wald rk F statistic)
First stages are broadly similar for other regression samples.

Table 11: OLS and 2SLS regressions for effect of total fixed assets on student outcomes, Q3+Q4 grants

	Enrolled in a Level 3 course		Progressed to HE		Employed (more than 90 days) 2 years after leaving college	
	OLS	2SLS	OLS	2SLS	OLS	2SLS
Log total fixed assets	.0131	.038*	.00474	.0175*	-.00138	.00508
	(.00813)	(.0184)	(.00497)	(.00814)	(.0028)	(.00578)
College FE	yes	yes	yes	yes	yes	yes
Grant cohort FE	yes	yes	yes	yes	yes	yes
KS4 cohort FE	yes	yes	yes	yes	yes	yes
Demographics	yes	yes	yes	yes	yes	yes
KS4 points	yes	yes	yes	yes	yes	yes
prior work experience	-	-	-	-	yes	yes
Clustered FE (college level)	yes	yes	yes	yes	yes	yes
Mean dep var	0.428		0.267		0.627	
Nb. of Observations	1,624,572	1,624,572	822,802	822,802	878,281	878,281
Nb. Of Colleges	61	61	61	61	61	61

Source: LSC, SFA, ILR, NPD and HESA. Notes: The table shows estimates from equation (3). Significance: *5%; **1%; ***0.1%. Results only reported when there is evidence of a significant effect in the reduced form regressions. Instruments are dummies for year since project start. First stage regression coefficients for Column 2 (from Equation 2) are as follows:

t: 0.244* (0.161)
t+1: 0.671*** (0.145)
t+2: 0.942*** (0.152)
t+3: 0.925*** (0.158)
t+4: 0.936*** (0.159)
F-Stat: 9.02 (Kleibergen-Paap Wald rk F statistic)
First stages are broadly similar for other regression samples.

Table 12: Labour market outcomes, Q4 grants

	Employed (more than 90 days) 1 year after leaving college		Employed (more than 90 days) 2 years after leaving college		Real annual earnings (zero for non-employed) 1 year after leaving college		Real annual earnings (zero for non-employed) 2 years after leaving college	
	Spec 1	Spec 2	Spec 1	Spec 2	Spec 1	Spec 2	Spec 1	Spec 2
Years relative to project approval								
t	.0291	.00948	.0296*	.0119	49.7	-37.2	176	105
	(.0156)	(.00745)	(.0135)	(.00649)	(169)	(128)	(182)	(140)
t+1	.00479	.00837	.00968	.0108	-72.3	-13.8	19.3	42.4
	(.0123)	(.00677)	(.0139)	(.00885)	(167)	(138)	(174)	(142)
t+2	.00908	.00456	.0138	.00979	-146	-118	-48.9	-23.7
	(.0117)	(.00714)	(.012)	(.00639)	(147)	(127)	(173)	(151)
t+3	.0155	-.000278	.0236	.00829	-212	-305*	-62.1	-121
	(.0136)	(.00637)	(.0154)	(.006)	(143)	(136)	(144)	(134)
t+4	.0456**	.0143*	.049**	.0201*	312	84.4	243	53.7
	(.0161)	(.00622)	(.0168)	(.00952)	(179)	(126)	(179)	(139)
College FE	yes	yes	yes	yes	yes	yes	yes	yes
Grant cohort FE	yes	yes	yes	yes	yes	yes	yes	yes
KS4 cohort FE	yes	yes	yes	yes	yes	yes	yes	yes
Demographics	no	yes	no	yes	yes	yes	yes	yes
KS4 points	no	yes	no	yes	yes	yes	yes	yes
Prior work experience	no	yes	no	yes	no	yes	no	yes
Clustered FE (college level)	yes	yes	yes	yes	yes	yes	yes	yes
Mean dep variable	0.58	0.58	0.62	0.62	7,695	7,695	9,404	9,404
Nb. of Observations	568,453	568,453	568,423	568,423	274,700	274,700	299,973	299,973
Nb. Of Colleges	38	38	38	38	38	38	38	38

Source: LSC, SFA, ILR, NPD and HMRC/DWP. Notes: The table shows estimates from equation (1), combining the pre-approval years $\tau \in \{-3,-2,-1\}$ into a baseline group and omit the corresponding dummies ($D_{t,g-3}, D_{t,g-2}$ and $D_{t,g-1}$). Significance levels: * 5%, ** 1%. Sample of individuals who attended a college which received a grant in the fourth quartile (Q4) over the sample period (treated colleges: between 2006-2009, control colleges: in or after 2011).

Table 13: Labour market outcomes, Q3+Q4 grants

	Employed (more than 90 days) 1 year after leaving college		Employed (more than 90 days) 2 years after leaving college		Real annual earnings (zero for non-employed) 1 year after leaving college		Real annual earnings (zero for non-employed) 2 years after leaving college	
	Spec 1	Spec 2	Spec 1	Spec 2	Spec 1	Spec 2	Spec 1	Spec 2
Years relative to project approval								
T	.0149	.00473	.017	.00698	-175	-216	-22.4	-42.9
	(.0116)	(.00543)	(.0102)	(.00508)	(202)	(161)	(189)	(142)
t+1	-.00647	-.00107	-.000212	.00234	-239	-172	-89.7	-51.5
	(.00965)	(.00548)	(.0111)	(.00696)	(225)	(173)	(209)	(162)
t+2	.00141	-.00118	.00575	.00143	-193	-163	-85.6	-52.8
	(.00911)	(.00571)	(.00966)	(.00602)	(216)	(154)	(203)	(157)
t+3	.00493	-.00286	.012	.00167	-39.3	-50.1	59.1	60.8
	(.0108)	(.00596)	(.0116)	(.00613)	(218)	(187)	(182)	(154)
t+4	.0288*	.0106	.0367**	.0149	385*	283*	376*	261*
	(.0123)	(.00642)	(.0134)	(.00803)	(172)	(139)	(143)	(122)
College FE	yes	yes	yes	yes	yes	yes	yes	yes
Grant cohort FE	yes	yes	yes	yes	yes	yes	yes	yes
KS4 cohort FE	yes	yes	yes	yes	yes	yes	yes	yes
Demographics	no	yes	no	yes	no	yes	no	yes
KS4 points	no	yes	no	yes	no	yes	no	yes
Prior work experience	no	yes	no	yes	no	yes	no	yes
Clustered FE (college level)	yes	yes	yes	yes	yes	yes	yes	yes
Mean dep variable	0.60	0.60	0.627	0.627	7,695	7,695	9,404	9,404
Nb. of Observations	878,324	878,324	878,281	878,281	439,900	439,900	480,230	480,230
Nb. Of Colleges	61	61	61	61	61	61	61	61

Source: LSC, SFA, ILR, NPD and HMRC/DWP. Notes: The table shows estimates from equation (1), combining the pre-approval years $\tau \in \{-3, -2, -1\}$ into a baseline group and omit the corresponding dummies ($D_{t,g-3}, D_{t,g-2}$ and $D_{t,g-1}$). Significance levels: * 5%, ** 1%. Sample of individuals who attended a college which received a grant in the third or fourth quartile (Q3+Q4) over the sample period (treated colleges: between 2006-2009, control colleges: in or after 2011).

Appendix

Table 14: Effect of grant approval on college-level outcomes, Q4 grants

	(1)	(2)	(3)	(4)	(5)	(6)
	Ln Capex	Ln Fixed Assets	Capex	Fixed Assets	Ln Operating Expenditure	Total FTE students
Years relative to project approval						
t-3	-.387	-.0146	166	-65.9	.0272	357
	(.365)	(.0852)	(1219)	(2906)	(.0397)	(425)
t-2	-.323	.0486	1308	1414	.0326	98.1
	(.277)	(.0891)	(1084)	(2988)	(.0306)	(146)
t-1	.	.	.	.	.	.
t	.753*	.204	1209	2535	.00695	16
	(.278)	(.12)	(1540)	(2751)	(.0256)	(213)
t+1	2.02***	.767***	14586***	14614***	.0216	-154
	(.344)	(.169)	(3060)	(4019)	(.0444)	(225)
t+2	2.13***	1.19***	15099***	28152***	.00537	-119
	(.421)	(.178)	(2870)	(4841)	(.0525)	(232)
t+3	1.24*	1.15***	5352*	30035***	.0531	1.33
	(.466)	(.21)	(1983)	(5106)	(.0655)	(249)
t+4	.00719	1.23***	2086	34074***	.111	-18.2
	(.459)	(.264)	(1504)	(6273)	(.096)	(792)
Adjusted R-squared	0.487	0.649	0.439	0.609	0.289	0.384
Nb. of colleges	346	346	346	346	343	356

Source: LSC, SFA and ILR. Notes: The table shows estimates from equation (1). Significance levels: * 5%, ** 1%. Sample of colleges which received a grant in the fourth quartile (Q4) over the sample period (treated colleges: between 2006-2009, control colleges: in or after 2011).

Table 15: Effect of grant approval on college-level outcomes, Q3+Q4 grants

	(1)	(2)	(3)	(4)	(5)	(6)
	Ln Capex	Ln Fixed Assets	Capex	Fixed Assets	Ln Operating Expenditure	Total FTE students
Years relative to project approval						
t-3	-.557	-.0223	-613	-385	.0433	149
	(.284)	(.0624)	(769)	(2057)	(.0281)	(272)
t-2	-.454*	.00495	280	47.2	.0232	-32.6
	(.22)	(.0603)	(640)	(1961)	(.021)	(87.5)
t-1	.	.	.	.	.	.
t	.777**	.191*	1563	2757	.0101	-4.79
	(.234)	(.0772)	(951)	(1578)	(.016)	(135)
t+1	1.69***	.652***	11379***	13143***	.00905	-135
	(.313)	(.122)	(2216)	(2803)	(.03)	(145)
t+2	1.57***	1***	11863***	24666***	-.0172	-150
	(.367)	(.136)	(2284)	(3696)	(.0404)	(150)
t+3	.889*	.962***	3880**	25546***	.0432	-167
	(.365)	(.151)	(1379)	(4040)	(.0429)	(186)
t+4	-.0236	.958***	1492	25584***	.0714	-303
	(.322)	(.171)	(1124)	(5157)	(.0595)	(506)
Adjusted R-squared	0.343	0.561	0.363	0.508	0.417	0.422
Observations	573	573	573	573	567	590

Source: LSC, SFA and ILR. Notes: The table shows estimates from equation (1). Significance levels: * 5%, ** 1%. Sample of colleges which received a grant in the third or fourth quartile (Q3+Q4) over the sample period (treated colleges: between 2006-2009, control colleges: in or after 2011).