DO CAPITAL INCENTIVES DISTORT TECHNOLOGY DIFFUSION? EVIDENCE ON CLOUD, BIG DATA AND AI.

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Abstract

The arrival of cloud computing means firms can access digital technologies as a service. Yet, capital incentive policies remain targeted towards investments in physical information technology (IT) capital. If cloud services are partial substitutes for IT capital, these policies may skew technology adoption. Exploiting a UK capital tax incentive as a quasi-natural experiment, we find that while the policy increased IT capital investment as intended, it slowed firm adoption of cloud computing and technologies that rely on the cloud, including big data analytics and artificial intelligence. Matched employer-employee data show a corresponding reduction in demand for data analytics workers.

Keywords: Capital incentives, Firms, Cloud computing, Artificial Intelligence
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1 Introduction

Policy tools have long been known to influence production technologies by incentivizing certain outputs, such as innovation or exports, and encouraging the use of specific inputs, such as local sourcing or capital investment (Juhasz et al., 2023).[1] Once enacted, such policies tend to persist (Bonomo et al., 2015), which can have unintended consequences for the path of technological change. For instance, lower taxes on capital (relative to labor income) may have accelerated the diffusion of labor-saving automation in the US (Acemoglu et al., 2020). In this paper, we add to these examples by exploring whether capital incentives, designed to encourage investment in physical information technology (IT), alter the adoption of cloud computing, big data analytics, and artificial intelligence (AI).[2]

Historically, firms mostly acquired new technologies through purchases of capital, including computers and servers (Comin and Hobijn, 2010; Jones and Liu, 2022). To encourage such investments and spur growth, policy makers frequently use capital incentive programs. These policies work by reducing the cost of capital and are in place in every OECD country, whether it be in the form of tax allowances, subsidies or grants (Jorgenson, 1963; Hall and Jorgenson, 1967; Tax Foundation, 2018).[3] A growing body of evidence confirms their effectiveness at stimulating investment, particularly for small and financially constrained firms (Cummins et al., 1994; House and Shapiro, 2008; Zwick and Mahon, 2017; Ohrn, 2018; Maffini et al., 2019). But how these incentives affect technology adoption, especially in a world of cloud computing services, has not previously been explored.

In recent years, the creation of cloud computing has fundamentally changed how firms can access digital technologies. Following the launch of Amazon Web Services in 2006, firms are now also able to acquire data storage, computing and software services through the cloud, substituting away from upfront investments in tangible digital technologies (DeStefano et al., 2025; OECD, 2014). Firms can rent a server from a cloud provider, for example, rather than buying it, such that IT services become a variable cost to the firm, available on demand and flexibly at any scale.[4] There are often also tax advantages for firms to purchase digital services in this way. Cloud service expenditures can also be immediately expensed for tax

[1] These industrial policies are attracting attention in view of the need to shift towards low-carbon production technologies (European Commission, 2023).

[2] Our AI measure reflects machine learning and natural language processing, our findings therefore speak to the diffusion of these AI forms rather than generative AI, which emerged after our sample period.

[3] There is also a literature studying how differences in the tax system, in particular around corporate income taxes, can be used to attract foreign multinational firms (Hines Jr, 1999; Devereux and Griffith, 1998; Devereux and Maffini, 2007).

[4] The diffusion of this new mode of accessing information technology (IT) has been remarkably rapid, with expenditure on cloud services accounting for approximately 25% of European firms' IT budgets in 2016. (Van Ark, 2016; OECD, 2017; Eurostat, 2018).

purposes, while, in the absence of capital incentive policies, IT capital investments cannot. This raises questions of whether capital incentive policies tilt technology choice back toward upfront purchases of physical IT. Since earlier technological vintages shape the path of future technology use, the impacts may extend beyond cloud adoption itself, and affect the diffusion of data technologies complementary to cloud, such as big data analytics and AI (Jovanovic and Lach, 1989; Atkeson and Kehoe, 2007; Comin and Hobijn, 2010; Jovanovic and Yatsenko, 2012).

This paper presents the first evidence of whether capital incentives affect patterns of IT technology diffusion across firms. We employ novel firm-level panel data which captures the adoption of new digital technologies, along with traditional investments in tangible capital, including total IT investment, as well as hardware and software.[5] In addition, we use matched employer-employee panel data to measure the wages of data analytics workers as an alternative proxy for technology adoption following Corrado et al. (2022).[6] If data technologies are complementary to data analytics skills (Brynjolfsson and McElheran, 2016), any impacts of the AIA policy on firm-level adoption of data analytics technologies should be mirrored in their demand for workers performing data analytics.

We study capital investment and technology diffusion in the setting of a quasi-natural experiment that exploits the introduction and adjustments to the eligibility threshold for a capital incentive policy in the UK, the Annual Investment Allowance (AIA). This scheme allowed firms to deduct the cost of investment in capital (including IT capital) against profits up to a threshold value, where this threshold was adjusted over time. These AIA changes impacted the marginal investment cost of some firms and not others, allowing us to use a difference-in-differences approach comparing treated versus control firms.[7]

As we describe in more detail in the next section of the paper, the policy eliminated the tax advantage of expensing cloud services relative to purchasing physical IT capital, at least up to the value of the threshold, steering firms toward tangible IT investment and away from cloud computing. For firms above the threshold, the tax advantage of purchasing cloud services remains. The effect of capital incentives on big data analytics and AI then

[5] We note that software is considered an intangible asset, but was nonetheless eligible for the AIA policy in addition to conventional tangible assets, as discussed in Section 2.

[6] Corrado et al. (2022) use changes in total salaries of workers who perform data tasks to construct economy-level time series of data investments. In our context we use the changes in wages of workers who perform data tasks as a measure of diffusion of firm-level data technologies.

[7] The introduction of the AIA will of course have affected the average tax rate for both treatment and control firms. However, as Fullerton (1984) writes, "average effective tax rate are appropriate for measuring cash flows and distributional burdens, while marginal effective tax rates are designed to encourage the use of new capital" (p. 30), indicating that it is the marginal rate that is relevant here. That the change in the marginal tax rate is zero for firms above the capital investment threshold explains why we do not use a regression kink design (Card et al., 2015).

depends on the extent to which firms can use their own IT capital, or rather, whether there are advantages to the flexible, scalable computing power that cloud provides. To the extent that there are (as suggested by Brown et al. (2011), Goldfarb et al. (2023) and DeStefano et al. (2025)), slower cloud adoption cascades into slower diffusion of big data and AI. Cloud, big data, and AI have been widely regarded as possible general purpose technologies, so that the answer to this question has broad potential implications for future employment growth, innovation, and productivity (Brynjolfsson and McAfee, 2014; Goldfarb et al., 2023).

A key identification challenge is that firms may adjust investment to remain below AIA thresholds, creating a selection problem. To address this issue, we follow the empirical approach of Bjuggren (2018), Saez et al. (2019) and Bøler et al. (2015), who examine the effects of employment protection legislation or R&D tax credits in settings that also feature changes in qualifying thresholds. Similar to these studies, we define treatment status using firms' historic (pre-AIA) capital investment, which we compare to (future) AIA tax-allowance thresholds.[8] In this way we obtain reduced form estimates of the intention to treat effects of this tax policy.[9]

Our empirical results show that firms whose marginal cost of capital fell due to AIA threshold increases were much more likely to increase tangible capital investment, compared to non-treated firms. Over the period 2007 to 2013, the policy increased total capital investment by 61.7%, with similar strong effects apparent up to the end of our sample period in 2019. While the elasticity of investment with respect to the user cost of capital implied by these estimates is large, this magnitude is consistent with estimates of an earlier change in UK depreciation allowances (Maffini et al., 2019) and estimates for the US (House and Shapiro, 2008; Zwick and Mahon, 2017), and reflects the fact that the AIA represented a substantial reduction in the marginal cost of capital for eligible firms, particularly smaller ones for whom the threshold was most binding. Treated firms also increase their investments in IT hardware and software, but there is no change in investment in assets that were ineligible for the incentive (such as land and buildings).

Importantly, we find that the AIA slowed adoption of cloud technologies, confirming that the capital incentive did alter technology diffusion. Cloud adoption was 17 percentage points

[8]Recognizing that there may be adjustment costs in reaching the desired capital stock for the firm (Chirinko, 1993), in our baseline estimations we average capital investment across time.

[9]As explained in the data section of the paper, the data on capital investment is available at a higher time frequency (annual) than the firm-level data on the digital technologies. To account for the staggered treatment design features of the AIA, in the regressions for capital investment we rely on Callaway and Sant'Anna (2021). These staggered treatment estimation methods are not possible with highly unbalanced panels, so for the digital technology variables we use long-difference regressions. Our results are also robust to using long-difference specifications throughout. However, our additional proxy for technology adoption, the demand for data analytics workers uses annual data, which we estimate following Callaway and Sant'Anna (2021). We return to this point in more detail in Section 4.

lower for firms eligible for the AIA, compared to a mean rate of cloud adoption of 28% over the same period. The negative effects of the capital incentive program are also found to be stronger for cloud data and storage services – those related to the storage and processing of data – than compared to accessing office and finance software or email through the cloud. Some back-of-the-envelope estimates of the aggregate impact suggest that the policy reduced overall cloud use in the UK by between 7 and 9 percentage points, or equivalently, the AIA slowed cloud diffusion by more than one year.

Moving to the impact of capital incentives on other data technologies, we find evidence that the AIA policy also lowered the likelihood of using big data analytics and AI, by 18 percentage points and 3 percentage points, respectively. For these technologies, back-of-the-envelope estimates suggest an aggregate slowdown in big data and AI use in the UK of 7 and 1 percentage points. While seemingly small, the slowdown is large compared to the level of big data and AI use in the UK, slowing their diffusion between one and two years. We estimate the use of big data would have been 14% higher and AI 30% higher in the absence of the capital incentive policy. These findings are consistent with cloud computing being complementary to the use of big data and AI.

To capture the extent of how these data technologies are used within the firm, we examine the effect of the AIA on the wages of workers performing data analytics tasks. Under the assumption that digital technologies are strongly biased towards specific skills (Brynjolfsson and McElheran, 2016), slower diffusion of data technologies (like cloud, big data and AI) should reduce firms' demand for such workers. Consistent with this, we find that firms treated by the AIA policy reduced the wages of workers in occupations that perform data analytics by 1.5% to 1.6% compared to non-treated firms. Importantly, we do not find an impact of the policy on other types of workers within the same firms, such as those who do not perform data tasks or those working on database creation. Thus, we do not see that the AIA affects labor demand in general, but rather reduces demand for data analytics workers, consistent with our evidence that the policy slowed the diffusion of new complementary digital technologies.

A key concern when assessing the effects of capital incentives on technology adoption is that treated and untreated firms may differ, most notably in size, raising the possibility that our estimates reflect underlying firm characteristics rather than the policy itself. We address this concern through a comprehensive set of robustness and falsification exercises. First, we show that treated and control firms exhibit parallel pre-policy trends for a host of annual outcomes including the demand for data analytics workers and IT services expenditures (a proxy for cloud adoption in the literature DeStefano et al. (2025)), as well as close antecedents to digital technology adoption, including PCs per employee, employment, sales

and labor productivity. [10]. Second, we conduct placebo and negative-control tests examining ineligible investment categories and cloud services unlikely to substitute for in-house IT capital—which yield no comparable effects. Third, we implement a difference-in-discontinuities design (Dechezleprêtre et al., 2016) that compares firms just above and below the relevant AIA thresholds before and after policy changes, isolating variation that is plausibly orthogonal to firm size and long-run technology trajectories. Fourth, we re-estimate our main results using a range of design restrictions that limit comparisons to more homogeneous firms, including employment-band-by-year controls, trimming of the largest and smallest firms, and specifications that focus on firms near the relevant investment thresholds. Taken together, these tests substantially limit the scope for differential firm size or pre-existing technology trajectories to explain our findings.

This paper contributes primarily to the literature on the path-dependencies in technology diffusion and how policy can be used to direct technological change. The direction of technological change exhibits path-dependency because new technologies often build on prior experience or share prerequisites (Acemoglu et al., 2012; Aghion et al., 2016). Policy can play an important role in redirecting technological change where the market outcome is not socially optimal, e.g. away from carbon-intensive or labor-replacing technologies (Aghion et al., 2016; Hémous and Olsen, 2021; Acemoglu, 2023).[11] This literature has also shown that temporary policies can have long-term impacts on technology use, such as temporary restrictions on foreign competition during the Napoleonic war and the diffusion of cotton spinning, or how temporary input cost advantages shaped the transition from wooden to metal shipbuilding (Juhasz, 2018; Hanlon, 2019). We add to this literature by showing that capital incentive policies can inadvertently affect the direction of technological adoption in ways that are at odds with their overall objectives.

A second literature, to which our paper contributes, examines the diffusion and performance effects of data technologies. The value of data assets in 9 European countries, measured by the total wages of occupations that perform data tasks, increased by more than 40% between 2010 and 2019 (Corrado et al., 2022). In US manufacturing for example, the use of data-driven decision making nearly tripled from 11% to 30% between 2005 and 2010 (Brynjolfsson and McElheran, 2016). In the EU, the use of big data analytics by firms expanded from 9% to 14% between 2015 and 2019 and the use of AI reached 6% by 2020 (OECD, 2023). Cloud computing appears to be an important driver for the adoption of

[10] We acknowledge that because cloud, big data, and AI were not measured prior to the AIA's introduction, we cannot demonstrate parallel pre-trends for these outcomes directly and therefore rely on examining worker level technology measures and close available antecedents

[11] This fits into a broader debate about the merits of capital incentives or alternative types of policy around innovation (Mazzucato, 2017; Bloom et al., 2013; Howell, 2017; Bloom et al., 2019).

big data and AI (Cho et al., 2023; DeStefano et al., 2025; Zolas et al., 2020) as it lowers the cost of collecting, storing and processing large amounts of information (Brown et al., 2011). Adoption of these data technologies has been linked to a variety of firm performance gains, including productivity, innovation and employment (Brynjolfsson and McElheran, 2016; Niebel, 2018; Koning et al., 2022). Policies that discourage the use of cloud services, and thus big data and AI, are of broader consequence if they hinder competitiveness at the firm level and productivity growth at the macro level.

Finally, we contribute additional evidence to the literature on the effects of tax incentives on subsidized capital investment. Within this literature, House and Shapiro (2008), Ohrn (2018) and Zwick and Mahon (2017) have all shown how tax policy, such as accelerated depreciation, can stimulate firm investment by reducing the cost of capital. For the UK, Maffini et al. (2019) examine the impact of an earlier accelerated depreciation policy on capital investment, also using changes in qualification thresholds to identify causal effects as we do in this paper.[12] Also for the UK, Gaggl and Wright (2017) examine the impact of a short-lived tax allowance on IT investment available to small firms, studying its effects on both IT investment and employment. In this paper, we first examine the impact of this particular program on investment, the intention of the policy, but distinguish from the above papers by also considering the broader unintended impacts on technology adoption.

The rest of the paper continues as follows: Section 2 presents details on the AIA policy, Section 3 describes the data and Section 4 lays out our estimation strategy. Section 5 presents the main results of the paper while Section 6 summarizes the results and provides some policy discussion.

2 Annual Investment Allowance Policy

The Annual Investment Allowance was introduced in the UK for the financial year (April) 2008-2009, with the objective of stimulating firms to invest in new forms of tangible capital and thereby encouraging economic growth (HMRC, 2018).[13] The scheme allowed firms to deduct their capital investment from their pre-tax profits up to an investment ceiling during their accounting year . As we discuss below, this ceiling has shifted a number of times over the sample period. The AIA incentive covered all long-term equipment used to produce or

[12]Our data do not allow us to measure financial constraints at the level of the firm that feature in their paper and so we cannot consider whether the effects of this policy were stronger on more or less constrained firms. The data also do not allow us to measure precisely firms' marginal tax rates as in (Maffini et al., 2019).

[13]The AIA was seen as a movement within UK tax policy away from a size or legal form linked incentive, towards one targeting investment (Crawford and Freedman, 2010). We consider earlier incentive schemes later in this section.

sell products – termed "plant and machinery" – which includes IT capital. Other types of capital, such as land and buildings, were not eligible for the allowance, which we use later as a test of the validity of our empirical approach.[14]

The policy was first mentioned in a March 2007 budget press notice one year prior to the start of the new allowance.[15] The policy appears to have been unanticipated before that point, with contemporaneous news headlines of the type "Budget 2007: Surprise overhaul announced for capital allowances from 2008".[16] As discussed later, we use firm investment in years prior to this announcement to determine our treatment and control groups.

Since its introduction, the AIA investment ceiling has changed many times (See Table 1). The initial ceiling for the financial year ending April 2009 was set at £50,000. In March 2010 it was announced this would increase to £100,000. A change in government then occurred in May of that year, and following a special budget in June 2010, it was announced the AIA ceiling would subsequently be cut to £25,000, effective from April 2012. This new lower threshold was in place for a period of only nine months (April 2012 to December 2012), when the government announced in the 2012 Autumn Statement there would be a temporary two-year ten-fold increase to £250,000 (effective from January 2013). The time period for this temporary increase was extended until January 2016 and at the same time the ceiling increased further to £500,000 in the 2014 Budget. A further demonstration of the uncertainty over the direction of future changes in this allowance is highlighted by noting that the 2015 election manifesto by the Conservative Party, which had formed the incumbent ruling party, stated that if elected, the supposedly temporary increase it had announced the year earlier would in fact be retained at a permanently higher, but unspecified, level. In the 2015 Budget, the AIA investment ceiling was then lowered to a "permanent" level of £200,000 (to start from January 2016). In the 2018 Budget it was announced this would increase to £1 million from January 2019. Not surprisingly, this approach to tax policy has been much criticized within the economic community (Miller and Pope, 2015).[17]

Neoclassical investment theory suggests that firms make capital investments in order to adjust to their optimal level of capital, which in turn depends on the cost of capital. The introduction and later changes in the AIA threshold lowered the cost of capital for some businesses, encouraging new investment. Harper and Liu (2013) also note that if internal financing is less costly than external financing, the AIA would have further positive effects

[14] A list of eligible and ineligible capital expenditure is contained here https://www.gov.uk/capital-allowances/what-you-can-claim-on

[15] Treasury (2007) – Press Notice 1.

[16] Available at https://www.accountingweb.co.uk/tax/hmrc-policy/budget-2007-surprise-overhaul-announced-for-capital-allowances-from-2008.

[17] Miller and Pope (2015) write 'In an example of how not to design the tax system, the annual investment allowance was decreased and then increased twice for a temporary period.' pp. 328.

TABLE 1. Annual Investment Allowance Ceiling, 2008 to 2019

	AIA Ceiling
March 2008 and Before	-
April 2008 – March 2010	£50,000
April 2010 - March 2012	£100,000
April 2012 - December 2012	£25,000
January 2013 - March 2014	£250,000
April 2014 - December 2015	£500,000
January 2016 - December 2018	£200,000
January 2019 Onwards	£1,000,000

Source: https://www.gov.uk/capital-allowances/annual-investment-allowance

on investment spending for financially constrained firms.

To understand how the AIA affects the choice between cloud and IT investment, consider a simplified example where the IT capital services provided by cloud services and physical IT capital are equivalent. With no capital incentive policy, the purchase of cloud services by firms is deductible in full in the year in which they incur, known as immediate expensing, whereas those in IT capital investments are not. Such a tax system encourages firms to access the IT they require as cloud services. Under the AIA this is altered. For investments in IT capital below the AIA investment threshold, the tax relief offered by the AIA mirrors the immediate expensing offered by purchasing cloud expenses. The AIA therefore removes the tax advantage of cloud that occurs in the absence of the AIA and physical hardware purchases are tax-equivalent to a cloud subscription. When cloud/investment spending is above the AIA threshold, then the tax advantage of cloud returns owing to the full annual deductibility. The tax system therefore is a potential determinant of the choice between capital or equivalent service expenditures, or it may encourage firms to spread hardware purchases across time.

Several further details about the AIA allowance will be important for the later empirical analysis. Firstly, the available allowance to any given firm in any given year depends on the timing of their financial year end.[18] For instance, firms with a financial year ending March 2010 would be eligible for an allowance of £50,000. However, firms with a financial year ending December 2010 would receive an allowance of £87,500, since they incur 3 months of the £50,000 allowance (until March 2010) and the remaining 9 months of the £100,000 allowance (from April 2010). Just over half of firms have a December (53%) year end in

[18] See https://www.gov.uk/capital-allowances/annual-investment-allowance

our data, while a further 16% have a March year end, with the remainder distributed fairly evenly among the remaining months. We use information in our data on the date of each firm's financial year end to calculate the AIA ceiling specific to each firm for a given year. This ensures that the capital allowance available to a firm matches the frequency with which their investment and digital technology use are reported in the data.[19]

Secondly, the effects of capital investment programs also depend on expectations about the future. As already mentioned, throughout the life of the policy (2008-2019), the AIA ceilings changed a number of times. These changes often occurred unexpectedly and were sometimes announced as being only temporary. As noted earlier, the initial policy introduction also appears to have been unanticipated. These policy changes therefore present an ideal context for the assessment of its impact.

Finally, while the introduction and changes to the AIA are expected to influence firm investment decisions, it is important to identify the presence of other policies during our sample period which may confound the results. We have identified two of interest. One potential policy was the First Year Allowance (FYA). The FYA was also a capital investment program introduced before our sample period and then ended in 2008, making a reappearance in 2010 for one year. This policy was similar to the AIA in that it provided tax allowances for investments in physical capital, but were targeted at small firms with revenue below £22.8 million.[20] To ensure that our results are only capturing the effects of the AIA, and not any residual impacts of the FYA, as a robustness test we exclude firms in our sample with revenue below the threshold necessary to qualify for the FYA.

A second policy of interest was an alteration to the definition of SMEs by the EU in 2008 which in turn affected qualification for the R&D Tax Relief Scheme for UK firms. This definition change shifted the qualifying threshold of assets from €43 million to €86 million, the employment threshold from 249 to 499, and the sales threshold from €50 million to €100 million (Dechezleprêtre et al., 2016). Again, we explore the robustness of our findings to the exclusion of firms that became eligible for the R&D incentive.[21]

[19] Although cloud providers themselves would also in principle be eligible for the AIA incentive, the tax incentive applies to investments up to the threshold, which are many orders smaller than the scale of cloud provider investments in data centers and the like.

[20] See Maffini et al. (2019) for further discussions on the FYA.

[21] During our sample period the UK did not have any policies that were specifically targeted at digital technologies beyond the AIA. A capital incentive scheme targeting IT investment by small businesses was in place between 1st April 2000 to 31st March 2004 which was empirically investigated by Gaggl and Wright (2017).

3 Data and Summary Statistics

The research relies on four types of data: panel data on firm use of cloud, big data and AI technologies; matched employer-employee data; details regarding the introduction and changes to the AIA; and firm capital investment data. Table 2 provides a description of the datasets used in this research.

All firm level data are taken from the Office for National Statistics (ONS), which is the UK's Census Bureau equivalent. Information on cloud, big data analytics and AI adoption is available through the E-commerce Survey. The survey is a stratified annual sample survey of around 7,700 UK businesses drawn from the Inter-Departmental Business Register (IDBR), with the sample covering firms with at least 10 employees in non-financial production and market-services sectors. In other words, the sampling frame reflects all industrial and services sectors of the economy apart from agriculture, mining, finance, and non-market sectors such as public administration, education and health (see Table A1). Larger firms are surveyed each year, while smaller firms are surveyed on a rolling basis. The survey is designed to produce representative estimates of aggregate digital activity, after applying sampling weights by firm size and industry strata. We examine the robustness of our regression results to applying these sampling weights as an indication of aggregate effects.

The E-commerce Survey contains questions on firm use of different types of cloud computing, covering its use of hardware services such as data, storage, processing, and software services, including finance software, office software, customer relationship management software and email. We code the firm as an adopter of cloud technologies if it uses any of these different cloud services and zero otherwise. We construct measures of big data and AI use in a similar manner. Firm use of big data is constructed as a binary variable which is equal to 1 if an enterprise reports that it analyzes big data via either of the following methods: the enterprise's own data collected with smart devices or sensors, data gathered from geolocation data from the use of portable devices, generated from social media, and data collected from other external sources.[22] The E-commerce survey includes information on two separate measures of AI technologies, notably machine learning and natural language processing. Similar to the construction of the other technology variables, we create a binary variable equal to 1 for firms using AI in either of these forms and zero otherwise. This definition therefore captures the forms of AI in use during our sample period and does not extend to generative

[22]The E-commerce survey defines big data and big data analytics as the following. Big data typically have characteristics such as: (1) vast amounts of data generated over time, (2) variety in terms of different formats of complex data, either structured or unstructured (for example text, video, images, voice, docs, sensor data, activity logs, click streams, coordinates). (3) velocity in terms of the high speed at which data are generated, become available and change over time. Big data analysis refers to the use of techniques, technologies and software tools for analyzing big data from our own business or other data sources.

AI, which emerged at scale only after 2022, beyond our sample window.

Measures of IT capital investment, as well as historic (pre-AIA) total investment in plant and machinery, and the date of each firm's financial year end, which we use to identify our set of treated firms, are taken from the ONS Annual Business Survey (ABS). The ABS is a large annual stratified sample survey of around 60,000–65,000 UK businesses drawn from IDBR, designed to measure the structure and performance of the business economy by collecting detailed information on turnover, purchases, employment costs, investment and approximate gross value added. Like the E-commerce survey, the ABS excludes financial and insurance activities, public administration and defense, education, human health and social work, most agriculture, and other largely non-market sectors. Large firms are typically surveyed annually and smaller firms on a rotating basis, ensuring strong coverage of economic activity despite not surveying every business each year.

The annual Business Structure Database (BSD) provides data on firm control variables, age, multi-establishment status and foreign ownership. The BSD is an annual point-in-time snapshot of the IDBR, covering nearly the entire population of UK businesses. The matched employer-employee data comes from the Annual Survey of Hours and Earnings, provided by the ONS, and comprises panel data on the wages of 1% of UK workers. Since the data is a sample of workers, this prevents us aggregating to the firm-level, and limits us to worker-level analysis. We examine the demand for workers who perform data analytics (or business intelligence) tasks, those that perform other data tasks (specifically creating database, data stores or software creation) and those that do not perform data tasks, using occupation information on time use from Corrado et al. (2022).[23]

Finally, details on the Annual Investment Allowance policy over time are provided by UK Tax Authority (HMRC). This data contains information on investment thresholds of the allowance, eligible investment, when the policy was introduced (2008) and details on changes in the thresholds over time up to 2019.

Since the E-commerce, ABS and the BSD use the same IDBR frame as other ONS business surveys, they can be linked with unique reporting units across the datasets. Worker-level data is matched to our firm level data using the HMRC PAYE records.

[23]Given that half of workers do not spend any time on data tasks, see Table A5, we use non-zero time use on data tasks to determine these groups.

TABLE 2. Data Sources and Coverage

Dataset	Source	Frequency / Sample Size	Sampling	Sector Coverage	Firm Coverage
E-Commerce	ONS	Annual; ∼7,700 firms	Stratified sample; large firms surveyed every year, smaller firms sampled on a rotating basis	Non-financial production and market-service sectors	10 or more employees
ABS	ONS	Annual; ∼65,000 firms	Stratified sample; large firms surveyed every year, smaller firms sampled on a rotating basis	Non-financial business economy	All sizes (including self-employed, those without employees)
BSD	ONS	Annual; ∼2million firms	Administrative snapshot of the IDBR business registry covering nearly the entire universe of UK businesses	Economy-wide firm coverage	All sizes (including self-employed, those without employees)
ASHE	ONS	Annual; ∼320,000 workers	1% random sample of workers from PAYE income tax records which reflects nearly the entire universe of UK workers	Economy-wide worker coverage	N/A (worker-level dataset)
AIA Policy	HMRC	Annual	N/A	Economy-wide policy coverage	N/A

The final sample of sectors for all analyses reflects non-financial production and market-service sectors (those covered by the E-Commerce survey), as listed in Table A1. We include firms of all sizes available in the data, such that investment regressions relying on the ABS include small firms, whereas regressions of technologies in the E-Commerce survey reflect firms with 10 or more employees. Investment regressions are robust to excluding firms with fewer than 10 employees.

While the HMRC tax administrative data used by Maffini et al. (2019) provide more comprehensive measures of AIA eligibility and investment, they do not capture firm technology use like the representative ONS survey datasets. Linking ONS survey data to the firms' HMRC tax records for research purposes is not currently possible under UK legislation. A comparison by ONS (2022) of R&D expenditure in the ONS ABS and HMRC tax records finds similar aggregate trends and the data are broadly consistent at the micro-level, with micro differences predominantly explained by firms not being sampled by the ONS survey, a similar comparison is not available for investment. The primary motive for the use of ONS data is that the technology measures on cloud, big data and AI are not available in the HMRC data. The E-Commerce Survey, Annual Business Survey, and Business Structure Database are collected under a common legal framework and are designed to be linked consistently at the firm level. Together these data contain investment data by firms, broken down into various types allowing us to replicate as closely as possible this aspect of the HMRC data.

Our baseline sample period focuses on the effects of the earlier years of the AIA from 2007 to 2013 for total investment and cloud. The year 2013 is chosen as this is the first time for which questions on cloud use were included in the E-commerce survey. We assume zero adoption for all firms in 2007, consistent with the assumption of DeStefano et al. (2025), as this is before cloud computing arrived in the UK. In the robustness section we show that the results are not dependent on the use of data for 2007 and also test for the use of the other time periods for which information on cloud adoption is available, namely 2015, 2017 and 2019. Information on the use of big data analytics are collected for the years 2015 and 2019 and for AI in the year 2019. Worker-level estimation of the demand for data workers and total investment at the firm level takes advantage of annual data for the years 2007 to 2013 and 2007 to 2019 (inclusive).

Table 3 provides summary statistics of the main variables and time periods we use in the main body of the paper. For the period up to 2013 our data show that 28% of firms in our sample use cloud, but that this varies across types of cloud service. For example, only 6% of firms use cloud for finance software, whereas 16% use cloud for storage of files. For big data analytics, 21% of firms use big data by 2015 (26% in 2019), while only 1.7% of firms use AI by 2019. Additional summary statistics for all firm variables for all time periods are available in Table A2, we separate these into those for untreated and treated observations in Tables A3 and A4, and worker summary statistics are in Table A5.

TABLE 3. Firm Descriptive statistics

Variables	Mean	SD	Observations	Sample coverage
Annual Investment Allowance				
AIA Dummy	0.095	0.294	25,117	(2007-2013)
AIA Available Allowance (continuous)	0.007	0.030	25,117	(2007-2013)
Firm Investment (logs)				
Total Investment	6.598	2.334	24,467	(2007-2013)
Software Investment	2.412	2.592	24,467	(2007-2013)
Hardware Investment	3.764	2.592	17,985	(2007-2013)
Plant & Machinery Investment	5.866	2.409	22,976	(2007-2013)
Vehicles Investment	3.358	2.195	19,097	(2007-2013)
Land & Buildings Investment	1.198	2.498	22,587	(2007-2013)
Firm Technology Adoption				
Cloud	0.281	0.449	2,206	(2007 & 2013)
Cloud Storage	0.157	0.364	2,206	(2007 & 2013)
Cloud Data	0.126	0.364	2,206	(2007 & 2013)
Cloud CRM	0.09	0.286	5,084	(2007 & 2013)
Cloud Finance software	0.055	0.228	2,206	(2007 & 2013)
Cloud Office software	0.068	0.252	2,206	(2007 & 2013)
Cloud Email	0.12	0.325	2,206	(2007 & 2013)
Cloud Own Software	0.072	0.403	2,206	(2007 & 2013)
Big Data Analytics	0.205	0.404	2,264	(2007 & 2015)
Big Data Analytics	0.259	0.44	1,748	(2007 & 2019)
Artificial Intelligence	0.017	0.13	1,748	(2007 & 2019)
Control Variables				
Multi-Establishment	0.751	0.433	24,467	(2007-2013)
Foreign-owned	0.330	0.470	24,467	(2007-2013)
Age (log)	3.304	0.400	24,467	(2007-2013)
Employment (log)	6.132	1.578	23,467	(2007-2013)

Note: All investment variables are in log thousands of UK pounds, deflated using 4 digit (2007 SIC codes) PPI deflators provided by the ONS. The AIA available allowance is in (nominal) millions of UK pounds.

4 Empirical Strategy

To identify the effect of the AIA capital allowances on capital investment, cloud technologies, big data and AI we use a difference-in-differences (DID) specification with some minor distinctions discussed below depending on whether we model IT investment (available annually) or the digital technology adoption variables (available in specific years). The difference-in-difference regressions measure the outcome of firm i in period t relative to the control group, before and after the introduction of the AIA allowance, expressed as follows, where Z_{it} takes the value one for the treatment group in the post-treatment period and zero otherwise:

$$y_{it} = \alpha + \beta Z_{it} + X'_{it}\gamma + \mu_i + \lambda_t + \varepsilon_{it}. \tag{1}$$

In Equation (1) the estimated coefficient β is the difference-in-difference parameter of interest. We include firm and year fixed effects, to control for slow-moving unobserved firm factors and common trends, reflected by μ_i and λ_t respectively. $X'_{it}\gamma$ is a vector of control variables including age, multi-establishment status, foreign ownership and lagged employment.[24] Our baseline period focuses on the earlier years of the AIA from 2007 to 2013, which corresponds to the year before the AIA was launched and the first year data was collected on cloud.

If the AIA policy affects firm adoption of data technologies, such as cloud, big data analytics or AI, then one would expect that this would also materialise in the demand for workers performing data analytics tasks. The worker-level estimation employs a similar annual difference-in-differences (DID) specification to the investment regressions above. It estimates the wages of worker w connected to firm i in period t before and after AIA treatment Z_{it}. We estimate Equation 2 separately for data analytics workers, other data workers and the remaining (non-data) workers. Thus we are comparing the demand for data analytics workers in firms treated by the AIA to the data analytics workers in non-treated firms:

$$y_{wit} = \alpha + \beta Z_{it} + \mu_w + \lambda_t + W'_{wt}\delta + X'_{it}\gamma + \epsilon_{it} \tag{2}$$

We include worker and year fixed effects, to control for slow-moving unobserved worker factors and common trends, reflected by μ_w and λ_t respectively. To control for potential sorting, we follow the literature and include a vector of worker controls $W'_{wt}\delta$, including age, tenure, tenure squared, skilled occupation dummy, gender and the interactions of the other worker controls with gender. $X'_{it}\gamma$ is a vector of firm control variables as above. We find few cases of workers switching into or out of data occupations, and results are robust to their

[24] The exclusion of these controls does not alter the results and has almost no impact on the magnitude of the estimated coefficients in the baseline regressions.

exclusion.[25]

We use changes in the AIA as a quasi-natural experiment to identify a set of treated firms for whom the marginal cost of investing (in capital) fell. To identify treated firms, we first calculate the average value of acquisitions of tangible capital by the firm prior to the announcement of the AIA (following the R&D incentive literature, such as Bjuggren (2018); Saez et al. (2019); Bøler et al. (2015)). We calculate this as the average across the years 2005 and 2006. We avoid using the year 2007 as the AIA policy was first announced in a press release in March of that year, although the results are unchanged if we include data from that year alongside those from 2005 and 2006. We note that these past investments are good predictors of future investments by the firm all the way up to the end of the data period in 2019. Regressing (logged) firm investment in 2008 on the average of past (2005–06) firm investment yields an estimated coefficient of 0.916 (s.e. 0.15). The corresponding estimates are 0.874 (s.e. 0.17) for 2010, 0.842 (s.e. 0.18) for 2013, 0.859 (s.e. 0.19) for 2015, 0.834 (s.e. 0.18) for 2017, and 0.815 (s.e. 0.20) for 2019.[26] It follows from the use of historic firm investment that we are capturing "intention to treat" estimates through Equation (1), that is, those whose historic investment would predict treatment at the time of the policy change. [27]

Firms receive the incentive on investment up to an allowance ceiling that varies according to their financial year end, as noted in the previous section of the paper. This necessitates the construction of a treatment variable that captures this. To give an example: assuming for the moment that the accounting year-end of the firm is the end of March, a firm with an average investment of £75,000 across 2005 and 2006 would be above the AIA ceiling in 2010 (AIA ceiling of £50,000) and therefore coded as zero (our control group), but in 2011 this firm's lagged investment would be beneath the threshold of £100,000 and therefore coded as one in that year (our treatment group). As noted in the previous section, we calculate these binary values each year using each firm's allowance ceiling, based on the date of their accounting year end.

As an extension, rather than a binary treatment variable, we also calculate a continuous

[25]For example, between 2007 and 2019 there are only 9,953 observations relating to workers switching into or out of data analytics occupations, compared to a total of 455,069 worker observations, noted in Table A5. The results are also robust to the switching of workers between firms and the exclusion of these movers.

[26]The number of observations for each regression is 3,558, 3,284, 2,587, 2,555, 3,011 and 2,502, respectively.

[27]One potential concern is that firms may re-time investments across years — for instance, deliberately reducing investment in a given year to remain below the AIA threshold and qualify for the allowance. Three features of our design limit this concern. First, we assign treatment using firms' historic pre-AIA investment, which is unaffected by any re-timing that occurs after the policy's introduction. Second, the frequency and unpredictability of AIA threshold changes — six changes between 2008 and 2019, several announced with little notice — would make systematic re-timing difficult to sustain. Third, we examine investment pre-trends below, which would reflect any such re-timing."

measure reflecting the number of pounds the firm is below the AIA ceiling. The continuous measure captures potential heterogeneity in treatment intensity, the intuition being that firms with more unspent allowance are likely to have a larger investment response. We use this continuous measure of the AIA allowance available to firms as a robustness test below. This logic also explains why the baseline DID approach is preferred to a regression discontinuity design. A discontinuity approach identifies effects only for firms just around the threshold, ignoring those for which the change in the AIA threshold is most significant.[28]

We note some differences in the DID specification depending on whether we use the capital investment or worker data or the less frequently available cloud, big data and AI measures. When studying the effects of AIA on capital investment or demand for data analytics workers this data is available on an annual basis and therefore the treatment status of the firm is determined by these annual changes in the marginal incentive to invest by year. The presence of firm/worker and year fixed effects in Equations 1 and 2 along with the changes to the AIA policy across years means the regression belongs to the two-way fixed effects model with staggered treatment design that has attracted considerable attention concerning 'forbidden comparisons', i.e. comparing treated firms to previously treated firms as well as those not treated (Goodman-Bacon, 2021; Borusyak et al., 2021; Callaway and Sant'Anna, 2021). As our preferred method, we use the approach of Callaway and Sant'Anna (2021), which accounts for staggered treatment designs and heterogeneous treatment effects under semi-balanced panel data. The approach presents event study estimates of investment, by comparing treated firms (for each cohort of AIA changes) against those that are never treated and not yet treated as a control group.[29] We also examine robustness to long-difference estimation, comparing investment in a future period to the year before the AIA policy, which also removes any previously treated firms by definition.

The measures of cloud, big data and AI are available only for a few specific years, so we estimate the model in long-differences, comparing technology adoption in a future period to the year before the AIA policy. When exploring these outcome variables our treatment is then determined as those firms for whom the marginal cost of investment (in capital) fell at some point between the start of the AIA (in 2007) and the given future period (2013/15/17/19 depending on available digital technology measure), while control firms are firms whose marginal cost of investment did not change over this period. We do this by measuring treatment status as the maximum of the year-by-year treatment dummies described above - i.e. firms that were treated in any year.[30] For the continuous measure we construct the

[28] We are not able to estimate a regression kink design due to relatively small samples for our technology regressions.

[29] The results are similar if we only use never treated firms as controls.

[30] The main results of the paper are unchanged if we use whether they are below the AIA ceiling on

mean value over the years between 2007 and the given future period (2013/15/17/19). In addition, we note that as we measure outcomes only in a single pre-treatment period and a single post-treatment period concerns over staggered treatment do not apply and we can estimate Equation 1 using OLS.[31] Finally, to speak to aggregate magnitudes, we also re-estimate the baseline regressions applying the survey sampling weights, which correct for the stratified sampling design by firm size and industry. The weighted estimates are used solely for the back-of-envelope aggregate calculations, the baseline regressions are unweighted firm-level estimates.

Because treated firms — those whose historic investment falls below the AIA ceiling — tend to be smaller on average than control firms, there is a risk that the baseline estimates capture size-related differences in technology trajectories rather than the policy itself. The pre-trends plots immediately below provide an assessment of this. For further robustness, we complement our baseline with a difference-in-discontinuities design (Dechezleprêtre et al., 2016) that compares firms just above and below the AIA threshold before and after policy changes, which we describe in more detail in Section 6.3. By restricting identification to firms in the neighborhood of the threshold, this design isolates variation in treatment that is plausibly orthogonal to firm size or other characteristics, providing a direct robustness check on the baseline DID. In addition, we examine robustness to DID estimation of the AIA effect separately within firm-size bands and to including employment-band-by-year fixed effects to absorb time-varying size trends.

The validity of difference-in-differences rests on parallel trends in the absence of treatment. We follow common practice in the literature and test this by comparing the treatment and control groups in the periods before treatment takes place. To do so we show graphically how the treatment effects of changes in the AIA evolve over time using the approach of Callaway and Sant'Anna (2021).

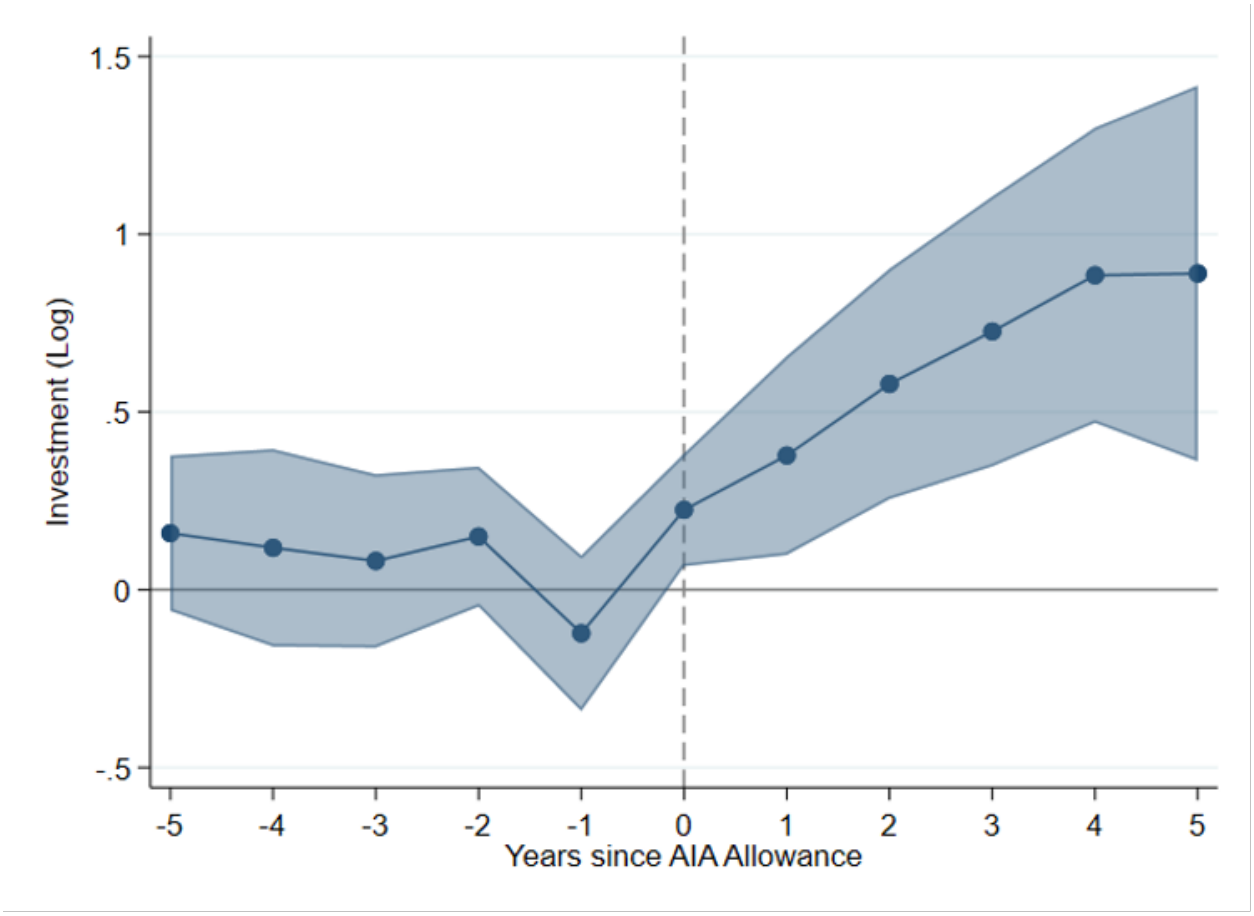
In Figure 1, we present the event study plot for total investment against its associated treatment-year dummies. It is clear that in the periods leading up to the different AIA changes, the treatment and control groups share similar pre-treatment trends. There is a large increase in investment by treated firms in the post-treatment period (consistent with the later results in Table 4 below). This effect is apparent in the year in which the marginal incentive for the firm changes, consistent with the ambitions of the AIA policy, and continues in the five years post-treatment. Our event study plots are robust to alternative similar

average over the period.

[31] With few periods, the degree of the treatment stagger is inherently limited, and in any case, without a semi-balanced panel it is not possible to employ new staggered treatment estimators, such as Callaway and Sant'Anna (2021).

definitions of firms' treatment status based on their historic investment.[32] For example, Figure A1 uses 2-year average investment data for the years 2006/07 while Figure A2 uses 3-year averages for the years 2005/06/07.

Figure 1. Total investment and years pre and post the AIA allowance



Notes: Data period: 2007-2013. The above figure presents the coefficients and 95 per cent confidence intervals of event study regressions – reflecting firm total investment in the periods before and after changes in the AIA threshold. The threshold is calculated using 2-year average investment in years 2005/2006. Event time is equal to 0, the preceding year is event time of –1, the year after the event is +1, and so on. The estimation follows the method of Callaway and Sant'Anna (2021), see Equation 1.

It is not possible to provide evidence on the absence of pre-trends for the firm digital technologies such as cloud, big data and AI as they had not been invented by the start of the AIA policy and were only added as questions to the E-commerce survey later. In their absence, we instead provide evidence for two alternative approaches to this question. Firstly, the worker level data is available on an annual basis and therefore we can report pre-trends for the wages of data analytics workers. As argued previously, the worker level data provides

[32]The absence of pre-trends is also apparent if we estimate these regressions over the period to 2019.

information on the extent these advanced information technologies are used within the firm. Secondly, we draw on annual measures of IT expenditures and stocks that are available to us, these are IT services expenditures (which has been used in the literature as a proxy for cloud adoption DeStefano et al. (2025)) and PCs per employee.

We examine the pre-treatment trends for the wages of data analytics workers (Figure A3), IT services expenditures (Figure A4) and PCs per employee (Figure A5), exploiting that these are measured on an annual basis. In the periods before the AIA changes we do not observe a differential trend in wages of data analytics workers between the treated and control firms (although the worker-level estimation is somewhat more noisy than the preceding firm-level plots). Around two years after treatment we observe a decline in the wages at treated firms, mirroring later results in Table 7. These results are consistent with prior research showing that technology adoption is associated with subsequent changes in labor demand (Bresnahan et al., 2002), but that such organizational adjustment typically materialize gradually over time (Juhász et al., 2020). We find similarly for the IT services expenditures and PCs per employee. There is no evidence that these expenditures change in the periods leading up to treatment under the AIA policy, with coefficient estimates that are close to zero and with no obvious trend. While there is also little evidence that these change significantly in the post-treatment period, we note that these measures do not capture upgrades to the quality of IT being used. [33] For completeness, we also plot pre-trends for measures of firm performance that are known to be positively correlated with technology adoption (Brynjolfsson and Hitt, 2000), including employment, sales and sales per employee in Figures A6, A7 and A8, respectively. Reassuringly, we find no evidence for pre-AIA-treatment differences in these measures.

5 Results

5.1 Investment and Cloud

This section presents the core results on the effect of the AIA on firm investment in IT capital and cloud adoption. The AIA was designed to stimulate investment in tangible capital, including IT hardware and software. We find evidence that the policy was successful in this regard. The results in Table 4 demonstrate that firms for whom the marginal costs of investment fell increased their total capital investment.[34]

The coefficient in column 1 implies that the policy increased total investment among

[33] Consistent data on software investment and hardware investment is only available from 2008, preventing analysis of pre-trends for these components of investment

[34] The absence of firm balance sheet data prevents analysis of the user cost of capital or financing.

treated firms by approximately 61.7% between 2007 and 2013.[35] This suggests a meaningful response to the policy change. Comparing it to existing estimates from the literature is made somewhat difficult by the fact that the regression includes the introduction and then changes to the investment threshold. For illustration, we therefore calculate the implied elasticity of investment with respect to the user cost of capital using the 2008 introduction of the policy and then the 2013 increase of the threshold to £250,000. Using the standard formula for the user costs of capital and the corporate tax rate for those years, a real discount rate of 5% and an economic depreciation of 20% suggest the policy reduced the user cost of capital by about 7.2% in 2008 and about 6.2% in 2013. The estimated coefficient of 0.481 would therefore imply an estimated elasticity of investment of 6.4 and 7.7 respectively. We note that this is similar to the estimated elasticities of between 8.3 and 8.9 for an earlier change in tax allowances for the UK reported by Maffini et al. (2019) as well as recent estimates using US bonus depreciation policies of around 7 from Zwick and Mahon (2017) and between 6 and 14 from House and Shapiro (2008).[36] The large magnitude appears consistent with the fact that the AIA represented a substantial reduction in the user cost of capital for eligible firms, particularly smaller ones for whom the threshold was most binding (as suggested in Table 8).

As shown in Tables A6 and A7, the results for investment are robust to a number of changes in sample period or years over which eligibility for the AIA is calculated. In Table A6, we show results for investment for the years up to 2015 (regression 1), 2017 (regression 2) and 2019 (regression 3). Relying on the same years for which cloud data are available and the same long-differences specification also yields consistent results (regression 4). In Table A7, we show that the effect of the AIA on investment holds irrespective of whether we define the treatment status of the firm using a 2-year average based on the years 2006/07 (regression 1), or a 3-year average based on the years 2005/06/07 (regression 3).

In contrast to its effects on investment, AIA eligibility reduces the propensity to adopt cloud by approximately 17 percentage points. The magnitude of the estimated coefficient is relatively large compared to the mean rate of cloud adoption, of 28% in our sample. These results reinforce the idea that firms view IT capital investment and purchases of cloud services as substitutes – a reduction in the relative price of IT capital leads to a substitution away from cloud services and towards tangible IT investment.

To give a sense of the aggregate slowdown in cloud use induced by the AIA policy, we

[35] Since the investment outcomes are in logs, the percentage increase in total investment is calculated as 61.7% = exp(0.481)-1.

[36] It is above those found from a number of older studies reported in Hassett and Hubbard (2002) typically focusing on larger firms, and above estimates weighted by firm size of Zwick and Mahon (2017).

TABLE 4. Effects of AIA on total capital investment and cloud adoption, 2007-2013

Regressions	(1)	(2)
Outcome	Total Investment	Cloud
AIA Dummy	0.481***	-0.165***
	(0.105)	(0.040)
Observations	21,757	2,200

Note: Time period: Total investment regressions use annual data for the years 2007 to 2013. Cloud regressions use data for 2007 and 2013. Total investment is in (log) thousands of pounds, while cloud is a dummy variable. Treated firms are identified taking the average investment values for firms in 2005/06 and calculating if this lagged investment average is above or below the AIA threshold in any given year. Regressions use a binary AIA eligibility indicator. Regression 1 is estimated using the method outlined in Callaway and Sant'Anna (2021) and regression 2 uses OLS. All regressions include year and firm fixed effects, as well as firm controls of lagged employment, a multi-plant dummy, foreign owned dummy and log age. These are not reported for brevity. Robust standard errors in parenthesis are clustered at the firm level. ***, ** and * indicate significance at the 1%, 5% and 10% level, respectively.

repeat our baseline estimation applying sampling weights.[37] The weighted regressions show similar coefficients to our baseline, the AIA policy reduced cloud use for treated firms by between 15 and 20 percentage points (depending upon the estimation period, 2007-2013 or 2007-2019). Since around 43%-46% of firms were treated by the AIA policy, this implies an aggregate reduction in cloud use of 7-9 percentage points. Over our sample period, aggregate cloud use increased by roughly 6 percentage points per year (whether measured over 2007-2013 or 2007-2019). Thus, the AIA policy appears to have slowed cloud diffusion by 1 to 1.2 years.

Again, we note the robustness of these results if we define firms' treatment status using a 2-year average of investment based on the years 2006/07 (regression 2), or a 3-year average based on the years 2005/06/07 (regression 4) (Table A7), or, as we show in Table A8, if we measure the effects of cloud for the years 2007 and 2015 (regression 1), 2017 (regression 2) or 2019 (regression 3). Across these regressions the estimated effect of the AIA on cloud adoption was between -12 and -17 percentage points.

We also explore a continuous treatment measure that takes into account the size of the investment incentive available to each firm. In Table A9 we replace the dummy treatment

[37]These aggregate estimates are approximate as they do not account for general equilibrium effects, and some sectors and firms with fewer than 10 employees are not surveyed in our data (see section 3). The estimated coefficient (standard error) on the AIA dummy for the 2007-13 time period in these weighted regressions is -0.199 (0.058) and -0.152 (0.031) for the period 2007-19.

variable with a continuous version that measures the number of pounds a firm's average 2005/06 investment is below the AIA threshold in that year. By so doing we capture differences in how strongly a firm was treated by the AIA - firms with a larger unused investment allowance are likely to have a stronger incentive to invest more.[38] These results provide further evidence that firms with the greatest scope for increasing investment responded to the AIA by increasing their investment and reducing the likelihood of adopting cloud services.[39] Each additional £1,000 of AIA allowance increases investment by 0.4% and reduces cloud diffusion by 0.3 percentage points.

5.2 Big Data and AI

We next explore whether the policy affected other types of technology adoption, namely big data analytics and AI. The effect of capital incentives on these technologies depends on the extent to which firms can use their own IT capital for big data analytics and AI, or rather, whether cloud computing is the preferred technology. If capital incentives lead firms to invest in more IT hardware for storing and processing data, then this may encourage big data and AI adoption. However, cloud services are often cited as being strongly complementary to big data analytics and AI, because the volumes of data involved require large amounts of storage and processing power. Cloud offers storage and processing capabilities in ways that are more flexible and cost-effective than installing the physical server infrastructure (Brown et al., 2011).[40] This opens the possibility that capital investment policies may instead act to slow the diffusion of big data analytics and AI across firms.

The evidence shows that capital incentives discourage the use of big data analytics and AI. According to our estimates, the AIA reduced the use of big data analytics by around 18 percentage points (using either a long difference between 2007 and 2015 or 2007 and 2019, see columns 1 and 2 in Table 5). Similarly, we find that firms that qualify for the AIA exhibit a lower propensity to adopt AI. We find that treated firms were around 3 percentage points less likely to adopt AI (see Table 5 column 3).

To give an indication of the magnitude of the aggregate slowdown in the use of these

[38] As the estimation method of Callaway and Sant'Anna (2021) is applicable only for binary treatment variables, in the investment regression we take advantage of the recent development by Gardner (2022) that allows for continuous treatment in two-way fixed effects settings with a staggered treatment design. This uses the estimation command that implements the approach of Gardner (2022) created by Butts and Gardner (2021).

[39] We also examined AIA allowance quartiles and found evidence that the impact of the AIA allowance is largely increasing linearly by quartile, consistent with the continuous measure employed in Table A9. Results are available upon request.

[40] As is often quoted in the IT systems literature (e.g. Armbrust et al. (2009)), the cost of purchasing 1 server for 100 hours from a cloud provider, is the same as the cost of purchasing 100 servers for 1 hour.

TABLE 5. The effects of AIA on the adoption of big data and AI

Regressions	(1)	(2)	(3)
Outcome	Big Data	Big Data	AI
Years	2007/15	2007/19	2007/19
AIA Dummy	-0.185***	-0.182***	-0.027***
	(0.030)	(0.034)	(0.011)
Observations	2,262	1,746	1,746

Note: Regression 1 uses data for 2007 and 2015; regression 2 and 3 use data for 2007 and 2019. The dependent variables big data and AI adoption are dummy variables. Treated firms are identified taking the average investment values for firms in 2005/06 and calculating if this lagged investment average is above or below the AIA threshold in any given year. All regressions use a binary AIA eligibility and are estimated via difference-in-differences. All regressions include year and firm fixed effects, as well as firm controls of lagged employment, a multi-plant dummy, foreign owned dummy and log age, not reported for brevity. Robust standard errors in parenthesis are clustered at the firm level. ***, ** and * indicate significance at the 1%, 5% and 10% level, respectively.

new digital technologies, we repeat our estimation applying sampling weights. The weighted regressions show similar coefficients to our baseline in Table 5 and we combine this with the fact that 46% of UK firms were treated by the AIA by 2019 to estimate the aggregate slowdown in technology diffusion.[41] Our results imply an aggregate reduction in big data analytics use of 7 percentage points and AI use by 1 percentage point. While seemingly small numbers, they should be considered in the context that only 3% of UK firms used AI by 2019.[42] Moreover, big data analytics use would have been 14% higher and AI 30% higher in 2019 in the absence of the AIA policy. Or put another way, the policy slowed big data analytics diffusion by 1.4 years and AI diffusion by 1.2 years.[43] This delay is large, especially for AI, which only emerged in the US around 2015 (Bloom et al., 2022).

6 Robustness and Falsification Tests

A key concern when assessing the effects of capital incentives on technology adoption is that treated and untreated firms may differ systematically most notably in size or in their underlying technology trajectory, raising the possibility that our estimates reflect pre-existing

[41] All statistics mentioned in this paragraph are weighted means (applying sampling weights). The estimated coefficients (standard error) in these weighted regressions are -0.239 (0.045) for big data and -0.024 (0.011) for AI.

[42] AI use is 3.4% in 2019, and assumed to be zero in 2007, corresponding to the 1.7% in Table 3.

[43] In our data, big data analytics and AI diffusion were increasing approximately 5 percentage points and 1 percentage point per year, respectively.

firm characteristics rather than the policy itself. Earlier in the paper, we document that treated and control firms exhibit parallel pre-policy trends in annual outcomes that are close antecedents to digital technology adoption, including investment dynamics, IT-services intensity and PCs per employee, providing reassurance on this potential issue.

In this section, we further address these concerns through a comprehensive and structured set of robustness and falsification exercises. First, we conduct placebo and negative-control tests using ineligible investment categories and cloud services that are unlikely to substitute for in-house IT capital, which yields no comparable effects. Second, we incorporate annual employer–employee data, which allows us to test whether the policy affected demand for workers using these technologies within the firm, and to compare these effects with those for placebo worker groups that should not be affected. Because these data are annual, they also enable us to assess the presence of pre-trends. Third, we implement a difference-in-discontinuities design (Dechezleprêtre et al., 2016) that compares firms just above and below the relevant investment thresholds before and after policy changes, isolating variation that is plausibly orthogonal to firm size. Finally, we re-estimate our main results using a range of design restrictions that limit comparisons to more homogeneous firms: we trim the tails of the distribution, estimate the AIA effect separately within firm-size bands, include employment-band-by-year and industry-by-year fixed effects, and exclude firms that were eligible for other capital allowance policies during the sample period.

6.1 Types of Cloud and Capital Investment

A natural concern is that any estimated effect of the AIA on technology adoption could reflect broad firm-level shocks, such as changes in demand, productivity, or scale, rather than a reallocation of investment driven by the policy itself. If this were true, the AIA would be expected to generate effects on investment and cloud adoption comparable to our baseline estimates across all investment categories and cloud service types. In this section, we assess the specificity of the policy channel by conducting a set of placebo and negative-control exercises that exploit the detailed nature of the UK data to examine how the AIA is linked to different types of investment and different types of cloud services. In Table 6 Panel A, we begin by separating the aggregate investment (shown in the previous Table 4) into hardware (column 1), software (column 2), plant and machinery (column 3), vehicles (column 4), and land and building investment (column 5). Similarly, in Panel B we study the effect of the AIA on the different types of cloud included in the E-Commerce survey. These are cloud databases (column 6), storage (column 7), customer relations management

(CRM) software (column 8), office software (column 9) and email (column 10).[44]

We find that the AIA incentivized firms to invest only in the types of capital that were eligible for the allowance, consistent with evidence on capital incentive policies in other contexts (Cummins et al., 1994; House and Shapiro, 2008; Zwick and Mahon, 2017; Ohrn, 2018; Maffini et al., 2019). For example, the policy led to an increase in plant and machinery, vehicles, hardware and software investments (see Table 6). Given the AIA removed the tax advantage of cloud services, that there remains a response on IT investment to being treated by the AIA suggests that some firms prefer physical IT. The treated firms increased investment in hardware by 41.3% and software by 27.8%. Importantly, we find that the AIA was not linked with increased investments in land and buildings by UK firms, a type of investment that was not eligible for the AIA.

The AIA did not impact adoption of all types of cloud services. The AIA capital incentive strongly predicts reduced rates of adopting cloud services related to the storage and processing of data. The effect of the policy is particularly pronounced for cloud hosting of databases, storage of files and CRM software. Firms treated by the AIA are around 9.4 percentage points less likely to adopt cloud database services and 9.7 percentage points less likely to adopt cloud storage compared to the control group. Of interest, we find no effect on the probability to adopt cloud for access to office software and email services. These represent the least technologically sophisticated cloud services in our data and the least likely to be viewed as a substitute for traditional IT capital investments. We find similar zero effects for finance software and hosting the firm's own software in Table A10 in the Appendix.

The fact that the AIA incentive does not affect ineligible capital investment (i.e. land and buildings), nor all cloud types (such as for accessing email), provides some reassurance that we are not capturing unobservable firm specific productivity or demand shocks that are increasing investment and technology adoption throughout the firm. Rather, the effects of the AIA are specific to the types of investment and technology adoption that would be expected to change as a consequence of the AIA policy and to the way that we classify firms as being treated by the AIA.

[44]We report results for finance software and own software in Table A10, along with Eurostat (2018) definitions of low, medium and high-tech types of cloud technologies.

TABLE 6. Effects of AIA on investment and cloud types

Panel A: Investment					
Regressions	(1)	(2)	(3)	(4)	(5)
Outcome	Hardware Investment	Software Investment	Plant & Mach. Investment	Vehicles Investment	Land & Buildings Investment
AIA Dummy	0.346***	0.246***	0.549***	0.214***	-0.012
	(0.079)	(0.070)	(0.100)	(0.072)	(0.086)
Observations	17,651	24,313	22,569	18,583	22,327
Panel B: Cloud Adoption					
Regressions	(6)	(7)	(8)	(9)	(10)
Outcome	Cloud Databases	Cloud Storage	Cloud CRM	Cloud Office	Cloud Email
AIA Dummy	-0.094***	-0.097***	-0.091***	0.001	-0.045
	(0.040)	(0.035)	(0.037)	(0.027)	(0.032)
Observations	2,200	2,200	2,200	2,200	2,200

Note: Time Period: Investment regressions use annual data for the years 2007 to 2013. Cloud regressions use data for 2007 and 2013. Panel A displays separate regressions for the different types of capital investment while Panel B presents estimations for each type of cloud services. Capital investment variables are in (logged) thousands of pounds, while the cloud variables are a dummy variable. Treated firms are identified taking the average investment values for firms in 2005/06 and calculating if this lagged investment average is above or below the AIA threshold in any given year. Regressions use a binary AIA eligibility indicator. The regressions in Panel A are estimated using the method outlined in Callaway and Sant'Anna (2021) and those in Panel B use difference-in-differences. All regressions include year and firm fixed effects, as well as firm controls of lagged employment, a multi-plant dummy, foreign owned dummy and log age. These are not reported for brevity. Robust standard errors in parenthesis are clustered at the firm level. ***, ** and * indicate significance at the 1%, 5% and 10% level, respectively.

6.2 Demand for Data Workers

To assess whether our firm-level results reflect the causal mechanism we propose rather than broader firm characteristics, we turn to worker-level outcomes that should respond to the AIA only for occupational tasks directly complementary to data analytics and digital technology use. This provides a natural negative-control framework: if the policy operates through its impact on digital technologies, labor-market effects should be concentrated among workers performing complementary tasks, with little response among other groups performing non-complementary tasks.

The previous sections showed that the AIA discouraged firms' adoption of data analytics technologies such as cloud, big data analytics, and AI. While these adoption measures are informative, they do not capture the extent to which such technologies are actually used within firms. Under the assumption that digital technologies are strongly skill-biased (Brynjolfsson and McElheran, 2016), we therefore examine whether treatment under the AIA also materialized in changes in the demand for workers engaged in data analytics tasks and less relevant for other tasks. An additional advantage of this approach is the availability of annual matched employer–employee data, which allows us not only to implement these negative-control tests, but also to assess pre-policy trends (see Section 4) and to compare outcomes across workers performing different roles within the same firms.

In Table 7 we find evidence that the firms treated by the AIA reduced their demand for data analytics workers, while the policy had no effect on the other types of workers. The wages of workers who perform data analytics fell by 1.5% to 1.6% in firms treated by the AIA, compared to these same workers in non-treated firms (columns 5 and 6). In contrast, the wages of workers who perform other data tasks, related to creating databases, data stores or software (columns 3 and 4), and workers who do not perform data tasks (columns 1 and 2), were not significantly affected. Thus, the AIA policy had no discernible effect on demand for workers upstream in the data value chain, who create and store data, but rather reduced demand specifically for workers engaged in downstream data processing and analysis. Moreover, the AIA policy did not affect labor demand in general for treated firms, but specifically reduced demand for the subset of workers performing data analytics tasks.[45] This is consistent with the evidence that the AIA slowed adoption of data technologies (namely cloud, big data analytics and AI) and reduced demand for the workers likely to use them.

[45] We find no consistent evidence of an effect of the AIA on total employment, sales or wages on affected firms.

TABLE 7. The effects of AIA on the demand for data workers

Regressions	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Worker Wages					
Occupation Group	Non-Data		Other Data		Data Analytics	
Years	2007-2013	2007-2019	2007-2013	2007-2019	2007-2013	2007-2019
AIA Dummy	-0.006	-0.008	0.008	-0.011	-0.015*	-0.016**
	(0.005)	(0.006)	(0.012)	(0.009)	(0.009)	(0.007)
Observations	144,136	288,347	30,812	61,863	51,415	104,859

Note: Worker-level regressions using annual data. Worker wages are in (logged) thousands of pounds. Treated firms are identified taking the average investment values for firms in 2005/06 and calculating if this lagged investment average is above or below the AIA threshold in any given year. Regressions use a binary AIA eligibility indicator. Data occupation groups are defined following Corrado et al. (2022). Specifically, Data Analytics reflects occupations with time used for data intelligence. The remaining occupations are decomposed into Other Data, those with time used for creating data stores, databases or software; and Non-Data, those that do not spend time on data activities. The regressions are estimated using the method outlined in Callaway and Sant'Anna (2021). All regressions include year and worker fixed effects, as well as firm controls of lagged employment, a multi-plant dummy, foreign owned dummy and log age and worker controls for age, tenure, tenure squared, skilled occupation dummy, gender and the interactions of the other worker controls with gender. These are not reported for brevity. Robust standard errors in parenthesis are clustered at the firm level. ***, ** and * indicate significance at the 1%, 5% and 10% level, respectively.

6.3 Difference-in-Discontinuity and Firm Size

The treated firms are those for whom the marginal incentive to invest was altered by the introduction and subsequent changes to the AIA capital incentive policy. In practice, the way that the AIA was introduced and altered over time means the treated firms in the empirical analysis have investment below the new AIA ceiling (and above the old ceiling) and therefore have lower initial investment and are on average smaller than non-treated firms. The summary statistics on untreated and treated firms in Tables A3 and A4 show this clearly. The adoption of emerging technologies is typically positively correlated with firm size. While these level differences should not impact our results given the difference-in-differences approach we take, there is the possibility that the digital technology regressions capture the effects of firm size on adoption rather than the AIA, or are at least confounded by the effects of firm size.

As discussed previously, initial evidence on this point is provided by the event study plots of employment, sales and sales per employee, using the fact that these are observed annually for treated and control firms. The results presented in Figure A6 for employment, Figure A7 for sales and Figure A8 for sales per employee do not show any obvious pre-trends, suggesting that this is not a concern and is unlikely to be a driver of differences in adoption rates. Nevertheless, in Table 8 we take multiple approaches to assess this issue.

To rule out the possibility that our results are driven by firm-size confounders, we first test for any difference-in-discontinuity at the AIA threshold following Dechezleprêtre et al. (2016). Under this approach, we use the continuous measure of AIA and separately add variables measuring the extent to which firms are above or below the AIA ceiling.[46] This is akin to the running variable in a regression discontinuity framework, enabling the identification of the effect of the capital incentive policy even if there is a confounding factor related to firm size around the AIA ceiling, provided the effect of the confounding factor does not vary over time. To allow for non-linearities in any confounding effect, we also use a quadratic version of the running variable. Regressions 1 and 2 confirm that the result remains unaltered by this alternative methodology, and we continue to find that the AIA policy had a negative effect on cloud adoption.

To further examine whether our results are driven by firm size, we next control for potential time-varying unobserved factors related to firm size by allowing the effects of the AIA policy to differ across firms with different numbers of employees (see Regression 3 in Table 8). Specifically, we estimate the effect of the AIA on firms with fewer than 50 employees in 2007 and for firms with more than 50 employees.[47] The coefficients provide

[46] These take a value of 0 for observations the opposite side of the ceiling.

[47] The results are unaltered if we allow for different trends of digital technology adoption for firms within

estimates of the effect of the AIA for firms within the same size band. The results show that the AIA policy caused both SMEs and large firms to become significantly less likely to adopt cloud, and therefore that unobservable time varying changes do not confound the results, at least for those within these employment size bands. The coefficient suggests that smaller firms (with fewer than 50 employees) that were treated by changes in the AIA ceilings were 37 percentage points less likely to adopt cloud technologies, compared to 14 percentage points for larger firms. In Appendix Table A11 we report results for cloud, big data, and AI technologies measured in 2007 and 2019 using 50 employees to separate small and large firms, while in Tables A12 and A13 we use thresholds of 100 and 150 employees, respectively. The results remain unchanged with these different thresholds. In regression 4, we also control for differences in the trend rate of adoption for firms in different (2-digit SIC) industries and again find results consistent with our baseline estimates.

Investment data in the UK is, as in most other countries, highly skewed. A small number of firms make very large investments, whereas most firms invest a more modest amount each year. To determine whether our results are driven by the presence of these largest firms in column 1 of Table 9 we exclude firms with the largest 5 percent of investment averaged across 2005/06, while in column 2 we exclude the largest 10 percent of investment. We also test whether the results for cloud adoption are driven by firms that make very little investment in capital, including IT capital. In columns 3 and 4 we also exclude the smallest 5 and 10 percent of firms. The results for cloud adoption are robust to these sample restrictions.

In columns 5 and 6 of Table 9 we exclude firms that were eligible for other policies during our sample period which may have influenced investment and cloud adoption behaviors. Until 2008 and again for the year 2010, a First Year Allowance (FYA) policy existed in the UK which provided tax allowances to small firms. Firms with sales up to £22.8 million were eligible to receive a tax rebate on capital investments through accelerated depreciation, as considered by Maffini et al. (2019). In order to examine the robustness of the effects of AIA on firm investment decisions, we exclude firms in our sample that ever had sales of less than £22.8 million in any year during our sample period. This is a conservative approach and results in the loss of more than a tenth of our sample. Despite this, our results are robust to the exclusion of these firms. The estimated effects on cloud adoption remain consistent with our baseline in sign and significance.

We also consider the possibility that our results capture a change in the definition of

different employment size bands. These are based on firm employment in 2007 (1-49, 50-99, 100-249, 250-499, 500-999, 1000+) interacted with year dummies. The results are also unchanged if we include more groups for smaller firm sizes (<25 employees, 26-50, 51-75, 51-75, 75-250, 251-500, 501-750, 751-1000, 1001-1500, 1501-2000, 2001+). Similar results are also obtained if we use revenue quartile-year effects. These results are available on request

TABLE 8. The effects of AIA and cloud adoption: confounders and additional controls

Regressions	(1)	(2)	(3)	(4)
Outcome	Cloud			
Robustness	Difference-in-discontinuity (Linear)	Difference-in-discontinuity (Quadratic)	Firm Size Effects	Industry 2-digit-year
AIA Dummy	-0.159***	-0.146***		-0.123***
	(0.040)	(0.040)		(0.045)
AIA Large Firms			-0.141***	
			(0.042)	
AIA Small Firms			-0.368***	
			(0.097)	
Observations	2,200	2,200	2,200	2,190

Note: Time period: Regressions use data for 2007 and 2013. Treated firms are identified taking the average investment values for firms in 2005/06 and calculating if this lagged investment average is above or below the AIA threshold in any given year. Regression 1 is a difference-in-discontinuity regression which uses the continuous measure of AIA and includes variables measuring the extent to which firms are above or below the AIA ceiling. These take a value of 0 for observations the opposite side of the ceiling. Regression 2 incorporates the quadratic version of the running variable. Regressions 3 uses a binary AIA eligibility indicator and are estimated via difference-in-differences. Regression 3 estimates the effect of the AIA on firms with <50 employees in 2007 and the effect for firms larger with >50 employees and includes employment size bands based on their employment in 2007 (1-49, 50-99, 100-249, 250-499, 500-999, 1000+) interacted with year dummies. Regression 4 includes 2-digit industry-year fixed effects. All regressions include firm controls of lagged employment, a multi-plant dummy, foreign owned dummy and log age, not reported for brevity. Robust standard errors in parentheses are clustered at the firm level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

SMEs by the EU in 2008 which in turn affected qualification for the R&D Tax Relief Scheme for UK firms. This definition change shifted the qualifying threshold of assets from €43 million to €86 million, the employment threshold from 249 to 499, and the sales threshold from €50 million to €100 million (Dechezleprêtre et al., 2016). We start by converting these thresholds to sterling equivalents using the average sterling-euro exchange rate in 2008 of 0.80 and then exclude firms that would have been affected by this change in the year the change occurred. Specifically, we exclude firms that became eligible for the R&D incentive because of the change in the scheme design. The results for these regressions again imply that this does not explain our main findings (see column 6 in Table 9). We continue to find that capital investment policies reduced the adoption of cloud services among treated firms.[48]

[48] As a final robustness test, we also exclude firms who changed their accounting year-end at any point in our sample period. The results presented in Table A14 continue to find consistent results with our baseline.

TABLE 9. The effects of AIA and cloud adoption: sample restrictions

Regressions	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Cloud					
Sample Restriction	<95th percentile investment	<90th percentile investment	>10th percentile investment	>5th percentile investment	Exclude 1st-year allowance	Exclude R&D tax credit
AIA Dummy	-0.149***	-0.136***	-0.149***	-0.118***	-0.104***	-0.186**
	(0.040)	(0.040)	(0.058)	(0.046)	(0.048)	(0.040)
Observations	2,090	1,980	1,980	2,090	1,962	2,094

Note: Time period: Cloud regressions use data for 2007 and 2013. The dependent variable cloud adoption is a dummy variable. Treated firms are identified taking the average investment values for firms in 2005/06 and calculating if this lagged investment average is above or below the AIA threshold in any given year. All regressions use a binary AIA eligibility indicator and are estimated via difference-in-differences. Regression 1 excludes largest firms in the top 5% of the investment distribution, while regression 2 excludes those in the top 10%. Regressions 3 and 4 exclude the smallest firms in the bottom 5% and 10% of the investment distribution, respectively. Regression 5 restricts firms from the sample which qualified for the first-year allowance while regression 6 excludes those firms which fell under the new EU SME classification in 2008. All regressions include year and firm fixed effects, as well as firm controls of lagged employment, a multi-plant dummy, foreign owned dummy and log age, not reported for brevity. Robust standard errors in parentheses are clustered at the firm level. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

7 Conclusion

The arrival of cloud computing is changing the way firms access IT; however, little is known about whether the policies designed for earlier forms of technology remain appropriate. This paper examines whether capital incentives alter firm decisions to adopt cloud or invest in physical IT, and also how this impacts the diffusion of big data analytics and AI. To do so, we take advantage of the introduction and subsequent changes to a UK tax incentive for tangible capital investment – the Annual Investment Allowance (AIA).

We find that firms eligible for the AIA increase their capital investment, including IT and hardware capital, in line with the aims of the policy. But these firms are significantly less likely to adopt cloud. Our results suggest that firms view IT capital investment and cloud adoption as (partial) substitutes – a reduction in the price of IT investment leads to a substitution away from cloud and towards traditional IT. Earlier research suggests smaller firms benefit most from the cloud (through the flexible variable costs it provides) and these are the same firms targeted by the capital incentive. That firms therefore respond by investing in IT capital suggests that some firms prefer this method of acquiring IT. Furthermore, the AIA reduced demand for data analytics workers - but had no effect on other worker groups - and induced a lower likelihood of using big data analytics and AI, confirming that cloud computing is a key complement for the use of big data and AI. Our estimates suggest that the policy slowed the aggregate diffusion of cloud by approximately one year, and big data analytics and AI by one to one and a half years.

Our results present a challenge for government policy. Every OECD economy currently has some form of capital incentive policy and many include or even explicitly target IT capital investments (as the UK did before 2005) (Tax Foundation, 2018). Firms in the UK are relatively early adopters of cloud compared to other high-income economies, in part due to the early rollout of superfast fiber broadband (DeStefano et al., 2025); therefore, these findings offer a possible prognosis for other economies. However, the tax treatment of service expenditures already encourages firms to become more IT intensive, at least if these services can be purchased through the cloud. This raises a question of whether capital incentive policies targeting tangible IT are necessary. It also raises a related question about whether these policies may inadvertently be slowing the diffusion of newer technologies, such as the cloud, that are delivered as online services. While the effect on cloud adoption matters in its own right, our results show this can lead to knock-on effects by further slowing the diffusion of other data-driven technologies that leverage the cloud, such as big data analytics and AI. If, as Goldfarb et al. (2023) suggest, AI and big data are general-purpose technologies, this may generate longer-term effects on growth through the virtuous circles of

innovation between those sectors creating and those using the technology (Bresnahan and Trajtenberg, 1995). These findings highlight an inherent tension in the design of capital incentive policies: by extending immediate expensing to physical IT investment, the AIA removes the pre-existing tax advantage that cloud services hold over tangible IT, and in doing so inadvertently tilts technology choice away from cloud. Policymakers who wish to encourage broad digital technology adoption — including cloud, big data, and AI — should therefore consider whether explicitly excluding IT capital from the scope of capital incentive programmes would better serve these objectives, preserving the tax advantage of cloud services that the current policy design erodes.

Capital incentive policies are often justified based on the market failures of economies of scale and credit market imperfections that bear most heavily on smaller firms. However, by shifting IT costs from a largely sunk cost to a variable cost, cloud itself can alleviate some of these market failures. More specifically, cloud computing shifts the economies of scale in IT from the user firm to the cloud provider - who install giant data centers comprised of hundreds of thousands of servers, but pass on a variable cost to cloud users. In this sense, cloud itself addresses some of the market failures that capital incentives are designed to correct. Our results suggest that policies designed for firms comprised of PCs, servers, bricks and mortar may need reconsideration for business models that are increasingly built on data and other intangibles.

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