Digital platform mergers and innovation: Evidence from the cloud computing market

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Abstract

This paper empirically analyses mergers and innovation in the cloud computing market, one of the fastest-growing digital markets. We first examine mergers by big tech firms and venture capital funding for young start-ups in this market. We find that leading firms in the market tend to acquire young start-ups, whereas non-leading firms tend to purchase more established firms to gain market share. We then conduct an ex-post evaluation of how mergers in this market affect the innovation output —measured by patents. The results show a positive impact of mergers on the number of granted patents. In this market, and our measure of innovation, acquisitions do not necessarily harm innovation. The breakdown of this empirical analysis reveals stronger positive effects when the firm holds a leadership position in the market, operates as a multisided platform, or when the target is a publicly traded company. The value of the acquisition does not exert any additional impact.

Keywords: cloud computing, innovation, mergers, patents.

JEL Classification: L11 , L41 , L63.

1 Introduction

Recent mergers and acquisitions by large technology firms have drawn increasing scrutiny from competition authorities, who fear such deals may reduce rivalry and hinder innovation in digital platform markets. Cloud computing is one prominent example where these concerns arise. Influential reports by Crémer et al. (2019), Furman et al. (2019), and Scott-Morton et al. (2019) emphasize the difficulties merger control faces in these sectors, especially the challenge of assessing long-term innovation

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effects in rapidly evolving markets. This has created a strong case for ex-post empirical evaluations of mergers in these markets to better inform regulatory oversight.

This paper examines mergers and acquisitions (M&A) in the U.S. cloud computing sector and their impact on innovation. What motivates our analysis is the classic 'replacement effect' proposed by Arrow (1962), which shows that an incumbent firm has weaker incentives to innovate than a potential entrant: successful innovation would cannibalize the incumbent's own existing profits from current products or technologies. Mergers exacerbate this effect by reducing the number of rivals, thereby internalizing business-stealing incentives among the merging parties and diminishing the credible threat of displacement by outsiders, which weakens the merged firm's motivation to innovate. Complementing this mechanism, Gilbert and Newbery (1982) show how a dominant firm may pursue preemptive patenting, racing to secure patents early primarily to block rivals and preserve market power; post-merger concentration (fewer rivals) weakens this incentive to defend dominance over aggressive innovation. These mechanisms thus predict that mergers reducing competition will dampen innovation incentives, a hypothesis we can test empirically in this market.

We also examine how specific merger characteristics influence post-merger innovation outcomes, focusing on four key dimensions: firm dominance (acquirer as market leader), multisided platform (acquirer as MSP) structure, target firm status (public vs. private), and deal value, each capturing a source of variation in innovation incentives. Dominant acquirers may face weaker competitive pressure and lower incentives to innovate (Gilbert and Newbery, 1982). In platform markets, Katz (2020) shows that MSP acquirers reduce target innovation more than non-MSP acquirers: strong network effects create 'winner-takes-most' environments where acquisitions generate a hold-up problem: entrants anticipating acquisition underinvest, while incumbents pursue an 'innovate-to-acquire' strategy that kills standalone innovation post-merger. Acquisitions of private targets enhance acquirers' post-merger innovation incentives by providing complementary, exploratory knowledge assets, whereas those of public targets, often pursued for scale in less competitive settings, yield fewer such innovative spillovers (Farida et al., 2024). Finally, larger deals typically signal greater market power consolidation and weaker post-merger innovation incentives (Arrow, 1962). Empirically, we include these four characteristics as covariates in difference-in-differences analysis. Each cloud computing M&A, regardless of these traits, has been matched to a control firm from outside the cloud computing market but with cloud-related innovations.

This is a timely and policy-relevant issue: regulators on both sides of the Atlantic have placed increasing emphasis on innovation in merger review, with the U.S. Federal Trade Commission and Department of Justice as well as the UK Competition and Markets Authority recently updating their merger guidelines to explicitly account for innovation effects. At the EU level, the Digital Markets Act also reflects growing concern about the market power and strategic behavior of large digital platforms. Sector-specific inquiries, such as those by the UK's communications regulator and the French Competition Authority,[1] underscore that cloud computing is at the very center of these debates. The market is highly concentrated, with three dominant firms,

[1] See https://tinyurl.com/ym5w3276 and The Office of Communications (2023).

Amazon, Microsoft, and Google, holding over 50% of global share,[2] and operating within diverse business models, including vertically integrated structures and multi-sided platforms. These features make cloud computing an instructive case for studying how acquisition strategies may reshape innovation incentives across different types of firms.

We compile a dataset from multiple sources, including Crunchbase for M&A activity, Thomson Reuters Datastream for financial, and the United States Patent and Trademark Office (USPTO) for patent data.

While existing research has analyzed mergers in sectors such as pharmaceuticals and hardware (Danzon et al., 2007; Ornaghi, 2009; Haucap et al., 2019; Kapoor and Lim, 2007; Bennato et al., 2021), work on digital platform markets is more recent and remains comparatively limited. A growing literature has begun to examine acquisition strategies of large digital firms and their consequences for competition and innovation (Gautier and Lamesch, 2021; Cabral, 2021; Cusumano et al., 2019). Yet, significant gaps remain in understanding how differences in business models and market structures shape innovation outcomes in the cloud computing sector in particular.

Our descriptive analysis identifies a distinct bifurcation in acquisition strategies between leading and non-leading firms. We define leading firms as the top five by global market share in any cloud segment.[3] Leading firms (Amazon, Microsoft, Google) primarily acquire young, innovative startups likely aiming to strengthen their technological leadership while minimizing regulatory attention. In contrast, non-leading firms (such as Cisco, Rackspace, and Dell) tend to target more established companies to gain access to technology, customers, and data, thereby attempting to close the competitive gap. These contrasting patterns motivate a detailed investigation into how M&A characteristics, including firm dominance, multisided platform (MSP) structure, target firm status (public versus private), and deal value, impact post-merger innovation outcomes.

We measure innovation outcomes using patent counts and estimate causal effects through a difference-in-differences (DiD) framework combined with propensity score matching. The treatment group comprises firms that undertook mergers within the cloud computing industry, while the control group consists of non-merging firms from other industries that nonetheless hold cloud-related patents. This design allows us to avoid within industry spillovers and isolate the impact of mergers on innovation in the cloud sector.

Contrary to prior evidence from other sectors, and against our theoretical predictions and motivation, we find no post-merger decline in innovation; on the contrary, mergers in the cloud market are associated with increased patenting activity. This positive effect is particularly pronounced when the acquirer is a market leader, operates as a MSP, or acquires a publicly listed target, whereas deal size shows no significant impact. To ensure robustness, we conduct several validation checks, including the use of citation-weighted patent counts, restriction to key patent classifications, and winsorization of skewed covariates.

[2] Synergies Research Group: https://tinyurl.com/48cwv7vh.

[3] See Figure 1 for firms and 2019 shares

The remainder of the paper is structured as follows. Section 2 reviews the relevant literature. Section 3 offers an overview of the cloud computing industry. Section 4 describes the data and presents descriptive evidence. Section 5 outlines our empirical strategy, followed by results and robustness checks in Section 6. Section 7 concludes.

2 Literature review

This paper contributes to the literature on the relationship between competition and innovation, with a particular focus on how mergers (changes in market structure) shape firms' innovation incentives. The longstanding debate centers on whether competition stimulates or dampens innovation incentives, and how this effect varies with firms' technological positions and strategic environments. Building on earlier frameworks, Gilbert and Newbery (1982) show that the threat of potential entry can strengthen a dominant incumbent's incentives to innovate, while other contributions stress that the competition–innovation nexus depends critically on technological dynamics and a firm's strategic position (Vickers, 1985; Boone, 2001). Drawing on these insights, Aghion et al. (2005, 2009) formalize an inverted U-shaped relationship in which innovation incentives rise with moderate competition but decline when competitive pressure becomes very intense.[4] Complementing this, Shapiro (2012) argues that pre-innovation competition tends to spur investment in new technologies, whereas post-innovation competition may weaken incentives to invest in improvements or follow-on innovations.

While much of this work focuses on competition more broadly, a smaller but growing literature examines how mergers affect innovation. Several papers (Federico et al., 2017, 2018; Motta and Tarantino, 2021) argue that traditional competition–innovation theories cannot be applied mechanically to mergers, because mergers simultaneously reduce the number of firms and enable coordination of innovation decisions, thereby internalizing externalities known as the *innovation diversion effect*. Federico et al. (2017) show that merged entities may reduce R&D investment to limit cannibalization, whereas Denicolò and Polo (2018) argue that a merged firm may instead consolidate research in a single unit, potentially increasing total R&D. Federico et al. (2018) further analyze how price coordination interacts with innovation incentives, and Bourreau et al. (2025) decompose merger effects into several components, highlighting both positive and negative implications for innovation.

Empirical evidence generally suggests that mergers tend to weaken innovation incentives, though effects are heterogeneous across sectors. Valentini (2012), for example, finds that U.S. medical device and photographic equipment mergers increase patenting but reduce measures of patent quality, while pharmaceutical studies report declines in R&D expenditure and patenting after mergers (Danzon et al., 2007; Ornaghi, 2009; Haucap et al., 2019). Similar concerns arise in cross-industry work: Szücs (2014) shows that mergers typically reduce R&D spending and R&D intensity, whereas more granular analyses find mixed effects across innovation measures. Bennato et al. (2021) study mergers in the hard disk drive industry and report that innovation outcomes vary by metric, and Malek et al. (2024) show that R&D project acquisitions in pharmaceuticals can reflect diverse strategic motives, ranging from consolidating

[4]For a more comprehensive review, see Jullien and Lefouili (2018).

existing positions to accessing external innovation, highlighting how acquisitions can either entrench dominance or serve as a channel for integrating outside technologies.

Some scholars argue that acquisitions by dominant platforms can foster innovation because the prospect of being acquired encourages startups to develop new technologies and business models (Bryan and Hovenkamp, 2020; Bourreau and de Streel, 2020; Motta and Peitz, 2020). Others warn that such acquisitions may create 'kill zones' that deter venture capital investment and reduce potential rivals' incentives to innovate (Kamepalli et al., 2020; Katz, 2020; Letina et al., 2024). Empirical work on Big Tech acquisitions finds that dominant firms mostly acquire startups to reinforce existing positions rather than to enter new markets (Argentesi et al., 2021; Gautier and Lamesch, 2021; Jin et al., 2023; Gautier and Maitry, 2024). For a more comprehensive overview of this literature, see Lefouili and Madio (2026).

To our knowledge, mergers in the cloud computing platform industry have received limited empirical attention in this context. This paper contributes to that emerging literature by examining how such deals relate to innovation in the cloud sector-measured by patent activity, and by linking these patterns to broader questions of merger policy, innovation incentives, and competition in digital platform markets. This question arises against a backdrop of ongoing debate about the competitive and innovative effects of large technology firm mergers, amid a wave of acquisitions by dominant platforms that, in many jurisdictions, have historically faced only modest ex ante scrutiny from competition authorities.

3 The cloud computing market

Cloud computing is one of the fastest-growing and most innovative markets, with worldwide spending of almost \$100 billion in 2019.[5] The industry is dominated by three GAFAM firms, Amazon, Microsoft, and Google, with Amazon entering first, followed by Microsoft, and Google expanding as a later entrant. Cloud services allow firms to access computing power, storage, and software on demand over the Internet, replacing in-house IT infrastructure and enabling substantial economies of scale.[6]

The market is commonly divided into three main segments: Infrastructure as a Service (IaaS), Platform as a Service (PaaS), and Software as a Service (SaaS). IaaS provides basic computing resources (servers, storage, networking); PaaS offers a development environment on top of that infrastructure; and SaaS delivers software applications directly to end users via the cloud. Public cloud deployment is the dominant model, in which providers manage infrastructure and serve multiple tenants, and most leading vendors (e.g. AWS, Microsoft Azure, Google Cloud) operate across several of these segments.[7]

Cloud providers can be vertically integrated or operate as multisided platforms.[8] Vertically integrated providers supply both the underlying platform and their own applications, while multisided platforms connect third-party developers and users,

[5] https://tinyurl.com/3khn6b2u (Accessed on 15 September 2024.)

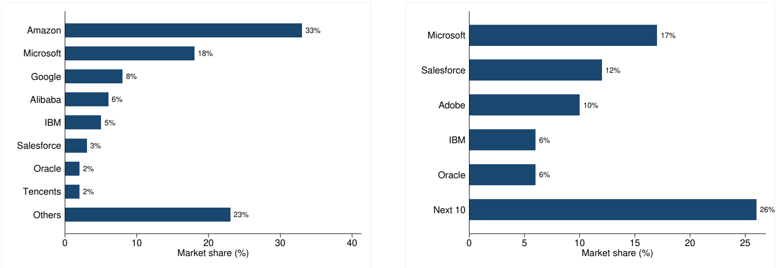
[6] https://tinyurl.com/3hf3vy6w (Accessed on 15 September 2024.)

[7] See Statista, Digital & Trends Cloud Computing and Industry Cloud in the United States for segment revenues.

[8] See Karunakaran (2016) for discussion.

generating indirect network effects. Leading firms such as Amazon, Microsoft, and Google combine both models, using developer-user interactions to expand application variety and strengthen their user base.

Fig. 1: Global market shares by vendors in cloud computing market in 2019



(a) IaaS and PaaS segments

(b) SaaS segments

Source: Synergy Research Group.

As noted by Crémer et al. (2019), and shown in Figure 1, the cloud market is highly concentrated among a few core players, with Amazon, Microsoft, and Google together accounting for a large share of IaaS and PaaS revenues, and Microsoft, Salesforce, and Adobe among the largest SaaS providers. No single firm dominates all segments, however, and capacity constraints as well as technological specialisation leave room for niche competitors, particularly in the more fragmented SaaS segment. This combination of strong incumbents and a competitive fringe makes cloud computing a salient setting to study the relationship between mergers, market power, and innovation.

We classify firms as leading if ranked in the top five of any segment; these include Amazon, Microsoft, Google, Salesforce, Adobe, IBM, and Oracle. All others, such as Rackspace, Cisco, and Dell, are non-leading. This characterization will play an important role in both our data description and empirical analysis.

4 Data

4.1 Merger data and merger strategies

We collated a dataset on US-based cloud computing companies to investigate the merger strategies (the decisions to merge) of cloud computing firms and how mergers affect innovation in the U.S. cloud computing market. The dataset is retrieved from the Crunchbase website,[9] covering 2010-2019. It contains information on U.S. cloud

[9] Crunchbase is a platform that provides information about startups, companies, investors, and other related entities in the business world. It offers a dataset that contains information such as company profiles, funding details (including funding rounds, investors, and amounts raised), acquisitions and mergers, key personnel (founders, executives, and board members), news articles, and more. It covers a wide range of industries and geographies. It allows users to explore the dynamics of the startup and venture capital landscape.

computing company characteristics, which include: company name, headquarters location, industry, operating status (whether the company is active or not), founding date, number of employees, funding activities, initial public offering (IPO) activities, number of products alive, number of investors, and importantly for our work, the information on acquisition status (whether the company is completely acquired or not).[10]

The dataset is cross-sectional and only captures firm characteristics and acquisition events at the time of each acquisition as reported in Crunchbase, anchored to the specific year when the transaction occurred.[11] As a result, our descriptive analysis is limited to understanding how acquiring firms select their targets based on observable characteristics at the time of the transaction.[12] This design does not allow us to assess post-acquisition outcomes such as product innovation or human capital. Had we access to longitudinal or panel data, we would be able to explore how acquired firms develop post-merger, but this is beyond the content of the current dataset.

While our dataset contains both merging and non-merging firms in the cloud computing market, our descriptive focus mainly on the subset of merging group since most of the variables of non-merging firms have a large number of missing values. For the similar reason, we also can not use these non-merging firms in the control group for our difference-in-difference analysis, and need to construct the control group from firms operating outside of the cloud computing market.[13]

Our descriptive analysis compares leading and non-leading firms within the merging sample and interprets differences in their acquisition pattern, distribution of acquired target characteristics like age, number of employees, etc., as indicative of strategic behavior. For example, if the mean age of acquired targets by leading firms is statistically less than the one by non-leading firms, we can interpret this as an indication of leading firms strategically choosing younger firms to acquire.

The U.S. cloud computing firms are active in M&A, totaling 430 acquisitions during the ten years of our sample.[14] Cisco and VMware lead with nine deals each (Table 1). Amazon, despite its market leadership, has made only four acquisitions, reflecting its first-mover advantage and in-house technological edge. Competitors, lacking this advantage, show greater incentive to buy firms for their technologies or innovations. Microsoft, Google, and Salesforce each acquired six firms, fewer than Cisco, VMware, or Rackspace, while IBM leads the top group with eight.

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[10]Announced-but-failed deals are not recorded as acquisitions, so the acquisition status in the data reflects only successfully completed transactions.

[11]While this provides a time stamp for each event, it does not allow for true longitudinal tracking of firms before or after acquisitions, except in rare cases where a firm is involved in multiple deals and thus appears in the data more than once, each as a separate snapshot. Therefore, the analysis is limited to patterns observable at the time of acquisition and does not reflect how firms evolve over time.

[12]However, in our main ex-post evaluation analysis, both financial and patent measurement variables used in propensity score matching are time-varying, and the only purely cross-sectional variable is the indicator of whether an acquisition occurs. Although we cannot analyze the dynamics of the merger event itself, we can examine the post-merger dynamics of patenting while controlling for time-varying financial characteristics at the firm level.

[13]This means our merger indicator variable will be one for merging firm in the cloud computing market and zero for these control firms operating outside of this market.

[14]To ensure the reliability of our merger data, we cross-check acquisition details and confirm whether merging entities offer cloud computing products using multiple alternative sources, including company websites and financial reports. In addition, we consider the inherent data validation procedures employed by Crunchbase, which involve manual curation, and community verification to enhance data accuracy.

Market leaders possess advanced technologies, broad services, and large customer bases. To consolidate their position, they often target companies offering complementary products or innovations, particularly in adjacent areas like AI or big data, rather than direct cloud rivals. Non-leading firms, seeking to close technology, product, and customer-base gaps, tend to acquire established cloud companies.

Table 1: Number of acquisitions by leading vs. non-leading firms 2010-2019

leading firm		non-leading firm	
IBM	8	Cisco	9
Google	6	VMware	9
Microsoft	6	Rackspace	7
Oracle	6	Hewlett Packard	6
Salesforce	5	SAP	6
Amazon	4	Dell	5
Adobe Systems	2	EMC	5

In deal value terms, IBM's $34B purchase of Red Hat in 2018 tops the list (Table 2). Of the seven top-share leaders (Amazon, Microsoft, Google, Salesforce, IBM, Adobe, Oracle), only IBM, Microsoft, Oracle, and Salesforce appear in the top ten by value, suggesting that Amazon and Google prefer smaller firms and start-ups. No non-leader appears in the top ten, indicating limited financial capacity; instead, they complete more, smaller deals. Cisco and Rackspace, for example, top the acquisition count but have no high-value entries. Six of the top ten deals involve non-cloud firms, including upstream semiconductor producers Broadcom and Nvidia, and private equity/investment companies, highlighting investor confidence in the sector's growth.

Table 2: Top (by value) acquisitions in the U.S. cloud computing market

	target company	acquired by	announced date	price
1	Red Hat	IBM	Oct 28, 2018	$34.0B
2	CA Technologies	Broadcom Limited	Jul 11, 2018	$18.9B
3	NetSuite	Oracle	Jul 28, 2016	$9.3B
4	GitHub	Microsoft	Jun 3, 2018	$7.5B
5	Mellanox Technologies	NVIDIA	Mar 12, 2019	$6.9B
6	Mulesoft	Salesforce	Mar 20, 2018	$6.5B
7	Informatica	Permira	Aug 7, 2015	$5.3B
8	Rackspace	Apollo	Aug 26, 2016	$4.3B
9	Publicis Sapient	Publicis Groupe	Nov 3, 2014	$3.7B
10	Riverbed Technology	Thoma Bravo	Dec 15, 2014	$3.5B

Remark 1: *Non-leading firms acquire more U.S. cloud computing companies than leading firms, but these acquisitions generally involve lower deal values.*

Table 3 compares targets of leaders (Amazon, Microsoft, Google, Salesforce, Oracle, IBM, Adobe) and major non-leaders (Cisco, Rackspace, Dell, etc.). On average, leaders' targets are typically younger, with fewer employees, investors, and funding, and are less likely to have gone public. This reflects different acquisition goals: leaders seek complementary technology/innovation from young start-ups, while others buy more established firms to compete more effectively. Firms with the largest market shares like Amazon may also avoid acquiring big targets to reduce antitrust scrutiny. Under the Hart-Scott-Rodino Act (1976), mergers meeting certain thresholds must notify the FTC and DOJ for clearance, with potential review for anti-competitive effects, including reduced innovation. Fear of intervention may explain leaders' preference for smaller targets. Additionally, dominant firms' superior data capabilities may help them identify promising young companies and optimize acquisition timing.

Table 3: Comparison of acquired target characteristics between leading firms (Amazon, Microsoft, Google, IBM, Oracle, Salesforce, and Adobe) and non-leading firms (Cisco, Dell, Rackspace, etc.)

	targets by leading firms					targets by non-leading firms				
variable	obs	mean	se	min	max	obs	mean	se	min	max
age*	40	7.200	0.813	1	18	110	9.336	0.857	1	58
number of employees*	40	3.175	0.299	1	9	108	4.185	0.234	1	9
number of investors*	34	4.265	0.526	1	16	83	5.470	0.360	1	16
funding amount ($M)	31	48.261	14.072	1	350	84	71.993	21.992	1.2	1704
number of funding rounds	34	3.088	0.320	1	6	84	3.643	0.259	1	12
IPO status	40	0.050	0.035	0	1	95	0.116	0.033	0	1

Note:* The mean values of the two groups are statistically different at a 5% significance level. The number of employees is a categorical variable, which takes the value one if the company has 1-10 employees, two if 11-50 employees, three if 51-100 employees, four if 101-250 employees, five if 251-500 employees, six if 501-1,000 employees, seven if 1,001-5,000 employees, eight if 5,001-10,000 employees, and nine if the company has more than 10,000 employees. IPO status has the value of 1 if the company has a successful IPO, zero otherwise.

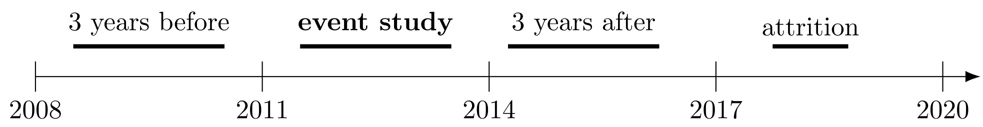
Remark 2: *Overall, leading firms predominantly target young startups for acquisition, whereas non-leading firms are more likely to acquire established companies.*[15]

[15] This does not contradict **Remark 1** because, aside from a few high-value deals, most acquisitions by market leaders involve young companies or startups.

4.2 Data for estimating the impacts of mergers on innovation

4.2.1 Merger data sample

To estimate the impacts of mergers on innovation, measured by patent counts in the cloud computing market, we focus on mergers occurring between January 1, 2011, and December 31, 2014. This four-year window was chosen to allow analysis of patent activity three years before and three years after each merger event, as illustrated in the timeline below.



Cloud computing is a relatively new market, with almost no patents recorded prior to 2008. On the post-merger side, patent filings and grants often experience delays. To address this, we conservatively allow a three-year lag for patent registration and concession, aiming to capture all patents generated up to 2017, three years after the latest mergers in 2014. This sample restriction thus enables a comprehensive evaluation of post-merger innovation.

Our sample from 2011 to 2014 includes 145 acquiring firms and 177 target firms; this dataset was used to construct Table 3. We obtained financial data for acquirers and targets, including net sales/revenue, R&D investment, net income, total assets, total debt, and gross profit margins, from Thomson Reuters Datastream. After merging the merger data with Thomson Reuters data, the sample was reduced to 36 acquirers and 62 target firms due to missing financial information for 109 acquiring firms and 155 target firms. When an acquirer completed multiple mergers within the same year, these were pooled, which further contracts the merger sample from 62 to 51 observations. This merged dataset will be used in the main empirical analysis, with any deviations clearly noted.

Finally, to avoid double counting, as done in Haucap et al. (2019), we combine the values of acquirers and targets. For acquirers with multiple targets during 2011–2014, all patenting activity across the 2008–2017 period is pooled.

4.2.2 Limitations of cloud innovation measures

Among popular measures of innovation, R&D investment is not applicable in our research as most leading firms in the cloud computing market are operating in many other industries. For instance, Amazon's business is also in the online marketplace or tablet PC manufacturing. Microsoft also produces personal computers, smartphones, and software. Therefore, it is problematic to identify which amount of R&D investment these firms invest in cloud computing technology. For this reason, we will focus on measuring innovation by the firm's patent activity.[16]

[16]In principle, besides examining the impact of M&A on innovation, we could have also explored its effect on share prices since most acquirers are publicly listed companies. However, many of these acquirers are multinational and operate across multiple markets. Consequently, the acquisition of a target firm might only

While patent counts are a widely used measure of innovation, we acknowledge their limitations in the cloud computing market, where proprietary software, trade secrets, and open-source contributions can play a significant role. Here, patents may not fully capture the breadth of innovation, as firms in this industry often rely on alternative strategies to protect and commercialize their technological advancements. A limitation of using patents as a measure of innovation in the U.S. cloud computing market lies in the inherent challenges related to patent validity and the unique characteristics of this technology sector. Cloud computing patents often cover complex, abstract software-based inventions, where distinguishing genuine technological advancements from broad or abstract ideas can be difficult. Jurisdictions apply differing standards to patent eligibility, and many cloud patents face legal scrutiny due to evolving interpretations of software patentability. This variability means that not all patents counted in analyses necessarily represent robust or meaningful innovations. Furthermore, larger, leading firms tend to file patents emphasizing concrete technical contributions with a stronger legal footing, while smaller or non-leading firms may pursue broader or more strategic patent filings, which risks inflating patent counts without corresponding increases in technological impact.

4.2.3 Patents as an effective measure of cloud innovation

Despite the limitations discussed above, patents provide a reliable and standardized proxy for innovation in cloud computing for three key reasons. First, cloud innovations (algorithms, infrastructure management systems, and data processing methods) are frequently patented as software or business methods (Crémer et al., 2019). Second, unlike proprietary APIs or internal tools, patents represent credible, disclosed technologies observable to rivals and investors. Third, alternative measures (product releases, GitHub commits, customer growth) are either unavailable at the firm level or prone to greater measurement error due to cloud firms' secrecy around proprietary stacks. Under data constraints, patents thus offer the most comparable measure of inventive activity.

The simple patent count is the most straightforward measure of a firm's patent output. The innovation literature criticizes this measure for its wide variation and lack of accountability for quality (Hall et al., 2001, 2005a,b), preferring patent citations to patent count due to its ability to capture quality. Therefore, citation-weighted patent counts are often seen as a good indicator, as they incorporate information about both the number of patents and the quality of patents. One concern with patent-based measures is truncation in both counts and citations, as recent patents have less time to be recorded, granted, and cited Hall et al. (2005). Hall et al. (2005) address truncation via statistical fixed effects and citation-lag models that extrapolate from historical patterns. [17] Our method is superior because it provides complete coverage within our analysis window: we allow a fixed three-year post-merger period for all mergers while collecting raw patent data through 2020. For a Q4-2014 merger, patents are

have a relatively minor influence on the overall share price of the acquiring company. Hence, we exclusively focus our analysis on patents.

[17] We could not construct the similar adjustment as only the aggregated number of forward citation is observed in our data, but not the details of these citations.

observable through 2020 (six+ years post-merger), capturing even those with long filing/granting lags, eliminating truncation within our three-year effects window without extrapolation. This procedural completeness is particularly valuable in fast-moving cloud computing. Nonetheless, we remain mindful of citation-window effects when interpreting citation-weighted metrics.

However, the patent count measure has key merits. It effectively captures patenting trends and is widely used in the literature, partly because it is straightforward to compute, simply tallying the number of patents filed. By contrast, citation counts require analyzing references from later patents and can be affected by examiner practices or applicants' strategic behavior, introducing subjectivity. Citations also appear only after a time lag, reducing their usefulness for assessing recent innovations, as in the emerging cloud computing industry, and making citation-weighted measures less reflective of current patent quality. For these reasons, we do not use citations as the main outcome in the preferred matching analysis, though we include them for robustness checks (with citation-weighted patent counts) and (in lagged form) as controls in the propensity score matching. Following Trajtenberg (1990), the citation-weighted patent counts [18] of a firm i in a period (year) is given by the formula:

$$CWP_{it} = \sum_{i=1}^{n_t}(1 + C_i)$$

, where n_t is the number of granted patents that the firm applied for in year t, C_i is the number of forward citations that each patent receives.

In addition to patent counts, we construct indicators of pre-merger patent activity: citations per patent and patent stock. Following Haucap et al. (2019), we take their values in 2007 (before our analysis period) for propensity score estimation. We compute firm i's patent stock (PS) in year t by the formula $PS_{it} = (1 - \delta)PS_{i,t-1} + P_{it}$ as in Bloom and Reenen (2002). We set the discount factor δ to be 0.15; a typical value in the innovation literature (Bloom and Reenen, 2002; Bloom et al., 2016). P_{it} denotes the number of new patents granted that year, and PS in 1998 is set to zero, the first year cloud-related patents are observed. Patent measures for acquirer and target are combined.

4.2.4 Patent data description

We downloaded information on 10,836 patents related to cloud technology from the U.S. Patent Office (USPTO) Patentsview API. The patent data includes information like patent's owner, application date, grant date, Cooperative Patent Classification (CPC) category of the patent and citation-specific information like number of citations that the patent received or cited. Patent and citation related metrics (i.e. patent count per year, citation-weighted patent counts, number of citations per patent) will be aggregated to the firm-level and then matched with firm's merger data. Based on this data, we then constructed the measure for innovation using a simple count of the number of new patents granted to a firm in a year. Patenting is a lengthy process. It

[18] We employ the term citation-weighted patent counts from Hall et al. (2005a)

takes months or years to grant patents after filing them. The lengthy, uncertain, and costly patenting process discourages small firms from patenting their inventions when the costs outweigh their commercial values. In addition, there are alternative options to protect inventions, like trade secrets or copyrights. Hence, patents do not capture all innovation activities and may undermine the innovation output, particularly for small firms. Finally, not all patents will lead to inventions/commercial values, as firms may use patents to protect their existing patents rather than innovate. Despite all these limitations, patent activity is still a reliable measurement of innovation, as it is closely related to innovation output, the bias against small firms exhibits milder effects in our dataset since a considerable number of these firms are excluded from our analysis upon the matching of merger entities and control firms.

Table 4 reports descriptive statistics for merging entities pre-merger. Average annual new patents are 11.55, with a mean patent stock of 28.57. R&D intensity is high at 12.07%, suggesting strong innovation incentives or inclusion of non-cloud R&D. Sales, net income, and assets indicate these are all large technology firms, consistent with their capacity for high-value acquisitions (as in Table 2).

Table 4: Summary statistics of merged entities based on pre-merger observations

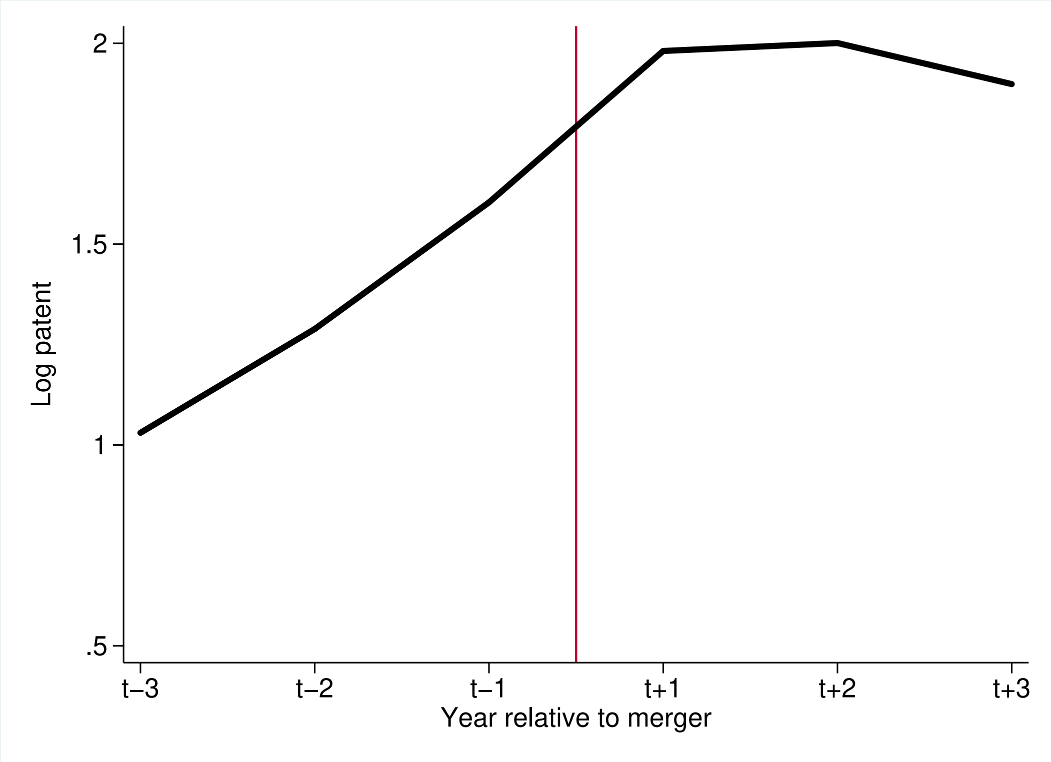
variable	mean	std	min	max
patents per year	11.549	23.610	0	134
citations per patent	12.216	38.782	0	276
citations-weighted patents	83.686	115.869	0	474
patents stock	28.570	63.952	0	379.369
net sale (million US$)	23252.056	31493.637	63.563	112069.158
R&D/sale (%)	12.074	6.368	0	25.550
net income (million US$)	2782.319	5167.197	-3122.808	23150
total asset (million US$)	34325.764	49620.303	47.338	220005
total debt/total asset(%)	13.212	11.261	0	41.180
gross profit margin(%)	63.147	20.219	10.200	90.960
leader dummy	0.196	0.401	0	1
multi-sided platform dummy	0.497	18.851	0	1
public target dummy	0.039	0.196	0	1
high-value acquisition ($> 1B$$)	0.098	0.300	0	1

4.2.5 Descriptive evidence-innovation activity before and after

Figure 2 shows how the (log) patent count of merging entities, calculated as the combined total for each acquirer and its target, evolves around the merger year. This is descriptive evidence only; it does not establish causality or control for overall time trends, but simply illustrates innovation patterns before and after the merger.

On average, patent output rises gradually in the years preceding the merger, peaks in the year immediately after, and then levels off. The figure plots the mean value of $\log(1+\text{patents})$ for all 62 target–acquirer pairs across the pre- and post-merger years. For example, one observation corresponds to Riverbed Technology and Thoma Bravo (item 10 in Table 2), with patent data for 2011 ($t-3$), 2012 ($t-2$), 2013 ($t-1$), 2015 ($t+1$), 2016 ($t+2$), and 2017 ($t+3$).

Fig. 2: Log (1+patents) (before and after)



Notes: The red line is the year of the merger.

5 Empirical strategy

We first present illustrative results from a two-way fixed effects model, with our main analysis using propensity score matching (PSM) combined with difference-in-differences (DiD) estimator to estimate the causal effect of mergers on the innovation of merging entities. The effect is defined as the difference between the observed post-merger outcome and the counterfactual outcome that would have occurred had the merger not taken place. In this setting, the counterfactual is constructed by matching treated firms (merging entities) with similar non-merging firms, based on pre-merger characteristics, to control for confounding factors. This approach helps address one of the main challenges in causal inference, isolating the effect of the merger from other variables that might also influence patenting activity. Hence, a simple regression without matching might not fully account for confounding factors, which means the estimated average treatment effects might be biased. Propensity score matching helps address these challenges by creating a balanced comparison group that closely resembles the treatment group in terms of observed characteristics. While propensity score matching (PSM) does not correct measurement error in the outcome variable itself, it can reduce bias from measurement error in the covariates used for matching, provided such error is not systematically related to treatment assignment. Measurement error in covariates can lead to imperfect balancing between treated and control groups, increasing the risk of omitted variable bias in treatment effect estimation. By matching treated and control units based on the propensity score (their likelihood of receiving the treatment), we can minimize the differences/imbalance in characteristics between the two groups caused by measurement errors that could influence the treatment effect. This reduces the likelihood that the observed outcomes are due to confounding variables other than the treatment itself, leading to more accurate and reliable estimates of the treatment's impact (refer to Cunningham, 2021 for an accessible reading on this topic). By carefully selecting and verifying the quality of our matching variables, and by using sources and definitions that are consistent across firms, we reduce random noise in these covariates and improve the comparability of matched pairs.

We aim to quantify the average treatment effect on the treated (ATT) k periods after the merger. We label with t the merger period and with i the firm. We denote with $P^1_{i,t+k}$ and $P^0_{i,t+k}$ the patent output k periods after the merger, in its occurrence (superscript 1), and in the counterfactual situation of its absence (superscript 0). The outcome depends on a set of pre-merger control variables $X_{i,t-1}$. We include only pre-merger values to mitigate the reverse causality problem. Our key variable of interest, the merger indicator $\mathrm{MA}_{i,t}$ is a dummy, which takes the value one if there is a merger (belongs to the group of merging firms) and zero (belongs to the group of control firms) otherwise. The ATT can be expressed as follows:

$$\begin{aligned} ATT_k &= E\left(P^1_{i,t+k} - P^0_{i,t+k} | X_{i,t-1}, \mathrm{MA}_{i,t} = 1\right) \\ &= E\left(P^1_{i,t+k} | X_{i,t-1}, \mathrm{MA}_{i,t} = 1\right) - E\left(P^0_{i,t+k} | X_{i,t-1}, \mathrm{MA}_{i,t} = 1\right). \end{aligned} \tag{1}$$

A widely used approach to estimate the ATT is to construct a panel of treated firms (merging entities) and a control group of firms with similar characteristics but unaffected by the merger. Identifying a suitable control group is the core step, as it allows estimation of the merger's causal effect on innovation using the DiD estimator. This is challenging for cloud computing, a global market, where perfect comparators are hard to find in either geographic or product space. Hi-tech products with similar innovation trends are scarce, and non-merging cloud computing firms are problematic because their innovation could be indirectly influenced by merger activity. As shown in the data section, most cloud computing firms are young, lack IPO status, and thus have no publicly available R&D or financial data, explaining why the number of merger observations more than halves. Additionally, these firms did not have any granted patents during our chosen analysis period. Hence, it is impossible to satisfy the condition of parallel trend in patent activity between treatment (merged) firms and control firms, which is the key assumption to identify the treatment effects using the difference-in-differences (DiD) methodology. For these reasons, we could not choose non-merging cloud computing firms as control firms in our analysis.

Instead, we use firms outside the cloud computing market that nonetheless hold cloud-related patents as the control group. These firms, by lacking a direct market presence, are plausible controls since they are not in direct competition with merged entities.[19] While this approach makes it feasible to estimate treatment effects using a group with comparable patenting activity, we recognize that it might introduce a potential risk of selection bias, as our results may not generalize to all cloud computing firms but only to those actively patenting (and possibly more engaged in innovation or acquisition activities).

One limitation is that the control group's innovation might not be fully independent of treatment. For example, Samsung, outside the cloud market but holding cloud-related patents, relies on Amazon services; mergers that increase Amazon's market power could raise prices, prompting Samsung to invest more in cloud innovation to reduce its dependence. Such interdependence can bias ATT estimates, with the sign depending on substitutability or complementarity between treatment and control groups, though formally testing this is complex.

The second requirement is that treatment and control groups share similar characteristics and pre-treatment innovation patterns (parallel trends). We address this by first estimating the propensity score, $\Pr(MA_{i,t} = 1|X_{i,t-1})$, via a Probit model. Covariates include the log of new patents (lagged one year), and, following Haucap et al. (2019), the log of pre-sample (1999–2007) patent stock. Additional one-year-lagged variables are: log citations per patent, log net sales, R&D-to-sales ratio (%), net income, log total assets, total-debt-to-assets ratio (%), and gross profit margin (%). For firms with multiple mergers within a year, we treat them as one merger with multiple targets; mergers in different years are separate observations.[20] Using nearest-neighbor matching, we select controls with similar characteristics and pre-merger innovation patterns; the balance is shown in the results section.

We then estimate the ATT with the DiD model:

$$\Delta \ln P_{i,t+k} = \beta_k + \gamma_k \mathrm{MA}_{i,t} + \varepsilon_{i,t+k}, \tag{2}$$

where γ_k measures the ATT for $k = 1, 2, 3$. Here, $\Delta \ln P_{i,t+k}$ is the change in ln(1 + new patents) from $t-1$ to $t+k$ between treatment and control units. The DiD estimator removes time-invariant unobservables but not time-varying ones; endogeneity of $\mathrm{MA}_{i,t}$ remains possible. While Haucap et al. (2019) propose IV methods, we lack suitable instruments and instead rely on the parallel-trends assumption.

Beyond the average effect, we also test heterogeneous impacts by firm type: leaders vs. non-leaders, multisided platforms (MSP) vs. one-sided firms, public vs. private acquisitions, and deals over $1B. Leading firms are expected to gain more efficiencies from mergers, enhancing innovation.[21] In cloud computing, efficiency involves cost-effective, reliable, and high-performance delivery of computing resources (Rashid and Chaturvedi, 2019). MSPs may leverage mergers through indirect network effects.

[19] Companies are categorized as participating in the cloud computing market if they generate revenues from cloud computing products within our analysis time frame, spanning 2008–2017. This period includes three years before the first merger in our sample and three years after the last. Firms not offering cloud products in this window but later acquiring a cloud firm, e.g., Nvidia's 2019 purchase of Mellanox, may still be considered as controls. Results remain consistent when such cases are excluded.

[20] Szücs (2014) recommends removing firms with multiple mergers to avoid confounding effects, but our sample is too small to do so.

[21] Efficiency and innovation are closely linked: efficiency maximizes output from resources, while innovation creates new products or processes that drive competitiveness.

We estimate:

$$\Delta \ln P_{Z,i,t+k} = \beta_{Z,k} + \gamma_{Z1,k}\mathrm{MA}_{i,t} + \gamma_{Z2,k}\mathrm{MA}_{i,t} \times \mathrm{Z}_i + \varepsilon_{Z,i,t+k}, \tag{3}$$

where $Z \in \{\text{leader, MSP, public, acquisition value} > 1B\$\}$.

Finally, as mergers occur at different times, staggered-treatment estimators such as Callaway and Sant'Anna (2021a) could be appropriate; however, our limited cross-section and time coverage make such methods less suitable here.

6 Results

6.1 Two-way fixed effects regression results

Before analyzing the main results from the DiD regression with propensity score matching, we run a two-way fixed effects regression based on the sample containing all merged firms and control firms prior to the matching. To be specific, we regress two patent measurements: simple patent count and citation-weighted patent counts on the merger indicator dummy, that takes the value one for all post-merger observations of the merged entities, and zero otherwise. We control for the time and firm effects. The results are presented in Table 5. As can be seen in Table 5, there is a mixed evidence of how mergers would impact patent outcome. Although mergers would lead to higher simple patent count, the impact of these events on citation-weighted patent counts is negative. Mergers appear to stimulate patent quantity while reducing quality. This pattern aligns with the innovation diversion effect: merging firms shift toward more but lower-value patents to avoid cannibalizing existing products, as predicted by Federico et al., 2017, 2018. Hence, mergers boost innovative output in volume but may concentrate efforts on incremental rather than breakthrough patents. However, because of the potential citation truncation limitation mentioned above, we should keep it conservative when interpreting the impacts of mergers on citation related metrics.

Table 5: Fixed-effects regressions before matching

	(1)	(2)
	ln(1+patents)	ln(1+citations weighted patents)
MA	0.401**	-0.509**
	(0.163)	(0.199)
N	1331	1331

Notes: $p < .01$(***), $p < .05$(**), $p < 0.10$(*). The estimation sample includes 51 merger entities and 70 control companies before matching in the period 2008-2018. MA is an indicator that takes a value of one in all post-merger periods for the merged entity. Firm and year fixed effects are included. Variables of merged entities are based on consolidated companies before and after M&As. Clustered standard errors in parentheses.

While standard two-way fixed effects (TWFE) assumes unconditional parallel trends across heterogeneous firm types, we prefer a propensity score matching (PSM) combined with difference-in-differences (DiD) approach. This hybrid method first ensures covariate balance on pre-treatment observables before estimating treatment effects, addressing TWFE limitations. We detail the PSM-DiD implementation in the following section. We discuss patent citations and related robustness checks in a dedicated subsection, as our primary outcome is the simple patent count.

6.2 PSM-DiD regression results

We first discuss the propensity score (the probability of conducting a merger), whose estimates are reported in Table 6. The propensity score is estimated for three periods before the merger. Most of the estimated coefficients are not statistically significant (though some are mildly significant), except for the coefficient for gross profit margin. This latter suggests firms with higher profitability are more likely to be involved in merger activities.[22] The number of observations, 662, is the sum of 51 (treated firms) x 3 (periods) = 153 treatment observations and 509 control observations.[23] As this is an event study analysis, we are also able to control for the time effects of the year of the merger.

Table 7 shows the result of differences between the matched groups of treatment and control firms after applying the nearest neighbor matching. Some characteristics show significant differences between the two groups. However, the mean value of features like a log of sales, log of assets, debt/assets, and gross profit margins are not statistically different across treatment and control groups. Importantly, we do not reject the equality of means of the propensity score between the two groups. These results let us infer that firms in the treatment and control group after matching have a similar probability of merging and characteristics in the pre-merger period, validating the control group. The matching reduces the sample to 51 treatment units and 51 control units, for a total of 102 observations.

Next, in Figure 3 we plot the average trajectories of log patents for merged entities relative to the respective control firms after the matching. As can be seen, before the merger, the innovation patterns of merged entities and control firms display a parallel trend. Hence, the condition that treatment firms and control firms behave similarly is met. Furthermore, we conduct a regression-based test to assess the validity of the parallel trend assumption. The outcomes of this test are presented in Table A.1 in the appendix, affirming that we do not have grounds to reject the parallel trend hypothesis between the treatment and control groups. After the merger, the merged

[22]One could question a reverse causality, where the merger firms could also have higher profitability. However, this is more a concern for R&D investment than patents. The impact of financial considerations on R&D investment differs from that of patents. R&D investment is typically a crucial component of a business plan, with decisions being made on an annual basis, though the frequency may vary among firms. Aligning strategies with the fiscal year is a common practice for many businesses. A company's strong financial performance in year $t-1$ can often lead to increased allocations for R&D in year t (though business plans are often more affected by the anticipation that the realization of higher profits, in which case it would be $t+1$). On the other hand, patents operate under a different set of uncertainties. The success of an investment in patent development is uncertain, and the process of obtaining patent registrations can be time-consuming, often spanning two to three years, with patent citations taking even longer to materialize.

[23]The number of control observations is not a multiple of three, as certain control firms lack observed covariates across all periods.

Table 6: Propensity score estimation

variable	coefficient	se
$\ln(1{+}\text{patents})_{t-1}$	0.307	0.223
$\ln(1 + \text{patent stock})_{2007}{}^{1}$	-0.157	0.396
$\ln(1{+}\text{citations per patent})_{t-1}$	0.314*	0.182
$\ln(\text{net sales, M\\$})_{t-1}$	0.968	0.551
$\%(\text{R\&D/net sales})_{t-1}$	-0.062*	0.032
$(\text{net income, M\\$})_{t-1}{}^{2}$	-0.062	0.049
$\ln(\text{total assets, M\\$})_{t-1}$	-0.775	0.495
$\%(\text{total debt/total asset})_{t-1}$	-0.008	0.016
$\%(\text{gross profit margin})_{t-1}$	0.067***	0.015
N	662	
R-squared	0.324	

Notes: $p < .01$(***), $p < .05$(**), $p < 0.10$(*). The dependent variable is one in the case of a merger. Time-varying regressors lagged one year relative to the merger. The regression includes time-fixed effects.

[1]Just like in Haucap et al. (2019), we have computed the patent stock exclusively for the year 2007. This aligns with the fact that our initial propensity score estimation begins in the subsequent year.

[2]Net income is presented in levels, reflecting the presence of negative values.

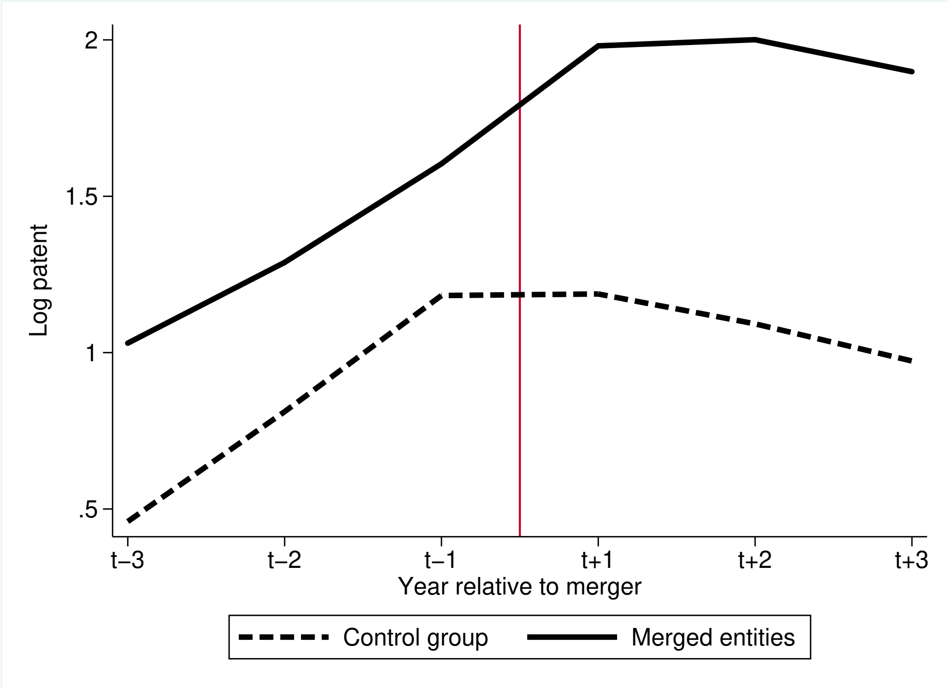
entities augment their patent activity in year $t + 1$. Then while the number of patents by merged entities becomes flat in year $t + 2$ before a fall in $t + 3$, the control group experiences a gradual decrease in $t + 2$ and $t + 3$. Though we observe a fall in patents in the third year after the merger, the level of patents by merged entities is still higher than the pre-merger level. Whereas, in the case of control firms, the level of patents in year $t + 3$ is lower than its pre-merger value in a year. This aftermath visually shows that mergers have the potential to impact positively the patent activity of merged entities.

Table 7: Balancing property after matching

variable	treated	control	t-stat	p-value
$\ln(1{+}\text{patents})_{t-1}$	1.604	1.183	1.809	0.074
† $\ln(1 + \text{patent stock})_{2007}$	0.199	0.300	2.090	0.041
$\ln(1{+}\text{citation per patent})_{t-1}$	1.533	1.133	1.683	0.095
$\ln(\text{net sales, M\\$})_{t-1}$	15.456	14.773	1.549	0.125
$\%(\text{R\&D/net sales})_{t-1}$	12.074	13.137	-0.684	0.496
$(\text{net income, M\\$})_{t-1}$	2.782	1.415	1.663	0.101
$\ln(\text{total assets, M\\$})_{t-1}$	15.846	15.114	1.674	0.097
$\%(\text{total debt/total asset})_{t-1}$	13.212	10.410	1.360	0.177
$\%(\text{gross profit margin})_{t-1}$	63.147	66.524	-0.765	0.446
propensity score	0.302	0.290	0.292	0.771

Notes: The table shows mean differences between treated (merging entities) and control observations for the matched sample, based on the propensity score. †See note in Table 6.

Fig. 3: Trajectories of log patent count for merged entities and control group



Notes: t denotes the time period in which the merger takes place.

The main results of the estimated ATT effects of mergers on patents of merged entities are documented in Table 8. The merged entities augment their patents by 45%, 63%, and 66% in the first year, second year, and third year after the merger. Consistently with the visual evidence provided in Figure 3 the magnitude of the effect grows. These effects are statistically significant. Therefore, we have evidence that mergers impact positively the innovation activities of merging firms. This effect is probably because mergers generate enough synergies/efficiencies to boost the innovation outcome. These results suggest that M&A in this market do not reduce the volume of patenting, although, if the findings from the previous section generalize, they may still be detrimental to the quality of innovation.

Table 9 documents the results of heterogeneous effects of mergers on innovation by different groups of firms. The comparison of leading firms versus non-leading firms is displayed in the first three columns of the top panel. The estimated coefficient on the interaction term between two dummies of merger event and leading firms is positive and statistically significant, with the highest effect achieved in year three after the merger. Mergers would be estimated to increase the innovation measured by patents of leading firms by 212% relative to other firms in the third year after the merger. This effect confirms that while mergers may not have an overall significant impact on merged entities, the leading firms benefit from their acquisitions and improve their innovation outcome significantly. The intuition is that leading firms can generate more

Table 8: Average treatment effects on the treated (ATT) estimation results

	Δ log(1+patents)		
	(1) t+1	(2) t+2	(3) t+3
MA	0.372**	0.487***	0.504***
	(0.143)	(0.148)	(0.173)
N	102	102	102

Notes: $p < .01$(***), $p < .05$(**), $p < 0.10$(*). The table shows regressions based on the matched sample after using propensity score matching. Dependent variable is $\ln(1+\text{patents})_{t+k}$ - $\ln(1+\text{patents})_{t-1}$, where t refers to the year of the merger. Robust standard errors are in parentheses. MA is an indicator variable that takes on a value of one if a firm is involved in a merger.

synergies from mergers, and fully capitalize on these effects to boost their innovation compared to non-leading firms. Moreover, as analyzed above, leading firms are more likely to acquire young start-ups with talented people and innovative projects. Hence, mergers would enable these projects to be successful, and drive innovation outcomes.

The last three columns of the top panel show how mergers affect MSP firms versus non-MSP firms. As expected, the coefficients for the interaction between MA and MSP are highly positive and significant in both periods t+2 and t+3. For instance, three years after the merger MSP firms are expected to increase their innovation by 68% relative to non-MSP firms. This result is possible because of the role of indirect network effects. By acquiring other firms with different/complementary products, MSP firms could expand their range of products/services, attract more users, and drive more developers to join their cloud marketplace. This effect would lead to more sales revenue, enabling firms to spend more on R&D investment and eventually generate a healthier innovation.

When facing the trade-off between building and buying, firms may prefer to use acquisitions to access competitors' patents and technologies instead of developing themselves. Hence, one can wonder whether the results above are actually driven by the firm's acquisitions in years that are subsequent to the mergers identified in our data sample. While it is true that the leading firms and MSP firms in our analysis continue to acquire other cloud companies after our data sample period, this does not affect the above results/findings for two reasons. First, most of the targets acquired by leading firms and MSP firms are young companies/start-ups, which do not have any patents or very few patents. Second, even when these targets have a number of patents, these patents have not been reassigned to acquirers, which means that our measurement of patents for leading/MSP firms is not affected by these acquisitions.

The findings from the bottom panel of Table 9 reveal other noteworthy trends in patenting behavior following M&A activity. Specifically, patents tend to increase significantly for public acquisitions compared to private acquisitions. This observation suggests that when acquirers target public companies, there may be greater synergies at play, resulting in heightened innovation and patenting activity. Public targets, being larger and subject to regulatory scrutiny, often necessitate a more thorough and strategic merger policy. This, in turn, can lead to enhanced efforts to integrate and leverage the technological capabilities of both firms, contributing to an upsurge in patenting activity, and hinting towards an effective merger policy.

Interestingly, the data do not demonstrate a significant difference in patenting behavior between very large acquisitions (exceeding 1 billion) and smaller acquisitions. This unexpected finding suggests that the deal value alone might not be the primary driver of increased patenting activity. While large acquisitions involve substantial financial investment, they may not consistently lead to a significant boost in innovation or patenting. Instead, other crucial factors, such as the strategic fit between acquiring and target firms' technologies, culture, and capabilities, likely exert a more influential role in determining the impact on patenting behavior.

Appendix Figure A.1 shows parallel trends by firm type, followed by four tables displaying the balancing properties of the four heterogeneous effects. The assumption holds clearly for leading firms and MSPs, but is more questionable for the smaller samples of public listed targets and high-value stakes.

This exercise highlights the importance of considering the nature of the target firm and the deal size when examining the patenting behavior resulting from M&A activity.

Our results are consistent with theoretical evidence by Bourreau et al. (2025), and Jullien and Lefouili (2018) that mergers can have overall positive or negative impacts on innovation. The key policy implication from our result is that mergers do not necessarily lead to less innovation. Our intuition here is in the cloud computing market, most of the merger cases are young start-up firms acquired by big tech firms, and this type of merger is more likely to promote innovation in this specific market. As mentioned in the descriptive evidence, the leading firms in the cloud computing market prefer to acquire young firms that develop complementary instead of substitute products. Thus, the merger would not generate any cannibalization effect, which could discourage the innovation incentive. Since leading firms in cloud computing acquire young start-ups because of their talented employees, innovative assets, or R&D projects, the merger would generate synergies, for instance, by allowing start-ups to have enough funding to develop their projects. These synergies would enhance significantly the innovation outcome, which would result in new or higher quality products/services and increased consumer welfare. Mergers in the cloud computing market do not necessarily soften market competition, as the key players remain the same. They compete intensely to acquire more market shares and profit in this expansive market. Therefore, there is no evidence to believe that innovation is harmful because of M&A weakening competition in the market.

Table 9: Effects on leading vs non-leading firms, MSP vs non-MSP, public vs private targets, and acquisition value $> 1B\$$ vs $< 1B\$$

	Δ log(1+patents)					
	(1) t+1	(2) t+2	(3) t+3	(4) t+1	(5) t+2	(6) t+3
MA	0.282**	0.413***	0.370**	0.272*	0.319*	0.320
	(0.146)	(0.151)	(0.174)	(0.155)	(0.166)	(0.197)
MA × leader	0.762**	0.635*	1.139***			
	(0.325)	(0.356)	(0.281)			
MA × MSP				0.284	0.478**	0.521**
				(0.249)	(0.229)	(0.251)
N	102	102	102	102	102	102
MA	0.346**	0.451***	0.488***	0.387***	0.463***	0.505***
	(0.143)	(0.146)	(0.175)	(0.143)	(0.149)	(0.179)
MA × public target	1.354***	1.836***	0.820***			
	(0.113)	(0.110)	(0.130)			
MA × acquisition value$> 1B$				-0.251	0.411	-0.013
				(0.693)	(0.598)	(0.388)
N	102	102	102	102	102	102

Notes: $p < .01(***)$, $p < .05(**)$, $p < 0.10$ $(*)$. The table shows regressions based on the matched sample after propensity score matching. The dependent variable is ln(1+patents)$_{t+k}$ - ln(1+patents)$_{t-1}$, where t is the merger year. Robust standard errors are in parentheses.
Treatment firms are cloud computing market operators with observed M&A from SDC Platinum ($MA_t = 1$). Controls are patenting firms outside cloud computing markets ($MA_t = 0$, no observed M&A). PSM matches on pre-merger observables (size, age, patents). Coefficient interpretation: Using the notation from Equation (3), γ_{z1} = post-merger effect for non-leader mergers vs. matched controls; $\gamma_{z1} + \gamma_{z2}$ = post-merger effect for leader mergers vs. matched controls. Leader is a dummy variable taking the value one if the firm is in the group of leading firms. MSP is an indicator that takes the value one if firms have a multi-sided platform business model. Public target is a dummy variable taking the value one if the target firm is public at the time of the acquisition. Acquisition value $> 1B\$$ is an indicator that takes the value one if the acquisition value is greater than 1 billion USD.

6.3 Robustness checks

In this section, we will outline a series of checks aimed at confirming the robustness of our findings.

6.3.1 Most popular patent category

Recognizing the potential contention that post-merger patent outcomes might be influenced by patents of lesser significance, we conduct an additional DiD regression using patent counts from the category G06F - Electric digital data processing within the CPC system. Notably, this category holds paramount importance in our patent dataset, encompassing 74% of the cloud computing patents in our sample. As a result of only counting the patents in this category, our sample has been reduced to 66 observations: 33 treated firms and 33 control firms. In addition, merged entities with public target has also been dropped out due to the fact that they do not have any patent in the category G06F. Therefore, we could not estimate the moderating effects of public targets in this case. The outcomes of the average treatment effects on the treated

(ATT) and moderating effects are documented in Tables 10 and 11. As evident from Table 10, the calculated ATT effects continue to exhibit positive sign in all periods and remains statistically significant in years two and three post-merger. Table 11 corroborates the trends observed in our benchmark outcomes. Notably, the moderating effects attributed to leading/MSP firms companies remain positive and considerably more pronounced than the primary ATT in the case of MSP firms. However, the role of acquisition value in influencing the patent outcomes within the G06F category appears mixed: firms acquiring high-value targets exhibit a lower patent count in this category one-year post-merger. Conversely, this relationship is reversed in the subsequent two periods, although these effects lack statistical significance.

Table 10: Average treatment effects on the treated (ATT) estimation results with cloud patent count in the CPC category G06F as the outcome

	Δ log(1+patents)		
	(1) t+1	(2) t+2	(3) t+3
MA	0.266	0.511**	0.487**
	(0.205)	(0.215)	(0.224)
N	66	66	66

Notes: Refer to the notes in Table 8 for further details.

6.3.2 Winsorizing skewed covariates in the propensity score estimation

In order to mitigate the potential influence of outliers, we re-conduct the propensity score matching and DiD regression analyses following the application of 90% winsorization to the two most skewed covariates: %R&D/net sales and net income (both exhibiting a skewness exceeding 2).[24] The calculated ATT and moderating effects subsequent to winsorization are displayed in Tables 12 and 13, respectively. These outcomes resemble our benchmark results, providing robust evidence that our findings remain unaffected by the presence of outliers.

6.3.3 Using citations-weighted patent counts as outcome variable

As discussed earlier, the utilization of patent count as an outcome variable may not fully encompass innovation quality. To address this limitation, alternative metrics incorporating patent citations are employed. Here, we present estimation outcomes using citation-weighted patent counts as the outcome variable to capture the impact of mergers on patent quality.

[24]This involves replacing the top 5% of the variable with its value at the 95^{th} percentile, and similarly, substituting the bottom 5% of the variable with its value at the 5^{th} percentile.

Table 11: Effects on leading vs. non-leading firms and MSP vs. non-MSP with cloud patent count in the CPC category G06F as the outcome

	Δ log(1+patents)					
	(1) t+1	(2) t+2	(3) t+3	(4) t+1	(5) t+2	(6) t+3
MA	0.213	0.483**	0.433*	0.020	0.157	0.242
	(0.218)	(0.229)	(0.236)	(0.223)	(0.223)	(0.257)
MA × leader	0.582***	0.309	0.587***			
	(0.217)	(0.328)	(0.183)			
MA × MSP				0.737**	1.063***	0.732**
				(0.347)	(0.319)	(0.304)
N	66	66	66	66	66	66
MA	0.266	0.511**	0.487**	0.304	0.506**	0.481**
	(0.205)	(0.215)	(0.224)	(0.207)	(0.221)	(0.230)
MA × public target	NA	NA	NA			
	NA	NA	NA			
MA × acquisition value$> 1B$				-1.253***	0.159	0.183
				(0.172)	(0.179)	(0.174)
N	66	66	66	66	66	66

Notes: Refer to the notes in Table 9 for further details.

Table 12: Average treatment effects on the treated (ATT) estimation results after 90% winsorization

	Δ log(1+patents)		
	(1) t+1	(2) t+2	(3) t+3
MA	0.372**	0.487***	0.504***
	(0.143)	(0.148)	(0.173)
N	102	102	102

Notes: Refer to the notes in Table 8 for further details.

The estimated ATT of mergers on citation-weighted patent counts in Table 14 exhibit similar positive signs as in our benchmark results but are not statistically significant. This result contrasts with the negative coefficient from the two-way fixed effects analysis. Overall, the evidence suggests that the additional patenting induced by mergers does not come at the expense of quality.

Similarly, the moderating effects of leading and MSP firms, as reported in Table 15, lack statistical significance, except for the positive and significant effect of leading firms at the $t+3$ period. In contrast, the effects of public targets and acquisition value show statistical significance at both the $t+1$ and $t+3$ periods post-merger, albeit with negative implications. This suggests that merged entities acquiring public targets and

Table 13: Effects on leading vs non-leading firms, MSP vs non-MSP, public vs private targets, and acquisition value $> 1B\$$ vs $< 1B\$$, after 90% winsorization

	Δ log(1+patents)					
	(1) t+1	(2) t+2	(3) t+3	(4) t+1	(5) t+2	(6) t+3
MA	0.282**	0.413***	0.370**	0.272*	0.319*	0.320
	(0.146)	(0.151)	(0.174)	(0.155)	(0.166)	(0.197)
MA × leader	0.762**	0.635*	1.139***			
	(0.325)	(0.356)	(0.281)			
MA × MSP				0.284	0.478**	0.521**
				(0.249)	(0.229)	(0.251)
N	102	102	102	102	102	102
MA	0.346**	0.451***	0.488***	0.387***	0.463***	0.505***
	(0.143)	(0.146)	(0.175)	(0.143)	(0.149)	(0.179)
MA × public target	1.354***	1.836***	0.820***			
	(0.113)	(0.110)	(0.130)			
MA x acquisition value$> 1B$				-0.251	0.411	-0.013
				(0.693)	(0.598)	(0.388)
N	102	102	102	102	102	102

Notes: Refer to the notes in Table 9 for further details.

having higher acquisition values are more prone to experiencing diminished patent quality.

This divergence can be attributed to citation truncation: patents often continue to receive citations long after issuance, so recent post-merger patents have fewer opportunities to accumulate citations (Hall et al., 2005a). Our procedural design mitigates count truncation but citation truncation persists, muting power in quality measures. These results thus complement our primary quantity findings: raw patent counts (with complete coverage) remain our main measure, while citation-weighted counts results provide insight into quality patterns despite truncation noise.

Table 14: Average treatment effects on the treated (ATT) estimation results with citation-weighted patent counts as the outcome

	Δ log(1+citation-weighted patents)		
	(1) t+1	(2) t+2	(3) t+3
MA	0.166	0.255	0.035
	(0.281)	(0.256)	(0.289)
N	102	102	102

Notes: Refer to the notes in Table 8 for further details.

Table 15: Effects on leading vs. non-leading firms, MSP vs. non-MSP, public vs. private targets, and acquisition value $> 1B\$$ vs $< 1B\$$ with citation-weighted patent counts as the outcome

	Δ log(1+citation-weighted patents)					
	(1) t+1	(2) t+2	(3) t+3	(4) t+1	(5) t+2	(6) t+3
MA	0.086	0.228	-0.099	0.203	0.215	-0.058
	(0.293)	(0.270)	(0.299)	(0.312)	(0.297)	(0.321)
MA × leader	0.682	0.231	1.138**			
	(0.691)	(0.536)	(0.513)			
MA × MSP				-0.104	0.114	0.262
				(0.504)	(0.405)	(0.440)
N	102	102	102	102	102	102
MA	0.205	0.248	0.068	0.287	0.271	0.114
	(0.283)	(0.260)	(0.292)	(0.285)	(0.266)	(0.296)
MA × public target	-1.993***	0.352*	-1.684***			
	(0.230)	(0.199)	(0.207)			
MA × acquisition value$> 1B$				-2.059***	-0.280	-1.339***
				(0.294)	(0.332)	(0.278)
N	102	102	102	102	102	102

Notes: Refer to the notes in Table 9 for further details.

7 Conclusions and discussion

While there are increasing discussions and debates among researchers and practitioners on whether M&A by big tech firms harm innovation, there has not been any ex-post analysis providing empirical evidence to address this. This work aims to fill this gap by studying merger activities and their impacts on innovation in the cloud computing market.

Our first descriptive evidence suggests two different M&A strategies by leading and non-leading firms, defined by their market shares. The leading firms are more likely to target young start-ups; the non-leading firms prefer to acquire more established firms. This result complements previous studies on merger activities and strategies (Jin et al., 2023; Argentesi et al., 2021; Gautier and Lamesch, 2021); they only focus on big tech mergers' strategies, in general, and not on a specific market. Most importantly, in addition to descriptive evidence, we provide an ex-post-merger analysis to evaluate the impact on the innovation activity of merged entities. Our results suggest that mergers undertaken by leading, MSP firms or targeting a public company affect innovation in the cloud computing market positively. This result is in line with previous theoretical findings, confirming that the effect of mergers on innovation is not always negative and can be positive. The intuition is that in the cloud computing market, synergies are likely to occur between big tech firms with funding resources and project management experience and young start-ups with talented employees and innovative ideas. These synergies are more pronounced for leading, MSP firms, and when the targets are publicly traded companies, thanks to their ability to capitalize efforts and leverage synergies via indirect network effects. This amplification will eventually generate positive feedback on the innovation outcome of these firms. The value of the merger appears to have minimal significance. Our results help ease some of the rising concerns about the potential harms of mergers and acquisitions in the digital platform market.

We enhance the robustness of our analysis through a range of validation measures. This includes focusing patents on the extensively utilized cooperative patent classification, investigating the impact of winsorizing highly skewed covariates, and employing citation-weighted patent counts as the outcome variable. The outcomes with the widely adopted cooperative patent classification affirm the baseline findings, highlighting even more pronounced innovation values. Notably, outliers do not play a significant role in our data. However, results stemming from citation usage exhibit variations, but their limitations require warranting cautious interpretation.

Our study has some limitations and can be extended in several ways. First, we only have a small sample of merging firms over a short period relative to other studies. This means the number of firm-level observations across different segments (i.e, IaaS, PaaS, SaaS) in our sample is limited, making estimates at the segment level unlikely to be statistically robust. Thus, our study is limited to the analysis that aggregates across different cloud computing segments. The potential solution is to expand the number of merger cases by looking at extra merger cases in the rest of the world. Since firms in the cloud computing market are genuinely competing globally, it is sensible not to limit merger cases in terms of geographical borders. To deal with the short time frame, we can employ quarterly data instead of yearly data to enrich the variation over time. Yet, the issue here is the firms' financial data like revenue, R&D, and income may not be available quarterly. Secondly, firms with multiple mergers can likely cause some co-founding effects, which bias the results. However, we cannot exclude these firms as suggested in the literature due to our small sample. This problem can be solved when we extend our data sample. Thirdly, our propensity score matching procedure required us to restrict the sample to publicly listed cloud computing firms due to insufficient financial data for private companies. This inevitably introduces a selection bias towards more established firms. However, given that private cloud computing firms exhibit very limited patenting activity in our dataset, we believe that excluding these firms has a minimal effect on the robustness of our findings. Additionally, we have not dealt with the endogeneity of merger decisions by using IV regression as in previous studies. It is possible to extend our work by evaluating the impacts of mergers on the innovation activity of non-merging competitors in addition to merged entities as in Haucap et al. (2019). Furthermore, it would be interesting to analyze the effects of mergers on the entry and exit of the cloud computing market by using survival analysis. Finally, there is room to study the effect of patent originality, which reflects the novelty and uniqueness of an innovation. This metric would allow us to assess the breadth of search and the extent to which M&A activity may influence the generation of genuinely new and groundbreaking ideas. By incorporating originality as a measure, we could better assess whether anti-competitive M&A deals have any bearing on the quality of innovation produced by the firms involved. Regrettably, we lack information on patent originality, but it may be scope for future research.

Declarations

7.1 Funding

The authors did not receive funds, grants or other support from any organization for the submitted work.

7.2 Data Availability

The data that underlie the findings of this study, titled “Digital platform mergers and innovation: Evidence from the cloud computing market,” are proprietary. This paper presents an empirical analysis of mergers and innovation within the cloud computing market, one of the rapidly expanding digital sectors.

The dataset utilised in this research was collated from the Crunchbase website and encompasses information about US-based cloud computing companies. Additionally, data on acquirers and targets was sourced from Thomson Reuters Datastream and includes variables such as net sales/revenue, R&D investment, net income, total assets, total debt, and gross profit margins.

Due to license restrictions imposed by the two sources, we regret to inform you that we will not be able to share the original dataset. However, we would be willing to explain how a replicator could gain access to these sources. Indeed, we are fully committed to sharing all the codes utilised in this study, alongside an in-depth breakdown of the variables. This includes the identification of firms employed as acquirers, targets, and units in the propensity score matching. We intend to provide this information in the form of an Excel spreadsheet, organised with firm names as rows and variables as columns. We will also include a description of the time period of interest. Notably, the spreadsheet will incorporate an indicator distinguishing the type of firm (acquirer, target, control unit). This comprehensive approach ensures that anyone granted access can replicate our work, even if they possess access to the two original data sources.

For any queries related to data access or additional information, please do not hesitate to reach out to Thanh Doan: thanh.doan@ofcom.org.uk. We encourage fellow researchers to explore and validate our findings using this available dataset to contribute to the advancement of knowledge in the domain of digital platform mergers, innovation dynamics, and the cloud computing market.

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Appendix

A.1 Parallel trend assumption test

Following a common practice in the literature, we evaluate the parallel trend assumption through an event-study regression performed after the matching process. This regression incorporates both leads (post-treatment periods) and lags (pre-treatment periods) of the treatment dummy variable:

$$Y_{it} = \theta_t + \eta_i + \sum_{l=-K}^{K-1} D_{it}^{l} \mu_l + v_{it}. \tag{4}$$

In the presented model, Y_{it} denotes the outcome variable for firm i, specifically ln(1 + patents). θ_t and η_i represent the fixed effects associated with time and firm, respectively. The variable D_{it}^{l} takes the value one if firm i has undergone a merger for a duration of l periods at time t. For instance, if firm i merges at $t = 3$, then D_{it}^{0} equals one for $t = 3$ and zero for other periods, D_{it}^{3} equals one for $t = 6$ and zero for other periods, and D_{it}^{-2} equals one for $t = 1$ and zero for other periods. The parameter μ_l captures the impact of treatment for various exposure lengths to the treatment.

Following the established practice in the literature, we normalize μ_{-1} to 0. As a result, the interpreted effects of μ_l reflect changes in outcomes relative to the control group during the period l preceding the treatment period. Consequently, when the estimated μ_l values for $l < -1$ lack statistical significance compared to 0, the validity of the parallel assumption cannot be disregarded.

We execute the regression based on equation 4 using a sample comprising 51 treatment firms and 51 appropriately matched control firms throughout the period spanning from 2008 to 2017. Following Callaway and Sant'Anna (2021b), to address the issue of selective treatment timing in the event study regression,[25]we construct the aggregated group-year treatment effects by using the R package *did*[26]. The outcomes of the collective group-year treatment effects during the lag periods extending up to six years before the merger are documented in table A.1. Notably, the estimated effects lack statistical significance for all the periods ranging from $t - 6$ to $t - 2$, indicating that there is no basis to reject the parallel trend assumption.

A.2 Fixed-effects estimation results

We also run a simple before and after fixed effects regression, which ignores the selection bias and endogeneity of merger decisions. Based on the sample of 62 target firms, we regress several patent measurements on the dummy postMA$_{i,t}$, which takes the value one, up to three years after the merger and zero up to three years before the merger. The year of the merger is excluded from the analysis. Thus, the total number of observations is $62 \times 6 = 372$. We also control for the time and firm fixed effects. The

[25] For example, there would be selective treatment timing if a firm chooses to be merged in earlier periods in order to experience larger benefits from the merger.

[26] See more details about this package at https://cran.r-project.org/web/packages/did/vignettes/pre-testing.html

Table A.1: Parallel trend assumption test

variable	coefficient	se
lag_{t-6}	-0.612	0.338
lag_{t-5}	-0.219	0.253
lag_{t-4}	-0.086	0.171
lag_{t-3}	-0.112	0.110
lag_{t-2}	-0.184	0.112
lag_{t-1}	0	NA
N	1020	

Notes: $p < .01$(***), $p < .05$(**), $p < 0.10$(*). The regression includes time and firm-fixed effects. Only pre-treatment periods are reported.

results are presented in Table A.2. As can be seen in the table, there is evidence that merger events would increase the number of new patents of merged entities, but reduce patent quality. However, these results can be misleading as they ignore selection and endogeneity biases.

Table A.2: Before and after fixed-effects regressions

	(1)	(2)
	ln(1+patents)	ln(1+citations weighted patents)
postMA	0.619***	-0.212
	(0.113)	(0.169)
N	372	372

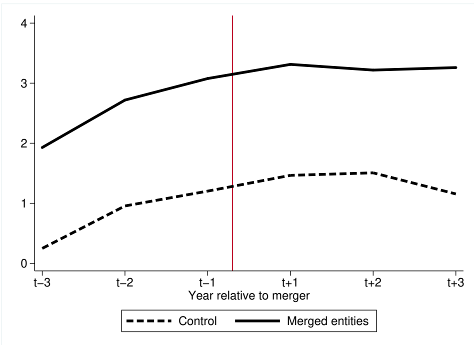
Notes: $p < .01$(***), $p < .05$(**), $p < 0.10$(*). postMA is an indicator that takes a value of one in all post-merger periods for the merged entity. Variables are based on consolidated companies before and after M&As. Clustered standard errors in parentheses.

Table A.3: Comparison of patenting activities between leading firms (Amazon, Microsoft, Google, IBM, Oracle, Salesforce, and Adobe) and non-leading firms (Cisco, Dell, Rackspace, etc.) in 2008-2017

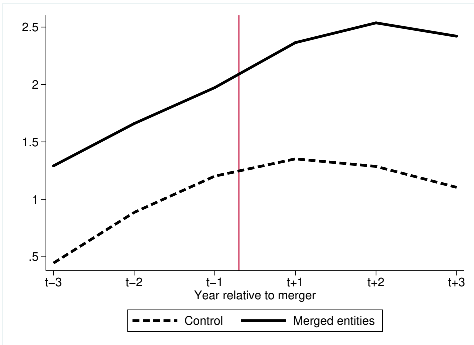
	leading firms					non-leading firms				
variable	obs	mean	se	min	max	obs	mean	se	min	max
patents per year*	70	33.50	43.50	0	187	1380	1.11	4.18	0	44
log(1+patents per year)*	70	2.76	1.36	0	5.240	1380	0.27	0.70	0	3.81
citations per patent*	70	4.81	7.22	0	36.80	1380	0.84	8.24	0	276

Note:* The mean values of the two groups are statistically different at a 5% significance level.

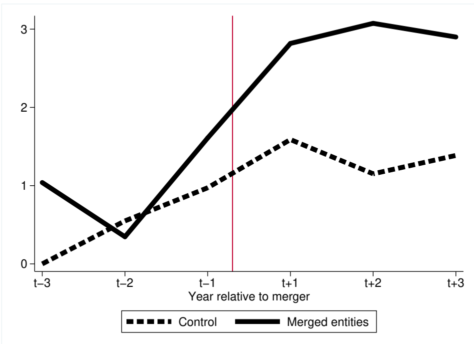
Fig. A.1: Trajectories of log patent count for merged entities and corresponding control firms



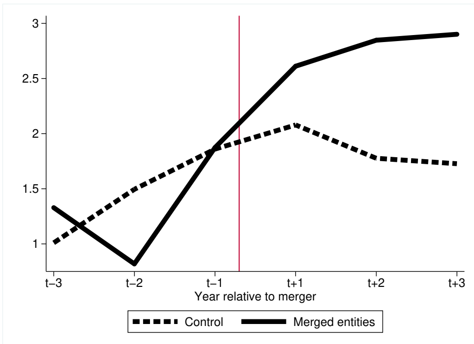
(a) Leading firms



(b) MSP firms



(c) Public listed targets



(d) Acquisition value > $1B

Notes: t denotes the time period in which the merger takes place.

Table A.4: Balancing property after matching of leaders

variable	treated	control	t-stat	p-value
ln(1+patents)$_{t-1}$	3.078	1.202	3.515	0.002
† $\ln(1 + \text{patent stock})_{2007}$	0.729	0.000	2.088	0.041
ln(1+citation per patent)$_{t-1}$	2.049	1.134	2.108	0.066
ln(net sales, M\$)$_{t-1}$	16.955	14.207	3.146	0.006
%(R&D/net sales)$_{t-1}$	12.176	12.571	-0.204	0.841
(net income, M\$)$_{t-1}$	8.916	1.304	2.693	0.021
ln(total assets, M\$)$_{t-1}$	17.411	14.606	3.352	0.004
%(total debt/total asset)$_{t-1}$	16.748	9.330	2.001	0.061
%(gross profit margin)$_{t-1}$	67.418	76.471	-0.965	0.348
propensity score	0.360	0.334	0.980	0.341

Notes: The table shows mean differences between treated (merging entities) and control observations for the matched sample, based on the propensity score. †See note in Table 6.

Table A.5: Balancing property after matching of MSP firms

variable	treated	control	t-stat	p-value
ln(1+patents)$_{t-1}$	1.973	1.202	2.444	0.018
† $\ln(1 + \text{patent stock})_{2007}$	0.322	0.000	2.499	0.018
ln(1+citation per patent)$_{t-1}$	1.884	1.159	2.359	2.022
ln(net sales, M\$)$_{t-1}$	15.522	14.544	1.847	0.070
%(R&D/net sales)$_{t-1}$	13.225	12.161	0.772	0.443
(net income, M\$)$_{t-1}$	4.091	1.272	2.233	0.031
ln(total assets, M\$)$_{t-1}$	16.051	14.973	2.053	0.045
%(total debt/total asset)$_{t-1}$	13.174	10.641	0.988	0.327
%(gross profit margin)$_{t-1}$	70.229	72.803	-0.543	0.589
propensity score	0.350	0.331	0.380	0.705

Notes: The table shows mean differences between treated (merging entities) and control observations for the matched sample, based on the propensity score. †See note in Table 6.

Table A.6: Balancing property after matching of firms acquiring public listed target

variable	treated	control	t-stat	p-value
ln(1+patents)$_{t-1}$	1.609	0.973	0.654	0.631
† $\ln(1 + \text{patent stock})_{2007}$	0.000	0.000	NA	NA
ln(1+citation per patent)$_{t-1}$	3.511	1.301	1.675	0.333
ln(net sales, M\$)$_{t-1}$	17.563	15.018	2.331	0.167
%(R&D/net sales)$_{t-1}$	6.935	12.670	-0.805	0.557
(net income, M\$)$_{t-1}$	2.480	0.476	8.083	0.058
ln(total assets, M\$)$_{t-1}$	18.169	15.467	2.091	0.183
%(total debt/total asset)$_{t-1}$	23.420	10.245	2.045	0.286
%(gross profit margin)$_{t-1}$	57.025	78.185	-0.543	0.589
propensity score	0.188	0.197	-0.101	0.929

Notes: The table shows mean differences between treated (merging entities) and control observations for the matched sample, based on the propensity score. †See note in Table 6.

Table A.7: Balancing property after matching of firms with acquisition value greater than \$ 1B

variable	treated	control	t-stat	p-value
$\ln(1+\text{patents})_{t-1}$	1.873	1.860	0.027	0.979
$\dagger\ln(1+\text{patent stock})_{2007}$	0.117	0.000	1	0.374
$\ln(1+\text{citation per patent})_{t-1}$	2.733	1.865	1.239	0.252
$\ln(\text{net sales, M\\$})_{t-1}$	16.154	15.845	0.275	0.792
$\%(\text{R\&D/net sales})_{t-1}$	10.824	13.884	-0.847	0.441
$(\text{net income, M\\$})_{t-1}$	2.459	3.051	-0.218	0.835
$\ln(\text{total assets, M\\$})_{t-1}$	16.748	16.370	0.322	0.759
$\%(\text{total debt/total asset})_{t-1}$	22.522	14.532	1.220	0.265
$\%(\text{gross profit margin})_{t-1}$	68.084	75.156	-0.896	0.407
propensity score	0.348	0.362	-0.093	0.928

Notes: The table shows mean differences between treated (merging entities) and control observations for the matched sample, based on the propensity score. †See note in Table 6.