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RESEARCH ARTICLE

Innovation and profitability following antitrust intervention against a dominant platform: The wild, wild west?

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Abstract

Research Summary: This study examines whether "unblocking" competition through antitrust intervention against a dominant platform can spur complementor innovation in platform ecosystems. Using a novel dataset on enterprise infrastructure software and a difference-in-differences design, we examine the relation between the U.S. antitrust intervention against Microsoft (dominant enterprise platform) and subsequent innovation and profitability of infrastructure applications firms (complementors). The data show that innovation among complementors—particularly ones with low market share—soared when the competitive pressure on the dominant platform amplified. However, the profitability of these complementors dropped. Our results contribute to understanding links between competition and innovation in platform ecosystems, as well as the opportunities and threats related to dominant platforms in those ecosystems.

Managerial Summary: Complementors (apps, services) that are owned by their platforms often have an unfair advantage. Antitrust action challenges the power of such dual platform-complementors, reasoning that unfair advantage blocks fair competition, and, in turn, reduces firms' incentives to innovate and thus limits consumer choice. We examine whether reducing anti-competitive barriers and platform-complementors' power revitalizes the platform ecosystem. Using a landmark antitrust case, we find mixed results: while complementors do innovate more, their profits go down. In particular, the low-share complementors that bring in the most innovation are also the ones that lose the most financially, suggesting that they may have over-relied on the platform for key assets. To develop a healthy ecosystem in the long run, platform owners may want to resist the temptation to keep complementors weak and instead help support their development to stand on their own.

KEYWORDS

antitrust, competition, innovation, platforms, technology ecosystems

1 | INTRODUCTION

> "*Microsoft created a weak, copycat product [Teams] and tied it to their dominant Office [platform], force installing it and blocking its removal. A carbon copy of their illegal behavior during the browser wars.*"—Workplace communications app Slack (complementor) announcing a complaint against Microsoft (platform-complementor) before the European Commission, 2020.

> "*Google has used Android [platform] as a vehicle to cement the dominance of its search engine... These practices have denied rivals [complementors] the chance to innovate.*"—European Commission, 2018.

Research argues that competition and innovation go hand-in-hand (Arrow, 1962; Katila & Chen, 2008; Thatchenkery & Katila, 2021). In the context of antitrust, dominant firms are argued to "block" competition by, for example, building barriers to entry or expansion, such as unequal access to distribution channels (Porter, 1979), or making it difficult or costly for rivals to compete, leading to innovation losses in their industries. Public policy interventions, including antitrust, aim to "unblock" competition to overcome anticompetitive barriers for the benefit of users and innovation (Khan, 2017).

Recent research extends arguments about competition and innovation into dominant *platforms*. Platforms are an interesting context because, in contrast to vertical industries, platform power extends beyond the platform market to affect innovation in *complementor* markets (Eisenmann, Parker, & Van Alstyne, 2011; Kapoor, 2018; Katila, Piezunka, Reineke, & Eisenhardt, 2022).[1] In particular, a dominant technology platform that offers products in its own complementor markets may discourage investment by rival complementors (Seamans &

[1]Complementors are actors in digital ecosystems that innovate on top of a platform's core resources with the intent to create products and services for the platform's end users. Collectively, the platform, complementors, and end users constitute an ecosystem (Teece, 2018).

Zhu, 2017; Zhu, 2019) or may exploit its market power to block complementors from growing enough to become a threat (Adner & Lieberman, 2021; Katila, Rosenberger, & Eisenhardt, 2008). In response, antitrust authorities worldwide have opened inquiries against Google, Amazon, Apple, Alibaba, and other technology platforms (Kendall & Copeland, 2020).[2] Yet despite substantial interest from policymakers (Crémer, De Montjoye, & Schweitzer, 2019) and experts in innovation, law, and economics (Butts, 2010; Jacobides & Lianos, 2021; Teece & Kahwaty, 2020), research in this area remains sparse, particularly from the complementor strategy perspective (Greene & Yao, 2014; Oberholzer-Gee & Yao, 2010).

A certain tension makes antitrust intervention in platform ecosystems particularly intriguing. Although dual platform-complementors may block rival complementors' opportunities to innovate (Kamepalli, Rajan, & Zingales, 2020), platforms often provide critical "infrastructure" for complementors (Ceccagnoli, Forman, Huang, & Wu, 2012; Katila et al., 2022; Zhu, 2019) and can help "maintain order" in the ecosystem (Gavetti, Helfat, & Marengo, 2017; Rietveld, Seamans, & Meggiorin, 2021). Wen and Zhu (2019), for example, found that Google's entry threat to Android reduced wasteful duplication such as redundant apps, and Zhang, Li, and Tong (2022) noted that Apple's iOS gatekeeping policies encouraged knowledge sharing by complementors. Diluting a platform's market power may thus backfire for complementors. The tension is an interesting one. In this article we ask, how does increased competitive pressure against a dominant platform through antitrust intervention relate to innovation and profitability of complementors?

We focus on a landmark antitrust intervention against a dominant platform owner, Microsoft, in which the platform was accused of, and subsequently prevented from, blocking "innovation competition" in complementor markets.[3] Microsoft blocked competition through (among other tactics) bundling its own infrastructure software with its enterprise platform and by creating efficient interoperability. Antitrust intervention reversed these effects. Using a difference-in-differences design that takes advantage of variation in Microsoft's market share across complementor markets, we examine how innovation and profit of U.S. enterprise infrastructure software firms changed after the intervention. A unique strength of our study is our detailed coverage of the population of complementors, as well as a battery of sensitivity and falsification tests, including matching, event studies, "honest" pre-trends tests, and 10-K data on perceptions of competition, to show that the observed effects are robust to a variety of empirical approaches. We supplement the quantitative analysis with interviews with industry participants and with a review of public documents regarding the case.

The key finding is that unblocking competition creates a "wild west" where many complementors race to innovate but fail to profit. Following the intervention, innovation in Microsoft's former complementor strongholds increased, consistent with the theoretical arguments that newly opened space would incentivize rival complementors to innovate. The boost was particularly pronounced among low-share complementors. Notably, firm entry did not increase, underscoring that innovation originated from incumbents' revitalized efforts, not firm turnover. However, complementors were not able to financially capitalize on their innovations:

[2]"Platform antitrust" differs from traditional antitrust because platforms are incentivized to share complementary assets with complementors in order to build the multisided ecosystem. In traditional vertical foreclosure, a vertically integrated downstream firm has no incentives to share assets with a rival upstream firm.

[3]Our focus is enterprise *infrastructure* software. In contrast, user-facing applications such as word processing and supply chain logistics are outside our scope. Prominent enterprise firms like SAP and Salesforce (Ceccagnoli et al., 2012) were *not* developing infrastructure software during our study period.

profits dropped after the intervention, suggesting that innovation increases did not turn into profitable sales increases. The tendency to boost innovation at the expense of profit was particularly pronounced for low-market-share firms, while high-market-share firms instead pursued internal efficiency.

Altogether, our findings emphasize the unique features of platform-based ecosystems. While much strategy research has focused on digital platforms' growing ecosystem footprint (Seamans & Zhu, 2017), and on the support that dominant platforms can provide to complementors (Ceccagnoli et al., 2012; Katila et al., 2022; Zhu, 2019), our findings contribute to an emerging stream that analyzes the *decreasing* influence of platforms and its implications for complementors, particularly regarding the *complementary assets* they now need to build on their own. More broadly, we add insight on how firms in technology markets interpret and respond to their competitive environment (Thatchenkery & Katila, 2021; Thatchenkery, Katila, & Chen, 2012), noting that who complementors perceive as main competitors changes after antitrust. Antitrust cases and competition regulation across the world (Hatmaker, 2022; Schechner & Mackrael, 2022) heighten the importance for the strategy field to understand these issues.

2 | RESEARCH BACKGROUND

Two streams of research are relevant for our research question. First, research on competition and innovation explains why complementors invest (or do not) in response to changing levels of competition from a dominant platform. Second, research on antitrust covers interventions that seek to make industries more competitive and more innovative.

2.1 | Competition and innovation

There is a long-standing debate about the relationship between competition and innovation (Chen, Katila, McDonald, & Eisenhardt, 2010; Katila & Chen, 2008; Thatchenkery & Katila, 2021). One side argues that the two go hand in hand; that is, markets with less competition have low incentives while markets with more competition have high incentives to innovate (Arrow, 1962). The logic here is that firms are pushed to pursue differentiation strategies when competition pressure increases while the absence of competitive threat makes firms complacent. The other side argues that innovation incentives are higher the more insulated the firms are from competition. Here, insulation is thought to provide firms with the resources and predictability to make innovation worthwhile (Schumpeter, 1942). We propose a more granular approach to unpack the relationship (which has attracted much scholarly interest, e.g., Aghion, Bloom, Blundell, Griffith, & Howitt, 2005) that incorporates the idea that how firms perceive the competitive environment matters (Thatchenkery & Katila, 2021).

Our theoretical framework focuses on the *contestability* at the heart of this discussion (Shapiro, 2012), arguing that an opportunity for profitable market-share grab (i.e., contestability) is the lever that spurs innovation: Firms will be more motivated to innovate the more opportunity they perceive to achieve profitable sales. Contestability is key in platform settings, we argue, because the behaviors of platform owners strongly influence perceived opportunities for market share gain across the ecosystem.

New and resource-constrained competitors are particularly likely to be incentivized if they perceive contestability. Innovation often originates when a firm that does not currently have a dominant position perceives that it can profit by differentiating from the dominant players (Baker, 2007; Shapiro, 2012). These low-share firms eventually have a chance to be market disruptors (Aghion, Akcigit, & Howitt, 2013; Schumpeter, 1934). In contrast, as Shapiro (2012) notes, a high-share firm that perceives that it is not able to grow much even if it successfully innovates has lower incentives to invest.

Because complementors in a platform setting innovate on top of the platform's technical infrastructure, dominant platforms strongly influence contestability and resulting innovation incentives. For instance, if dominant platforms and their in-house complementors control a large bulk of the customer base, rival complementors would lack prospects to gain market share (Eisenmann et al., 2011). Once antitrust intervention limits the platform's ability to block competition, however, these incentives would likely change.

Prior literature provides further insight regarding how firms profit from innovation in platform ecosystems. Because platforms must incentivize complementors to join, they often share complementary assets that facilitate product development (Rietveld et al., 2021; Teece, 1986, 2018).[4] Restraining a dominant platform may reduce its motivation or ability to share assets in this way. Thus, profit implications of antitrust may not be straightforward, particularly for resource-constrained complementors.

2.2 | Antitrust

Strategy research on antitrust is particularly relevant background for our research question, which takes a firm-focused lens on policy interventions that aim to promote vigorous competition. Strategy work to-date has studied mostly low-tech industries and anticompetitive practices that block "price competition," including research on M&As where merging firms would wield too much combined market power (e.g., Li & Agarwal, 2017), or collusion where a group of participants use practices that eliminate competition (e.g., Mezias & Boyle, 2005).

Our focus is on another type of anticompetitive practice, in which a single actor—such as a dominant platform—is accused of blocking (i.e., weakening or eliminating) competition. In contrast to traditional antitrust, work in this stream—particularly on platforms—emphasizes technology-based industries and "innovation competition" (Parker, Petropoulos, & Van Alstyne, 2020; Salop, 2021, p. 567). The intent of such antitrust intervention is to unblock competition and level the playing field, thus expanding opportunities for innovation (Pitofsky, 2001). Pricing concerns are less relevant than innovation and consumer choice (Coyle, 2019). Another key difference is the reach of anticompetitive practices: it is ecosystem-wide, as platform dominance can pull innovation incentives down not only in the platform market but also in adjacent complementor markets. The concern, as Shapiro (2012, p. 370) explains it, is that blocking by a dominant platform will "harm by retarding innovation" in the entire ecosystem.

Strategy research in this area has emerged relatively recently. Conceptual research on antitrust remedies against dominant platforms (e.g., Jacobides & Lianos, 2021; Kühn & Van Reenen, 2007) often focuses on rival platform owners. Another stream asks what happens to

[4]Note that in a non-platform market, firms have an incentive to take ownership of complementary assets needed to commercialize an innovation, not share them across the value chain.

complementor entries and exits when platform dominance increases (Wen & Zhu, 2019; Zhu & Liu, 2018). Altogether, prior work shows that platform dominance tends to limit competition and change the ecosystem. Research on the reverse scenario—what happens to ecosystems when dominant platforms can no longer block competition—remains sparse, particularly regarding complementors. It is an open question whether weakening a dominant platform would bring back innovation and level the playing field for profits among complementors. The hypotheses that follow examine this question.

3 | HYPOTHESES

Building on a theoretical framework of competition and innovation, we examine how innovation incentives and profit of complementors change when a dominant platform's ability to block competition is reduced via antitrust intervention.

In H1a, we propose that the prospect of profitable sales motivates complementors to innovate and that the higher the prospect, the more incentivized the complementor will be. Before intervention, complementors often perceive that profitable sales are blocked by the dominant platform's anticompetitive practices that grant it cost and visibility advantages. With those practices in place, any innovation by complementors is at high risk of being sidelined, even if their product is superior (Wang & Shaver, 2016; Wen & Zhu, 2019). Complementors' incentives to innovate are thus muted. Antitrust intervenes by unblocking competition. After the intervention, the more the complementors perceive that the innovation investments are viable again, the more likely they are to invest in R&D.

We argue that the increase in complementors' incentives will be more pronounced in markets where the platform-complementor has high market share. The higher its market share, the higher the perceived contestability of the market after the antitrust intervention.

> **Hypothesis H1a.** *Increases in competitive pressure against a dominant platform through antitrust drive increases in complementor innovation: In particular, the higher the platform-complementor's share in a market, the greater the antitrust intervention's positive impact on complementor innovation in the market.*

While complementors are in general more incentivized to innovate post-intervention, in H1b, we propose that there are likely differences across complementors that also depend on *their own* pre-intervention market share. High-market-share complementors, we propose, have lower incentives because they likely have less room to grow, whereas low-market-share complementors have more room to grow and thus higher incentives. The latter may also believe that, due to their smaller size, they are likelier to go unnoticed and avoid strong pushback from other complementors. High-share complementors are more noticeable to competitors and may even fear being next on the radar of antitrust authorities, decreasing their incentives to compete aggressively (including through innovation). We propose,

> **Hypothesis H1b.** *Increases in competitive pressure against a dominant platform have a more positive impact on a low-market-share (vs. high-market-share) complementor's innovation.*

Even though complementors may perceive a prospect for profitable sales (H1a and H1b), in H2a and H2b, we ask whether they will actually realize the profits that motivated them to invest in the first place. Access to complementary assets (e.g., development assets) likely influences whether complementors can create value from their innovations (Teece, 2018). The answer is complicated by the fact that pre-intervention practices may have indirectly benefited resource-constrained complementors. For instance, if the platform-complementor had significant market share, it was likely seen as a reliable alternative that built visibility for the entire market, including for the rival complementors. It is also likely that pre-antitrust, platform-complementors with high market share were able to prevent "excess" competition by steering complementors toward opportunities not already covered by others, leaving enough market space for each (see Wen & Zhu, 2019). The platform's gatekeeping policies can also have acted as a useful disciplining force, selectively weeding out or promoting offerings based on quality to the benefit of the ecosystem as a whole (Rietveld et al., 2021; Zhang et al., 2022). If the platform-complementor had higher market share it was also likely to have developed more of these specialized assets in the first place (from which it directly benefitted in its own complementor role). Other pre-intervention benefits may have included increasing compatibility and faster and more reliable access to end-users (Gavetti et al., 2017; Wareham, Fox, & Cano Giner, 2014), which platforms may have provided to complementors to improve ecosystem viability (Kapoor, 2018; Helfat & Raubitschek, 2018), thus lowering cost.

In H2a, we propose that the higher the platform-complementor's market share, the more the antitrust intervention pulls down complementors' profit in the market. Complementors likely underestimate the assets the platform has provided while also overestimating their own ability to develop such assets themselves. Once the competitive pressure intensifies, the platform will often be limited in its ability and incentive to provide such benefits. The platform may now shift from developing shared assets to improving the quality of its own complementor products. This effect may be more pronounced in markets where the platform has higher market share and thus more to lose. As a result, rival complementors may need to build many assets from scratch that they were previously provided by the platform. The costly activities of building visibility and increasing sales will also fall on them. All these costs, and the possibility that sales may also stagnate, risk hurting the profitability of complementors post-intervention, and these effects are larger the larger the hole that the platform-complementor leaves in the market.

> **Hypothesis H2a.** *Increases in competitive pressure against a dominant platform through antitrust drive decreases in complementor profit: In particular, the higher the platform-complementor's share in a market, the greater the antitrust intervention's negative impact on complementor profit in the market.*

In H2b, we again propose differences across complementors depending on their pre-intervention market share. We expect high-market share complementors to weather change better because they likely already have many of the complementary assets needed for innovation in-house or are better positioned to develop them quickly and efficiently. Relative to low-market-share complementors, these firms also have high market presence, so their visibility is less likely to suffer as the platform weakens. Low-market-share complementors, in contrast, have more likely relied on platform-provided assets and have yet to develop their own replacement assets. Overall, we propose that the negative profit impact of the antitrust intervention is

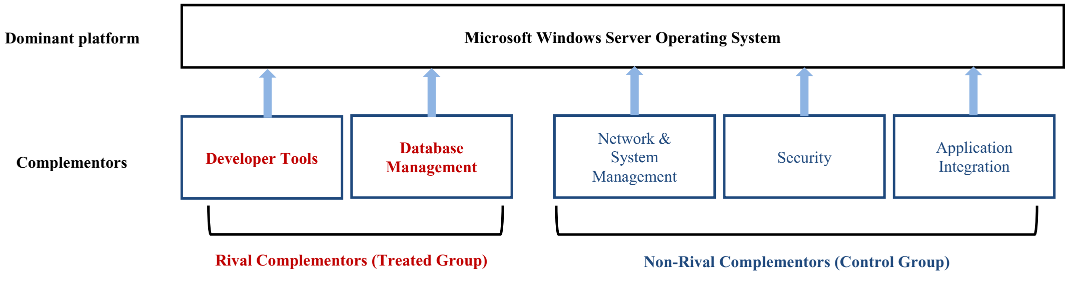

FIGURE 1 Structure of enterprise infrastructure software

likely amplified by the focal complementor's market share: high-market-share complementors are less negatively impacted, and low-share ones more negatively impacted. We propose,

> **Hypothesis H2b.** *Increases in competitive pressure against a dominant platform have a more negative impact on a low-market share (vs. high-market-share) complementor's profit.*

4 | METHODS

4.1 | Empirical context: Enterprise infrastructure software

We tested the hypotheses in the enterprise infrastructure software ecosystem between 1998 and 2004, the 3 years before and after the Microsoft antitrust intervention. The *dominant enterprise platform*[5] was Microsoft's Windows Server Operating System. In this ecosystem, Windows Server provided the hub and the essential technical foundation on which complementors developed infrastructure applications that covered critical core backend IT functions (Thatchenkery & Katila, 2021), as visualized in Figure 1. Microsoft is a dual platform-complementor, offering both the platform and complementor products.

Enterprise infrastructure software is a particularly relevant context for several reasons. Infrastructure was a large and growing industry with total U.S. sales of roughly $20 billion in 2000. While many user-facing markets were saturated, infrastructure markets featured room for growth, and were often described as a "strategically important growth opportunity" (Gartner Research, 2001). This ensured that competition and innovation decisions were important for ecosystem firms and profitable market-share gain was possible. Second, complementor markets were neither too concentrated nor too fragmented (Campbell-Kelly, 2003), providing rich but tractable variation across complementors in each market. Third, infrastructure applications required compatibility with a platform (e.g., Windows Server, Linux, etc.), but complementors

[5]In enterprise software, there are two distinct platforms: the user operating system (which runs user-facing apps) and the enterprise server operating system (which runs infrastructure apps). The user platform was dominated by Microsoft's Windows operating system, which held 93% market share in 2001. The enterprise platform was dominated by Windows Server, with 51% market share. In addition to Windows Server, the other enterprise platforms were Linux (multiple vendors combining for 23%) and UNIX (multiple vendors combining for 11%) (IDC, 2002). Altogether, Microsoft held a dominant position in both user and enterprise platforms.

sold and distributed directly to enterprise customers. As a result, sales and profits were tied more directly to each complementor.[6]

A core strength of our data is its comprehensive coverage of the entire population of public U.S. firms in complementor markets, namely, the five infrastructure applications markets as defined by the standard industry source Gartner Research: *application integration* and *developer tools* that help different applications work together, *database management* that helps store and manage data, *network and system management* used to manage the overall IT system, and *security* that protects from attacks and manages access to the system within the organization. Notably, Microsoft did not have high market share in every complementor market, a fact which creates useful variation for our research design.

4.2 | Competition shock: Microsoft antitrust intervention

Microsoft's alleged blocking of competition as a dominant enterprise platform—and the antitrust remedies detailed below—provide an opportune setting to examine our research question on how changes in competition change innovation and profit. The government's antitrust case focused on the "threat to future innovation" (Shapiro, 2012, p. 400; see also Rubinfeld, 2020) as is typical of antitrust interventions against dominant technology platforms. As the first major antitrust intervention against a U.S. software firm, *U.S. v. Microsoft* was a shock to the ecosystem with participants finding it difficult to *anticipate* its effects. Enterprise software did not receive nearly as much mainstream media attention or scrutiny as user-facing software (i.e., the "browser war" against Netscape), which further made antitrust effects harder to anticipate. As Microsoft's "home" regulator, U.S. authorities also had a wider and stronger array of remedies at their disposal than regulators elsewhere (Fox, 2019), making the case a particularly *strong intervention*.[7]

In the antitrust case, the U.S. Department of Justice alleged that Microsoft repeatedly used its monopoly power in central platforms to block competition in complementor markets. Two ways of blocking were central in enterprise infrastructure: alleged practices that increased *sales* for Microsoft's own complementors at the expense of rivals, and practices that increased *costs* for rival complementors—together influencing complementors' perceived prospects of profitable sales. Microsoft was accused of restricting access to distribution channels (via bundling, and exclusionary hardware ties) and of making it difficult and costly for rivals to compete (via closed interfaces and switching costs),[8] as detailed in the online appendix.

4.3 | Sample construction

We built a dataset of public U.S. firms in enterprise infrastructure software between 1998 and 2004, starting 3 years before and ending 3 years after the Microsoft antitrust *settlement*. We

[6]In contrast, in settings like Apple's App Store or Google Play where the platform controls distribution, profit impact may be harder to assess.

[7]Legal scholars evaluated the consequences of specific clauses within the settlement (e.g., Page & Childers, 2007), but empirically grounded work on broader implications is scarce. Exceptions are Lerner (2001) on allegations that Microsoft blocked innovation *prior* to the intervention and Genakos, Kühn, and Van Reenen (2018) on inter-platform (consumer, enterprise) evidence of Microsoft's incentives to restrict interoperability *across* platforms.

[8]U.S. Department of Justice, *U.S. v. Microsoft*: *Plaintiff's Findings of Fact*, 1999.

selected 2001, the year of the settlement, as the year of intervention because, although there was an initial ruling in 2000,[9] firms facing antitrust action in the United States do *not* have to comply with a ruling while it is being appealed; thus there was no expected change in Microsoft's behavior in response to the 2000 ruling. The fact that this was the first major antitrust case in software added further uncertainty regarding whether the ruling would be upheld. The 2001 settlement, in contrast, was binding on both parties and not subject to appeal or reversals. By agreeing to the settlement, Microsoft publicly agreed to abide by its terms and change its behavior.

We focused on public firms because enterprise customers require long-term support—particularly for the core backend functions managed by infrastructure applications—and are thus reluctant to take a chance on unproven young firms (Eisenmann, Pao, & Barley, 2014). Partly as a consequence, enterprise software complementors tended to go public relatively early compared to firms in other industries, which allowed us to capture the bulk of the industry through public firms (Campbell-Kelly, 2003). As noted below, we also added *private firm data* collected from Corptech and Crunchbase, and report additional details below. Our analyses focused on U.S. firms because the United States was the biggest market for infrastructure applications at the time (Gartner Research, 2001) and because the antitrust intervention was focused on the U.S. market.

Because infrastructure software is not distinguished from other types of software in standard industrial classifications, we took several steps to identify firms, triangulating between multiple sources to improve coverage and create a comprehensive dataset. We started by compiling a list of all public software firms in the United States. Consistent with prior work (Thatchenkery & Katila, 2021), we began with firms classified under the SIC code of "prepackaged software" 7372. Between 1998 and 2004, there were 886 public software firms in the United States. After excluding the 322 firms that developed products only for consumers (to focus on enterprise), we compared the remaining firms' product portfolios with Gartner Research's *IT Glossary*. We classified a firm as an infrastructure software company if most of its product portfolio matched the Gartner keywords for infrastructure software (Thatchenkery, 2017).[10] We triangulated this information with *The Software Catalog* (annual listing of software products) and asked two industry experts to review the list.

Of the resulting 102 enterprise infrastructure software firms, we excluded 18 firms that were not public for *both* the pretreatment and posttreatment periods. Because going public is a milestone event that encourages focus on immediate profit rather than innovation (Bernstein, 2015), limiting the sample to firms that were public before *and* after treatment helps control for alternative mechanisms that might change innovation and profit (results are consistent when including the excluded firms). We also excluded four firms that were platform owners themselves to focus on the implications for purely complementor firms.[11]

It is noteworthy that firm entry was rare (only four public firms entered the industry between 2002 and 2004), and that there was little movement across the five markets, affecting only 1.2% of observations. Our pre-match sample was 78 infrastructure applications firms. Firms exhibited

[9]The ruling was issued in 2000, but Microsoft immediately appealed, casting uncertainty over whether the proposed penalties would actually be imposed.

[10]We primarily relied on the 2001 version of the glossary, but also cross-referenced against categories found in Gartner reports from earlier and later years in our sample time frame and found them to be consistent.

[11]Results are robust to randomly assigning the dual platform-complementor firms to be either platform (excluded from the sample) or complementor (included in the sample).

typical regional patterns for software, with 32% headquartered in the San Francisco Bay, 10% in the Los Angeles area, and 9% in the Boston area.

4.4 | Data sources

Firms were classified into complementor markets using firm press releases (Thatchenkery, 2017) and categories from Gartner Research and Gartner Research's IT Glossary (Pontikes, 2012). We consulted Gartner Research for market share data. Patent data were collected using the NBER database (Hall, Jaffe, & Trajtenberg, 2001) and we added citations from the USPTO. We collected firm data including R&D expenditures, firm size, IPO year, location, and profits from Compustat, supplemented by SEC filings and CapitalIQ.

For sensitivity analyses, product data were collected through press releases (Thatchenkery & Katila, 2021) in Factiva and LexisNexis and private firm data through CorpTech and Crunchbase.[12] Press coverage and court documents, retrieved from Factiva, LexisNexis, Westlaw, and U.S. government archives, provided qualitative data on the antitrust case.

4.5 | Research design

Using a difference-in-differences design, we exploited variation in Microsoft's market share across complementor markets to separate the treated from the control group. While Microsoft is a dual platform-complementor in all five complementor markets, its market share varied. As the "treated group," we used firms in markets where Microsoft had a strong market presence as defined by Gartner (detailed below), where the antitrust intervention could foster a significant market share transfer to rival complementors (Shapiro, 2012). In contrast, the "control group" comprised firms in markets where Microsoft had only a marginal market presence. While we used a binary treatment for our main analysis, we also ran a "continuous treatment" (variable with differing treatment intensity) design for robustness (Angrist & Pischke, 2009, p. 234), with consistent results (Table A1).

At the time of the case, all five infrastructure applications markets had two to four of what Gartner called "leader" firms with at least 10% market share each (Gartner Research, 2001). Microsoft was one such firm in two markets, with 16% market share in database management (#3 vendor) and 15% market share in developer tools (#2 vendor). Firms in these two markets—where platform-complementor had a high market share—are the *treated group* (labeled as *rival complementors*). In contrast, Microsoft held a substantially weaker or even insignificant position in application integration (0.3% market share), network and system management (1.6% market share), and security (<1% market share). Firms in those markets are the *control group* (labeled as *non-rival complementors*).

[12]CorpTech directory—published from 1986 until 2004—provides detailed, yearly lists of private firms in our market segments pre and post intervention. We triangulate CorpTech data with Crunchbase. Crunchbase provides recent data and lists firm founding dates, allowing us to track new entries in detail, but it is limited for a few reasons: (a) It was founded in 2007 and so data on firms in our time period is retrospective (in contrast, CorpTech is a time capsule snapshot); (b) Crunchbase is a single snapshot of each firm's activities, so we cannot be sure that the markets listed for each firm are the same markets the firm was in during our study period; (c) because it is crowdsourced, Crunchbase's accuracy—particularly for inactive firms—may be limited.

4.6 | Measures

4.6.1 | Dependent variables

We measured *complementor innovation* by yearly counts of patents each firm had applied for (and later received). Patents are a particularly appropriate measure of innovation because each patent is novel, nonobvious and useful, describing both a problem and its solution (Katila, 2002; Ziedonis, 2008). Each patent thus identifies new problem-solving efforts that are added into the industry to capture market share. Patenting is also standard in software during our study period from the mid-1990s to mid-2010s (Cockburn & MacGarvie, 2011). The USPTO released official guidelines for software patenting in 1996, and software patents' validity was confirmed by the 1998 U.S. Court of Appeals *State Street* decision. The U.S. Supreme Court's *Alice Corp* decision in 2014 reversed this trend, introducing stricter criteria. Patents were thus a highly relevant measure of software innovation during our period. The fact that innovation in infrastructure software centered on performance and functionality—rather than the more ambiguous features (aesthetics, fads) often privileged in user-facing apps—also made tracking innovation by patents feasible. We also used yearly *R&D expenditures* and *citation-weighted patents* as alternative dependent variables, with consistent results (Table A2). We collected 10 years of citation data from the USPTO.

Consistent with prior work on competition in the software industry, we measured *complementor profit* as return on sales annually for each firm (Suarez, Cusumano, & Kahl, 2013; Young, Smith, & Grimm, 1996). ROS is more appropriate than alternatives such as return on assets because software firms rely primarily on intangible assets (e.g., human capital) and because software firms tend to reinvest their profits to continuously grow the business. We also measured profit as net income (logged) and superior profits (ROS at least double the industry average), with consistent results (Table A2), and split profit into value creation and asset utilization components in mechanism tests.

4.6.2 | Independent variables

Because we ran difference-in-differences, the explanatory variable of interest is an interaction between treated group and posttreatment period. We measured *rival complementor* (treated group) as a binary indicator set to 1 if the firm competed in a complementor market in which Microsoft's in-house complementor had high market share (developer tools, database management) and 0 otherwise.[13] We measured *post-intervention* (posttreatment period) as a binary indicator set to 1 if the year was 2002–2004. We measured *low-market-share complementor* as a binary indicator set to 1 if the firm had less than 10% market share prior to the intervention and 0 otherwise.

4.6.3 | Control variables

Because scale matters in software innovation (Campbell-Kelly, 2003), with product visibility and bargaining power accruing to firms with higher market footprint, we controlled for *size* by

[13]Multimarket competition is not common in infrastructure software. If the firm operated in both treated and control markets (only a handful of firms), we assigned it to (a) treated only because the firm faced strong competition with Microsoft complementors at least in some markets, (b) in robustness tests randomly assigned the firm to either treated or control, and (c) dropped these firms altogether. Results were robust to alternatives.

annual revenue in millions of U.S. dollars, adjusted for inflation and logged to correct for skew. We also measured size as *number of employees*, with consistent results (Table A3). Because greater investment in R&D is likely to influence patents and possibly profit, we controlled for *R&D intensity*, measured by dividing R&D expenditure by total revenue annually. Results using *R&D expenditures* adjusted for inflation and logged to correct for skew were consistent (Table A3).

Because firms that have been public for longer may face increased pressure to prioritize short-term financial returns at the expense of innovation (Bernstein, 2015), and may also be more sensitive to the potential exercise of market power (Zhang & Gimeno, 2010), we controlled for *years public* as the logged number of years since the firm's IPO. We controlled for *acquisitions*, measured as a yearly count, because acquisitions by the focal firm may give the firm more "raw material" for innovation (Ahuja & Katila, 2001) but also increase costs related to integration (Kim & Finkelstein, 2009).

We controlled for any unobserved *market effects* with fixed effects for each of the five complementor markets. We controlled for three *geographical regions* with high numbers of enterprise software firms—San Francisco, Boston, and Los Angeles—because rivalry and knowledge spillovers within a region can impact innovation and profits (Owen-Smith & Powell, 2004). Region effects dropped out from fixed effects regressions but were included in random effects regressions. Macroeconomic variation was controlled with *year effects*.

4.6.4 | Matching variables

To improve comparability of treated and control firms, as detailed below, we used propensity score matching. We matched on *geographic region*,[14] *size*, *IPO year*, *R&D intensity*, and *pre-sample patents* (when predicting innovation) or *pre-sample ROS* (when predicting profit).[15] Except for pre-sample variables, which were averaged from 1995 to 1997, we used 3-year averages of pretreatment values (i.e., 1998–2000) to ensure that the matching was not influenced by the treatment itself (Flammer, 2015). The matched sample was 374 firm-years (66 firms).

4.7 | Statistical method: Difference-in-differences

Ideally, we would randomly assign complementors to markets where a platform "blocked" competition versus not, observing differences in innovation and profits after the block is lifted. Using observational data, however, we need to account for selection bias that could arise from unobserved factors associated with complementor firms competing in specific markets and with firm performance. Complementors may have, for example, avoided markets where Microsoft was a substantial threat because they did not have the resources to compete. Complementors may also have selected into Microsoft's strongholds because they were following the lead of the platform firm. Our results would thus be driven by differences in firm characteristics, not

[14]We matched for whether the firm was headquartered in regions with high numbers of enterprise software firms, using three separate dummy variables for San Francisco, Boston, and Los Angeles. Results are robust to exact hand matching of treated firms to control firms located in the same or adjacent state (Table A4).

[15]Pre-sample patents and pre-sample profit were highly correlated with firm size and R&D intensity, so we used them as alternative matching criteria in a robustness check, with consistent results.

competition. We address this possible bias with difference-in-differences, as well as matching, fixed effects, and firm heterogeneity controls.

We use a *difference-in-differences* research design (Abadie, 2005), which allows us to correct for selection bias when the treated and control groups are not a perfect match in the outcome variables but the unobserved differences between the two groups remain the same over time (parallel trends assumption). We first calculate the differences in outcomes for treated firms and the untreated comparison group of control firms before versus after treatment and then the difference in those two numbers. Formally,

$$\mathrm{DV}_{it} = \begin{array}{l}\beta_0 + \beta_1 \text{Rival Complementor}_i + \beta_2 \text{Post-Intervention}_t + \beta_3 \text{Post-Intervention}_t \\ \times \text{Rival Complementor}_i + \gamma C_{it} + \epsilon_{it}\end{array}$$

where C_{it} represents the control variables and ϵ_{it} is the error term. β_3 is the coefficient on the difference-in-differences estimator, which captures the effect of the antitrust intervention on the treated group of complementors (see Bertrand, Duflo, & Mullainathan, 2004).

We ran a difference-in-differences analysis using panel regressions with firm fixed effects and standard errors clustered at the firm level.[16] Fixed effects models help control for any baseline (i.e., time-invariant) heterogeneity between firms, and were preferred over random effects by a Hausman test for both dependent variables (Hausman, 1978).

For the models predicting innovation, a count variable, we used *fixed effects Poisson regressions*. A Poisson estimator is preferred to a log-OLS estimator, for two reasons. First, applying a log-OLS specification to count data can produce biased estimates, especially in the presence of heteroscedasticity (O'Hara & Kotze, 2010). Second, Poisson outperforms log-OLS when the data contain a substantial number of zeroes (Burtch, Carnahan, & Greenwood, 2018; Silva & Tenreyro, 2011), as is the case with a number of non-patenting firms in our sample.

For the models predicting profit, we used a *fixed effects panel OLS regression*. To avoid collinearity with firm and year fixed effects (Friebel, Heinz, Krueger, & Zubanov, 2017), we included only the *Post-Intervention x Rival Complementor* interaction in fixed effects regressions. Consistent with prior work, our estimation period is 3 years before and after the 2001 intervention (Flammer, 2015). Results are robust to 5 years before and after, and to other temporal dynamics including short- versus long-term effects. Robustness checks are summarized in Table 4.

We drew from Angrist and Pischke (2009) and Cunningham (2021), as well as recent work by Roth (2022) to validate the difference-in-differences design. Specifically, we verified treatment relevance and validity (parallel trends), described below.

4.8 | Validation of the research design: Relevance of treatment

Treatment in a difference-in-differences design must be *relevant*, that is, trigger actual changes in the treated group. If the treatment is not relevant, then any observed difference between the

[16]Results are consistent when we cluster at the market level in a single-market complementor sample (Cameron & Miller, 2015, see Table A3).

treated and control group would be a statistical artifact and not a genuine response to the intervention.

In our study, a relevant antitrust intervention should constrain the platform's behavior and change treated firms' perceptions about sales prospects. Notably, not all antitrust interventions achieve this outcome, as in the case of a 1956 intervention affecting telecom monopolist AT&T (Bell Labs) (Watzinger, Fackler, Nagler, & Schnitzer, 2020).

To examine the relevance of the Microsoft case, we first obtained evidence regarding changes in platform behavior. Anecdotal evidence suggests that there indeed was a noticeable change in Microsoft's behavior in treated markets, for instance, that it "made Microsoft's leaders skittish about bundling" (Rivkin & Van Den Steen, 2009). Data on complementor markets suggest a similar conclusion: Microsoft's growth slowed particularly in treated markets (average post-intervention growth rate of 5.4% in treated vs. 19.3% in control markets), indicating that Microsoft was newly constrained in exploiting its market power against the treated group (Gartner Research, 2005). In server operating systems (the platform market), Microsoft's growth also stalled after the intervention, and Linux became an increasingly popular alternative, suggesting the intervention may have helped preclude a winner-take-all outcome (Schilling, 2002). Yearly market research estimates put Microsoft at 45–60% market share in the enterprise server platform post-intervention, a sharp contrast to its 85–95% share in the user platform (IDC, 2019). Bill Gates, Microsoft's CEO at the time, repeatedly stated that "there's no doubt the antitrust lawsuit was bad for Microsoft" (Novet, 2019).

A related point is whether complementors paid attention to changes in the platform's behavior. To obtain systematic evidence on *perceptions* of competition (Thatchenkery & Katila, 2021), we studied the lists of competitors in complementors' 10-Ks. In the 3 years leading up to the intervention, 91% of the treated firms listed Microsoft as a competitive threat while only 66% of the controls did so. In the 3 years after the intervention, the proportion of treated firms listing Microsoft dropped to 66%, compared to 60% among the control firms. The way treated firms described Microsoft also changed. In its 1998 10-K, treated firm Borland Software (developer tools) identified Microsoft as a threat because, in addition to directly competing in developer tools, it also made "operating environments." In 2003, however, Microsoft is simply noted as a direct competitor, with no mention of its platform ownership. Thus, there is significant evidence that (a) Microsoft was perceived as more of a competitive threat by treated firms compared to control firms and (b) that perceptions substantially changed following the antitrust intervention.

To corroborate the logic that low-share complementors drive the results (H1b, H2b), we examined how perceptions of competitive pressure by low versus high market-share firms differed. As expected, the number of treated *low-market-share* complementors listing Microsoft as a competitor decreased from 94% prior to the intervention to 61% after. In contrast, this number among treated *high-market-share* complementors did not change after the intervention (remaining at 80%). Thus, while high-share complementors did not change their perceptions of the competitive threat posed by Microsoft, low-share firms did, consistent with our theoretical predictions.

As a final check on relevance, following Mahmood, Zhu, and Zaheer (2017) and Burtch et al. (2018), we tested *random implementation* of treatment, in which we randomized assignment to treatment for each firm in our sample. There is no effect of random implementation (Table A1), providing further confidence in our results.

4.9 | Validation of the research design: Parallel trends

A key assumption of the difference-in-differences design is that the treated and control groups would have experienced *parallel trends* absent the treatment (Abadie, 2005). This assumption holds when the (a) treatment is exogenous, (b) dynamics of the treated versus control group are comparable pretreatment (i.e., the control group "approximates the traveling path of the treated group"), and (c) differences between the control and the counterfactual treated group remain stable post treatment (Cunningham, 2021, p. 422).

4.9.1 | Exogeneity of treatment

The intuition behind the exogeneity assumption is that if treatment is endogenous, the treated group would have diverged in the absence of (independent of) treatment, violating the parallel trends assumption (Cunningham, 2021). In our case, if another variable influences both a firm's presence in treated markets and its innovation (or profit), then the observed difference between the treated and control groups could be driven by something other than the treatment. While we cannot conclusively prove that treatment is exogenous, we can rule out sources of endogeneity.

We first examined whether some markets were given special attention by Microsoft (and in turn more likely to be treated) based on expected gains. If this were the case, the treated versus control groups would respond differently not because of treatment but because of selection bias (Callaway, Goodman-Bacon, & Sant'Anna, 2021). For example, if Microsoft invested more in treated markets because these markets were seen as technically more promising, that could conflate treatment with outcomes. However, on average, pre-intervention patenting was in fact moderately higher in control markets (where complementor firms averaged 4.9 patents/year) compared to treated markets (3.4 patents/year), which suggests that control markets were possibly seen as more promising. The *matching* algorithm also ensures that the control group is very similar to the treated group in patents and R&D intensity (Table A5), which alleviates the concern. Second, if the treated markets were in general just better markets (independent of innovation), we would *not* expect results to differ for (and originate from) low versus high market share firms (H1b, H2b).

We also ruled out influences of changes in the competitive environment of each market (i.e., market concentration) that may have coincided with the intervention and created endogeneity. Although our extensive search of case evidence and industry material did not suggest this, highly concentrated market segments could have been targeted by regulators, or innovation incentives of complementors could have changed with shifts in market concentration. This seems unlikely for several reasons, however. First, market concentration did not systematically differ nor change differently in treated versus control markets.[17] Second, models where we control for market concentration (HHI) and the number of competitors in the firm's markets are consistent with our original findings (Table A3).

[17]At the time of the intervention, two markets (one treated, one control) met the typical FTC benchmark for "high concentration" (HHI > 2,500); the other three markets were mildly concentrated (HHIs of 950–1,200).

4.9.2 | Comparable dynamics in the pretreatment period

Unlike RCT studies (in which covariate balancing is often visualized), the difference-in-differences method does not require the assumption that the treated and control groups are similar in covariates. Rather, what matters for validity is that the covariate *dynamics* of the two groups are comparable in the pretreatment period (parallel trends), and that how the groups differ from each other (except for the treatment) remains stable over time. While it is not possible to prove conclusively that parallel trends exist pretreatment, we follow the best practice of evaluating whether the parallel trends assumption is likely to hold for our study. We provide evidence through visualizing raw data, through an event study, and an "honest" pre-trends test.

We first simply "*show the raw data*" (Cunningham, 2021, p. 426) as a precursor to more formal analysis of parallel trends (Wing, Simon, & Bello-Gomez, 2018). We plotted yearly means of treated versus control firms' innovation in Figure 2 and profits in Figure 3. Both provide supportive evidence of parallel trends. Prior to treatment, the treated and control group trends were similar (i.e., roughly parallel). Posttreatment, they diverge.

We then probed the parallel trends assumption with an *event study* (Cunningham, 2021, p. 452; Binder, 1998). Event studies track whether the treated versus control groups' dynamics are comparable in the pretreatment period by including anticipatory effects (leads) and posttreatment effects (lags) in a regression (see Table A1 Models 4 and 8), visualized in Figures 4

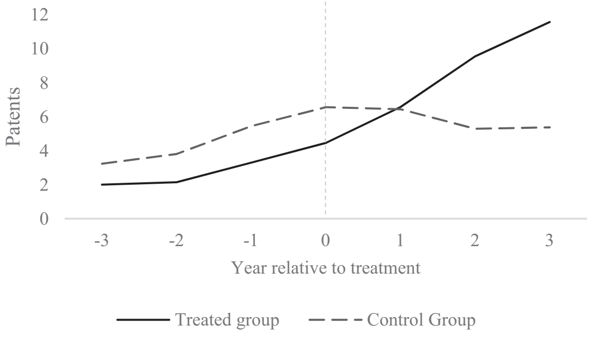

FIGURE 2 Visualization of trends using count measure of complementor innovation (yearly means)

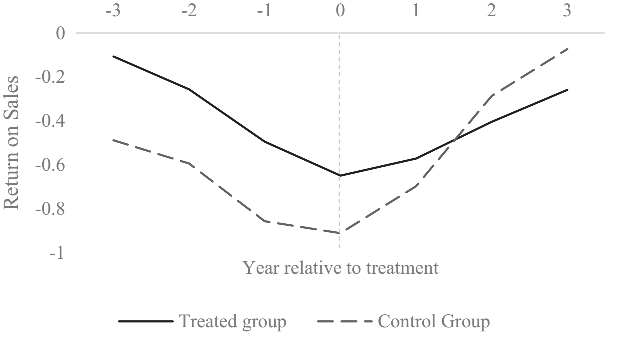

FIGURE 3 Visualization of trends using ROS measure of complementor profit (yearly means)

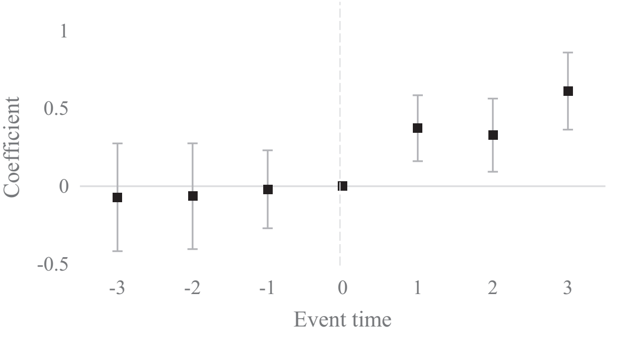

FIGURE 4 Event study: Estimates of antitrust intervention's effects on complementor innovation using leads and lags (90% CI)

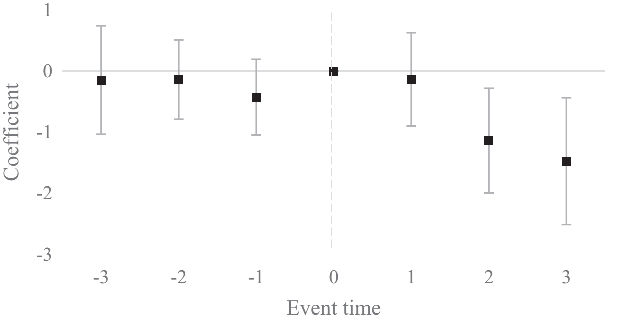

FIGURE 5 Event study: Estimates of antitrust intervention's effects on complementor profit using leads and lags (90% CI)

and 5. As expected, placebo pretreatment leads are not statistically different from zero prior to treatment, suggesting that the treated and control groups were on parallel trends prior to treatment, only differing from zero after the intervention. A test of whether the pretreatment coefficients jointly differ from zero (dqd in Stata; Mora & Reggio, 2015) provides further support: the *p*-value for innovation is .52 and for profit is .81, indicating that violation of parallel trends was not detected. Taken together, the event study analyses increase our confidence in the parallel trends assumption.

Third, we implemented the *"honest" pre-trends* test by Roth (2022), which probes whether the event study may lack statistical power to detect a violation of the parallel trends assumption. To implement the test, we impute event study estimation results (coefficients and variance–covariance matrix) into the *pretrends* package in R (Roth, 2022) and calculate the likelihood of observing the event study coefficients if parallel trends were violated vs. not. The resulting small likelihood ratios (0.088 for innovation and 0.065 for profit) indicate that lack of statistical power is not likely to be an issue, providing further confidence that the parallel trends assumption likely holds for our data.

4.9.3 | Stable groups

Another significant assumption of the difference-in-differences design is that the parallel trends assumption holds post treatment for the counterfactual treated group and for the control group. We first confirmed that there was no significant migration between treated and control groups (Abadie, 2005). At the end of our period (in 2003 and 2004, respectively), two firms that had previously been in control markets entered a treated market. We included and excluded these firms with no change in results. As noted below, entry and exit of public firms was also remarkably nonexistent, further validating the research design.

It is also possible to apply *matching* (as described above), which allows us to restrict to a set of observations in the control group for which the parallel trends assumption is more likely to hold (Wing et al., 2018). The intuition is that this assumption "may hold in a restricted sample… even if it does not hold across all groups and times" (Wing et al., 2018, p. 458). Consistency of results with and without matching, as described below, thus provides further confidence in our study design.

Finally, *anticipation* effects, or *repeat* or *reversed treatments*, could contaminate treatment and the stable groups assumption, but neither seem present during our study period. If firms anticipate the intervention then they may start to change their behavior beforehand, which would make our results spurious (Wing et al., 2018). However, the intervention was unprecedented within the software industry (Rubinfeld, 2020), making its effects difficult to anticipate. Event study analyses with nonsignificant lead year coefficients (Table A1) provide systematic confirmation. We also found that no other antitrust interventions took place in the market during the study period, so repeat treatments are an unlikely concern. Finally, settlement is particularly attractive as a treatment because it was binding for both parties and rules out reversals through appeal.

5 | RESULTS

Descriptive statistics are reported in Table 1. Complementor firms in infrastructure software produced roughly five to six patents per year on average. Average return on sales during the study time period (1998–2004) is −0.27, though the standard deviation is quite large (2.19).[18] Correlations among independent variables are mostly low, and variance inflation factors (VIFs) are under the recommended cutoff of 5 (years public is the only exception with VIF of 9.24). Because VIFs are a sufficient but not a necessary indicator for multicollinearity, we also randomly estimated subsets of the study sample by dropping 1 year at a time (Echambadi, Campbell, & Agarwal, 2006), as well as by both including and excluding *years public* and *size* variables. Results were consistent (Table A3), indicating that coefficients are stable.

5.1 | Regression analysis: Main results

5.1.1 | Hypothesis testing

Tables 2 and 3 report the difference-in-differences results for innovation and profit, respectively. The tables first report results using the full, unmatched sample (Models 1–2) and then add

[18]The unexpectedly low profitability of enterprise software is consistent with observations of the volatility of the software industry in general during the sample time frame.

TABLE 1 Descriptive statistics and correlations

	Variable	Mean	*SD*	1	2	3	4	5	6	7
1	Complementor innovation	5.43	19.27							
2	Complementor profit	−0.27	2.19	0.04						
3	Rival complementor	0.56	0.50	−0.11	−0.03					
4	Post-intervention	0.43	0.50	0.09	0.001	0.01				
5	Size (logged)	0.56	0.66	0.56	0.07	0.06	0.14			
6	R&D intensity	0.38	0.99	−0.05	0.25	−0.03	−0.08	−0.17		
7	Years public	6.44	4.15	0.25	0.002	0.02	0.30	0.41	−0.11	
8	Acquisitions	0.06	0.24	0.14	0.02	0.004	0.02	0.31	−0.04	0.20

Note: 374 firm-years. Correlations above .10 are significant at $p < .05$.

TABLE 2 Difference-in-differences: Fixed effects Poisson predicting complementor innovation (patents)

	All complementors				Split samples	
	Unmatched sample		Matched sample		Low market share	High market share
	1	2	3	4	5	6
Rival complementor × post-intervention		0.51 (.000)		0.53 (.000)	0.45 (.01)	−0.02 (.90)
Controls						
Size (logged)	1.21 (.000)	1.33 (.000)	2.60 (.000)	2.63 (.000)	−0.21 (.25)	4.37 (.000)
R&D intensity	0.17 (.70)	−0.30 (.50)	3.47 (.000)	3.15 (.000)	−0.30 (.50)	10.34 (.000)
Years public	−0.80 (.000)	−0.96 (.000)	−0.44 (.003)	−0.70 (.000)	−0.86 (.000)	−0.42 (.41)
Acquisitions	−0.48 (.000)	−0.43 (.000)	−0.59 (.000)	−0.57 (.000)	−0.72 (.000)	−0.57 (.000)
Firm effects	Yes	Yes	Yes	Yes	Yes	Yes
Market effects	Yes	Yes	Yes	Yes	Yes	Yes
Year effects	Yes	Yes	Yes	Yes	Yes	Yes
Log likelihood	−612.80	−599.20	−797.40	−784.90	−529.78	−303.94

Note: 374 firm-years. *p*-values in parentheses (two-tailed tests). Split sample analyses are reported on the matched sample.

matching (Models 3–4). Unmatched results are highly consistent with those using the matched sample, providing further confidence in our research design.[19]

[19]As Angrist and Pischke (2009) note, because matching and control variables are alternatives in a regression setting, we expect regressions with and without matching to produce highly consistent results; however, the exact magnitude of the estimated treatment effect may differ. Our results are consistent with the expectation.

TABLE 3 Difference-in-differences: Fixed effects panel OLS predicting complementor profit (return on sales)

	All complementors				Split samples	
	Unmatched sample		Matched sample		Low market share	High market share
	1	2	3	4	5	6
Rival complementor × post-intervention		−0.44 (.01)		−0.58 (.01)	−0.39 (.08)	−0.56 (.35)
Controls						
Size (logged)	−0.06 (.74)	−0.15 (.42)	−0.04 (.87)	−0.16 (.58)	0.12 (.73)	−0.44 (.58)
R&D intensity	−0.03 (.003)	−0.02 (.01)	−0.02 (.000)	−0.02 (.000)	−0.02 (.000)	0.69 (.67)
Years public	0.19 (.28)	0.21 (.23)	0.23 (.33)	0.29 (.24)	0.25 (.27)	−0.64 (.26)
Acquisitions	0.13 (.45)	0.11 (.52)	0.39 (.22)	0.38 (.18)	0.24 (.33)	0.41 (.26)
Firm effects	Yes	Yes	Yes	Yes	Yes	Yes
Market effects	Yes	Yes	Yes	Yes	Yes	Yes
Year effects	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	.19	.21	.27	.30	.26	.19

Note: 374 firm-years. *p*-values in parentheses (two-tailed tests). Split sample analyses are reported on the matched sample.

Hypothesis H1a predicted that the treated group of rival complementors (i.e., complementors that operated in Microsoft's pre-intervention strongholds) would be more strongly impacted by the intervention and thus innovate more than the control group of non-rival complementors. The difference-in-differences estimator is positive ($\beta = .53$, $p = .000$) in Table 3. The treated firms introduced an average of 4.2 *more* patents yearly relative to controls, supporting H1a.

Hypothesis H2a predicted that the profit of treated rival complementors would sink after the intervention. The difference-in-differences estimator is negative ($\beta = -.58$, $p = .01$). Treated complementors experienced an average drop of 9.1 percentage points in ROS compared to control firms.

In H1b and H2b, we argued that results would be particularly driven by low-share rivals that were most aggressively in pursuit of new sales that were previously blocked. To contrast low- versus high-share firms, we followed best practice from Kapoor and Furr (2015) to compare split samples (Kapoor & Furr, 2015, p. 429; Lee, Hoetker, & Qualls, 2015), distinguishing low-market-share (<10%) complementors from the high-share ones (Tables 2 and 3, Models 5–6). For innovation, the difference-in-differences estimator is positive for the low-market-share sample ($\beta = .45$, $p = .01$) but indistinguishable from zero for high-market-share firms ($\beta = -.02$ $p = .90$), supporting H1b. We observe a similar, albeit less dramatic, pattern for profit (H2b) ($\beta = -.39$, $p = .08$ for low-share; $\beta = -.56$, $p = .35$ for high-share).

To test the moderating hypotheses more directly, we tested for differences across the split sample regressions. Ideally, we would run a Wald test, but the prerequisite of equal unobserved heterogeneity (Lee et al., 2015) was not fulfilled. We thus followed standard practice and ran a three-way interaction (Table A6) and event study, similar to a triple-differences design (figure A2; see Wing et al., 2018).[20] These additional tests provide confirmation for innovation but do not display as clear a pattern for profit. Overall, we find strong support for H1a and H1b (antitrust pushes up innovation incentives, particularly of low-share firms) and for H2a (antitrust pulls down profit). We find tentative but not as strong support for H2b's claim that low-share firm profit was particularly affected.

5.1.2 | Omitted variable bias

To investigate the sensitivity of our results to omitted variable bias, we followed Frank (2000) and calculated how much bias needs to be present to invalidate our results, in two ways. First, we calculated the impact threshold of a confounding variable (ITCV), which tells us how strongly correlated a hypothetical omitted confounding variable would need to be to invalidate our results (Frank, 2000). ITCV is 0.04 for innovation and −0.11 for profit. In other words, to overturn the results, partial correlations between the difference-in-differences estimator, the dependent variable, and the omitted confounding variable would have to be above 0.20 ($=\sqrt{|0.04|}$) for innovation and above 0.33 ($=\sqrt{|-0.11|}$) for profit. Using current control variables as a yardstick (Busenbark, Yoon, Gamache, & Withers, 2022; Larcker & Rusticus, 2010), any hypothetical omitted variable would thus need to have a larger impact than any of our (highly influential, "standard") controls to overturn the results. It seems unlikely that an omitted variable would cross these thresholds.

Second, we calculated robustness of inference to replacement (RIR) (Frank, 2000) defined as the percentage of observations for which the observed treatment effect would need to be driven entirely by an omitted variable, not by the treatment, to invalidate the findings. Interpretation of the RIRs is "grounded in logical intuition, such that scholars typically determine whether the number of requisite overturned treatment cases appears reasonable" (Busenbark et al., 2022, p. 44). In our data, 23% of the treatment cases for innovation and 35% of cases for profit would need to be entirely overturned. These numbers are much higher than thresholds accepted in prior work (e.g., Busenbark, Lange, & Certo, 2017). Overall, our ITCV and RIR analyses indicate that it is unlikely that an omitted variable is driving the results.

We ran a battery of additional tests for statistical robustness and to rule out alternative explanations. Tests are summarized in Table 4 and reported in detail in the online appendix.

5.2 | Regression analysis: Mechanism tests

We ran several tests to probe the proposed theoretical mechanisms underlying our hypothesized effects on innovation and profits to further rule out that our results could be spurious.

[20]We plot event study charts following the template of Dranove, Garthwaite, and Hermosilla (2021) in figure A2. The pattern for low-share firms mirrors that seen in the full sample: coefficients are statistically insignificant prior to treatment and only diverge from 0 after treatment, as expected. High-share firms do not have this clear pattern, further providing confidence that the low-share firms are the ones driving the results.

TABLE 4 Summary of robustness checks

Concern	Test	Location
Statistical robustness		
Results are confounded by violation of parallel trends	Examine raw data and event studies, honest pre-trends	Figures 2–5, Table A1
Results are sensitive to omitted variable bias	Run ITCV and RIR analyses	Results: Omitted variable bias 5.1.2
Results are sensitive to length of pretreatment or posttreatment windows	Run results on alternate pretreatment and posttreatment windows	Table 5
Results are sensitive to serial correlation	Collapse pretreatment vs. posttreatment, random implementation	Table A1
Results are sensitive to market share cutoff for treated versus control	Run continuous treatment variable	Table A1
Results are sensitive to non-patenting firms	Run zero-inflated Poisson and log-OLS	Table A2
Results are sensitive to operationalization of DV	Run alternate dependent variables	Table A2
Results are sensitive to choice of controls	Run alternate controls	Table A3
Results are sensitive to matching approaches	Run results without matching and with alternative matching schemes	Tables 2-3 and A4–A5
Alternative explanations		
Results are driven by dot-com crash in 2000	Run event study, examine diff-in-diff figures	Figures 2 and 3, Table A1
Results do not capture changes in variability in the DVs	Run results predicting extreme outcomes	Table A2
Results are driven by multimarket contact	Run analyses only on single market firms	Table A3
Results are driven by statistical artifacts	Run mechanism tests for innovation and profit	Tables A7–A9
Results are driven by "baseline" rate of innovation	Control for pre-sample patenting	Table A10

5.2.1 | Innovation: Variety increases

In our theoretical framework, we argued that treated (as opposed to control) complementors were more motivated to innovate because they saw opportunities to pursue profitable sales increases in the recently opened space. We probed this underlying mechanism in several ways. First, we parsed shifts in patenting within the very limited number of complementors operating in both treated and control markets. Prior to the intervention (1998–2000), among firms that were simultaneously in treated and control markets, about 45% of patents were in treated and 55% were in control markets. After the intervention (2002–2004), for the same firms, 68% of patents were in treated and 32% of patents were in control markets, pointing to a shift in innovative effort toward the treated markets, with more opportunity, as expected.

Next, we examined *number of hits and flops*, defined as patents with citations in the top (or bottom) quartile of the industry (Ahuja & Lampert, 2001). If our arguments held, we would expect treated firms' variability in patents—mixture of hits and flops—to increase after antitrust as they engaged in a variety of search approaches to pursue market share. This is indeed what we saw (Table A7), driven by low-market-share complementors. Results using superior (measured as patent output above the 95th percentile) and citation-weighted patents were consistent (Table A2).

Third, we examined whether antitrust intervention merely prompted treated firms to quickly file relatively low-quality patent applications, given reduced *IP threat* from the platform firm. However, results were consistent to using both citation-weighted and unweighted patent counts (Table A2), supporting that patents truly reflected innovation.

5.2.2 | Profit: Complementary assets

We theorized that changes in profit were related to treated firms' (in)ability to capture value through *complementary assets* (e.g., development assets), rather than changes in internal efficiency. We collected both quantitative and qualitative data in support.

We first gathered anecdotal evidence from the antitrust case documents and contemporary news articles. If changes in access to complementary assets explained the profit declines, as we have argued, we would expect Microsoft's inclination to provide these assets to have dropped following the intervention. Qualitative evidence provides support. While Microsoft continued to provide essential assets such as software development kits and was forced by the intervention to open its APIs, there is evidence that it was less inclined to go "above and beyond" following the intervention. The most prominent example was Microsoft's introduction of a proprietary implementation of Java in 1997. Microsoft Java provided several benefits to complementors and won awards for reliability and customer support (Neffenger, 1998), with PC Magazine calling it "the fastest and most reliable Java implementation available" (PC Magazine, 1998). But the antitrust investigation suggested that the implementation had involved a conscious effort to "kill cross-platform Java."[21] In the wake of the investigation, Microsoft decided to phase out its proprietary version of Java, despite the benefits it provided complementors and their disappointment at its loss. Gartner Research noted: "For those who had any remaining hope that Microsoft would reverse strategy and support Java, this announcement reinforces Gartner's prediction that Microsoft would completely abandon support" (Gartner Research, 2002). Microsoft did not make a second attempt to introduce its own Java implementation until mid-2021. Thus, while Microsoft continued to offer essential complementary assets, it was more cautious and reluctant following the antitrust intervention.

To corroborate the mechanism that treated firms lost access to complementary assets after antitrust, we followed Grullon, Larkin, and Michaely (2019) and decomposed profit into external versus internal components. The firm's external ability to *capture value* was measured by price–cost margin (operating profits net of depreciation, divided by sales) and the firm's *internal efficiency* by asset utilization (total

[21]U.S. Department of Justice, *U.S. v. Microsoft*: *Plaintiff's Findings of Fact*, 1999.

sales divided by total assets). If complementary assets were the mechanism, we should see a drop in external value capture post-intervention. This is indeed what we saw (Table A8b).[22]

To further probe the role of complementary assets, we examined changes in *commercialization* of innovation, measured by product introductions (Katila, Thatchenkery, Christensen, & Zenios, 2017; Thatchenkery & Katila, 2021). Through an additional, painstaking effort, we collected data on 3,684 new products by complementors. Positive results on patents but not commercial (product) innovation (Table A8a) again pointed to external value capture, rather than internal efficiency, as a likely explanation and indicated that the treated group did not proceed to profit from their newly patented ideas, most likely because they lacked complementary assets to commercialize. Split sample regressions (Kapoor & Furr, 2015) also revealed no difference between low- versus high-market-share complementors (Table A8a), further supporting the notion that even though low-share complementors increased technical innovation (patents), they lacked the complementary assets to commercialize the innovation (products).

Third, we split firms by their pre-sample product introductions, expecting that product leaders (firms in the upper quartiles) likely had relatively more complementary assets to introduce products relative to product laggards (lower quartiles). Consistent with this expectation, we found that product leaders in the treated group did not suffer a profit penalty after the intervention, but that product laggards, who were less likely to have complementary assets, did (Table A9).

Fourth, we expected negative profit effects to be transient rather than persistent as treated firms likely eventually adapted to the new competitive environment by building complementary assets (Sidak & Teece, 2009). Data provide support. The negative effect on profit wanes over time. With a 10-year posttreatment window, profit for the treated group is statistically indistinguishable from that of the control group (Table 5b). Results which excluded short-term observations (i.e., 2002–2004) to focus on the medium and long term are consistent. In contrast, the positive effect on innovation is robust over time (Table 5a).

Finally, *entry by new firms* could explain our results. Perhaps, it was the entrant (not the incumbent) complementors that generated innovation that customers valued (e.g., Agarwal, Audretsch, & Sarkar, 2010). In fact, antitrust often encourages entry and innovation by an entirely new set of firms (Parker et al., 2020). Although public-firm turnover was almost non-existent during our study period, as noted above, *private firm entries* may have increased. The data we compiled from CorpTech and Crunchbase, however, indicated that entry rates *dropped* rather than increased in treated markets post-intervention.[23] This pattern is consistent with our argument that increased challenges to develop assets in the treated markets were a more likely explanation, and entry a less likely explanation. Lack of access to complementary assets may have pushed away rather than attracted entry in the

[22]These findings were consistent with the other data patterns that we gathered, including that the treated firms experienced lower sales growth but higher R&D expenditures compared to control firms post antitrust. Non-R&D expenditures (e.g., operational, administrative costs) remained roughly equivalent. Controlling for firms' downstream capabilities (Ceccagnoli et al., 2012) (pre-sample products) did not change the results.

[23]Triangulation of CorpTech and Crunchbase records indicates that the population of private firms decreased in infrastructure software between 1998 and 2004 and the decrease is higher in treated markets (−29.9%) compared to control markets (− 20.3%). Overall, the population of firms in the treated group shrank relative to controls.

TABLE 5 (a) Temporal dynamics: Fixed effects Poisson predicting complementor innovation (patents). (b) Temporal dynamics: Fixed effects panel OLS predicting complementor profit (return on sales)

(a)			
	1996–2006 (5 years pre- and post) 1	**1998–2011 (10 years post) 2**	**1998–2011 (long term, 2002–2004 excluded) 3**
Rival complementor × post-intervention	0.91 (.000)	0.85 (.000)	1.08 (.000)
Controls			
Size (logged)	0.94 (.000)	1.01 (.000)	1.38 (.000)
R&D intensity	−1.24 (.000)	−2.89 (.000)	3.10 (.000)
Years public	−0.62 (.000)	−0.06 (.53)	−0.10 (.31)
Acquisitions	−0.26 (.000)	−0.25 (.000)	−0.54 (.000)
Firm effects	Yes	Yes	Yes
Market effects	Yes	Yes	Yes
Year effects	Yes	Yes	Yes
Log likelihood	−1,236.47	−1757.58	−923.53
(b)			
	1996–2006 (5 years pre- and post) 1	**1998–2011 (10 years post) 2**	**1998–2011 (long term, 2002–2004 excluded) 3**
Rival complementor × post-intervention	−0.68 (.06)	−0.37 (.16)	−0.05 (.82)
Controls			
Size (logged)	0.11 (.76)	−0.37 (.37)	−0.16 (.48)
R&D intensity	−0.35 (.000)	−0.36 (.000)	−0.36 (.000)
Years public	−0.07 (.87)	−0.13 (.76)	−0.23 (.28)
Acquisitions	−0.02 (.91)	0.10 (.35)	−0.26 (.22)
Firm effects	Yes	Yes	Yes
Market effects	Yes	Yes	Yes
Year effects	Yes	Yes	Yes
R-squared	.48	.47	.47

Note: *p*-values in parentheses (two-tailed tests).

short term. These and other alternative explanations are summarized in Table 4. Overall, while our results should be interpreted with caution (as is the case with any archival study), we present a broad range of evidence consistent with the idea that antitrust intervention against a dominant platform spurs innovation while dampening profits, particularly among low-market-share complementors.

6 | DISCUSSION

> "*Most harmful of all is the message that Microsoft's actions have conveyed to every enterprise with the potential to innovate in the computer industry...[Microsoft] will use its prodigious market power and immense profits to harm any firm that...could intensify competition against one of Microsoft's core products*." –Thomas Penfield Jackson, U.S. District Judge, 1999 Findings of fact.

This study started with the question of what happens to ecosystems when antitrust limits a dominant platform's ability to block competition. We introduced a seeming tension: Antitrust intervention against the platform may incentivize rival complementors to innovate when they see potential for market share gain, but it can also harm complementors if they depend on the platform for complementary assets to innovate. We focused on a landmark antitrust intervention in which the dual platform-complementor Microsoft was accused of, and subsequently prevented from, blocking competition in complementor markets. Using a novel dataset on infrastructure software from 1998 to 2004, we showed how placing competitive pressure on Microsoft resulted in a tension between innovation and profitability for complementors. Following the intervention, innovation went up but profits went down—particularly among low-market-share complementors. Implications, especially for the long-standing debate about the relationship between competition and innovation (Katila & Chen, 2008; Thatchenkery & Katila, 2021), are discussed below.

6.1 | Implications for complementors

Innovative complementors are critical for platform ecosystem success. In fact, many platforms enter complementor markets with homegrown products to jumpstart creativity and to ensure availability of high-quality complementor products (Gawer & Henderson, 2007). Yet our findings indicate that platform involvement in complementor markets can be a mixed bag from a complementor perspective.

Our data and our interviews with industry informants suggest that complementors are highly sensitive to the threat posed by a dominant platform. With the platform no longer in a position to privilege its own offerings, complementor firms often rush to capture newly viable opportunities, sparking an increase in innovation. At the same time, the ensuing "free-for-all" makes it difficult to commercialize and profit from those innovations, as complementors—especially low-market-share ones—may lack the discipline and guidance to select the right opportunities and find themselves in a race to develop assets that were previously facilitated by the dominant platform. Overall, intensifying the competitive pressures placed on the platform can create a "wild west" with both benefits and drawbacks for complementors.

Our evidence suggested that the underlying reasons for the drift between innovation and profit could be embedded in strategic capabilities. Complementors possibly received more free assets from the platform than they might have realized. They seemed slower to develop the assets themselves and thus profit from innovation, at least in the short term. The answer to whether inducing competition through antitrust unblocks innovation opportunities is thus ambiguous: antitrust intervention against a dominant platform does seem to spark patenting but this does not automatically result in products that customers value. These findings are particularly significant for regulatory changes that aim to re-invigorate ecosystems by increasing the competitive pressure on dominant platforms.

6.2 | Implications for platform ecosystems

Our findings also have broader implications for platform ecosystems (Kapoor & Agarwal, 2017). Prior work has presented a tradeoff between ecosystem stability and evolvability (Cennamo & Santaló, 2019; Katila & Ahuja, 2002; Parker & Van Alstyne, 2018; Wareham et al., 2014). We find such a trade-off in the wake of a weakened platform: innovation surges—particularly among low-market share complementors—but few firms manage to profit. And while financial performance may not be a concern from the policymaker's perspective, it is important to individual firms and may affect the viability and attractiveness of the ecosystem in the long run. Boosting competition by weakening a dominant platform may have unexpectedly far-reaching and complex consequences. To develop a healthy ecosystem in the long run, platform owners may want to resist the temptation to keep complementors weak and instead help support their development to stand on their own.

Does a platform need to be dominant for these effects to emerge? The answer may depend on the industry sector. In enterprise infrastructure software—a non-faddish industry focused on technical performance and functionality—platform dominance is likely more sustaining. In consumer-facing industries, in contrast, consumer preferences are highly varied and change rapidly, creating opportunities for smaller platforms to target a niche before broadening to a mainstream audience (Katila et al., 2022; Rietveld & Eggers, 2018). If such a pattern is typical in the industry, then platform dominance may be less relevant. Exploration of how varying degrees of dominance, including changes in which platform is currently dominant, influence complementor behavior is an interesting path for future work.

There are also implications for strategy research at the interface of public and private sectors (see Rathje & Katila, 2021). Our unique contribution is to public-sector interventions in private platform ecosystems. Prior strategy work on antitrust focused on less technology-intensive industries with a vertical structure and cast doubt on whether antitrust can actually stimulate competition and in turn innovation (Delmas, Russo, & Montes-Sancho, 2007; Kang, 2020; Madsen & Walker, 2017). In contrast, our findings support the idea that public-sector interventions can stimulate innovation. A key difference in our work is interdependence: complementors and the platform need each other. The platform in particular has an incentive to support complementors in ways that possibly initiate the effects we observed. But while the impact on innovation may be positive, we also documented reductions in profit, which suggests that public sector involvement may inhibit value capture in platform ecosystems.

Finally, our findings speak to *digital platform antitrust* by demonstrating a robust positive relationship between an antitrust intervention against a dominant technology platform and complementor innovation. This is highly relevant to the ongoing global debate over big tech platforms and whether regulators should attempt to limit their influence. We show that an antitrust remedy that is behavioral—rather than breaking up the dominant firm into smaller pieces—can spark innovation by complementors. However, antitrust intervention is *not* an automatic lever for more innovation. The intervention needs to actually change how firms perceive their competitive environment (Cattani, Sands, Porac, & Greenberg, 2018; Thatchenkery & Katila, 2021) and create incentives to reconsider a market that was previously closed to them. Even then, building needed assets is likely to be costly and take more time than anticipated.

Furthermore, antitrust intervention did *not* enable entrepreneurial entry. Rather, innovation increases were driven by incumbent complementors (particularly low-market-share ones). The payoff for most complementors - particularly complementors that did not already have high market share themselves - thus appears limited. Overall, while policymakers may be satisfied with a boost in ecosystem innovation, the observed reduction in profit suggests that, from the strategy perspective, antitrust intervention may not have delivered for complementors.

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Research data are not shared.

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SUPPORTING INFORMATION

Additional supporting information can be found online in the Supporting Information section at the end of this article.

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ONLINE APPENDIX:
Innovation and Profitability Following Antitrust Intervention Against a Dominant Platform: The Wild, Wild West?

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Strategic Management Journal, 2023

Antitrust

Competition interventions. While we focus on blocking competition in this paper, two other streams of research on competition interventions are also relevant to understand. A stream on M&As argues that, by eliminating competition, mergers can cut incentives for firms to invest in R&D (Cunningham et al., 2021).[2] "Vertical deals" for example can eliminate potential rivals and thus reshape ecosystems and value chains (Stefanadis, 1997). Altogether, this stream is consistent with the argument that competition and innovation *decreases* go hand in hand, but it rarely examines *increases* in competition.

A stream on collusion–i.e. business practices that eliminate competition among a small group of firms in an industry–reaches similar conclusions on "innovation competition." A study by Mezias and Boyle (2005), for instance, examined a collusive trust among film producers in the United States in the early 20th century. The authors found that colluding firms were slow to adopt key innovations such as feature-length films, a result broadly consistent with the argument that competition and innovation decreases go hand in hand.[3]

Vertical foreclosure. Conceptually, we can also draw a direct parallel between the vertical foreclosure literature - where a firm uses one line of business to disadvantage rivals in another line - and antitrust in platform ecosystems. In a classic example, "a flourmill that also owned a bakery could…degrade quality when selling to rival bakers—or refuse to do business with them entirely" (Khan, 2017: 731), much in the same way as a dual platform-complementor could ration platform services to rival complementors and thereby undermine innovation. In platform ecosystems, the dual platform-complementor's ability to favor its own complementor products (such as a refusal to open up an interface) could similarly give it an unfair advantage in visibility, costs and convenience, thus potentially blocking a range of viable opportunities from rivals in complementor markets.

While vertical foreclosure is an important analogy, it also differs in important ways. First, complementors are highly dependent on platforms for market access, creating potential for abuse by the platform. Platform dependence is further exacerbated by network effects that often "tip" platform markets to one or two dominant platforms, meaning that complementors need to release products on the dominant platform in order to access the bulk of their potential

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[2] Although some M&A scholars suggest that synergies provided by mergers could support rather than eliminate innovation (e.g. in rapidly moving technology industries where temporary monopolies are common), others reason that in settings where network effects promote more permanent consolidation of market share (platform ecosystems), there is a need for M&A antitrust to ensure long-term innovation (Federico et al., 2019).
[3] One recent exception is Kang (2020) who showed that manufacturing firms spent more on R&D and product development *while colluding.* That is, cartels that aim to eliminate competition and to raise prices caused firms to invest more in R&D. Because Kang's sample was restricted to manufacturing firms (R&D investments typically target process innovations), implications for other types of settings are likely different.

customers. In contrast, a vertically-integrated firm (e.g. flour mill-bakery), unless they achieve an outright monopoly, cannot completely block access to customers. Second, platforms are dependent on complementors: because complementor innovation is crucial to growing the platform ecosystem, platforms are incentivized to share complementary assets. In contrast, a flour mill that owns a bakery has no incentive to share assets with competing bakeries as it gains no benefit from an increased variety of competitors in the value chain. These characteristics of platforms (interdependencies that create potential for abuse, but also potential support to grow the ecosystem) differentiate platforms from vertical foreclosure and make them interesting to study.

Microsoft case details. Regarding sales, interoperability and bundling were the main issues. Prior to the intervention, Microsoft was shown to inhibit the functionality of competing applications (e.g., purposefully making non-Microsoft applications run more slowly or less reliably) and alleged to have created technical 'backchannels' that would improve integration between its own applications and the platform while sabotaging rival applications–actions that potentially increased sales of Microsoft's own complementor products. Microsoft was also accused of bundling its own complementor applications with the platform and allegedly making it difficult to remove them, or outright integrating rival complementor functionality in Windows Server (aka platform envelopment). Microsoft also engaged in exclusionary contracting where it would penalize a hardware manufacturer for pre-installing competing complementor apps by threatening to revoke their platform license or charging them a higher licensing fee. The intervention barred these tactics. Microsoft could no longer prevent customers from uninstalling a Microsoft application and replacing it with a competing product, or setting a competing application as the system default. Microsoft was specifically instructed to offer customers a "separate and unbiased choice" of applications they could use on a Microsoft platform.[4] Microsoft was also barred from exclusionary contracting and had to allow hardware manufacturers to pre-install any complementor apps they wished. Altogether, the pre-intervention practices inflated Microsoft's own complementor applications' market share and deflated that of complementors, and the intervention barred such attempts.

Regarding costs, prior to the intervention, Microsoft slowed down complementors' development by keeping APIs (application programming interfaces) closed. These practices created friction in development and elevated other complementors' development costs while giving a cost advantage to Microsoft's own offerings. Microsoft further sold "access licenses" that helped with integration by granting complementors full access, but added cost (Gartner, 2000). The antitrust intervention forced Microsoft to reverse these practices and open many of its APIs in order to facilitate smoother application development. Microsoft was also forbidden from "retaliating" (via penalties such as higher platform licensing fees or cutting off access to technical resources such as APIs and SDKs) against competing software firms that developed or bundled competing applications.[5] Altogether, the antitrust intervention focused on reducing Microsoft's ability to exploit its dominance through bundling, interoperability and cost advantages for its own complementor applications.

Matching: Main Analysis and Robustness

Matching on region, firm size and IPO year addresses the possibility that differences in these characteristics (e.g. through resource availability) across treated and controls could influence innovation or profit. Matching on R&D, patents, and ROS aims to lessen concerns that treated firms may be more profitable or more technology intensive than controls.

[4] U.S. Department of Justice, *US v. Microsoft*: *Plaintiff's Findings of Fact,* 1999.
[5] U.S. Department of Justice, *US v. Microsoft: Revised Proposed Final Judgment*, 2001.

We employed propensity score matching (PSM) in our main analysis. As de Figueiredo, Feldman, and Rawley (2019) note, in samples with multiple confounders, PSM remains stable even as the number of potential confounders increases, reducing researcher's subjective assessments about "similarity," and increasing the number of treated firms that can be matched (reducing the data that need to be thrown away). To reduce the likelihood of poor matches, we eliminated extreme values of the propensity score by trimming the top and bottom 5% from the sample. Treated firms were matched without replacement (i.e., control firms are not re-used).

Table A4 reports results for alternative matching schemes, including different trims for PSM (top 1%, no trim). For *coarsened exact matching* (CEM), we used the same matching criteria as our main PSM analysis, resulting in a CEM matched sample of 310 firm-years (54 firms). For *exact hand matching,* we followed prior work (Hsu, 2006) by first identifying an exact match for each treated firm (looking for a match in the same decile for continuous variables). If an exact match was not found, we relaxed the non-treated firm's IPO year to the year before or after that of the treated firm. If there was still no match, we relaxed the geographic region to include neighboring states. If there was still no match, we relaxed the firm size and R&D expenditure criteria to firms in the same quintile. If there was still no match, we dropped the treated firm from the analysis and proceeded to match the next treated firm. Our exact matching sample is 298 firm-years (50 firms). To illustrate the similarity of the treated and the matched control firms, Table A5 reports descriptive statistics. The control group is very similar to the treated along the matching criteria, and thus likely provides a reliable counterfactual. Note that our estimates are doubly robust as we also add controls for firm heterogeneity and firm fixed effects in all models.

Regression Analysis: Statistical Robustness

Alternate specifications. We tested alternatives to fixed effects Poisson when predicting innovation. Because a number of our firms did not patent, we ran *zero-inflated Poisson*. For robustness, we also ran *panel OLS*. Results are consistent (Table A3a).

Serial correlation. Because firm fixed effects were included, we probed the sensitivity of the models to serial correlation per Bertrand et al. (2004). We collapsed our multi-year panel into a panel of two, with one observation containing the three-year-average of variables prior and another observation after the treatment. Results are consistent indicating that serial correlation is not a significant concern (Table A1).

Continuous treatment. Our treatment vs control design used a variable with differing treatment intensity (Microsoft's market share in each complementor market). In main analyses, we discretized the treatment because interpreting difference-in-differences coefficients with a continuous (rather than binary) treatment variable can be challenging due to treatment effect heterogeneity (Callaway et al., 2021). For robustness, we followed Card (1992) and estimated the effect of a *continuous* treatment (Microsoft's market share in the focal complementor's markets). Results in Table A1 are highly consistent.

Alternative Explanations and Auxiliary Analyses

We also test alternative explanations for our results. First, could our results be influenced by **multimarket contact** (MMC)? Multimarket theory argues that greater overlap between firms *reduces* competitive intensity and could lift profits (Gimeno, 1999). So if control firms were also multimarket firms that were more "friendly" with Microsoft, it could explain why they were not influenced. We do not find a major difference in treated vs controls in this regard (average number of market segments is 1.64 among treated firms and 1.46 for control firms). Although most complementors compete in 1-2 markets, making MMC less of a concern, we also test its potential influence by limiting the sample to complementors that compete in only one market (and who therefore cannot have multimarket contact with Microsoft or any other firm). Results for single-market complementors (Table A3) are consistent, reducing the concern about multimarket forbearance as an explanation.

Second, could our results be driven by the **dot-com crash** in the year 2000 rather than the antitrust intervention? Again, this does not seem likely. As shown in figure 3, *both* the treated and control groups were impacted by the crash in 2000, and their divergence begins after the intervention (year 0), not after the crash (year -1). Tests with placebo years, noted above, suggest a similar conclusion. If the crash were an explanation, we would also expect to see similar trends for both treated and control firms, but this is not the case. In particular, we would not expect the control firms' profit to recover faster than treated firms'.

Third, is it possible that the prospect of profitable sales that motivated innovators could be about "big paydays" rather than average profits, and this **variability** could be more significant than averages? If this were a likely explanation, we would expect treated firms to experience more extreme (positive and negative) profit outcomes given their investments in innovation. Our data do not support this explanation, however. Models predicting whether the firm achieves "superior profits" in a year (i.e. 2x the industry average or >95th percentile) were consistent with our main results (Table A2). In fact, treated firms were *less* likely to experience extreme positive outcomes after the intervention.

Finally, **baseline innovation capabilities** could be an alternative explanation. Is it possible that high market share firms were already inventing at a high rate and so had less room to grow (making it hard to empirically observe an effect)? Again, this explanation seems unlikely. When we included *pre-sample patents* as a control for baseline innovation capabilities (Table A10), low market share firms still remained the ones driving the effect, supporting our original findings.

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APPENDIX Table A1. Difference-in-differences validity tests

	Complementor Innovation (Patents)				Complementor Profit (ROS)			
	Collapse pre- and post	Continuous Treatment	Random Implementation of Treatment	Event Study	Collapse pre- and post	Continuous Treatment	Random Implementation of Treatment	Event Study
	1	2	3	4	5	6	7	8
Rival complementor x Post-intervention	1.43				-0.37			
	(0.000)				(0.05)			
Microsoft complementor market share x Post-intervention		3.13				-4.10		
		(0.000)				(0.03)		
Randomized treatment x Post-intervention			-0.06				0.04	
			(0.56)				(0.72)	
Event study								
Rival complementor x Three years pre-intervention				-0.07				-0.15
				(0.74)				(0.79)
Rival complementor x Two years pre-intervention				-0.06				-0.14
				(0.56)				(0.72)
Rival complementor x One year pre-intervention				-0.02				-0.43
				(0.90)				(0.77)
Rival complementor x One year post-intervention				0.37				-0.14
				(0.004)				(0.26)
Rival complementor x Two years post-intervention				0.33				-1.14
				(0.02)				(0.03)
Rival complementor x Three years post-intervention				0.61				-1.47
				(0.000)				(0.02)
Controls								
Size (logged)	0.04	1.72	1.65	1.43	0.15	-0.05	-0.12	-0.13
	(0.65)	(0.000)	(0.000)	(0.000)	(0.000)	(0.88)	(0.71)	(0.72)
R&D intensity	-1.12	1.11	0.59	-0.22	0.66	-0.02	-0.02	-0.02
	(0.000)	(0.01)	(0.70)	(0.63)	(0.003)	(0.000)	(0.000)	(0.002)
Years public	-2.72	-0.95	-0.52	-1.05	-0.75	0.38	0.22	0.41
	(0.11)	(0.000)	(0.40)	(0.000)	(0.53)	(0.22)	(0.34)	(0.16)
Acquisitions	-0.36	-0.31	-0.35	-0.40	-1.13	0.19	0.09	0.11
	(0.73)	(0.000)	(0.01)	(0.000)	(0.27)	(0.37)	(0.57)	(0.59)
Firm fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
Market fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
Log likelihood	-81.47	-923.70	-632.00	-934.72				
R-squared					0.37	0.30	0.19	0.89

P-values in parentheses (two-tailed tests). Innovation results are reported using fixed effects Poisson and profit using fixed effects panel OLS.

APPENDIX Table A2. Robustness tests: Alternate dependent variables

	Complementor Innovation				Complementor Profit			
	Main measure: Patents (Poisson)	Citation-weighted patents (Poisson)	R&D expenditures (logged, OLS)	Superior patent output (>95 percentile, logit)	Main measure: ROS (OLS)	Net income (logged, OLS)	Superior profits (>2X average, logit)	Superior profits (>95 percentile, logit)
	1	2	3	4	6	7	8	9
Rival complementor x Post-intervention	0.42	0.33	0.05	0.11	-0.70	-0.02	-2.88	-1.37
	(0.000)	(0.000)	(0.08)	(0.08)	(0.02)	(0.10)	(0.09)	(0.09)
Controls								
Size (logged)	1.75	2.43	0.32	0.16	-0.09	0.02	-0.03	0.25
	(0.000)	(0.000)	(0.000)	(0.11)	(0.79)	(0.13)	(0.93)	(0.40)
R&D intensity	1.19	2.78		-0.001	-0.02	0.000	-0.47	-1.50
	(0.01)	(0.000)		(0.06)	(0.000)	(0.41)	(0.59)	(0.37)
Years public	-0.94	-1.45	-0.10	-0.07	0.44	-0.02	0.11	0.27
	(0.000)	(0.000)	(0.001)	(0.33)	(0.13)	(0.02)	(0.80)	(0.48)
Acquisitions	-0.32	-0.41	-0.01	-0.14	0.21	0.001	0.97	-0.22
	(0.000)	(0.000)	(0.70)	(0.03)	(0.27)	(0.94)	(0.24)	(0.76)
Firm fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
Market fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
Log likelihood	-941.01	-1701.48		-1032.23			-79.94	-107.83
R-squared			0.65		0.32	0.27		

P-values in parentheses (two-tailed tests)

APPENDIX Table A3a. Robustness tests: Alternate specifications predicting complementor innovation

	Zero-Inflated Poisson	Panel OLS[1]	Single Market Firms	Drop Firm Size	Drop R&D	Size: Employees	R&D: Expenditures	Size (Emp) & R&D Expenditures	Drop Years Public	HHI	Number of Competitors
	1	2	3	4	5	6	7	8	9	10	11
Rival complementor x Post-intervention	0.34	0.33	0.61	0.47	0.63	0.40	0.75	0.68	0.38	0.37	0.61
	(0.001)	(0.03)	(0.02)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.002)	(0.000)
Controls											
Size (revenue)	1.03	1.17	0.82		2.29		0.87		2.76	2.67	2.68
	(0.000)	(0.000)	(0.04)		(0.000)		(0.000)		(0.000)	(0.000)	(0.000)
Size (employees)						4.36		1.38			
						(0.000)		(0.000)			
R&D (intensity)	0.93	0.01	0.11	-0.02		1.57			3.86	3.34	3.14
	(0.03)	(0.001)	(0.81)	(0.96)		(0.000)			(0.000)	(0.000)	(0.000)
R&D (expenditures)							4.62	4.78			
							(0.000)	(0.000)			
Years public	-0.32	-0.56	-1.95	-1.28	-1.03	-0.74	-0.77	-0.71		-0.74	-0.78
	(0.03)	(0.002)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)		(0.000)	(0.000)
Acquisitions	-0.43	0.06	-0.93	-0.14	-0.62	-0.68	-0.64	-0.64	-0.61	-0.53	-0.57
	(0.000)	(0.65)	(0.002)	(0.02)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Market HHI										-0.0003	
										(0.001)	
Number of competitors in market											0.02
											(0.08)
Firm fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Market fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Log likelihood	-1045.00	-294.67	-294.67	-1027.36	-813.69	-808.65	-742.10	-745.51	-797.62	-779.60	-783.38

P-values in parentheses (two-tailed tests). Results are reported using fixed effects Poisson unless otherwise noted.

[1] For OLS model predicting patents, dependent variable is log(patents+1)

APPENDIX Table A3b. Robustness tests: Alternate specifications predicting complementor profit

	Single Market Firms	Drop Firm Size	Drop R&D	Size: Employees	R&D: Expenditures	Size (Emp) & R&D Expenditures	Drop Years Public	HHI	Number of Competitors
	1	2	3	4	5	6	7	8	9
Rival complementor x Post-intervention	-0.57	-0.67	-1.28	-0.68	-1.26	-1.24	-0.67	-0.63	-0.63
	(0.08)	(0.01)	(0.10)	(0.01)	(0.11)	(0.08)	(0.02)	(0.01)	(0.04)
Controls									
Size (revenue)	-0.06		-0.64		-0.26		-0.04	-0.11	-0.06
	(0.69)		(0.45)		(0.83)		(0.90)	(0.75)	(0.87)
Size (employees)				-0.23		0.12			
				(0.68)		(0.87)			
R&D (intensity)	-0.02	-0.33		-0.33			-0.33	-0.32	-0.33
	(0.001)	(0.000)		(0.000)			(0.000)	(0.000)	(0.000)
R&D (expenditures)					-0.97	-1.62			
					(0.52)	(0.29)			
Years public	0.16	0.14	1.46	0.18	1.41	1.36		0.17	0.16
	(0.46)	(0.61)	(0.30)	(0.56)	(0.34)	(0.30)		(0.56)	(0.61)
Acquisitions	0.44	0.27	0.20	0.29	0.23	0.23	0.28	0.28	0.28
	(0.53)	(0.38)	(0.43)	(0.36)	(0.37)	(0.36)	(0.39)	(0.39)	(0.37)
Market HHI								0.0002	
								(0.39)	
Number of competitors in market									0.01
									(0.65)
Firm fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Market fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
R-squared	0.36	0.44	0.08	0.46	0.08	0.08	0.46	0.46	0.46

P-values in parentheses (two-tailed tests). Results are reported using fixed effects panel OLS.

APPENDIX Table A4. Robustness tests: Alternate matching methods

	Complementor Innovation (Patents)					Complementor Profit (ROS)				
	No Matching	PSM, No Trim	PSM, Trim 1%	CEM	Exact Match	No Matching	PSM, No Trim	PSM, Trim 1%	CEM	Exact Match
	1	2	3	4	5	6	7	8	9	10
Rival complementor x Post-intervention	0.51	0.59	0.57	0.59	0.55	-0.44	-0.88	-0.89	-0.74	-0.49
	(0.000)	(0.000)	(0.000)	(0.003)	(0.000)	(0.01)	(0.06)	(0.06)	(0.03)	(0.02)
Controls										
Size (logged)	1.33	1.51	1.52	1.47	1.59	-0.15	-0.83	-0.83	-1.10	-0.42
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.42)	(0.26)	(0.26)	(0.10)	(0.11)
R&D intensity	-0.30	-0.12	-0.20	-0.41	0.26	-0.02	-0.02	-0.02	-0.02	-0.02
	(0.50)	(0.71)	(0.56)	(0.39)	(0.63)	(0.01)	(0.002)	(0.002)	(0.000)	(0.03)
Years public	-0.96	-1.09	-1.11	-1.79	-0.82	0.21	0.46	0.45	0.47	0.35
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.23)	(0.09)	(0.10)	(0.13)	(0.15)
Acquisitions	-0.43	-0.41	-0.42	-0.18	-0.34	0.11	0.11	0.11	0.16	0.04
	(0.000)	(0.000)	(0.000)	(0.13)	(0.000)	(0.52)	(0.29)	(0.30)	(0.16)	(0.89)
Firm fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Market fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Log likelihood	-599.22	-1045.46	-1031.87	-349.92	-461.63					
R-squared						0.21	0.30	0.30	0.32	0.24

P-values in parentheses (two-tailed tests). Innovation results are reported using fixed effects Poisson and profit using fixed effects panel OLS.
Full sample (no matching) is 78 firms, CEM matched sample is 54 firms, exact (hand) matched sample is 50 firms.

APPENDIX Table A5. Summary statistics for treated vs. control firms

	Before Matching				After Matching			
Variables (Matching Criteria)	Treated Mean	Control Mean	p-value (t-test)	p-value (KS-test)	Treated Mean	Control Mean	p-value (t-test)	p-value (KS-test)
Size (logged)	0.65	0.47	0.08	0.00	0.58	0.50	0.78	0.38
R&D intensity	0.36	0.41	0.59	0.41	0.37	0.40	0.73	0.87
Years public	7.01	6.27	0.01	0.67	6.45	6.42	0.42	0.93
Region: Silicon Valley	0.26	0.36	0.11	0.27	0.32	0.34	0.47	0.64
Region: Boston	0.09	0.12	0.31	0.83	0.10	0.08	0.46	0.92
Region: Los Angeles	0.11	0.10	0.72	0.98	0.09	0.10	0.87	0.99
Pre-sample patents[1]	1.83	1.62	0.55	0.13	1.70	1.69	0.97	0.44
Pre-sample profits[1]	-0.20	-0.08	0.09	0.01	-0.16	-0.12	0.37	0.19

[1] "Pre-sample" variables are averaged from 1995-1997
KS-test: Kolmogorov-Smirnov test for equality of distribution
Two-tailed tests. N=416 before matching, N=374 after propensity score matching

Appendix Table A6. Three-way interaction (triple difference) comparing low versus high market share complementors

	Complementor Innovation (Patents)			Complementor Profit (ROS)
	Main controls	No R&D intensity	Pre-sample patents	Main controls
	1	2	1	4
Low-market-share complementor x Rival complementor x Post-intervention	0.14	0.47	0.36	-0.78
	(0.002)	(0.001)	(0.007)	(0.02)
Controls				
Size (logged)	0.12	1.74	1.67	-0.05
	(0.000)	(0.000)	(0.000)	(0.89)
R&D intensity	0.42			-0.32
	(0.002)			(0.000)
Pre-sample patents			0.16	
			(0.001)	
Years public	0.15	-0.93	-0.82	0.35
	(0.000)	(0.000)	(0.000)	(0.27)
Acquisitions	0.06	-0.38	-0.38	0.18
	(0.000)	(0.000)	(0.000)	(0.40)
Firm fixed effects	Y	Y	Y	Y
Market fixed effects	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y
Log likelihood	-945.23	-945.23	-945.23	
R-squared				0.46

P-values in parentheses (two-tailed tests). Innovation results are reported using fixed effects Poisson and profit using fixed effects panel OLS.
Models 2-3 serve as extra robustness checks regarding differences in baseline innovation rates among high versus low market share complementors.

APPENDIX Table A7. Mechanism tests: Patent variability (hits and flops)

	All complementors			Low market share complementors			High market share complementors		
	Hits and Flops	**Hits**	**Flops**	**Hits and Flops**	**Hits**	**Flops**	**Hits and Flops**	**Hits**	**Flops**
	1	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**
Rival complementor x Post-intervention	0.94	0.61	1.26	1.65	1.62	0.17	-0.17	-0.76	0.17
	(0.000)	(0.03)	(0.000)	(0.000)	(0.000)	(0.01)	(0.51)	(0.06)	(0.79)
Controls									
Size (logged)	3.29	3.13	3.61	1.99	1.78	2.11	6.16	6.10	7.03
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.004)	(0.000)	(0.000)	(0.000)
R&D intensity	5.36	5.16	6.10	3.23	3.01	3.82	15.27	16.11	17.35
	(0.000)	(0.000)	(0.000)	(0.001)	(0.02)	(0.01)	(0.000)	(0.000)	(0.000)
Years public	-1.28	-2.12	-0.87	-0.83	-0.89	-0.97	1.25	-0.55	3.40
	(0.000)	(0.000)	(0.09)	(0.01)	(0.05)	(0.05)	(0.29)	(0.72)	(0.06)
Acquisitions	-0.46	-0.19	-0.81	-0.66	-0.50	-1.17	-0.57	-0.26	-0.92
	(0.000)	(0.25)	(0.000)	(0.01)	(0.07)	(0.03)	(0.002)	(0.31)	(0.001)
Firm fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Market fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Log likelihood	-293.80	-217.20	-159.70	-160.40	-112.80	-83.82	-157.90	-133.70	-83.99

P-values in parentheses (two-tailed tests)

Only includes firms with at least one patent (50 firms, 294 firm-years)

Following Ahuja & Lampert (2001), hits (flops) are defined as number of patents with citations in the top (bottom) quartile relative to other firms in the industry.

APPENDIX Table A8a. Mechanism tests: New product introductions

	All complementors	Low market share complementors	High market share complementors
	1	2	3
Rival complementor x Post-intervention	0.06	0.14	-0.25
	(0.68)	(0.14)	(0.17)
Controls			
Size (logged)	-0.27	-0.01	0.16
	(0.03)	(0.90)	(0.29)
R&D intensity	0.02	0.02	1.55
	(0.14)	(0.005)	(0.01)
Years public	-0.20	-0.08	-0.40
	(0.15)	(0.40)	(0.08)
Acquisitions	0.16	0.18	-0.14
	(0.10)	(0.02)	(0.27)
Firm fixed effects	Y	Y	Y
Market fixed effects	Y	Y	Y
Year fixed effects	Y	Y	Y
Log likelihood	-999.77	-863.01	-179.80

P-values in parentheses (two-tailed tests). Results are reported using fixed effects Poisson. Robust to alternate dependent variables (new product lines, new product versions, etc).

APPENDIX Table A8b. Mechanism tests: Profit decomposition

	All complementors		Low market share complementors		High market share complementors	
	Price-cost margin	Asset utilization	Price-cost margin	Asset utilization	Price-cost margin	Asset utilization
	1	2	5	6	3	4
Rival complementor x Post-intervention	-0.30	-0.26	-0.27	-0.33	-0.07	0.10
	(0.02)	(0.27)	(0.07)	(0.28)	(0.16)	(0.06)
Controls						
Size (logged)	-0.27	0.38	-0.21	0.23	-0.20	0.16
	(0.10)	(0.33)	(0.31)	(0.63)	(0.27)	(0.15)
R&D intensity	-0.03	-0.01	-0.03	-0.004	-0.86	-0.41
	(0.000)	(0.09)	(0.000)	(0.19)	(0.07)	(0.41)
Years public	0.11	0.03	0.10	0.04	-0.13	-0.23
	(0.39)	(0.88)	(0.49)	(0.88)	(0.31)	(0.22)
Acquisitions	0.07	-0.12	0.08	-0.07	0.06	0.10
	(0.39)	(0.33)	(0.53)	(0.33)	(0.46)	(0.04)
Firm fixed effects	Y	Y	Y	Y	Y	Y
Market fixed effects	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y
R-squared	0.32	0.02	0.24	0.03	0.20	0.32

P-values in parentheses (two-tailed tests). Results are reported using fixed effects panel OLS.

APPENDIX Table A9. Mechanism tests: Product leaders versus laggards

	Product Leaders				Product Laggards			
	Complementor Innovation	Complementor Profit			Complementor Innovation	Complementor Profit		
	Patents (Poisson)	ROS (OLS)	Lerner index (OLS)	Asset utilization (OLS)	Patents (Poisson)	ROS (OLS)	Lerner index (OLS)	Asset utilization (OLS)
	1	2	3	4	5	6	7	8
Rival complementor x Post-intervention	2.46	0.01	-0.20	-0.14	0.31	-0.96	-0.26	-1.06
	(0.000)	(0.93)	(0.77)	(0.11)	(0.02)	(0.02)	(0.07)	(0.17)
Controls								
Size (logged)	4.31	0.39	-0.05	0.11	2.65	-1.61	-0.13	0.15
	(0.000)	(0.25)	(0.72)	(0.14)	(0.000)	(0.05)	(0.49)	(0.75)
R&D intensity	10.92	-0.38	0.15	1.13	0.17	-2.46	-0.71	-3.04
	(0.000)	(0.66)	(0.78)	(0.12)	(0.79)	(0.08)	(0.27)	(0.34)
Years public	-17.34	0.54	-2.52	3.42	-1.06	0.83	-0.18	-1.31
	(0.000)	(0.91)	(0.30)	(0.05)	(0.01)	(0.24)	(0.41)	(0.28)
Acquisitions	-0.80	-0.11	-0.04	-0.04	-1.27	1.18	0.24	-0.16
	(0.000)	(0.06)	(0.56)	(0.31)	(0.24)	(0.26)	(0.27)	(0.66)
Firm fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
Market fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
Log likelihood	-131.60				-185.40			
R-squared		0.71	0.63	0.82		0.39	0.25	0.11

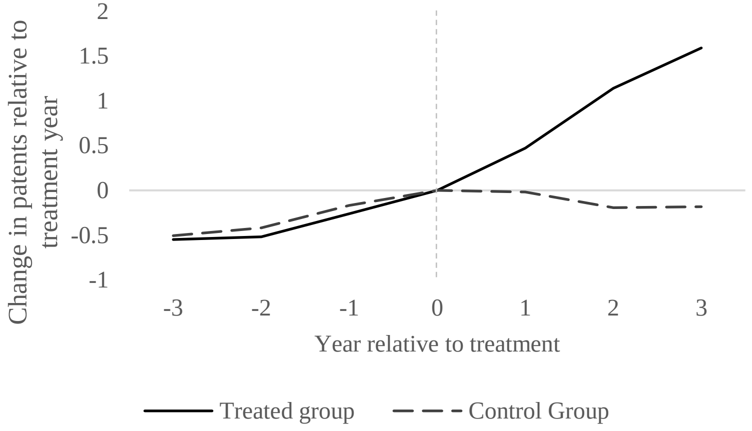
P-values in parentheses (two-tailed tests).

Appendix Table A10. Robustness checks: Control for pre-sample patenting when predicting complementor innovation

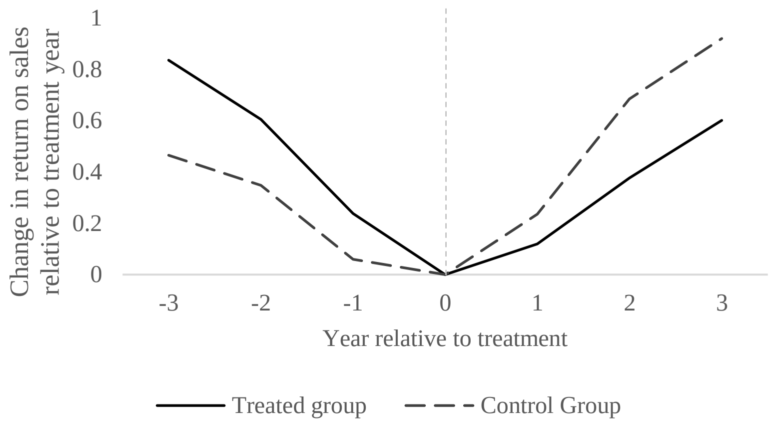
	All complementors	Low market share complementors	High market share complementors
	1	**2**	**3**
Rival complementor x Post-intervention	0.59	0.39	0.09
	(0.000)	(0.02)	(0.56)
Controls			
Size (logged)	2.21	0.58	4.17
	(0.000)	(0.000)	(0.000)
R&D intensity	-0.03	-0.30	9.51
	(0.40)	(0.34)	(0.000)
Years public	-1.05	-0.72	-0.88
	(0.000)	(0.000)	(0.07)
Acquisitions	-0.63	-0.95	-0.54
	(0.000)	(0.000)	(0.000)
Pre-sample number of patents	0.14	0.27	0.06
	(0.000)	(0.000)	(0.46)
Market fixed effects	Y	Y	Y
Year fixed effects	Y	Y	Y
Chi-squared	1066.00	231.82	573.77

P-values in parentheses (two-tailed tests). Results are reported using random effects Poisson to avoid collinearity with pre-sample patent control. j

APPENDIX Figure A1a. Visualization of trends using relative change measure of measure of complementor innovation

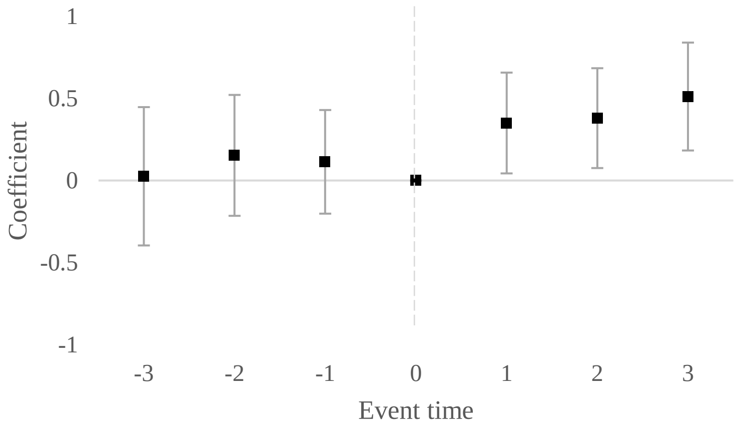


APPENDIX Figure A1b. Visualization of trends using relative change complementor profit



Yearly change (relative to treatment year) in the mean of dependent variable in treatment versus control groups

APPENDIX Figure A2a. Event study: triple-diff estimates of intervention's effects on <u>low</u> market share complementor innovation using leads and lags (H1b)



APPENDIX Figure A2b. Event study: triple-diff estimates of intervention's effects on <u>low</u> market share complementor profit using leads and lags (H2b)