INNOVATION AND PROFITABILITY FOLLOWING ANTITRUST INTERVENTION AGAINST A DOMINANT PLATFORM: THE WILD, WILD WEST?

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Abstract

Drawing on research on competitive repositioning, we examine how an antitrust intervention against a dominant technology platform prompts changes in competition in an ecosystem, in turn influencing complementor and platform performance. In particular, we argue that sparking new opportunities for complementors by weakening a dominant platform's market power can drive increases in innovation but dampen the viability of the platform ecosystem by reducing profits. Using a novel dataset on enterprise infrastructure software from 1998 to 2004 and a difference-in-differences design using matching and synthetic controls, we examine the relationship between the U.S. Department of Justice's antitrust intervention against Microsoft (dominant enterprise server platform) and subsequent innovation and profitability by infrastructure applications firms (enterprise complementors). The data show that innovation—measured by citation-weighted patents—is increased following the antitrust intervention. However, profitability is reduced. The counterintuitive finding is that antitrust intervention benefits ecosystems in the form of innovation but may threaten the financial viability of some of the very firms it is meant to help. Our results contribute to understanding links between competition and innovation, and the opportunities and threats related to dominant platforms in their ecosystems.

"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.

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.

Much research argues that the threat posed by a dominant firm has detrimental effects for competitors, potentially killing innovation and reducing product choice for customers (Kapoor 2018; Wen & Zhu, 2019). The threat may be particularly significant when it comes to dominant technology *platforms* that also offer products in markets complementary to the platform, because other complementors can be discouraged from investing in products and technologies that overlap with those of the platform owner (Kapoor & Agarwal, 2017; Seamans & Zhu, 2017; Zhu, 2019).[1] In response, regulators worldwide have opened antitrust inquiries against platform owners such as Google, Amazon, Apple, and Alibaba, alleging that they use their platforms to block competition and innovation (Espinoza, 2020; Kendall & Copeland, 2020). However, research on regulations aimed at technology platforms remains sparse,[2] and the effects of regulators' efforts to curb the power of dominant platforms are poorly understood.

Extant platforms research points towards a potential performance paradox. On the one hand, because dominant platforms can shape their ecosystems and provide a stabilizing "order" to customer expectations and the role of different complementors (Gavetti, Helfat, & Marengo, 2017; Gawer & Henderson, 2007), reducing their power may invite wasteful imitation and lower-quality offerings, which may lower the pull of the ecosystem for

[1] In our usage, 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.

[2] Prior work has typically focused on regulations blocking collusion, or on lessening the power of incumbents as a group, often in public infrastructure and manufacturing (Kang, 2020; Madsen & Walker, 2017), not on interventions targeting a singular technology platform.

everyone (Hannah & Eisenhardt, 2018; Helfat & Raubitschek, 2018; Zhang, Li, & Tong, 2020).[3] On the other hand, dominant platforms limit the incentives of complementors to innovate in markets where they overlap with the platform firm (Kamepalli et al., 2020; Wen & Zhu, 2019), which may reduce competition and variety to the detriment of innovation and the ecosystem as a whole (Gawer & Henderson, 2007; Ozcan & Hannah, 2020). Overall, how changes in competitive threat by a dominant platform modify the innovativeness and profitability of complementors is an open question.

In this paper, we examine this question in the specific case where antitrust intervention attempts to alleviate the competitive threat by *weakening* the dominant platform. We ask, *How is antitrust intervention against a dominant platform related to innovation and profitability of platform ecosystem firms?* Although much research focuses on the implications of dominant firms' growing market power (Seamans & Zhu, 2017; Wen & Zhu, 2019), we know comparatively little about situations in which regulators effectively put a harness on a dominant firm. Drawing on competitive repositioning research (Gimeno et al. 2006; Seamans & Zhu, 2017), we propose that such interventions can lower the competitive threat to other firms in the ecosystem, making more opportunities seem viable and thus prompting innovation. Nevertheless, we argue, these new opportunities may add wasteful development and increase repositioning costs, decreasing profit. This presents an interesting tradeoff for platform ecosystems. Ongoing antitrust cases in the U.S., Europe, and China heighten the importance for the strategy field to understand these issues.

We focus on the example of the U.S. Department of Justice's landmark antitrust

[3] Empirical work that examines the reverse case (i.e. increasing power of platforms) is consistent with this argument. Wen and Zhu's (2019) study of the Android ecosystem finds that Google's entry threat reduced complementor innovation yet increased profits, possibly by reducing wasteful development such as redundant apps. Similarly, Zhang, Li, and Tong (2020) show that Apple's iOS gatekeeping policies encouraged knowledge sharing while Seamans and Zhu (2017) find that entry by Craigslist prompted efficiency-focused measures such as cost-cutting in local newspaper markets.

intervention against a dominant technology platform owner, Microsoft. Despite the case's critical importance to antitrust policy, particularly in technology industries (Wall Street Journal, 2019), empirical research on its consequences has been limited.[4] Using a difference-in-differences design that takes advantage of variation in Microsoft's competitiveness across complementor markets, we examine how innovation and profitability in enterprise infrastructure software[5] changed following the intervention. A unique strength of our study is our comprehensive coverage of the population of complementors (enterprise infrastructure developers) on both sides of the intervention. We supplement the quantitative analysis with interviews with industry participants and a review of public documents regarding the case.

The key finding is that antitrust intervention spurs innovation yet surprisingly dampens profits across the ecosystem. After the intervention, innovation by rival complementors (those that faced a significant competitive threat from Microsoft pre-antitrust) increased. Our supplementary analysis further showed that these firms adopted more diverse technical problem-solving approaches after the intervention, suggesting that they were attempting to reposition in the newly opened space. Simultaneously, however, profitability of firms in the ecosystem (both rival complementors and the platform) dropped, suggesting that weakening a dominant platform makes it difficult for firms to financially benefit. Our results thus suggest balancing the benefits of innovation against possibly weaker financial viability of ecosystem firms when considering undermining the power of a dominant platform.

RESEARCH BACKGROUND

Two streams of research – one on competitive repositioning, and the other on

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[4] Lerner's (2001) broad-ranging review of all software markets examined the allegations that Microsoft impeded innovation *prior* to the intervention but, given that the study was conducted while the case was still ongoing, was not able to examine its consequences.

[5] Our focus is on enterprise *infrastructure* software. In contrast, user-facing applications covering a wide range of end-user functions, such as word processing and supply chain logistics, are outside the scope of our study. Examples of prominent user-facing applications firms include SAP and Salesforce (see e.g. Ceccagnoli et al., 2012), which were *not* developing infrastructure software during our study time period.

regulation – are relevant for our research question. Research on competitive repositioning explains why complementor firms invest (or not) in response to changing levels of threat from a dominant firm. The research stream on regulation, meanwhile, covers regulatory interventions that seek to make ecosystems more competitive and more innovative.

Competitive Repositioning

Research on *competition-driven repositioning* examines how firms change strategies and reposition (i.e., change their market positioning) when competition in the industry changes (Gimeno, Chen, & Bae, 2006; Seamans & Zhu, 2017; Wen & Zhu, 2019). In particular, competitive repositioning argues that firms seek "alignment" with the intensity of competitive threats in the environment (Gimeno et al. 2006). Dominant platforms are a key source of competitive threat that often drives firms to reposition. Platform owners can privilege their own products on the platform or bundle other products with the platform, giving the platform owner an advantage in pricing (as the bundle may be cheaper than buying products separately), convenience, and visibility (Eisenmann et al., 2011; Wen & Zhu, 2019). Changes in such a competitive threat are likely to push complementor firms to reposition their technologies and products – either by altering the types of offerings they have or their participation in the market altogether.

Innovation. Competitive repositioning research has specifically examined what happens to innovation when a platform becomes dominant. Wen and Zhu (2019) showed that complementors diverted innovation away from the markets in which the dominant platform (Google) posed a greater threat. Zhu and Liu (2018) similarly found that third-party sellers were more likely to exit product markets following entry by Amazon. The key insight is that complementors often reposition when facing a heightened threat from the dominant platform.

Related arguments in organizational learning, often labelled the "threat rigidity view" (Chattopadhyay, Glick, & Huber, 2001; Staw, Sandelands, & Dutton, 1981), are consistent with repositioning findings. These arguments suggest that rigidities--such as limits in the firm's ability to solve problems--arise when managers perceive competition as a serious threat. When this occurs, information processing is constrained, managers reduce the range of approaches they use to solve problems and innovation is diverted away from environments where the competitive threat is high. Chen and colleagues (2007), for example, found that when perceived competitive tension in a firm's markets was high, managers initiated a rigid response rather than attempted to differentiate from the dominant firm. Similarly, Katila and Chen (2008) documented that robotics firms introduced less innovative products when they engaged in head-to-head "racing" with competitors. Although these studies were not in platform markets, collectively they support the theoretical argument that greater competitive threat from a dominant firm restricts innovation.

Although competitive repositioning research is relatively silent on what happens when dominant firm threat *weakens* (e.g., through regulatory intervention), our proposal is that arguments can be extended to this case. We elaborate on this point in the hypotheses below.

Profitability. Competitive repositioning research has also examined what happens to profitability when a platform becomes dominant. Again, the focus is on cases where the threat is increasing. These arguments suggest that although innovation may suffer as described above, individual complementor firms may be able to profit, especially if the platform serves as a disciplining force that reduces wasteful development such as redundant apps (Wen & Zhu, 2019). One potential role of a dominant firm is to function as a source of "order" that sets expectations and prevents low-quality or wasteful development by other firms (Helfat & Raubitschek, 2018; Wen & Zhu 2019). Once this order is lifted, profit implications may not be straightforward. One possibility is that a more level playing field introduced by reduced platform competition could incentivize complementors to compete more fiercely in pursuit of previously unavailable opportunities, which could increase costs and drive down profit. Overall, while it does not focus on reduced platform threat, prior work suggests that profit implications may be negative, particularly if the variety that is generated is wasteful in terms of low quality or duplication of existing efforts.

Learning research similarly confirms that a dominant firm helps increase efficiency of rivals, aiding profitability (Dutton & Jackson, 1987; Staw et al., 1981). Gilbert's (2005) analysis of how newspapers responded to the threat of online media showed that the more salient the threat, the more newspapers focused on increasing efficiency in existing businesses. Similarly, Chattopadhyay and colleagues' multi-industry study (2001) found that firms facing higher environmental threat took more internal, efficiency-enhancing actions (rather than riskier, externally directed actions). Studies on learning races find similar effects, as exposure to competition from larger firms can actually help "strengthen" smaller firms, forcing them to increase their survivability (Barnett & McKendrick, 2004). Altogether, this work establishes a link between dominant players and stronger performance of ecosystem firms, but the case of diminished threat from the dominant firm is more rarely examined.

Reducing the threat of dominant competitors: Prior work on regulatory interventions

As noted above, prior work on technology platforms rarely examines reductions in a platform's threat. Ecosystems typically evolve in a path-dependent way towards more dominant players (Katz & Shapiro, 1994), not less. This makes regulation (e.g., antitrust) one of the few paths towards pushing down the power of dominant platforms, making it relevant to consider prior work on regulatory interventions meant to reduce the competitive threat posed by incumbents and dominant firms. Although these studies are often phenomenon- rather than theory-driven, they provide important context for our hypotheses, and are largely consistent with the competitive repositioning framework we outlined above.

Regulation. A rich stream of research has examined how regulatory interventions (including deregulation) influence competition and firm strategy (Delmas, Russo, & Montes-

Sancho, 2007; Gimeno, Chen, Bae, 2006; Madsen & Walker, 2017). This work has examined the consequences of relaxing pricing and entry restrictions in trucking (Madsen & Walker, 2017), removal of mandatory vertical integration in electric utilities (Delmas et al., 2007) and deregulation in airlines (Gimeno et al., 2006), for example. The typical intent of these interventions is to level the competitive playing field by eliminating excess profits, increasing the variety of strategic approaches taken by firms, and encouraging innovation.

There is nascent evidence that regulations meant to foster competition can in fact encourage innovation, although this evidence is not about antitrust specifically. Bloom and colleagues (2019), for example, found that increased competition through regulatory intervention in international trade drove increases in innovation, especially in markets that had little competition to start with (i.e., in early phases of the competition-innovation inverted U; Aghion et al., 2005), although not all empirical studies confirmed this pattern. Similarly, Delmas and colleagues (2007) found that in electric utilities *removal* of regulations that favored incumbents helped stimulate the introduction of green technologies.

Another research stream examines profitability. Although the common objective of regulation is to stimulate greater competition and thus decrease dominant firms' relative performance, empirical results are mixed. Madsen and Walker (2017), for instance, found that, after trucking deregulation, incumbent firms were unexpectedly more profitable than entrants. One possible reason, they proposed, was that existing assets aided repositioning of incumbents while entrants had to build those assets. This suggests that costs of repositioning, in addition to the benefits of newly opened market spaces, may play a major role.

Our focus in this paper, **antitrust regulation,** is meant to prevent specific firms – such as a dominant platform – from exploiting market power.[6] Again, the intent is to expand

[6] While there is no formal market share cutoff for "dominance" in U.S. law, a single firm exceeding 50% market share is a common benchmark used by regulators, particularly when competitor market shares are much lower.

consumer choice, reduce prices, and increase innovation (Pitofsky, 2001). For technology platforms, pricing concerns are less relevant and innovation and the related consumer choice more relevant (Coyle, 2019). The supposition in all antitrust arguments is that dominant firms harm innovation of rivals and should be regulated, but empirical work remains scant.

Antitrust interventions target three broad types of behavior (see Fig. A1 in the appendix). Strategy research on antitrust regulation has mostly focused on the first two categories: collusion between incumbents and M&As where merging firms wield too much combined market power. Mezias and Boyle (2005), for instance, examined a collusive trust among film producers in the United States in the early 20th century and found that colluding firms were slow to adopt key innovations such as feature-length films, a result broadly consistent with the argument that antitrust intervention could aid innovation.[7]

The third type of antitrust intervention, *blocking competition*, is the focus of this paper. This type typically addresses a single firm, such as a dominant platform, that is pushing other competitors out of the market. Dominant platform's ability to "tie" its own complementor products to the platform gives it an unfair advantage in visibility and convenience, thus blocking a range of viable opportunities from competitors (Eisenmann et al., 2011). Research regarding "blocking competition" is sparse, however, focusing mainly on low-tech industries with a traditional "vertical" structure, in contrast to platforms and their complementors in multi-industry ecosystems. As a result, the implications of antitrust interventions against dominant platforms in technology ecosystems are not yet clear.

HYPOTHESES

In the hypotheses that follow, we examine the ability of ecosystem firms to innovate and profit following an antitrust regulatory intervention that aims to weaken the competitive

[7] One recent exception is Kang (2020) who showed that manufacturing firms spent more on R&D and product development *while colluding*. One explanation for the difference could be that Kang focused on *investments* in innovation, not on productivity. Further, because Kang's sample was restricted to manufacturing firms (R&D investments typically target process innovations), implications for technology platforms are likely different.

threat by a dominant platform in the industry. We examine implications for both complementors (H1, H2) and the dominant platform (H3, H4).

Implications for complementors. In H1, we propose that antitrust intervention against a dominant platform drives increases in complementors' innovation. Before intervention, when a dominant platform is free to block competition through anti-competitive tactics, any new innovation introduced by complementors is at high risk of being sidelined in favor of the dominant firm's competing product, even if that product is inferior (Wang & Shaver, 2016; Wen & Zhu, 2019). As a consequence, complementors are constrained in their strategic options. We propose that reducing the power of the dominant platform through antitrust can make investments in the market viable again and encourage pursuit of diverse opportunities. Following the intervention, then, we propose that complementors are more likely to believe they will have a fair chance to compete on the merits of their innovations. Thus, weakening a dominant platform is likely to induce competing firms to innovate in the newly opened space, letting all flowers bloom (Cennamo & Santalo, 2019).

Learning arguments support the reasoning by suggesting an additional mechanism: if weakened competition is perceived as an opportunity, exploration and more flexible thinking is elevated (Chattopadhyay et al., 2001; Staw et al., 1981). Keeping with the arguments from this competition-induced threat and learning view, we expect that complementors' efforts to innovate are likely to include more diverse problem-solving approaches once the threat from the dominant firm has been reduced. Since competitive threat results in restrictions on information processing, such as a narrowing in the field of attention (Staw et al., 1981), we propose that reversing the threat has an opposite effect and is thus likely to broaden and increase the problem-solving approaches that are used. Altogether, we propose that antitrust intervention against a dominant firm is likely to increase innovation by complementors that are now less threatened.

H1. Antitrust intervention against a dominant platform has a positive impact on rival complementors' innovation.

In H2 we propose that antitrust action against a dominant platform pulls down complementor profitability. Prior research suggests a surprising benefit to dominant platforms: they can act as a gatekeeper and provide order to the ecosystem (Wareham et al., 2014). In particular, dominant platforms can reduce wasteful development by steering firms towards opportunities not already covered by others and prevent "excess" competition, leaving enough market space for each complementor (see Helfat & Raubitschek, 2018; Wen & Zhu, 2019). Some research also argues that platform's gatekeeping policies can act as a useful disciplining force, weeding out (low-quality) flowers that may not benefit the ecosystem as a whole (Zhang, Li, & Tong, 2020). Once the threat from the dominant firm is lifted, we propose that "all flowers blooming" becomes more difficult. Restricting the platform owner's ability to tie its own complementor products to the platform is likely to prompt sudden and simultaneous repositioning by many complementor firms seeking to take advantage of new opportunities. This is likely to increase costs and resource changes, add wasteful development that can potentially hurt the ecosystem as a whole, and reduce profit.

Learning arguments similarly suggest unexpected benefits to a dominant firm's "order": firms facing a greater competitive threat are likely to be more cautious and deliberate in their actions, paying special attention to costs and profit margins and streamlining operations (Staw et al., 1981; Gilbert, 2005). Simply put, competitive threat of a dominant firm can impose a discipline that focuses attention on profitability (Barnett & McKendrick, 2004). With the threat of the dominant firm suddenly lifted and replaced with new opportunities following antitrust, we expect that firms are likely to turn to riskier and more experimental strategies while placing less emphasis on efficiency, likely reducing profit. This negative effect may be amplified if firms are pursuing similar opportunities simultaneously, leading to counterproductive racing. We propose:

> *H2. Antitrust intervention against a dominant platform has a negative impact on rival complementors' profitability.*

Implications for the dominant firm. Hypotheses 3-4 examine the implications of the antitrust intervention for the dominant platform. In H3 we propose that after being sanctioned through an antitrust intervention, the platform faces ambiguous, but likely negative, implications for innovation. On the one hand, the ecosystem as a whole now likely becomes more innovative, as we have argued, and it is possible that "a rising tide lifts all boats," now also elevating the platform's chances to innovate (Gawer & Henderson, 2007). On the other hand, in keeping with the competitive repositioning arguments, intervention may make the dominant platform more constrained in the actions it can take to profit from its innovations, thus potentially making investment in R&D and innovation less attractive. It is also likely that the platform's efforts to create more innovation will be less effective given it has been shielded from innovation-inducing competition due to its dominant position (Barnett & McKendrick, 2004), making any repositioning efforts costly in terms of time and learning.

The learning view similarly suggests that when a dominant firm faces stricter regulation, the environment is likely seen as shifting from a relatively controlled opportunity to one that is threatening. Such a threat makes problem-solving rigid and consequently less diverse and creative. Although the arguments are potentially ambiguous, we propose, keeping with the competitive repositioning view:

> *H3. Antitrust intervention against a dominant platform has a negative impact on the dominant platform's innovation.*

In H4 we similarly propose that profit implications of antitrust regulation against the dominant platform are somewhat ambiguous, but likely negative. From the platform perspective, increased competitive repositioning may cause fragmentation in the ecosystem, with possible low-quality or low-value offerings (Gawer & Henderson, 2007). This ecosystem effect is likely particularly significant for the platform firm, which is generally responsible for maintaining quality standards across the ecosystem and may now have to expend more effort to make sure the ecosystem is not flooded by low-quality complementor offerings (Wareham et al., 2014; Cennamo & Santalo, 2019). Weakened control over the ecosystem can also leave the platform itself more vulnerable to competition from other platforms, thereby increasing costs and potentially lowering revenue.

From the learning perspective, however, the profit implications are ambiguous. On the one hand, competitive pressure could push the platform to be more cautious and deliberate in its actions, paying special attention to costs and profit margins and streamlining operations. On the other hand, the platform's costs will likely rise. Prior to antitrust, the dominant firm is free to use tactics like bundling to force customers to buy products together (Eisenmann et al., 2011), leveraging high levels of market power in one market to cross-subsidize other products and increase visibility to customers at the expense of competitors. After antitrust intervention, this becomes less possible for the dominant firm, likely reducing sales volume and profits. Similarly, costs and resource needs would differ now that the dominant firm has to earn profit by competing on the merits of its products. Thus, we hypothesize:

H4. Antitrust intervention against a dominant platform has a negative impact on the dominant platform's profitability.

METHOD

Empirical Context: Enterprise Infrastructure Software

We tested our hypotheses in the enterprise infrastructure software industry between 1998 and 2004, that is, the three years before and after the Microsoft antitrust intervention. At this time, the dominant enterprise platform was Microsoft's Windows Server Operating System ("Windows Server"), which held 51% market share in 2001,[8] and complementors were the

[8] The other server platforms were Linux server operating systems (multiple vendors combining for 23%) and UNIX server operating systems (multiple vendors combining for 11%) (IDC, 2002).

infrastructure applications firms listed in Figure 1.[9] Complementors' infrastructure applications run on the server operating system, covering critical backend IT functions.

Enterprise infrastructure software is a particularly relevant context for several reasons. First, the Microsoft antitrust case allows us to examine *reduced* platform threat, a rarely examined situation. Second, at the time of the antitrust case, infrastructure software was a large and growing industry with total sales of roughly $20 billion in the United States in 2000, ensuring that competitive decisions were strategically important for ecosystem firms. Turnover in the industry was also relatively rare, suggesting that the changes in competition were likely due to antitrust, not new entrants, and that the repositioning costs can be more consistently compared across incumbent complementors. Third, innovation in infrastructure applications focuses on performance and functionality, rather than the more ambiguous features of fame or usability often privileged in user-facing applications, making consistent measures of innovation more available. Finally, relatively short development cycles (1-2 years) enable firms to quickly reposition in response to changes in the competitive environment, making antitrust's imminent effects relatively straightforward to assess.

The 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 standard industry source Gartner Research: *application integration* and *developer tools* help different applications work together, *database management* helps store and manage data, *network & system management* is used to manage the overall IT system, and *security* protects from attacks and manages access to the system within the organization (Gartner, 2001). Notably, Microsoft did not pose a significant competitive threat in every complementor market—holding above 10% market share only in two (database management

[9] In enterprise software, there are two distinct platforms: the user operating system (which runs user-facing applications) and the server operating system (our focus). The user platform was dominated by Microsoft's Windows operating system ("Windows," which held 93% market share in 2001) but also included Linux and Mac OS. Microsoft therefore held a dominant position in both user-facing and server platforms.

and developer tools)—a fact which creates useful variation for our research design.

Regulatory Shock: Microsoft Antitrust Intervention

Our study examines the competitive implications of the *United States v. Microsoft Corp* landmark antitrust case. *U.S. v. Microsoft* is widely seen as the blueprint for the current antitrust inquiries into platform owners such as Google and Apple (Wall Street Journal, 2019). Initially settled in 2001, it was the first major regulatory intervention against a software firm. As Microsoft's "home" regulator, U.S. authorities had a wider and more severe array of antitrust remedies at their disposal than regulators in other jurisdictions, making the U.S. intervention most salient to Microsoft and its complementors.

The government's case was primarily about "threat to future innovation," as is typical of antitrust interventions against dominant technology platforms (Lohr, 2019). The U.S. Department of Justice alleged that Microsoft repeatedly used its monopoly power in central platforms (i.e., Windows Server for infrastructure applications) to block competition from complementors. Prior to the intervention, Microsoft allegedly made it difficult to remove its own complementor applications and would inhibit the functionality of competing applications (e.g., by purposefully making non-Microsoft applications run more slowly or less reliably). The latter problem was particularly salient in infrastructure software, as Microsoft was alleged to have created technical 'backchannels' that would improve integration between their own infrastructure applications and Windows Server while sabotaging competing applications. For example, rival complementor Novell alleged that Microsoft would "*…criticize products offered by Novell and other competitors for technical problems*..." that were caused *"…by Microsoft's refusal to allow effective interoperability"* with Windows Server (Novell, 2002). The 2001 intervention restricted Microsoft in several ways, including making it more difficult to "tie" complementor products to its platforms. The intervention also forced Microsoft to open many of its APIs (application programming interfaces) to other firms in order to facilitate smoother application development by complementors.

Although infrastructure software did not receive as much mainstream attention as user-facing applications (e.g., the "browser war" against Netscape), enterprise infrastructure applications were key to the *United States v. Microsoft Corp* case. Many user-facing application markets were becoming saturated while enterprise infrastructure markets featured more room for growth, making infrastructure software strategically important to Microsoft (Gartner, 2001). The case also attracted numerous complaints from other infrastructure firms such as Novell, alleging that Microsoft had similarly harmed their businesses. Despite the importance of the case to platforms and antitrust regulation, surprisingly little empirical research has focused on the impacts of the intervention.[10]

Sample Construction

We build a dataset of public U.S. firms in the enterprise infrastructure applications ecosystem between 1998 and 2004, starting 3 years prior and ending 3 years after the Microsoft antitrust intervention. We focus on public firms because enterprise infrastructure clients require long-term support – particularly for the core back-end functions managed by infrastructure applications – and are thus reluctant to take a chance on unproven young firms. Furthermore, during our study time frame, enterprise software firms tended to go public relatively early compared to other industries, which allows us to capture the bulk of the industry through public firms (Campbell-Kelly, 2003). We focus on U.S. firms because the U.S. was the biggest market for infrastructure applications at the time (Gartner, 2001) and because the intervention was focused on the U.S.

Because infrastructure software is not distinguished from other types of software in standard industrial classifications, we took several steps (outlined below) to identify the firms that operated in the ecosystem. We also took care to triangulate between multiple sources to

[10] While legal scholars have attempted to evaluate the consequences of specific clauses within the settlement (e.g., Page 2007), empirical work on broader implications for innovation and profitability has been scarce.

improve the coverage and to create a comprehensive dataset. We started by compiling a list of all public software firms in the United States. Consistent with prior work (Lavie, 2007), 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 U.S. After excluding the 322 firms that developed products only for consumers (to focus on enterprise software), we compared the remaining firms' product portfolios with Gartner Research's *IT Glossary*. We classified a firm as an infrastructure software company if the majority of its product portfolio matched the Gartner keywords.[11] We triangulated this information with *The Software Catalog* (an annual listing of software products) and asked two industry experts to review the list.

Of the resulting 102 enterprise infrastructure software firms that were public during 1998-2004, we excluded 18 firms that were not public for *both* the pre- and post-treatment periods. (Results are consistent when including the firms that entered or exited in the study timeframe.) Because going public is a milestone event that encourages focus on immediate profitability (Bernstein, 2015) rather than innovation, limiting the sample to firms that were public before and after treatment helps control for alternative mechanisms that might change both innovation and profitability. We also required all firms to operate in the industry during the entire study time frame. Entry of new competitors into the industry was rare after the intervention (only four public firms entered between 2002 and 2004), and there was little movement across segments (affecting only 1.2% of observations). Finally, we excluded four firms that were platform owners themselves to focus on the implications for purely complementor firms.[12] Our final sample consists of 78 infrastructure applications firms. Sample firms exhibited typical regional patterns for software, with 32% headquartered in the San Francisco Bay Area, 10% in the Los Angeles area, and 9% in the Boston area.

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

Research design

To test the effects of antitrust, we exploit variation in the competitive threat posed by Microsoft across infrastructure applications (i.e., complementor) markets. As a treated group (labeled *rival complementors*), we used firms in complementor markets where Microsoft had a strong market position and thus where the antitrust intervention represented a significant reduction in platform owner's competitive threat. In contrast, in control markets where Microsoft had only a marginal market presence, a weakened Microsoft would not significantly change the competitive environment, making them relevant counterfactuals.

At the time of the case, infrastructure application markets were dominated by 3-5 leading firms with roughly 10-20% market share each (Gartner, 2001). We therefore measured treatment as competing in infrastructure applications (i.e., complementor) markets where Microsoft held at least 10% market share at the time of the intervention. Microsoft was the #3 vendor in database management (15% market share) and #2 vendor in development tools (17% market share). Firms in these two markets are the treated group. In contrast, Microsoft held a substantially weaker position in application integration (4% market share), network & system management (2% market share), and security (<1% market share). Firms in those three markets are the control group. Using Gartner's ranking of top 5 firms in each market as an alternative cut-off produced consistent results.

Causal inference

In an ideal world, we would randomly assign complementors to markets where Microsoft was a competitive threat vs not, and observe the differences in innovation and profitability after the antitrust intervention. Using observational data, we need to account for possible selection bias that could arise from unobserved factors associated with competing in specific markets and with firm performance. Complementors may have, for example, avoided markets where Microsoft was a threat because they did not feel they had the resources to compete. Alternatively, firms may have selected into Microsoft's markets because they were following the lead of the platform firm. Our results would thus be driven by differences in firm characteristics and not competitive repositioning after the antitrust intervention. We address this possible bias with difference-in-differences, matching, and synthetic controls.

Difference in differences. 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 have parallel trends prior to treatment. We first calculate the differences in outcomes for treated firms and counterfactual "control" firms before versus after treatment and then the difference in those two numbers (Bertrand, Duflo, & Mullainathan, 2004). Formally,

$$DV_{it} = \beta_0 + \beta_1 RivalComplementor_{it} + \beta_2 PostIntervention_t + \beta_3 PostIntervention_t \times RivalComplementor_{it} + C_{it}\lambda + \alpha_i + u_{it} + \epsilon_{it}$$

where C_{it} represents control variables, α_i is the firm fixed effect, u_{it} is the year fixed effect, and ϵ_{it} is the error term. β_3 is the coefficient on the difference-in-difference estimator, which captures the effect of the antitrust intervention on firms facing a significant competitive threat from Microsoft (treated firms).

Consistent with prior work, our estimation period is three years before and three years after the 2001 intervention (Flammer, 2015). However, results are robust to five-year windows (1996-2006) or to extending the post-treatment window to ten years (1998-2011).

Matching. We use *matching* to further strengthen causal inference. Matching reduces the impact of confounding variables by eliminating statistical differences between treatment and control groups along key observable attributes (Rubin, 1997). Matching thus ensures that the treated firms and control firms are as similar as possible prior to treatment. We match on *geographical region*, *years public*, *firm size*, *R&D intensity*, and *pre-sample patents* (when predicting innovation) or *pre-sample profits* (when predicting profitability).[13] Matching

[13] Pre-sample patents and pre-sample profitability 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.

criteria are explained further in the online appendix.

We employed propensity score matching (PSM) and verified robustness using alternative methods including coarsened exact matching (CEM) and exact matching. As de Figueiredo, Feldman, and Rawley (2019) note, in samples with multiple confounders, PSM is preferred because it remains stable even as the number of potential confounders increases, reduces researcher's subjective assessments about "similarity," and increases the number of treated firms that can be matched (thus reducing the data that need to be thrown away). To reduce the likelihood of poor-quality matches, we eliminate extreme values of the propensity score by trimming the top and bottom 5% from the sample (Athey & Imbens 2017). Other cut-offs yield consistent results. Treated firms are matched without replacement (i.e., control firms are not re-used). The matched sample consists of 376 firm-years (66 firms).

To assess the quality of our matching, we followed standard practice and compared the distribution of key variables before versus after matching (Flammer, 2015) in Table 1. After matching, the control group is similar to the treated, particularly along the matching criteria. Note that we control for firm heterogeneity with firm fixed effects in all models.

Data Sources

We collected *firm financial data* from Compustat, supplemented by SEC filings and CapitalIQ. We collected *patent data* using the NBER patent database (Hall, Jaffe, and Trajtenberg, 2001) and the USPTO database. We also collected *firm characteristics,* such as region and year of IPO, from Compustat. We classified firms into complementor markets using *firm press releases* and categories from Gartner Research (Pontikes 2012). For sensitivity analyses, we collected product data through *firm press releases* (Thatchenkery & Katila, 2021). Finally, we collected qualitative data from press coverage and *court documents,* retrieved from Factiva, LexisNexis, and U.S. government archives.

Measures

Dependent variables. We measured *firm innovation* as an annual citation-weighted patent count. We count patents in the year in which the application was filed. Since later patents have less time to accumulate citations, we multiply citations by an adjustment factor using the NBER data (Hall et al., 2001). Patents are a particularly appropriate measure of innovation for our analysis because by definition each granted patent is novel, non-obvious and useful (Walker, 1995). Because each patent describes a problem and a solution to that problem (Katila, 2002), each patent that is added to the industry's knowledge base thus appropriately captures the regulator's intent to add new ways of problem-solving to industry. Patenting is also standard in software during our study time 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 US Court of Appeals *State Street* decision. The Supreme Court's *Alice Corp* decision in 2014 reversed this trend introducing stricter criteria. Patents are thus a particularly relevant measure of software innovation during our study time period. As a robustness check, we also used *unweighted patent counts* and *R&D investment* as dependent variables, with consistent results (available from the authors).

Consistent with prior work on competition in the software industry, we measured *firm profitability* as return on sales annually (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 in order to continuously grow their business (Suarez et al., 2013). To allow comparisons with prior work (Madsen & Walker, 2017), we also measure profitability as gross profit (logged) and superior profits (ROS above the industry average), with consistent results.

Independent variables. Because we run difference-in-differences, the explanatory variable of interest is an interaction between treatment group and post-treatment time period. We measured *rival complementor* (treatment group) as a binary indicator set to 1 if the firm competed in at least one complementor market in which Microsoft was a significant competitive threat pre antitrust.[14] We ran an alternate analysis in which treated firms are counted as treated only if they compete exclusively in these high-threat markets, with consistent results. We measured *Post-intervention* (post-treatment period) as a binary indicator set to 1 if the year was 2002-2004.

Controls. We included several controls. We controlled for *firm size* as annual revenue, adjusted for inflation and logged to correct for skew, because larger firms may have resource advantages that can boost innovation and profit. We also measured size as *number of employees*, with consistent results. 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 sales annually. Results using *R&D expenditures* were consistent.

Because firms that have been public for longer may face increased pressure to prioritize short-term financial returns, potentially at the expense of innovation (Bernstein, 2015), we controlled for *years public* as the logged number of years since the firm's IPO.

We controlled for *firm acquisitions*, measured as a yearly count, because acquisitions by the focal firm may increase costs related to integration (Kim & Finkelstein, 2009) and give the firm more "raw material" for innovation (Ahuja & Katila, 2001).

We controlled for any unobserved market effects with fixed effects for each of the five *complementor markets*. We controlled for three *geographic regions* with high numbers of enterprise software firms, namely, San Francisco, Boston, and Los Angeles (Orange County) because knowledge spillovers within a region can enhance innovation (Owen-Smith

[14] As noted above, Microsoft held 15% and 17% market share in the two treated complementor markets (database management, development tools), and 4%, 2%, and <1% market share in the three control markets (application integration, network & system management, security).

& Powell, 2004). Region effects drop out from fixed effects regressions but are included in random effects regressions. Macroeconomic variation was controlled with *year effects.*

Statistical Method

Difference-in-differences. We ran a difference-in-differences analysis using panel regressions with firm fixed effects. 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, we used *fixed effects Poisson regressions* because our outcome is a count variable (results are also robust to *negative binomial* regressions). Because non-patenting firms do not exhibit variation in the dependent variable and are thus automatically dropped in fixed effects regressions, we also report *zero-inflated* and *random effects* Poisson regressions, with consistent results. For our models predicting profitability, we used *generalized least squares fixed effects regression*. In keeping with best practice to avoid collinearity (Friebel et al., 2017), we include only the *Post-Intervention x Rival Complementor* interaction in fixed effects regressions. Results are robust to including the "main effects" for post-intervention and rival complementors (available from the authors).[15] OLS results had a consistent pattern.

To investigate possible omitted variable bias, we calculated the impact threshold for a confounding variable (ITCV) (Frank, 2000). ITCV indicates how many observations in our data would need to be influenced by an omitted variable to overturn the results. ITCV indicates that omitted variables would need to drive the outcome in 42% of cases for innovation and 35% of cases for profitability. Given that we control for time-invariant heterogeneity with firm effects and use difference-in-differences to further reduce selection bias, it seems unlikely that an omitted variable would cross these thresholds.

[15] We also test the robustness of our results to an ANCOVA estimator (Friebel et al., 2017), in which we limit observations to only the post-treatment period, control for the pre-treatment level of the dependent variable, and examine the effect of rival complementor. Results for this ANCOVA estimator are consistent.

Synthetic controls. To test our hypotheses regarding implications for Microsoft, we used the synthetic control method, a variation on difference-in-differences that is particularly useful in situations with only one treated firm, especially when the firm is somewhat of an outlier for which counterfactual controls are not readily available (Fremeth, Holburn, & Richter, 2016). Synthetic control is created by a weighted average of actual control firms to better approximate the characteristics of the treated firm (Abadie & Gardeazabal 2003). If the synthetic control closely matches the observed data in the pre-treatment period, then divergence between the synthetic control and the actual treated firm post-treatment may be attributed to the treatment (Abadie & Gardeazabal 2003). Divergence is assessed *visually* through graphs comparing the estimated outcomes for the actual treated firm(s) and the synthetic control(s) (Fremeth et al., 2016).

We construct the Microsoft synthetic control out of other technology platform firms that were not subject to antitrust inquiry at the time and also competed with complementors within their own ecosystems. We further match on size and R&D expenditures (consistent with the matching done for complementors). Synthetic controls require a strong match in the outcome prior to treatment, so the exact firms used in each synthetic control are different for each dependent variable. Our synthetic control is constructed out of Oracle, Cisco, Dell, HP, and Intel for profitability and Cisco, Intel, and Sun Microsystems for innovation.[16]

We also use the synthetic control method as a robustness check for our hypotheses about complementor infrastructure applications firms, as reported below.

RESULTS

Descriptive statistics are reported in Table 2. Complementor firms in infrastructure software produced six unweighted patents per year on average. Average return on sales

[16] Intel entered into a settlement with the FTC in 1999 regarding alleged improper behavior in IP litigation. While sometimes referred to as an "antitrust" suit, it was not about blocking competition. No other firms used in synthetic Microsoft were subject to notable regulatory action during the sample time frame.

during the study time period (1998-2004) is -0.32, though the standard deviation is quite large (2.09). Correlations among independent variables are mostly low and variance inflation factors (VIFs) are mostly under the recommended cutoff of 5, with the exception of years public (VIF of 9.36). Results are robust to removing that variable. Because VIFs are a sufficient but not a necessary indicator for multicollinearity, we also randomly estimated subsets of the estimation sample by dropping one year at a time (Echambadi et al., 2006) with results that indicated that coefficients were stable.

Regression analysis (H1, H2)

We first test implications for complementors. We used the dqd command in Stata to evaluate the parallel trends assumption for difference-in-differences (Mora & Reggio, 2015). The p-value for innovation is 0.52 and the p-value for profitability is 0.81, supporting the null hypothesis of parallel paths prior to treatment. Visual inspection of innovation and profitability before and after 2001 lends further supporting evidence (see Figures 2a and 2b).

Table 3 reports difference-in-differences results. Hypothesis 1 predicted that complementors facing high competitive threat from the platform would be more innovative after the antitrust intervention. Models 1-4 report Poisson results. Model 1 reports results for control variables and Model 2 adds the difference-in-differences estimator, which is positive and significant (p<.001). Firms facing high platform threat introduced an average of 14.66 *more* patents (citation-weighted) per year compared to control firms, supporting hypothesis 1.

Hypothesis 2 predicted that profitability for complementors facing high platform threat would drop after the intervention. Models 5-7 report GLS results. Model 5 includes control variables and Model 6 adds the difference-in-differences estimator, which is negative and significant (p=.026). Firms facing high platform threat experienced an average drop of 9.5 percentage points in ROS compared to control firms, supporting hypothesis 2.

Random effects (Models 3 and 7), zero-inflated (Model 4) and synthetic control (appendix, figures A2-A3) results are consistent. Results are also consistent to extending the observation window to 10 years post the intervention (until 2011).

Robustness checks on regression analyses (H1, H2)

Validation of research design. We ran several tests to validate our research design, particularly the relevance of our treatment and selection of treated (versus control) firms.

Relevance of intervention. We examined whether the Microsoft antitrust intervention did in fact reduce the competitive threat posed by Microsoft and constrained its actions vis-à-vis complementors. Bill Gates, Microsoft's CEO at the time, repeatedly stated that "*there's no doubt the antitrust lawsuit was bad for Microsoft*" (Novet, 2019), claiming it prevented the company from vigorously pursuing new opportunities in the market. Observers also noted a change in Microsoft's behavior in complementor markets, namely, that it "*made Microsoft's leaders skittish about bundling*" (Rivkin & Van Den Steen, 2009). In the server operating system market (the core platform for our setting), Microsoft's growth stalled, with yearly market research estimates ranging between 45-60% market share in server operating systems, a sharp contrast to its 95% market share in user operating systems (IDC, 2019). Thus, there is concrete evidence that Microsoft was constrained following the intervention.

Selection of treated firms. A difference-in-differences design necessitates a binary treatment variable and may be sensitive to the cutoff selected (10% market share or top five rank). We first note that the two "treated" markets were ones in which Microsoft was ranked #2 and #3 (with 17% and 15% market share, respectively), so the results are robust to a top three or top four treatment cutoff or to lower market share thresholds (e.g. 5%). We also tested a continuous treatment variable, *dominant platform market share*, measured as Microsoft's market share in each of the complementor's markets (averaged) at the time of the intervention. Results were consistent. Our results are thus not dependent on the exact cutoff for our treatment variable.

Following Mahmood and colleagues (2017), we ran placebo tests to further examine the validity of the treatment criteria. In the first test, we randomized assignment to treatment for each firm in our sample. The results disappear after randomization, strengthening validity of the treatment criteria (available from authors). The second placebo test probed the timing of treatment with lead and lag regressions (Seamans & Zhu, 2017). We ran the models with alternative time indicator variables: 1) one year prior, 2) the year of, 3) the year after, and 4) two or more years after the intervention, and interacted each with our treatment indicator. As expected, we see a negative coefficient on profitability and more strongly positive coefficient on innovation only *after* the intervention (results available from authors).

Because we included firm fixed effects in a difference-in-differences analysis, we probed the sensitivity of our results to serial correlation. We collapsed our multi-year panel into a panel of two, with one observation (containing the three-year-average of all variables) prior to treatment and one observation after the treatment (Bertrand et al. 2004). Results are consistent (available from authors).

Mechanism tests. We ran several tests to further probe our proposed theoretical mechanisms for innovation and profitability.

Innovation: All flowers bloom? We argued that complementor firms saw increased innovation because they were able to pursue more varied opportunities as they repositioned in the recently opened space. To test this mechanism, we examined changes in *patent class diversity* before and after the intervention, measured by a Blau index. If diversity of problem-solving approaches used by firms changed as a consequence of antitrust, we would expect a change in patent class diversity. This is indeed what we saw. Patent diversity dropped for control firms but slightly increased for treated firms, suggesting that treated firms pursued a broader range of technological opportunities post antitrust. Results for *patent generality,* measured as the diversity of forward citations to a patent (Hall et al. 2001), are consistent.

One alternative mechanism would be that antitrust encourages rival complementors to quickly file a large number of patent applications (because they no longer face a strong IP threat from the dominant firm). So rather than presenting truly new ideas and innovation, as regulators intended, complementors may produce a high volume of low-quality innovations. This seems a less likely mechanism, as our results are consistent to using both citation-weighted and unweighted patent counts. If the new alternatives post-intervention were low-quality, we would expect to see our results supported by non-weighted counts but diverge for the citation-weighted counts that measure quality of patents. Since they do not, a deluge of patent applications is a less likely, and innovative new approaches a more likely, explanation.

Profitability: Wasteful development. If our theorized mechanisms of wasteful development and lower efficiency are driving our profitability results, we would expect to see *lower sales* (because products are not valued by customers despite their technical merits) and *greater costs* (because of reduced efficiency) among treated firms compared to control firms. Our data lend support to this mechanism. We find that treated firms experience lower sales growth than control firms after antitrust whereas R&D expenditures increase for the treated firms, providing support for our theorized mechanisms. Interestingly, non-R&D expenditures (e.g., operational, administrative costs) remained roughly equivalent, and, including a control for firm's *downstream capabilities* (Ceccagnoli et al., 2012) (measured as pre-sample products) does not change the results. Overall, our results suggest that complementors that had faced higher competitive threat from Microsoft boosted innovation after antitrust but were not able to benefit through sales and enlarge the pie.

To further test the mechanism of wasteful duplication, we examined changes in *commercialization of innovation,* measured as new products. Complementors introduced 3,684 new products during the study time period. The wasteful development argument suggests we should find **no** incentive for firms to turn their technical ideas into products in a crowded market. This prediction is supported, as there is no effect of the intervention if we replace firm patents with firm new products as the dependent variable in our difference-in-difference analysis (available from authors) or if we simply compare average new product introductions for each group before and after the intervention. Positive results on technical (patent) but not commercial (product) innovation indicate that firms did not proceed to profit from their newly patented ideas, providing support for the wasteful development mechanism.

Alternative explanations. We also test alternative explanations for our results. First, could our results be influenced by multimarket contact (MMC) by control (but not by treated) firms? Multimarket theory argues that greater overlap between firms *reduces* competitive intensity and could lift profits (Gimeno, 1999). Although most complementors compete in 1-2 markets, making MMC less of a concern, we 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 are consistent with our main analysis reducing the concern.

Second, could our results be driven by the dot-com crash in the year 2000? This does not seem a likely explanation. As shown in figures 2-3, *both* the treatment and control groups are impacted by the crash in 2000, and their divergence begins specifically after the intervention (year 0), not after the crash (year -1). If the crash were an explanation, we would also expect to see similar recovery for both treated and control firms, but this is not the case. In particular, we would not expect the control firms to recover faster than treated firms.

Could Microsoft simply have chosen “better” complementor markets to compete in, thus explaining the higher innovation of treated firms? This explanation again seems unlikely. As noted above, Microsoft was in all market segments at the time of the intervention, but simply had not achieved a traceable (not even 5%) market share in control markets. We would also not expect a sharp change in the baseline “potential” of markets in which Microsoft poses a high versus a low threat to coincide with the timing of the intervention if the selection explanation were correct. We also controlled for "*market crowdedness*" with a count of competitors in each market yearly, in addition to market fixed effects, in case Microsoft had picked high-growth markets. Results were again highly consistent. Overall, it seems unlikely that selection to markets would drive our results.

Synthetic controls analysis (H3, H4)

While our primary focus is on complementors, we also examined the implications of antitrust for Microsoft as the dominant platform to test hypotheses 3 and 4. As noted above, synthetic control analyses are reported using visualizations. H3 argued that antitrust intervention would decrease Microsoft's innovation. Figure 4 shows that Microsoft's innovation decreased immediately following the intervention but later increased above the synthetic control. Overall evidence is thus mixed. It is possible that after an initial dip in innovation, over time a "rising tide lifts all boats" and the platform begins to benefit from these ecosystem effects. We return to these unexpected effects in the Discussion.

H4 argued that Microsoft's profitability would decrease following the antitrust intervention. As shown in Figure 5, Microsoft's profitability stagnated while the synthetic control's profitability exhibited a positive trend, supporting hypothesis 4.[17] Robustness tests are reported in the appendix.

DISCUSSION

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

We started the paper with the observation that, while there is increasing political will around the world for antitrust intervention against technology platforms, the implications of

[17] Results should be interpreted with caution since visual inspection indicates that the synthetic control does not align with Microsoft's pre-treatment performance. However, the synthetic control does exhibit parallel trends and can therefore still be informative (Doudchenko & Imbens, 2016).

such interventions are not fully understood. In this study, we examined how regulatory intervention against a dominant platform owner – Microsoft – was related to changes in innovation and profitability in enterprise infrastructure software. Put simply, we find that following antitrust intervention, patenting goes up but profits go down.

Contributions

We contribute to research on competitive repositioning by examining the implications of a *reduced* threat (rather than the more commonly studied increased threat) from a dominant firm. We find that reduced threat increases innovation. In particular, innovation activities become more attractive when the dominant firm is less able to block competition from complementor innovations. However, our findings also suggest that there are drawbacks to weakening a dominant firm. In particular, profitability suffers. With a dominant firm weakened, other firms may become less attentive to efficiency and pursue riskier, more experimental strategies. With many firms making this shift at once, competitive intensity is increased and wasteful development may occur. In this way, dominant platforms may impose unexpectedly beneficial "order" to their ecosystems. Thus, while the reduced platform threat has positive implications for innovation, this may not translate into increased profit.

Overall, our findings suggest that weakening a dominant firm can create a "wild west." Firms may rush to innovate in order to capture newly viable opportunities but lack the discipline to select the right ones, perhaps pursuing low-value opportunities and finding it harder to actually profit. Thus, while competition adds diversity in terms of how problems are approached, it also hurts financial performance through wasteful effort.

Our findings also have implications for platform ecosystems research (Helfat & Raubitsechk, 2018; Kapoor & Agarwal, 2017). Prior work has presented a tradeoff between stability and evolvability of an ecosystem (Wareham et al, 2014). Our findings suggest that maximizing innovation across an ecosystem may come at the expense of profitability for the platform and its rival complementors. Notably, the negative results on profitability (and positive results on innovation) hold even when extending our post-treatment window to 2011, ten years following the intervention. Extending prior work that has emphasized a balance between generativity and platform or complementor performance (Cennamo & Santalo, 2019; Wareham et al., 2014), we suggest that the innovation benefits of increased innovation and competition within an ecosystem should be balanced against the potential drawbacks in terms of profitability and, potentially, the long-term viability of the ecosystem.

There are also implications for research on regulation. Prior work has focused on less technology-intensive industries with a more traditional structure, yielding mixed results that cast doubt on whether regulators can stimulate competition (Delmas et al., 2007; Madsen & Walker, 2017; Kang, 2020). We examine the implications of a landmark regulatory intervention in a technology-intensive, platform-based industry and find that regulation can prompt increases in innovation. There could therefore be a meaningful social benefit to regulating dominant technology platforms in the name of increased innovation. As the U.S. Department of Justice noted in its recent suit against Google, "*If the government does not enforce the antitrust laws to enable competition, we could lose the next wave of innovation.*"

Our study also provides insights to policymakers. The heavy focus on Netscape and the browser wars in mainstream coverage of the Microsoft intervention led some to write off the antitrust intervention as ineffectual, since Netscape did not survive and Microsoft maintained dominance in user-facing software. Yet we demonstrate a robust positive relationship between the intervention and complementor innovation in backend infrastructure software. Moreover, while the negative association between the intervention and profitability may be of concern to complementor firms, regulators are primarily focused on innovation and consumer welfare (Coyle, 2019). Since greater innovation and more intense competition benefits customers, we find evidence of a tangible social benefit to antitrust enforcement against dominant platforms, even if firm profitability decreases and some of the firms the intervention is meant to help may not survive.

CONCLUSION

Protecting competition and fostering innovation is central to many technology ecosystems. Regulators' efforts to curb the power of dominant platforms seems to boost innovation as intended but may put ecosystem profitability in jeopardy. This suggests a trade-off in which the benefits to discipline that the dominant firm had previously imposed on the industry should be balanced against the benefits of increased innovation.

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Table 1. Comparison of sample means for treated vs. control groups

	Full Sample (N=416)			Propensity Score Matching (N=376)		
	Treated	Control	T-Stat	Treated	Control	T-Stat
Variables	**1**	**2**	**3**	**4**	**5**	**6**
Matching criteria						
Firm size (logged)	0.65	0.47	-2.77	0.53	0.49	-0.81
	(0.71)	(0.62)		(0.47)	(0.46)	
Firm years public	7.01	6.27	-1.77	6.45	6.42	-0.09
	(4.82)	(3.71)		(4.13)	(3.67)	
Geographic region: California[1]	0.44	0.39	-1.01	0.45	0.44	-0.26
	(0.50)	(0.50)		(0.50)	(0.50)	
Firm R&D intensity	0.36	0.41	0.54	0.37	0.41	0.35
	(1.09)	(0.81)		(0.86)	(1.09)	

[1] Firms were matched by state. We include California in the table because it contained the largest proportion of sample firms (42%
Distributions of firms among other states in the control and treatment groups were also comparable.

Table 2. Descriptive statistics and correlations

	Variable	Mean	SD	1	2	3	4	5	6	7
1	Firm innovation	26.73	69.10							
2	Firm profitability	-0.30	2.35	0.04						
3	Rival complementor	0.55	0.50	-0.09	-0.03					
4	Post-intervention	0.33	0.47	-0.01	-0.03	0.01				
5	Firm size (logged)	0.62	0.64	0.49	0.05	0.10	0.13			
6	Firm R&D intensity	0.39	0.99	-0.06	0.33	-0.06	-0.05	-0.17		
7	Firm years public	6.15	4.03	0.22	-0.01	-0.002	0.23	0.42	-0.12	
8	Firm acquisitions	0.05	0.23	0.19	0.01	-0.01	0.01	0.32	-0.04	0.19

376 firm-years

Correlations above .10 are significant at p<.05

Table 3. Difference-in-differences using propensity score matching predicting complementor innovation and profitability

	Poisson				Generalized Least Squares		
	Innovation (Citation-Weighted Patents)				Profitability (Return on Sales)		
	Controls Only (FE)	Full Model (FE)	Random Effects	Zero-Inflated	Controls Only (FE)	Full Model (FE)	Random Effects
	1	2	3	4	5	6	7
Rival complementor			-1.25				-0.001
			(0.000)				(0.77)
Rival complementor x Post-intervention		0.47	0.49	0.47		-0.70	-0.53
		(0.000)	(0.000)	(0.000)		(0.02)	(0.000)
Controls							
Firm size (logged)	1.66	1.68	1.63	1.69	0.05	-0.10	0.29
	(0.000)	(0.000)	(0.000)	(0.000)	(0.86)	(0.77)	(0.004)
Firm R&D intensity	1.13	0.99	0.01	1.46	-0.02	-0.02	-0.03
	(0.000)	(0.000)	(0.83)	(0.000)	(0.000)	(0.000)	(0.000)
Firm years public	-0.91	-1.06	-1.08	-1.07	0.35	0.44	0.26
	(0.000)	(0.000)	(0.000)	(0.000)	(0.23)	(0.14)	(0.002)
Firm acquisitions	-0.31	-0.32	-0.35	-0.32	0.23	0.21	0.09
	(0.000)	(0.000)	(0.000)	(0.000)	(0.30)	(0.26)	(0.54)
Firm fixed effects	Y	Y	N	Y	Y	Y	N
Market fixed effects	Y	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y	Y
Region fixed effects	N	N	Y	N	N	N	Y

376 firm-years. P-values in parentheses.

Figure 1. Structure of Enterprise Infrastructure Software

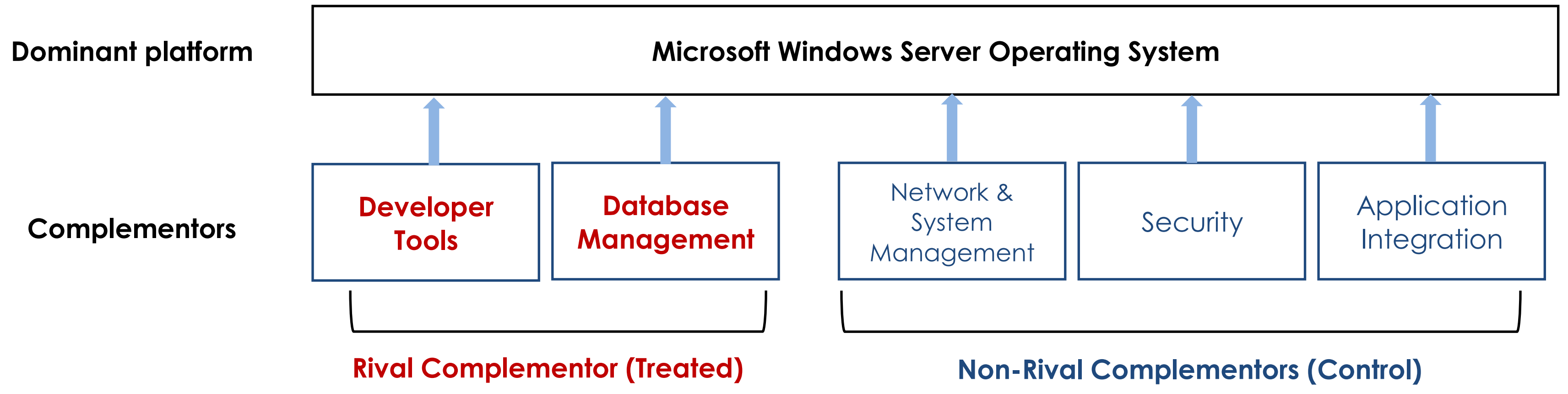


Figure 2. Difference-in-differences analysis: Complementor innovation (H1)

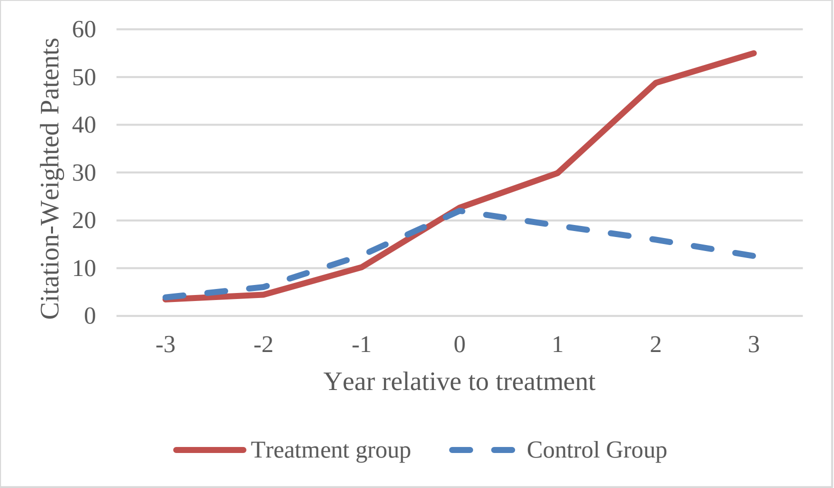


Figure 3. Difference-in-differences analysis: Complementor profitability (H2)

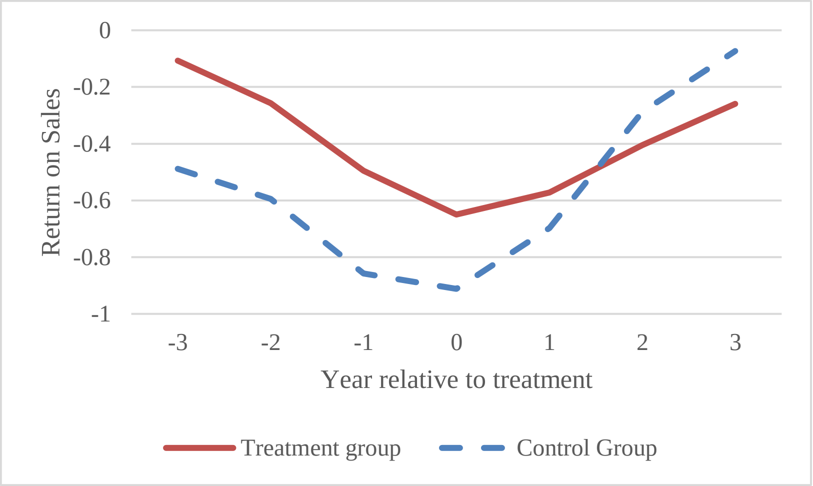


Figure 4. Synthetic control analysis: Microsoft innovation (H3)

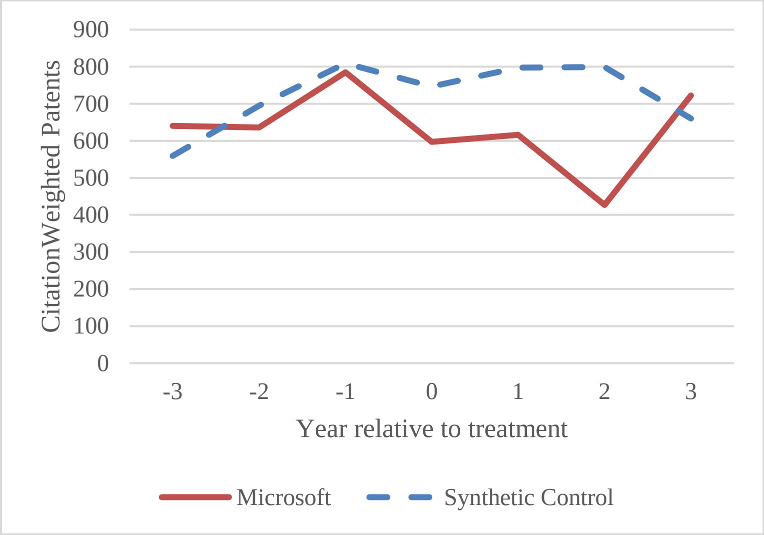
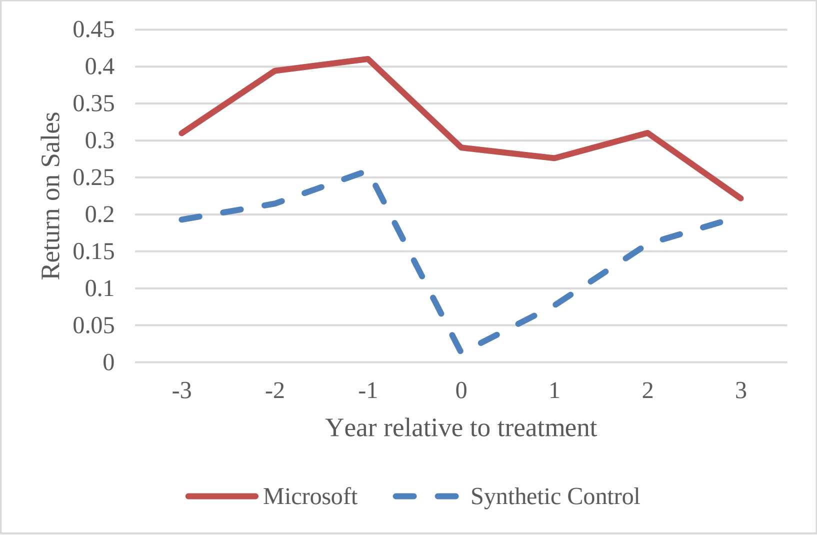


Figure 5. Synthetic control analysis: Microsoft profitability (H4)



ONLINE APPENDIX

Matching criteria (all matching methods)

Geographical region. Prior work indicates that regional clustering can influence both the product markets in which firms compete and firms' innovation and profitability (Owen-Smith & Powell, 2004). We therefore matched our treated firms with control firms that were located in the same state.

IPO year. Our interviewees confirmed that going public can influence a firm's decisions about product markets in which to compete, such as pushing firms to broaden product lines in order to grow. Research also finds that going public and the length of time since IPO drive down a firm's innovation and increase focus on short-term profitability (Bernstein, 2015). We therefore matched our treated firms to control firms that went public in the same year.

Firm size. Prior work indicates that firm size can influence decisions about product markets in which to compete as well as the firm's innovation and performance (Barnett and McKendrick, 2004). Larger firms typically have greater slack resources, which should make them more flexible to compete across more product markets and innovate. We matched treated to control firms on firm size measured by annual revenue, logged and adjusted for inflation.

R&D expenditures. Greater investment in R&D is likely to influence innovation but also represent an increased cost for the firm, which can potentially influence profit. Some product markets may also be more R&D intensive than others. We therefore matched our treated firms to control firms on annual R&D expenditures, logged and adjusted for inflation.

Pre-sample innovation. Because firms may differ in their baseline propensity to innovate, we follow prior work (Blundell et al. 2002) and match on the firm's total patents in the two years prior to the start of the study time period.

Pre-sample profitability. Because firms differ in their past performance, for our models predicting profitability, we match on the average return on sales in the two years prior to the start of the study time period. Because both presample measures are highly correlated with firm size, they are used as an alternative matching criteria only.

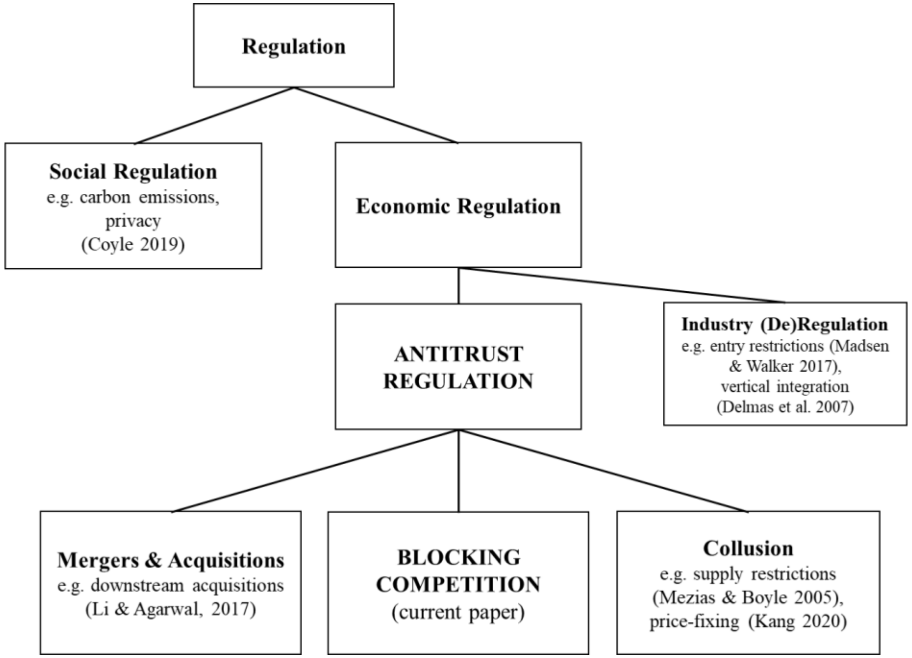
Robustness: Complementor firm analyses using exact matching

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). Regression results are highly consistent with our main PSM results.

Robustness: Microsoft profitability synthetic control

Following prior work (Fremeth et al., 2016), we test the robustness of our Microsoft profitability (H4) synthetic control results using *placebo* tests. We first test whether results hold with a false treatment year. Figures A4-A5 show the results when using 1998 or 2003 as the treatment year, where as expected, no clear pattern emerges. We also run placebo tests using non-Microsoft platform firms that were not subject to antitrust treatment as the treated firm. Figures A6-A7 show two examples (Sun Microsystems and Intel) where again no clear pattern emerges, further supporting our main results for hypothesis 4.

Figure A1. Types of regulation



Adapted from MacGregor and Madsen, 2018. Capitalized denotes the focus of current paper.

Figure A2. Synthetic control analysis: Complementor innovation (H1)

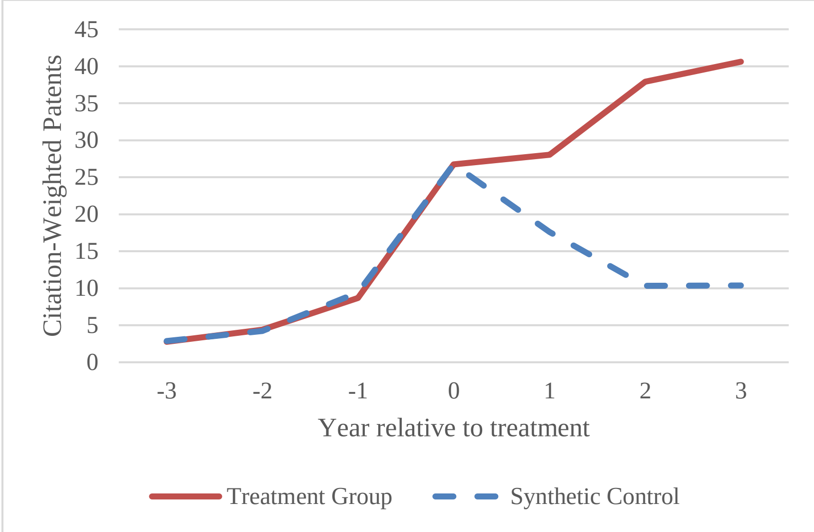


Figure A3 Synthetic control analysis: Complementor profitability (H2)

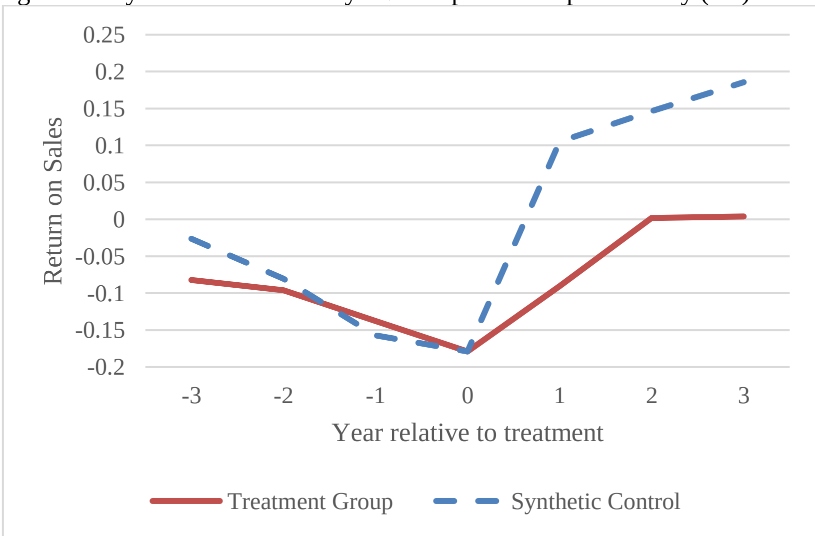


Figure A4. Synthetic control robustness check: Microsoft profitability (H4), **Placebo** treatment year (1998)

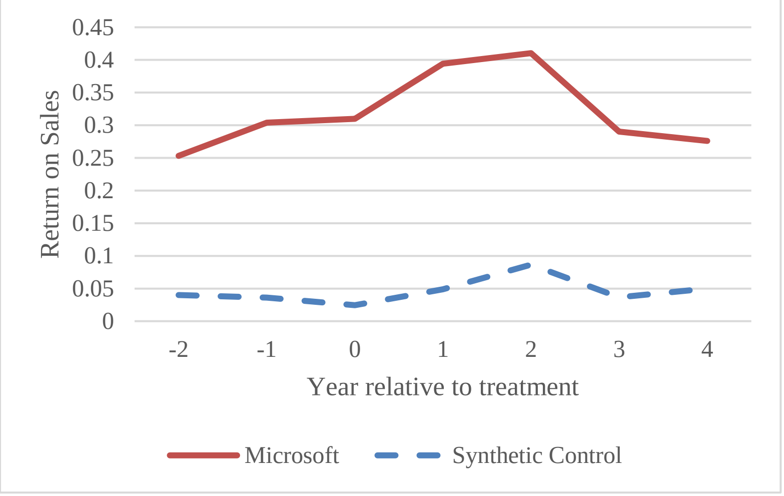


Figure A5. Synthetic control robustness check: Microsoft profitability (H4), **Placebo** treatment year (2003)

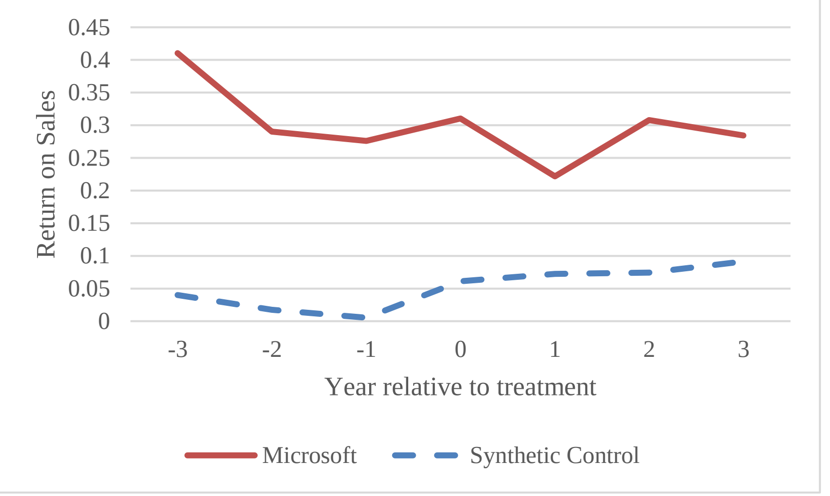


Figure A6. Synthetic control robustness check: Microsoft profitability (H4), **Placebo** treated firm (Sun Microsystems)

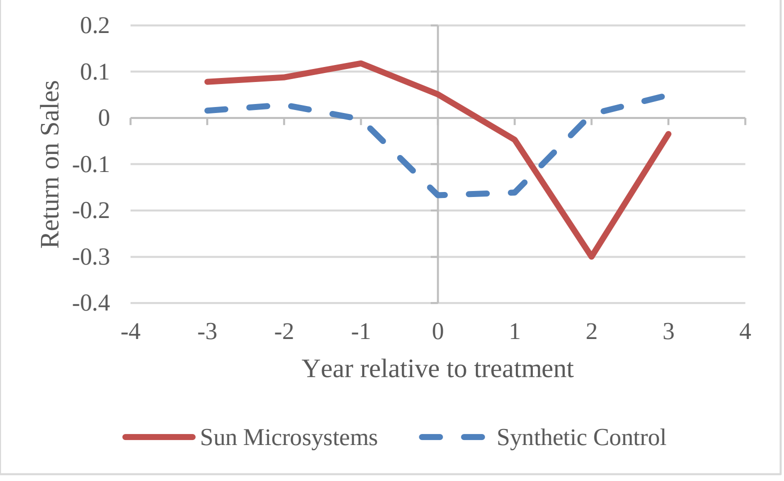


Figure A7. Synthetic control robustness check: Microsoft profitability (H4), **Placebo** treated firm (Intel)