Competition in the Academic Publishing Market

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Es lebe die Freiheit

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Introduction

RESEARCH institutions around the globe have formed purchasing consortia to negotiate with the academic publishers large-scale contracts that make open-access publishing the default in their subscription-based academic journals. The so-called 'transformative agreements' aim to transform the universities' payments for subscriptions to journals into payments for publishing their researchers' work open access. Accordingly, such contracts usually make every publication from an eligible institution open access by default, without financial or administrative burdens for the researchers. The university libraries process the costs conveniently in the background. Hence, researchers benefit from frictionless and effortless open access, often in established journals.

In this dissertation, I study the economics behind these transformative agreements. I investigate their competition implications as well as competition in the academic publishing market more broadly. I focus on the transformative agreements between the publishers Springer Nature and John Wiley & Sons with the alliance of German research institutions, called 'DEAL,' but my findings are generally applicable.

In Chapter 1, which has been published in *Managerial and Decision Economics* and coauthored with Justus Haucap and Nima Moshgbar, we investigate how the introduction of the 'DEAL' contracts changed the publication behavior of researchers at German institutions in the discipline of chemistry. Using a causal difference-in-differences design, we can empirically show that the likelihood of a paper (co)authored by eligible researchers appearing in a journal covered by the DEAL has significantly increased. Decomposing the effect across the journal reputation range, we find that those assigned with the highest and lowest reputation do not benefit from the DEAL. In contrast, mid-tier journals face a significant shift from researchers based in Germany towards them.

In Chapter 2, which has been published in *Research Policy* and coauthored with Justus Haucap and Leon Knoke, we study the gender differences in academic publication behavior. We further investigate the role of coauthors in this process, i.e., to which extent single authors and author groups differ in their publication behavior. We investigate researchers in Germany in the fields of economics, finance, and management and how they reacted to the introduction of the DEAL as well as the cut-off from recent publications in Elsevier journals. Other than Springer Nature and John Wiley & Sons, the publisher Elsevier not only concluded no agreement with the German research institutions. The latter also ended their subscriptions to the publisher's journals, which resulted in Elsevier blocking access to its outlets. The responses of men and women to these changes differ. At the margin, men tend to seek reputation by continuing to publish in Elsevier journals. At the same time, women opt out of journals with access restrictions and more towards those with frictionless open access. By doing so, they contribute more to the public good of open science. However, they may harm their careers given that the discipline of economics is especially highly focused on the reputation of the journals in which researchers place their work.

In Chapter 3, which has been accepted for publication in the journal *Scientometrics*, I extend the analysis of the impact of the German DEAL agreements from chemistry alone to eight disciplines. Notably, I can replicate the positive effect in chemistry and economics and find a slightly significant positive reaction in materials science. In contrast, I do not detect a positive reaction in any other field. There, I only find null effects. Parallel developments in the academic publishing market are a potential reason for that. While the emergence of enormous interdisciplinary and fully open-access journals is similar for Germany and the rest of the world, smaller consortia in Germany have closed transformative agreements with plenty of publishers aside from the DEAL agreements with Springer Nature and Wiley. Nonetheless, they cover journals in the fields with null results as well as in those with positive reactions to the DEAL. However, at least descriptive, suggestive evidence exists for some ‘Matthew’ effect, namely that the DEAL publishers Springer Nature and Wiley benefit the most from their transformative agreements in disciplines in which they already had a dominant position prior to the introduction of the DEAL conditions.

In Chapter 4, I investigate citation differentials between different types of access to publications. I exploit that the publisher Elsevier introduced the so-called ‘X journals,’ i.e., ‘mirror’ journals with the same scope, editorial board, and peer review process as their established ‘parent’ journals. The distinctive feature was that all publications in X journals were open-access by default. In parallel, the publisher continued to offer an open-access option for purchase in the parent journals. Furthermore, publishing in the parent journals without open access, i.e., requiring a subscription to read a paper, remained the most common option. The setting provides me with three different access types – open access in the X journal and open access or restricted access to the parent journal – while the quality of the papers should be arguably the same given the identical editorial boards and peer review standards.

I identify a notable citation disadvantage for publications in the X derivatives and no citation advantage for open-access to publications in the hybrid parent journals. It does not contribute to the promotion of competition in the academic publishing market as newly launched journals, like the X journals, have a competitive disadvantage due to the lower number of citations. For authors, it is less attractive to publish their work there, which poses a significant hurdle for new outlets to establish themselves as considerable publication alternatives for researchers. One potential way to counter this is through strict publication requirements of funding agencies. My results show that work supported by leading European funding bodies is related to a much higher uptake of open access, which, in turn, may lift the reputation of the journals in which the work supported by prestigious grants is published.

The final Chapter 5 is somewhat unrelated to the rest of this thesis. It is a coauthored work with Kai Fischer and J. James Reade and has been published in the journal *Labour Economics*. In this chapter, we examine the direct effect of an infection with the COVID-19 virus on the labor productivity of the individual. We study male soccer players in the elite leagues of Germany and Italy and provide causal evidence that infection leads to a significantly lower performance once the player returns to the pitch. This deterioration is not short-lived but persists for more than half a year. Other respiratory infections do not cause the same effect. The findings have important implications for measuring the economic impact of the COVID-19 pandemic and evaluating non-pharmaceutical interventions as they are meant to reduce the number of infections.

In total, this thesis provides a nuanced economic evaluation of transformative agreements between university libraries and academic publishers. It offers an in-depth assessment of the agreements' competitive effects. It further reveals potential ways to foster competition in the academic publishing market in the digital age, as the core challenge in this market is not necessarily promoting open access at any cost but spurring competition. In a market that inherently benefits the incumbent firms, accomplishing a level playing field for the established firms, commercial fully open access publishers, and societies and university presses should lead to market outcomes that lower prices, allow for the rejection of low-quality research, and decrease access barriers for less well-endowed countries to the global body of research. It could enhance scientific discovery, economic growth, and, ultimately, the progress of society.

Chapter 1

The Impact of the German 'DEAL' on Competition in the Academic Publishing Market

Coauthored with Justus Haucap and Nima Moshgbar

Published in 'Managerial and Decision Economics'

1.1 Introduction

Academics across many disciplines have become rather discontent about the academic publishing process. Some academics have long been critical of the merits and the organization of the peer review process (for economics see, e.g., Laband, 1990; Hamermesh, 1994; Frey, 2003; Azar, 2007; and Ellison, 2002b & 2011; for management, e.g., Lewin, 2014) or about the "publish or perish" philosophy prevalent in many disciplines (for economics, see, e.g., Akerlof, 2020; Heckman and Moktan, 2020; or van Dalen, 2021). There has also been a long-standing criticism of the high and increasing prices of journals in the 'STM' fields, i.e., science, technology, and medicine (see, e.g., Edlin and Rubinfeld, 2004; Resnick, 2019).

In response, various academics have tried to initiate – more or less successfully – boycotts by authors, reviewers, and editors of highly-priced STM journals, to bring down journal prices (see, e.g., Bergstrom, 2001; or Flood, 2012). The best-known example may have been the so-called "cost of knowledge campaign" that was launched in response to a blog post by prominent mathematician Timothy Gowers (2012). Many of the boycott campaigns specifically targeted the publisher Elsevier and its high prices for subscriptions as well as its practice of selling large subscription bundles featuring many unwanted titles. Similarly, academic libraries have long complained about the sharp and continuous increase of prices, often leaving them with less budget for books and journals considered less important than the so-called top journals that are indispensable (McCabe, 2002). Even competition authorities such as the UK Office of Fair Trade (OFT) have investigated the leading commercial publishers' behavior without taking action though (Vickery, 2003).

In response to the growing criticism by academics and academic libraries, an increasing number of research funding organizations have started to require both their employees and recipients of research grants not to transfer their copyrights any longer to publishing houses to facilitate parallel publications in research repositories such as arXIv, EconStor, RePEc, SSRN and so on.[1] A prominent example is the US 'National Institutes of Health' (NIH), the first organization to adopt this policy. Many others have followed. The University of Utrecht in the Netherlands recently changed its evaluation scheme for hiring and promotions. From 2022 on, one evaluation criterion is the individual's engagement in 'open science' that also encompasses open access to research publications (Woolston, 2021).

Many academics have repeatedly suggested shifting publications to open access outlets – a process that has proven relatively slow and challenging due to the underlying collective action and coordination problems already described by Bergstrom (2001). In addition, academics have suggested forming purchasing alliances to increase academic libraries' buyer power (see, e.g., Haucap et al., 2005).

In Germany, the so-called 'Alliance Initiative,' a task force of all German research institutions,[2] has been assigned with negotiating collective, nationwide open access agreements with the three largest commercial publishers of scholarly journals, namely Elsevier, Springer Nature, and Wiley. The initiative acts on behalf of all German academic institutions, including universities, research institutes, and their libraries. The objective of the so-called "Projekt DEAL" is to secure

(a) immediate open access publication of all new research articles by authors from German research institutions,

(b) permanent full-text access to the publishers' complete journal portfolio, and

(c) fair pricing for these services according to a simple cost model based on the number of articles published.[3]

[1] In 2016, Elsevier acquired SSRN, see `https://www.elsevier.com/about/press-releases/corporate/elsevier-acquires-the-social-science-research-network-ssrn,-the-leading-social-science-and-humanities-repository-and-online-community`, published May 17, 2016, last checked June 23, 2023.

[2] Adjunct institutions are the Alexander von Humboldt Foundation, the National Academy of Sciences Leopoldina, the German Research Foundation (DFG), the German Academic Exchange Service (DAAD), the Fraunhofer Society, the Helmholtz Society, the German Rectors' Conference (HRK) representing all universities and colleges, the Leibniz Association, the Max Planck Society and the German research council (Wissenschaftsrat).

[3] `https://deal-konsortium.de/en/about-deal/rationale-and-objectives`, last checked

While negotiations broke down with Elsevier in 2018, agreements the consortium reached agreements with Wiley and Springer Nature in 2019. More precisely, 'Projekt DEAL' signed a three-year contract with Wiley on 15 January 2019. Researchers at more than 700 German academic institutions are now able to (a) access content from Wiley journals back to 1997 and (b) publish open access in nearly the whole journal portfolio of the respective publisher (with few exemptions). A similar agreement was reached with Springer Nature on 11 August 2019, when 'Projekt DEAL' signed a 'memorandum of understanding' with Springer Nature, followed by a three-year contract starting 1 January 2020. The agreement enables open-access publishing of articles in approximately 2,500 Springer Nature journals and offers participating institutions extensive access to the publisher's journal portfolio.

Several smaller publishers consequently complained with the German competition authority, the Federal Cartel Office (FCO), as the DEAL consortium did not enter into negotiations with smaller publishers such as C. H. Beck, De Gruyter or Mohr Siebeck in Germany or Taylor and Francis, the several university presses, and others abroad.[4] The competition concerns are two-fold: First, libraries in Germany most likely have to finance the Projekt DEAL in the end and will have fewer resources to subscribe to journals not published by Wiley or Springer Nature, thereby impeding competition in the journal subscription or reader market. Second, authors from qualified institutions may prefer to publish in Springer Nature and Wiley journals, as they can publish open access in these journals at no private marginal cost once the agreements are concluded. Hence, the DEAL agreements may also affect competition for authors or papers.

In this paper, we focus on the second concern and analyze whether the DEAL contracts affect incentives for authors in their choice of submission. For that purpose, we apply a difference-in-differences (DiD) approach to estimate the treatment

June 23, 2023.

[4]See (in German): `https://www.buchreport.de/news/noch-allianz-oder-schon-kartell`, published August 29, 2019, last checked June 23, 2023.

effect on the treated (TT) authors' choice of journals for publication. Put differently, we analyze whether scholars that are eligible for open-access publications in Wiley and Springer Nature journals under the DEAL agreements show a different publication pattern than scholars that are not eligible. While we can only observe publications but not authors' submission behavior, submissions and publications are typically correlated. Manuscript turnaround times differ substantially between different fields of science and are relatively long in some disciplines, such as economics (see, e.g, Ellison, 2002b). Hence, most articles published in economics journals in 2019 and 2020 will have been submitted well before the announcement of the DEAL agreements. Therefore, our analysis focuses on chemistry, which has much faster turnaround times, so we expect the DEAL agreements to have at least some impact already. Since, however, the contracts have only been in force for quite a short period – since 2019 and 2020 with Wiley and Springer Nature, respectively — our results have to be regarded accordingly as early empirical evidence.

Even though the observation period has been short, we find a statistically significant increase in the likelihood of publishing in eligible Springer Nature or Wiley journals, amounting to 3.8% for authors from eligible institutions in the treatment period. The main driver of this effect are journals in the mid-range of journal quality and reputation. This suggests that open-access publications in eligible journals under DEAL contracts are attractive for researchers. While definite conclusions on the persistence of such an observation have to remain for future research at this stage, such a development may have severe implications for competition in the STM journal market, as large commercial publishers may have advantages in the competition for authors. As libraries tend to subscribe to those journal packages where the most important papers are published and the best authors publish, libraries may rather cancel other subscriptions in the future or be less willing to finance other journals' open access charges, leading to a disadvantage for non-DEAL journals. Hence, large-scale DEAL-like contracts can further strengthen the leading publishing groups' positions. In turn, national science alliances may prefer to negotiate contracts with large publishing firms with the most extensive journal portfolio. This competitive imbalance may induce further market concentration in an already concentrated market (Larivière et al., 2015), and, in the long term, possibly lead to further price increases.

This paper proceeds as follows. In Section 1.2, we briefly describe some key characteristics and developments in the academic publishing market. Section 1.3 provides details on the German DEAL agreements. The empirical analysis is provided in section 1.4. Section 1.5 discusses the resulting competition implications. Section 1.6 concludes.

1.2 The Academic Publishing Market

As with any other media outlet, academic journals are platforms that unite authors and readers, i.e., they operate in two-sided markets. Readers are interested in scientific research and results, while authors are interested in publishing their ideas and findings. In principle, readers are interested in the most important results in their fields, which are published in so-called 'top journals.' From a reader's perspective, academic journals are complements rather than substitutes, as knowledge about a particular study published in a particular journal cannot easily be substituted by knowledge about a different study published in a different journal. From that perspective, having access to as many journals as possible is beneficial. Libraries, however, only have limited budgets, so they typically cannot subscribe to all journals but have to make choices between titles. Hence, journals compete for library budgets. As some journals are almost indispensable, as they publish the most important research results, the academic journal market works differently than most other markets. McCabe (2002) have shown that increasing top journal prices can lead libraries to cancel subscriptions to other journals — a clear sign of complementarity rather than substitution effects.

In principle, readers are not interested in journals as such but in papers. Technological progress, particularly digitization, allows for a novel form of unbundling. Readers can purchase single articles and obtain them quickly through electronic intra-library article sharing or from research repositories. These possibilities imply that journal subscriptions become less beneficial from the readers' perspective as long as single papers can be easily accessed.

Ellison (2011) convincingly argues that the role of academic journals has fundamentally changed. Traditionally, journals have two functions: research dissemination and signaling research quality based on quality assurance processes such as peer review. With the rise of digitization, the information dissemination function has receded in performance. Many research results and ideas are well-known long before publication due to pre-print servers and research repositories. Nowadays, social media platforms also contribute to the circulation of research. As the dissemination function of journals is dramatically reduced, a journal's primary function is to serve as a quality signal for authors but also less informed readers. This development, in turn, means that journal subscriptions would be less valuable for libraries, as the journals' information dissemination function has become less critical, and the quality signaling function does not require library subscriptions. In order to deal with the development of journal subscriptions becoming more dispensable, publishers have started to offer bundles and packages that include the essential top journals that libraries cannot substitute.

From an author's perspective, two related aspects are important in choosing publication outlets: journal reputation and visibility which facilitates citations, which are sometimes described as the ultimate currency among scholars. Journal reputation is typically an imperfect function of citation frequency and other factors (see, e.g., Bräuninger and Haucap, 2003). Citations, in turn, are a function of journal reputation (so there is a clear endogeneity issue) but also of other factors. In particular, several studies have found that open access positively affects citations in various ways even though the findings are not unambiguous (see, e.g., Antelman, 2004; Eysenbach, 2006; Atchison and Bull, 2015; McCabe and Snyder, 2014, 2015, 2021; Mueller-Langer and Watt, 2010). For the author, publications outlets are substitutes to some degree. Hence, journals compete to attract high-quality papers or authors. One competitive advantage in that process is the option to publish open access, as this can enhance visibility and increase citations and, thereby, an author's H-index or some other measure of citations.

In principle, authors aim to publish in journals with the highest reputation and citation rates that attract the most readers, while readers also mainly focus on the top journals. This lends enormous market power to top journals (see, e.g., Heckman and Moktan, 2020), which is rather difficult to break due to an underlying coordination problem between authors and readers. As Bergstrom (2001) has explained long ago, the academic publication market inherently faces a coordination problem typical for two-sided markets. Theoretically, the scientific community could move to other less expensive journals, such as non-profit open-access journals. Practically, this is unlikely to happen, however, due to the underlying collective action problem.

While scholars and scientists may jointly be better off if the best research would be published open access in low-priced journals, no scholar has strong incentives to be the first to move. Young researchers especially have powerful incentives to publish in well-established journals with a long-standing reputation to gain visibility and reputation. Empirical research by Heyman et al. (2016) suggests that in the Elsevier boycott, "only 37% of the 'won't publish' signatories are clearly boycotting Elsevier by publishing elsewhere." As the authors explain, the situation "actually resembles a social dilemma in which people might reason: If I still publish in impactful Elsevier journals and most other researchers/signatories stop publishing in these journals, it will be good for my résumé/career, while Elsevier will have to change its ways." Nevertheless, there exists preliminary evidence that the termination of subscriptions to Elsevier journals by many German universities following the failed DEAL negotiations with the publisher is related to a decrease in Elsevier's market share among German-based researchers (Fraser et al., 2023).

Open Access (OA) is often suggested as an alternative. Suber (2012) defines mainly two columns: Green and Gold Open Access. While the latter encompasses free access to an article published in a peer-reviewed journal, green OA only allows an upload of an article in non-reviewed repositories for papers. The final article is still published behind a subscription wall. Green OA sometimes includes a delay or waiting period before it is posted in a repository, but it is likely less expensive than Gold OA. But the establishment of OA has raised its own questions. McCabe and Snyder (2005) discuss the risk of lower quality in OA publications. The main argument is that publishers can increase profits by the additional publication of papers as they receive an additional OA charge per paper. McCabe and Snyder (2005) suggest separating publication fees into a 'submission' and an 'acceptance' part. Another commitment to quality would be establishing or preserving a long-run reputation that would be harmed by too many low-quality publications.

In an evaluation of the status quo in 2008, Björk et al. (2010) found that 20.4% of scientific articles have been published open access using a random sample of 1837 articles. Another study finds an average share of approximately 24% for 2005-2010 using a sample of some 100,000 publications in 14 disciplines, i.e., some 1,300 articles per discipline per year. 21.4% are published under green OA, 2.4% under gold OA with an annual growth rate of 1% (Gargouri et al., 2012). A more recent study finds a share of 27.9% using a sample drawn from the Crossref database and a share of 36.1% using the World of Science database (Piwowar et al., 2018). Solomon (2013) provides a deeper investigation of the types of publishers that make OA articles available. The largest share of one-half of the journals and 43% of the published articles is held by universities and societies that run their own open-access programs. In 2010, commercial publishers (such as Elsevier, Springer Nature, Wiley) counted for one-third of the journals and 42% of the articles. The university-based journals are often free of charge and can largely be found in countries with less settled research institutions and infrastructure than in the US or Western Europe. Furthermore, it is noted that a growing number of research projects financed by foundations and government agencies in North America and Europe require OA publication of the project results (Solomon, 2013).

Until recently, the movement towards open access has been slow. Schools and departments have been unwilling or unable to provide structured green OA to eligible papers. Unlike in other industries such as music, transportation, or travel booking, the academic publishing market has not seen dramatic shifts in market power caused by the internet. One reason may be the lack of substitutes (Björk, 2017). The Max Planck Digital Library (MPDL), an administrative sub-unit of the German Max Planck Society, published a white paper that argues in favor of a large-scale transition of academic publishing towards open access (Schimmer et al., 2015). Dividing the estimated total subscription fees by the number of articles published, the authors find costs per article between 3,800€ and 5,000€ for the mainly subscription-based model. This money could be used to pay the 'article processing charges' (APC) that have to be paid for an OA publication. As the authors calculate an average APC of 2,000€, they do not only see full coverage for a transition but also the chance for sufficient savings (Schimmer et al., 2015).

Further detailed analyses of the economic effects of copyrights, open access publishing, and its costs and benefits, risks and opportunities are provided by Mueller-Langer and Scheufen (2013), Scheufen (2015), and Eger and Scheufen (2018).

1.3 The German DEAL

The German 'DEAL' is a project addressing many issues discussed in the previous section. The "Alliance of Science Organizations" is a network of nearly all research institutions in Germany. The members are universities, colleges, research libraries, the German Research Foundation (DFG), the Max Planck Society, the Fraunhofer Society, the Leibniz Association, the Helmholtz Association, and all their subunits. Together with further entities, the group of members consists of more than 700 institutions from all fields of research in academia. This makes the German DEAL globally unprecedented in scope and size. This alliance aims to negotiate 'publish and read' agreements with all major publishers against the backdrop of rising fees of big publishing companies. These agreements should cover immediate and full open access (gold OA) and full access to the publishers' full journal portfolios.[5] Negotiations started with three major publishers Elsevier, Springer Nature, and Wiley. The DEAL task force did not reach a contract with Elsevier. The main dispute was around Elsevier's intention to split the 'publish and read' agreement into two separate contracts and offer only green OA (Hunter, 2018). The alliance signed two DEAL agreements with Wiley and Springer Nature.

The DEAL agreements are administrated by the Max Planck Digital Library (MPDL) Services GmbH, a subsidiary of the Max Planck Society. In general, both DEAL contracts encompass the same two major aspects: a 'publish' and a 'read' part. The former means that every article published at an eligible journal is immediately after publication available under Gold OA. The latter provides all German research institutions full access to the online journal databases of the publishers (Hunter, 2020). The research institutions do not pay subscription fees for any included journal anymore. There is rather a fixed Article Processing Charge (APC) that is paid per article by the MPDL to the publishers that also contains some price

[5] `https://deal-konsortium.de/en/about-deal/rationale-and-objectives`, last checked June 23, 2023.

for the access to the publisher's journal portfolio.[6] In turn, the MPDL charges the institutions for the publication costs of their researchers. In the beginning, this is meant to be covered by the former subscription fees. In later years, the payments of the institutions shall also reflect an institution's individual research output.[7] Researchers do not have to pay the APC fee in general as their institutions cover this fee. Nevertheless, the institutions could require some cost sharing in the future in case the institution's budget is not sufficient to cover the costs for all publications of its researchers within a billing period.[8]

Date	**Publisher**	**Event**
18.08.2016	Elsevier	Start of negotiations
28.04.2017	Wiley	Start of negotiations
17.05.2017	Springer Nature	Start of negotiations
05.07.2018	Elsevier	DEAL consortium suspends further negotiations
15.01.2019	Wiley	Signing of the DEAL agreement for 2019-2021
22.01.2019	Wiley	Submissions to Full OA journals fall under DEAL conditions
01.07.2019	Wiley	Submissions to Hybrid journals fall under DEAL conditions
22.08.2019	Springer Nature	Memorandum of Understanding signed
08.01.2020	Springer Nature	Signing of the DEAL agreement for 2020-2022
01.01.2020 (retroactive)	Springer Nature	Submissions to Hybrid journals fall under DEAL conditions
01.08.2020	Springer Nature	Submissions to Full OA journals fall under DEAL conditions

Table 1.1: Time line of the DEAL negotiations

[6] Thus, it is not directly comparable to APCs from other publishers that only cover the publication of a paper.

[7] See `https://web.archive.org/web/20210620182559/https://www.projekt-deal.de/faq-for-participating-institutions/`, copy from the web archive, copy date June 20, 2021, last checked August 16, 2023.

[8] See `https://web.archive.org/web/20210620185659/https://www.projekt-deal.de/faq-for-authors/`, copy from the web archive, copy date June 20, 2021, last checked August 16, 2023.

Part of the DEAL are three types of journals: Hybrid journals are those that are sold globally on a subscription base. The hybrid part stems from the fact that articles from authors with a German affiliation are published open access as outlined earlier. Full OA journals are journals that are already published as full open access publications. The last type is 'read only' journals. Authors *cannot* publish Gold OA in these journals but the whole content is fully available at all German research institutions. Table 1.1 displays the timeline of the negotiations and the dates, when the different journal types fall under the DEAL conditions.

1.3.1 The DEAL contracts with Wiley and Springer Nature

Wiley and the German research alliance signed their DEAL contract on 15 January 2019. The agreement came into operation on 22 January 2019 for existing full open access journals, and on 1 July 2019 for hybrid journals. The agreement was set to expire on 31 December 2021. It is automatically extended by one year if no party objects.[9] Eligible for publication under the agreement are corresponding authors affiliated with an institution that is part of the German research alliance. The 'read' part grants full access to all Wiley journals from 1997 onward. The fee for publishing a paper in a hybrid journal amounts to 2,750€. For Full OA journals, an individual publication fee exists. Wiley grants a 20% discount on the scheduled price (Sander et al., 2019, entire contract). Part of the contract are 1,747 journals.[10] By that, nearly the complete portfolio of the Wiley group is part of the DEAL. Table 1.2 distinguishes the number of included journals by type, i.e., hybrid, full OA, or 'read-only.'

The contract between Springer Nature and the MPDL was signed on 8 January

[9] In the meantime, it has been continuously prolonged, see the press statement of the publisher from November 21, 2022, `https://newsroom.wiley.com/press-releases/press-release-details/2022/Wiley-and-Projekt-DEAL-Extend-Open-Access-Agreement-into-Fifth-Year-0/default.aspx`, last checked June 23, 2023.

[10] Last update October 12, 2020. The number has changed in the meantime to 1,986 (last update June 19, 2023), see `https://keeper.mpdl.mpg.de/f/fed54cfc4e7f4c178137/?dl=1`, last checked June 23, 2023.

2020. The 'publish part' entered into force with retroactive effect from 1 January 2020 for hybrid journals and full OA journals from 1 August 2020. The contract expires at 31 December 2022, with an option to extend the contract by 12 months.[11] The read part was immediately active. Equivalently to the Wiley contract, the publication fee per research paper is 2,750€. Springer Nature also provides a 20% rebate for publication in its full OA journals (Kieselbach, 2020, full contract). The eligible journals also encompass publications from Springer subsidiaries such as BioMed Central, Pleiades Publishing, and Palgrave Macmillan. In total, the contract encompasses 2,857 journals. The RHS of table 1.2 shows the shares of the three journal types. Similar to Wiley, the lion's share consists of hybrid journals.[12] The whole portfolio of the publisher contains 3,175 journals. By that, approximately 92% of the whole portfolio is part of the DEAL agreement.[13]

	Wiley		*Springer Nature*	
Journal type	#Journals	Percentage	#Journals	Percentage
Hybrid	1,437	82.26%	2,086	73.01%
Full Open Access	226	12.94%	452	15.82%
Read Only	76	4.35%	319	11.17%
Miscellaneous	8	0.46%		
Total	1,747	100%	2,857	100%

The numbers encompass all academic disciplines, not only chemistry.

Table 1.2: 'DEAL' journals of Wiley and Springer Nature by journal type

1.3.2 Transformative agreements in other countries

The German DEAL contracts with Springer Nature and Wiley are not the first and not the only transformative agreements between research consortia and pub-

[11] In the meantime, this contract has been extended as well. See the company's press release on September 6, 2022: https://group.springernature.com/de/group/media/press-releases/springer-nature-and-projekt-deal-extend-partnership/23457336, last checked June 23, 2023.

[12] See https://keeper.mpdl.mpg.de/f/a6dc1e1ed4fc4becb194/?dl=1, last updated: October 8, 2020, last checked June 23, 2023.

[13] See https://resource-cms.springernature.com/springer-cms/rest/v1/content/18466124/data/v2, last update October 8, 2020, last checked June 23, 2023.

lishers. Wiley has closed large scale agreements with Hungarian, Austrian, Dutch, Finnish, Hungarian, Norwegian, Swedish and UK universities. This does not encompass necessarily all research institutions of a country but large consortia.[14] Springer Nature has similar contracts with universities within the mentioned countries and additionally closed transformative agreements in Italy, Poland, Qatar, and Switzerland.[15] Elsevier closed such contracts in Hungary, [16] Italy,[17] Poland,[18] Sweden,[19] and Switzerland[20]. There exist further bundling contracts in many countries and some publish and read contracts with several single universities and research institutions. But neither those small contracts nor the consortial agreements have the size of the German DEAL in terms of the number of participating institutions. There already exist early descriptive evaluations of pilot agreements between universities in the UK (Marques & Stone, 2020) and Sweden (Olsson et al., 2020) in the literature.

1.4 Empirical Analysis

Due to the DEAL contracts with Wiley and Springer Nature, German research institutions benefit from favorable conditions regarding fees and access to obtaining and publishing academic publications. Primarily the new possibility to publish one's own research with open access in peer-reviewed journals otherwise subject to access barriers appears as a significant new incentive to researchers. Thus, researchers from German research institutions may increasingly aim at publishing in journals

[14]See https://authorservices.wiley.com/author-resources/Journal-Authors/open-access/affiliation-policies-payments/index.html, last checked June 23, 2023.

[15]See https://www.springer.com/gp/open-access/springer-open-choice/springer-compact, last checked June 23, 2023.

[16]See https://www.elsevier.com/open-access/agreements/hungary, last checked June 23, 2023.

[17]See https://www.elsevier.com/open-access/agreements/crui, last checked June 23, 2023.

[18]See https://www.elsevier.com/open-access/agreements/poland, last checked June 23, 2023.

[19]See https://www.elsevier.com/open-access/agreements/sweden-bibsam, last checked June 23, 2023.

[20]See https://www.swissuniversities.ch/en/themen/digitalisierung/open-access/publisher-negotiations, last checked June 23, 2023.

covered by the German DEAL. Henceforth, the underlying research question is, if at all, the publishing behavior of researchers at German institutions reacts to potential incentives set by this DEAL. In order to obtain an empirical answer to this question, we estimate whether there is a response in the likelihood for authors affiliated with German research institutions to publish their articles in journals subject to the DEAL in the treatment period. We estimate the treatment effect on the treated of the German DEAL in a difference-in-differences (DiD) framework. In our setting, the treatment happens on the level of the journal choice. Researchers adjusting their publication preferences are captured by the change in the probability of a paper appearing in a treated journal.

We choose the field of chemistry as a suitable field for analysis due to several reasons. According to Björk and Solomon (2013), it is a discipline with a comparatively low time lag between submission and publication of a paper, which is a crucial aspect against the backdrop of the relatively short treatment time period to the present day.[21] Also, chemistry is a rather small field of research among fields of natural science, in which the support for the DEAL negotiations in Germany was particularly strong.[22]

The number of journals switching between treated and untreated publishers within our time window is vanishingly low. In total, we find five journals[23] and adjust the treatment measurement accordingly. Based on this, we rule out journal publisher changes as a confounding factor for our analysis. We consider the selection of the treatment as unrelated to the expected outcome. There should be no scientific reason why, for example, Wiley publications are subject to treatment instead

[21] Medicine has even lower average turnaround times. However, due to the COVID-19 pandemic, publishing in medical sciences has been severely disrupted.

[22] See, for example, the disciplines of the researchers that withdrew from editorial boards of Elsevier journals after the negotiations with the publishers failed: See `https://web.archive.org/web/20200715170007/https://www.projekt-deal.de/elsevier-news/`, copy from the web archive, copy date July 15, 2020, last checked June 23, 2023.

[23] The affected journals are 'Cereal Chemistry,' 'Chemical Papers,' 'Journal - American Water Works Association' (Journal AWWA), 'Journal of Rubber Research,' and 'Mining, Metallurgy and Exploration.'

of Taylor & Francis or Elsevier apart from reasons merely related to negotiations.

The large publishers have large journal portfolios that vary considerably across the quality range in the field of chemistry as well as in other disciplines. Hence, we cannot say that DEAL aims at fostering particular (quality) types of journals. Given this starting point, we are confident to have a valid framework applying the canonical changes-in-changes approach as outlined by Athey and Imbens (2006). Our specific setting also allows us to rule out anticipatory behavior as journals hardly switch publishers, such that a particular journal could not 'opt into' a DEAL publisher. On the other hand, authors might, in anticipation, withhold manuscripts to benefit from a contract closure. However, the inefficacious negotiations with Elsevier have shown that a successful contract is not a foregone conclusion. To further back these arguments up, the descriptive statistics of annual as well as monthly publications from authors with German affiliations do not support such a conclusion (as shown in Tables 1.4 and 1.8 in the appendix). Hence, we consider the large-scale withholding of papers as negligibly small if existent.

1.4.1 Data

We make use of a full sample of all publications in the field of chemistry from 2016 to 2020 available on Scopus,[24] a database that collects academic publications and citations. Run by the publisher Elsevier, it currently contains some 84 million entries, 25,800 journals and 249,000 books.[25] With about 1.4 million observations from 1,005 journals, the data set contains the full range of publications in the field of chemistry in the given period of time. These data are matched with lagged ranking scores from SCImago containing data on the H-Index on the journal level and

[24]We obtained the data from Scopus via the Scopus API using the *pybliometrics* library for Python developed by Rose and Kitchin (2019). The download for the years 2016-2019 took place between October 30 and November 12, 2020. The download for 2020 took place on May 6, 2021.

[25]See `https://www.elsevier.com/solutions/scopus?dgcid=RN_AGCM_Sourced_300005030`, last checked June 23, 2023.

the SCImago Journal Rank (SJR) criterion for the years 2015-2019 in chemistry.[26] SCImago built its database and ranking system upon data from Scopus.[27] We identify the journals of the leading publishers Elsevier, Springer Nature, Wiley, and the American Chemical Society via journal lists publicly available from the publishers. The data set is complemented with all other chemistry journals from the SCImago list. With these 1,005 journals, we are confident to have a full sample that includes all relevant journals of the discipline for the years 2016 up to 2020. Of the 1.4 million observations, some 1.3 million are research articles. Our quantitative analysis only uses this publication type as the DEAL agreements – in contrast to editorials, letters, reviews, etc. – focus on this common type of article. Table 1.9 in the appendix provides information on the publications per country.

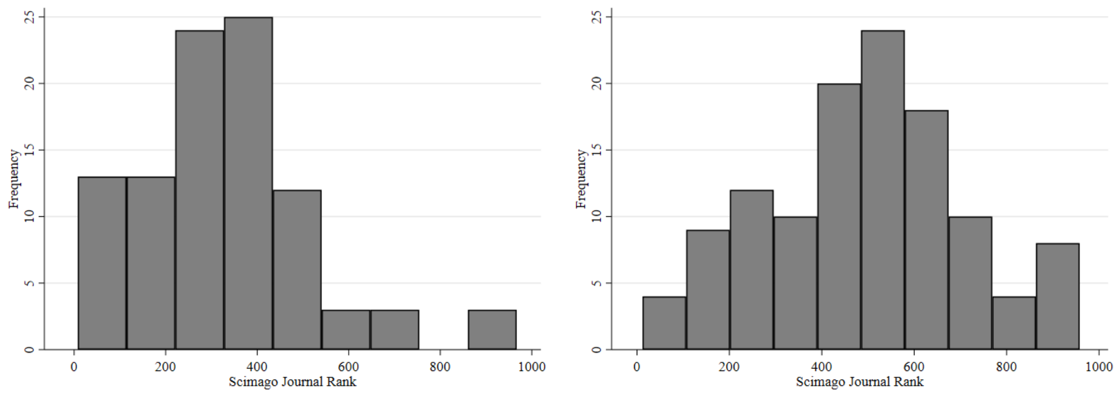
According to the DEAL, corresponding authors of an article affiliated with eligible research institutions may benefit from the contract. The Scopus data set gives information on all co-authors of each research article in the order in which they appear on the respective paper. No explicit information on the corresponding authors is added. We deduce the corresponding authors' countries of affiliation from the very order of the authors' appearance on the papers. Accordingly, country dummy variables are constructed on this assumption. The order of the authors in chemistry is typically not alphabetical as in economics. We may assign some articles to countries other than Germany if several corresponding authors are apparent and their order is such that the corresponding German author is not named first. As the Tables 1.5, 1.6, and 1.7 in the Appendix show, most papers have at most four authors. Among research groups, most of the teams have at least two authors from the same country, often all of them affiliated with the same. Hence, we consider the threat of miss-assignments as negligibly small.

[26]See `https://www.scimagojr.com/journalrank.php?area=1600`. (last checked June 23, 2023) for the ranking tables.

[27]See `https://www.scimagojr.com/aboutus.php`, last checked June 23, 2023.

1.4.2 Descriptive statistics of DEAL journals

The journals that fall under the DEAL agreements (DEAL journals) are differently distributed with respect to their rank. Figure 1.1 shows the distribution of journals across ranks for Wiley (1.1a) and Springer Nature (1.1b) respectively. This is based on the SCImago journal ranking using the data for all journals listed in the field of chemistry in 2018 – the year before the first DEAL agreement was closed. The rank is based on the SJR criterion.[28]



(a) Wiley (b) Springer Nature

Distribution of journals in the field of chemistry by journal rank. Wiley: $N = 96$, Springer Nature: $N = 119$. SCImago Journal Rank is descending: Higher quality has a lower rank.

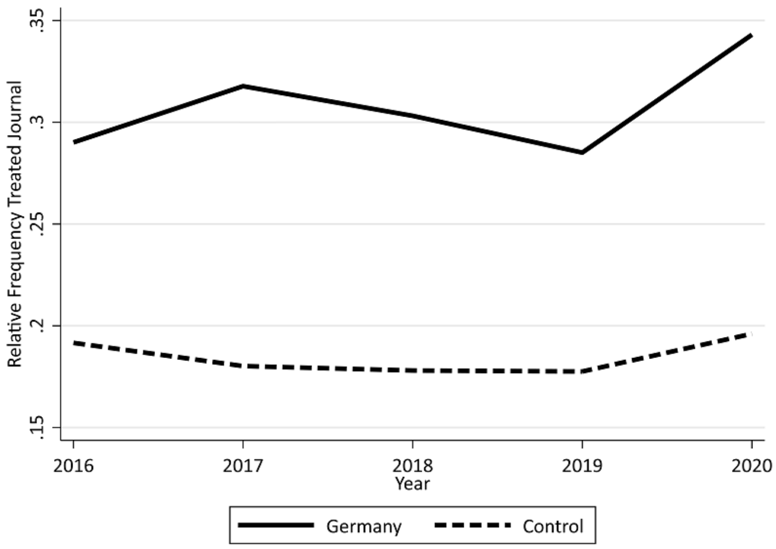
Figure 1.1: Comparison of Journal ranks by publisher

Wiley journals subject to the DEAL contract show a left-skewed distribution implying that Wiley's journal portfolio consists, in general, disproportionally more top-ranked journals. Springer Nature, on the other hand, shows a less skewed distributed portfolio with a roughly symmetric peak in the middle of the ranking range.

Figure 1.2 shows the annual shares of publications in DEAL journals by scientists from (a) German institutions and (b) other institutions, both concerning their total number of publications. The treatment and control group show an increase in the treatment period (as of mid-2019). However, the increase is much more pronounced

[28]See for an evaluation of this measure e.g. Mañana-Rodríguez (2015)

in the treatment group of corresponding authors with a German affiliation. To further substantiate the parallel trends assumption, we investigate every country in the sample separately. As a consequence, we exclude 28 countries[29] as these show very particular trends in the share of papers published in journals from Springer Nature and Wiley.[30] Our main results are, in general, robust to this exclusion, as shown in Table 1.16 in the appendix.



Yearly share of publications in DEAL journals over time distinguished between *treated* German institutions and all other institutions as *control*. Share measured in percentage. Sample: Main sample as outlined in beforehand excluding other types than scientific articles and observations from January 2016. A plot for the full sample can be found in Figure 1.6 in the appendix.

Figure 1.2: Publication Trends of Treatment and Control Group

1.4.3 Empirical Results

Table 1.3 displays summary statistics of DEAL journals in German research institutions (treatment group) and all other research institutions (control group), distin-

[29]These countries are Algeria, Argentina, Bangladesh, Bulgaria, Czech Republic, Ecuador, Egypt, Estonia, Ethiopia, Hungary, India, Iran, Iraq, Kazakhstan, Malaysia, Morocco, the Netherlands, New Zealand, Nigeria, Pakistan, Saudi Arabia, Serbia, Slovakia, South Africa, South Korea, Tunisia, Turkey, and Ukraine.

[30]Level differences between countries are controlled for in the DiD approach by construction. However, country-specific trends that may arise from unobserved confounding factors are much more challenging to control. In order to ensure these are appropriately isolated, we introduce country and year interactions in addition.

guishing between pre-treatment and treatment periods.[31] The treatment period is defined as beginning as of 1 July 2019 (beginning of phase 1).[32] The share of publications from German research institutions in DEAL journals between the treatment and pre-treatment periods shows a positive difference of 5.11%. In contrast, the difference for the control group amounts to 2.93%. Both treatment and control groups show an increase between pre- and post-treatment periods in the share of publications in journals subject to the German DEAL agreement. The increase for the treated German institutions is 2.18 percentage points higher, though, which is the plain difference-in-differences coefficient on sheer means. In order to control for

	Germany	Δ	Control	Δ	Plain DiD
Treatment	34.45%	5.11%	20.46%	2.93%	2.18%
Pre-Treatment	29.34%		17.53%		
N	48,499		922,310		970,809

Treatment period is as of 1st July 2019. Differences for the average share of publications from German institutions and others (*control*). This is the sample mean comparison for the main sample as outlined in Section 1.4.2 excluding other types than scientific articles and observations from January 2016. For the sample including the previously mentioned 28 excluded countries, see Table 1.10 in the appendix.

Table 1.3: Sample mean differences for the share of publications in DEAL journals

potential other confounding factors, we purge this plain DiD coefficient from Table 1.3 in a set of regression analyses.[33] The underlying research question is whether the academic community responds to publication-related incentives subject to the German DEAL contracts by shifting research projects to eligible journals. In order to find empirical answers to this question, we estimate the average treatment effect on the treated (TT) using a heteroskedastic probit model. We restrict our analysis to the publication type 'scientific article,' which encompasses the vast majority of

[31] Further descriptive statistics for the sample are provided in the appendix.

[32] Even though Wiley's full OA journals became eligible in January 2019, we consider July an appropriate starting point as the hybrid journals fell under the DEAL conditions from July on. This journal type accounts for the lion's share and benefits from open access.

[33] Further plain DiD coefficients for the full sample are displayed in Table 1.16 in the appendix.

observations in our sample. The binary dependent variable takes the value 1 if a research article is published in a journal included in the DEAL conditions and 0 otherwise.

We control for publication year and month as well as for the country of an author's academic affiliation. Most important, however, is the journal's reputation or quality, as this is likely to be the major driver of the author's journal choice. In order to isolate the treatment effect from journal quality, we include the one-year lagged H-Index of a journal in our regressions.[34]

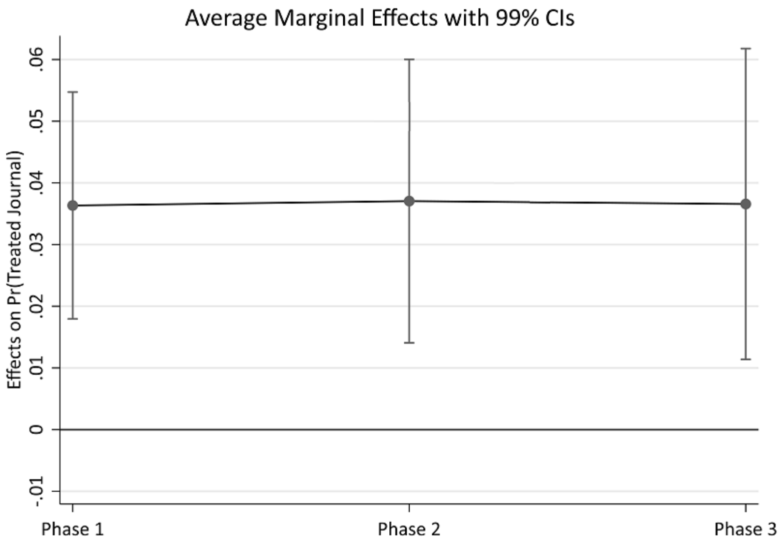
Additionally, we interact the fixed effects for country and year with each other in order to separate the post-treatment interaction effect for German institutions from other country-specific or time-specific factors. It is crucial for the validity of the underlying DiD estimation using the interaction of treated entities (Germany) and treated time period (dates after 1 July 2019) by the very construction. The data show a notable skewness of dates of publication towards January of a respective year, suggesting that publications are reported to be published in January if the exact date of publication is not made available on Scopus.[35]

We estimate a heteroskedastic probit model, a generalization of the probit model accounting for potential bias caused by heteroskedasticity. As the probit model is non-linear, present heteroskedasticity causes bias in the point estimates rather than only wrong standard errors as in linear models such as OLS. The variance is modeled explicitly following Harvey (1976). We suspect heteroskedasticity stems from a

[34]See e.g. Bornmann and Daniel (2007) for an overview of this bibliometric measure. We use the SCImago Journal Rank as an alternative impact measure. See González-Pereira et al. (2010) and Guerrero-Bote and Moya-Anegón (2012) for the background of this measure. Further measures would be, e.g., the Journal Impact Factor (JIF). However, Bornmann and Marx (2016) note that all criteria are highly correlated, so we consider applying H-index and SJR as appropriate.

[35]In the main results, observations from January 2016 were removed as the shares of publications in DEAL journals appear unusual. The correlation of pre-treatment shares between authors affiliated with treated institutions and control institutions is considerably higher if January 2016 is excluded. With January 2016 included, the pre-treatment correlation between treated and control amounts to 0.72, whereas excluding only the month of January 2016 from the data set, this correlation rises considerably to 0.85 rendering the common trend assumption of a DiD estimator much more plausible. In a robustness check, in which these observations are included, the main result remains robust, see Table 1.12 in the appendix.

considerable variation in journal quality as measured by the H-index and variation over time captured by the year fixed effects. The null hypothesis of homoskedasticity is constantly rejected. Eventually, we apply Eicker–White standard errors, robust to heteroskedasticity, for all regressions. The baseline coefficient of interest, the interaction of the post-treatment period after 1. July 2019 and eligible institutions (Germany), GER_{TREAT}, is positive and statistically significant. The corresponding average marginal effect (AME) amounts to 3.81%. That is, on average, authors from treated institutions are subject to nearly 4% higher likelihood to choose a DEAL journal for their publications in the treatment period. Regression coefficients can be found in Table 1.11 in the appendix. We further ensure that the effect is not sensitive to the choice of the quality parameter. Using the SCImago Journal Rank (SJR) instead, we can confirm our results – see Table 1.14 and Figure 1.7 in the appendix. There, we also present an alternative specification as a linear probability model (OLS) in Table 1.15. The OLS estimate confirms our core finding as well.



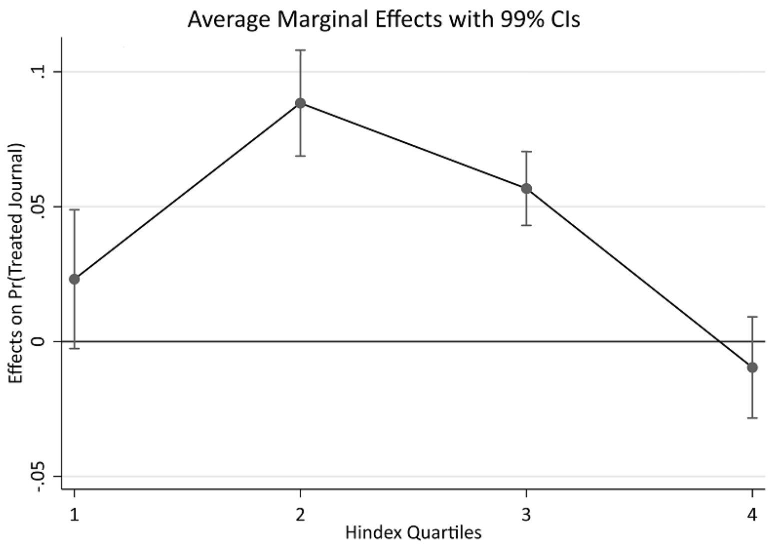
Average marginal effects of the heteroskedastic probit model for the three treatment phases. Point estimates along with 99% confidence intervals. Phase 1 as of 1. July 2019. Phase 2 as of 1. January 2020. Phase 3 as of 1. August 2020. Sample: Main sample as outlined in Section 1.4.2 excluding other types than scientific articles and observations from January 2016. The related regression table can be found in Table 1.13 in the appendix.

Figure 1.3: Average Marginal Effects of DEAL by Phase

In the second step, we distinguish the three phases of the German DEAL contracts with Wiley and Springer Nature. To do that, we interact the three phases with the dummy variable for treated institutions (Germany). Phase 1 takes on the value 1 as of 1 July 2019, otherwise 0. Phase 2 takes on the value 1 as of 1 January 2020, otherwise 0. Phase 3 takes on the value 1 as of 1 August 2020, otherwise 0. Figure 1.3 shows the corresponding average marginal effects along with 99% confidence intervals obtained by the delta method.[36] The AME for phase 1 becomes 3.63%, for phase 2 it gets 3.70% and for the last phase 3.66%. These are not significantly different from each other. Standard errors of the estimates increase as the phases proceed. In turn, phase 3 is very close to the end of our time series, so the true effect might appear only in 2021 or later. The marginal effects are significant from the first treatment phase on and for all three. Note that as of phase 2, both the Wiley and the Springer Nature contracts have become effective. Against the backdrop of the very short time period at our disposal for this early empirical analysis, the shown results might provide a lower bound of a development yet to unfold.

We not only find a significant effect of the DEAL on publication preferences in chemistry, but we also analyze the heterogeneity of effects concerning journal quality. Put differently, in a third step, we calculate the DEAL effect along the distribution of journal quality from our estimates. These are obtained by means of interactions with quartiles of the H-Index, where the first quartile captures journals with the lowest H-Index. Figure 1.4 displays the heterogeneous treatment effect decomposed by journal quality. One can see an inverse u-shaped pattern. We find among the highest-ranked journals no treatment effect at all. For the journals with the lowest H-Index, the treatment effect is 2.31%, but also insignificant on the 1% level. Compared to that, the effect on the journals in quartiles 2 and 3 is much larger. For quartile 2, we find an AME of 8.84%, nearly four times the size of the AME for quartile 1. For quartile 3, we find a marginal effect of 5.67% on average.

[36] Due to a large number of observations, we consider using the 99% confidence interval as appropriate.

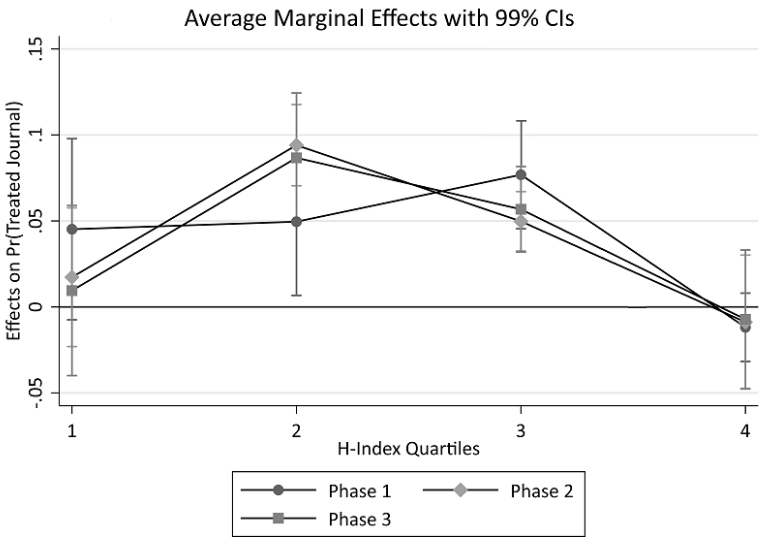


Average marginal effects of the heteroskedastic probit model. It displays the interaction term between treatment (Y/N) and journal quality grouped into quartiles. Measure: H-index. A higher quartile implies a higher H-Index. Point estimates along with 99% confidence intervals. A robustness check using SCImago Journal Rank (SJR) as quality criterion can be found in Figure 1.7 in the appendix. Sample: Main sample as outlined in Section 1.4.2 excluding other types than scientific articles and observations from January 2016. The related regression table can be found in Table 1.11 in the appendix.

Figure 1.4: Average Marginal Effects of DEAL by H-Index

These findings are in line with the theory. The main incentive for publishing open access is the larger scientific audience that can be reached compared to subscription-based publications. As discussed in Section 1.2, this is related to more citations. Usually, academic libraries subscribe at least to the 'must stock' journals of a discipline, such that the highest-ranked journals reach a vast audience in academia, as nearly all researchers should have access to those journals. On the other hand, journals at the lower end of the H-Index distribution are less read and adopted, such that the addition of open access might not decisively add incentives. In contrast, journals in the center of the quality distribution are often field journals with relatively high quality and field-specific recognition. However, university libraries with low budgets are less likely to subscribe to them if their faculties do not engage in the particular sub-fields. Hence, the theory would predict that journals in this range should become relatively more attractive with open access compared to similar journals from non-DEAL publishers.

Figure 1.5 additionally separates the treatment effect in two dimensions: By H-Index and by phases. The effects are very similar for phases 2 and 3, in which the Springer Nature journals became eligible for DEAL. For phase 1, when only the Wiley journals became part of the DEAL, we find a more substantial effect for the third quartile. It might correspond to Wiley journals being over-proportionally located in the upper half of the quality/reputation distribution, as shown in Figure 1.1a. The treatment phases 2 and 3, in which the Springer Nature titles became part of the DEAL, are similar in shape and magnitude.



This plot shows the average marginal effects of the heteroskedastic probit model. It displays the interaction term between treatment and journal quality grouped into quartiles. Measure: H-Index. Other than in Figure 1.4, we decompose the treatment into the three phases and interact them with the H-Index variable. A higher quartile implies a higher H-Index. Point estimates along with 99% confidence intervals. Phase 1 as of 1 July 2019. Phase 2 as of 1 January 2020. Phase 3 as of 1 August 2020. Sample: Main sample as outlined in Section 1.4.2 excluding other types than scientific articles and observations from January 2016. The related regression table can be found in Table 1.13 in the appendix.

Figure 1.5: Average Marginal Effects of DEAL by H-Index and Phase

1.5 Implications for competition among journals

Our empirical analysis has found a positive effect of the DEAL on publication behavior in chemistry, even though the evaluation period has been relatively short. This finding suggests that smaller publishers' concern that journals covered by the DEAL agreements may have an advantage in attracting authors may not be irrelevant. Such an advantage directly affects competition in the journals market, as the DEAL consortium has not negotiated with smaller publishers.

The competition concerns stem from the two-sided market nature of academic journals (see, e.g., Armstrong, 2021). Readers and, therefore, libraries are particularly interested in journals with essential contributions. If DEAL journals obtain a competitive edge over non-DEAL journals in attracting contributions, libraries may instead give up non-DEAL journals in times of budget restrictions. This tendency may be strengthened through bundling journal portfolios as already analyzed by McCabe (2002). Since libraries feel they cannot give up on journal bundles that include leading journals, they may be more willing to cancel subscriptions to smaller publishers, especially if they have more difficulties attracting high-quality contributions. Hence, competition between journals may be affected through two cumulative effects: First, DEAL journals appear to have an advantage in attracting authors. While the two-sided market logic suggests that positive indirect network effects between authors and readers can lead to market concentration, the DEAL agreements may even spur this process. Second, libraries may be left with less money and incentives to pay for both subscriptions and open-access publishing in non-DEAL journals. Focusing on a few large publishers may appear reasonable in light of the transaction costs assigned with the negotiations of such complex contracts. Nevertheless, leaving out small publishers carries the risk of further strengthening the dominance of large commercial publishers and their bundling practices.

While the DEAL agreements may solve researchers' current trade-off between publishing in well-reputed journals and publishing open access (see Armstrong, 2015), there can be unintended side effects of erecting barriers to entry for small publishers and further increasing the ongoing market concentration process. The risk may be negligible if we only consider the German DEAL contracts in isolation. The implications may be much more far-reaching if other countries negotiate similar deals (also see Hunter, 2018). For example, Olsson et al. (2020) critically evaluate the Swedish pilot agreement with Springer Nature and consider it expensive, raising the concern that libraries may be left with less money for both subscriptions of smaller publishers' journals and financing open access publications in these journals.

In fact, researchers may find it more challenging to obtain funding for open-access publications in smaller open-access journals, as librarians and faculty administrations may point towards the extensive DEAL portfolio. In the German case, the DEAL journal portfolio comprises some 4,000 journals. In addition, since the DEAL agreements significantly lower transaction costs for open-access publications in the journals covered, researchers in Germany may also prefer to submit to these journals to save the hassle or transaction costs.

In order to avoid potentially harmful side effects of further increasing market power in the academic journal market, DEAL negotiating consortia should rapidly expand their offer to smaller publishers.

1.6 Conclusion

The DEAL agreements between the German research alliance on the one side and Springer Nature and Wiley on the other facilitate easy open-access publishing for researchers located in Germany while simultaneously giving them access to the publishers' extensive journal portfolio. While these DEAL agreements, at first sight, appear attractive from the perspective of professional subscribers, there can be severe unintended side effects for market competition in the long term. Even in the short period following the conclusion of DEAL agreements with Wiley and Springer Nature in 2019, our early empirical analysis reveals that researchers' submission behavior in the field of chemistry has changed. Eligible researchers have increased their publications in Wiley and Springer Nature journals at the cost of other journals. While the effect is not overly large yet, it is statistically significant and may increase over time as the agreements become even more well-known among scientists. Journals covered by the DEAL agreements have a competitive advantage in attracting authors. Given the two-sided market logic that good authors and papers attract readers, which in turn attract authors, the competitive advantage of the DEAL agreements may even be underestimated in the short run.

Overall, two competition concerns arise. The DEAL consortium has only negotiated with large commercial publishers, namely Elsevier, Springer Nature, and Wiley. At the same time, it appears to be unwilling to engage in similar negotiations with smaller publishers. No agreement was reached with Elsevier. Smaller publishers did not even get the option to sign any form of agreement. Given that DEAL agreements are now in place with Springer Nature and Wiley, two related competition concerns emerge.

First, academic libraries may be, at least in the long run, left with fewer funds and incentives to subscribe to non-DEAL journals published by smaller publishers or to fund open access publications in these journals. Secondly, eligible authors may prefer to publish in journals included in the DEAL agreements, thereby giving DEAL journals a competitive advantage over non-DEAL journals in attracting good papers. As the academic publishing markets underlie the logic of two-sided markets, these effects may further spur the concentration process in the academic journal market. Hence, research institutions and academic libraries should rapidly also start negotiations with smaller academic publishing houses.

The concerns identified in our analysis also go beyond the academic publishing sector. They concern many platform-driven markets. A recent example is the global cooperation of news publishers with Google[37] or with Facebook in the US (Newton, 2019) and Australia. Industrial economists such as Gans (2021) have voiced similar concerns regarding the new 'Australian News Media Bargaining Code,' where platforms such as Google and Facebook prefer to negotiate with large media corporations only, leaving out smaller publishers. Similarly, suppose national science and library organizations only enter into negotiations with large publishers. In that case, small publishers may vanish, and barriers to entry may be even higher than before in the academic journal market. Hence, national science and library organizations should also offer transformative agreements to smaller publishers to avoid further market concentration and an increase in the large publishers' already substantial market power.

[37]See, e.g., the blog entry by Sundar Pichai, CEO of Google and Alphabet, on October 1, 2020: `https://blog.google/outreach-initiatives/google-news-initiative/google-news-showcase/`, last checked June 23, 2023.

1.7 Appendix

Descriptive Statistics

Year	2016	2017	2018	2019	2020
# Articles: All Countries	244,216	250,940	266,121	284,691	275,750
Change to previous year	–	+2.75%	+6.05%	+6.98%	−3.14%
# Articles: Germany	10,471	10,569	10,338	10,877	9,842
Change to previous year	–	+0.94%	−2.19%	+5.21%	−9.52%

The decrease in 2020 is likely to be caused by the COVID-19 pandemic. As chemistry relies on laboratory experiments, researchers need open university campuses, which was in many countries not the case throughout 2020. Full sample including all countries and including observations from January 2016, but excluding other types than scientific articles.

Table 1.4: Published Articles per Year and Related Growth.

#Authors	#Articles	Percentage Share	Cumulative Share
1	505,840	38.27%	38.27%
2	416,992	31.55%	69.82%
3	224,666	17.00%	86.82%
4	99,604	7.54%	94.35%
≥5	74,616	5.65%	100.00%
Total	1,321,718		

Sample as in Table 1.4.

Table 1.5: Number of papers by the Number of Authors

#Authors	#Articles	Percentage Share
2	112,045	40.82%
3	102,291	37.27%
4	60,117	21.90%
Total	274,453	100.00%

We consider research groups as groups of two to four members. Sample as in Table 1.4.

Table 1.6: Number of Articles from Research Groups with Different National Affiliations

Number of Authors	Number of Articles	2 Authors		3 Authors		4 Authors	
		with the Same National Affiliation					
2	416,992	304,947	73.13%				
3	224,666	203,618	90.37%	122,375	54.47%		
4	99,604	95,735	96.12%	62,491	62.74%	39,487	39.64%

Sample as in Table 1.4.

Table 1.7: Share of Research Groups with Authors of the Same National Affiliation

Year	Jan	Feb	Mar	Apr	May	Jun	Jul	Aug	Sep	Oct	Nov	Dec
2016	3,353	706	740	714	706	579	532	608	628	663	604	638
2017	3,455	641	600	564	606	627	581	590	649	641	730	885
2018	2,016	656	720	722	637	698	701	732	772	709	772	1,203
2019	2,850	721	755	747	686	786	634	588	689	714	662	1,045
2020	1,964	746	863	718	803	760	837	767	830	717	282	555

Sample as in Table 1.4, but restricted to Germany.

Table 1.8: Articles from Authors with German Affiliation per Year and Month.

Country	#Articles	Country	#Articles
Afghanistan	2	Albania	65
Algeria	3,035	Angola	2
Argentina	4,592	Armenia	341
Australia	20,915	Austria	5,810
Azerbaijan	642	Bahamas	4
Bahrain	57	Bangladesh	1,163
Belarus	949	Belgium	7,829
Benin	34	Bermuda	5
Bolivia	29	Bosnia Herzegovina	198
Botswana	102	Brazil	25,666
Brunei Darussalam	146	Bulgaria	1,723
Burkina Faso	28	Burundi	8
Cambodia	9	Cameroon	553
Canada	22,356	Cape Verde	6
Chile	2,844	China	426,018
Colombia	3,502	Congo	11
Costa Rica	219	Cote d'Ivoire	85
Croatia	1,699	Cuba	393
Curaçao	1	Cyprus	540
Czech Republic	8,342	DR Congo	62
Denmark	5,196	Djibouti	6
Dominican Republic	11	Ecuador	669
Egypt	11,124	El Salvador	8
Eritrea	2	Estonia	795
Ethiopia	674	Faroe Islands	2
Fiji	64	Finland	4,532
France	42,568	French Guiana	4
French Polynesia	9	Gabon	7
Gambia	4	Georgia	157
Germany	57,179	Ghana	375
Gibraltar	2	Greece	3,742
Greenland	8	Grenada	3
Guam	6	Guatemala	10
Guinea	1	Guyana	1
Honduras	12	Hong Kong	3,383
Hungary	3,839	Iceland	278
India	88,500	Indonesia	3,766
Iran	39,722	Iraq	3,647
Ireland	2,230	Israel	4,941
Italy	29,925	Jamaica	91
Japan	60,509	Jordan	980
Kazakhstan	1,162	Kenya	330
Kiribati	1	Kuwait	467
Kyrgyzstan	29	Laos	5
Latvia	568	Lebanon	538
Lesotho	7	Liberia	3
Libya	102	Liechtenstein	21
Lithuania	1,352	Luxembourg	632
Macao	745	Madagascar	27
Malawi	27	Malaysia	8,389
Mali	5	Malta	82
Mauritania	27	Mauritius	125
Mexico	9,199	Moldova	290

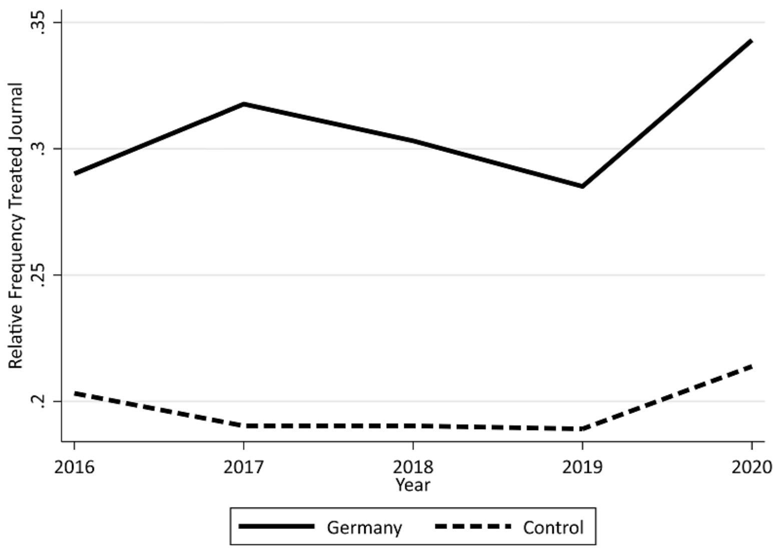
Monaco	24	Mongolia	98
Montenegro	39	Morocco	1,829
Mozambique	37	Myanmar	28
Namibia	49	Nepal	157
Netherlands	10,235	New Caledonia	5
New Zealand	2,081	Niger	4
Nigeria	2,290	North Korea	121
North Macedonia	193	Norway	3,008
Oman	598	Pakistan	9,260
Palau	1	Palestine	305
Panama	75	Papua New Guinea	8
Paraguay	30	Peru	406
Philippines	558	Poland	22,785
Portugal	7,138	Puerto Rico	376
Qatar	1,572	Romania	7,956
Russian Federation	40,923	Rwanda	42
St. Kitts and Nevis	11	Saudi Arabia	10,097
Senegal	59	Serbia	3,471
Seychelles	2	Sierra Leone	2
Singapore	9,839	Slovakia	1,724
Slovenia	1,916	Somalia	2
South Africa	4,630	South Georgia and the South Sandwich Islands	3
South Korea	47,867	South Sudan	4
Spain	31,470	Sri Lanka	390
Sudan	245	Suriname	1
Swaziland	8	Sweden	7,630
Switzerland	11,042	Syria	191
Taiwan	15,697	Tajikistan	44
Tanzania	175	Thailand	6,127
Timor Leste	1	Togo	17
Trinidad and Tobago	57	Tunisia	2,731
Turkey	16,564	Turkmenistan	1
Uganda	70	Ukraine	4,809
United Arab Emirates	1,700	United Kingdom	35,865
United States	152,743	Uruguay	537
Uzbekistan	245	Vatican City State	1
Venezuela	272	Viet Nam	4,265
Virgin Islands	1	Yemen	232
Zambia	29	Zimbabwe	55
Total	1,418,173		

Table 1.9: Number of papers per country measured by the affiliation of its first author including all observations in the sample.

	Germany	Δ	Control	Δ	Plain DiD
Treatment Pre-Treatment	34.45% 29.34%	5.11%	22.05% 18.70%	3.35%	1.76%
N	48,499		1,192,717		1,241,216

Treatment period is as of 1st July 2019. Differences for the average share of publications from German institutions and others (*control*). Full sample including the previously excluded 28 countries as outlined in Section 1.4.2 and including observations from January 2016, but excluding other types than scientific articles.

Table 1.10: Differences in Sample Means: Full Sample



Yearly share of publications in DEAL journals over time distinguished between *treated* German institutions and all other institutions as *control*. Share measured in percentage. Sample: Full sample including the previously excluded 28 countries as outlined in Section 1.4.2 and including observations from January 2016, but excluding other types than scientific articles.

Figure 1.6: Publication Trends of Treatment and Control Group

Regression Tables

Regression			
Variable		Coefficient	S.E.
GER_{TREAT}		0.0526**	(0.0221)
H-Index$_{Q2} \times GER_{TREAT}$		1.2233***	(0.2122)
H-Index$_{Q3} \times GER_{TREAT}$		0.3911***	(0.0522)
H-Index$_{Q4} \times GER_{TREAT}$		-0.0706***	(0.0184)
LR-test of $ln(\sigma) = 0$	$\chi^2(7)$	7961.68***	
F-Statistic $(country_i \times year_j)$		13046.52***	
$Wald$	$\chi^2(231)$	25683.19***	
N		970,809	
Average Marginal Effects			
GER_{TREAT}		0.0381***	(0.0049)
H-Index$_{Q1} \times GER_{TREAT}$		0.0231**	(0.0100)
H-Index$_{Q2} \times GER_{TREAT}$		0.0884***	(0.0076)
H-Index$_{Q3} \times GER_{TREAT}$		0.0567***	(0.0053)
H-Index$_{Q4} \times GER_{TREAT}$		-0.0096	(0.0073)

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Eicker-White-Standard Errors in parentheses. $GER_{TREAT} = 1$ if country = Germany and time $\geq$ July 1, 2019. Variables for modeling variance: *H-Index*$_{t-1}$ (categorical, 4 quartiles) and *year* (categorical). Base category for *H-Index*: Quartile 1. Additional controls: Country, year, month. $country_i$ captures all country dummies included, $year_j$ captures all year dummies included. Sample: Main sample as outlined in Section 1.4.2, excluding other types than scientific articles and observations from January 2016.

Table 1.11: Results heteroskedastic probit model with baseline treatment and decomposition by H-Index.

Regression			
Variable		Coefficient	S.E.
GER_{TREAT}		0.0408	(0.0284)
H-Index$_{Q2}$ $\times$ GER_{TREAT}		1.6193***	(0.3089)
H-Index$_{Q3}$ $\times$ GER_{TREAT}		0.5591***	(0.0690)
H-Index$_{Q4}$ $\times$ GER_{TREAT}		-0.0619***	(0.0227)
LR-test of $ln(\sigma) = 0$	$\chi^2(7)$	5355.10***	
F-Statistic $(country_i \times year_j)$	$\chi^2(213)$	10285.50***	
$Wald$	$\chi^2(231)$	30284.05***	
N		1,025,945	
Average Marginal Effect			
GER_{TREAT}		0.0366***	(0.0050)

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Eicker-White-Standard Errors in parentheses. $GER_{TREAT} = 1$ if country = Germany and time $\geq$ July 1, 2019. Variables for modeling variance: *H-Index*$_{t-1}$ (categorical, 4 quartiles) and *year* (categorical). Base category for *H-Index*: Quartile 1. Additional controls: Country, year, month. Sample: Main sample as outlined in Section 1.4.2, excluding other types than scientific articles, but including observations from January 2016.

Table 1.12: Results from heteroskedastic probit model with baseline treatment and decomposition by H-Index – Full Sample incl. January 2016.

	Regression		
Variable		Coefficient	S.E.
$GER_{Phase\,1}$		0.1034**	(0.0442)
$GER_{Phase\,2}$		0.0411	(0.0363)
$GER_{Phase\,3}$		0.0228	(0.0454)
H-Index$_{Q2}$ $\times$ $GER_{Phase\,1}$		0.8268**	(0.3622)
H-Index$_{Q3}$ $\times$ $GER_{Phase\,1}$		0.6033***	(0.1216)
H-Index$_{Q4}$ $\times$ $GER_{Phase\,1}$		-0.1265***	(0.0446)
H-Index$_{Q2}$ $\times$ $GER_{Phase\,2}$		1.689***	(0.3557)
H-Index$_{Q3}$ $\times$ $GER_{Phase\,2}$		0.4443***	(0.696)
H-Index$_{Q4}$ $\times$ $GER_{Phase\,2}$		-0.0581**	(0.0227)
H-Index$_{Q2}$ $\times$ $GER_{Phase\,3}$		1.5771***	(0.3918)
H-Index$_{Q3}$ $\times$ $GER_{Phase\,3}$		0.5215***	(0.0951)
H-Index$_{Q4}$ $\times$ $GER_{Phase\,3}$		-0.0367	(0.0363)
LR-test of $ln(\sigma) = 0$	$\chi^2(7)$	6271.80***	
F-Statistic ($country_i \times trend$)	$\chi^2(125)$	4631.92***	
$Wald$	$\chi^2(163)$	25791.18***	
N		970,809	
	Average Marginal Effects		
$GER_{Phase\,1}$		0.0363***	(0.0071)
$GER_{Phase\,2}$		0.0370***	(0.0089)
$GER_{Phase\,3}$		0.0366***	(0.0098)
H-Index$_{Q1}$ $\times$ $GER_{Phase\,1}$		0.0452**	(0.0204)
H-Index$_{Q2}$ $\times$ $GER_{Phase\,1}$		0.0496***	(0.0167)
H-Index$_{Q3}$ $\times$ $GER_{Phase\,1}$		0.0768***	(0.0122)
H-Index$_{Q4}$ $\times$ $GER_{Phase\,1}$		-0.0118	(0.0077)
H-Index$_{Q1}$ $\times$ $GER_{Phase\,2}$		0.0173**	(0.0157)
H-Index$_{Q2}$ $\times$ $GER_{Phase\,2}$		0.0941***	(0.0092)
H-Index$_{Q3}$ $\times$ $GER_{Phase\,2}$		0.0498***	(0.0067)
H-Index$_{Q4}$ $\times$ $GER_{Phase\,2}$		-0.0088	(0.0151)
H-Index$_{Q1}$ $\times$ $GER_{Phase\,3}$		0.0095	(0.0192)
H-Index$_{Q2}$ $\times$ $GER_{Phase\,3}$		0.0867***	(0.0146)
H-Index$_{Q3}$ $\times$ $GER_{Phase\,3}$		0.0568***	(0.0096)
H-Index$_{Q4}$ $\times$ $GER_{Phase\,3}$		-0.0072	(0.0156)

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Eicker-White-Standard Errors in parentheses. $GER_{Phase} = 1$ if country = Germany and time $\geq$ July 1, 2019 (Phase 1), $\geq$ January 1, 2020 (P. 2), or $\geq$ July 1, 2020 (P. 3). Variables for modeling variance: *H-Index*$_{t-1}$ (categorical, 4 quartiles) and *year* (categorical). Base category for *H-Index*: Quartile 1. Time FEs controlled for by means of a quadratic polynomial of the underlying time trend as suggested in Carter and Signorino (2010) and Gösser and Moshgbar (2020). Additional controls: Country, year, month. Main sample as outlined in Section 1.4.2 excluding other types than scientific articles and observations from January 2016.

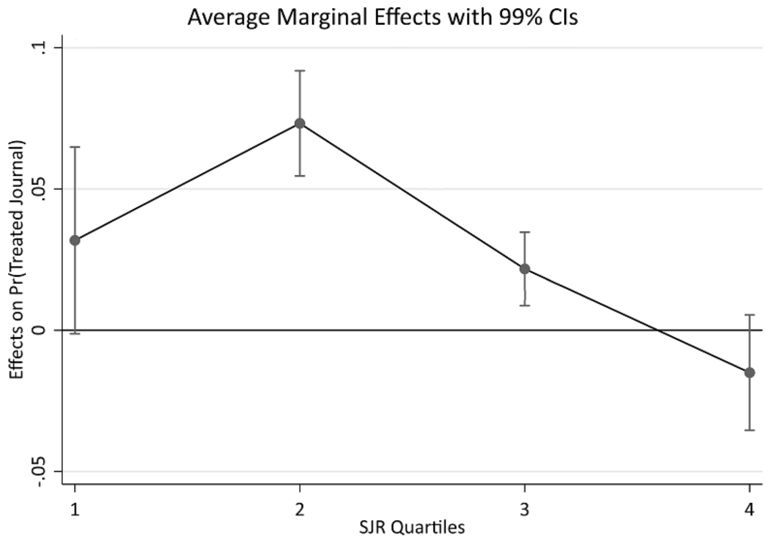
Table 1.13: Results Heteroskedastic Probit Model with Treatment Separated by Phases and Decomposition by H-Index.

Regression			
Variable		Coefficient	S.E.
GER_{TREAT}		0.0526**	(0.0221)
$\text{SJR}_{Q2} \times GER_{TREAT}$		1.2233***	(0.2122)
$\text{SJR}_{Q3} \times GER_{TREAT}$		0.3911***	(0.0522)
$\text{SJR}_{Q4} \times GER_{TREAT}$		-0.0706***	(0.0184)
LR-test of $ln(\sigma) = 0$	$\chi^2(7)$	7342.64***	
$Wald$	$\chi^2(231)$	23212.90***	
N		961,949	
Average Marginal Effects			
GER_{TREAT}		0.0251***	(0.0055)
$\text{SJR}_{Q1} \times GER_{TREAT}$		0.0318**	(0.0128)
$\text{SJR}_{Q2} \times GER_{TREAT}$		0.0732***	(0.0072)
$\text{SJR}_{Q3} \times GER_{TREAT}$		0.0217***	(0.0050)
$\text{SJR}_{Q4} \times GER_{TREAT}$		-0.0150*	(0.0079)

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Eicker-White-Standard Errors in parentheses. $GER_{TREAT} = 1$ if country = Germany and time $\geq$ July 1, 2019. Variables for modeling variance: SJR_{t-1} (categorical, 4 quartiles) and *year* (categorical). Base category for SJR: Quartile 1. Additional controls: Country, year, month. Sample: Main sample as outlined in Section 1.4.2, excluding other types than scientific articles and observations from January 2016. Variation in N compared to Table 1.11 due to missing SJR values.

Table 1.14: Results from heteroskedastic probit model with basic treatment and decomposition by SJR.



This plot shows the average marginal effects of the heteroskedastic probit model based on the results displayed in Table 1.14. It displays the interaction term between treatment (Y/N) and journal quality grouped into quartiles. Measure: SCImago Journal Rank (SJR). A higher quartile implies a higher SJR. Point estimates along with 99% confidence intervals. Th related regression output can be found in Table 1.14.

Figure 1.7: Average Marginal Effects of DEAL by SJR

Regression		
Variable	Coefficient	S.E.
GER_{TREAT}	0.0465***	(0.0147)
F-Statistic(231, 970577)	277.22***	
R^2	0.0734	
N	970,809	
Average Marginal Effect		
GER_{TREAT}	0.0219**	(0.0089)

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Eicker-White-Standard Errors in parentheses. Base category H-Index: Quartile 1. Additional controls: Country, year, month. Sample: Main sample as outlined in Section 1.4.2, excluding other types than scientific articles and observations from January 2016.

Table 1.15: Results of the OLS model.

Variable	Coefficient	S.E.
GER_{TREAT}	0.0176***	(0.0048)
$GER_{Phase\,1}$	0.0073	(0.0078)
$GER_{Phase\,2}$	0.0118*	(0.0063)
$GER_{Phase\,3}$	0.0422***	(0.0091)
N		1,241,216

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Eicker-White-Standard Errors in parentheses. These coefficients are obtained via OLS regressions, in which the binary dummy for a journal being a DEAL journal was regressed on the dummies for country = Germany and treatment time. GER_{TREAT} represents the basic regression, the three GER_{Phase} interaction terms were obtained in a separate regression. Sample: Full sample including the previously excluded 28 countries as outlined in Section 1.4.2 and observations from January 2016, but excluding other types than scientific articles.

Table 1.16: Plain DiD coefficients for the Full Sample specification.

Declaration of Contribution

I hereby declare that the chapter “The Impact of the German ‘DEAL’ on Competition in the Academic Publishing Market” is coauthored with Justus Haucap and Nima Moshgbar.

All authors contributed equally to the chapter.

Signature of coauthor Justus Haucap: ______________________

Signature of coauthor Nima Moshgbar: Nima Moshgbar

Chapter 2

The Role of Gender and Coauthors in Academic Publication Behavior

Coauthored with Justus Haucap and Leon Knoke
Published in 'Research Policy'

2.1 Introduction

Choosing an outlet for an academic paper is a high-stakes decision for researchers, as their careers depend to a large degree on publication success. Journal publications are decisive for both the reputation that researchers enjoy and, relatedly, for the distribution and reception of research results and ideas. At the same time, individuals differ in their willingness to compete in general and for high stakes in particular. One factor in this respect is a person's gender (see, e.g., Buser et al., 2023; Heinz et al., 2016; Andersen et al., 2013; Balafoutas et al., 2012; Gneezy et al., 2009). Men and women differ in risk-taking (Charness & Gneezy, 2012) as well as in their social interactions (Friebel et al., 2021). Competitiveness, risk-taking, and social behavior all tie in with the academic publication process. Women are still underrepresented in research positions even though in the European Union, for example, they account for more than half of all university graduates (European Commission, 2019).

Explanations for this "leaky pipeline" (Sonnert & Holton, 1995), which are based on differences in productivity between male and female researchers often work with bibliometric measures that capture the impact of a person's scientific contributions in one way or another. However, when comparing the ranks and citations of journals in which men and women publish, one implicitly assumes that researchers behave in the same way when publishing their work. Put differently, assume women have a lower likelihood than men to publish in *Research Policy* due to some reason unrelated to research quality. Then, comparing the shares of men and women among *Research Policy* publications does not correctly reflect ability differentials between the genders to publish work in this journal.

In this paper, we present a causal analysis of differences between men and women in their publication behavior by investigating the discipline of economics. We extend the analysis by studying how the presence of coauthors alters the publishing patterns of single authors, as women tend to publish their work more often alone than men in economics. We study publications in economics as economists put particularly strong emphasis on journal rankings (Fourcade et al., 2015; Heckman & Moktan, 2020).

In more detail, we investigate the collective undertaking of virtually all German research institutions to counter the market power of leading academic publishers Elsevier, Springer, and Wiley through collective negotiations for large-scale open-access agreements to replace the existing journal subscription model. While the latter two publishers entered into agreements, Elsevier did not. In response, German universities canceled their subscriptions, which cut off their researchers in Germany from direct access to the most recent articles published in Elsevier journals, which still prevails. For the individual scientist, the changes were exogenous. The situation provides us with two arguably natural experiments which we exploit to study behavior in reaction to this variation in the attractiveness of the affected journals.

We find gender and the types of authorship to make a significant difference, comparing single and coauthored papers as well as groups with different gender majorities. Moreover, differences between single authors and groups vary with gender. In contrast, we do not find that international collaborations (in contrast to domestic ones) or other observed differences between male and female researchers affect our findings regarding the variation in publication choices. Looking at gender more granularly, we find that women have left Elsevier journals in the lower range of the quality distribution, but we see no clear trend concerning reputation in their shift towards journals covered by the new transformative agreements. At the same time, men reduce their publications in lower-tier Elsevier journals but actually *increase* publications in top-tier Elsevier outlets. This affects publication records and contributes to gender differences.

The findings may also contribute to a better understanding of the so-called research productivity puzzle (Cole & Zuckerman, 1984; Xie & Shauman, 1998; Prpić, 2002; Kelchtermans & Veugelers, 2013), according to which female researchers have at times been found to be less productive with respect to research output than their male colleagues. We show that female economists have a higher tendency to opt out of Elsevier journals, which are regularly higher ranked in economics, while men remain attached to them. Furthermore, the publication behavior of single authors and groups differs more heavily for women. Generally, men appear to choose journals more strategically for their career than women. We carefully conclude that men put more emphasis on reputation while women emphasize the broad availability of their research. By switching towards open access and away from paywall-protected Elsevier outlets, female researchers contribute more to the public good of freely accessible research.

Methodologically, we estimate a difference-in-differences model that looks at the effect of changes in journal attractiveness on the publication behavior of affected economists. Related work by Haucap et al. (2021) has looked at short-term effects of the transformative agreements in the field of chemistry. By decomposing effects by gender and coauthors, we substantially expand that study.[1] We introduce the Elsevier cut-off as a second natural experiment to contrast arguably positive and negative publication incentives.

By studying gender differences in academia, particularly in economics, this paper contributes to an emerging strand of the literature. Overall, the presence of women in economics is still comparatively low (Auriol et al., 2022; Bayer & Rouse, 2016). It leaves an essential desideratum, as – apart from the already sketched 'productivity puzzle' – many gender differences exist in academia. Women in life sciences and STEM disciplines are underrepresented in prestigious journals (Graddy-Reed et al., 2019; Lerchenmueller & Sorenson, 2018; Holman et al., 2018). The latter find that the difference between female and male publications is more significant for wealthy countries such as Germany than for poorer ones. In economics, papers are less

[1] Gender differences in management research have also been analyzed by Nielsen and Börjeson (2019), but they focus on differences in research topics rather than outlets.

cited if the corresponding author is female (Maddi & Gingras, 2021). Women are confronted with higher standards in publishing compared to men (Hengel, 2022), a more hostile environment (Dupas et al., 2021; Wu, 2018), and they are less well connected in their discipline, looking at the count of women compared to men in the acknowledgments of published papers (Rose & Georg, 2021).

The remainder of this paper is structured as follows. Section 2.2 provides an overview of out event setting, the data and descriptive statistics. Section 2.3 explains our empirical strategy. Section 2.4 presents our findings and contextualizes their implications with broader economic theory on gender differences and group behavior. Section 2.5 concludes.

2.2 Background, Data, and Descriptive Statistics

Institutional background of the natural experiments: We study the so-called 'DEAL' contracts between academic publishers Springer Nature and Wiley and virtually all German academic institutions, as well as the failed negotiations with publisher Elsevier. The 'DEAL' contracts are so-called 'transformative' publication agreements that jointly encompass an approximated number of 19,000 annual publications in journals published by Springer Nature and Wiley.[2] The eponymous 'transformation' happens on the payment level. By and large, universities no longer pay for journal subscriptions but are charged a publication fee for every paper published by a scholar from that university. In return, it is published with open access by default. The DEAL contracts are so-called 'publish-and-read' agreements, as universities now only pay for publications of their own researchers, while they obtain access to the publishers' journal portfolios 'for free.'[3]

[2]See `https://esac-initiative.org/about/transformative-agreements/agreement-registry/wiley2019deal/` for the Wiley contract and `https://esac-initiative.org/about/transformative-agreements/agreement-registry/sn2020deal/` for the Springer Nature contract. Both websites were last checked on July 26, 2023. For more detailed descriptions and discussions of these contracts, see, e.g., Borrego et al. (2021), Haucap et al. (2021), and Machovec (2020).

[3]For an analysis of the fee setting by publishers, see (Schmal, 2023a).

Researchers at affiliated institutions have become eligible to publish their papers with open access in well-established subscription-based journals with an open-access option (so-called 'hybrid' journals) without any direct charges to the researchers themselves. While these journals account for the vast majority of outlets, the DEAL contracts also offer a 20% discount on the publication fees of full open-access journals, which are not covered by the fixed fee for hybrid journal publications. Open access exposes research to a potentially much larger audience. As the 'DEAL' concerns highly reputed journals and publishers, it avoids the problem of a potentially low reputation of pure open-access journals (McCabe & Snyder, 2005). Thus, publications of eligible authors should, ceteris paribus, shift towards the included journals – especially below the very top journals that are often widely accessible irrespective of the publishing model.

While Wiley and Springer Nature concluded DEAL agreements beginning in 2019 and 2020, respectively, the negotiations with Elsevier were suspended in the autumn of 2016. As a consequence, 74 institutions in Germany canceled their Elsevier journal subscriptions. After a short break, Elsevier continued granting access to researchers from institutions without contracts. At the end of 2017, a further 110 German institutions terminated their contracts. Again, Elsevier continued to provide access to its journals. The conflict ended with the German research alliance announcing its withdrawal from the negotiations in July 2018.[4] In turn, Elsevier cut off all institutions without a contract from access to its journals (Borrego et al., 2021). It affected researchers working at German institutions directly and potentially indirectly via the negative publicity caused.[5]

Fraser et al. (2023) have conducted a descriptive analysis of this cut-off and found

[4] `https://www.hrk.de/press/press-releases/press-release/meldung/deal-and-elsevier-negotiations-elsevier-demands-unacceptable-for-the-academic-community-4409/`, published July 5, 2018, last checked August 16, 2023.

[5] Examples of leading Elsevier journals in economics are the *Journal of Financial Economics* or *Research Policy*. Among the top 100 journals of the past decade ranked by RePEc, Elsevier published 42 of them, see `https://ideas.repec.org/top/top.journals.all10.html`, using the version of August 4, 2022, the list is subject to change as it functions on a rolling basis.

fewer papers from German authors were published in Elsevier journals. However, Fraser et al. (2023) do not apply econometric causal inference methods and neglect the gender dimension. Furthermore, a surge in uncertainty regarding the permanent availability of the papers may have possibly been another driver of this behavior. Hence, the actual treatment may have occurred through a change in expectations. Last, Elsevier continued to publish research behind a subscription paywall by default.

We utilize these two distinct events to obtain more general evidence on the impact of gender and coauthors on publication behavior. Via the benefits of the DEAL agreements, Springer and Wiley journals became more attractive for authors from Germany (vis-a-vis journals from all other publishers including Elsevier), as authors have been able to publish their papers open access without direct charges to them.[6] Furthermore, they have no hassle costs as the billing procedure is organized by the universities in the background. In stark contrast, Elsevier journals became less attractive for German authors (not only vis-a-vis Wiley and Springer but compared to all other publishers), as articles in Elsevier journals became more difficult to access for other scientists based in Germany.[7]

These changes in the attractiveness of journals of the three affected publishers are arguably exogenous. We draw more general insights from the analysis of these events as the change in publication incentives ties in with behavioral differences mentioned at the beginning of this paper. More risk-averse individuals might avoid Elsevier journals, especially when expecting more countries to follow Germany's example of

[6] A peculiarity of our choice of the discipline is that even though Elsevier, Springer Nature, and Wiley are the leading publishers measured by the number of journals they host, university and society presses dominate the very top of the list. Among the well-known 'top 5' journals, none is hosted by one of the three publishers; among the top 20 journals as listed by RePEc, only two journals are published by Elsevier, and one by Springer Nature, see `https://ideas.repec.org/top/top.journals.all10.html`, last checked July 26, 2023, the list is subject to change. Nevertheless, the three publishers are dominant among the journals that make up the majority of publications, namely top and mid-tier general interest and field journals.

[7] A potential way to circumvent these access hurdles, though, is the use of predatory repositories such as 'sci-hub' that store digital copies of published articles regardless of the occurring copyright infringements.

canceling subscriptions. In contrast, free open access makes journals covered by the DEAL more attractive as papers published there can reach a much larger audience. Furthermore, publishing with an open access license contributes to the public good of open science, another dimension where men and women have been found to differ (see, e.g., Andreoni & Vesterlund, 2001).

Construction of the dataset: We have built a dataset of scientific publications in economics from 2015 to 2022, consisting of three parts. First, we formed a set of journals to be included in our analysis. We used the 'SCImago' journal rankings, a comprehensive database of the SCImago Lab that lists and ranks thousands of academic journals across disciplines. We began with all journals assigned to the category 'economics, econometrics, and finance' in 2021.[8] To the best of our knowledge, the SCImago database is the largest and most comprehensive journal database publicly available. Still, the topic clusters of SCImago are slightly fuzzy. To tackle type I errors, we manually removed journals that are – according to their description of their aim and scope – a bad fit for an analysis of economics journals.[9]

For type II errors, we additionally made use of the journal ranking of the RePEc ('Research Papers in Economics') database, which disseminates working papers and publications in economics. We used the aggregate ranking for the last ten years to ensure we use journals currently important for the discipline.[10] We compare the top 200 journals with the SCImago list and add important missing journals. Technically, we also exclude journals listed by SCImago but without an assigned SCImago Journal Rank (SJR) value. Overall, we cover a set of 986 journals, which encompasses a broad range of economic policy in specific domains, finance, management, as well as

[8]See `https://www.scimagojr.com/journalrank.php?area=2000&year=2021`, last checked August 16, 2023.

[9]We did this very carefully and only removed fully misplaced journals. Thus, our database also includes journals only partially related to economic questions. This is to ensure we cover the discipline in a broader sense, i.e., also researchers and institutions that do not belong to the leading Western economics departments in terms of geographic location but also their research agenda.

[10]IDEAS/RePEc Aggregate Rankings (Last 10 Years) for Journals: `https://ideas.repec.org/top/top.journals.all10.html`. Rankings were accessed in June 2022.

social science or sustainability issues related to economic policy. The vast majority use English as a language, but a few outlets also publish articles in other languages such as French or Spanish.[11]

Based on that list, we accessed the Scopus database via the 'pybliometrics' library for Python of Rose and Kitchin (2019).[12,13] We combine the article metadata with one-year lagged ranking scores using each journal's Scimago Journal Rank (SJR), as mentioned before. This was done as many authors tend to have some ranking of the outlets they want to submit to in their minds. The most recent ranking we consider is one year before publication.[14] For example, all publications from 2022 get assigned the SJR value of their outlet from 2021. Thereby, we can add an impact or reputation measure of the journal to every publication. The SJR has become an accepted quality measure that is highly correlated with a journal's H-Index (Braun et al., 2006) or the 'Journal Impact Factor' (Ahlgren & Waltman, 2014; Guerrero-Bote & Moya-Anegón, 2012). In contrast to the 'journal H-Index,' the SJR has more inter-temporal variation as it can also adjust downwards over time.

Our analysis mainly focuses on behavioral differences by gender, alone (single authors) and in groups. We apply the *Namsor Gender Guesser* algorithm which utilizes artificial intelligence to compute the probability of a person's gender based on their first name by considering an extensive library of country-specific, alphabetical, regional, and ethnic information. For example, it has been proven reliable by Sebo

[11] A full list of included outlets is available as supplementary material.

[12] Based on our Scopus data, we are only able to study the likelihood of being published, which is not only dependent on the authors but also, e.g., editorial decisions or referee behavior. An analysis based on submissions would be cleaner. However, this is not part of our data and would likely cover only a few journals if one gets hands with such data. As we study journal choice, conference submissions or working paper repositories are no remedy in our setting. Given that editorial boards are independent, we also do not suspect any effect driven by them.

[13] Scopus is considered one of the leading bibliographic databases besides Clarivate's 'Web of Science' and Google Scholar. The latter cannot be (legally) accessed to receive large-scale publication data. Compared to Web of Science, Scopus includes a more extensive set of publications in journals (Baas et al., 2020; Visser et al., 2021). A new alternative to collecting bibliometric data is the 'Dimensions' database that also encompasses data on grants and patents (Hook et al., 2018).

[14] This might collide with submissions that take several years to be published within a journal. As the rankings are correlated over time, we consider any potential distortion as negligibly small.

(2021).[15] We predict the gender of each author and use a cutoff value of 70% calibrated probability. Based on that, we define male, female, and mixed teams.[16]

#Authors	Frequency	Share	Cum.
1	82,088	24.85%	24.85%
2	105,104	31.82%	56.67%
3	85,363	25.84%	82.51%
4	39,258	11.88%	94.39%
5	11,131	3.37%	97.76%
6	3,804	1.15%	98.91%
7	1,543	0.47%	99.38%
8	752	0.23%	99.61%
9	449	0.14%	99.74%
10	246	0.07%	99.82%
>10	599	0.18%	100%
Total	330,337	100%	

Table 2.1: Number of authors in the raw sample

We restrict our sample to papers with one to four authors, accounting for the lion's share of 94.39% of our observations as displayed by Table 2.1. More than half of our starting sample is either single-authored or coauthored by two researchers. Accordingly, the median number of authors is 2, while the mean is 2.47. The trade-off between accuracy and tractability is, in our view, best solved with a sample of 1-4 authors. The core advantage of limiting the size of author teams is limiting the combinations of mixed-gender teams. For example, there can be one to four women in a mixed group of five researchers. We conjecture that omitting publications with five or more authors is not problematic, as every additional author reduces the likelihood that we can identify the genders of *all* authors of a paper, such that adding publications with a high number of coauthors is likely to contribute only a few additional observations for which we know every author's gender with a sufficiently high probability.

[15] It is also used by the platform RePEc to track the share of female economists, see 'The RePEc Blog' entry on March 7, 2022: `https://blog.repec.org/2022/03/07/2378/`, last checked August 17, 2023.

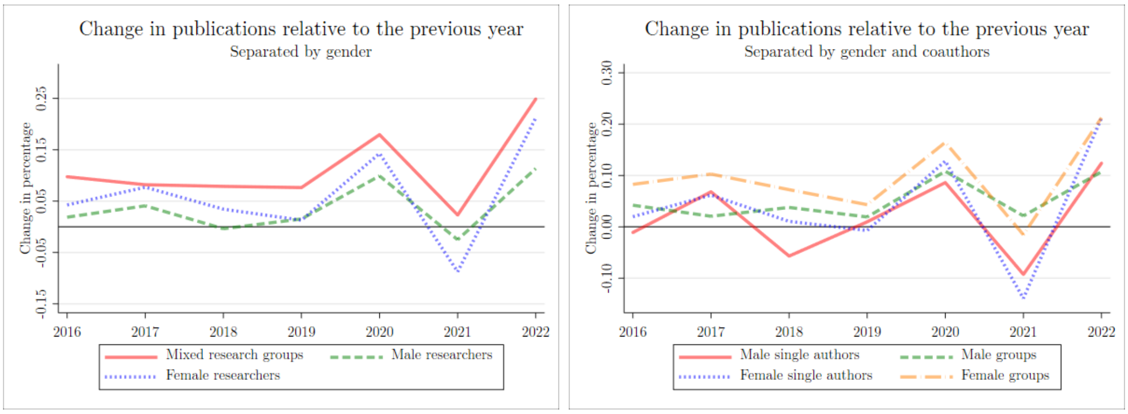
[16] We acknowledge that there may be authors with a non-binary gender in our sample. We would take this into account. However, the employed name algorithm does not allow for that.

We focus on scientific and review articles. Table 2.5 in Appendix A shows that these two types account for 94.15% of all observations. Furthermore, the DEAL contracts encompass only these two types of articles. Neglected are, among others, book reviews, and editorials. In total, we start with 330,337 papers. Narrowing the data to articles and reviews, we still have 311,015 observations.[17] Hence, we consider this to be a substantial sample for the time covered.

Regarding the identification of names, the spread between 50% and 70%, together with the inability of the *Namsor* algorithm to identify some of the names, leads to an inevitable decrease in the number of observations. The most important reason for the drop to 243,375 observations is that often only the initials of an author's first name are registered. It makes gender identification via the name close to impossible. As Table 2.3 further below and Table 2.6 in Appendix A show, missing values appear to be spread equally across years and publishers (in relative terms), which is reassuring that the loss due to missing first names does not induce any biases. Additionally, inspecting the distribution of papers across journals with different impact factors in the final dataset highlights that the distribution of the whole dataset as well as the one with gender information, is congruent, as Figure 2.12 in Appendix A highlights.

Descriptive statistics: Generally, the number of publications steadily grows over time (Bornmann et al., 2021). Figure 2.1 confirms this for our sample. The two panels display the annual growth in publications separated by gender on the left and between single authors and groups for both genders on the right. As Figure 2.1 shows, there has been growth for all three gender groups except for the year 2021, which has been amid the COVID-19 pandemic. In addition, male single authors had fewer publications in 2018 than in 2017. Furthermore, a trend towards collaboration is obvious given the persistently higher growth rates in publications of mixed teams.

[17] This sample has an average author count of 2.49. Restricting it to 1-4 authors covers 94.42% of the observations.



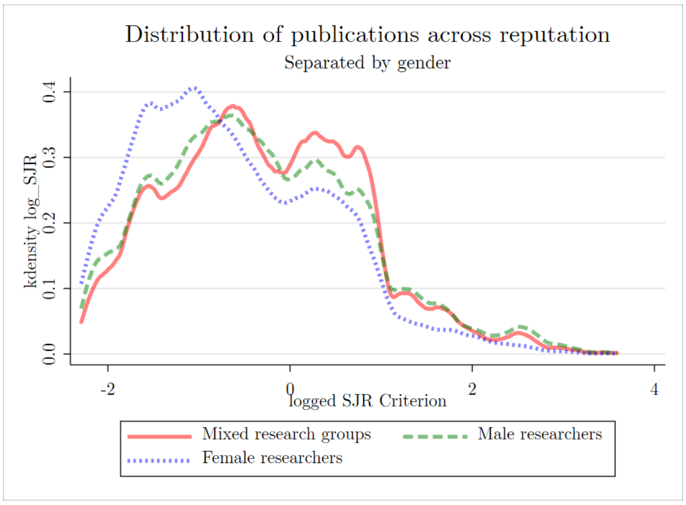
Annual growth rates in the number of publications relative to the previous year distinguished by gender groups (LHS) and the presence of coauthors for single gender groups (RHS). Mixed gender research groups are not displayed in the right panel by construction. N=311,015.

Figure 2.1: Publications over time by gender

In terms of journal reputation, there are notable differences. Using the logarithmic SCImago Journal Rank criterion, one can see in Figure 2.2 that the distribution of all female research groups has a much higher density in lower parts of the logged SJR range. Completely male as well as mixed teams have a higher share of publications from a logged SJR value of approximately -0.75, which is equivalent to an SJR value of 0.47. Examples of journals with such a rating (in 2021) are the *Review of Financial Economics* or the *International Journal of the Economics of Business*. It corresponds to #479 and #481 in the SCImago ranking based on the SJR criterion. For comparison, the RePEc ranking lists them at #393 and #450 using the aggregate rankings for journals.[18] In turn, it implies that virtually all journals relevant to a career in academic economics are above this threshold.

As men and women tend to prefer different subfields, which are also related to varying citation counts (Maddi & Gingras, 2021), a publication in a top-field journal predominantly studied by men might receive more citations than a more female-influenced top-field publication. Of course, there exist more precise and nuanced metrics than plain citation comparisons to evaluate publications, which come along with their own challenges, but tend to be more informative (Waltman, 2016;

[18]See `https://ideas.repec.org/top/top.journals.all.html`, last checked January 19, 2023.



A higher SJR value implies a better journal ranking. $N = 311{,}015$.

Figure 2.2: Distribution of publications across quality by gender

Waltman & van Eck, 2013; Waltman et al., 2011). While women are underrepresented in most journals that generate a vast number of citations, this does not imply that publications by single female researchers or entirely female teams are less important compared to those of their male colleagues, even though they may be published in less influential outlets, looking through the (distorted) lens of a purely citation-based evaluation. In contrast, evaluating research in alternative ways, as suggested by the 'Leiden manifesto' (Hicks et al., 2015), might lead to a better comparison of male and female research output.

	National Collaborations			Intl. Collaborations			Single authors		Total
	Count	% Total	% Collab	Count	% Total	% Collab	Count	% Total	
Mixed	18,584	18.99 %	(18.99%)	79,303	81.01 %	(81.01%)	–	–	97,887
Male	18,122	15.52 %	(26.38%)	50,587	43.34 %	(73.62%)	48,022	41.14 %	116,731
Female	2,955	10.28 %	(25.07%)	8,834	30.72 %	(74.93%)	16,968	59.00 %	28,757
Total	39,661	16.30 %	(22.23%)	138,724	57.00 %	(77.77%)	64,990	26.70 %	243,375
	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)

'% Total' represents the share of each count as a fraction (measured in percent) of the total number for each gender group, i.e., each counted in column (2), (5), and (8) weighted by the total number presented in column (10). '% Collab' is the share of each count as a fraction (measured in percent) of all collaborations for each gender group, i.e., each counted in column (2) and (5), weighted by the sum of coauthored papers, namely the sum of columns (2) and (5).

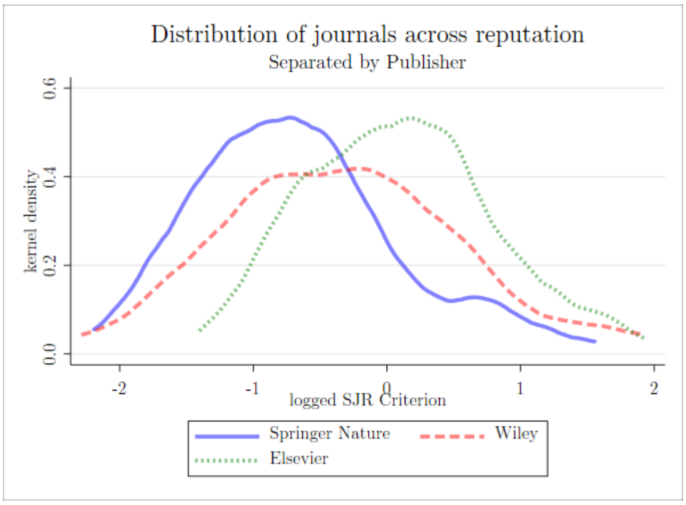
Table 2.2: Number of papers by gender and the type of collaboration

Another driver of differences may be differences between male and female researchers with respect to international collaborations, if international teams exhibit different publication patterns from national ones. Table 2.2 shows this distinction in the upper part by distinguishing national and international teams and, in addition, single-authored papers, as those cannot be collaborative ex vi termini. Looking solely at coauthored papers, one can see that the share of international collaborations among coauthored papers does not differ substantially between men and women (see the percentage shares in brackets). For both genders, approximately one quarter (26.38% for men, 25.07% for women) of the publications authored by teams stem from domestic collaborations, while 3/4 are published by international teams (in terms of affiliations). However, female economists tend to write relatively more single-authored papers than their male colleagues (column 9). We conclude that gender differences are unlikely to be driven by differences in international collaborations but rather by differences in single vs. joint authorship and gender composition of research groups.[19] In column (10) of Table 2.2, one can also see variation in gender representation in the discipline. Publications of purely female teams only account for about a quarter of the publications of purely male teams (both include single-authors).

As the setting of our analysis uses publishers as leading subjects of analysis, their journal portfolios are essential. Figure 2.3 uses the logarithmic SJR criterion again and plots the empirical densities of the publishers Springer Nature and Wiley (which are part of the DEAL) and Elsevier, which left the negotiations and later cut off German research institutions. One can see that Wiley has a larger representation at the top of the distribution (on the right side) than Springer Nature. Elsevier, however, exceeds both of them in terms of journal reputation in economics.

Looking at the market structure in economics publishing, presented in Table 2.3, one can see that the 'big 3' Elsevier, Springer Nature, and Wiley account for almost

[19] We further test this econometrically. In Appendix A, we elaborate in detail on that issue and show that differences in collaborations across gender groups do not drive our results.



Distribution of the weighted average logarithmic SJR criterion per journal weighted by the number of publications per year in each journal, i.e., for each journal j with N_{jy} in each year $y \in [2015, 2022]$, we compute $SJR_j = \frac{1}{N_j} \sum_{y=2015}^{y=2022} SJR_{jy}$ as the SJR varies per year. $N =$ 311,015.

Figure 2.3: Distribution of journals across quality by publisher

half of all published articles in our sample. The next three publishers, Taylor & Francis, Emerald, and Routledge, jointly account only for a slightly higher share than Springer Nature alone. Furthermore, none of these six publishers is a society, university owned, or a fully open-access publisher.

	all observations		**obs. with gender ident.**	
Publishers	Frequency	Share	Frequency	Share
Other	118,548	38.12 %	93,195	38.29 %
Elsevier	85,547	27.51 %	64,156	26.36 %
Springer Nature	36,883	11.86 %	29,110	11.96 %
Wiley	31,194	10.03 %	25,809	10.60 %
Taylor & Francis	15,652	5.03 %	12,079	4.96 %
Emerald	12,353	3.97 %	10,123	4.16 %
Routledge	10,838	3.48 %	8,903	3.66 %
Total	311,015	100 %	243,375	100 %

Table 2.3: Publications by publisher with and without missing gender identification

2.3 Empirical Strategy

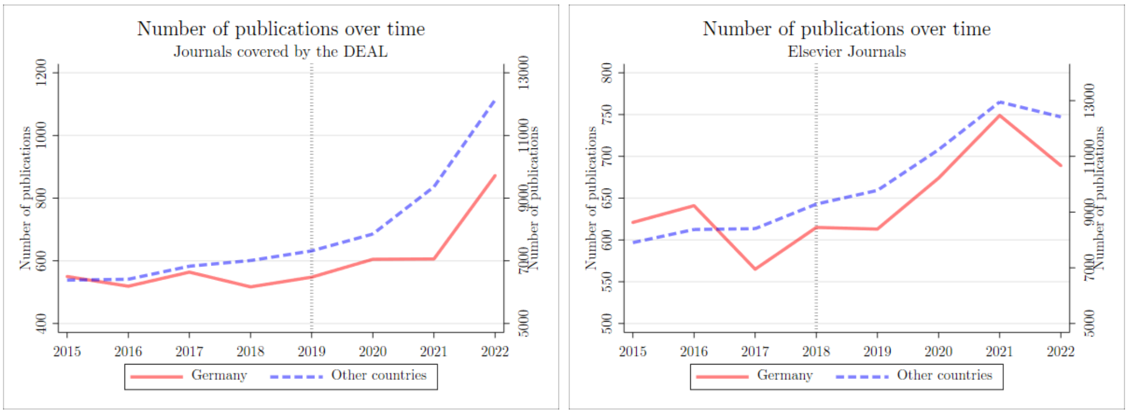
We employ a difference-in-differences design using a linear probability model. The dependent variable is a binary indicator whether or not a paper is published in a journal (later) covered by the DEAL or published by Elsevier.[20]

We set the point of treatment for the DEAL contracts on 1 July 2019, when the bulk of the Wiley journals became part of the contract. Only slightly later, on 1 January 2020, the hybrid journals of Springer Nature were included in the 'DEAL' as well. For the academic brawl with Elsevier, we use 5 July 2018 as treatment day, as this was the day the German research alliance announced the suspension of all negotiations with that publisher. An objection to this approach may be the long paper turnaround times in economics (Hadavand et al., forthcoming). However, shifting the point of treatment forward in time would lead to non-negligible arbitrariness. Furthermore, it does not capture that the knowledge of the contract's benefits disperses spontaneously and unfocused among researchers. In addition, having three post-treatment years in our data, we are confident of picking up the majority of publishing delays.

Our identification rests on the arguably unanticipated cut-off from access to Elsevier journals as well as the introduction of the DEAL conditions. It appears unrealistic that researchers actively sought employment at German institutions due to this changed environment. One objection might be that the Elsevier conflict existed before German institutions were locked out. Put differently, the academic publishing market is in motion, and other developments may be taking place in parallel. Figure 2.4 shows the number of publications by authors from Germany and all other institutions. The left panel shows the number of papers for journals covered by the DEAL, and the right one those in Elsevier outlets. The numbers are

[20]Even though this setting may suggest a logit or probit estimation (as in Haucap et al., 2021), the linear probability model is well-suited for the reliable estimation of marginal effects (Heckman & Snyder, 1997).

scaled differently to allow for a better visual comparison.



Left panel: DEAL journals, right panel: Elsevier journals. In both panels, the left y-axis shows the number of publications by German authors, the right one those by authors from all other countries. The dotted vertical lines in both plots mark the year in which the treatment event happened. N = 311,015.

Figure 2.4: Number of publications over time differentiated whether coming from Germany

One can see that the trends for the DEAL journals follow the same path. For Elsevier, this holds as well for the years from 2017 on. Beforehand, we see a drop from 2016 to 2017 for German authors that is not reflected in the overall development apart from Germany. To validate that this anomaly does not affect our analysis, we compute the results for our core finding on gender differences in publications in Elsevier journals excluding the years 2015 and 2016. The results remain qualitatively unchanged.[21]

To preserve sufficient statistical power and since we want to study gender differences, we focus on a canonical pre/post difference-in-differences setting as a year-by-year analysis of behavioral patterns may not be able to add anything to the analysis, in our case of publication behavior. The outcome of interest on the left side is the probability of a paper i to appear in a journal covered by the DEAL (i.e., Springer Nature or Wiley) or else in an Elsevier outlet. Precisely, we measure whether more papers in the treated journals are (co)authored by researchers from Germany. Assuming that there is no parallel shock in the quality of the submissions from this

[21] See Tables 2.13 and 2.26 in Appendix A and B, respectively.

group, it implies that these researchers submit more often to the affected outlets. Ideally, we would like to measure submissions. However, no data is available, and we consider our setup a sufficiently good approximation.

The dependent variable $\mathbb{1}_i^{Publ.}$ is a binary indicator that can take the values $\{0, 1\}$, whether a paper i is published in a journal of a treated publisher ($\mathbb{1}_i^{Publ.} = 1$) or not ($\mathbb{1}_i^{Publ.} = 0$). The treated publisher is either Elsevier ('Els') or Springer Nature and Wiley jointly (DEAL). The variable $\mathbb{1}^{GER}$ is an indicator for an affiliation with a German institution.[22] $\mathbb{1}_i^{DiD}$ is the difference-in-differences (DiD) indicator, i.e., $\mathbb{1}_T \times \mathbb{1}_i^{GER}$. X_i' are covariates that are added without any interaction terms, namely gender and SJR.

Our analysis is based on an involved difference-in-differences model that allows for interactions of covariates with the plain treatment effect.

$$\begin{aligned} \mathbb{1}_i^{Publ.} = g_i + SJR_i + g_i \times SJR_i + \mathbb{1}_i^{GER} + \mathbb{1}_i^{DiD} + \mathbb{1}_i^{DiD} \times g_i \\ + \mathbb{1}_i^{DiD} \times SJR_i + \mathbb{1}_i^{DiD} \times g_i \times SJR_i + y_i + m_i + \epsilon_i \end{aligned} \tag{2.1}$$

Again, the dependent variable of eq. (2.1) is the binary indicator $\mathbb{1}_i^{Publ.} \in \{0, 1\}$ that captures whether a paper is published by a treated publisher or not. On the right, g_i represents gender and SJR_i the SCImago journal rank, measured in quartiles to enable the interaction terms, i.e., it captures the respective quartile of the journal distribution in which the journal is placed, which in turn has published paper i. We interact gender and reputation (measured by the SJR quartile) to account for the differences in publication behavior shown in the previous section. Here, quartile 1 implies the lowest reputation as the SJR increases in quality. y_i and m_i describe the time fixed effects for the year and the month of publication i (as the treatments happen within a year). In this specification, we rely on the established two-way fixed effects differences-in-difference design, as the correction models do not allow

[22]The affiliation of the corresponding author must be in Germany. If the the paper is coauthored by someone from a German institution, but he or she is not the corresponding author, the paper is not eligible to the DEAL conditions. For the Elsevier cut-off, we do not have this binding constraint. To have a consistent set-up, we stick to the approach of only coding those papers 'German' for which at least the corresponding author is affiliated with a German institution.

for treatment interactions.[23]

A peculiarity of modeling the dependent variable as a binary indicator whether a paper is published in a journal of a treated publisher is that the choice for one journal is, inevitably, a choice against all other outlets. As a robustness check, we exclude the years 2020 onwards from the analysis of the Elsevier cut-off to account for the DEAL introduction and find the results to be qualitatively the same. When excluding Elsevier publications from the analysis of the DEAL treatment, we observe qualitatively different results. However, it is not a weakness of our design but evidence for the direction of the switch away from Elsevier towards Springer and Wiley journals.

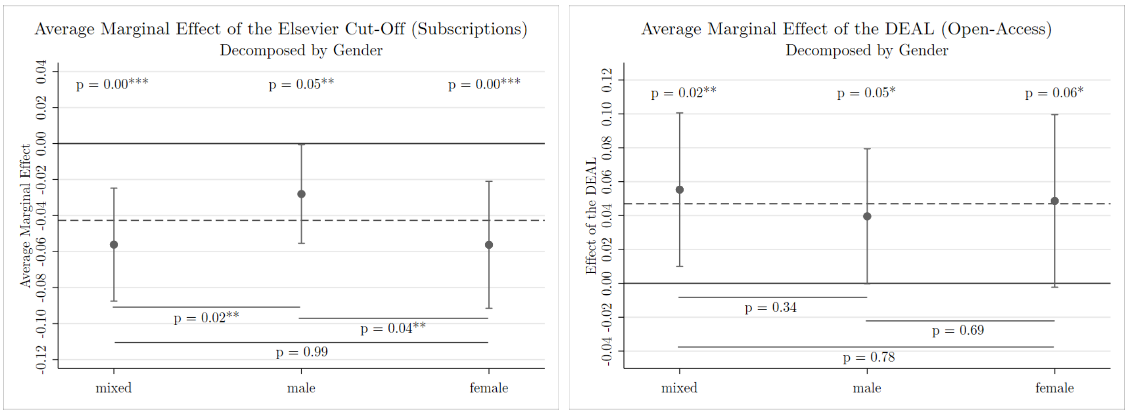
Last, our time window for the analysis includes the global COVID-19 pandemic. There are not only direct effects on productivity by the infection (Fischer et al., 2022) but public measures against COVID-19 also induced a bigger wedge in the academic gender gap. Women's and, in particular, mothers' research was significantly reduced due to an increase in care work necessary due to closed kindergartens and schools (Deryugina et al., 2021b; Ucar et al., 2022). This may have led to two different reactions: Female researchers may have reduced their number of projects, or they maybe reduced the effort devoted to their projects. In both cases, our empirical setting using difference-in-differences nets out these changes, as they generally affected women in academia, no matter whether they work in Germany, France, the UK, the US, Scandinavia or elsewhere. In contrast, the DEAL treatment and the Elsevier cut-off only affected female (and male) researchers in Germany. Hence, any effect of COVID-19 should appear in both the treatment group (Germany) and the control group (elsewhere), such that, by construction, it does not affect our results, as we do not simply conduct a before-after analysis, but employ a difference-in-differences design.

[23]Nonetheless, we compute a doubly robust difference-in-differences correction specification for a simplified regression model. The results are presented in Appendix A in Tables 2.9 (DEAL) and 2.10 (Elsevier).

2.4 Results

2.4.1 Decomposition by Gender

In the first step of our analysis, we study gender differences in response to varying publication incentives, namely the disadvantage of the cut-off of all German research institutions from recent Elsevier publications as well as the benefit of frictionless open-access offered by the DEAL to authors of German institutions. Figure 2.5 presents the decomposition of the average marginal effects (AME) by gender for both events. We cluster the authors in this first step into entirely male, entirely female, and mixed-gender research groups. The first and second groups include both author teams and single authors. The dashed lines in Figure 2.5 represent the average marginal effects of the events on aggregate. In general, economists in Germany turned towards DEAL journals (average effect of +4.69%) and away from Elsevier outlets (average effect of −4.27%). For the cut-off from Elsevier, depicted in the left panel, we find negative effects for all three gender groups, but those of female and mixed-gender teams are twice as large than the reaction of men. Both the reactions of mixed teams and of females significantly differ from the behavior of men.



Dep. Var.: $\mathbb{1}_i^{DEAL}$ using eq. (2.1). Standard Errors heteroskedasticity-robust and clustered on the journal level. N = 243,375. Left panel: Effect of the Elsevier cut-off. Right panel: Effect of the DEAL introduction. Dashed lines: Plain average effects of the events regardless of gender. See Table 2.18 in Appendix B for details on the estimates for the DEAL and Table 2.19 for details on the estimates for Elsevier.

Figure 2.5: Marginal effects of the varying publication incentives decomposed by gender

Regarding the positive publication incentive of the DEAL, the marginal effects for all three gender groups are similar in their size. The coefficient for mixed groups, in which the corresponding author works in Germany, is significant at the 5% level. We find a slight violation of this statistical threshold for male and female teams. As there are fewer observations, especially for women, we still consider this somewhat explanatory. The coefficients do not differ significantly, as all point estimates are close to the aggregate effect. The general dynamics seem to be partially driven by a shift away from Elsevier outlets. The average marginal effect diminishes and becomes wholly insignificant after excluding Elsevier outlets from the regression.[24] We address this with our complementary analysis of the Elsevier cut-off since the findings suggest that the move towards the DEAL journals is, at least to some extent, fueled by the move away from Elsevier.

An objection to the presented findings might be the already discussed inherent disadvantage of our setting: Publishing a paper in one journal implies that it cannot appear in any rival journal. In the case of the DEAL treatment, it implies that any paper additionally attracted by the enhanced publication conditions corresponds to one paper less somewhere else, e.g., in Elsevier journals. Hence, an adverse effect for Elsevier might be just technically caused by the pull factor of the DEAL. Therefore, we conducted a separate analysis restricted to 2015 to 2019. This period has only a slight overlap with the DEAL, which should be negligibly small given the publication lag.[25] The results remain qualitatively the same even though we lose 43.7% of the observations. The effect for females is even slightly higher in absolute terms, now amounting to a decrease of -5.9% and highly significant ($p = 0.015$). Thus, not only the baseline effect but also the gender patterns are already present and robust in the short run. In addition, in this robustness check, the estimate for male researchers

[24] The results for this robustness check are presented in Table 2.11 in Appendix A.

[25] Table 2.12 in Appendix A provides all results for the marginal effect on aggregate, as well as the decomposition by gender. In an additional robustness check presented in Table 2.13 in Appendix A, we exclude the years 2015 and 2016 from the regression for the Elsevier cut-off because Figure 2.4 displays a decrease in the number of Elsevier publications from German corresponding authors between the years 2016 and 2017. The results remain qualitatively the same.

becomes insignificant. It is further evidence that men behave differently.

Last, we run a robustness check, omitting the publications from July to December 2019, as Springer Nature entered the DEAL only in January 2020. The aggregate baseline effect is virtually the same, and the decomposition by gender varies very slightly.[26] Furthermore, we compute robustness checks for higher gender accuracy cut-offs, namely for 80% and 90%. The marginal effects are qualitatively the same.[27]

2.4.2 Decomposition of the Mixed Gender Groups

To deeper analyze the gender gap, we decompose the mixed-gender group further. In particular, we split it into three subgroups that capture a male majority, equal representation, and a female majority. We define a group of researchers as 'mostly' female or male in case 2 out of 3 or 3 out of 4 persons of a team have arguably the same gender as computed by our name-matching algorithm. In the 'equally mixed' group remain teams with equal shares of genders (1:1 in two-person teams and 2:2 in four-person teams). The entirely male and female groups remain unchanged. Table 2.4 shows the distribution across these five groups. This exercise bears the additional challenge of partially identified research groups. Take, for example, a group of three, in which one author is female, one is male, and for one, we cannot identify the gender with a probability $> 70\%$. Until now, we have assigned such a group to the 'mixed' category, but now the third gender matters. To avoid lowering the bar, we code these publications as 'unidentified' and restrict our analysis to those publications in which all gender probabilities exceed the 70% threshold. We lose 20,529 or 8.46% of our observations.[28]

Assuming a 'one person, one vote' principle within a team for the decision where to submit a paper, we can disentangle whether women in mixed teams are the

[26] See Table 2.14 in Appendix A for details.

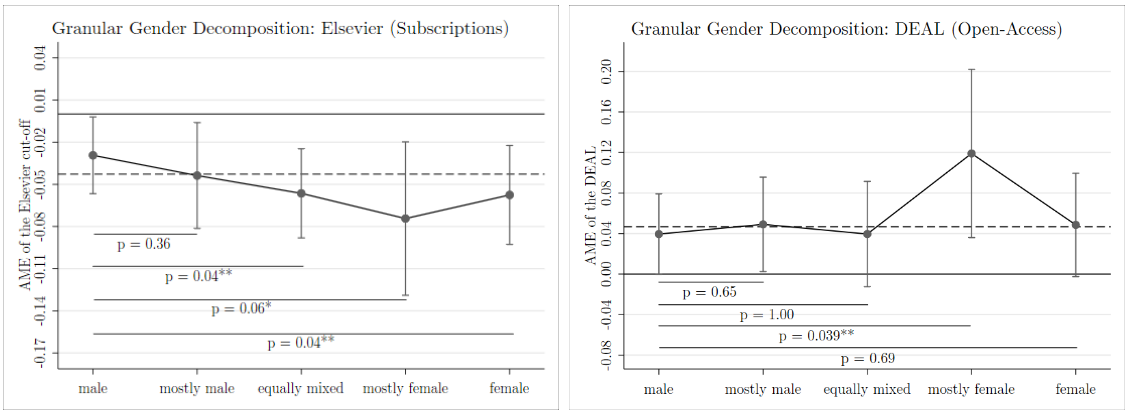
[27] The results are shown in Appendix A, Table 2.15 presents the results for the DEAL introduction and Table 2.16 those for the Elsevier cut-off.

[28] We have conducted robustness checks for the plain effects and the baseline gender decomposition excluding these unidentified observations. The results (see Table 2.17 in the appendix) are highly similar and qualitatively the same.

Gender Group	Frequency	Share	Cumul.
Unidentified	20,596	8.46 %	8.46 %
Fully Male	116,731	47.96 %	56.43 %
Mostly Male	33,868	13.92 %	70.34 %
Equally Mixed	30,552	12.55 %	82.90 %
Mostly Female	12,871	5.29 %	88.18 %
Fully Female	28,757	11.82 %	100.00 %
Total	243,375	100 %	

Table 2.4: Publications separated by the gender composition of the group of authors

driver of this significant negative effect.[29] Figure 2.6 plots the disentangled and sorted from fully male to fully female authorship for both the Elsevier cut-off (left panel) as well as the DEAL (right panel). Even though this is not a dynamic computation, connecting the point estimates for Elsevier on the left highlights the most interesting finding: A nearly steady downward slope the 'more female' a research group becomes. This effect is robust to the exclusion of the years 2015 and 2016, as mentioned beforehand and shown in Table 2.26 in Appendix B.



The left panel shows the marginal effects for the Elsevier cut-off (subscriptions), and the right panel shows those for the DEAL contracts (open-access). Dashed line: Aggregate average marginal effect, see Tables 2.19 (DEAL) and 2.18 (Elsevier) in Appendix B. Dep. Vars.: $\mathbb{1}_i^{Els}$ (LHS), $\mathbb{1}_i^{DEAL}$ (RHS) using eq. (2.1). Standard errors heteroskedasticity-robust and clustered on the journal level. 95% confidence bands. N = 222,779. Tables 2.22 (Elsevier) and 2.23 (DEAL) in Appendix B provide further details on the average marginal effects shown here.

Figure 2.6: Marginal effects of the granular gender decomposition

[29] Note that such an approach sets aside hierarchical structures such as junior researchers collaborating with a tenured professor, whose vote may have more influence in such a decision.

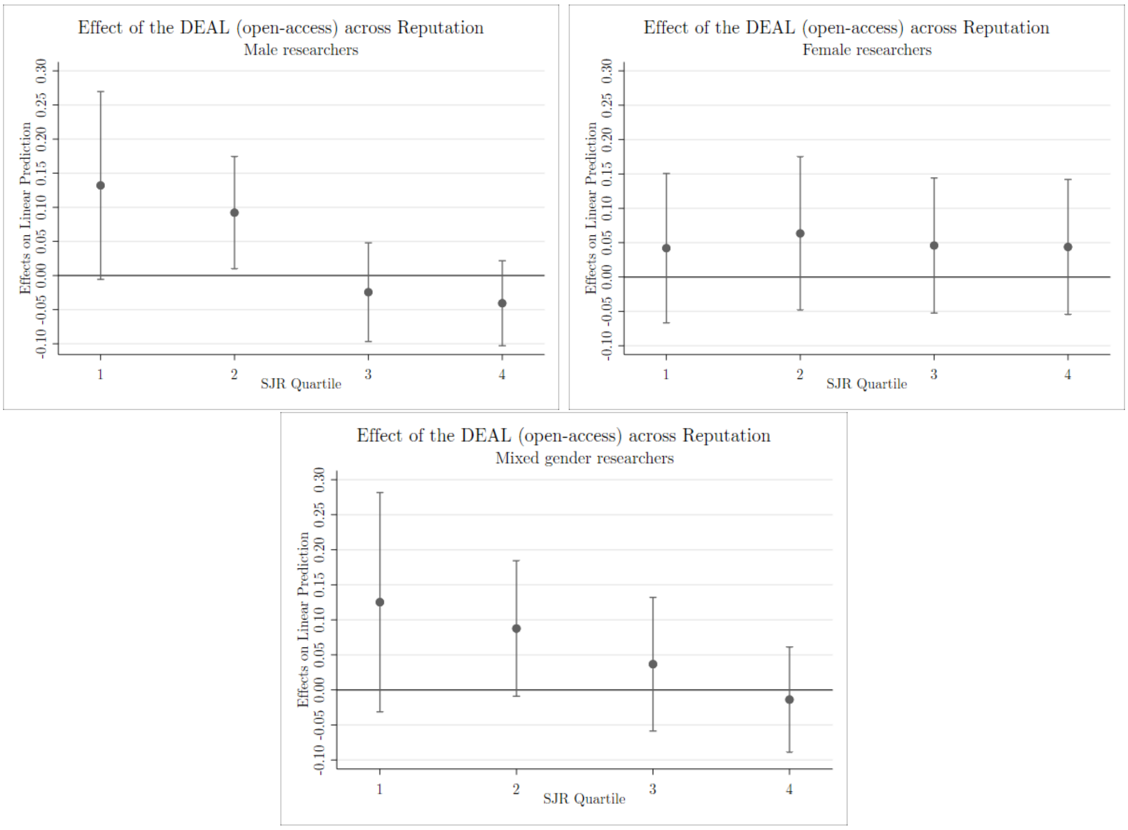
The estimate for entirely male teams is significantly negative but closest to zero. A minority of one woman within a research group of three or four members slightly lowers the point estimate. However, it is indistinguishable from the fully male estimate. Nevertheless, the effect for equally mixed teams is already statistically different from that for all-male teams. The same holds for mostly and entirely female teams. Thus, once men lose the numerical majority within a team, publication behavior significantly differs from fully male author groups concerning Elsevier.

Additionally, we have conducted this more granular gender decomposition for the DEAL contracts' positive incentive. The results are shown in the right panel of Figure 2.6. We do not see an inverse pattern but slightly higher point estimates for mostly and purely female teams – even though they do not differ statistically. Surprisingly, the coefficient for groups with a majority of women is the only one that differs significantly from all-male teams. The coefficients for entirely male and female authorships are both insignificant at the 5% level, but significant at the 10% level ($p_{male} = 0.051$, $p_{female} = 0.062$).

2.4.3 Decomposition by Gender and Reputation

In the third step of our analysis, we disentangle the separate gender effects across the SJR ranking criterion in response to varying publication incentives. We reapply the 'baseline' gender decomposition, which involves returning to the less detailed aggregation of mixed-gender author groups. Opting for a more granular approach involving double decomposition by gender and reputation would lead to a lack of statistical power. Therefore, we stick to the baseline method to maintain robustness in our results. Beginning with the positive publication incentive of the DEAL agreements, the upper two panels in Figure 2.7 show the choices of male and female researchers, both single authors and teams. The lower panel displays the effect for gender-mixed groups. The quartiles are increasing in quality. Thus, quartile 1 comprises the journals with the lowest impact (relying on the SJR), while quartile 4 contains the top 25%.

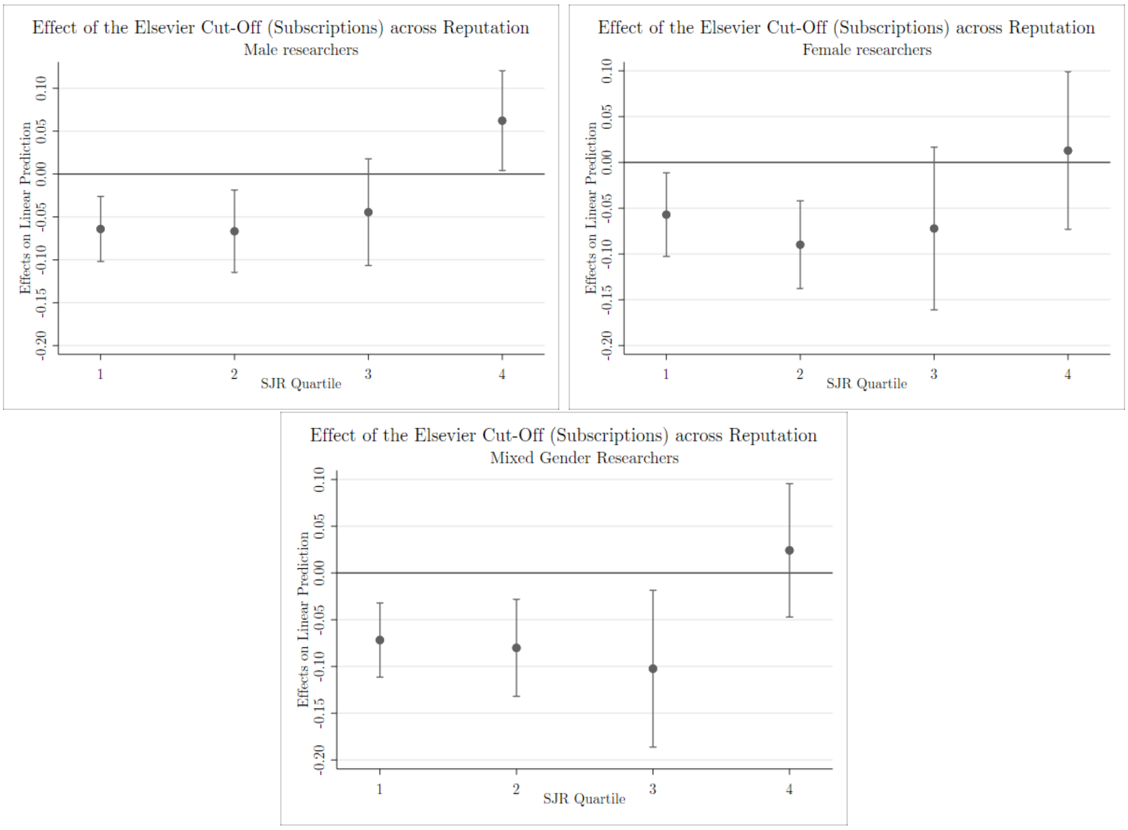
Looking at the panels in Figure 2.7, one finds only one coefficient being significantly positive for males in the second quartile, i.e., in lower ranked journals. For mixed research groups, the coefficient for the second quartile is significant at the 10% level ($t = 1.78$, $p = 0.076$). Women, in contrast, do not show a significant effect at all. Turning again towards males and mixed groups, which also consist partially of men, we find that they choose the free and easy open-access benefit of the DEAL journals for papers in journals with lower citation rates. These journals are typically less often subscribed by libraries around the globe. Therefore, open access to articles in such outlets may lead to a large increase in potential readership and citations compared to top journals, which are subscribed by libraries anyway.



Average marginal effects for each SJR quartile computed holding each of the three gender groups fixed. Dep. Var.: $\mathbb{1}_i^{DEAL}$ using eq. (2.1). Standard errors heteroskedasticity-robust and clustered on the journal level. N = 243,375. 95% confidence bands provided. Details are shown in Table 2.20 in Appendix B.

Figure 2.7: Effect of the DEAL (open-access) decomposed by gender and reputation

We conduct the same double decomposition in Figure 2.8 for the negative publication incentive of the Elsevier cut-off. We see for all three gender groups negative point estimates for the first two quartiles. It implies that researchers and research groups, regardless of their gender, opt out of lower ranked Elsevier journals. For mixed teams, we also identify a shift away in the second-best third quartile, while neither male nor female researchers nor research groups do show a reaction. For the top quartile #4, we cannot find a significant reaction among females or mixed groups. It implies that female researchers and research groups tend to opt out of Elsevier outlets for less sophisticated work and publish it elsewhere. For leading journals, their behavior remains unaffected.



Dep. Var.: $\mathbb{1}_i^{Els}$ using eq. (2.1). Standard errors heteroskedasticity-robust and clustered on the journal level. $N = 243{,}375$. 95% confidence bands provided. Table 2.21 in Appendix B details the estimates.

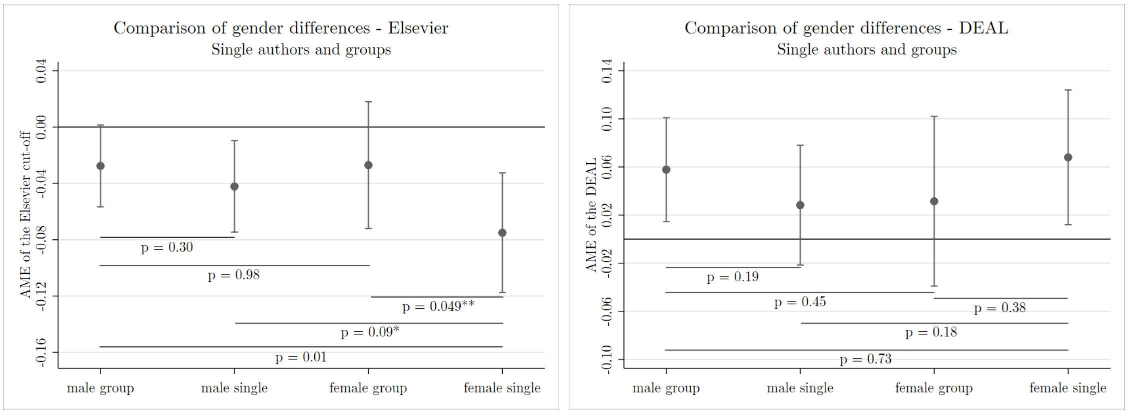
Figure 2.8: Elsevier Effect (subscriptions) decomposed by gender and reputation

Male researchers react somehow differently to the Elsevier cut-off, as the plain gender effect already suggested. While the overall effect turned out to be significant for male researchers even though it only amounts to half the size of the female effect in absolute terms, we find a move away from Elsevier for journals in the lower half of the impact distribution just as for females and mixed groups. Surprisingly, we detect a highly significant but *positive* effect of +6.2% ($p = 0.036$) on the highest ranked quartile for men. Its magnitude is larger than the baseline effect but in absolute terms. All things equal, male researchers from Germany have published more often in the highest-tier Elsevier journals after the cut-off. This may possibly be related to a higher perceived probability of being published in such an outlet, as the public debate might have suggested a shift away from Elsevier journals.

This behavior has important implications for gender differences in the discipline. More than in other scientific disciplines, economics has a highly convex valuation of journals, i.e., an extreme emphasis on the so-called ‘top 5’ journals and, then, the top field journals, as well as a notable wedge between an outlet’s reputation and its relevance (Heckman & Moktan, 2020; Haucap & Muck, 2015). Any shift away from reputed journals may be related to a loss in recognition of one’s own work.

2.4.4 Decomposition by Gender and Reputation: Single Authors Compared to Teams

In the last step of the analysis, we look at groups and single authors within the two single-gender categories for both events. Figure 2.9 shows the results for the Elsevier cut-off in the left panel and those for the DEAL introduction in the right panel. In both cases, the two left (right) coefficients display the difference between male (female) single authors and author groups. Other than for the granular decomposition by gender, we also find differences for the introduction of the DEAL contracts, but now distinguishing between single authors and author groups of the same gender.

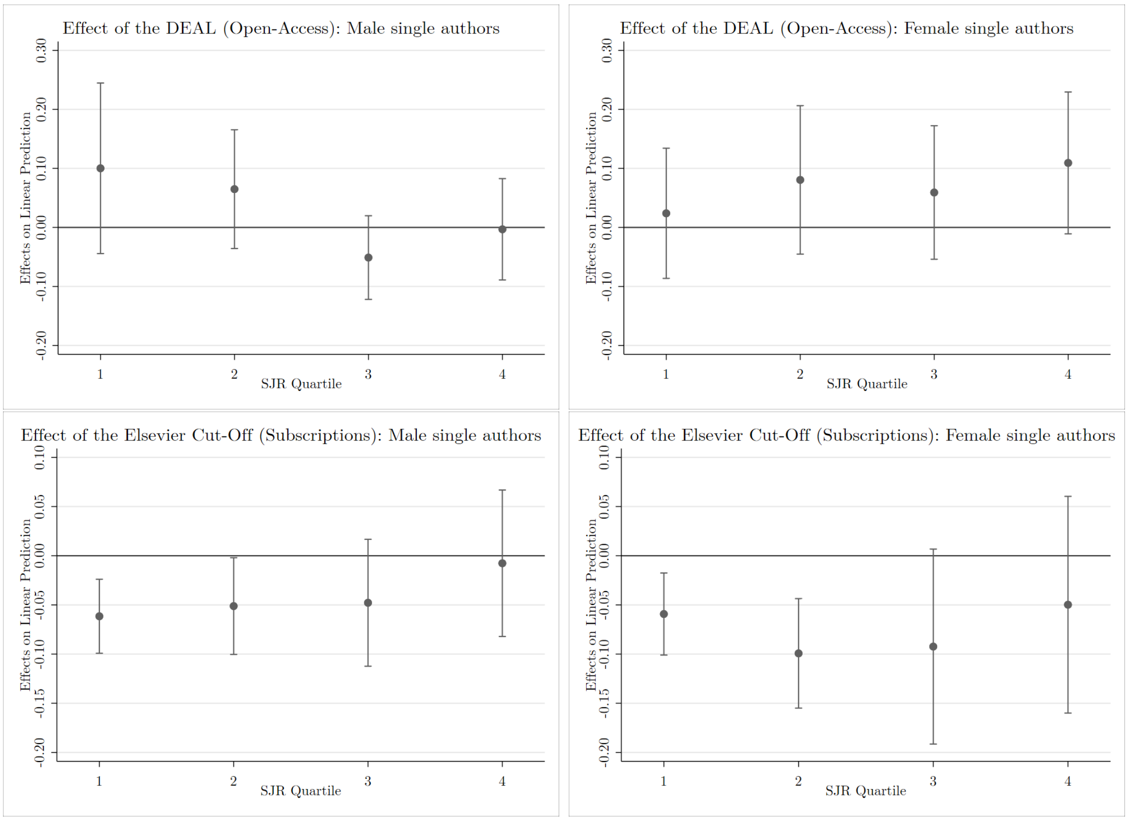


The left panel shows the marginal effects for the Elsevier cut-off (subscriptions), and the right panel shows those for the DEAL contracts (open-access). Dep. Vars.: $\mathbb{1}_i^{Els}$ (LHS), $\mathbb{1}_i^{DEAL}$ (RHS) using eq. (2.1). Standard errors heteroskedasticity-robust and clustered on the journal level. 95% confidence bands. N = 222,779. p-values obtained with Wald tests. Tables 2.24 and 2.25 in Appendix B provide further details on the effects shown here.

Figure 2.9: Marginal effects of the distinction between groups and single authors

There is notable variation between single authors and same-gender groups for Elsevier. The marginal effect for male single authors is significant and negative, while for male groups it is indistinguishable from zero, but both do not differ significantly from each other. The same pattern holds for women, but the estimates are significantly different here. As an additional dimension of heterogeneity, we also find differences between male and female single authors ($p = 0.09$) but not for groups, where the estimates are virtually the same. Hence, the heterogeneity across genders stems from single-authored papers. Put differently, the negative effect for Elsevier appears to get washed out once a paper is coauthored, which also vanishes gender differences.

The right panel shows the decomposition for the DEAL, where we detect a different pattern. Male groups opt into eligible journals but not male single authors. In contrast, papers single-authored by a woman are published more often in DEAL outlets, whereas female groups do not react at all. All confidence intervals are broader than those for the Elsevier regression. Also, the p-values obtained from Wald tests tell us that no estimate is significantly different from each other. Single female authors react to negative and the positive incentives quite heavily. Once they team up with other females for a joint publication, both effects – the positive DEAL as well as the negative Elsevier reaction – disappear. Among men, we observe no opt-out of Elsevier among groups but for single authors. However, we see a positive reaction to the DEAL only among groups. Thus, we find differences for both genders when the decision is made in a group or individually.



Left panels: Male researchers, right panels: Female researchers. Upper panels: Marginal effects for the DEAL agreements (open-access) across reputation, Lower panel: Marginal effects for the Elsevier cut-off (subscriptions) across reputation. Dep. Vars.: $\mathbb{1}_i^{DEAL}$ (upper), $\mathbb{1}_i^{Els}$ (RHS) using eq. (2.1). Standard errors heteroskedasticity-robust and clustered on the journal level. 95% confidence bands. $N = 222,779$. Table 2.27 in Appendix B provides further details.

Figure 2.10: Effect decomposition for single authors by gender and reputation

Next, in Figure 2.10, we look closer at the reaction of single authors to both events. Here we know that group effects correcting individual behavior cannot be present. Among the DEAL journals, we find no effect for male authors at any SJR quartile. Women, however, do not only opt significantly into DEAL journals but do so only at the top quartile of the impact distribution, at least at the 10%

significance level (AME = 10.93%, p = 0.075). The benefit is ambiguous from a purely self-centered perspective. Leading journals are typically subscribed by most research institutions, so that open-access hardly removes any access barriers for potential readers.

Furthermore, even in the top quarter, journals covered by the DEAL tend to be less reputed than the average Elsevier journal in economics. Regarding the Elsevier cut-off, we find negative effects for the lower two quartiles among men and women. While the effect for the lowest impact journals is nearly the same, the shift away from the second quartile amounts to -9.9% for women (p = 0.000) but only to -5.12% (p = 0.041) for men. Hence, the effect for single female authors is much more pronounced with twice the size of the male reaction.

2.4.5 Contextualization and Interpretation of the Findings

The decision where to submit and subsequently publish a paper is a high-stakes decision for a researcher as the journal is often used as an important signal of a publication's quality. Second, a paper's outlet also affects citations, which serves as an ability signal as well. We carefully draw from our findings that men appear to focus, at least at the margin, on journal reputation when choosing a publication outlet, while accepting that the article might be hidden behind paywalls for at least some potential recipients. In contrast, women tend to accentuate, at the margin, the visibility of their research. We base this conclusion on the slightly higher uptake of DEAL journals carrying immediate open-access, but especially on the more substantial shift away from Elsevier by female researchers. The finding is consistent with broader evidence on gender differences in the provision of public goods. Nowell and Tinkler (1994), Eckel and Grossman (1998), and Andreoni and Vesterlund (2001) have shown that women contribute more to public goods than men. Our findings align with that, as freely accessible research for all members of society is a public good. In contrast, reputation is a private good.

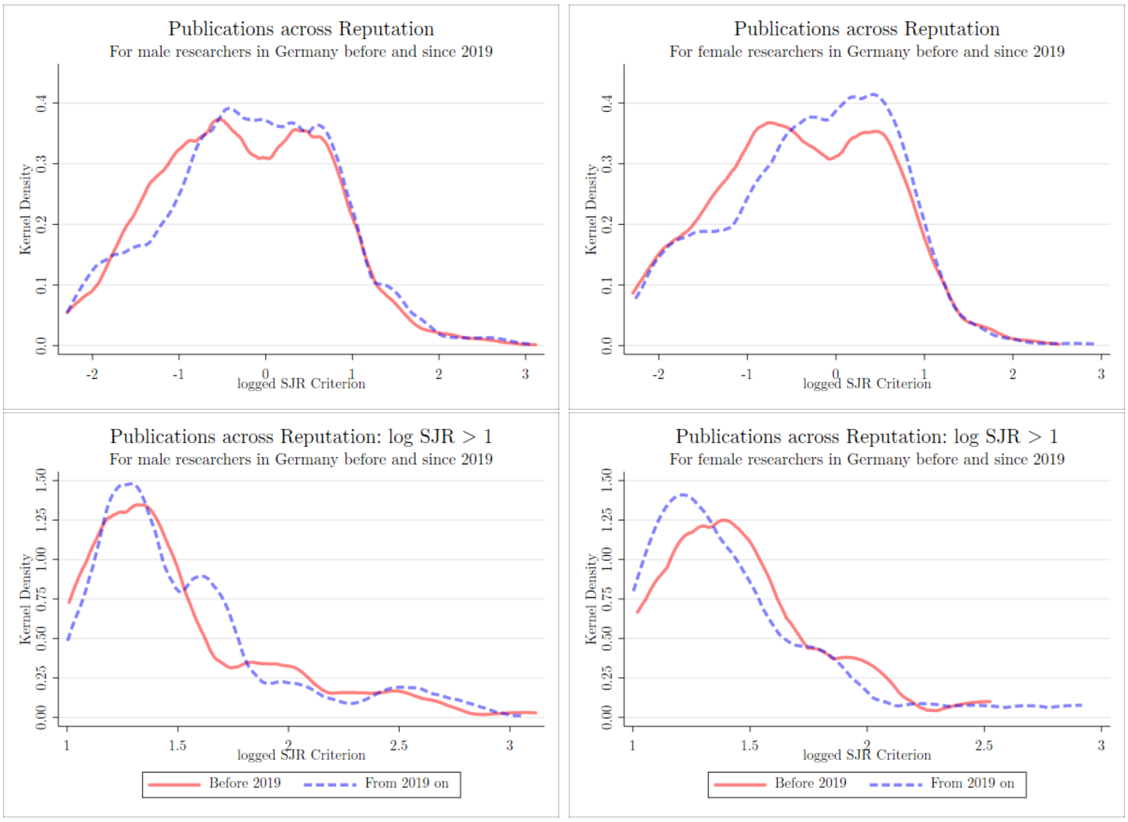
The higher tendency towards public good provision may also coincide with a higher risk aversion among women relative to men (Booth et al., 2014; Borghans et al., 2009). While a theoretical risk of a complete server shutdown exists, open-access articles imply free access without any temporal limitation. Articles published in outlets requiring a subscription are locked behind a paywall once a subscription is canceled. There is an overall call to shift towards open-access journals and continued dissatisfaction with Elsevier's pricing policy (Bergstrom, 2001). Highly priced journals come with the risk that additional institutions may cancel their subscriptions. While subscriptions lock out the general public per se, canceled subscriptions by universities would further decrease visibility.[30]

These factors may induce a vicious circle for female researchers. A more risk-averse journal choice will lead, ceteris paribus, to a lower reputation in the discipline. Further discouraged by additional obstacles, they may continue to select journals suboptimal for their careers. Outlets with lower impact factors may also lead to less project funding or collaborations with other researchers, which may harm future research output and eventually result again in publications with lower impact. These considerations are perfectly in line with Kelchtermans and Veugelers (2013, p. 273), who find that "women have a significant lower probability of reaching top performance for the first time in their career, . . . but there is no evidence for a gender bias hindering repeated top performance." It points towards a vicious circle that reduces women's chances of reaching the top.

[30]Admittedly, a general objection exists to the relevance of risk aversion as an explanation of behavior and to the Elsevier cut-off being a treatment: Predatory repositories or 'shadow libraries' such as *sci-hub* provide predatory copies of publications behind paywalls for free, which infringes the rights of the legal owners of the articles, i.e., the publishers. Nevertheless, it may be a viable alternative to authorized access to Elsevier sources. In fact, researchers cite papers uploaded on sci-hub more frequently than unavailable equivalents (Correa et al., 2022). On the other hand, Elsevier has already filed a lawsuit against the platform in 2017 (Schiermeier, 2017) and may take further actions to fight this kind of piracy, especially in case too many researchers start to rely on that. Furthermore, given their illegality, public bodies cannot officially refer to these repositories. Especially risk averse researchers are, therefore, unlikely to completely rely on their research being made available through such channels. Put differently, given that we find considerable reactions to the Elsevier cut-off, researchers apparently do not consider the presence of predatory repositories a suitable substitute for legal access to Elsevier journals or may sanction the publisher for its behavior unrelated to the access options.

The findings on differences between author groups and single authors may relate to the research on differences in decision-making between individuals and groups who decide jointly. The experimental economics literature provides broad evidence that groups in many settings act closer to a rational benchmark than individual decision makers (Bornstein & Yaniv, 1998; Charness & Gneezy, 2012; Cooper & Kagel, 2005; Kocher & Sutter, 2005). Related to our case, an aversion against Elsevier as a publisher may cancel out for both women and men.

Charness and Rustichini (2011) experimentally show that women cooperate more often when observed by other women. Possibly within research groups, they may set aside a potential antipathy against Elsevier and consider their coauthors' potential career opportunities. It corresponds to our finding that the shift away from Elsevier increases the higher the share of women in a research group. The hypothesis also fits with our finding that author groups with a majority of women or entirely female tend to opt into the DEAL more often than male groups. In general, the cancellation of Elsevier subscriptions has little impact on the accessibility of publications since this holds only for many institutions in Germany but not the rest of the world. Furthermore, the slight loss (if existent) is likely to be more than offset by the loss in reputation, as shifting away from established Elsevier outlets may negatively affect a researcher's career.



Distribution of publications from German research institutions separated by gender and time. Left panels: Male researchers (single and teams), Right panels: Female researchers (single and teams). Upper panels: Distribution across the whole range of the logged SJR criterion. Lower panels: Distribution restricted to values of log SJR >1. N = 243,375.

Figure 2.11: Distribution of publications separated by gender and time

Figure 2.11 shows the density of publications from economists at German institutions across quality, separated by gender and time. We separate the sample at the end of 2018, i.e., we take the median date between July 2018 (Elsevier cut-off) and July 2019 (start of the DEAL). For both men and women, we see a notable increase in publications in the middle of the quality distribution, accompanied by a shift away from lower-tier outlets. It may be mainly related to the journals of Springer-Nature as part of the DEAL, because its outlets publish more papers and are lower ranked than Wiley journals.

The main difference is at the top end of the distributions. For journals with a logarithmic SJR $\gtrsim$ 1.4, the density of publications by female researchers is almost always lower for the later years than before. For men, the opposite is true, with more publications in the interval [1.5, 1.75]. The same holds at the very top at logSJR >2.5. Given the importance of journal choice especially in economics, this may affect the chances of female researchers to be promoted compared to their male colleagues.

The behavior of single female authors particularly corresponds to the broader findings on gender differences in public good provision. The 'DEAL' was often communicated as a 'game changer' that leads to substantial improvements in the academic publishing market.[31] The subscription-based system has been criticized by the DEAL organizers as "untenable", while the open-access encompassed by the DEAL has been praised: "To qualify its human capital through education, power new discoveries, and enable society ... to prosper, German research must be ... available for everyone ... in its final published form."[32] This is a statement of the DEAL officials, but its tonality is representative. Other critiques are even harsher: "Academic publishers make [Rupert] Murdoch look like a socialist."[33] Thus, women might not only be more worried about the broad availability of their research but also more attracted to support the ideal of open science embodied by the DEAL and, in parallel, might want to penalize the business conduct of Elsevier. Only when it comes to research groups, these social preferences are leveled out in accordance with previous research. As women write single authored papers more often than men, antipathies towards Elsevier are less often overruled.

[31] See, e.g., the official statement that "this transitional strategy takes a giant and incisive step forward on the road to making open the default in scholarly communication and, thus, enabling further evolution in research practices." See `https://web.archive.org/web/20230103182522/https://deal-operations.de/en/here-is-the-deal/deal-approach` for details. This is a back-up from the web archive, copy date January 3, 2023, last checked August 17, 2023.

[32] See `https://web.archive.org/web/20230103182448/https://deal-operations.de/en/here-is-the-deal/change-the-system` (both quotes – this is a back-up from the web archive, copy date January 3, 2023, last checked August 17, 2023.).

[33] See `https://www.theguardian.com/commentisfree/2011/aug/29/academic-publishers-murdoch-socialist`, published August 29, 2011, by George Monbiot, last checked August 17, 2023.

2.5 Conclusion and Outlook

Our paper has studied gender and coauthor differences in publication behavior. We look at two events related to negotiations between all German research institutions and leading academic publishers in economics and adjacent fields, namely Elsevier, Springer Nature, and Wiley. We exploit two plausibly exogenous events for researchers: The DEAL agreements that grant researchers open-access publishing free of charge for articles published in nearly all of Springer Nature and Wiley journals. We find an overall positive shift towards the included journals but no significant differences between female, male, and mixed-gender research teams in the uptake of this benefit. However, women particularly submit single-authored work to the included outlets.

In contrast, the response to the Elsevier cut-off differs notably between genders. The German research institutions and the publisher discontinued their negotiations with a loud uproar. Some hundred universities and colleges terminated their subscriptions. In July 2018, Elsevier cut off these institutions from unpaid access to its journals. It caused a good amount of publicity, but the actual effect on articles published in Elsevier journals is low: The rest of the world remained unaffected. While researchers from German institutions may also be able to circumvent the newly erected paywalls in one way or another, the uncertainty of whether other countries would possibly follow this decision may have surged.

All-female research groups and teams with a majority of women significantly reduced their publications in Elsevier journals. Their male colleagues only reduced publications in lower-tier journals but even published more often in the *top* quarter of the quality distribution. Disentangling the single-gender categories into single authors and single-gender teams, we uncovered essential differences also in this dimension. While male individuals tend to opt out of Elsevier and into DEAL journals, author groups seem to 'correct' this behavior. Similarly, female single-authored papers are published differently than papers from female author groups. Individual females heavily withdraw from Elsevier, while coauthored papers of women do not show any reaction. In contrast, such groups do not opt into the DEAL, while single female authors do.

The implications of these behavioral differences are twofold and, especially in academia, potentially severe. Suppose one proposes the objective of transforming the market for academic publications. In that case, the 'Elsevier experiment' raises doubts about to which extent male researchers – who are predominant in most academic disciplines – contribute to this objective. Even though we observe a tendency towards open-access in the lower ranks of the quality distribution, the behavior at the top appears to perpetuate the role of Elsevier and the position of incumbent journals and publication patterns more generally. Considering the significant profits of commercial publishers (Larivière et al., 2015) and the opportunities of the digital dissemination of research, large movements such as the 'Plan S' try to overcome subscription-based paywalls for academic publications.[34] In light of the observed patterns, it remains an open question whether such initiatives will succeed.

Sticking to the status quo bears important consequences: Shifting away from high impact outlets may affect the career opportunities of women. In economics, publishing in the highest-ranked journals is of major importance. Excluding a publisher of many influential journals may backfire. It is even more severe as publications have a larger impact on womens' careers than mens' (Lutter et al., 2022) and since women publish less than men (Xie & Shauman, 1998; Prpić, 2002). It may induce a 'vicious circle' that hinders women from pursuing the same careers as men. Effectively, women contribute more to the public good of open science but may pay a higher price for it. This service to the profession is likely to contribute to the gender gap instead of closing it. It is particularly applicable to single-authored papers, as these are often job market or early career publications which shape academic careers.

[34]See `https://www.coalition-s.org/why-plan-s/`, last checked August 17, 2023. For an economic analysis, see Armstrong (2021).

In that sense, our results also add to the understanding of the so-called research 'productivity puzzle' that describes the surprising differences in academic productivity between men and women. If publication behavior not only differs between genders but particularly harms women, it might be one explanatory channel (out of plenty) why women appear to be less productive than men in academia, as lower-ranked publications tend to attract fewer grants or top-tier researchers that are willing to collaborate even if the *actual* quality of the publications by females is equivalent to those by males.

It remains an open question to which extent these imbalances may change when a journal's reputation is no longer the only criterion for research evaluation. While there is an overall push towards more open-access, initiatives such as the 'Coalition on Reforming Research Assessment' (CoARA) call for further-reaching reforms in evaluating academic research. For example, it is proposed to abandon ranking measures such as the SJR or the H-index.[35] Once such criteria become more relevant, male researchers might follow their female colleagues in adjusting their publisher choice towards those that strongly focus on open science. Until then, they appear to benefit at the expense of the more pro-social behavior of their female colleagues.

[35] See the 'agreement on reforming research assessment, July 20, 2022: `https://coara.eu/app/uploads/2022/09/2022_07_19_rra_agreement_final.pdf`. 580 organizations have signed it so far (June 12, 2023).

2.6 Appendix A

Additional data

In this part of Appendix A, we present additional data tables and an additional figure that provide additional information on the dataset we use. We refer to these tables throughout the text to offer the reader to dive deeper into the composition of the dataset we use.

Type	Frequency	Share	Cum.
Article	298,493	90.36%	90.36%
Review	12,522	3.79%	94.15%
Book chapter	6,225	1.88%	96.04%
Editorial	4,067	1.23%	97.27%
Note	3,685	1.12%	98.38%
Conference Paper	3,106	0.94%	99.32%
Erratum	1,109	0.34%	99.66%
Letter	549	0.17%	99.82%
Short survey	420	0.13%	99.95%
Undefined	127	0.04%	99.99%
Retracted	28	0.01%	100%
Data paper	5	0.00%	100%
Book	1	0.00%	100%
Total	330,337	100%	

Table 2.5: Types of publications in the data

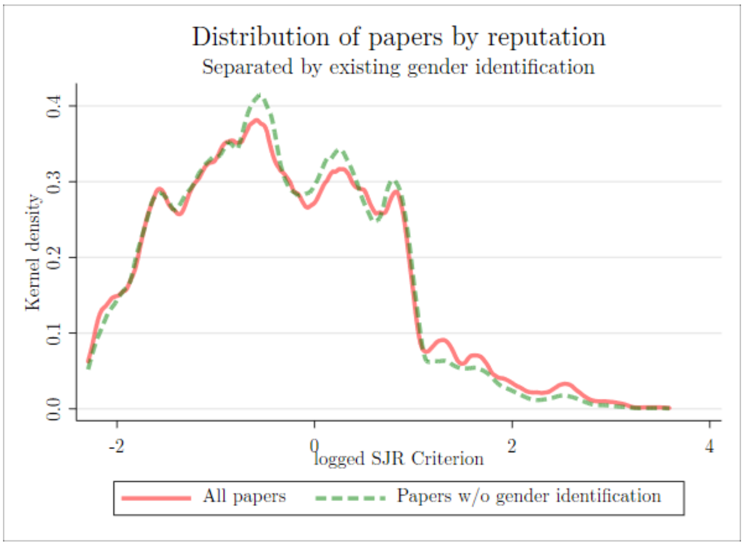
	all observations		**obs. w/ gender ident.**	
Year	Frequency	Share	Frequency	Share
2015	30,332	9.75%	24,900	10.23%
2016	31,858	10.24%	26,119	10.73%
2017	34,257	11.01%	27,692	11.38%
2018	36,224	11.65%	28,567	11.74%
2019	38,243	12.30%	29,674	12.19%
2020	44,478	14.30%	33,729	13.86%
2021	45,716	14.70%	33,300	13.68%
2022	49,907	16.05%	39,394	16.19%
Total	311,015	100 %	243,375	100 %

Only considering the publication types 'article' and 'review'.

Table 2.6: Publications by year

Year	Mixed Gender		Male Res.		Female Res.		Total
	Freq.	Share	Freq.	Share	Freq.	Share	
2015	8,652	34.75%	13,241	53.18%	3,007	12.08%	24,900
2016	9,496	36.36%	13,488	51.64%	3,135	12.00%	26,119
2017	10,274	37.10%	14,041	50.70%	3,377	12.20%	27,692
2018	11,085	38.80%	13,989	48.97%	3,493	12.23%	28,567
2019	11,933	40.21%	14,203	47.86%	3,538	11.92%	29,674
2020	14,074	41.73%	15,611	46.28%	4,044	11.99%	33,729
2021	14,393	43.22%	15,217	45.70%	3,690	11.08%	33,300
2022	17,980	45.64%	16,941	43.00%	4,473	11.35%	39,394
Total	97,887	40.22%	116,731	47.96%	28,757	11.82%	243,375

Table 2.7: Publications by gender and year



N = 311,015 (all papers), N = 67,640 (papers w/o gender identification)

Figure 2.12: Distribution of publications across quality – missing gender identification

Germany	Mixed	Male	Female	Total
0	28,025	26,813	4,696	59,534
1	1,695	2,621	306	4,622
Total	29,720	29,434	5,002	64,156

Table 2.8: Publications in Elsevier journals separated by gender and nationality

Robustness Checks

Doubly-robust Difference-in-Differences estimation: To increase confidence that our causal difference-in-differences design is correctly specified, we additionally compute doubly-robust estimators as suggested by Sant'Anna and Zhao (2020) in their difference-in-differences correction. Their model is applicable when the parallel trends assumption holds after conditioning on covariates, what we do in our model. To do so, we specify a more parsimonious model as shown in eq. 2.2 below. Here, we compute the canonical difference-in-differences setting: $\mathbb{1}_T$ is a binary pre/post treatment indicator. The remaining variables are used as defined in Section 2.3. We use this specification solely to identify baseline results, which can be found in Tables 2.9 (DEAL open-access) and 2.10 (Elsevier cut-off) below.

$$\mathbb{1}_i^{Publ.} = \mathbb{1}_i^T + \mathbb{1}_i^{GER} + \mathbb{1}_i^T \times \mathbb{1}_i^{GER} + X_i' + \epsilon_i \tag{2.2}$$

In both cases, we first compute the pure effect and sequentially add the two central covariates, journal rank (captured by the SJR quartile) and gender.

	AME	Std. Err.	t-stat.	p-value	95% CI	
Plain	0.0239	0.0183	1.30	0.192	-0.0120	0.0599
w/ SJR	0.0403	0.0188	2.14	0.032	0.0034	0.0772
w/ SJR, gender	0.0394	0.0186	2.12	0.034	0.0029	0.0760

Estimation using eq. (2.2) above and the doubly robust difference-in-differences correction by Sant'Anna and Zhao (2020). Dep. var. $\mathbb{1}_i^{DEAL}$. Outcome model with weighted least squares, treatment model with inverse probability tilting. Standard errors heteroskedasticity-robust and clustered on the journal level. N = 243,375.

Table 2.9: Average marginal effect of the DEAL (open-access) – doubly robust specification

	AME	Std. Err.	t-stat.	p-value	95% CI	
Plain	-0.0188	0.0154	-1.22	0.222	-0.0489	0.0114
w/ SJR	-0.0432	0.0155	-2.78	0.005	-0.0736	-0.0127
w/ SJR, gender	-0.0411	0.0155	-2.66	0.008	-0.0715	-0.0108

Estimation using eq. (2.2) above and the doubly robust difference-in-differences correction by Sant'Anna and Zhao (2020). Dep. var. $\mathbb{1}_i^{DEAL}$. Outcome model with weighted least squares, treatment model with inverse probability tilting. Standard errors heteroskedasticity-robust and clustered on the journal level. N = 243,375.

Table 2.10: Effect of the Elsevier cut-off (subscriptions) – doubly robust specification

Excluding observations to evaluate effect interference: While we have excluded observations in the previous robustness check section simply because of data quality considerations, we exclude in this section data from the regressions to ensure that the overlapping events of the Elsevier cut-off and the DEAL introduction do not distort our findings or else to explain how they affect each other. In the following table 2.11, we exclude Elsevier publications to study the effect of the DEAL on researchers in Germany without havinng Elsevier in the control group.

	AME	Std. Err.	t-stat.	p-value	95% CI	
Plain Effect	0.0262	0.0242	1.09	0.278	-0.0212	0.0737
Mixed	0.0328	0.0289	1.14	0.256	-0.0239	0.0894
Male	0.0270	0.0251	1.07	0.283	-0.0223	0.0763
Female	0.0048	0.0298	0.16	0.873	-0.0537	0.0632

Dep. Var.: $\mathbb{1}_i^{DEAL}$ using eq. (2.1). Standard errors heteroskedasticity-robust and clustered on the journal level. N = 179,219. Regression for the DEAL effect excluding Elsevier publications.

Table 2.11: Effect of the DEAL (open-access) separated by gender w/o Elsevier journals

In the following two tables, we narrow the time span for the effects of the Elsevier cut-off. In Table 2.12, we exclude the years 2020 - 2022 from the post-treatment window to exclude any DEAL effect from the regression. In Table 2.13, we exclude the years 2015 and 2016 from the pre-treatment window because Figure 2.4 in Section 2.3 in the main text has shown that there was a major decrease in Elsevier publications between 2016 and 2017.

	AME	Std. Err.	t-stat.	p-value	95% CI	
Plain Effect	-0.0395	0.0146	-2.71	0.007	-0.0681	-0.0109
Mixed	-0.0474	0.0193	-2.46	0.014	-0.0852	-0.0096
Male	-0.0289	0.0162	-1.78	0.075	-0.0607	0.0030
Female	-0.0591	0.0243	-2.43	0.015	-0.1069	-0.0114

Dep. Var.: $\mathbb{1}_i^{Els}$ using eq. (2.1). Standard errors heteroskedasticity-robust and clustered on the journal level. N = 136,952. Time span covered: 2015-2019

Table 2.12: Elsevier Effect (subscriptions) decomposed by gender – years 2015 - 2019

	AME	Std. Err.	t-stat.	p-value	95% CI	
Plain Effect	-0.0279	0.0142	-1.96	0.050	-0.0559	0
Mixed	-0.0409	0.0169	-2.42	0.016	-0.0741	-0.0078
Male	-0.0127	0.0145	-0.88	0.380	-0.0411	0.0157
Female	-0.0424	0.0181	-2.34	0.020	-0.0781	-0.0068

Estimates for the baseline effect and the gender decomposition for Elsevier journals excluding the years 2015 and 2016. Dep. Var.: $\mathbb{1}_i^{Els}$ using eq. (2.1). Standard errors heteroskedasticity-robust and clustered on the journal level. N = 192,356.

Table 2.13: Elsevier effect (subscriptions) decomposed by gender, years 2017 – 2022

Table 2.14 excludes all observations from July to December 2019 as in this period, the DEAL conditions have been in place for Wiley journals but not for Springer Nature journals yet.

	AME	Std. Err.	t-stat.	p-value	95% CI	
Plain Effect	0.0469	0.0215	2.1800	0.030	0.0046	0.0891
Mixed	0.0560	0.0256	2.1900	0.029	0.0059	0.1062
Male	0.0400	0.0220	1.8200	0.070	-0.0032	0.0833
Female	0.0434	0.0271	1.6000	0.110	-0.0098	0.0966

Dep. Var.: $\mathbb{1}_i^{DEAL}$ using eq. (2.1). Standard errors heteroskedasticity-robust and clustered on the journal level. N = 231,914. Regression for the DEAL effect excluding publications from July to December 2019.

Table 2.14: Average marginal effect of the DEAL (open-access) separated by gender w/o observations from July - December 2019

Excluding observations due to varying data accuracy: Throughout the analysis, we work with one specification of data accuracy levels. For example, we use a cut-off for the level of certainty with respect to the gender identification of 70%, i.e., for each observation, the assigned gender (if assigned) is at least with 70% probability (as estimated by the Namsor algorithm) correct. The subsequent tables provide for both events, the DEAL open-access and the Elsevier cut-off, two robustness checks that compute the plain effect of the event as well as the main gender decomposition using only observations with a minimum of 80% or else 90% gender identification accuracy. While the number of observations necessarily decreases, we have fewer mismatched observations. Table 2.15 presents the results for the DEAL open-access and Table 2.16 for the Elsevier cut-off.

	AME	Std. Err.	t-stat.	p-value	95% CI	
			80% threshold			
Plain Effect	0.0461	0.0197	2.34	0.019	0.0075	0.0847
Mixed	0.0531	0.0231	2.30	0.022	0.0078	0.0985
Male	0.0404	0.0203	1.99	0.047	0.0005	0.0804
Female	0.0442	0.0260	1.70	0.089	-0.0068	0.0953
			90% threshold			
Plain Effect	0.0447	0.0198	2.2600	0.024	0.0059	0.0834
Mixed	0.0509	0.0234	2.17	0.030	0.0049	0.0969
Male	0.0383	0.0203	1.88	0.060	-0.0016	0.0783
Female	0.0473	0.0259	1.83	0.068	-0.0035	0.0981

Dep. Var.: $\mathbb{1}_i^{DEAL}$ using eq. (2.1). Standard errors heteroskedasticity-robust and clustered on the journal level. $N_{80\%} = 232{,}815$, $N_{90\%} = 219{,}015$. Regression for the DEAL effect (open-access) using alternative gender identifications based on higher cut-off values for the accuracy of the gender prediction (80% and 90%.)

Table 2.15: Effect of the DEAL (open-access) for different gender accuracy levels

	AME	Std. Err.	t-stat.	p-value	95% CI	
			80% threshold			
Plain Effect	-0.0413	0.0136	-3.05	0.0020	-0.0680	-0.0147
Mixed	-0.0539	0.0159	-3.38	0.001	-0.0853	-0.0226
Male	-0.0270	0.0141	-1.91	0.056	-0.0546	0.0007
Female	-0.0552	0.0179	-3.08	0.002	-0.0904	-0.0200
			90% threshold			
Plain Effect	-0.0391	0.0138	-2.83	0.0050	-0.0662	-0.0120
Mixed	-0.0497	0.0162	-3.07	0.002	-0.0815	-0.0179
Male	-0.0243	0.0142	-1.71	0.087	-0.0521	0.0036
Female	-0.0598	0.0183	-3.27	0.001	-0.0956	-0.0239

Dep. Var.: $\mathbb{1}_i^{Els}$ using eq. (2.1). Standard errors heteroskedasticity-robust and clustered on the journal level. $N_{80\%} = 232{,}815$, $N_{90\%} = 219{,}015$. Regression for the Elsevier cut-off using alternative gender identifications based on higher cut-off values for the accuracy of the gender prediction (80% and 90%.)

Table 2.16: Effect of the Elsevier cut-off (subscriptions) for different gender accuracy levels

Second, as elaborated on in Subsection 2.4.2, when applying the granular gender decomposition, we lose some observations. In Table 2.17, we exclude these unidentified observations from our main specification to ensure that they do not drive our results.

	AME	Std. Err.	t-stat.	p-value	95% CI	
DEAL (Open-Access)						
Plain Effect	0.0469	0.0196	2.40	0.017	0.0085	0.0854
Mixed	0.0553	0.0231	2.40	0.0170	0.0099	0.1006
Male	0.0396	0.0203	1.95	0.0520	-0.0003	0.0794
Female	0.0486	0.0259	1.87	0.0610	-0.0023	0.0996
Elsevier (Subscriptions)						
Plain Effect	-0.0417	0.0134	-3.12	0.002	-0.0679	-0.0155
Mixed	-0.0549	0.0155	-3.54	0.000	-0.0854	-0.0245
Male	-0.0291	0.0139	-2.09	0.037	-0.0564	-0.0018
Female	-0.0573	0.0179	-3.19	0.001	-0.0925	-0.0221

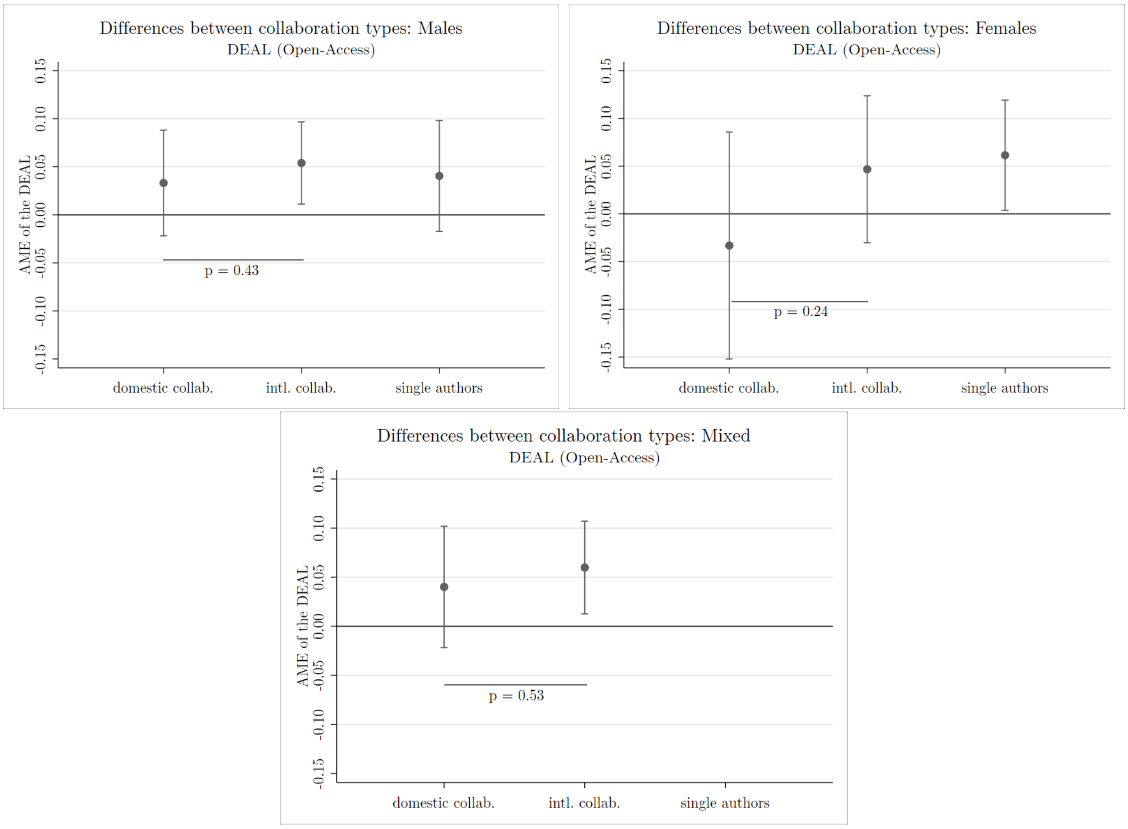
Dep. Vars.: $\mathbb{1}_i^{DEAL}$ (upper), $\mathbb{1}_i^{Els}$ (lower) using eq. (2.1). Standard errors heteroskedasticity-robust and clustered on the journal level. N = 222,779.

Table 2.17: Effects for DEAL (open-ccess) and Elsevier (subscriptions) excluding the 'unidentified' granular gender category in the main specification

Differences between collaborations: To further distinguish the effects between types of groups, i.e., teams and single researchers, we study the differences between domestic and international collaborations. The group members' affiliation must be located in the same country to qualify for the former. For the latter, at least one author must have its main affiliation in another country.[36]

Starting again with the DEAL treatment, Figure 2.13 shows three panels, one for each gender group, that show the effects for domestic collaborations, international collaborations, and single authors for comparison. There are no mixed-gender single authors, so the lower panel provides only two coefficients. We find the same difference between the two types of collaborations for males and for mixed-gender groups. While domestic teams from Germany do not react, international teams tend to pick up the DEAL. It might be an indicator that team members with better research networks (and, therefore, collaborating across borders) act more strategically. It could, however, also be the case that these teams intentionally assign one of the members from Germany as the corresponding author to benefit from the frictionless open-access to their publication. Women, in contrast, do not react if collaborating in teams, no matter whether they are in domestic or international teams. But as shown in Figure 2.9 in the main text, women who write their papers alone opt for the DEAL – other than their male colleagues who do the same.

[36] We are aware that an increasing number of researchers have multiple affiliations (Hottenrott & Lawson, 2017) potentially also stemming from different countries. Here we rely on the country mentioned in the postal correspondence address of the researchers.

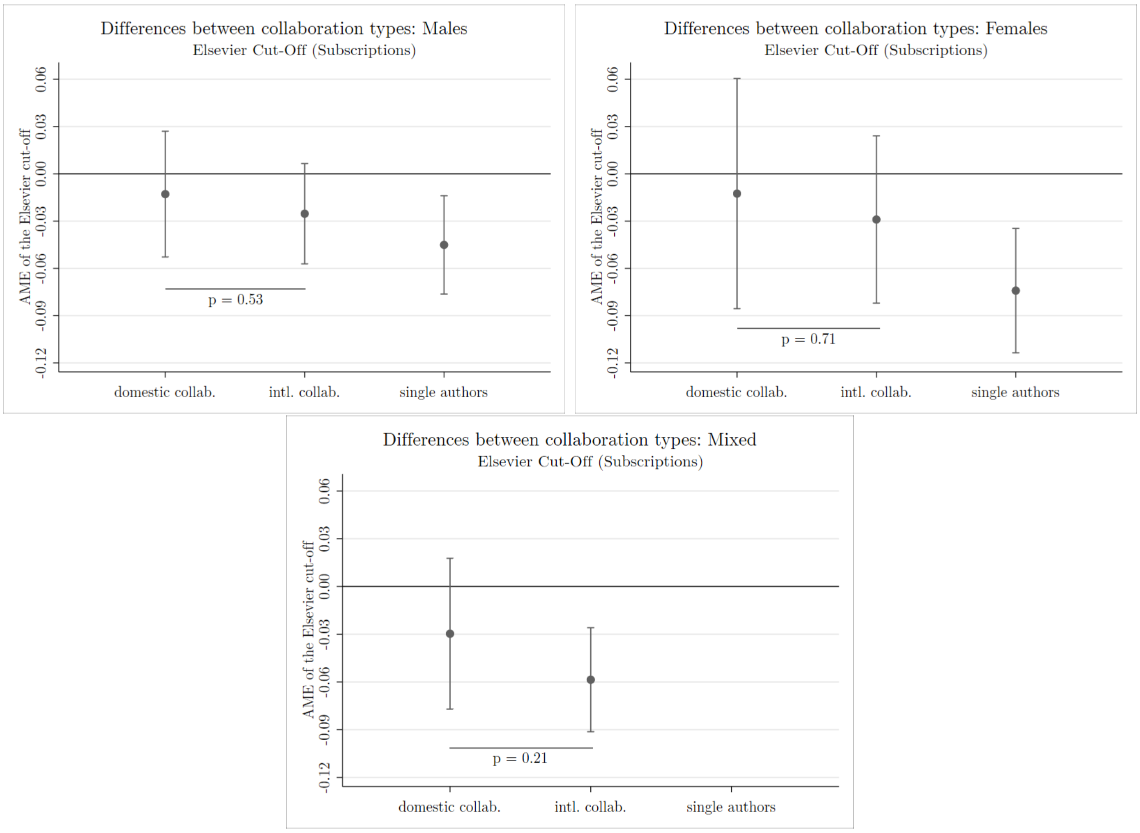


Dep. Var.: $\mathbb{1}_i^{DEAL}$ using eq. (2.1) additionally adding an interacted categorical variable for domestic and international collaborations as well as single authorship to the regression. For mixed teams, a single author estimate cannot be computed by definition. Standard errors heteroskedasticity-robust and clustered on the journal level. $N = 243{,}375$. 95% confidence bands provided. p-values obtained with Wald tests. Table 2.28 in Appendix B provides further details on the estimates.

Figure 2.13: Effect of the DEAL (open-access) decomposed by gender & collaboration type

Figure 2.14 shows the equivalent results for the Elsevier cut-off. Here, we cannot detect differences between domestic and international teams for men and women. Both coefficients are insignificant and nearly the same in absolute size. As shown before, male and female single authors shift away from Elsevier outlets. In contrast, international mixed-gender research groups shift away from the publisher as well. We can only speculate why this is the case. One possible explanation is that these groups are the most diverse as they vary in gender and country of affiliation.

The Wald tests comparing the coefficients for domestic and international teams emphasize that there do not exist significant differences. Hence, differences between the genders in terms of collaborations are not driving our previous findings.



Dep. Var.: $\mathbb{1}_i^{Els}$ using eq. (2.1) additionally adding an interacted categorical variable for domestic and international collaborations as well as single authorship to the regression. For mixed teams, a single author estimate cannot be computed by definition. Standard errors heteroskedasticity-robust and clustered on the journal level. N = 243,375. 95% confidence bands provided. p-values obtained with Wald tests. Table 2.29 in Appendix B provides further details on the estimates.

Figure 2.14: Elsevier effect (subscriptions) decomposed by gender and collaboration type

2.7 Appendix B

This appendix provides the computational details of all plots presented throughout the main text. In each table note, we provide a link to the initial figure to which it relates.

	AME	Std. Err.	t-stat.	p-value	95% CI	
Plain Effect	-0.0427	0.0135	-3.17	0.002	-0.0691	-0.0162
Mixed	-0.0561	0.0160	-3.51	0.000	-0.0875	-0.0248
Male	-0.0280	0.0140	-2.01	0.045	-=.0554	-0.0006
Female	-0.0563	0.0180	-3.13	0.002	-0.0915	-0.0210

Dep. Var.: $\mathbb{1}_i^{Els}$ using eq. (2.1). The plain effect describes the average marginal effect of the Elsevier cut-off on the likelihood of a paper to appear in an Elsevier journal regardless of the authors' gender. Standard errors heteroskedasticity-robust and clustered on the journal level. N = 243,375.

Table 2.18: Effect of the Elsevier cut-off (subscriptions) separated by gender

	AME	Std. Err.	t-stat.	p-value	95% CI	
Plain Effect	0.0469	0.0196	2.40	0.017	0.0085	0.0854
Mixed	0.0553	0.0231	2.40	0.017	0.0100	0.1006
Male	0.0396	0.0203	1.95	0.052	0.0003	0.0794
Female	0.0486	0.0259	1.87	0.061	0.0023	0.0996

Dep. Var.: $\mathbb{1}_i^{DEAL}$ using eq. (2.1). Estimates for the results shown in Fig. 2.5. The plain effect describes the average marginal effect of the DEAL introduction on the likelihood of a paper to appear in an eligible journal regardless of the authors' gender. Standard errors heteroskedasticity-robust and clustered on the journal level. N = 243,375.

Table 2.19: Effect of the DEAL (open-access) separated by gender

	AME	Std. Err.	t-stat.	p-value	95% CI	
			Gender: Mixed			
SJR q1	0.1252	0.0797	1.57	0.117	-0.0313	0.2817
SJR q2	0.0876	0.0493	1.78	0.076	-0.0091	0.1843
SJR q3	0.0367	0.0486	0.75	0.451	-0.0587	0.1320
SJR q4	-0.0139	0.0383	-0.36	0.717	-0.0889	0.0612
			Gender: Male			
SJR q1	0.1320	0.0701	1.88	0.060	-0.0056	0.2697
SJR q2	0.0922	0.0419	2.20	0.028	0.0099	0.1744
SJR q3	-0.0244	0.0369	-0.66	0.508	-0.0968	0.0479
SJR q4	-0.0406	0.0317	-1.28	0.201	-0.1029	0.0217
			Gender: Female			
SJR q1	0.0419	0.0554	0.76	0.449	-0.0667	0.1507
SJR q2	0.0635	0.0568	1.12	0.264	-0.0480	0.1750
SJR q3	0.0458	0.0501	0.92	0.360	-0.0524	0.1441
SJR q4	0.0438	0.0501	0.87	0.382	-0.0545	0.1419

Dep. Var.: $\mathbb{1}_i^{DEAL}$ using eq. (2.1). Standard errors heteroskedasticity-robust and clustered on the journal level. N = 243,375. Estimates for the plots in Figure 2.7.

Table 2.20: Effect of the DEAL (open-access) decomposed by gender and SJR

	AME	Std. Err.	t-stat.	p-value	95% CI	
Fully Male	-0.0293	0.0139	-2.1100	0.0350	-0.0566	-0.0020
Mostly Male	-0.0437	0.0192	-2.2800	0.0230	-0.0814	-0.0061
Equally Mixed	-0.0564	0.0162	-3.4900	0.0010	-0.0881	-0.0247
Mostly Female	-0.0743	0.0278	-2.6700	0.0080	-0.1289	-0.0198
Fully Female	-0.0575	0.0179	-3.2100	0.0010	-0.0928	-0.0223

Estimates for the left panel in Figure 2.6. Dep. Var.: $\mathbb{1}_i^{Els}$ using eq. (2.1). Standard errors heteroskedasticity-robust and clustered on the journal level. N = 222,779.

Table 2.22: Elsevier effect (subscriptions) using the granular gender decomposition

	AME	Std. Err.	t-stat.	p-value	95% CI	
Gender: Mixed						
SJR q1	-0.0718	0.0202	-3.55	0.000	-0.1115	-0.0321
SJR q2	-0.0801	0.0264	-3.03	0.002	-0.1320	-0.0283
SJR q3	-0.1024	0.0427	-2.40	0.017	-0.1863	-0.0185
SJR q4	0.0242	0.0364	0.67	0.506	-0.0472	0.0955
Gender: Male						
SJR q1	-0.0640	0.0193	-3.31	0.001	-0.1020	-0.0261
SJR q2	-0.0668	0.0245	-2.73	0.007	-0.1148	-0.0187
SJR q3	-0.0445	0.0317	-1.40	0.161	-0.1067	0.0177
SJR q4	0.0623	0.0296	2.10	0.036	0.0041	0.1204
Gender: Female						
SJR q1	-0.0570	0.0233	-2.45	0.014	-0.1027	-0.0114
SJR q2	-0.0899	0.0244	-3.68	0.000	-0.1378	-0.0420
SJR q3	-0.0722	0.0453	-1.59	0.111	-0.1610	0.0166
SJR q4	0.0129	0.0439	0.29	0.769	-0.0732	0.0990

Dep. Var.: $\mathbb{1}_i^{Els}$ using eq. (2.1). Standard errors heteroskedasticity-robust and clustered on the journal level. N = 243,375. Estimates for the plots in Figure 2.8.

Table 2.21: Effect of the Elsevier cut-off (subscriptions) decomposed by gender and SJR

	AME	Std. Err.	t-stat.	p-value	95% CI	
Fully Male	0.0395	0.0202	1.95	0.051	-0.0002	0.0792
Mostly Male	0.0491	0.0238	2.06	0.039	0.0024	0.0957
Equally Mixed	0.0395	0.0265	1.49	0.136	-0.0125	0.0915
Mostly Female	0.1190	0.0423	2.81	0.005	0.0360	0.2021
Fully Female	0.0485	0.0260	1.87	0.062	-0.0025	0.0995

Estimates for the right panel in Figure 2.6. Dep. Var.: $\mathbb{1}_i^{DEAL}$ using eq. (2.1). Standard errors heteroskedasticity-robust and clustered on the journal level. N = 222,779.

Table 2.23: Effect of the DEAL (open-access) using the granular gender decomposition

	AME	Std. Err.	t-stat.	p-value	95% CI	
Male group	-0.0276	0.0148	-1.86	0.062	-0.0567	0.0014
Male single	-0.0421	0.0165	-2.55	0.011	-0.0746	-0.0097
Female group	-0.0270	0.0229	-1.18	0.239	-0.0720	0.0180
Female single	-0.0750	0.0216	-3.47	0.001	-0.1175	-0.0326

Estimates for the left panel in Figure 2.9. Dep. Var.: $\mathbb{1}_i^{Els}$ using eq. (2.1). Standard errors heteroskedasticity-robust and clustered on the journal level. N = 222,779.

Table 2.24: Effect of Elsevier (subscriptions) – single and multiple authors

	AME	Std. Err.	t-stat.	p-value	95% CI	
Male group	0.0577	0.0220	2.62	0.009	0.0146	0.1009
Male single	0.0283	0.0254	1.11	0.266	-0.0215	0.0781
Female group	0.0315	0.0359	0.88	0.381	-0.0391	0.1020
Female single	0.0680	0.0285	2.38	0.017	0.0120	0.1240

Estimates for the right panel in Figure 2.9. Dep. Var.: $\mathbb{1}_i^{DEAL}$ using eq. (2.1). Standard errors heteroskedasticity-robust and clustered on the journal level. N = 222,779.

Table 2.25: Effect of the DEAL (open-access) – single and multiple authors

	AME	Std. Err.	t-stat.	p-value	95% CI	
Fully Male	-0.0136	0.0145	-0.94	0.347	-0.0421	0.0148
Mostly Male	-0.0277	0.0206	-1.34	0.179	-0.0682	0.0128
Equally Mixed	-0.0401	0.0170	-2.36	0.019	-0.0735	-0.0067
Mostly Female	-0.0579	0.0276	-2.09	0.037	-0.1121	-0.0036
Fully Female	-0.0433	0.0181	-2.39	0.017	-0.0789	-0.0077

Estimates for the granular gender decomposition for Elsevier journals excluding the years 2015 and 2016. Dep. Var.: $\mathbb{1}_i^{Els}$ using eq. (2.1). Standard errors heteroskedasticity-robust and clustered on the journal level. N = 174,927.

Table 2.26: Elsevier effect (subscriptions) using the granular gender decomposition, years 2017 – 2022

	AME	Std. Err.	t-stat.	p-value	95% CI	
Effect of the DEAL: Male single						
SJR q1	0.1002	0.0736	1.36	0.174	-0.0443	0.2447
SJR q2	0.0649	0.0513	1.27	0.206	-0.0357	0.1655
SJR q3	-0.0511	0.0361	-1.41	0.158	-0.1220	0.0198
SJR q4	-0.0032	0.0438	-0.07	0.942	-0.0890	0.0827
Effect of the DEAL: Female single						
SJR q1	0.0239	0.0562	0.43	0.671	-0.0864	0.1342
SJR q2	0.0805	0.0641	1.26	0.209	-0.0452	0.2063
SJR q3	0.0592	0.0577	1.03	0.305	-0.0539	0.1723
SJR q4	0.1093	0.0612	1.78	0.075	-0.0109	0.2295
Effect of the Elsevier cut-off: Male single						
SJR q1	-0.0615	0.0192	-3.21	0.001	-0.0992	-0.0239
SJR q2	-0.0512	0.0251	-2.04	0.041	-0.1004	-0.0020
SJR q3	-0.0478	0.0329	-1.45	0.146	-0.1123	0.0167
SJR q4	-0.0077	0.0379	-0.20	0.840	-0.0821	0.0668
Effect of the Elsevier cut-off: Female single						
SJR q1	-0.0593	0.0213	-2.79	0.005	-0.1010	-0.0175
SJR q2	-0.0993	0.0284	-3.50	0.000	-0.1549	-0.0436
SJR q3	-0.0924	0.0505	-1.83	0.068	-0.1915	0.0067
SJR q4	-0.0498	0.0561	-0.89	0.375	-0.1600	0.0604

Dep. Var.: $\mathbb{1}_i^{DEAL}$ using eq. (2.1). Standard errors heteroskedasticity-robust and clustered on the journal level. $N = 222{,}779$. Estimates for the plots in Figure 2.10.

Table 2.27: Effect of the DEAL (open-access) decomposed by gender and SJR

	AME	Std. Err.	t-stat.	p-value	95% CI	
			Gender: Mixed			
Domestic collab.	0.0401	0.0315	1.27	0.204	-0.0217	0.1019
Intl. collab.	0.0598	0.0241	2.49	0.013	0.0126	0.1070
Single authors			–			
			Gender: Male			
Domestic collab.	0.0331	0.0280	1.18	0.237	-0.0218	0.0880
Intl. collab.	0.0539	0.0218	2.48	0.013	0.0112	0.0967
Single authors	0.0404	0.0294	1.37	0.169	-0.0173	0.0982
			Gender: Female			
Domestic collab.	-0.0332	0.0606	-0.55	0.584	-0.1520	0.0857
Intl. collab.	0.0467	0.0392	1.19	0.234	-0.0302	0.1237
Single authors	0.0614	0.0294	2.09	0.037	0.0036	0.1191

Dep. Var.: $\mathbb{1}_i^{DEAL}$ using eq. (2.1). Standard errors heteroskedasticity-robust and clustered on the journal level. N = 222,779. Estimates for the plots in Figure 2.13.

Table 2.28: Effect of the DEAL (open-access) decomposed by gender and collaboration type

	AME	Std. Err.	t-stat.	p-value	95% CI	
			Gender: Mixed			
Domestic collab.	-0.0297	0.0241	-1.23	0.219	-0.0771	0.0177
Intl. collab.	-0.0586	0.0167	-3.52	0.000	-0.0913	-0.0259
Single authors			–			
			Gender: Male			
Domestic collab.	-0.0129	0.0203	-0.63	0.527	-0.0528	0.0270
Intl. collab.	-0.0253	0.0162	-1.56	0.119	-0.0572	0.0065
Single authors	-0.0451	0.0159	-2.84	0.005	-0.0763	-0.0139
			Gender: Female			
Domestic collab.	-0.0125	0.0372	-0.34	0.736	-0.0856	0.0605
Intl. collab.	-0.0290	0.0270	-1.07	0.284	-0.0821	0.0241
Single authors	-0.0741	0.0201	-3.68	0.000	-0.1136	-0.0346

Dep. Var.: $\mathbb{1}_i^{Els}$ using eq. (2.1). Standard errors heteroskedasticity-robust and clustered on the journal level. N = 222,779. Estimates for the plots in Figure 2.14.

Table 2.29: Effect of the Elsevier cut-off (subscriptions) decomposed by gender and collaboration type

Declaration of Contribution

I hereby declare that the chapter "The Role of Gender and Coauthors in Academic Publication Behavior" is coauthored with Justus Haucap and Leon Knoke.

All authors contributed equally to the chapter.

Signature of coauthor Justus Haucap: ______________________

Signature of coauthor Leon Knoke: ______________________

Chapter 3

How Transformative are Transformative Agreements? Evidence from Germany Across Disciplines

Single-authored project

Accepted for publication in 'Scientometrics'

3.1 Introduction

Publishing research without subscription paywalls is an established topic of the scientific community (Suber, 2012). To bolster the take-up of open access, large library consortia of universities and other research institutions negotiate so-called 'transformative agreements' (TAs) that shall transform the payments streams from journal subscriptions to payments for publishing papers with open access. In contrast to setting up new fully open access journals (such as *PLoS One* in the early 2000s), these agreements create an open access option within subscription-based journals or else cover the open access option in existing hybrid journals (see, e.g., Haucap et al., 2021). Those are outlets that require a subscription but allow for open access for single articles when paid additionally (e.g., 2,890 USD / 2,290 EUR + VAT in *Scientometrics* or equivalently in plenty of other journals).[1]

The agreements constitute a significant change currently proceeding in academic publishing. Due to the ongoing digitization of research, there are others as well. Mega-journals such as the already mentioned *PLoS One* break the chains of aggregating publications to an issue: The internet neither requires space limits nor papers to be bundled to physical journals sent out via mail. Preprint servers such as the non-profit platform *arXiv*, as well as *ResearchSquare* owned by Springer Nature, or *SSRN* owned by Elsevier, disseminate research without the need for a journal. The interdisciplinary electronic science journal *eLife* switched to publishing peer reviews alongside the submissions, replacing the established back-and-forth process of reviewing, editing, and eventually publishing a widely adjusted version as the definitive one (Eisen et al., 2022).

While transformative agreements, by design, transform many publications from restricted to open access, I demonstrate in this analysis across disciplines that they hardly transform the publication behavior of academics in the sense that they shift

[1]See `https://www.springer.com/journal/11192/how-to-publish-with-us#Fees%20and%20Funding`, last checked June 23, 2023. Fees are subject to change.

their publications to eligible publishers. The topic is rather new and, therefore, not extensively studied. Nevertheless, a small body of research already exists. Borrego et al. (2021) and Moskovkin et al. (2022) study these kind of contracts in a broad sense. The former examine what exactly is understood as a 'transformative agreement' while the latter investigate the research output before and after the closure of such an agreement. Haucap et al. (2021) use causal inference methods to study the behavioral reaction to introducing the German transformative 'DEAL' agreements, but only in chemistry. Schmal et al. (2023) take another approach. They use the German DEAL agreements as well but utilize them to study gender differences in the overall publication behavior of economists.

In this paper, I study the effects of the transformative DEAL agreements between the German research institutions and the academic publishers Springer Nature and Wiley across eight academic disciplines and a residual 'multidisciplinary' category. In particular, I reexamine chemistry and economics to replicate the findings of Haucap et al. (2021) and Schmal et al. (2023). I substantially extend their work by investigating the effect on publication behavior in environmental studies, philosophy, physics, psychology, material science, and dentistry. I consider 5,862 journals that published 6.1 million papers from 2016 to 2022. The central question is whether there is an increase in the likelihood of a paper being published in a journal owned or managed by the two publishers included in DEAL, Springer Nature and Wiley.

I can confirm the positive effects of chemistry and economics. In addition, I find a slightly significant effect in material science. However, there are no other significant reactions. In the subsequent analysis, I provide suggestive evidence for the substantial prevalence of null results. I discuss mainly two essential developments in academic publishing: First, the stark growth of fully open-access journals, second, the emergence of many additional transformative agreements closed between smaller consortia of German research institutions and other academic publishers. As these prominent developments do *not* seem to drive my results, I suggest that transformative agreements may trigger a 'Matthew effect' in publishing, such that the considerable DEAL agreements only affect fields in which Springer Nature and Wiley already have a prominent market position. In general, it is not trivial to net out other influential developments in the academic publishing market, such as those above, which hampers a clear-cut analysis. However, this imbroglio is by itself an important insight: Due to the plenty of initiatives attempting to reform the academic publishing market, it is difficult to purge the effects of each. This, however, makes policy evaluations more complicated.

The remainder of this paper is structured as follows. Section 3.2 comprehensively sketches the contract mechanism of the transformative agreements. Section 3.3 describes the empirical methods. Section 3.4 presents my findings and theoretical hypotheses as well as suggestive evidence to contextualize them. Section 3.5 concludes the study.

3.2 The Functioning of Transformative Agreements

Transformative agreements in academic publishing usually consist of a 'read' part that shall guarantee access to the existing body of research behind subscription paywalls and a 'publish' part that shall make all new submissions to the journal portfolio of a publisher fully open access for everyone.[2] Such agreements slightly vary conceptually, whether designed as a 'publish-and-read' or a 'read-and-publish' contract (Hinchliffe, 2019). Under a 'publish-and-read' regime, the publishers generate revenue only on a case-by-case basis instead of lump-sum subscription fees. For every publication, the institutions pay a fixed fee to the publisher, which also covers access to the publisher's portfolio of journals and papers. Every paper is published under an open-access license by default, and subscription fees to the covered publishers are abolished.

[2]See, e.g., `https://esac-initiative.org/about/transformative-agreements/`, last checked July 5, 2023.

In July 2019 and January 2020, an alliance of all German research institutions entered into such 'publish-and-read' agreements called 'DEAL' with the academic publishers Wiley and Springer Nature, which were meant to be the largest contracts of their kind back then.[3] While still having to pay submission fees, researchers do not have to administer the purchase of open access to their publications anymore but receive it free of charge and hassle. Neither do they have to apply for funding (e.g., from their institutional library or a third party), nor do they have to do any paperwork as the libraries entirely process the billing procedures in the background.[4] For the minority of journals already published fully open access by these two publishers, they are charged a 20% lower publication fee, which still has to be processed by the researchers individually and is not centrally billed by the clearing institutions.

The vast majority of the so-called hybrid journals allow researchers to benefit from the reputation of established outlets and free worldwide access to their research (see, e.g., Schmal, 2023b).[5] By and large, this led to an increase in the likelihood that a paper appears in an eligible journal in the field of chemistry (Haucap et al., 2021). As a byproduct of their research on publication behavior, Schmal et al. (2023) find a positive effect of the DEAL on eligible journals in the field of economics as well. The present paper shall broaden their findings by looking at the effect of the 'DEAL' on the likelihood of a paper being published in an eligible journal in various disciplines.

Basic economic reasoning suggests that the established hybrid journals should see a positive effect given that the outlet provides reputation, and open access may lead

[3]See the Springer Nature press release: `https://group.springernature.com/de/group/media/press-releases/springer-nature-projekt-deal/17553230`. Published January 9, 2020, last checked July 3, 2023.

[4]One peculiarity of the German case is that the alliance of research institutions also negotiated with the publisher Elsevier. After the failure of these negotiations, the publisher cut off researchers at virtually all German institutions from recent publications in its journals. This happened in July 2018, which coincides not directly with the start of the DEAL conditions, but still bears some simultaneity (Schmal et al., 2023).

[5]Note that there exists in parallel the alternative of 'green' open-access, i.e., the option that a researcher publishes their work in a restricted access journal but also shares it an a freely accessible repository.

to a broader audience reached, which may translate into more citations. While fully open access journals suffer from the detrimental incentive to accept more papers, which, ceteris paribus, should lead to a lower quality of the marginally accepted paper, and, in turn, of the average quality, (McCabe & Snyder, 2005), open access to established journals that do not rely directly on the revenue generated by the publications should not cause the acceptance of weaker submissions as long as they continue to generate subscription revenues from other countries or institutions. At least, the incentive to do as in the fully open access case should be smaller. For example, Elsevier claims for the influential 'economics of science and innovation' journal *Research Policy* that it has no impact on peer review or the chances of a paper being accepted whether one chooses restricted or open access for the paper at the beginning of a submission.[6]

3.3 Empirical Setting

I assemble a dataset of academic publications from 2016 to 2022 covering eight disciplines: environmental studies, philosophy, physics, psychology, material science, chemistry, dentistry, and economics. For chemistry, I use the data from Haucap et al. (2021) and add the publications for 2021 and 2022 that have not been part of their study. For economics, I use the data of Schmal et al. (2023), who have gathered data for this discipline and adjacent overlapping fields such as finance, management, and economic policy. I retrieve all publication records from the Scopus database using the Python library 'pybliometrics' of Rose and Kitchin (2019). My sample encompasses 6,125,687 observations consisting of articles and reviews; these two paper types (other than, e.g., editorials or comments) are the only types that fall under the DEAL conditions and count for the vast majority of publications. The papers have been published in 5,862 journals that are assigned to the disciplines as

[6]See the open-access instructions for *Research Policy*, `https://www.elsevier.com/journals/research-policy/0048-7333/open-access-options`, last checked July 18, 2023.

Table 3.1 displays. The number of papers per journal varies a lot across fields. In philosophy, a journal includes only a few publications. Physics, in contrast, is a field that hosts many 'mega journals' that publish thousands of papers each year.

I have selected the disciplines by two criteria: The number of journals should be narrow enough and manageable. Second, I want to cover various types of fields. For example, philosophy represents the humanities, dentistry the medical sciences, environmental studies represent earth sciences, material science and physics shall broaden the analysis of natural sciences. Psychology represents life sciences as well as social sciences, the latter together with economics. To obtain lists of relevant journals, I have used the Scimago database. It is a challenging task as journals are often related to several disciplines.[7] The matching is even fuzzier in social sciences and humanities. Table 3.1 details the paper count by field. Table 3.11 in Appendix A also shows the number of publications per year. One can see a steady increase from 2016 - 2022 with a decrease in 2021, probably due to the disruptions of the COVID-19 pandemic that had many direct and indirect effects on the productivity of researchers (Fischer et al., 2022; Abramo et al., 2022).

Table 3.12 in Appendix A displays the share of publications with a corresponding author from Germany.[8] It ranges from 2.31% in material science to 5.59% in economics and adjacent fields. The total share of publications with German corresponding authors is 3.31%. The number is lower than the share of publications assigned to Germany by the SCImago Country ranking (5.22% across all fields from 1996 -2022).[9] However, it is not entirely clear how the platform assigns papers to countries. Most likely, they count all publications with a German contribution, i.e., also publications with a coauthor from a German institution not being the corre-

[7]For example, *Quantitative Science Studies*, which is not part of the sample, is mapped to mathematics and to social sciences, where it belongs to the subgroups library and information sciences and cultural studies, see `https://www.scimagojr.com/journalsearch.php?q=21101062805&tip=sid&clean=0`, last checked July 25, 2023.

[8]The beneficial 'DEAL' conditions of frictionless and free open access do only apply if the *corresponding* author is affiliated with a German institution.

[9]See for details the SCImago Country ranking: `https://www.scimagojr.com/countryrank.php` using Scopus data up to April 2023. Last checked July 22, 2023.

sponding author. This approach would also increase the share in my sample.

Discipline	#Papers	Share	#Journals	Share
Env. Studies	862,043	14.07%	944	16.10%
Philosophy	98,718	1.61%	652	11.12%
Physics	917,083	14.97%	612	10.44%
Psychology	408,332	6.67%	997	17.01%
Material Science	785,064	12.82%	591	10.08%
Chemistry	1,493,148	24.38%	855	14.59%
Dentistry	107,135	1.75%	206	3.51%
Economics	198,773	3.24%	975	16.65%
Multidisciplinary	1,255,401	20.49%	886	15.11%
Total	6,125,687	100%	5,862	100%

Table 3.1: Publications and journals by field

I abstain from including medicine directly due to the COVID-19 pandemic, the massive shift towards preprint servers, and distorted research and publication behavior in this discipline. Of course, the pandemic has indirectly also affected the other disciplines, but less severely than medicine (Gao et al., 2021). Instead, I use dentistry, which is much narrower and less affected by the COVID-19 pandemic, to represent a medical discipline.

Generally speaking, I obtained the data in the following way. I began with a selection of suitable fields as described beforehand. Based on that, I obtained the lists of journals assigned to environmental studies, philosophy, physics, psychology, material sciences, and dentistry. For chemistry, I use the selection of Haucap et al. (2021), and for economics, the one of Schmal et al. (2023). For the latter two disciplines, I also use the data of the two cited publications. After receiving the data from Scopus for the other disciplines, I code those publications as 'multidisciplinary' that occur in several fields.[10] However, one should be aware that with this approach, a paper published in the *Journal of Economic Psychology*, being part of

[10]For technical reasons, I cannot do that with the publications for chemistry in the years 2016-2020, for which I use the data from Haucap et al. (2021). Therefore, there may be papers in the dataset that occur in chemistry and, for example, physics. I acknowledge that this is an issue, but because I conduct the analysis separated by field, it should not be a major one.

the psychology and the economics dataset, is classified in the same category as a paper published in *Progress in Polymer Science*. The reader should, therefore, take the 'multidisciplinary' category with a grain of salt.

Last, I elicit the country of the corresponding author as the DEAL only applies to corresponding authors (and not any author of a paper) being affiliated with a German institution. I only need the binary distinction German/non-German. So, it is trivial to identify those papers with author groups that are entirely non-German and those that are entirely German. The distinction is relevant for publications with authors from both types of institutions. For chemistry, as said before, I rely on the data of Haucap et al. (2021) and for economics on those of Schmal et al. (2023). Regarding the remaining six disciplines, I meticulously gather data for all observations that are coauthored by an international team with at least one author from a German institution using the Scopus database again.

Before moving on, I briefly elaborate on the corresponding author identification of Haucap et al. (2021) and Schmal et al. (2023). While the latter also meticulously gather the exact corresponding author and their country affiliation, Haucap et al. (2021) circumvent this task by using the first author as the corresponding author, assuming that the authors sort themselves by their importance and that the most relevant first author is also responsible for the correspondence. Ordering by importance or relevance is common across scientific fields (Lapidow & Scudder, 2019; Waltman, 2012). Alphabetical order declined between 1981 and 2011 (Waltman, 2012), even though Engers et al. (1999) derive that it is the only theoretical equilibrium for ordering coauthors. The literature mostly names three disciplines that deviate from the 'first author = most involved author' principle: mathematics, economics, and the subfield of high energy physics (Costas & Bordons, 2011; Waltman, 2012). As chemistry is not listed, I am confident that the approach taken by Haucap et al. (2021) is reliable, and I stick to it for this field.[11]

[11]Note that this holds for all years up to and including 2021. For 2022, there is an exact author identification. Any differences between the two methods should be negligibly small.

Econometrically, I apply a difference-in-differences design using a linear probability model to compute, ideally, the causal effect of the introduction of the DEAL agreements on the likelihood that a paper appears in a journal hosted by Springer Nature or Wiley. The dependent variable is a binary dummy indicating whether a paper appears in a journal later covered by the DEAL agreements. Here, I follow the approach of Schmal et al. (2023). I set the point of treatment at July 1, 2019, when the Wiley hybrid journals became part of the contract. Since it takes some time from submission to publication (Hadavand et al., forthcoming), the effect may not play a role initially. As a robustness check, I also compute the effect for January 1, 2020, when the Springer Nature journals entered the DEAL, which accounts for most journals. As I set up a canonical difference-in-differences model, I abstain from decomposing the treatment effect into time windows as done in an event study. It shall strengthen the ability of my model to detect an effect since my post-treatment observations only last until the end of 2022 and any effect requires months or even years to fully unfold.

Crucially, I control for the reputation of a journal captured by its relative position within a specific discipline (e.g., the *American Economic Review* would be in the top quantile in economics). In particular, I use the SCImago Journal Rank (SJR), which measures the academic impact of an academic outlet.[12] For higher tractability, I arrange the journals in quartiles *per discipline* to avoid varying SJR averages across disciplines distorting my findings. Such differences can be caused, for example, by discipline-specific citation habits. Suppose a discipline cites, on average, more papers than others. In that case, the total number of citations per journal should be, ceteris paribus, higher along the whole impact distribution than for other disciplines with a more parsimonious citing behavior. It leads to slightly varying quartile sizes, as

[12]Many different metrics exist for measuring a journal's quality, relevance, and impact. According to Mingers and Yang (2017), the SJR criterion is highly correlated with the impact factor (0.806), the 5-year impact factor (0.835), the article influence score AIS (0.906), and the 'source normalized impact per paper' criterion SNIP (0.807). That makes me confident that the SJR is suitable for quantifying journal impact.

shown in Table 3.2.

Quartile	#publications	Share	Cumulative
SJR quartile 1	1,536,240	25.08%	25.08%
SJR quartile 2	1,539,122	25.13%	50.21%
SJR quartile 3	1,521,019	24.83%	75.03%
SJR quartile 4	1,529,306	24.97%	100.00%
Total	6,125,687	100%	

The SJR criterion increases in impact. Quartile 1 embodies publications in journals with the lowest and quartile 4 those in journals with the highest impact. The empirical quartiles deviate slightly from the theoretical size of 1/4 as they are based on the journal-level SCImago ranking. Large journals at the quartile threshold slightly distort the categorization. Given the small size of the variation, this should be negligible. The SJR quartiles are computed on the final dataset after removing observations without an SJR value and reassigning duplicates. In Appendix B, Table 3.13 provides the numbers for the raw SJR quartiles, Table 3.22 provides the marginal effects.

Table 3.2: Number of publications by SJR quartile

Technically, I must exclude papers appearing in journals without an SJR value. In addition, I use the one-year lagged SJR values for each journal. For example, a paper published in 2020 is assigned the 2019 SJR value of its outlet. It accounts for the submission-publication lag, as a researcher can only consider a journal's reputation at the moment of the submission and not the publication.

I also include fixed effects for the time. To do so, I use categorical variables for the month and year of a publication. In addition, I interact the treatment covariate with the ranking quartile. In total, the regression equation looks as follows. I run a separate regression for each discipline and a pooled one in which I also control for the field. This is obsolete in the field-specific regressions.

$$\mathbb{1}_i^{Publ.} = SJR_i + \mathbb{1}_i^{GER} + \mathbb{1}_i^{DiD} + \mathbb{1}_i^{DiD} \times SJR_i + T_i' + \underbrace{field_i}_{pooled\,reg.} + \epsilon_i \tag{3.1}$$

The dependent variable $\mathbb{1}_i^{Publ.}$ on the left is a binary indicator that turns 1 if paper i is published in a journal covered by the DEAL. Using a linear probability model, I estimate the marginal effects of the covariates on the binary dependent variable switching from 0 to 1. On the right, the covariate SJR_i captures the SJR quartile of a paper's outlet. $field_i$ is a categorical identifier for the discipline a paper belongs to and is only included in the pooled regressions that include the observations of all fields. $\mathbb{1}_i^{GER}$ turns 1 for a corresponding author from Germany and is 0 otherwise. $\mathbb{1}_i^{DiD}$ is the difference-in-differences indicator variable that turns 1 if a paper has a corresponding German author *and* has been published after the DEAL was introduced. The interaction variables consist of the already explained covariates. T_i' is a time vector containing the two covariates $year_i$ and $month_i$. As a robustness check, I replace the two separate time variables with the binary indicator $\mathbb{1}_i^T$ that captures whether a paper has been published before ($\mathbb{1}_i^T = 0$) or after ($\mathbb{1}_i^T = 1$) the DEAL became active. I provide the results for the respective regressions in Tables 3.25 (pooled regressions) and 3.23 and 3.24 (discipline specific regressions) in Appendix B. Eventually, ϵ_i is the idiosyncratic error term.

A crucial econometric assumption is that only the treatment group is affected by the treatment. Given that several countries have formed consortia to negotiate transformative agreements,[13] this condition is not fully satisfied in my setting. I address this in two ways. On the one hand, I assign the whole world except for Germany as a control group in my primary analysis. By construction, it assigns some countries with their own transformative agreements to the control group. Due to the large number of countries, every country only counts for a meager share of publications. Hence, the individual impact of other transformative agreements should be negligibly low. My second approach supports this: I exclude those countries from my analysis that have the highest *share* of their publications covered by transformative agreements.[14] It includes contracts with Springer Nature and Wiley but also other publishers. Due to the binary setting in my analysis, the '$1 - p$' issue arises in the sense that a decision in favor of one publisher always implies a decision against any

[13]Here, I use the ESAC Transformative Agreement Registry, `https://esac-initiative.org/about/transformative-agreements/agreement-registry/`, last updated June 30, 2023, last checked July 13, 2023.

[14]For computational ease, I apply here the 'first author identification,' i.e., I exclude a paper if its first author is affiliated with an institution in a country with its own transformative agreements. As discussed beforehand, this is slightly fuzzier than the exact corresponding author identification but should be a sufficient approximation as the first author is not only often the corresponding one but also, for the large number of papers stemming from one country, it is correct as well.

other publisher.[15] So, transformative agreements closed with publishers other than Springer Nature and Wiley might still affect the likelihood of a paper appearing in the latter two as they become mechanically weakly less attractive when agreements with other publishers are concluded.

There are 595 transformative agreements that began in one of the years 2019 - 2022,[16] in 42 countries and by the 'eifl' association[17] for several developing countries (even though it covers only 60 planned annual publications and is, by that, very small). Counting only contracts with more than 100 annual publications, the number of TAs diminishes to 237 contracts in 32 countries. Among the 42 countries, transformative agreements cover only in sixteen of them more than 10% of the annual publications of these countries.[18]

Country	TA Pubs	Total Pubs	TA share
Sweden	22,846	45,694	50.00%
The Netherlands	3,1380	66,274	47.35%
Hungary	5,877	12,693	46.30%
Austria	10,153	29,338	34.61%
Norway	8,481	26,115	32.48%
Finland	6,821	23,400	29.15%
United Kingdom	54,987	224,582	24.48%
Ireland (Rep.)	3,563	17,124	20.81%
Spain	19,428	104,350	18.62%
Slovenia	1,277	7,214	17.70%
Switzerland	8,301	50,893	16.31%
Germany	29,305	195,359	15.00%
Denmark	41,10	31,341	13.11%
Australia	14,302	114,649	12.47%
Portugal	3,207	30,627	10.47%
Qatar	480	4,607	10.42%

Displaying countries with transformative agreements starting in the years 2019 - 2022 and having an accumulated share by country of > 10%. Source: ESAC registry, last update June 30, 2023, see `https://esac-initiative.org/about/transformative-agreements/agreement-registry/`.

Table 3.3: Share of transformative agreements on total research output by country

[15] This is equivalent to the $1-p$ problem in topic modeling, see, e.g., Schmal (2023c).

[16] Contract extensions are counted as separate contracts.

[17] See for information `https://eifl.net/page/about`, last checked July 24, 2023.

[18] To measure the annual publications of a country, I use the SCImago Country Ranking of 2019 and weight the annual number of publications (called "documents") by the annual (expected) number of publications as listed in the ESAC database. I use the year 2019 to net out direct negative or indirect catch-up effects of COVID-19 on publishing.

Table 3.3 displays the countries with transformative agreements that jointly account for at least 10% of a country's overall annual publication output. One can see that nearly all countries are located in Europe. Sweden, the Netherlands, and Hungary have the highest share of covered publications, while the United Kingdom has the highest absolute number of covered publications. Within this group, Germany has a relatively small share of only 15% of its annual publications covered. The distinctive feature is that the DEAL contracts with Springer Nature and Wiley account for nearly 2/3 of the covered TA publications in Germany (19,000 planned annual publications $\approx$ 64.84%).

Lastly, studying the situation in Germany, one has to be aware that in parallel to the introduction of the transformative DEAL agreements, the alliance of German research institutions also negotiated with Elsevier. However, both sides did not agree until 2023. Quite the opposite, most German institutions quit their subscriptions. As a consequence, since July 2018, the publisher denied researchers from these institutions access to recent publications in its journals (Fraser et al., 2023; Schmal et al., 2023). The rift may have induced researchers from affected institutions to shift their papers to other publishers' journals, such as those having closed DEAL agreements. In a sensitivity check, I exclude publications in Elsevier journals.

3.4 Results

3.4.1 Empirical Findings

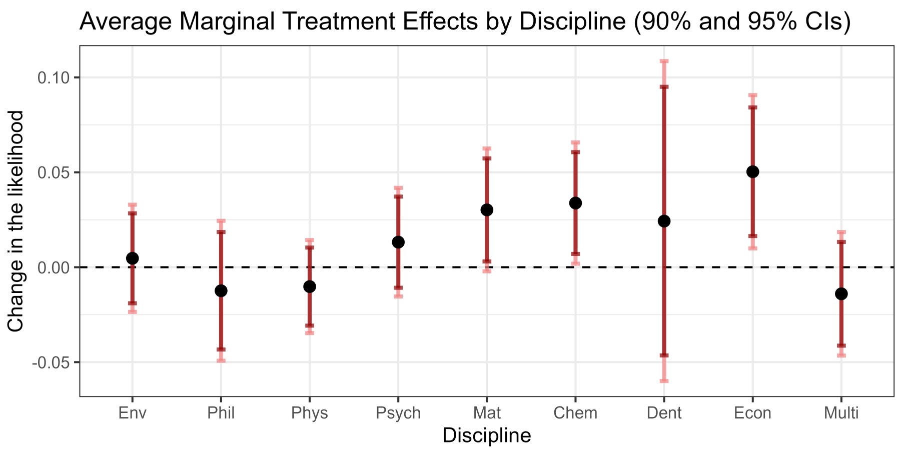
Using a difference-in-differences design, I compute the effect of the DEAL-induced frictionless open access incentive on researcher behavior across fields. Table 3.4 below presents the average marginal effects (AME) of the aggregate effect of the DEAL for the treatment point at the beginning of the Wiley contract on July 1, 2019. I report 95% confidence bands throughout my results. The raw number of observations is often high, especially for the natural sciences. Given that only publications from Germany after June 2019 are counted as treated, this number is much smaller, even for disciplines with many observations, as Table 3.14 in Appendix A highlights.

Treatment Time	AME	Std. Err.	t-statistic	p-value	95% CI	
Wiley	0.0019	0.0070	0.27	0.784	-0.0119	0.0157
Springer Nature	0.0024	0.0072	0.33	0.741	-0.0117	0.0165

Time of treatment: Wiley: July 1, 2019; Springer Nature: January 1, 2020. Controlling for time using year and month fixed effects. Standard Errors heteroskedasticity-robust and clustered on the journal level. N = 6,125,687. Separate regressions for the two treatment points.

Table 3.4: Average Marginal Effect on publishing in an eligible journal on aggregate

Looking at Table 3.4, the average marginal effect is insignificant and close to zero for both times of treatment. On aggregate, the DEAL apparently did not significantly change publication patterns of researchers up to now. Given that my data last until 2022, I have a treatment period of 3.5 years (3 for the Springer timing), which should have been enough time to capture existing effects even if one takes into account long turnaround times of submissions that lead to a staggered visibility of the actual effect. Thus, the result is unlikely to be driven by a too short time window.



90% Confidence bands in dark red, 95% confidence band extensions in light red provided. Standard errors heteroskedasticity-robust and clustered on the journal level. Each marginal effect is computed based on separate regressions for each discipline. Details on the coefficients can be found in Table 3.5 below.

Figure 3.1: Marginal effects separated by discipline

Contrary to the aggregate result, I find notable differences between the fields when decomposing the overall effect into one for each discipline. Figure 3.1 presents the average marginal effect (AME) for each field using the Wiley treatment timing, Table 3.5 below provides the details (Table 3.21 in Appendix B presents the results for the Springer Nature timing). The marginal effects are computed separately for each discipline.

	AME	Std.Err.	t-stat.	p-value	95% CI		*N*
Env. Studies	0.0047	0.0144	0.32	0.746	-0.0237	0.0330	862,043
Philosophy	-0.0124	0.0188	-0.66	0.509	-0.0493	0.0245	98,718
Physics	-0.0102	0.0125	-0.82	0.413	-0.0348	0.0143	917,083
Psychology	0.0132	0.0146	0.90	0.368	-0.0155	0.0419	408,332
Material Sc.	0.0302*	0.0165	1.82	0.069	-0.0023	0.0626	785,064
Chemistry	0.0338**	0.0163	2.08	0.038	0.0018	0.0657	1,493,148
Dentistry	0.0243	0.0430	0.56	0.573	-0.0605	0.1091	107,135
Economics	0.0503**	0.0206	2.44	0.015	0.0098	0.0908	198,773
Multidiscipl.	-0.0140	0.0166	-0.85	0.398	-0.0465	0.0185	1,255,401

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard Errors heteroskedasticity-robust and clustered on the journal level. Observations for each regression reported in the last column on the right. The coefficients are plotted in Figure 3.1 above.

Table 3.5: Marginal effects separated by discipline

I detect a significant positive effect of the DEAL agreements in material science, chemistry, and economics. The latter is qualitatively equivalent to the coefficient estimated by Schmal et al. (2023).[19] Journal publications are important in virtually every academic discipline, but the field of academic economics is particularly strongly involved with publication strategies (Heckman & Moktan, 2020; Fourcade et al., 2015), so the additional incentive of 'free' (for the researcher) open access may play a larger role for the discipline. Interestingly, the marginal effect for chemistry amounts to 3.64%, which comes close to the estimate of 3.81% of Haucap et al. (2021) using a heteroskedastic probit model. The crucial difference between the two estimates is that Haucap et al. (2021) only study a very short time window of 1.5 years, which ends with the observations of 2020. Given that my estimate for data

[19]The authors compute an effect size of 0.0469. It varies slightly in absolute terms due to the assignment of several journals to the 'multidisciplinary' category.

lasting two years longer is arguably the same, I carefully conclude that there seems to be no increase over time but more likely a single shift towards journals covered by the DEAL. It opposes the hypothesis that it takes time for the DEAL benefits to be spread among the research community. In that case, the effect should vary. Most likely, it should be higher the longer the time window lasts because more and more researchers had the chance to learn about the DEAL conditions in Germany. Lastly, material science also displays a positive take-up of the DEAL, highly similar to the effect on chemistry, but only significant on the 10% level ($p = 0.069$).

In contrast, I cannot detect any significance for the coefficients of the marginal effects in environmental studies, philosophy, physics, psychology, dentistry, and multidisciplinary publications. All coefficients are 'fully' insignificant in the sense that their p-values are not even close to any significance level. Furthermore, most of them are close to zero in absolute terms. Table 3.18 in Appendix A displays the twenty journals with the highest change in the share of publications with corresponding authors at German institutions. Five of them are related to chemistry and four to economics, the two disciplines for which I find significantly positive reactions to the introduction of the DEAL.[20]

Excluding countries with their own transformative agreements: As discussed in Section 3.3, transformative agreements exist with many publishers in several countries. As Table 3.3 has shown, 16 countries have concluded contracts that cover at least 10% of their annual publications. In this robustness check, I exclude 15 of them (the sixteenth is Germany in the treatment group) from the analysis. Table 3.6 below provides the marginal effects for the regressions excluding these countries. One can see that hardly anything changes qualitatively. Quantitatively, the effects vary only slightly. As before, I can only detect significant changes in ma-

[20]However, one has to acknowledge that also four journals from environmental studies and four from philosophy occur. As mentioned, both do not show any reaction in total. While for the latter, a lack of statistical power is easily conceivable, this does not really apply to environmental studies.

terial science, chemistry, and economics. The effect for chemistry, however, is now only significant on the 10% significance level ($p = 0.067$). Hence, neither removing or including these countries substantially affects the regressions.

	AME	Std.Err.	t-stat.	p-value	95% CI		N
Env. Studies	0.0005	0.0155	0.03	0.976	-0.0299	0.0308	716,543
Philosophy	-0.0112	0.0191	-0.58	0.559	-0.0487	0.0264	74,388
Physics	-0.0140	0.0139	-1.01	0.314	-0.0412	0.0133	811,939
Psychology	0.0150	0.0153	0.98	0.327	-0.0150	0.0449	304,449
Material Sc.	0.0308*	0.0170	1.81	0.070	-0.0026	0.0642	710,880
Chemistry	0.0321*	0.0175	1.83	0.067	-0.0023	0.0665	1,340,721
Dentistry	0.0178	0.0455	0.39	0.695	-0.0718	0.1075	90,259
Economics	0.0487**	0.0223	2.18	0.029	0.0049	0.0924	146,940
Multidiscipl.	-0.0170	0.0174	-0.98	0.327	-0.0512	0.0171	1,126,232

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard Errors heteroskedasticity-robust and clustered on the journal level. Observations for each regression reported in the last column on the right. The list of excluded countries can be found in Table 3.3, putting Germany as treated country aside.

Table 3.6: Marginal effects separated by discipline excluding countries with their own transformative agreements

On the one hand, this underlines the robustness of my primary approach, which assigns all countries to the control group. In contrast, many excluded countries also closed transformative agreements with Springer Nature and Wiley. Suppose these agreements had led to a higher interest in the journals of the latter two publishers. In that case, the treatment effect for authors from Germany should have been larger than before, as the transformative agreements with the DEAL publishers in other countries would have pushed the likelihood of a paper published from authors in the control group countries upward. Thus, *ex*cluding them from the analysis would widen the gap between treatment and control, but if and only if the transformative agreements in the excluded countries would have had a positive effect on Springer Nature and Wiley. Even though it is limited in its explanatory power, the high number of transformative agreements in those countries seems to generate a level playing field for publishers as not only the leading ones – namely Springer Nature and Wiley – benefit from frictionless open access, since the marginal effects are not pushed upwards when excluding the other TA countries.

3.4.2 Economic Mechanism

The large prevalence of null results across disciplines is, nevertheless, puzzling. While researchers in three disciplines take up the frictionless open access, all other fields remain inert. One potential reason for that is purely mechanical. Long lags between submission and publication of a paper may be an essential driver of the null effects. Many publications in the treatment window are likely to have been submitted before the DEAL contracts were in place. On the other hand, work submitted under the DEAL conditions may still need to be published. Thus, the actual treatment effect may be diluted by submission behavior yet unaffected as the DEAL was not in place when the authors decided where to submit their work and by submissions that did not turn into publications yet. With a longer post-treatment time window, this problem should disappear. In light of a treatment period of 3 - 3.5 years, this explanatory approach is not fully convincing. Aside from that mechanical reason, three different economic reasons are conceptually conceivable:

(I) Lack of knowledge – researchers may simply have not heard about the DEAL and its benefits. They cannot acknowledge an incentive they are not aware of.

(II) The incentive is not sufficient to change publication preferences either because the eligible journals have no relevance for the researchers at all or else all journals are stellar outlets in their fields such that it does not need any further incentive to encourage researchers to submit and subsequently publish their work in the DEAL journals.

(III) The benefit is outrivaled by incentives to publish in other journals in parallel.

Regarding hypothesis (I), it is difficult to figure out to which extent researchers have been informed by their institutions about the open access advantage in Springer Nature and Wiley journals. There exists sufficient effort by many academic institutions to disseminate information on how to publish with open access. However, it is questionable how quickly this insight diffuses. A lack of knowledge stands to reason as an explanatory channel for the plethora of null effects in addition to the fact that an existing effect may not be detectable *yet*.

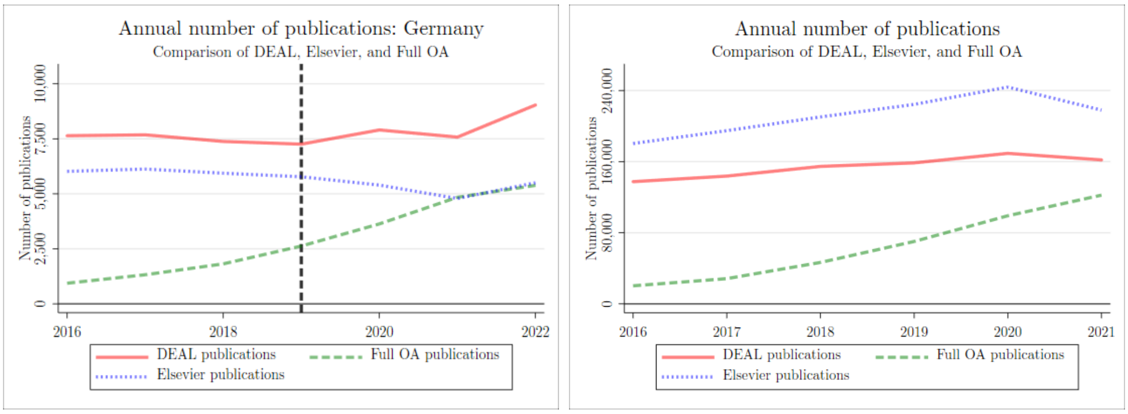
Hypothesis (II) claims that the incentive of frictionless open access in eligible journals does not play a role. To make this case, it is helpful to think about the two corner solutions: Either the journals of Springer Nature and Wiley are in their entirety so strong that even without the open-access incentive, every researcher in all the disciplines with null effects aims at getting their work published in a journal hosted by these two publishers. In that case, the additional open access does not change the behavior. The opposite, also in support of (II), is that all the covered journals are so weak in their reputation that additional frictionless open access does not change anything in the evaluation of the researchers. Free open access will not change their opinion if they never publish a paper in an eligible journal.

The latter corner solution is rather unlikely, given the broad spectrum the two publishers cover. According to the publishers, Springer Nature has a portfolio of more than 3,000 and Wiley of more than 1,600 journals.[21] In these large sets of outlets, one will find top-, mid-, and low-tier outlets for many academic disciplines. Haucap et al. (2021) and Schmal et al. (2023) could further show for chemistry and economics & management that the journal ranges of the two publishers varies but their journals are spread across the reputation scale using the SJR criterion.

However, the former interpretation could be true to some extent. Of course, the two publishing houses do not only run top-tier but also weaker journals that may benefit from the open access incentive. On the other hand, Springer Nature and Wiley are ranked second and third in terms of annual publications after Elsevier. Thus, in many fields, they already hold strong positions. If researchers choose other publishers, this might be for a reason that overrules additional open access

[21] See for Springer Nature: `https://www.springernature.com/gp/products/journals`, last checked July 14, 2023, and for Wiley: `https://onlinelibrary.wiley.com/library-info/products/journals`, last checked July 14, 2023.

covered by the DEAL. Put differently, journals of Springer Nature and Wiley could already have reached their full publication potential in the sense that reasons to publish in other publishers' journals may not be challenged by frictionless open-access. The consideration that other factors may outlevel the DEAL benefit leads me to Hypothesis (III).



The left panel plots the publications from authors with a German affiliation, the right panel those from all other authors. Full OA publications captures publications in journals of leading fully open access publishers. DEAL publications encompass publications in Springer Nature and Wiley journals. All three categories contain fully open access publications.

Figure 3.2: Development of publications over time

This last hypothesis states that other incentives may override the DEAL benefit and they were introduced in parallel to the DEAL. It corresponds to (I) in the sense that other incentives in specific disciplines could be more present to the researchers when deciding where to submit and publish their work. The most significant transition in the academic publishing market is the general move towards open access. While transformative agreements are one step in that direction, many fully open-access journals are emerging, i.e., journals publishing every paper with open-access by default. In turn, they cannot charge any subscription fees but do charge publication fees for every article.[22]

Figure 3.2 gives an impression of the development of publications over time, the left panel presents the situation in Germany, the right one the rest of the world. One

[22]This holds for the vast majority of so-called 'gold' open-access journals. 'Diamond' open-access publications go even one step further by publishing everything with open-access but *without* charging publication fees (see, e.g., Schmal, 2023a).

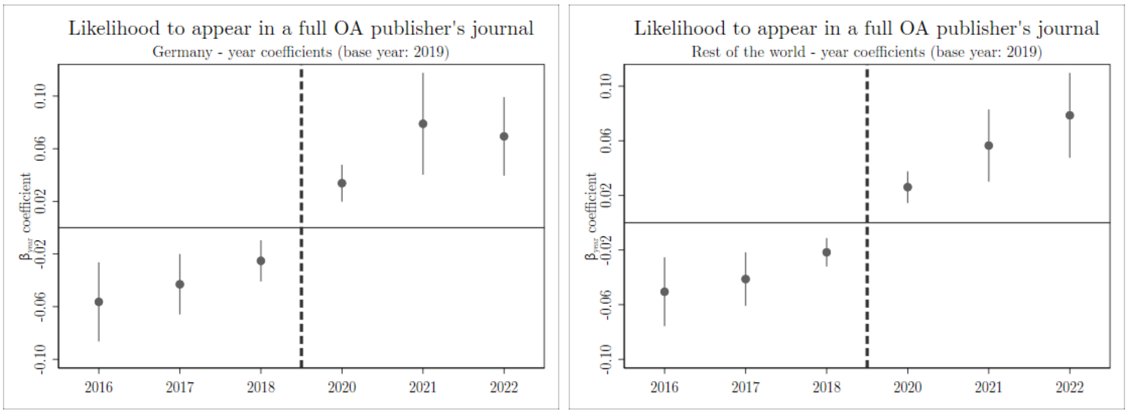
can immediately see that the group of leading fully open access publishers (eLife, Frontiers, Hindawi, MDPI, Public Library of Science (PLoS); in alphabetical order) faces massive growth, while the growth rate of the DEAL publishers Springer Nature and Wiley is generally lower, the same holds for Elsevier. In Germany, the absolute number of articles in Elsevier journals was even decreasing for several years. This might be related to the conflict between the German research community and the publisher, which I discuss later on.

The growth of fully open-access publishers: Taking a different perspective on the rise of fully open access publishers, Figure 3.3 displays the coefficients for the year dummies in an OLS regression that computes the likelihood of a paper appearing in a journal of a fully open-access publisher regardless of any other covariates. The coefficients are computed relative to the base year 2019, in which the DEAL started. The parsimonious setup only uses the year dummies as regressors. Hence, the equation is as follows:

$$\mathbb{1}_i^{Full\,OA} = \beta_y Y_i' + \epsilon_i \tag{3.2}$$

The dependent variable on the left is a binary variable that turns 1 if paper i is published in a journal of a fully open-access publisher named beforehand (i.e., eLife, Frontiers, Hindawi, MDPI, Public Library of Science). On the right, I use the vector of categorical year variables Y', which contains a dummy variable for each year. ϵ_i is the idiosyncratic error term, and I include a constant in the regression.

Especially in the most recent years, 2021 and 2022, the probability of appearing in such an outlet has significantly increased relative to the base year of 2019 in both Germany (LHS) and the rest of the world (RHS). Nonetheless, the year coefficients for the previous years, 2016-2018, are also significantly lower compared to the base year 2019. Hence, the growth in fully open-access publications has taken place for quite some time. It is an additional indicator that – besides the introduction of transformative agreements – several developments in academic publishing take place, which may weaken the statistical impact of such contracts. However, it is not directly distorting my analysis as it is not restricted to the treatment phase and neither exclusive to treatment or control group, but is a sustained process.

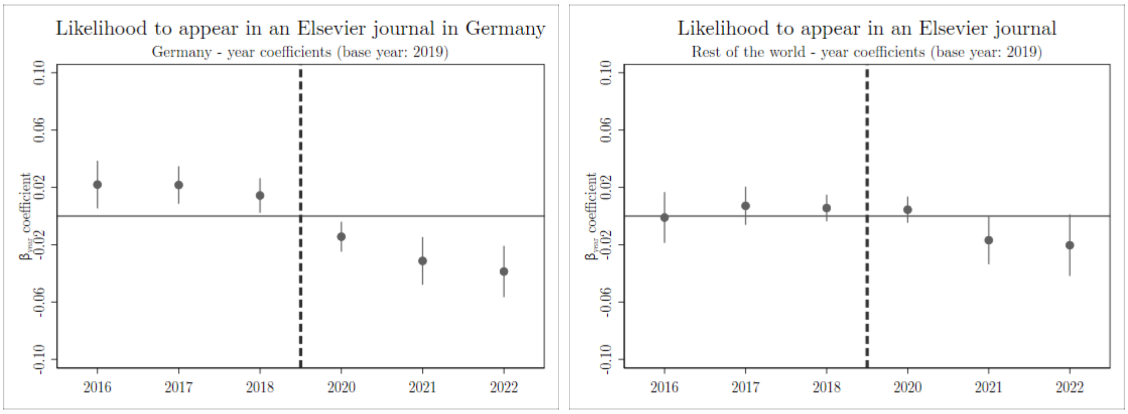


Dependent variable: Binary indicator whether a paper has been published in a journal of one of the following full open access publishing firms: eLife, Frontiers, Hindawi, MDPI, Public Library of Science. Explanatory variables: A set of dummies for each year as shown in eq. (3.2). Reference year: 2019. The dashed line in the plot marks this. Standard errors heteroskedasticity robust and clustered on the journal level. 95% confidence bands shown. $N_{LHS} = 203,055$, $N_{RHS} = 5,922,632$. LHS: Publications from Germany. RHS: Publications from all other countries. Numerical estimates are shown in Tables 3.26 (LHS) and 3.27 (RHS) in Appendix B.

Figure 3.3: Year coefficients for a paper to appear in a journal of a fully open access publisher

The transformative agreements with the established publishers Wiley and Springer Nature may have been closed 'too late,' so fully open-access competitors could have already established themselves. From a competition perspective, this is important as transformative agreements like the DEAL can potentially raise the market entry barriers for new fully open-access publishers (Schmal, 2023a). However, one has to consider that in the causal-inference framework of difference-in-differences, any *general* trend towards fully open-access journals should econometrically net out. Hence, given the shift towards these journals is highly similar in Germany and the rest of the world, it should not affect my results. However, differences can arise in case the take-up of fully open-access journals developed in Germany is different to the rest of the world in parallel to the introduction of the DEAL contracts.

The German Elsevier cut-off: While the patterns of the turn towards fully open access publishers are equivalent for Germany and the rest of the world, a notable difference arises for the leading commercial publisher Elsevier. Springer Nature and Wiley closed DEAL agreements with the German consortium. Elsevier was also in negotiations, but they ultimately failed.[23] This led to a cut-off of virtually all German research institutions from recent Elsevier publications in July 2018, which prevailed over the course of this study (Fraser et al., 2023; Schmal et al., 2023).



Dependent variable: Binary indicator whether a paper has been published in an Elsevier journal. Explanatory variables: A set of dummies for each year as shown in eq. (3.2). Reference year: 2019. The dashed line in the plot marks this. Standard errors heteroskedasticity robust and clustered on the journal level. 95% confidence bands shown. $N_{LHS} = 203,055$, $N_{RHS} = 5,922,632$ LHS: Publications from Germany. RHS: Publications from all other countries. Exact estimates are shown in Tables 3.28 (LHS) and 3.29 (RHS) in Appendix B.

Figure 3.4: Year coefficients for a paper to appear in an Elsevier journal

Figure 3.4 demonstrates that relative to the baseline year of 2019, the likelihood of a paper from a German corresponding author to appear in an Elsevier journal significantly decreased (only taking into account time variables as sketched in eq. 3.2). For the rest of the world, one can also see some slight negative development for Elsevier publications, but it is much less pronounced than the shift away from the publisher in Germany. Furthermore, the year coefficients are jointly insignificant at the 1% significance level as $F(6, 5859) = 2.64$, $p = 0.0148$, see Table 3.29 in

[23] After a new attempt was taken, the German research institutions and Elsevier concluded an agreement in September 2023, see the press release by the HRK representing the research institutions: `https://www.hrk.de/presse/pressemitteilungen/pressemitteilung/meldung/the-deal-consortium-and-elsevier-announce-transformative-open-access-agreement-for-germany-5006/`, published September 6, 2023, last checked December 11, 2023.

Discipline	#Elsevier.	#All.	Elsevier share
Env. Studies	1,816	6,015	30.19%
Philosophy	0	1,462	0%
Physics	1,547	10,570	14.64%
Psychology	1,114	5,818	19.15%
Material Sc.	1,710	6,691	25.56%
Chemistry	4,410	24,668	17.88%
Dentistry	106	1,417	7.48%
Economics	910	3,429	26.54%
Multidiscipl.	3,407	9,594	35.51%

Table 3.7: Market share of Elsevier across disciplines among publications from Germany ahead of the Elsevier cut-off

Appendix B for details.

The differences across disciplines might be simply driven by shifts away from Elsevier towards the two DEAL publishers. If so, the effect should be the strongest for disciplines in which Elsevier holds the highest market shares prior to the cut-off. Table 3.7 presents them below. Except for philosophy, where the publisher does not host any journals, and dentistry, it has a strong market position everywhere. The highest shares are among multidisciplinary journals (35.47%) and environmental studies (30.16%), both with null effects for the DEAL publishers. However, the positive effects I detect for material science and economics appear in fields with a strong presence of Elsevier as well. On the other hand, the publisher has a somewhat weaker position in chemistry.

The general shift away from Elsevier in Germany depicted in Figures 3.2 and 3.4 is part of the control group in the main regressions. Thus, excluding them implies that the downward trend of this publisher is missing, which bolsters the likelihood of appearing in a journal as part of the control group. In Table 3.15 in Appendix A, I present the sensitivity analysis for excluding Elsevier from the sample. One can see that the significantly positive effects for material science, chemistry, and economics disappear, and the marginal effect for the other disciplines remains statistically insignificant except for 'multidisciplinary' papers. Here, excluding publications in

Elsevier journals leads to a significantly negative coefficient. While the shift away from Elsevier may have rooted researchers in the three disciplines mentioned above to journals of the two DEAL publishers, the strong movement towards other publishers of multidisciplinary papers was statistically reduced by the presence of Elsevier in the control group. Excluding this publisher led to stronger growth in the control group, which, in turn, caused the marginal effect of the DEAL in this discipline to become negative.[24]

Competing transformative agreements: The DEAL agreements are, by far, the most extensive transformative agreements in Germany, simply because they are closed not only with two of the largest publishers but also because virtually all research institutions in Germany are part of the buyer consortium. Nevertheless, other contracts fall in the study's time frame as well. Besides the centralized DEAL consortium, plenty of consortia negotiated additional transformative agreements with other publishers. They encompass fewer institutions or a smaller and fixed amount of covered publications. The 'ESAC' database lists 61 TAs beginning in the years 2019 - 2022 aside from the two DEAL agreements.[25] The four years are consciously chosen as they cover the treatment window of my difference-in-differences analysis.

As an example, I look at TAs closed with publishers specializing in physics and multidisciplinary papers, as these are two disciplines with negative coefficients (even though wholly insignificant) and with a large sample size.[26] The Max Planck Digital Library (MPDL), an administrative branch of the Max Planck Institutes,

[24]The generally negative coefficient for multidisciplinary papers might be caused by the particularly strong market position of fully open-access publishers for these kinds of publications, e.g., MDPI or Frontiers. Figure 3.5 in Appendix A shows that the full open-access publishers grew particularly strongly (relative to 2019) in Germany in the field of multidisciplinary publications compared to the rest of the world. Together with the exclusion of the negative development of Elsevier, this is likely to explain the negative coefficient of the marginal effect of the DEAL for this field.

[25]See the ESAC Transformative Agreement Registry, `https://esac-initiative.org/about/transformative-agreements/agreement-registry/`, last updated June 30, 2023, last checked July 13, 2023.

[26]The field of philosophy has a negative marginal effect as well, but is a rather small discipline, as Table 3.1 beforehand demonstrates.

concluded transformative agreements with the American Physical Society (APS) and the American Institute of Physics (AIP) for the years 2020-2025 and 2020-2022 respectively. They expect to cover 350 (2020) or 380 (2021-2025) publications in APS journals and 120 (2020 - 2022) in AIP outlets. The TIB consortium, led by the eponymous TIB library of the Leibniz University Hannover, also negotiated a TA with AIP, which shall cover 550 publications annually from 2021 to 2023. Similarly, both organizations closed TAs with the Institute of Physics (IOP), which cover the years 2018-2024 (MPDL) and 2019 - 2024 (TIB). The MPDL plans to fund 140 publications per year (2018-2021) and, from 2022 on, 170 publications. The TIB agreements even encompass 400 papers in the first contract period (2019-2021) and 600 papers from 2022-2024. Hence, all of these agreements are non-negligible in their size. In addition, APS, AIP, and IOP are three leading discipline-specific publishers in physics.

Publisher	#Papers	Share	Cum.
Springer Nature [DEAL]	4,370	14.94%	14.94%
Elsevier	3,708	12.68%	27.63%
American Physical Society*	3,436	11.75%	39.38%
American Institute of Physics*	3,144	10.75%	50.13%
Institute of Physics*	2,410	8.24%	58.37%
The Optical Society	1,577	5.39%	63.76%
IEEE	1,239	4.24%	68.00%
MDPI	1,141	3.90%	71.90%
Royal Society of Chemistry*	1,116	3.82%	75.72%
Oxford University Press*	877	3.00%	78.72%
Other	6,222	21.28%	100%
Total	29,241		

The asterisk signs publishers which have concluded transformative agreements with German consortia aside from the DEAL that are active in at least one year in 2019-2022 and cover at least 100 expected publications. Springer Nature is exempt as it closed the DEAL. Overall time window: 2016 - 2022.

Table 3.8: Largest publishers by publications from German authors in physics

The transformative agreements fall directly in the treatment window of the DEALs. Even though I cannot provide causal evidence, it may support hypothesis (III) holds, i.e., other publication incentives could outrival the DEAL benefits

Publisher	#Papers	Share	Cum.
Elsevier	9,724	27.16%	27.16%
MDPI	4,750	13.27%	40.43%
Wiley [DEAL]	4,645	12.98%	53.41%
American Chemical Society*	3,768	10.53%	63.93%
Springer Nature [DEAL]	3,315	9.26%	73.19%
Royal Society of Chemistry*	1,489	4.16%	77.35%
Institute of Physics*	1,284	3.59%	80.94%
American Physical Society*	1,180	3.30%	84.24%
American Institute of Physics*	585	1.63%	85.87%
Electrochemical Society*	566	1.58%	87.45%
Other	4,493	12.55%	100%
Total	35,798		

The asterisk signs publishers which have concluded transformative agreements with German consortia aside from the DEAL that are active in at least one year in 2019-2022 and cover at least 100 expected publications. Springer Nature and Wiley are exempt as they closed the DEAL. The label 'multidisciplinary' captures those observations that occur at least in two different disciplines in the dataset. Overall time window: 2016 - 2022.

Table 3.9: Largest publishers by 'multidisciplinary' publications from German authors

or other characteristics of the publishing market in these disciplines are more dominant than the DEAL. Tables 3.8 and 3.9 show the ten largest publishers in terms of publications from authors with a German affiliation. The asterisk behind a name marks the existence of a transformative agreement between some German institutions or a German consortium, which is different from the DEAL project and includes at least 100 annual publications. One can easily see that among the ten most important publishers in physics in Germany, five of them have a TA aside from the transformative DEAL agreements with Springer Nature and Wiley. For multidisciplinary publications, there are even six publishers covered.

Market shares of the DEAL publishers: While it is a strong indicator that changes are happening in the academic publishing market aside from the DEAL agreements, there seems to be no clear evidence why the point estimates for physics and multidisciplinary publications are negative and insignificant. This is because – as shown in Tables 3.16 and 3.17 in Appendix A – there also exist five competing transformative agreements in the disciplines of chemistry and economics in Germany, which both significantly shift towards the DEAL outlets. Hence, further reasons are likely to exist for the difference between chemistry and the many null effects of the other disciplines.

Table 3.10 displays the differences in the market shares of the two DEAL publishers, Springer Nature and Wiley, ahead of the beginning of the DEAL conditions across disciplines and separated by publications from authors with affiliations in Germany and the rest of the world. One can see that both publishers have higher market shares in Germany than in the rest of the world in every discipline studied in this paper except for psychology.[27] However, the spread reaches its maximum for chemistry, dentistry, and economics. If one puts the difference in market shares in relation to the global market share (without Germany), one can see that Wiley and Springer's market share in Germany in chemistry is 73.6% higher compared to the rest of the world, as column (9) highlights. For economics, the difference counts for 42.8%, and for dentistry, it is 42.6%. Hence, the two disciplines that face a significant shift towards DEAL are already heavily relying on both publishers in Germany. In addition, the combined market share of Wiley and Springer Nature in Germany is the lowest for physics and the third lowest for multidisciplinary papers, considering all disciplines studied in this paper.[28]

Even though still suggestive, it is meaningful implicative evidence that the DEAL might be more substantial in those disciplines, in which Springer Nature and Wiley already possess a strong market position. Vice versa, for papers in physics and those assigned to multiple disciplines, Wiley and Springer Nature tend to have a less attractive portfolio ahead of the introduction of the DEAL agreements. It corresponds to the well-known 'Matthew effect' in science (Merton, 1968), which states

[27]This is reasonable in light of the strong position of Springer Nature in Germany due to the fact that Springer has its roots in Germany, where it was founded in 1842 and became one of the most important academic publishers in the first half of the 20th century, see `https://www.springer.com/gp/about-springer/history`, last checked August 14, 2023.

[28]As mentioned, dentistry and, in addition, philosophy are two disciplines with null effects but a particularly strong position of the two DEAL publishers. Here, the comparatively low number of observations is likely to cause the estimated null results. Especially for dentistry, the confidence intervals are quite large as one could see in Figure 3.1 beforehand.

Field	non-German publications DEAL	other	DEAL share	German publications DEAL	other	DEAL share	Δ abs.	rel.
Env.	85,290	241,196	26.1%	2,634	6,001	30.5%	4.4pp	16.8%
Phil.	9,006	32,377	21.8%	597	1,461	29.0%	7.2pp	33.3%
Phys.	64,582	377,125	14.6%	2,545	12,439	17.0%	2.4pp	16.2%
Psych.	35,513	135,549	20.8%	1,624	6,760	19.4%	-1.4pp	-6.7%
Mat.	69,151	304,250	18.5%	2,288	7,282	23.9%	5.4pp	29.1%
Chem.	149,830	650,818	18.7%	11,490	23,869	32.5%	13.8pp	73.6%
Dent.	11,724	36,294	24.4%	675	1,264	34.8%	10.4pp	42.6%
Econ.	17,906	67,718	20.9%	1,578	3,707	29.9%	8.9pp	42.8%
Mult.	79,065	363,672	17.9%	3,043	10,652	22.2%	4.4pp	24.4%
	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)

The DEAL column lists all publications of the two covered publishers Springer Nature and Wiley. All publications before July 1, 2019 are listed. The 'DEAL share' columns capture the share of the DEAL publications of the total publications. In particular, to compute columns (4) and (7), column (2) is divided by the sum of columns (2) and (3); column (5) is divided by the sum of columns (5) and (6), respectively. Column (8) shows the difference in DEAL shares, i.e., column (7) - column (4), in percentage points (pp), the last column (9) weights the absolute difference by the DEAL share of the non-German publications, i.e., dividing (8) by (4).

Table 3.10: Publications of the DEAL publishers across fields and separated by the country affiliation of the researchers

that successful researchers become even more successful by receiving high credit for collaborative work. The 'Matthew effect' in the present case relates to publishers. If they already possess a strong market position, transformative agreements may further bolster their market shares. Quite the opposite, if in a weaker position, the transformative agreements might not have the same effect.

3.5 Conclusion and Outlook

How transformative are transformative agreements? After looking at the results of this paper, the question can be altered to: Are transformative agreements transformative regarding publication preferences of researchers? By design, the DEAL fosters open access, including it for every publication in an eligible journal by default. Regarding competition in the publishing market, the evidence from Germany can offer only an opaque picture. Analyzing the impact of the two German transformative agreements with Springer Nature and Wiley on eight disciplines (and a residual multidisciplinary field) and 6.1 million publications raises one central issue: Either the actual (theoretically positive) effect is not quite visible in the econometric estimation, or the DEAL contracts do not change the publishing market in the sense that they cause overwhelming interest among academics to publish their work in eligible journals (at least in the short run). My data ends in December 2022, covering three years of treatment in the case of Springer Nature and even 3.5 years in the case of Wiley. So, even if such treatments require much time to unfold completely, the average turnaround time of a paper is shorter than the treatment window. However, it takes time until such policy changes become widely known among academics. Hence, even such a treatment window can only offer early evidence.

Plenty of null effects offset the positive effects observed in chemistry, economics, and materials science. They suggest that the multitudinous parallel upheavals in the academic publishing market are likely to play a role and are highly discipline-specific, particularly in Germany, where the cut-off from Elsevier has been an additional factor. The suggestive evidence that the effects are the strongest in those disciplines where the two treated publishers have had a dominant position ex-ante is not helpful for competition in these fields. In contrast, it does not (yet) seem to change the publishing landscape in disciplines where the two DEAL publishers do not possess such a vital role. If true and persistent over time, it does not support the concerns raised by Haucap et al. (2021) that the DEAL will foster concentration in the academic publishing market *per se*.

Looking at policy implications, the heterogeneous findings and the entangled environment raise the yet-to-be-answered question of how one can evaluate such interventions properly. Due to the high amount of money involved, this is a nontrivial task. Not only is a discipline-specific evaluation necessary, but my findings also raise the question of how the leading publishers react to potentially continuing declines in submissions as they are the only source of income under transformative agreements.

3.6 Appendix A

Year	#publications	Share	Cumulative
2016	725,057	11.84%	11.84%
2017	757,120	12.36%	24.20%
2018	819,885	13.38%	37.58%
2019	892,285	14.57%	52.15%
2020	953,805	15.57%	67.72%
2021	930,715	15.19%	82.91%
2022	1,046,820	17.09%	100%
Total	6,125,687	100%	

Table 3.11: Publications by year

Discipline	#Non German	#German	German share	Total
Env. Studies	841,110	20,933	2.43%	862,043
Philosophy	94,227	4,491	4.55%	98,718
Physics	887,832	29,251	3.19%	917,083
Psychology	389,408	18,914	4.63%	408,322
Material Science	766,921	18,143	2.31%	785,064
Chemistry	1,432,437	60,711	4.07%	1,493,148
Dentistry	103,426	3,709	3.46%	107,135
Economics	187,670	11,103	5.59%	198,773
Multidisciplinary	1,219,601	35,800	2.85%	1,255,401
Total	5,922,632	203,055	3.31%	6,125,687

Table 3.12: Publications by discipline and the share of German corresponding authors

Quartile	#publications	Share	Cumulative
SJR quartile 1	1,325,710	21.64%	21.64%
SJR quartile 2	1,546,492	25.25%	46.89%
SJR quartile 3	1,605,302	26.21%	73.09%
SJR quartile 4	1,648,183	26.91%	100.00%
Total	6,125,687	100%	

The SJR criterion increases in impact. Quartile 1 embodies publications from journals with the lowest impact and quartile 4 those from journals with the highest impact. Numbers for the raw SJR quartiles computed before removing duplicates and publications without an SJR value.

Table 3.13: Number of publications by SJR quartile – raw data

Field	Not treated	Treated	Total
Env. Studies	849,745	12,298	862,043
Philosophy	96,285	2,433	98,718
Physics	902,816	14,267	917,083
Psychology	397,792	10,530	408,322
Material Sc.	776,491	8,573	785,064
Chemistry	1,467,796	25,352	1,493,148
Dentistry	105,365	1,770	107,135
Economics	192,955	5,818	198,773
Multidiscipl.	1,233,296	22,105	1,255,401
Total	6,022,541	103,146	6,125,687

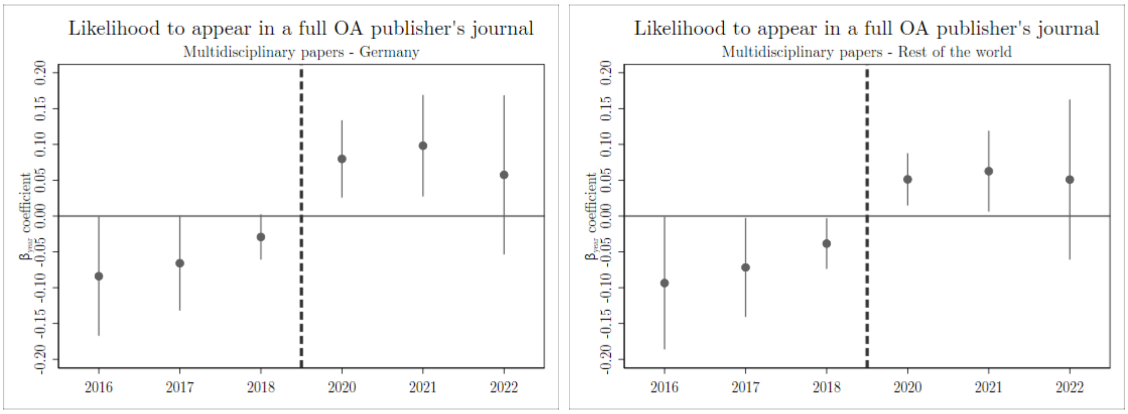
Column 2 – 'not treated' – aggregates all control group observations as well as treatment group observations ahead of the treatment.

Table 3.14: Fraction of treated observations by discipline

	AME	Std.Err.	t-stat.	p-value	95% CI		N
Env. Studies	-0.0157	0.0203	-0.77	0.441	-0.0555	0.0242	599,316
Philosophy	-0.0124	0.0188	-0.66	0.509	-0.0493	0.0245	98,718
Physics	-0.0176	0.0148	-1.19	0.236	-0.0468	0.0116	774,018
Psychology	0.0095	0.0210	0.46	0.649	-0.0316	0.0507	337,165
Material Sc.	0.0320	0.0210	1.52	0.128	-0.0092	0.0733	550,180
Chemistry	0.0289	0.0199	1.45	0.146	-0.0101	0.0680	1,125,873
Dentistry	0.0332	0.0491	0.68	0.500	-0.0637	0.1301	95,423
Economics	0.0394	0.0256	1.54	0.124	-0.0108	0.0896	150,026
Multidiscipl.	-0.0454**	0.0219	-2.08	0.038	-0.0884	-0.0025	813,372

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors heteroskedasticity-robust and clustered on the journal level. Observations for each regression reported in the last column on the right.

Table 3.15: Marginal effects separated by discipline excluding publications in Elsevier journals



Dependent variable: Binary indicator whether a *multidisciplinary* paper has been published in a journal of one of the following full open access publishing firms: eLife, Frontiers, Hindawi, MDPI, Public Library of Science. Explanatory variables: A set of dummies for each year as shown in eq. (3.2). Reference year: 2019. The dashed line in the plot marks this. Standard errors heteroskedasticity robust and clustered on the journal level. 95% confidence bands shown. $N_{LHS} = 35,800$, $N_{RHS} = 1,219,601$ LHS: Publications from Germany. RHS: Publications from all other countries. Exact estimates are shown in Tables 3.30 (LHS) and 3.31 (RHS).

Figure 3.5: Year coefficients for a paper to appear in a journal of a full open access publisher – multidisciplinary papers.

Publisher	#Papers	Share	Cum.
Wiley [DEAL]	15,811	26.04%	26.04%
American Chemical Society*	11,479	18.91%	44.95%
Elsevier	9,604	15.82%	60.77%
Royal Society of Chemistry*	6,668	10.98%	71.75%
MDPI	5,874	9.68%	81.43%
Springer Nature [DEAL]	4,831	7.96%	89.39%
American Institute of Physics*	1,085	1.79%	91.17%
de Gruyter*	814	1.34%	92.52%
Taylor & Francis*	634	1.04%	93.56%
Thieme	556	0.92%	94.48%
Other	3,351	5.52%	100%
Total	60,710		

The asterisk signs publishers which have concluded transformative agreements with German consortia aside from the DEAL that are active in at least one year in 2019-2022 and cover at least 100 expected publications. Springer and Wiley are exempt as they closed the DEAL. Overall time window: 2016 - 2022.

Table 3.16: Largest publishers by publications from German authors in chemistry

Publisher	#Papers	Share	Cum.
Elsevier	2,731	26.45%	26.45%
Springer Nature [DEAL]	2,236	21.65%	48.10%
Wiley [DEAL]	1,236	11.97%	60.07%
Routledge	516	5.00%	65.06%
de Gruyter*	471	4.56%	69.62%
Taylor-Francis*	403	3.90%	73.53%
Oxford University Press*	163	1.58%	76.61%
Emerald Group Publishing	243	2.34%	78.95%
Academic Press	237	2.29%	81.24%
Cambridge University Press*	163	1.58%	82.82%
Other	1,774	17.18%	100%
Total	10,327		

The asterisk signs publishers which have closed transformative agreements with German consortia aside from the DEAL that are active in at least one year in 2019-2022 and cover at least 100 expected publications. Springer and Wiley are exempt as they closed the DEAL. Overall time window: 2016 - 2022.

Table 3.17: Largest publishers by publications from German authors in economics

Journal Title	Change	Field	Publisher
Tijdschrift Voor Economische en Sociale Geografie (Journal of Economic and Human Geography)	0.2667	**Economics**	Wiley
Palaeobiodiversity and Palaeoenvironments	0.2297	Env. Studies	Springer Nature
PFG-Journal of Photogrammetry, Remote Sensing and Geoinformation Science	0.2185	Physics	Springer Nature
WMU Journal of Maritime Affairs	0.2015	Env. Studies	Springer Nature
Current Protocols in Nucleic Acid Chemistry	0.1822	**Chemistry**	Wiley
GENEVA Risk and Insurance Review	0.1818	**Economics**	Springer Nature
Journal of Biomolecular NMR	0.1792	**Chemistry**	Springer Nature
Journal of Neuropsychology	0.1724	Psychology	Wiley
Journal of Polymer Science, Part A: Polymer Chemistry	0.1677	**Chemistry**	Wiley
ChemistryOpen	0.1672	**Chemistry**	Wiley
International Economics and Economic Policy	0.1649	**Economics**	Springer Nature
Philosophy and Technology	0.1546	Philosophy	Springer Nature
European Journal for Philosophy of Science	0.1500	Philosophy	Springer Nature
Natural Resource Modelling	0.1461	Env. Studies	Wiley
IMF Economic Review	0.1351	**Economics**	Springer Nature
Engineering in Life Sciences	0.1337	Env. Studies	Wiley
Journal of Philosophy of Education	0.1208	Philosophy	Wiley‡
Ethik in der Medizin	0.1197	Philosophy	Springer Nature
Spectroscopy Europe	0.1183	**Chemistry**	Wiley
Progress in Photovoltaics: Research and Applications	0.1181	**Multidisc.***	Wiley

The asterisk marks the 'multidisciplinary' of 'Progress in Photovoltaics: Research and Applications,' because even though the category 'multidisciplinary' is not significantly affected by the DEAL, its journals are assigned to several other categories, in this case, the journal is assigned to Material Sciences and Physics. The former is significantly affected.
The change is computed as the cumulative annual nominal change in market shares relative to 2018, the year before the Wiley DEAL was established. Thus, 'change' uses the changes from 2022 to 2021, 2021 to 2020, 2020 to 2019, and 2019 to 2018 and adds them up. The table shows the 20 journals with the highest values for this type of change.
‡ Oxford University Press since 2023

Table 3.18: List of DEAL journals with the highest change (since 2018) in the share of corresponding authors from German institutions.

3.7 Appendix B

Treatment Time	AME	Std. Err.	t-statistic	p-value	95% CI	
Wiley	-0.0017	0.0076	-0.23	0.822	-0.0166	0.0132
Springer Nature	-0.0014	0.0079	-0.17	0.864	-0.0168	0.0141

Time of treatment: Wiley: July 1, 2019, Springer Nature: January 1, 2020. Controlling for time using year and month fixed effects. Observations of countries with transformative agreements that cover at least 10% of the annual publication output excluded. Standard errors heteroskedasticity-robust and clustered on the journal level. N = 5,377,369.

Table 3.19: Average marginal effect on publishing in an eligible journal on aggregate – excluding publications from countries with other transformative agreements

Treatment Time	AME	Std. Err.	t-statistic	p-value	95% CI	
Wiley	0.0019	0.0074	0.26	0.794	-0.0126	0.0164

Time of treatment: Wiley: July 1, 2019 (equivalent to the Springer Treatment timing in this setting). Controlling for time using year and month fixed effects. Observations in the months July - December 2019 are excluded. Standard errors heteroskedasticity-robust and clustered on the journal level. N = 5,762,310.

Table 3.20: Average marginal effect on publishing in an eligible journal on aggregate – without observations of the second half of 2019

	AME	Std.Err.	t-stat.	p-value	95% CI		N
Env. Studies	0.0095	0.0150	0.63	0.528	-0.0120	0.0389	862,047
Philosophy	-0.0021	0.0182	-0.12	0.907	-0.0380	0.0337	98,687
Physics	-0.0134	0.0147	-0.91	0.364	-0.0423	0.0156	917,083
Psychology	0.0103	0.0163	0.63	0.526	-0.0216	0.0422	408,332
Material Sc.	0.0365**	0.0181	2.02	0.044	0.0010	0.0719	785,064
Chemistry	0.0418**	0.0177	2.36	0.018	0.0071	0.0766	1,493,147
Dentistry	0.0292	0.0475	0.61	0.539	-0.0644	0.1228	107,135
Economics	0.0468**	0.0229	2.04	0.042	0.0018	0.0919	198,723
Multidiscipl.	-0.0097	0.0175	-0.55	0.580	-0.0440	0.0246	1,255,469

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors heteroskedasticity-robust and clustered on the journal level. Observations for each regression reported in the last column on the right.

Table 3.21: Marginal effects separated by discipline using the Springer Timing

	AME	Std.Err.	t-stat.	p-value	95% CI		*N*
Env. Studies	0.0037	0.0145	0.26	0.798	-0.0247	0.0321	862,047
Philosophy	-0.0121	0.0185	-0.66	0.512	-0.0484	0.0242	98,687
Physics	-0.0058	0.0134	-0.43	0.666	-0.0321	0.0206	917,083
Psychology	0.0121	0.0154	0.79	0.43	-0.0181	0.0423	408,332
Material Sc.	0.0334*	0.0181	1.84	0.066	-0.0022	0.0691	785,064
Chemistry	0.0356**	0.0165	2.16	0.031	0.0033	0.0680	1,493,147
Dentistry	0.0180	0.0495	0.36	0.717	-0.0796	0.1156	107,135
Economics	0.0522**	0.0209	2.5	0.013	0.0112	0.0932	198,723
Multidiscipl.	-0.0089	0.0171	-0.52	0.602	-0.0426	0.0247	1,255,469

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors heteroskedasticity-robust and clustered on the journal level. Treatment Timing: Wiley, July 1, 2019. Observations for each regression reported in the last column on the right.

Table 3.22: Marginal effects separated by discipline using the raw SJR quartile measure

	AME	Std.Err.	t-stat.	p-value	95% CI		*N*
Env. Studies	0.0028	0.0153	0.18	0.857	-0.0273	0.0328	862,043
Philosophy	-0.0060	0.0202	-0.29	0.768	-0.0457	0.0338	98,718
Physics	-0.0120	0.0136	-0.88	0.379	-0.0386	0.0147	917,083
Psychology	0.0246*	0.0132	1.86	0.063	-0.0013	0.0506	408,332
Material Sc.	0.0361**	0.0176	2.05	0.041	0.0014	0.0708	785,064
Chemistry	0.0314*	0.0169	1.86	0.063	-0.0017	0.0645	1,493,148
Dentistry	0.0356	0.0430	0.83	0.408	-0.0492	0.1204	107,135
Economics	0.0525**	0.0212	2.47	0.014	0.0109	0.0941	198,773
Multidiscipl.	-0.0109	0.0190	-0.57	0.567	-0.0481	0.0264	1,255,401

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors heteroskedasticity-robust and clustered on the journal level. Observations for each regression reported in the last column on the right. Timing with binary pre/post treatment dummy.

Table 3.23: Marginal effects separated by discipline using the Wiley Timing and 2×2 time controls

	AME	Std.Err.	t-stat.	p-value	95% CI		*N*
Env. Studies	0.0072	0.0155	0.47	0.639	-0.0231	0.0376	862,043
Philosophy	0.0021	0.0196	0.11	0.914	-0.0364	0.0406	98,718
Physics	-0.0093	0.0152	-0.61	0.54	-0.0392	0.0206	917,083
Psychology	0.0234	0.0151	1.55	0.121	-0.0062	0.0529	408,332
Material Sc.	0.0457**	0.0186	2.46	0.014	0.0092	0.0821	785,064
Chemistry	0.0442**	0.0183	2.42	0.016	0.0084	0.0800	1,493,148
Dentistry	0.0363	0.0462	0.79	0.433	-0.0547	0.1273	107,135
Economics	0.0504**	0.0236	2.13	0.033	0.0040	0.0968	198,773
Multidiscipl.	-0.0072	0.0193	-0.37	0.711	-0.0451	0.0308	1,255,401

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors heteroskedasticity-robust and clustered on the journal level. Observations for each regression reported in the last column on the right. Timing with binary pre/post treatment dummy.

Table 3.24: Marginal effects separated by discipline using the Springer Timing and 2×2 time controls

Treatment Time	AME	Std. Err.	t-statistic	p-value	95% CI	
Wiley	0.0046	0.0073	0.63	0.529	-0.0097	0.0188
Springer Nature	0.0075	0.0073	1.03	0.304	-0.0068	0.0217

Time of treatment: Wiley: July 1, 2019, Springer Nature: January 1, 2020. Timing with binary pre/post treatment dummy. Standard errors heteroskedasticity-robust and clustered on the journal level. $N = 6{,}125{,}687$.

Table 3.25: Average marginal effect on publishing in an eligible journal on aggregate using 2×2 time controls

Year	Coefficient	Std. Er.	t-statistic	p-value	95% CI	
2016	-0.0564***	0.0153	-3.67	0.000	-0.0864	-0.0263
2017	-0.0430***	0.0117	-3.67	0.000	-0.0659	-0.0200
2018	-0.0252***	0.0080	-3.15	0.002	-0.0409	-0.0095
2019	*base year*					
2020	0.0338***	0.0072	4.72	0.000	0.0197	0.0478
2021	0.0790***	0.0198	4.00	0.000	0.0402	0.1177
2022	0.0694***	0.0152	4.55	0.000	0.0395	0.0992
Constant	0.0907***	0.0228	3.97	0.000	0.0459	0.1355
$F(6, 4567)$	4.40					
$Prob > F$	0.0002					
R^2	0.0274					
N	203,055					

$p < 0.10$ *, $p < 0.05$ **, $p < 0.01$ ***. Standard errors are heteroskedasticity-robust and clustered on the journal level.

Table 3.26: Regression results for a paper from Germany appearing in a fully open access publisher's journal

Year	Coefficient	Std. Er.	t-statistic	p-value	95% CI	
2016	-0.0010	0.0090	-0.11	0.910	-0.0187	0.0167
2017	0.0071	0.0068	1.05	0.293	-0.0062	0.0205
2018	0.0056	0.0048	1.18	0.239	-0.0037	0.0149
2019	*base year*					
2020	0.0044	0.0047	0.95	0.340	-0.0047	0.0136
2021	-0.0169**	0.0086	-1.97	0.049	-0.0336	-0.0001
2022	-0.0204*	0.0110	-1.85	0.064	-0.0419	0.0012
Constant	0.2639***	0.0197	13.4	0.000	0.2253	0.3025
$F(6, 5859)$	5.92					
$Prob > F$	0.0000					
R^2	0.0243					
N	5,922,632					

$p < 0.10$ *, $p < 0.05$ **, $p < 0.01$ ***. Standard errors are heteroskedasticity-robust and clustered on the journal level.

Table 3.27: Regression results for a paper from the rest of the world appearing in a fully open access publisher's journal

Year	Coefficient	Std. Er.	t-statistic	p-value	95% CI	
2016	0.0220**	0.0085	2.59	0.010	0.0053	0.0386
2017	0.0217***	0.0067	3.24	0.001	0.0085	0.0348
2018	0.0143**	0.0062	2.33	0.020	0.0023	0.0264
2019	*base year*					
2020	-0.0144***	0.0053	-2.7	0.007	-0.0249	-0.0040
2021	-0.0313***	0.0085	-3.69	0.000	-0.0479	-0.0147
2022	-0.0387***	0.0091	-4.24	0.000	-0.0566	-0.0208
constant	0.2023***	0.0161	12.56	0.000	0.1707	0.2338
$F(6, 4567)$	4.61					
$Prob > F$	0.0001					
R^2	0.0035					
N	203,055					

$p < 0.10$ *, $p < 0.05$ **, $p < 0.01$ ***. Standard errors are heteroskedasticity-robust and clustered on the journal level.

Table 3.28: Regression results for a paper from Germany appearing in an Elsevier journal

Year	Coefficient	Std. Er.	t-statistic	p-value	95% CI	
2016	-0.0010	0.0090	-0.11	0.910	-0.0187	0.0167
2017	0.0071	0.0068	1.05	0.293	-0.0062	0.0205
2018	0.0056	0.0048	1.18	0.239	-0.0037	0.0149
2019	*base year*					
2020	0.0044	0.0047	0.95	0.340	-0.0047	0.0136
2021	-0.0169**	0.0086	-1.97	0.049	-0.0336	-0.0001
2022	-0.0204	0.0110	-1.85	0.064	-0.0419	0.0012
Constant	0.2639***	0.0197	13.4	0.000	0.2253	0.3025
$F(6, 5859)$	2.64					
$Prob > F$	0.0148					
R^2	0.0006					
N	5,922,632					

$p < 0.10$ *, $p < 0.05$ **, $p < 0.01$ ***. Standard errors are heteroskedasticity-robust and clustered on the journal level.

Table 3.29: Regression results for a paper from the rest of the world appearing in an Elsevier journal

Year	Coefficient	Std. Er.	t-statistic	p-value	95% CI	
2016	-0.0840**	0.0423	-1.98	0.048	-0.1671	-0.0009
2017	-0.0658*	0.0337	-1.95	0.051	-0.1321	0.0004
2018	-0.0293*	0.0161	-1.81	0.070	-0.0609	0.0024
2019	*base year*					
2020	0.0798***	0.0275	2.9	0.004	0.0258	0.1337
2021	0.0982***	0.0361	2.72	0.007	0.0273	0.1691
2022	0.0575	0.0566	1.02	0.310	-0.0536	0.1686
Constant	0.1104**	0.0556	1.99	0.047	0.0013	0.2195
$F(6,697)$	2.93					
$Prob > F$	0.0079					
R^2	0.0364					
N	38,500					

$p < 0.10$ *, $p < 0.05$ **, $p < 0.01$ ***. Standard errors are heteroskedasticity-robust and clustered on the journal level.

Table 3.30: Regression results for a paper from Germany appearing in a full open access publisher's journal – multidisciplinary papers

Year	Coefficient	Std. Er.	t-statistic	p-value	95% CI	
2016	-0.0935**	0.0472	-1.98	0.048	-0.1862	-0.0009
2017	-0.0717**	0.0352	-2.04	0.042	-0.1408	-0.0026
2018	-0.0384**	0.0180	-2.13	0.033	-0.0738	-0.0031
2019	*base year*					
2020	0.0512***	0.0186	2.75	0.006	0.0147	0.0877
2021	0.0626**	0.0288	2.18	0.030	0.0062	0.1191
2022	0.0509	0.0570	0.89	0.372	-0.0609	0.1627
Constant	0.1166**	0.0586	1.99	0.047	0.0016	0.2316
$F(6,885)$	2.86					
$Prob > F$	0.0092					
R^2	0.0276					
N	1,219,601					

$p < 0.10$ *, $p < 0.05$ **, $p < 0.01$ ***. Standard errors are heteroskedasticity-robust and clustered on the journal level.

Table 3.31: Regression results for a paper from the rest of the world appearing in a full open access publisher's journal – multidisciplinary papers

Chapter 4

The X Factor: Open Access, New Journals, and Incumbent Competitors

Single-authored project

4.1 Introduction

Open access to scientific research is a long-cherished dream that may become a reality in the digital age, as there are no longer physical barriers to disseminating new findings. Leading public bodies such as the US White House propose open science to "provide access to the results of the nation's taxpayer-supported research, accelerate discovery and innovation, promote public trust, and drive more equitable outcomes."[1] Nevertheless, researchers appear to remain skeptical regarding the quality of such publications. If one enters the query "are open access" into the search engine Google (see Figure 4.3 in the appendix), the autocomplete function suggests completing it as 'are open access journals peer-reviewed / bad / free / credible / cited more.' Not only the reliability of open access publications remains unclear, but the proposals of the search engine also address the often stated 'citation advantage' (see, e.g., Wang et al., 2015) of open access publications due to the absence of paywalls.

I utilize the unique situation of Elsevier operating fully open-access derivatives of established journals to answer three questions: Is there a citation advantage for open-access publications when quality is arguably the same? How do established and fully open-access journals differ in this regard? Which role play geography and external funding? Distinctive of my data is that I can net out any variation in quality while investigating 70 outlets and more than 120,000 publications across several disciplines. I do not only show that there exists no citation benefit for open access to publications in established incumbent journals. My results highlight a stark citation disadvantage for journals that have recently entered the market and lack a grown reputation. Due to the prominent role of citations for researchers (see, e.g., Teplitskiy et al., 2022), the latter finding represents a strong barrier to entry for

[1] See the announcement of the Biden-Harris administration, `https://www.whitehouse.gov/ostp/news-updates/2023/01/11/fact-sheet-biden-harris-administration-announces-new-actions-to-advance-open-and-equitable-research`, published on January 11, 2023, last checked July 10, 2023.

new (and usually fully open access) journals. This will perpetuate the oligopolistic market composition in scientific publishing.

After a few pilots ran in 2018, the scientific publisher Elsevier launched in 2019 a new open-access option by creating open-access forks of leading journals – the so-called 'X journals.' The distinctive factor of these outlets was that *"[t]he editorial and peer review process is identical for the parent journal and the OA mirror journal. ... During the submission process and just before acceptance, authors can choose whether to publish in the parent journal or the OA mirror journal ... [which] have the same title as their parent journal, distinguished by the letter X after their name."* (Harrison, 2019). The journals selected to receive a 'twin' were usually 'hybrid' journals, i.e., they operated with subscriptions but offered authors to publish an accepted article with open access for an extra fee. Furthermore, researchers can choose to store their papers additionally in freely accessible repositories.

The complex setting provides me with a quasi-causal setting as I can compare *two* types of open access with restricted access requiring a subscription to access the paper. Using Poisson regressions to compute the publication option's impact on a paper's citation count, I find no difference between publications in incumbent journals with any kind of open access to them and restricted access but a significant negative relation between a paper being published in an X mirror journal and its citations. The findings reemphasize the relevance of a journal's reputation for both the recipients and, ultimately, the authors. Given the enormous number of journals nowadays, a simple 'X' might be already enough to create the impression of a different outlet. It may decrease the willingness of authors to submit to such journals. While Elsevier already discontinued many X journals, this may apply even more strongly to newly set up open access journals that shall pave the way to open science because they do have not only different names but also other editorial boards than the leading (hybrid) journals. This paper is related to earlier work on the effect of open access on citations. Many publications refer to the early findings of Lawrence (2001) and Eysenbach (2006) who identify the so-called 'open access citation advantage.' However, these papers rather study online compared to print availability. Already a randomized controlled trial (Davis et al., 2008), as well as an early literature review (Craig et al., 2007), do not find any citation advantage anymore. McCabe and Snyder (2014, 2021) take a more granular look at this question. They introduce journal fixed-effects to control for unobserved heterogeneity in the outlets and found these effects to control for most of the citation advantage found in earlier studies. They also decompose the effect across perceived quality and identify a strong positive effect for the best journals but a significantly negative effect for weaker outlets. Furthermore, the effects of open access to the final versions of a paper in a journal are blurred by the easy and broad dissemination of working papers, pre-, and postprint versions on online repositories such as *SSRN* or *arXiv* (McCabe & Snyder, 2015).

This paper adds to the literature as it investigates differences between fully and hybrid open-access papers as well as papers requiring a subscription while completely netting out quality differences in the journals. Related to my approach is the study of Wang et al. (2015) about the switch of *Nature Communications* from restricted to open access. However, their work only compares point estimate averages without statistical testing. My paper investigates the availability effect of open access but also the reputation effect of a journal on citations. It further examines the relevance of the geography of affiliations as well as funding for choosing open access and the number of citations a paper receives. I identify an individual citation abatement for less well-established X journals, which strengthens the position of the large incumbent commercial publishers with their large stock of settled journals. However, strict open-access requirements of prestigious grants might not only enable a larger audience to read novel and meaningful findings but could also strengthen new market entrants among the journals, as my analysis highlights.

The remainder of this paper is structured as follows. In Section 4.2, I sketch the theoretical background of the dichotomy of X and parent journals and propose an economic mechanism of researcher behavior. Section 4.3 provides descriptive statistics and describes my empirical approach. Section 4.4 shows my results. In Section 4.5, I discuss the economic implications of my findings. Section 4.6 concludes.

4.2 Theoretical Background

The positioning of the 'X' journals: In general – and without the special case of X journals, there exist three types of journals by access type:

I Subscription-based journals that one can only access with a license that has to be acquired individually or, in most cases, by an institution. Examples of this are the journals of the American Economic Association, e.g., the *American Economic Review*.

II Hybrid journals that require a subscription but offer the authors to purchase open access to their paper for a fixed price. Many established journals nowadays have such a business model. An example of that is *Research Policy*. Here, the default is the requirement of a subscription to access a publication, but authors may purchase open-access to their paper by paying a fee of 3,710 USD + VAT.[2]

III Fully or gold open-access journals, in which every paper is published with open access by default and not upon a special order as in a hybrid journal.[3] An example is the interdisciplinary science journal *PLoS One*.

[2] See `https://www.elsevier.com/journals/research-policy/0048-7333/open-access-options`, last checked July 10, 2023.

[3] 'Gold' or 'full' open access refers to final publications without any access barriers. In contrast, 'green open access' means that for a final publication behind a paywall, a copy or earlier version is uploaded on a publicly available repository. While papers published with open access in a hybrid journal can also be considered gold open access, gold open access *journals* exclusively publish papers with open access in contrast to hybrid journals.

Aside from open access to the final publication via hybrid or gold open-access, there exists also the option of 'green' open-access, i.e., free access to a version of the paper stored in a freely accessible repository or on a website.[4] The benefit for readers is that they do not need to have a subscription or purchase the paper they want to read. The downside is that it is less convenient to search for a freely accessible version on a platform different from the journal or publisher website. Furthermore, they have to evaluate whether the 'green' version is clearly the same or just similar to the final version published in the journal.

The only source of revenue for fully open-access journals is publishing papers, as they cannot sell subscriptions by construction. It raises the gloomy incentive for editors to accept additional papers or for a publisher to pressure the editorial boards to accept more papers, which inevitably lowers the average quality of the published papers (McCabe & Snyder, 2005; Armstrong, 2015). Elsevier's introduction of X journals ties in with hybrid and fully open-access journals. While the existing parent journals usually were hybrid journals requiring a subscription but offering optional open access, the X derivatives became fully open-access journals. While there is no official reason why Elsevier introduced this second open-access channel, strong suggestive evidence exists in the institutional environment and the announcement by Harrison (2019).

In September 2018, the so-called 'cOAlition S' proposed 'Plan S' to push forward open access in academic publications. It was supported, among others, by the European Commission and the European Research Council, both major funders of research.[5] Principle #8 of 'Plan S' states that *"The Funders do not support the 'hybrid' model of publishing."*[6] Shortly later, Elsevier launched the first pilots for its X journals. One reason for their introduction has been that *"[t]he OA mirror*

[4]See, for example, Schmal (2023a) for more details on the open-access 'colors.' Examples for such repositories are *arXiv*, *ResearchSquare*, or *SSRN*.

[5]See the press release on September 4, 2018, `https://www.coalition-s.org/coalition-s-launch/`, last checked July 10, 2023.

[6]See `https://www.coalition-s.org/plan_s_principles/`, last checked July 10, 2023.

journals have not been launched with any one funding body in mind, but over the last two years, we have seen an increase in funders focusing on fully gold OA journals. We therefore hope that the OA mirror journals will provide another option that authors and funding bodies can consider."(Harrison, 2019)

Even though neither this quote nor the whole article refers explicitly to 'Plan S,' it stands to reason that Elsevier attempted to circumvent the funding ban for open access in hybrid journals by establishing fully open-access clones that fall under the conditions of the initial 'Plan S' and similar approaches. Already the first revision of 'Plan S' in May 2019 specifically pointed out that mirror journals are considered hybrid journals and are, therefore, excluded from funding.[7] It is likely one reason why the X derivatives never succeeded in the number of publications. Nevertheless, their existence provides me with a unique setting of three different access types that arguably have the same quality due to the same journal scope and, more importantly, the same editorial board.

After introducing the 'X' option, scholars interested in publishing their work with open access could choose between the full open access 'X' derivative or open access within the subscription-based parent journal. Figure 4.1 sketches this. I label the combination of the parent journal and its mirror 'journal compound' as it aggregates the three types of access: *restricted access* and *subscription-based open access* within the parent journal, and *X open access* within the mirror X journal. As cited above, editorial boards and the peer review were the same for the parent and the mirror journal. By that, the variation between restricted access and subscription-based open access publication is accessibility. The variation between restricted access and X open access is accessibility and a different name (the added X), including a different ISSN identifier, which also led to different citation metrics. The only difference between hybrid and X open access is the variation in the journal's name and the quantitative reputation measures (as between RA and XOA), because the

[7] See for details `https://www.coalition-s.org/rationale-for-the-revisions/`, published May 31, 2019, last checked July 10, 2023.

qualitative assignment of reputation was meant to be adopted from the established parent journal.

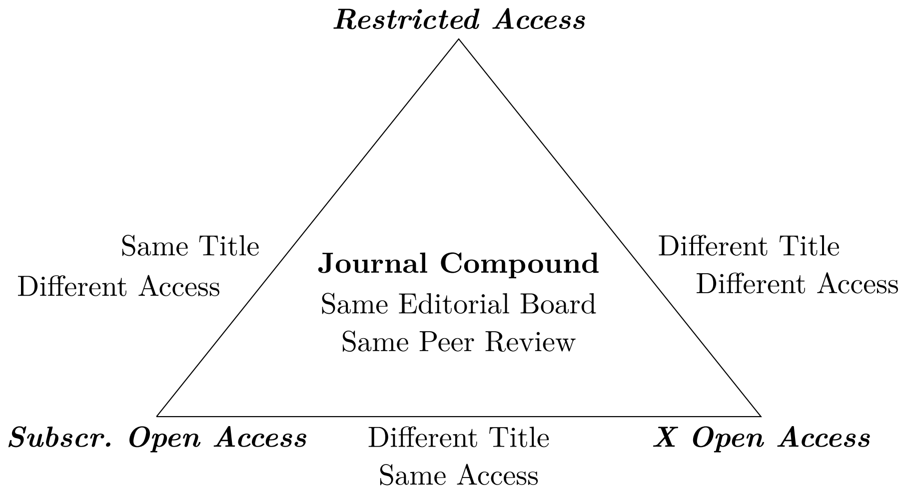


Figure 4.1: Structure of the three access options within a journal compound

As reported by Asai (2023), the prices for X and subscription-based open access were nearly always the same in the initial year 2019 but varied considerably afterwards. Elsevier stopped the X experiment after a short period for many journals and returned step by step to only using the hybrid publication model (Shortliffe & Peleg, 2019) and discontinuing many of the X derivatives. While a couple of X journals still exist in the 'tradition' of a mirror journal, others have separated from their parent journals by electing new editors – even though a notable overlap between the editorial boards often remains.

Economic mechanism: Researchers have a dual role in academic publishing. On the one hand, they are authors of papers. On the other hand, they are readers. As authors, they want to publish their papers – optimally in a highly respected journal in their field or else in the highest journal possible with respect to the scope and methods of their paper.[8] As recipients, it is essential to read material of a high

[8]There exist alternative incentives such as generating a lot of public attention or publishing many papers in a short period of time. Both may lead to journal choices that do not only take into account the ranking of a journal. However, it is reasonable to assume that a journal's reputation

quality that benefits their work and that they can rely on. By construction, only a tiny share of papers can appear in top-ranked publications that everyone in a field knows, so many publications appear in journals that may be less established and known within a field or across fields. When referring to such papers, researchers must ensure that it is work of decent quality.

As there exist thousands of journals, researchers need heuristics to accelerate the evaluation of a paper's outlet when conducting, for example, a literature review. An evident approach is an evaluation of the author or group of authors. For the group of persons a researcher personally knows, evaluating their ability and the corresponding (expected) quality of the paper is relatively easy. However, when personal relations do not exist, researchers may use other approximations to evaluate the inherent ability of a researcher and the derived quality of their paper. One indicator is an author's affiliation, as many consider a publication of researchers from a prestigious university as a high-quality publication simply based on the institutional reputation. While this is a granular approach, it comes with high arbitrariness in the cut-off decisions.[9] To avoid this I use a broader but clear-cut evaluation based on geography.

For this geographical distinction, I use the emerging separation between the so-called 'global North' and the 'global South,' which implies a distinction between economically developed countries with high GDP per capita (North) and likewise developing countries (South). The distinction may also reflect reputation of and tacit trust in the higher education systems and, by that, in the quality of the research output. All things equal, it may be easier for publications from the global North to gain interest and, subsequently, citations relative to publications coauthored by researchers based in the global South. In addition, I distinguish along the lines of the World Bank's income classification. Lastly, I use the age of the university system as separating factor and approximation for trust in the research output.

is for many researchers likely to be the strongest incentive to publish there.

[9]For example, it is unclear how many universities are considered as leading or how to capture the composition of author groups in terms of top-, mid- or low-tier institutions.

Instead of looking at the characteristics of the authors, a conceivable alternative is evaluating the journal in which the paper in question is published. To do so, journal rankings such as the Journal Impact Factor (JIF) may serve as a quick and convenient way to get an impression of an outlet's reputation, as such measures may carry more information than the mere count of citations of a paper does (Laband, 2013; Osterloh & Frey, 2020) – even though critics argue that metrics such as the JIF are essentially constructed based on aggregating citation counts, which carries its own problems (Wooding, 2020). Nevertheless, the number of citations might not fully reflect the actual rigor of a paper but also how topical its content is or whether it contributes to a highly debated issue. Moreover, in their role as authors, researchers use rankings as well. Here, they use it to decide where to publish their work (Śpiewanowski & Talavera, 2021). Hence, a higher journal ranking is likely to increase the credibility of a research paper, which, in turn, should foster citations.

A third driver of citations is the availability of research. One can only read work that is accessible. Hence, for every paper published with access restrictions, the researcher must have a compatible license to read the paper.[10] This might not always be the case, especially for less essential journals or those with a strong regional focus. Open access may enable more researchers to read and cite one's work in such situations. It should, therefore, be an upward driver of citations, regardless of the journal's quality.

$$Citations = f(paper\,quality,\ journal\,reputation,\ accessibility)$$

One can aggregate the sketched drivers in a simple form that describes citations as a function of a paper's underlying quality, which is often difficult to observe directly.[11] Furthermore, it is a function of the reputation of the journal it is published in. Last,

[10] A possible but illegal workaround of paywalls is using predatory repositories such as 'sci-hub.'

[11] In addition, it is, of course, debatable what 'quality' actually means. Ellison (2002a) suggests a distinction between *'q'* and *'r'* quality, where the former means the impact of the main ideas carried in the paper. The latter encompasses the other aspects that may be typically requested by referees, such as the technical rigor of a paper, e.g., robustness checks of empirical estimations or generalizations of theoretical models.

it is considered to be a function of accessibility. Clearly, all three factors increase the number of citations if they go up. If a paper is published under an open-access license, it increases its availability.

The journal's reputation channel is more ambiguous. In my setting, the distinctive factor is the overall reputation correlated with the open access *type*. Here, a 'birth defect' of Elsevier's X journals plays a significant role. Due to their (assumed) intention to circumvent the ban of hybrid journals from open-access funding, the publisher needed to establish independent journals with their own identifying ISSN number that differs from the respective parent journal. This, however, also implies an independent journal impact factor and many other metrics. Put differently, although the journals borrowed their names (except for the additional 'X,' the editorial board, and the rigor in peer review from their established incumbent parents, they were formally new journals. Hence, they had no long history of publications, no journal impact factor, and a name that also varies at least slightly.

Adding an 'X' to the name might appear negligible at first sight. Looking at the discipline of economics, one quickly sees that small differences may have large implications. While the *Economic Journal* – hosted by the British Royal Economic Society – is a leading outlet, this does not necessarily apply to the '*Economia Journal*' from the Latin American and Caribbean Economic Association. The *Journal of Economics and Statistics* is hardly similar to the *Review of Economics and Statistics*. The *Eurasian Economic Review* has different standing than the *European Economic Review*. These and many other examples sound highly similar and often share the same abbreviations but differ conspicuously in scope, method, and rankings. In a world with thousands of similar-sounding journals, adding an 'X' is not just a further letter but might imply a wholly different journal.

4.3 Descriptive Statistics and Empirical Strategy

Data and Descriptive Statistics: The core of the data forms a list of 35 journal compounds, i.e., the pairs of the established 'main' journals and its 'X' fork. The set is based on the list used by Asai (2023) as well as on a manual search of Elsevier's journal library. I have retrieved the metadata of all publications in these compounds from 2018 until 2022 directly from the Scopus database using the *pybliometrics* wrapper of Rose and Kitchin (2019). It carries the benefit that Elsevier hosts all journals and the database. Thus, it is likely that the publication records are highly accurate. Starting with 128,364 publications, I only proceed with the paper types 'article,' 'review,' and 'note.' The three categories account for 97.1% of all records. Furthermore, letters to the editors, errata, and editorials have other functions than disseminating novel research, so I abstain from including them. In total, I use 123,939 publications in 70 journals or else 35 journal compounds.[12]

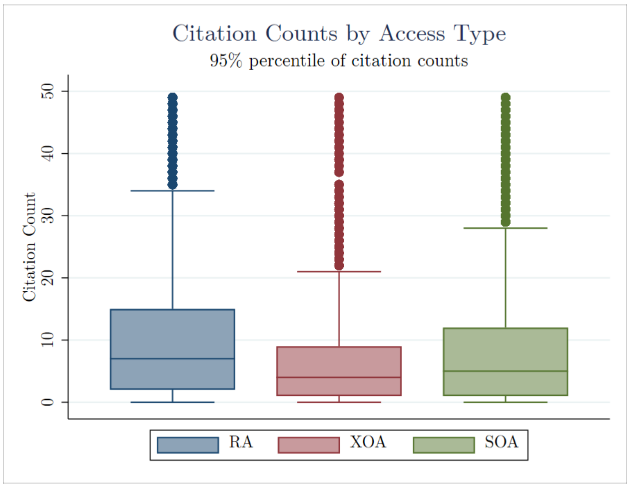
Year	Restricted Access	X Journal OA	Subscription OA	Total
2018	21,114	11	1,941	23,066
2019	21,560	714	2,063	24,337
2020	21,568	565	2,358	24,491
2021	21,807	516	3,301	25,624
2022	21,779	698	3,944	26,421
Total	107,828	2,504	13,607	123,939

Table 4.1: Number of publications by access type and year

Table 4.1 shows the distribution of the papers across the five years included in the analysis. There are few X journal observations in 2018, when Elsevier experimented with the new format, followed by a steep increase in 2019 when the X journals offi-

[12]I manually verified that the journal compounds continue to share the same editorial board, aims and scope, and peer review process. I remove those records of X journals that separated from their parent journals. This affects *Atmospheric Environment X* from 2021 on, *Biosensors and Bioelectronics X* from 2020 on, *Food Chemistry X* from 2022 on, and the *Journal of Asian Earth Sciences X* from 2020 on. In total, I lose 702 observations by that. Furthermore, the *International Journal of Pharmaceutics* and *Vaccine* both have X derivatives that do not share the same editorial board but boards with extremely high overlap. I keep them in the sample but run a robustness check that shows that excluding both journals does not qualitatively affect my results.

cially entered the market. Open access (green or hybrid-gold) to publications in the incumbent parent journals (SOA) occurs much more often. The lion's share of publications in the present sample is still published without open access, though. Table 4.2 shows all 35 journal compounds and the number of publications differentiated by the publishing type. It also lists whenever an X journal has been discontinued in the time window of the analysis or whether it separated from its parent journal in a way that it got its own editorial board and scope. I obtained the publication data from March 20-30, 2023.



The 95th percentile of the overall distribution of citation counts corresponds to 49 citations. Meaning of the abbreviations: RA = restricted access, SOA = subscription-based open access, XOA = X journal open access.

Figure 4.2: Boxplots for the number of citations by access type

The core variable of interest is the number of citations a publication has received. The overall mean citation rate is 13.89, the median is 7, the minimum value is, obviously, 0, and the maximum is 5,946 for a paper by Harris et al. (2019) about a global data platform for medical research. Figure 4.2 highlights that the differences in the number of citations do not only vary in the average count but also the overall distribution, as one can draw in particular from the inter-quartile range and the upper adjacent values. Radicchi et al. (2008) find an overall 'universality of citation distributions' across fields, meaning that the accumulation of citations varies across fields, but the overall pattern is similar. Waltman (2012) shows in a reconsideration

Compound	RA	XOA	SOA	Total	Notes
Analytica Chimica Acta	4,068	27	318	4,413	
Atmospheric Environment	2,605	96	483	3,184	X sep. in 2021
Biosensors and Bioelectronics	3,821	10	325	4,156	X sep. in 2020
Chaos, Solitons and Fractals	3,710	51	158	3,919	
Chemical Engineering Science	3,629	98	211	3,938	X disc. in 2023
Chemical Physics Letters	3,968	25	176	4,169	X disc. in 2021
Computers and Graphics	563	8	86	657	X disc. in 2019
Contraception	677	83	164	924	
Cytokine	1,373	36	172	1,581	
Ecological Engineering	1,314	15	164	1,493	X disc. in 2021
Energy Conversion and Mgmt.	5,535	307	413	6,255	
European Journal of Obstetrics...	2,003	134	150	2,287	
Food Chemistry	10,581	120	883	11,584	X sep. in 2021
Gene	3,451	30	218	3,699	X disc. in 2021
Intl. Journal of Pharmaceutics*	4,203	125	490	4,818	
Journal of Asian Earth Sciences	1,631	8	58	1,697	X sep. in 2020
Journal of Biomedical Informatics	91	20	896	1,007	X disc. in 2020
Journal of Computational Physics	3,152	68	262	3,482	X disc. in 2023
Journal of Dentistry	817	13	201	1,031	X disc. in 2020
Journal of Hydrology	5,241	93	658	5,992	
Journal of Non-Crystalline Solids	2,572	103	181	2,856	
Journal of Pediatrics	2,033	23	1,202	3,258	
Journal of Structural Biology	473	72	159	704	
Materials Letters	9,083	127	232	9,442	
Nutrition	1,222	9	174	1,405	X disc. in 2021
Optical Materials	4,732	164	126	5,022	
Research Policy	609	3	220	832	X disc. in 2020
Resources, Cons. and Recycling	2,060	29	452	2,541	X disc. in 2020
Respiratory Medicine	333	24	877	1,234	fully disc. in 2021
Sleep Medicine	1,471	46	188	1,705	
Toxicon	1,006	112	137	1,255	
Vaccine*	3,230	204	1,824	5,258	
Veterinary Parasitology	822	29	121	972	X disc. in 2020
Water Research	4,504	127	844	5,475	
World Neurosurgery	11,246	65	383	11,694	X sep. in 2022

RA = restricted access, SOA = subscription-based open access, XOA = X journal open access.
* marks journal compounds with highly similar but not equal editorial boards for X and parent journal. 'disc.' is the abbreviation for 'discontinued,' 'sep.' abbreviates 'separated.'

Table 4.2: Number of publications by journal compound and access type

that this universality holds for many but not (almost) all fields. Nevertheless, they find that one comes closer to the initial hypothesis once papers without citations are excluded. I keep the zero-cited papers in my data but conduct a robustness check excluding them, confirming the results.

Empirical Strategy: I apply Poisson regressions to regress the number of citations of a particular paper, which is count data, on a categorical variable for the access option and further covariates.[13] In the main specification, I use a categorical specifi-

[13]In the case of overdispersion, a negative-binomial specification might appear preferable. However, a Poisson estimation bears the significant advantage that its coefficients remain consistent

cation with three outcomes – restricted subscription-based access (RA), open-access to a paper in a parent journal (SOA), and X open-access (XOA) to a paper in an X journal – to estimate the effect of the separate open-access options relative to restricted access.

Regarding the control variables, I follow the approach of McCabe and Snyder (2014), who add age and age^2 to capture the nonlinear time dimension of citations where I define $age = 2023 - publication\ year$. One shortcoming of my data is that I can only use cumulative citations per paper and no citation windows that analyze the number of citations within a fixed time window. Wang (2013) finds a window of three years to be most informative in most cases. I address this by two sets of regressions that study solely the year 2019 for a three-year citation window or else the year 2020 for the two-year equivalent.

I also control for the number of authors as more authors may lead to more citations since the paper might be more visible. I add a journal fixed effect but use the journal compound, i.e., the combination of parent and X journal, as the journals are rooted in different disciplines and within these disciplines in different relative reputational quantiles. I cluster the standard errors also on the journal compound level. Robustness checks with bootstrapped standard errors confirm this choice of clustering. I also compute selected regressions using ordinary least squares (OLS) instead of Poisson. Again, my findings are qualitatively the same. To assess the relation between third-party funding and open access, I conduct binomial and multinomial logit regressions using access types as dependent variables.

Overall, the identifying assumption of my setting relies on the proposition that the establishment of the X option of publishing occurred rather randomly. As mentioned beforehand, the introduction of the 'Plan S' together with the suggested criteria for funding open access options, was unrelated to the journals and based on general developments in the publishing market. Thus, the *timing* of the introduction

even if the distributional assumptions are violated. The negative-binomial specification requires much stronger distributional assumptions and becomes inconsistent when they are not met.

of the X journals can, indeed, be considered idiosyncratic.

In contrast, the journals that received an X companion appear to have been chose consciously by Elsevier. Table 4.14 in the appendix shows the list of journals, their journal-specific H Index, and the quartile within the SCImago Journal ranking for each field.[14] Inspecting this list, Elsevier focused on journals leading in their fields. The set of journals seems widely homogeneous in their relative quality.

Lastly, X journals might have been chosen by different types of researchers compared for the incumbent parent journals, even though aims, scope, and peer review were the same. I cannot rule it out but it does not invalidate the findings as they address the competition implications, which apply also or especially if author characteristics varied.

4.4 Results

4.4.1 Baseline results

This section presents the results for the relationship between the number of citations and the type of access to a paper. Every table presents a battery of regression results in which I step-wise add regressors. In general, the abbreviation OA refers to both types of open access in contrast to restricted access (RA). XOA refers to open-access publications in X journals, while SOA refers to open access to papers published in subscription-based parent journals (both gold and green). In any case, I restrict my sample to the X journals and their parent companions, i.e., there are no journals in my sample that did not have an X twin at some point.

I begin with the central part of the analysis: The distinction between open access in subscription-based journals and open access in their 'X twins.' Table 4.3 presents the findings of my main specification. Once I control for age and the number of coauthors as well as journal compound fixed effects, I detect a citation *disadvantage*

[14]See Guerrero-Bote and Moya-Anegón (2012) for a technical description of this metric.

of -0.371 for papers published in X journals, which translates into a decrease by 31% or nearly one-third relative to papers published in the subscription-based main journals. Given an average RA citation count of 14.29, this 'X factor' implies a reduction of 4.43 citations for an X journal paper relative to a restricted access publication in a parent journal. This value corresponds to the coefficient of -3.818 estimated in the OLS specification in the sixth column of Table 4.3, which is slightly smaller but indistinguishable from the computation based on the Poisson regression as $F(1,34) = 0.30, p = 0.59$ for a Wald test of $\beta_{XOA} \stackrel{!}{=} -4.43$. I can rule out equality of the coefficients for the two types of open access as a Wald test of $\beta_{XOA} \stackrel{!}{=} \beta_{SOA}$ in the Poisson specification of column 5 leads to a test statistic of $\chi^2(1) = 52,81,\ p = 0.000$ and for the OLS specification to $F(1,34) = 10.69,\ p = 0.0025$.

	Poisson	Poisson	Poisson	Poisson	Poisson	OLS
XOA	-0.565***	-0.574***	-0.305***	-0.374***	-0.371***	-3.818**
	(0.107)	(0.135)	(0.052)	(0.039)	(0.038)	(1.117)
SOA	-0.144	-0.234***	-0.036	-0.021	-0.019	-0.517
	(0.119)	(0.058)	(0.053)	(0.055)	(0.050)	(0.620)
age			0.421***	1.288***	1.292***	8.518***
			(0.011)	(0.068)	(0.068)	(1.112)
age^2				-0.132***	-0.132***	-0.474***
				(0.009)	(0.009)	(0.102)
#authors					0.016***	0.202***
					(0.002)	(0.040)
constant	2.660***	2.755***	1.352***	0.145	0.044	-5.407*
	(0.132)	(0.004)	(0.040)	(0.121)	(0.127)	(2.511)
journal FE	NO	YES	YES	YES	YES	YES
N	123,939	123,939	123,939	123,939	123,922	123,922

$^{*}\,p < 0.05$, $^{**}\,p < 0.01$, $^{***}\,p < 0.001$. Dependent Variable: Number of citations in levels. Standard Errors in parentheses clustered on the journal compound level (i.e., X journals and main journals together). Alternative specification with bootstrapped standard errors in Table 4.15 in the appendix. Coefficients for X journal and subscription-based open access publications relative to the reference category of restricted access publications in the main journals.

Table 4.3: Regression results for the impact of the different access options

Regarding open access[15] to papers published in an incumbent parent journal (SOA), one can see that – with one exception in column 2 – open access publications do *not* differ in the number of citations in my sample. In contrast, not only are the

[15]This captures both gold and green open access routes, in contrast, X open access captures, by default, only gold open access.

standard errors too large for statistical significance, but the coefficients are also close to zero. These findings confirm earlier evidence but contradict the shared wisdom of an open-access citation advantage, at least for my sample. Even though the regression without any covariates but including the journal compound fixed effects leads to a significant negative effect also for this type of open access (column 2), any significance vanishes once adding a linear covariate for age. It is perfectly reasonable as my dataset started in 2018 when the first X journal 'pilots' were run. In the following years, especially in 2019-2021, X journals were a substitute for subscription-based open access such that a larger share of SOA publications is relatively young, given that in 2021 and 2022, Elsevier discontinued many X journals. In response, subscription-based open access became the only publishing option without access barriers. The median publication year reflects the higher share of younger publications of those with hybrid open access: While it is 2020 for X journals, it is 2021 for subscription-based open access. As the parent journals are all highly ranked and well-established in their disciplines, many institutions might have subscribed these outlets in any case. So, the access advantage for (SOA) papers in these journals diminishes.

The triangular setting of both restricted and open access within the subscription-based parent journals and open access in the X derivatives likewise allows digesting the 'X factor' of appearing in a novel journal from the open access effect. Holding quality constant not only econometrically but actually through the same editorial boards and processes, the citation disadvantage for X journal publications should be entirely related to the new name and independent citation and impact measures of the outlets. From a citation perspective, assigning the X forks new ISSN numbers was a mistake, leading to a different journal impact factor and journal ranking. Together with the new name, X journals may have appeared as novel market entrants that challenge the incumbent hybrid journals rather than complement them.

Citation Window Computations: Plenty of bibliometric research has evaluated and discussed the relevance of citation windows when working with citation data, e.g., Glänzel and Garfield (2004), Abramo et al. (2011), Campanario (2011), and Wang et al. (2015). Essentially, it is the question of when to evaluate a paper's impact properly, such that one does not truncate a notable amount of yet-to-be-made citations in the future. Reviewing the literature carefully, two or three years after publication, the accumulated number of citations should sufficiently represent a paper's relevance as measured in citations. Put differently, the average paper should have exceeded its citation peak within one of the two time windows.

	3 year citation window Publications in 2019			**2 year citation window** Publications in 2020		
	Poisson	Poisson	Poisson	Poisson	Poisson	Poisson
XOA	-0.418***	-0.309***	-0.309***	-0.353*	-0.303***	-0.301***
	(0.099)	(0.064)	(0.063)	(0.141)	(0.084)	(0.083)
SOA	0.052	-0.034	-0.032	-0.074	-0.150*	-0.149*
	(0.196)	(0.059)	(0.052)	(0.150)	(0.068)	(0.063)
#authors			0.020***			0.016***
			(0.004)			(0.003)
constant	3.001***	3.133***	3.022***	2.734***	2.814***	2.721***
	(0.149)	(0.002)	(0.024)	(0.142)	(0.003)	(0.019)
journal FE	NO	YES	YES	NO	YES	YES
N	24,337	24,337	24,332	24,491	24,491	24,488

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Dependent Variable: Number of citations in levels. Standard Errors in parentheses clustered on the journal compound level. Coefficients for XOA and SOA publications relative to the reference category of restricted access publications. Coefficients of age and age^2 omitted by construction.

Table 4.4: Impact of the different access options using quasi citation windows

As mentioned beforehand, to evaluate the number of citations within a citation window, one does need the timing of the citations. The Scopus data I use in this paper do not provide this information but solely gather the number of citations accumulated until the bibliometric data have been retrieved from the database. I did this in late March 2023, so I consider the end of 2022 for calculating citation windows as sufficiently precise, as even the first quarter of the year has yet to be over. Nevertheless, I still face the problem of having only accumulated data. I address it by constructing two 'quasi windows.' Using only the publications in 2019, I essentially have a three-year window for this subset of papers. Equivalently for 2020, I have a two-year 'quasi window.' As I use only one year of observations in both regressions, the covariates *age* and age^2 become obsolete. In addition, in both years, nearly all X journals were still active.

I provide the results for the regressions of these two quasi-citation windows in Table 4.4. For the three-year window on the left, one can see that the results are qualitatively the same as in the baseline regression. Also, the values of the coefficients are very similar to those shown in the main specification in Table 4.3. In contrast, those of the two-year window on the right, only relying on the observations from 2020, find not only a significantly negative effect for XOA publications but also a significant and negative effect for open-access publications in the established parent journals. The coefficients of the regressions, including journal fixed effects, are both significant on the 5% level but have half the size of those for the X journals. One has to treat this carefully, but it serves as evidence that open access might increase citations in the medium and long run or, reversely, a citation disadvantage may disappear over time, probably due to better availability than papers with restricted access. It does not apply to X journals, which may reemphasize that reputation disadvantages hardly disappear over time and that this is a fundamental disadvantage for papers published in such journals.

4.4.2 Extensions

Geographic differences of the authors: To better understand the underlying factors of the disparities in citations, I further extend my analysis by looking at differences between researchers being based at institutions at the global North and the global South. Here, I broadly follow the country classification of the United Nations Conference on Trade and Development (UNCTAD).[16] The 'global North'

[16]See `https://unctadstat.unctad.org/EN/Classifications.html`. It states that *"The developing economies broadly comprise Africa, Latin America and the Caribbean, Asia without*

indicator comes along with a broad understanding of leading higher education systems instead of taking a narrow view of looking only at the top 100 universities (or similar). Nevertheless, one should remember that the analysis looks at the *within* journal compound level, i.e., the same editorial boards handled the papers, and they appear within the same journal compound.

	Broad definition		*Narrow definition*		
	Global North	Other	Global North	Other	Total
RA	12,759	85,510	3,907	94,362	98,269
XOA	582	1,676	168	2,090	2,258
SOA	3,403	8,129	1,034	10,498	11,532
Total	16,744	95,315	5,109	106,950	112,059

The 'global North' category includes all publications up to nine authors that have a majority of coauthors affiliated with an institution based in a country of the global North (broad definition) or else all publications up to nine authors where all coauthors are affiliated with an institution based in a country of the global North (narrow definition). 'Other' must not be set equal to 'global South' as it also comprises papers with equal shares of North/South authors (broad definition). Likewise for the narrow definition, 'other' also encompasses papers with a majority of authors from the global North but without unanimity.

Table 4.5: Publications from countries in the 'global North' by access type

To classify a publication as stemming from the 'global North,' I rely on two methods. First, I set $\mathbb{1}_{global\ North} = 1$ if a *majority* of authors have an affiliation with a Western institution (broad definition). Alternatively, I code a paper as 'global North' only if *all* authors come from an institution in a developed global North country. For obvious reasons, this considerably decreases the sample, as Table 4.5 shows. For higher tractability, I restrict my sample to publications with up to nine authors. This covers 90.42% of all publications.

The robustness check with bootstrapped standard errors on the LHS of Table 4.22 in the appendix shows that the base SOA coefficient also becomes fully significant under this specification. However, the point estimate still is much smaller in its

Israel, Japan, and the Republic of Korea, and Oceania without Australia and New Zealand. The developed economies broadly comprise Northern America and Europe, Israel, Japan, the Republic of Korea, Australia, and New Zealand." In addition, I compute various robustness checks using the described alternative distinctions based on income and how established the higher education systems are in appendix B.

	Poisson	OLS	log OLS	Poisson	OLS	log OLS
	Global North: broad definition			Global North: narrow definition		
XOA	-0.352***	-4.035**	-0.325***	-0.354***	-4.021**	-0.323***
	(0.048)	(1.264)	(0.045)	(0.048)	(1.277)	(0.045)
SOA	-0.077	-1.345*	-0.061	-0.078	-1.318*	-0.056
	(0.048)	(0.611)	(0.040)	(0.046)	(0.599)	(0.040)
North	-0.079*	-0.976	-0.095*	-0.154**	-1.811*	-0.142**
	(0.034)	(0.631)	(0.040)	(0.053)	(0.762)	(0.052)
XOA × North	0.045	1.392	0.110	0.316	4.512*	0.340**
	(0.101)	(1.306)	(0.063)	(0.172)	(2.010)	(0.111)
SOA × North	0.209***	2.692**	0.142**	0.225	3.057	0.203*
	(0.059)	(0.969)	(0.040)	(0.138)	(1.559)	(0.088)
age	1.274***	8.277***	1.063***	1.262***	8.395***	1.068***
	(0.070)	(1.112)	(0.063)	(0.069)	(1.152)	(0.063)
age^2	-0.129***	-0.433***	-0.103***	-0.128***	-0.449***	-0.104***
	(0.009)	(0.110)	(0.009)	(0.009)	(0.112)	(0.009)
#authors	0.024**	0.253*	0.032***	0.022**	0.227*	0.030***
	(0.009)	(0.104)	(0.007)	(0.008)	(0.096)	(0.007)
constant	0.032	-5.230*	0.029	0.062	-5.331*	0.036
	(0.146)	(2.520)	(0.099)	(0.140)	(2.611)	(0.101)
journal FE	YES	YES	YES	YES	YES	YES
N	112,059	112,059	100,302	100,424	100,424	89,944

$^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Dependent Variable: Number of citations in levels (columns 1, 2, 4, 5), or logs (columns 3, 6). Standard Errors in parentheses clustered on the journal compound level. Alternative specification with bootstrapped standard errors in Table 4.22 in the appendix.

Table 4.6: Impact of the affiliation countries of authors: Global North

absolute size than the XOA coefficient. Also, the interaction effect between an affiliation with the global North and publishing with SOA outnumbers the negative SOA base effect in absolute terms. Hence, while open-access publications suffer from a citation disadvantage in general, this does not hold anymore if a sufficient amount of authors from, on average, better endowed institutions in the global North are involved.

Switching from the broad to the narrow definition of a ‘global North’ paper, one can see notable differences between the specifications. Now, all authors are affiliated with an institution from a country being part of the global North as defined by UNCTAD. First, one can see on the RHS of Table 4.6 that the plain covariate for a paper coauthored by a team entirely from the global North is significantly negative in the Poisson specification. Thus, papers from such universities are cited less often than their ‘global South’ or ‘mixed-North-South’ counterparts.

Given the general strength of higher education systems in the more developed global North of the world, how is this reasonable? While I cannot provide abundant evidence, one reasonable hypothesis is that the geographical and academic proximity of authors and editors since editors appear to have some home bias (Bethmann et al., 2023; Rubin et al., 2023). In addition, Colussi (2018) shows that any professional relationship between an author and an editor starkly increases the likelihood of being published in a particular journal. Moreover, Rose and Shekhar (2023) find that strong networks of academic advisers accelerate the careers of their graduate students. Aside from academia, jury voting in entertainment contests seems biased by proximity to the candidates (Budzinski et al., 2023).

All things equal, it implies that the papers of such known contributors may be of lower quality. While personal relations might be the most robust shifter for the probability of acceptance, other dimensions of proximity, such as the editor being familiar with the institution of the submitter, might be another driver of acceptance. In total, these issues imply an acceptance advantage for submissions from institutions in the global North, which, in turn, could explain the citation disadvantage, which might reveal their lower quality (on average).

Second, I detect a significant and positive interaction effect for publications with open access in the novel X journals stemming from authors from the global North. The effect is significant on the 5% and 1% level in the OLS and the log OLS specification, and has a p-value of $p = 0.066$ in the Poisson specification.[17] The interaction effect outnumbers the negative baseline effect for publications from these countries regardless of the access type. Further, it comes close to the citation penalty for X journal open-access papers.

The disadvantage of publishing in such an outlet vanishes for authors from the global North. It is odd under the hypothesis that publications from the global

[17]The bootstrap on the RHS of Table 4.22 in the appendix shows that the Poisson interaction coefficient becomes significant on the 5% level for this standard error computation.

North might be slightly inferior. In light of the likely lack of reputation of the novel X journals, the absence of access barriers combined with the overall reputation of established research institutions in rich, economically developed countries may successfully tackle this market entry barrier as the work of academics from globally well-established universities may help establish new journals as additional market participants. Consider the extreme example of a Nobel prize winner publishing in such an X journal. It is more than likely that the name on top of the paper would 'overrule' the disadvantages stemming from the outlet the paper is published in.

For robustness, I apply two alternative ways for the geographical distinction. The first one is based on income as defined by the World Bank. Here, I separate those with 'high income' from all other countries. The rationale behind it is that countries with high income are able to spend more on their higher education systems, which should lead to higher quality of the institutions and, on average, to better and more trustworthy research output. The second approach is based on the fact that the higher education system as we know it emerged from medieval Western Europe and was also established in the colonies that were created by the European empires. Due to the fact that research institutions in Western Europe, Australia, Canada, and the US are so old and established, this is another potential driver of trust in these institutions. The computations for both approaches are presented in appendix B and qualitatively tie in with the previous findings.

The impact of funding: As mentioned in the theoretical background of the formation of the X journals, funding organizations are also interested in research supported by their grants being published with open access. Furthermore, third-party funding might be an additional source of money to pay for open-access fees. At that point, it is important to clarify what funding means here. Using metadata on publications from Scopus, I am able to access 'funding' variables that contain what researchers report on funding that they obtained in addition to being employed at their institution. The quality of these variables depends, for apparent reasons, on whether researchers correctly and fully report their funding sources. As funders often make it mandatory that work supported by their grants is earmarked with a referral to the funder and mentioning a grant number, I consider this source widely reliable and potential errors idiosyncratic.

Funding Type	$\mathbb{1}_{funding} = 1$	$\mathbb{1}_{funding} = 0$	Total
General Funding	74,490	48,999	123,939
EU Funding	1,951	121,988	123,939
US Funding	5,133	118,806	123,939

The category 'General Funding' includes all papers that report some kind of funding by reporting a grant or project number. The category 'EU funding' includes all publications that report the European Commission, the European Research Council, or the Horizon 2020 scheme of the European Commission as funding entity. All three are part of the European Union. The category 'US Funding' includes all publications that name the National Science Foundation or the National Institutes for Health as funding entities.

Table 4.7: Number of funded papers by funding type

Table 4.7 presents the number of papers that report having received any kind of funding in the first row. Here, a peculiarity of my data comes into play. One can already see that a majority of papers report external funding. I measure that by counting those papers that report a funding number or ID. In contrast, even more papers (86,240) report a funding entity. Investigating the data in more detail, the disparity between the latter value and the one reported in Table 4.7 stems mostly from researchers reporting universities as funders without mentioning a specific funding number. It is unclear whether researchers just report their university as it employs them. As said beforehand, this variable relies entirely on the self-reporting of the submitting authors. To rule out mere acknowledgments of employers, I code $\mathbb{1}_{funding} = 1$ only if a funding ID is clearly mentioned.

Besides this rather broad definition of funding, which leads to the high count of funded publications as shown in the top row of Table 4.7, more restrictive definitions are useful. In particular, I redefine the indicator for funding in a way that it turns one only for funding from the European Commission (EC), the European Research

Council (ERC), or the Horizon 2020 (H2020) scheme, a major research funding program with a size of some 80 billion EUR that has been issued by the European Commission.[18] Constructing the funding indicator alternatively for the US, it only turns one if research is funded by the National Science Foundation (NSF) or the National Institutes for Health (NIH). These institutions are leading funding bodies, and receiving a grant from such funders is usually considered a precious signal for a researcher about their underlying and unobserved ability. Hence, it is reasonable to argue that such grants correlate with researchers who can produce high-quality research. As shown in Table 4.7, the two narrower variables do not code all observations without funding as zero but also those with funding from other sources than those mentioned. To ensure that this mechanical effect does not drive my results, I conduct robustness checks for all regressions that use these funding variables such that I exclude all observations that have received funding from any other source. Even though the sample size diminishes substantially, the results are qualitatively quite similar.[19]

	Logit	Logit
$\mathbb{1}_{funding}$	-0.2514	-0.1526
	(0.2223)	(0.1054)
constant	-1.7547***	-2.3418***
	(0.2722)	(0.0858)
journal FE	NO	YES
N	123,939	123,939

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Dependent Variable: Binary indicator that turns one if a paper has been published with open access. Standard Errors in parentheses clustered on the journal compound level.

Table 4.8: The effect of funding on the probability of open-access

[18]See `https://research-and-innovation.ec.europa.eu/funding/funding-opportunities/funding-programmes-and-open-calls/horizon-2020_en`, last checked July 10, 2023.

[19]The construction of the two smaller funding indicator variables, namely for EU funding and US funding, slightly conflicts with the modeling of the general funding variable. For the former, I identify funding entities, which, by construction, has to be done using the funder's name instead of the funding ID variable. This leads to 166 cases (8.5%) where the EU funding indicator turns one, but a funding ID is missing (which I use to set the general funding indicator $\mathbb{1}_{funding} = 1$) and 224 cases (4.4%) for the corresponding US case. As one does not simply mention such prestigious institutions without having a relation to them, I choose to keep these values in my regressions.

In the first step, I present a parsimonious logit regression that tests whether a paper that received any type of funding is subsequently more often published with open access, i.e., I regress an indicator for open access on an indicator of whether a paper has received funding as reported by the authors. Table 4.8 presents the respective results. The left column disregards journal fixed-effects and the right one includes them. One can see that the fixed effect does not make a meaningful difference. In both cases, funding is *not* related to a higher tendency of the researchers to publish their supported work with open access.

	Multinomial Logit *Reference: RA*
	XOA
$\mathbb{1}_{funding}$	-0.3892***
	(0.1139)
constant	-4.7021***
	(0.0881)
journal FE	YES
	SOA
$\mathbb{1}_{funding}$	-0.1005
	(0.1246)
constant	-2.4658***
	(0.1022)
journal FE	YES
N	123,939

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Dependent Variable: Binary indicator that turns one if a paper has been published with open access. Standard Errors in parentheses clustered on the journal compound level.

Table 4.9: The effect of funding on the probability of open-access by type

Separating the open-access variable by type, i.e., into SOA and XOA, and using a multinomial logit model, Table 4.9 shows that the likelihood of a paper being published with XOA relative to restricted access when having received funding is significantly lower. At the same time, the effect on SOA is indistinguishable from zero. Hence, for the broad definition of funding, there even exists a disadvantage for the novel X journals. In the second step, I apply the two more selective indicators, that only include the very prestigious funding bodies from the European Union and the United States. Table 4.10 presents the findings for the logit regressions with the two more restrictive funding indicators. The left side shows the results for the funding variable that captures only the EU schemes, and the right side presents those of the US schemes. Other than the null effect for funded research in general, the effects for the selected leading research funding entities from the EU indicate that these grants lead to a much higher uptake of open access.

	EU: ERC, EC & H2020		US: NSF & NIH	
	Logit	Logit	Logit	Logit
$\mathbb{1}_{funding}$	1.2410***	1.3119***	0.9644***	0.4753
	(0.1780)	(0.1104)	(0.2446)	(0.3142)
constant	-1.9303***	-2.5115***	-1.9559***	-2.4889***
	(0.1890)	(0.0055)	(0.1883)	(0.0170)
journal FE	NO	YES	NO	YES
N	123,939	123,939	123,939	123,939

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Dependent Variable: Binary indicator that turns one if a paper has been published with open access. Standard Errors in parentheses clustered on the journal compound level. Table 4.23 in the appendix presents results excluding observations with funding from other sources.

Table 4.10: The effect of funding on the probability of open-access: Effects for selected funding bodies from the EU and the US

It is not a coincidence but part of the grant requirements. For example, the 'Annotated Model Grant Agreement' of the Horizon 2020 program states in Article 29.2 (p. 245) that *"Each beneficiary must ensure open access (free of charge, online access for any user) to all peer-reviewed scientific publications relating to its results. In particular, it must: (a) as soon as possible and at the latest on publication, deposit a machine-readable electronic copy of the published version or final peer-reviewed manuscript accepted for publication in a repository for scientific publications;"*[20] The criteria of the US National Science Foundation sound highly similar: *"NSF requires that either the version of record or the final accepted manuscript in peer-reviewed scholarly journals . . . be deposited in a public access compliant repository designated by NSF."*[21] It encompasses both green and hybrid-gold open access. However, I

[20]See `https://ec.europa.eu/research/participants/data/ref/h2020/grants_manual/amga/h2020-amga_en.pdf`, Version 5.2 of the agreement, date: June 26, 2019. Last checked July 10, 2023.

[21]See NSF document NSF18-041, Frequently Asked Questions (FAQs) for Public Access, `https:`

cannot detect a positive uptake of open access among research papers funded by the two US institutions. The funding coefficient becomes insignificant once I control for journal fixed effects.

Both frameworks do not rule out an exact copy of a paper to be published as a working paper without the layout of the journal publication. Nevertheless, it might be more convenient for many researchers to publish it under an open-access license in the first place. For risk-averse researchers, publishing the final publication with open access may also be preferable to using parent journals without open access and uploading a copy somewhere, as there may remain some uncertainty about whether this is sufficient to comply with the rules.

	Multinomial Logit	
	EU	US
	Reference: RA	
	XOA	
$\mathbb{1}_{funding}$	1.2255***	0.3883
	(0.2017)	(0.2178)
constant	-5.0542***	-5.0319***
	(0.0105)	(0.0109)
journal FE	YES	
	SOA	
$\mathbb{1}_{funding}$	1.3312***	0.4895
	(0.1130)	(0.3424)
constant	-2.5940***	-2.5712***
	(0.0057)	(0.0187)
journal FE	YES	YES
N	123,939	123,939

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Dependent Variable: Categorical variable that turns one if a paper has been published with X open access, and two with subscription based open access. Reference category: Restricted access. Standard Errors in parentheses clustered on the journal compound level. Table 4.24 in the appendix provides a robustness check that mutually excludes all other funding scheme from the regressions.

Table 4.11: The effect of funding on the probability of open-access by type – EU & US

The differences between the two systems also become visible when looking at the multinomial logit estimators for the impact of funding on the choice of the two open access types relative to publishing behind a subscription paywall. Table 4.11 shows

`//www.nsf.gov/pubs/2018/nsf18041/nsf18041.pdf`, last checked July 10, 2023.

no effect of funding on any open access type for US funding. Quite the opposite, papers funded by the European Union schemes appear more often in both X journals and as open access in hybrid journals.

Whatever the actual reason for the differences between the EU and the US funding is, the strong focus of the EU on open access seems to pay off. It leads me to the question of whether open access also pays off in terms of citations of the funded papers. Hottenrott and Lawson (2017) and Yan et al. (2018) find that funded research generally receives more citations than publications without specific third-party funding.[22] I cannot confirm their findings with my sample, as Table 4.12 demonstrates. Just as funding (in its general specification) is unrelated to open access, it does not correlate with a higher count of citations.

	Poisson	OLS	log OLS
XOA	-0.359***	-3.905**	-0.300***
	(0.048)	(1.197)	(0.054)
SOA	-0.051	-0.641	-0.013
	(0.111)	(1.145)	(0.078)
Funding	-0.004	-0.341	0.032
	(0.036)	(0.488)	(0.030)
XOA×Funding	-0.023	0.112	-0.018
	(0.068)	(1.051)	(0.061)
SOA×Funding	0.050	0.200	-0.016
	(0.111)	(1.237)	(0.076)
⋮	⋮	⋮	⋮
Constant	0.047	-5.151	0.066
	(0.115)	(2.578)	(0.105)
journal FE	YES	YES	YES
N	123,922	123,922	110,984

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Dependent Variable: Number of citations in levels (columns 1, 2), or logs (column 3). Standard Errors in parentheses clustered on the journal compound level.

Table 4.12: Regression results for citations addressing funding

However, this only holds for the whole set of funding sources. The situation changes again when I replace the general funding indicator with the two indicators that capture either the leading EU or US funders. Table 4.13 displays the results

[22]Bryan and Ozcan (2021) identify an increase in citations from patents for open access papers funded by the NIH.

for that. As always in this study, the X journals have a significant citation disadvantage. And as before, publications with open access in parent journals do not have any citation advantage compared to restricted access. When including the funding indicator variable that only captures grants from the EC, ERC, and H2020, a notable interaction exists between the three European schemes and citations of SOA publications. In particular, papers supported by EU funding and published with open access in an incumbent journal receive substantially more citations, as the coefficient of +0.276 in column 1 (and the respective coefficients for the linear and log OLS specifications) suggest. Another noteworthy observation is the fact that the baseline Poisson coefficient for EU funding is significantly negative, i.e., research funded by the three mentioned EU schemes, on average, receives fewer citations than research either unfunded or funded by other bodies. It corresponds to the negative coefficient of authors entirely affiliated with institutions based in the global North. Again, it is probably easier for funded research to become accepted at a journal due to the reputation of the grants and the likely proximity between authors and editors.

Crossing the Atlantic, I cannot identify an interaction effect for funding from the US entities NSF and NIH as the right part of Table 4.13 demonstrates. I also cannot replicate the negative baseline coefficient for these publications. It corresponds to the fact that I also do not find a higher tendency of research funded by the NSF or NIH to be published under open-access conditions as final publications. I draw two things from that finding. Other than earlier research, it does not seem the case that funding fosters citations, but funding that fosters open access may turn the open access obligation into an open access citation advantage. Why is that, given that could hardly detect such an advantage beforehand? The open access requirements of the European funding schemes seem to shift publications to open access that would have likely appeared under restricted access otherwise. These publications may attract interest either because of their quality, their research question, or a

	European Union			United States		
	Poisson	OLS	log OLS	Poisson	OLS	log OLS
XOA	-0.376***	-3.844**	-0.314***	-0.372***	-3.868**	-0.314***
	(0.039)	(1.117)	(0.050)	(0.037)	(1.125)	(0.047)
SOA	-0.032	-0.640	-0.032	-0.007	-0.389	-0.024
	(0.052)	(0.652)	(0.041)	(0.053)	(0.654)	(0.041)
Funding (EU/US)	-0.078**	-0.884	-0.036	-0.022	-0.449	0.004
	(0.029)	(0.589)	(0.031)	(0.041)	(0.584)	(0.040)
XOA×Funding	0.148	1.184	0.064	0.017	0.914	0.031
	(0.131)	(1.494)	(0.103)	(0.113)	(1.869)	(0.104)
SOA×Funding	0.276***	3.394*	0.200**	-0.104	-1.290	0.006
	(0.082)	(1.449)	(0.063)	(0.068)	(1.003)	(0.048)
⋮	⋮	⋮	⋮	⋮	⋮	⋮
journal FE	YES	YES	YES	YES	YES	YES
N	123,922	123,922	110,984	123,922	123,922	110,984

$^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Dependent Variable: Number of citations in levels. Standard Errors in parentheses clustered on the journal compound level. Alternative specifications are provided in the appendix: Table 4.25 presents bootstrapped standard errors instead of the clustered standard errors displayed here. Table 4.26 in the appendix presents the results for the regressions that mutually exclude all other funding schemes.

Table 4.13: Regression results for citations addressing funding from the EU and the US

combination of both. Open access now allows a broader audience to read and cite this work.

The US case is an attractive counterfactual in this domain as there exists no shift towards open access, i.e., there is no inflow of papers with a high probability of having high quality or relevance and, consequentially, no reaction in the citations. Hence, it seems to be this shift towards open access of strong publications in the first place that overcomes the challenge that researchers publishing critical work either do not care for open access or even consciously reject it, potentially due to the publication fees or doubts regarding the trustworthiness of open access even in hybrid journals.

It is important to acknowledge that only the interaction effect with incumbent subscription-based open access is positive for the EU funding, even though researchers are more likely to publish their work in X journals as well. Hence, the open access obligation removes access barriers, leading to higher citation counts.

But it cannot overcome the reputation disadvantage of recently established journals.

Robustness Checks: A large set of robustness checks in the appendix backs my baseline findings and extensions. Table 4.15 uses the same setting as in my main specification shown in Table 4.3 but computes the standard errors based on bootstrapping with 250 replications instead of clustering them. Tables 4.16 and 4.17 compute the same regressions restricting the dataset to its 99$^{\text{th}}$ or else 95$^{\text{th}}$ percentile (in terms of citations) to ensure that outliers do not drive the results. Table 4.18 estimates negative-binomial regressions as an alternative to the used Poisson specification here. In all cases, the results vary only slightly and remain qualitatively the same.

In Table 4.20 in the appendix, I show that the results also remain the same when excluding the two journal compounds without having editorial boards that are the same for parent and mirror journal, namely the *International Journal of Pharmaceutics*, *Vaccine*, and their respective X equivalents. Table 4.21 presents the baseline results without considering uncited papers as suggested by Waltman (2012). The results are qualitatively the same, and the point estimates are highly similar.

Last, the majority of my observations fall into the global COVID-19 pandemic. It affected researchers in many dimensions. First, it triggered a massive response in scholarly output on the disease and its implications (Haghani & Bliemer, 2020). Further, it may have adversely affected researchers in their productivity by the non-pharmaceutical interventions such as kindergarten, school, and university closures (Kwon et al., 2023) as well as by direct effects of a cured COVID-19 infection on one's own bodily constitution (Fischer et al., 2022). While I cannot directly address the indirect effects on productivity, I can avoid that my results are driven by the highly upward-pushed interest in medical publications. To do so, I rerun the main regression excluding nine medical and pharmaceutical journals. Table 4.19 lists the excluded outlets and shows that my findings qualitatively remain unaffected and are quantitatively highly similar to the coefficients of my principal regression.

4.5 Economic Implications

In the present sample of journals and publications, I cannot detect a citation advantage for open access to publications in incumbent journals but a significant disadvantage for novel and relatively unknown journals, even if the quality is arguably the same. This core finding of my analysis is not only of academic interest but bears critical economic implications for the scientific publishing market. It is considerably large, with an annual size of some 19bn GBP or 21.5bn EUR (Buranyi, 2017).[23] Further, its function as a distributor and platform for communicating novel insights gives it extraordinary importance.

Academic journals can be considered two-sided markets (McCabe & Snyder, 2007; Jeon & Rochet, 2010). They embody strong market power as prestigious outlets attract more submissions, which should lead to a higher quality of published submissions. This again attracts more attention and recognition from readers. Due to the tremendous amount of publications nowadays, many researchers only have a brief look at the journal a paper is published in to evaluate its (assumed) quality and the (assumed) ability of the authors. This mechanism makes it hard to set up new journals as they need to gain recognition from authors and readers (who are, in many cases, the same people). Furthermore, according to Schmal et al. (2023) male researchers tend to seek reputation through their publications, so they might be less likely to shift to such newly established outlets. Even though the extension on funding has shown that prestigious grants increase the likelihood of a paper being published in an X journal, it does not seem to pay off in citations. While non-mainstream journals suffer from this problem as well (Chavarro et al., 2017), they

[23]The GBP-EUR conversion was done using the exchange rate of June 27, 2017, when the article was published, see `https://www.exchangerates.org.uk/EUR-GBP-spot-exchange-rates-history-2017.html`, last checked July 10, 2023.

do not compete with the leading outlets by definition as they specifically cover their own niches.

Reversely, new market entrants that might challenge the leading incumbent publishing houses might suffer from the 'X factor' that their newly established outlets must have a different title, necessarily a different ISSN identifier, and, therefore, are not established by definition and cannot have a presentable journal impact factor or other merits. This first-mover advantage for the established parent journals or else second-mover disadvantage for the X newcomers is often present among platforms.

While my findings empirically confirm the two-sided market hypothesis, they also reemphasize the competition issue by that. Together with the contract-based finding that the big transformative agreements may strengthen the large publishers regardless of the attractiveness of their journals (Schmal, 2023a), my results further challenge the hope of many researchers for a shift towards more open science and less market power of the leading commercial publishers. There exist examples such as the *Journal of the European Economic Association* that had been launched after a dispute between the society and the publisher Elsevier (Bolton et al., 2003). Furthermore, there exist two additional branch journals of the *Journal of Political Economy* (JPE), namely the *JPE: Microeconomics* and the *JPE: Macroeconomics.* The American Economic Association launched several *American Economic Journals*, top-notch derivatives of the leading *American Economic Review* that cover different economic subfields.

However, the number of publications, e.g., for the *American Economic Journal: Economic Policy* has been some 50 papers in the past three years (Luttmer, 2022). This is just a drop in the ocean of annual publications in economics, management, and adjacent fields (Schmal et al., 2023). At the same time, the number of academic publications grows in the long run with an annual rate of 4.1% (Bornmann et al., 2021). Schmal et al. (2023), detect in their study covering the years since 2015 an even higher annual growth rate of 5-7%. Thus, initiatives such as the *American*

Economic Journals do not even cover the annual growth in publications, let alone a substantial change. Therefore, a significant shift from journals hosted by commercial publishers to those of non-profit societies and university presses has not happened, and there is no sign of such a change shortly. The disadvantages for researchers to publish in newly established, unknown, unranked, and rather obscure outlets are non-negligible. Semi-successful attempts, such as the *Berkeley Electronic (B.E.)* journals, fuel further doubts as to whether new journals will establish themselves.[24]

On the other hand, the significantly positive interaction term between X journal open access and all coauthors being affiliated with institutions from the global North suggests that the citation penalty for new journals in the market can be overcome if enough well-established scholars give these new participants an initial stimulus as citations may boost the perceived quality of a journal, which will further strengthen its position in the market. The same holds for prestigious grants that push publications on supported research projects in specific journals.

Regarding the estimated extensions, my results suggest that open access is rewarded more if a paper stems from authors with university affiliations in economically developed countries. This is a disadvantage for researchers from developing countries, who already face the challenge of lower funding and less-developed academic networks.[25] While open access, by definition, helps financially disadvantaged academics when accessing research, it may become a hurdle when publishing their own research as open access.

A last economic implication leads back to the early raised concern by McCabe and Snyder (2005) that open access journals are incentivized to accept more papers due to the business model based on publications instead of subscriptions. If researchers realize that open access does not pay off in terms of citations for their

[24]For example, the *B.E. Journal of Macroeconomics* reached its peak SJR of 1.447 in 2008. Since then, it collapsed to 0.217, 15% of the all-time high. See `https://www.scimagojr.com/journalsearch.php?q=8300153213&tip=sid`, last checked July 10, 2023.

[25]The geographic diversity of authors particularly in Elsevier's X journals has been examined by Smith et al. (2021).

publications in strong journals, they might focus on publishing weaker work with open access. It would make open access a substitute for quality instead of a complement, as in the case of top publications. It would not only reinforce the subjacent quality concerns regarding open access but also lock out the general public and developing countries from research not idiosyncratically but rather from leading work. This argument is in favor of transformative agreements as they make any publication in any journal of an eligible publisher open access by default (see, e.g., Haucap et al., 2021; Schmal, 2024) such that the issue above may not play a role anymore.

What speaks against these contracts is that they make it more attractive for eligible researchers to publish in the included journals. These are usually established outlets so that they may keep researchers away from new market participants. It is particularly problematic as 89.8% of all transformative agreements are closed between publishers and countries from the global West. Even more serious is that contracts with countries not from the global West cover only 2.9% of all publications estimated to be published under such agreements.[26] As I could show beforehand, authors from the global North as well as from long-standing higher education systems in the global West do not suffer from a citation disadvantage in newly set up journals. Funding bodies can also significantly push papers towards fully open-access outlets. Hence, they could be a core driver in establishing a higher level of competition in the publishing market by strategically requiring submissions to these journals and not hybrid or green open-access to publications in established journals. However, the transformative agreements will encourage them to stick with those often well-established publishers.

[26]The numbers are computed based on the transformative agreement registry of the ESAC initiative, see `https://esac-initiative.org/about/transformative-agreements/agreement-registry/`. Last database update: May 12, 2023. Every contract is counted separately, for example, the German 'DEAL' agreements are listed twice, once for Wiley and once for Springer Nature.

4.6 Conclusion

My analysis of the unique setting of two open access options alongside the restricted-access publishing option does not only reject the existence of such an advantage when studying within journal variation across 70 journals and 35 compounds but also detects what I subsume the 'X factor,' a significant decrease in citations for open access publications in newly launched journals even though they rely on the editorial boards of their parent journals and should be, thus, qualitatively indistinguishable from their counterparts.

While this disadvantage for non-established open access is prominent, it diminishes among publications from authors affiliated with institutions from the global North relative to the papers from other countries. Hence, researchers reading these publications might perceive open access to papers from often well-established universities as a complement to research quality but rather a substitute for quality when it comes to open access to papers from non-Northern institutions, even though that happens *within* the same journals with the same editorial boards and peer review processes.

A way to foster competition is likely to be via clearly specified open access requirements of grants. As the extension on funding has shown, publications supported by EU funding are more often published under an open access license. Funding bodies should, therefore, consider whether they tighten the requirement of publishing results not only with open access but within a fully open access journal, i.e., ruling out hybrid outlets. This corresponds to the suggestion of Schmal (2023a) to introduce shades in the color scheme of structuring types of open access in a way that full open access to a paper has one shade if it is published in a hybrid journal and another one if it is published in a fully open access journal.

Future research should take into account a longer time span and, if possible, a broader set of journals that covers not only different publishers but also different quality ranges. As the studied setting is highly unique, it remains an open question whether this trifold scenario can be investigated with different journals but revisiting it in a few years might contribute a further understanding of the long-run effects of open access in incumbent and new journals relative to subscription-based outlets.

My results have non-negligible implications for the ongoing changes in the academic publishing market, especially concerning the 'transformative agreements' between often large publishers and many university consortia. While the primary demand by the universities – all papers being published as open access immediately – is satisfied by construction, it makes publishing in the journals of these publishers c.p. more attractive even though newly established competing journals already suffer from a citation disadvantage as my results for Elsevier's X journals have shown.

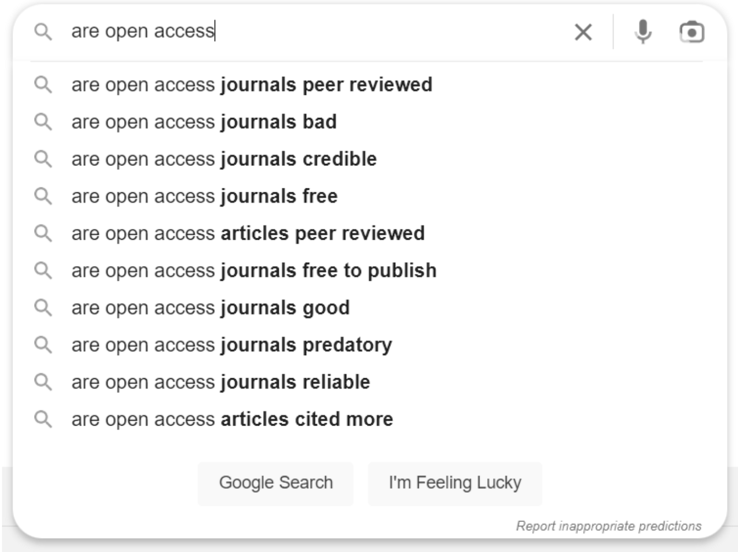
The comparative advantage of open access to papers in incumbent journals compared to newly established gold open access journals may strengthen the position of the already large publishing houses. In the medium and long run, this mechanism could harm market entrants and impede smaller players. It is likely to foster further concentration and less competition in the market for academic publishing.

4.7 Appendix A

Journal Compound	H Index	Journal Rank
Analytica Chimica Acta	224	q1
Atmospheric Environment	270	q1-q2
Biosensors and Bioelectronics	222	q1
Chaos, Solitons and Fractals	160	q1
Chemical Engineering Science	280	q1
Chemical Physics Letters	248	q2
Computers and Graphics	79	q2
Contraception	110	q1
Cytokine	130	q1-q2
Ecological Engineering	150	q1
Energy Conversion and Mgmt.	232	q1
European Journal of Obstetrics...	111	q2
Food Chemistry	302	q1
Gene	188	q1-q2
Intl. Journal of Pharmaceutics	244	q1
Journal of Asian Earth Sciences	146	q1
Journal of Biomedical Informatics	121	q1
Journal of Computational Physics	275	q1
Journal of Dentistry	130	q1
Journal of Hydrology	260	q1
Journal of Non-Crystalline Solids	188	q1-q2
Journal of Pediatrics	227	q1
Journal of Structural Biology	156	q1
Materials Letters	164	q1-q2
Nutrition	156	q1-q2
Optical Materials	113	q2
Research Policy	271	q1
Resources, Cons. and Recycling	170	q1
Respiratory Medicine	134	q1
Sleep Medicine	141	q1
Toxicon	140	q3
Vaccine	205	q1
Veterinary Parasitology	138	q2
Water Research	354	q1
World Neurosurgery	106	q2

H Index computed on the journal level. Journal rank: quartile within the SCImago Journal Ranking from 2018 to 2022 in the main research category as reported by SCImago. q1 means that a journal is in the top quartile of a certain discipline during 2018-2022. The H index is computed on the journal level (see Braun et al., 2006, for the conceptual idea) and from 2023. Due to the concave functional form of this measure, it should be highly similar to its past values.

Table 4.14: Journal reputation



Autocomplete Suggestion generated by entering the query "are open access" into the search box of `https://www.google.com/` without being logged into a Google account using the browser Google Chrome Version 113.0.5672.93 (64-Bit). Day of the search: May 11, 2023. Results might vary slightly with different specifications.

Figure 4.3: Google autocomplete for the search query "are open access"

	Poisson	Poisson	Poisson	Poisson	Poisson	OLS
XOA	-0.565***	-0.574***	-0.305***	-0.374***	-0.371***	-3.818***
	(0.033)	(0.035)	(0.030)	(0.033)	(0.033)	(0.290)
SOA	-0.144***	-0.234***	-0.036*	-0.021	-0.019	-0.517*
	(0.037)	(0.018)	(0.018)	(0.019)	(0.018)	(0.250)
age			0.421***	1.288***	1.292***	8.518***
			(0.003)	(0.020)	(0.022)	(0.233)
age^2				-0.132***	-0.132***	-0.474***
				(0.003)	(0.003)	(0.041)
#authors					0.016***	0.202***
					(0.001)	(0.024)
constant	2.660***	2.755***	1.352***	0.145***	0.044	-5.407***
	(0.005)	(0.023)	(0.021)	(0.037)	(0.042)	(0.495)
journal FE	NO	YES	YES	YES	YES	YES
N	123,939	123,939	123,939	123,939	123,922	123,922

$^{*}\, p < 0.05$, $^{**}\, p < 0.01$, $^{***}\, p < 0.001$. Dependent Variable: Number of citations in levels. Standard errors in parentheses bootstrapped with 250 replications. Coefficients for X journal and open access publications relative to the reference category of subscription based publications in the main journals.

Table 4.15: Regression results X journals and open access combined: Bootstrapped SEs

	Poisson	Poisson	Poisson	Poisson	Poisson	OLS
XOA	-0.498***	-0.500***	-0.261***	-0.329***	-0.327***	-3.082**
	(0.096)	(0.116)	(0.046)	(0.036)	(0.035)	(0.862)
SOA	-0.206*	-0.246***	-0.060	-0.044	-0.042	-0.664
	(0.100)	(0.057)	(0.052)	(0.054)	(0.048)	(0.538)
age			0.390***	1.234***	1.239***	8.119***
			(0.009)	(0.052)	(0.052)	(1.096)
age^2				-0.130***	-0.130***	-0.573***
				(0.007)	(0.007)	(0.111)
#authors					0.015***	0.180***
					(0.002)	(0.028)
constant	2.541***	2.678***	1.394***	0.236*	0.137	-4.191
	(0.120)	(0.004)	(0.034)	(0.095)	(0.098)	(2.107)
journal FE	NO	YES	YES	YES	YES	YES
N	122,721	122,721	122,721	122,721	122,704	122,704

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Dependent Variable: Number of citations in levels. Standard errors in parentheses clustered on the journal compound level. Alternative specification excluding the citation counts exceeding the 99th percentile. Coefficients for X journal and open access publications relative to the reference category of subscription based publications in the main journals.

Table 4.16: Regression results: 99% computation

	Poisson	Poisson	Poisson	Poisson	Poisson	OLS
XOA	-0.418***	-0.431***	-0.240***	-0.307***	-0.304***	-2.372***
	(0.082)	(0.090)	(0.036)	(0.030)	(0.028)	(0.522)
SOA	-0.185*	-0.222***	-0.060	-0.045	-0.043	-0.477
	(0.089)	(0.060)	(0.057)	(0.058)	(0.051)	(0.456)
age			0.354***	1.149***	1.154***	7.099***
			(0.013)	(0.050)	(0.050)	(0.895)
age^2				-0.124***	-0.124***	-0.603***
				(0.006)	(0.006)	(0.112)
#authors					0.015***	0.161***
					(0.002)	(0.022)
constant	2.328***	2.538***	1.402***	0.341***	0.240*	-2.535
	(0.099)	(0.004)	(0.046)	(0.100)	(0.102)	(1.444)
journal FE	NO	YES	YES	YES	YES	YES
N	117,745	117,745	117,745	117,745	117,729	117,729

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Dependent Variable: Number of citations in levels. Standard errors in parentheses clustered on the journal compound level. Alternative specification excluding the citation counts exceeding the 95th percentile. Coefficients for X journal and open access publications relative to the reference category of subscription based publications in the main journals.

Table 4.17: Regression results 95% computation

	Neg Bin	Neg Bin	Neg Bin	Neg Bin	Neg Bin
XOA	-0.565***	-0.500***	-0.302***	-0.374***	-0.366***
	(0.107)	(0.096)	(0.054)	(0.045)	(0.044)
SOA	-0.144	-0.236**	-0.073	-0.032	-0.036
	(0.119)	(0.078)	(0.069)	(0.071)	(0.066)
age			0.504***	1.427***	1.430***
			(0.014)	(0.062)	(0.061)
				-0.151***	-0.151***
				(0.009)	(0.009)
#authors					0.024***
					(0.006)
constant	2.660***	2.752***	1.055***	-0.082	-0.232*
	(0.132)	(0.005)	(0.041)	(0.095)	(0.109)
journal FE	NO	YES	YES	YES	YES
ln(α)	0.424***	0.231***	-0.127	-0.192**	-0.201**
	(0.066)	(0.053)	(0.070)	(0.070)	(0.068)
N	123,939	123,939	123,939	123,939	123,922

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Dependent Variable: Number of citations in levels. Standard errors in parentheses clustered on the journal compound level. Alternative specification using a negative-binomial setting instead of Poisson. Coefficients for X journal and open access publications relative to the reference category of subscription based publications in the main journals.

Table 4.18: Regression results: Negative-Binomial specification

	Poisson	Poisson	Poisson	Poisson	Poisson	OLS
XOA	-0.522***	-0.558***	-0.298***	-0.369***	-0.366***	-4.270**
	(0.125)	(0.164)	(0.061)	(0.044)	(0.042)	(1.311)
SOA	-0.010	-0.244***	-0.022	-0.008	-0.012	-0.621
	(0.118)	(0.048)	(0.036)	(0.038)	(0.038)	(0.531)
age			0.423***	1.294***	1.299***	9.094***
			(0.012)	(0.078)	(0.078)	(1.274)
age^2				-0.133***	-0.133***	-0.506***
				(0.010)	(0.010)	(0.116)
#authors					0.023***	0.311***
					(0.004)	(0.074)
constant	2.700***	2.755***	1.344***	0.135	-0.015	-7.374*
	(0.151)	(0.003)	(0.046)	(0.138)	(0.151)	(3.050)
journal FE	NO	YES	YES	YES	YES	
N	100,431	100,431	100,431	100,431	100,426	100,426

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Dependent Variable: Number of citations in levels. Standard errors in parentheses clustered on the journal compound level. Coefficients for X journal and open access publications relative to the reference category of subscription based publications in the main journals. Excluding medicince journals that may have been affected by COVID-19, namely Contraception, the European Journal of Obstetrics & Gynecology and Reproductive Biology, Cytokine, Gene, International Journal of Pharmaceutics, the Journal of Dentistry, the Journal of Pediatrics, Respiratory Medicine, Sleep Medicine, and Vaccine, and all of their X derivatives.

Table 4.19: Regression results excluding medicine journals

	Poisson	Poisson	Poisson	Poisson	Poisson	OLS
XOA	-0.538***	-0.560***	-0.308***	-0.383***	-0.379***	-4.268**
	(0.113)	(0.152)	(0.057)	(0.042)	(0.041)	(1.225)
SOA	-0.114	-0.248***	-0.053	-0.039	-0.034	-0.829
	(0.132)	(0.062)	(0.057)	(0.059)	(0.053)	(0.616)
age			0.421***	1.296***	1.301***	8.755***
			(0.011)	(0.073)	(0.073)	(1.192)
age^2				-0.133***	-0.133***	-0.501***
				(0.010)	(0.010)	(0.108)
#authors					0.016***	0.218***
					(0.003)	(0.046)
constant	2.665***	2.756***	1.351***	0.134	0.026	-5.880*
	(0.140)	(0.004)	(0.042)	(0.129)	(0.135)	(2.697)
journal FE	NO	YES	YES	YES	YES	
N	113,863	113,863	113,863	113,863	113,851	113,851

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Dependent Variable: Number of citations in levels. Standard errors in parentheses clustered on the journal compound level. Coefficients for X journal and open access publications relative to the reference category of subscription based publications in the main journals. Excluding International Journal of Pharmaceutics, Vaccine, and their X derivatives.

Table 4.20: Regression results excluding compounds not sharing the same editorial board

	Poisson	Poisson	Poisson	Poisson	Poisson	OLS
XOA	-0.482***	-0.493***	-0.274***	-0.336***	-0.334***	-4.156***
	(0.091)	(0.124)	(0.050)	(0.039)	(0.038)	(1.079)
SOA	-0.080	-0.189***	-0.013	0.002	0.000	-0.271
	(0.097)	(0.043)	(0.034)	(0.035)	(0.034)	(0.537)
age			0.380***	1.095***	1.099***	9.319***
			(0.011)	(0.054)	(0.055)	(1.174)
age^2				-0.108***	-0.108***	-0.578***
				(0.007)	(0.007)	(0.114)
#authors					0.012***	0.178***
					(0.003)	(0.047)
constant	2.762***	2.816***	1.532***	0.522***	0.443***	-6.199*
	(0.116)	(0.003)	(0.039)	(0.096)	(0.105)	(2.700)
journal FE	NO	YES	YES	YES	YES	YES
N	110,990	110,990	110,990	110,990	110,984	110,984

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Dependent Variable: Number of citations in levels excluding 12,949 observations without citations. Standard errors in parentheses clustered on the journal compound level. Coefficients for X journal and open access publications relative to the reference category of subscription based publications in the main journals.

Table 4.21: Regression results excluding uncited papers

	Poisson	OLS	log OLS	Poisson	OLS	log OLS
	broad definition			narrow definition		
XOA	0.352***	-4.035***	-0.325***	-0.354***	-4.021***	-0.323***
	(0.038)	(0.370)	(0.024)	(0.041)	(0.369)	(0.026)
SOA	-0.077***	-1.345***	-0.061***	-0.078***	-1.318***	-0.056***
	(0.017)	(0.217)	(0.012)	(0.018)	(0.237)	(0.012)
Global North	-0.079***	-0.976**	-0.095***	-0.154***	-1.811***	-0.142***
	(0.022)	(0.322)	(0.011)	(0.032)	(0.381)	(0.018)
XOA × Global North	0.045	1.392	0.110*	0.316*	4.512***	0.340***
	(0.074)	(0.767)	(0.045)	(0.140)	(1.261)	(0.093)
SOA × Global North	0.209***	2.692***	0.142***	0.225**	3.057***	0.203***
	(0.046)	(0.624)	(0.021)	(0.079)	(0.873)	(0.040)
age	1.274***	8.277***	1.063***	1.262***	8.395***	1.068***
	(0.018)	(0.230)	(0.010)	(0.016)	(0.215)	(0.010)
age^2	-0.129***	-0.433***	-0.103***	-0.128***	-0.449***	-0.104***
	(0.003)	(0.041)	(0.002)	(0.003)	(0.040)	(0.002)
#authors	0.024***	0.253***	0.032***	0.022***	0.227***	0.030***
	(0.003)	(0.034)	(0.002)	(0.002)	(0.031)	(0.002)
constant	0.032	-5.230***	0.029	0.062	-5.331***	0.036
	(0.036)	(0.486)	(0.021)	(0.035)	(0.463)	(0.022)
journal FE	YES	YES	YES	YES	YES	YES
N	112,059	112,059	100,302	100,424	100,424	89,944

$^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Dependent Variable: Number of citations in levels (columns 1, 2, 4, 5), or logs (columns 3, 6). Standard errors in parentheses bootstrapped with 250 replications.

Table 4.22: Impact of the affiliation countries of authors: Global North – bootstrapped SEs

	EU: ERC, EC & H2020		US: NSF & NIH	
	Logit	Logit	Logit	Logit
$\mathbb{1}_{funding}$	1.0721***	1.1905***	0.7698*	0.3541
	(0.2683)	(0.1073)	(0.3075)	(0.3270)
constant	-1.7614***	-1.9535***	-1.7614	-1.8125
	(0.2724)	(0.0207)	(0.2724)	(0.0727)
journal FE	NO	YES	NO	YES
N	50,560	50,560	53,742	53,742

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Dependent Variable: Binary indicator that turns one if a paper has been published with open access. Standard errors in parentheses clustered on the journal compound level. The regression for EU funding excludes all observations that have received any kind of funding as defined in Table 4.7. In addition, it excludes those observations that have received US funding but do not mention a funding number. The regression for US funding excludes all observations that have received any kind of funding as defined in Table 4.7. In addition, it excludes those observations that have received EU funding but do not mention a funding number.

Table 4.23: The effect of funding on the probability of open-access: Effects for selected funding bodies from the EU and the US – excluding observations with funding from any other source

	Multinomial Logit	
	EU	US
	Reference: RA	
	XOA	
$\mathbb{1}_{funding}$	0.9051***	0.1714
	(0.2375)	(0.1871)
constant	-4,0324***	-4.0317
	(0.0428)	(0.0367)
journal FE	YES	
	SOA	
$\mathbb{1}_{funding}$	1.2639***	0.3850
	(0.1124)	(0.3676)
constant	-2.0948***	-1.9300***
	(0.0228)	(0.0835)
journal FE	YES	YES
N	50,560	53,742

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Dependent Variable: Categorical variable that turns one if a paper has been published with X open access, and two with subscription based open access. Reference category: Restricted access. Standard errors in parentheses clustered on the journal compound level. The regression for EU funding excludes all observations that have received US funding as defined in Table 4.7. The regression for US funding excludes all observations that have received EU funding as defined in Table 4.7.

Table 4.24: The effect of funding on the probability of open-access by type – EU & US – excluding observations with funding from any other source

	European Union			United States		
	Poisson	OLS	log OLS	Poisson	OLS	log OLS
XOA	-0.376***	-3.844***	-0.314***	-0.372***	-3.868***	-0.314***
	(0.032)	(0.271)	(0.021)	(0.033)	(0.284)	(0.021)
SOA	-0.032	-0.640**	-0.032**	-0.007	-0.389	-0.024*
	(0.018)	(0.240)	(0.010)	(0.021)	(0.282)	(0.011)
Funding (EU/US)	-0.078*	-0.884	-0.036	-0.022	-0.449	0.004
	(0.034)	(0.608)	(0.023)	(0.029)	(0.381)	(0.017)
XOA×Funding	0.148	1.184	0.064	0.017	0.914	0.031
	(0.134)	(1.585)	(0.099)	(0.151)	(1.319)	(0.105)
SOA×Funding	0.276***	3.394**	0.200***	-0.104	-1.290	0.006
	(0.075)	(1.300)	(0.045)	(0.085)	(1.162)	(0.034)
⋮	⋮	⋮	⋮	⋮	⋮	⋮
Constant	0.043	-5.416***	0.089***	0.043	-5.410***	0.090***
	(0.040)	(0.481)	(0.019)	(0.042)	(0.516)	(0.019)
journal FE	YES	YES	YES	YES	YES	YES
N	123,922	123,922	110,984	123,922	123,922	110,984

$^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Dependent Variable: Number of citations in levels (columns 1, 2, 4, 5), or logs (columns 3, 6). Standard errors in parentheses bootstrapped with 250 replications.

Table 4.25: Regression results addressing funding from the EU and the US – bootstrapped SEs

	European Union			United States		
	Poisson	OLS	log OLS	Poisson	OLS	log OLS
XOA	-0.344***	-4.223**	-0.312***	-0.346***	-4.254**	-0.313***
	(0.053)	(1.311)	(0.059)	(0.053)	(1.336)	(0.060)
SOA	-0.006	-0.544	-0.031	-0.011	-0.620	-0.024
	(0.096)	(0.920)	(0.071)	(0.102)	(0.997)	(0.073)
Funding (EU/US)	-0.075+	-0.920	-0.007	-0.017	-0.535	0.029
	(0.045)	(0.894)	(0.046)	(0.050)	(0.776)	(0.045)
XOA×Funding	0.118	1.197	0.047	0.015	1.199	0.023
	(0.135)	(1.663)	(0.099)	(0.116)	(1.850)	(0.108)
SOA×Funding	0.260*	2.572	0.174+	-0.054	-0.667	-0.004
	(0.118)	(1.604)	(0.088)	(0.100)	(1.245)	(0.070)
⋮	⋮	⋮	⋮	⋮	⋮	⋮
Constant	-0.070	-3.114	0.188	-0.058	-2.871	0.180
	(0.129)	(2.839)	(0.119)	(0.116)	(2.686)	(0.112)
journal FE	YES	YES	YES	YES	YES	YES
N	50,544	50,544	43,554	53,726	53,726	46,357

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Dependent Variable: Number of citations in levels (columns 1, 2, 4, 5), or logs (columns 3, 6). Standard errors in parentheses clustered on the journal compound level. The regression for EU funding excludes all observations that have received US funding as defined in Table 4.7. The regression for US funding excludes all observations that have received EU funding as defined in Table 4.7. Due to the smaller sample size, I also report significance on the 10% level using a +.

Table 4.26: Regression results addressing funding from the EU and the US – excluding observations with funding from any other source

4.8 Appendix B

Alternative geographical distinctions

For geographical distinction, I apply the binary separation of the world in a developed 'global North' and a developing 'global South.' In this appendix, I present additional evidence by using two different ways to separate affiliation countries of the author groups of the publications, namely based on income and age of the higher education system. Both variables shall capture the reputation of the research output of researchers based at institutions in countries with either high income or a longstanding tradition of university-based research and education.

High Income Countries:
The first one is based on the World Bank's country classification by income[27] using data from the bank's fiscal year 2020, which includes information up to the end of 2019. This is to avoid distortions from the COVID-19 pandemic and its economic consequences. The identification of this distinction builds upon the consideration

[27] See `https://datatopics.worldbank.org/world-development-indicators/the-world-by-income-and-region.html` for more information.

that high-income countries can spend more on their higher education systems.

	Broad definition		*Narrow definition*		
	High Income	Other	High Income	Other	Total
RA	13,634	84,635	4,203	94,066	98,269
XOA	600	1,658	174	2,084	2,258
SOA	3,561	7,971	1,098	10,434	11,532
Total	17,795	94,264	5,475	106,584	112,059

The 'high income' category includes all publications up to nine authors that have a majority of coauthors affiliated with an institution based in a high income country as defined by the World Bank (broad definition) or else all publications up to nine authors where all coauthors are affiliated with an institution based in a high income country (narrow definition).

Table 4.27: Publications from high income countries by access type

	Poisson	OLS	log OLS	Poisson	OLS	log OLS
	broad definition			narrow definition		
XOA	-0.358***	-3.986**	-0.325***	-0.359***	-3.978**	-0.322***
	(0.047)	(1.254)	(0.045)	(0.047)	(1.266)	(0.045)
SOA	-0.073	-1.306*	-0.059	-0.076	-1.310*	-0.055
	(0.048)	(0.610)	(0.040)	(0.045)	(0.596)	(0.039)
High Income	-0.061	-0.745	-0.078*	-0.143***	-1.695*	-0.119**
	(0.032)	(0.589)	(0.035)	(0.037)	(0.623)	(0.037)
XOA×High Income	0.055	1.068	0.097	0.289	4.152*	0.320**
	(0.087)	(1.142)	(0.061)	(0.170)	(1.940)	(0.113)
SOA×High Income	0.180**	2.368*	0.122**	0.213	2.831	0.170
	(0.060)	(0.949)	(0.038)	(0.133)	(1.468)	(0.084)
age	1.273***	8.275***	1.062***	1.265***	8.390***	1.068***
	(0.070)	(1.112)	(0.063)	(0.070)	(1.157)	(0.063)
age^2	-0.129***	-0.432***	-0.103***	-0.128***	-0.451***	-0.104***
	(0.010)	(0.110)	(0.009)	(0.009)	(0.112)	(0.009)
#authors	0.024**	0.257*	0.032***	0.022**	0.232*	0.031***
	(0.009)	(0.106)	(0.007)	(0.008)	(0.096)	(0.007)
constant	0.030	-5.253*	0.028	0.060	-5.253	0.034
	(0.148)	(2.527)	(0.100)	(0.142)	(2.614)	(0.101)
journal FE	YES	YES	YES	YES	YES	YES
N	112,059	112,059	100,302	99,739	99,739	89,331

$^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Dependent Variable: Number of citations in levels (columns 1, 2, 4, 5), or logs (columns 3, 6). Standard errors in parentheses clustered on the journal compound level. Alternative specification with bootstrapped standard errors in Table 4.29 below.

Table 4.28: Impact of the affiliation countries of authors: High Income Countries

	Poisson	OLS	log OLS	Poisson	OLS	log OLS
	broad definition			narrow definition		
XOA	-0.358***	-3.986***	-0.325***	-0.359***	-3.978***	-0.322***
	(0.041)	(0.335)	(0.026)	(0.037)	(0.345)	(0.027)
SOA	-0.073***	-1.306***	-0.059***	-0.076***	-1.310***	-0.055***
	(0.017)	(0.230)	(0.013)	(0.019)	(0.233)	(0.012)
High Income	-0.061**	-0.745*	-0.078***	-0.143***	-1.695***	-0.119***
	(0.022)	(0.304)	(0.010)	(0.035)	(0.387)	(0.018)
XOA×High Income	0.055	1.068	0.097	0.289*	4.152***	0.320***
	(0.075)	(0.728)	(0.051)	(0.136)	(1.218)	(0.091)
SOA×High Income	0.180***	2.368***	0.122***	0.213**	2.831***	0.170***
	(0.051)	(0.684)	(0.024)	(0.074)	(0.760)	(0.041)
age	1.273***	8.275***	1.062***	1.265***	8.390***	1.068***
	(0.018)	(0.228)	(0.011)	(0.018)	(0.217)	(0.011)
age^2	-0.129***	-0.432***	-0.103***	-0.128***	-0.451***	-0.104***
	(0.003)	(0.042)	(0.002)	(0.003)	(0.039)	(0.002)
#authors	0.024***	0.257***	0.032***	0.022***	0.232***	0.031***
	(0.002)	(0.034)	(0.002)	(0.003)	(0.034)	(0.002)
constant	0.030	-5.253***	0.028	0.060	-5.253***	0.034
	(0.040)	(0.462)	(0.021)	(0.037)	(0.436)	(0.022)
journal FE	YES	YES	YES	YES	YES	YES
N	112,059	112,059	100,302	99,739	99,739	89,331

$^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Dependent Variable: Number of citations in levels (columns 1, 2, 4, 5), or logs (columns 3, 6). Standard errors in parentheses bootstrapped with 250 replications.

Table 4.29: Impact of the affiliation countries of authors: High Income Countries – bootstrapped SEs

Established Higher Education Systems:
An alternative distinction is using a more narrow but slightly outdated distinction between 'Western' and 'non-Western' countries. The modern understanding of a university as a higher education institution emerged from medieval Western Europe and is a genuine European creation (Rüegg, 1992), which got exported during the age of colonization to what became the United States, Canada, and Australia. These countries are also among the leading economic powers globally, spend heavily on their higher education systems, and consist of open societies that ensure freedom of research. The endowment, the institutional environment, and the high standards for their higher education institutions lead to the strong reputation of Western universities. For example, Australia, France, Germany, the United Kingdom, and the United States host more than half of all exchange students, while more than half stem from Asian countries (Jon et al., 2014).

Thus, although the 'global West' definition might appear slightly outdated, it captures well the location of long-established research infrastructures. In this robustness check, I use the member states of the European Union between the years 1995 and 2003, when it had its largest size in *Western* Europe before expanding to post-Soviet countries in 2004 and later on. Additionally, I include Australia, Canada, and the United States as the largest non-European Western countries, which also have well-established university systems due to the colonization by the British and French.

	Broad definition		Narrow definition	
	West = 0	West = 1	West = 0	West = 1
RA	98,710	9,119	105,104	2,725
XOA	2,038	466	2,369	135
SOA	10,836	2,770	12,786	820
Total	111,584	12,355	120,259	3,680

Table 4.30: Publications from Western countries with established higher education systems by access type

	Poisson	OLS	log OLS	Poisson	OLS	log OLS
	broad definition			narrow definition		
XOA	-0.361***	-3.983**	-0.325***	-0.362***	-3.983**	-0.324***
	(0.045)	(1.234)	(0.044)	(0.044)	(1.240)	(0.045)
SOA	-0.061	-1.131	-0.053	-0.062	-1.130*	-0.050
	(0.046)	(0.564)	(0.039)	(0.043)	(0.549)	(0.038)
Estd. HE	-0.025	-0.203	-0.049	-0.083*	-0.954	-0.073*
	(0.039)	(0.621)	(0.039)	(0.037)	(0.592)	(0.031)
XOA×Estd. HE	0.064	1.151	0.116	0.204	3.480	0.310**
	(0.104)	(1.250)	(0.070)	(0.167)	(1.913)	(0.112)
SOA×Estd. HE	0.173*	2.286*	0.127**	0.130	2.053	0.158
	(0.074)	(1.074)	(0.045)	(0.134)	(1.374)	(0.095)
age	1.273***	8.269***	1.062***	1.263***	8.327***	1.065***
	(0.070)	(1.109)	(0.063)	(0.069)	(1.159)	(0.063)
age^2	-0.129***	-0.432***	-0.103***	-0.128***	-0.441***	-0.104***
	(0.009)	(0.110)	(0.009)	(0.009)	(0.113)	(0.009)
#authors	0.025**	0.273*	0.033***	0.024**	0.260*	0.032***
	(0.009)	(0.103)	(0.007)	(0.008)	(0.098)	(0.007)
constant	0.021	-5.378*	0.018	0.053	-5.299	0.025
	(0.146)	(2.540)	(0.100)	(0.143)	(2.665)	(0.102)
journal FE	YES	YES	YES	YES	YES	YES
N	112,059	112,059	100,302	104,138	104,138	93,259

$^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Dependent Variable: Number of citations in levels (columns 1, 2, 4, 5), or logs (columns 3, 6). Standard errors in parentheses clustered on the journal compound level. Alternative specification with bootstrapped standard errors in Table 4.32 below.

Table 4.31: Impact of the affiliation countries of authors: Established Higher Education Systems

	Poisson	OLS	log OLS	Poisson	OLS	log OLS
	broad definition			narrow definition		
XOA	-0.361***	-3.983***	-0.325***	-0.362***	-3.983***	-0.324***
	(0.035)	(0.325)	(0.025)	(0.038)	(0.336)	(0.024)
SOA	-0.061**	-1.131***	-0.053***	-0.062**	-1.130***	-0.050***
	(0.020)	(0.250)	(0.013)	(0.021)	(0.256)	(0.012)
Estd. HE	-0.025	-0.203	-0.049***	-0.083*	-0.954	-0.073***
	(0.030)	(0.431)	(0.013)	(0.040)	(0.498)	(0.022)
XOA×Estd. HE	0.064	1.151	0.116*	0.204	3.480**	0.310**
	(0.085)	(0.780)	(0.055)	(0.150)	(1.302)	(0.097)
SOA×Estd. HE	0.173**	2.286**	0.127***	0.130	2.053*	0.158***
	(0.058)	(0.776)	(0.026)	(0.086)	(0.938)	(0.047)
age	1.273***	8.269***	1.062***	1.263***	8.327***	1.065***
	(0.018)	(0.227)	(0.010)	(0.017)	(0.217)	(0.011)
age^2	-0.129***	-0.432***	-0.103***	-0.128***	-0.441***	-0.104***
	(0.003)	(0.042)	(0.002)	(0.003)	(0.040)	(0.002)
#authors	0.025***	0.273***	0.033***	0.024***	0.260***	0.032***
	(0.002)	(0.032)	(0.001)	(0.002)	(0.031)	(0.001)
constant	0.021	-5.378***	0.018	0.053	-5.299***	0.025
	(0.038)	(0.480)	(0.021)	(0.035)	(0.437)	(0.022)
journal FE	YES	YES	YES	YES	YES	YES
N	112,059	112,059	100,302	104,138	104,138	93,259

$^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$. Dependent Variable: Number of citations in levels (columns 1, 2, 4, 5), or logs (columns 3, 6). Standard errors in parentheses bootstrapped with 250 replications

Table 4.32: Impact of the affiliation countries of authors: Established Higher Education Systems – bootstrapped SEs

Chapter 5

What cannot be cured must be endured: The long-lasting effect of a COVID-19 infection on workplace productivity

Coauthored with Kai Fischer and J. James Reade

Published in 'Labour Economics'

5.1 Introduction

To counteract the spread of COVID-19, governments have introduced a wide range of non-pharmaceutical interventions (NPI), such as distancing rules or work-from-home directives. Though indispensable from an epidemiologist's perspective, measures such as the closing of schools and universities and the general reduction in economic activity has come with high direct and indirect costs for society as early economic evaluations emphasize (see, for example, the review articles by Brodeur et al., 2021; Padhan & Prabheesh, 2021) To date, most of this research has primarily considered the costs of NPIs. Besides the obvious effect of fewer infections and deaths in total, research quantifying the individual and economic benefits of infection prevention is hitherto missing. This paper addresses this gap in the literature by documenting the significant and persistent effect of a COVID-19 infection on individual labor productivity.

We contribute these novel findings by studying the performance of professional soccer players after a COVID-19 infection – a setting unrivaled in data quality: We use a highly granular nested panel data set that encompasses a sub-panel for every single match for nearly two years in the top-tier male soccer leagues of Germany and Italy. Thereby we take advantage of an institutional setting that is unique across occupations not only in terms of data availability. First, professional soccer is an industry that quickly resumed business, mostly unaffected by NPIs, after only a short interruption in spring 2020 – differently from many other industries. Second, the top European leagues implemented a uniquely rigorous testing procedure: Every player was PCR-tested at least once per week and often several times.

Our findings hardly suffer from measurement errors caused by unknown positives. Thus, we circumvent the issue of true case numbers being much higher than reported ones – a problem most occupations face (Hortaçsu et al., 2021; Manski & Molinari, 2021). Hence, we are actually able to estimate a population effect and not only the impact of COVID-19 of those showing up in a hospital. Given the popularity of the sport, we can exploit extremely detailed records of all players in every match. This allows us to disentangle individual and team productivity and to detect short- and long-run effects which would remain unobserved outside this industry. Eventually, medical studies on 'long COVID' are subject to methodological problems due to a reliance on patients' self-reported health or subsamples with strong symptoms (Yelin et al., 2020; Maxwell, 2021). By solely considering observational data, we avoid this issue.

To estimate the effects of a COVID-19 infection, we apply a staggered difference-in-differences framework. We compare infected with non-infected players before and after the infection and exploit the arguably idiosyncratic timing of infections with the virus for identification. In the context of our analysis, we consider productivity as a function of various individual health aspects, such as acceleration, condition, and endurance, but also cognitive capability. Our empirical analysis addresses two questions: Does a COVID-19 infection affect the probability of a player participating in a match and the length of time he stays on the pitch? This extensive margin captures general absence effects related to the infection but also takes up the non-consideration of post-infected players by the team managers. Second, is the performance of previously infected players lower once they play again? Here, our interest lies in productivity across matches as well as within a match – the intensive margin effects.

At the extensive margin, we find that once players are cleared to play by a team's medical staff, their time on the pitch decreases by more than 5 percent. At the intensive margin, we are able to identify a significant deterioration in infected players' productivity of 5–7 percent after an infection. This effect becomes visible right after a player's return to the pitch but remains persistent for more than eight months – a notable difference from what we find for common respiratory infections.

Exploiting the very rich nature of our data, we further assess players' performance throughout every single match. We identify a disproportional decrease in productivity toward the end of a game. This pattern might even be underestimated as the weakest players are likely to be substituted off. Our analysis reveals notable heterogeneity across age groups. Players above the age of 30 are twice as severely hit as players aged 26 to 30. For younger players up to 25 years of age there exists no significant effect at all.

Our paper also contributes to the strand of research which addresses differences between COVID-19 infections and other respiratory infections. For example, Briggs and Vassall (2021) approximate the costs of continuing health deterioration to amount up to 30 percent of the overall costs caused by the disease including fatalities. In our setting, we highlight that an infection with COVID-19 is indeed different from other respiratory infections, because a productivity deterioration of around 5.1 percent persists over the course of more than eight months. In contrast, we do not find productivity effects originating from colds and similar illnesses.

As in many industries, soccer is a team production. Therefore, we investigate how the individual performance deterioration affects the overall group outcome. A priori, it is unclear whether non-infected players might overcompensate the weakness of their colleagues or suffer from lower performance as well. Our findings support the latter. Players' joint performance tends to be even lower than the accumulated individual deterioration of infected team members.

Of particular interest for this paper is research on productivity effects during the pandemic. For example, Bloom et al. (2020) investigate firm-level productivity using a large panel from the UK and identify a decline in total factor productivity of 3–5%. Morikawa (2021) has shown that low-productivity firms in particular have drawn from public subsidy schemes. Regarding working from home, the findings for productivity are mixed. While Barrero et al. (2021) find an overall positive effect on worker productivity in the UK, Etheridge et al. (2020) do not find significant differences for the UK, Morikawa (2022) identifies a decline for the Japanese economy.

Using chess tournaments, Künn et al. (2022) identify a deterioration for cognitively demanding tasks.

Altindag et al. (2021) find that online learners during COVID-19 shutdowns have significantly worse outcomes compared to fellow students in classrooms. Particularly among academics, Deryugina et al. (2021a) find that womens' productivity was much more affected through the channel of lockdowns and the related burdens of childcare. In a broader sense, Adams-Prassl et al. (2020, 2022) show that workers have been unequally affected by the COVID-19 pandemic due to different possibilities to move their work to their homes. In contrast to those and many other economic papers, our primary focus lies on the *direct* effect of an infection itself on individual productivity and not indirect channels such as NPIs, which are exploited above.

By analyzing soccer players, we also contribute to a large body of economic research, which has frequently applied sports data to uncover otherwise hidden economic mechanisms (Bar-Eli et al., 2020). Among others, this concerns the testing of theoretical hypotheses from game theory (e.g., Bhaskar, 2008; Chiappori et al., 2002; Kassis et al., 2021), identifying psychological drivers of cognitive performance (e.g., Apesteguia & Palacios-Huerta, 2010; Gonzalez-Diaz & Palacios-Huerta, 2016), or deriving conclusions for public and labor economics (e.g., Caselli et al., 2022; Kahn & Sherer, 1988; Kleven et al., 2013; Lichter et al., 2017; Parsons et al., 2011; Principe & Ours, 2022).

The remainder of this paper proceeds as follows. In Section 5.2, we provide background information on the setting of this natural experiment and explain the data used. In Section 5.3, we outline our empirical analysis. Section 5.4 presents and discusses our results at the individual and the team level. Section 5.5 concludes with a summary, discusses limitations, and provides an outlook for future research.

5.2 Institutional Setting and Data

Germany's governmental agency for infectious diseases, the 'Robert-Koch-Institut' (RKI), registered about 26.3 million cases and more than 137,000 deaths related to a COVID-19 infection (up to June 2, 2022, within an overall population of 83 million).[1] A similar but worse pattern can be found in Italy. The governmental health agency 'Istituto Superiore di Sanita' reported (up to July 28, 2021) 17.5 million cases and some 164,000 casualties (within an overall population of 60 million).[2]

Only the registered cases in both countries make up for more than 25 percent of the overall population. As COVID-19 affects people of all age groups, millions of those with a cured infection are part of the labor force, which might potentially affect their productivity at work. Moreover, the discussed countries are just two examples and many countries face similar magnitudes of cases among their citizens. The problem of potentially persistent negative effects of an infection on subsequent productivity may be sizable given the large numbers of infected and recovered individuals.

We construct a novel dataset consisting of data on player and match statistics, as well as data on COVID-19 infections of players in Germany's Bundesliga and Italy's Serie A. Both leagues are their country's highest division in men's soccer and among the most successful five leagues worldwide.[3] The two leagues have characteristics that make them particularly appropriate to study. The Bundesliga was the first major soccer league to resume its season in 2020 after the suspension of almost all

[1] Source for data on case numbers: `https://www.rki.de/DE/Content/InfAZ/N/Neuartiges_Coronavirus/Daten/Altersverteilung.html` (incl. individuals with multiple infections). Source for data on casualties: `https://www.rki.de/DE/Content/InfAZ/N/Neuartiges_Coronavirus/Projekte_RKI/COVID-19_Todesfaelle.html`, last update June 2, 2022.

[2] Source for data on cases and casualties: Report Esteso ISS – COVID-19: Sorveglianza, impatto delle infeziomi ed efficacia vaccinale (national update): `https://www.epicentro.iss.it/coronavirus/bollettino/Bollettino-sorveglianza-integrata-COVID-19_31-maggio-2022.pdf`, published June 3, 2022, data up to May 31, 2022 (incl. individuals with multiple infections).

[3] In the European Football Association's five-year ranking, the Serie A was ranked #3 and the Bundesliga #4 in 2021, see `https://www.transfermarkt.com/uefa/5jahreswertung/statistik`, last checked August 16, 2023.

leagues in spring.[4] Italy was hit severely by the virus in Spring 2020 (as shown by the 7-day incidence rates of Italy on the LHS of Figure 5.1), but continued its season in June, too.[5] Hence, for both leagues we have players that have been infected in early stages of the pandemic. This allows us to estimate persistent and long-run effects among the infected individuals as we cover a time span of more than 12 months after the outbreak of COVID-19 in Germany and Italy.

We have granular data at the match and at the minute level, allowing for overall but also match phase-specific analyses. We amend this data with information on the injuries and sicknesses that forced players to miss matches. In addition, we include information on player nominations for the national teams during this period of time.[6] We collect data on all COVID-19 infections in both leagues since the outbreak of the pandemic. While every infection has to be communicated to the local authorities by the club carrying out the testing, clubs may prefer to keep an infection anonymous, only announcing the number of cases. We identified the large majority of all infected players via a meticulous review of newspapers, reliable websites, and statements from the clubs, players, and soccer associations.

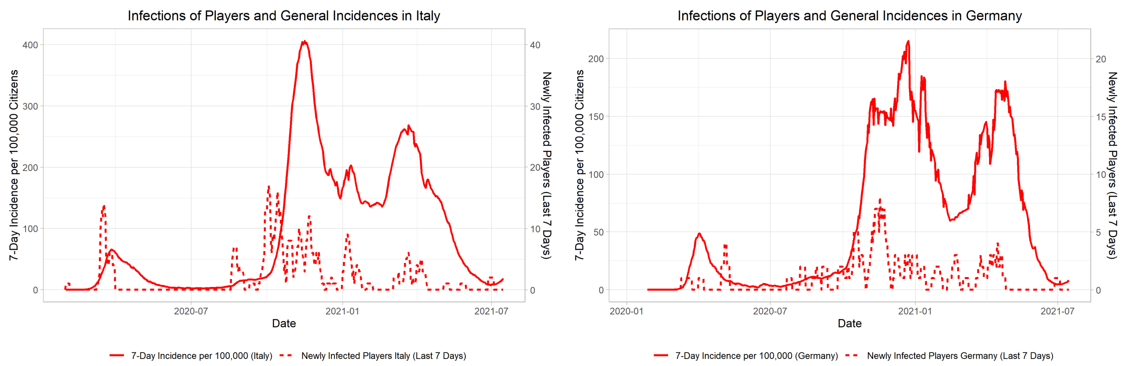
There had been 81 true-positive tests among players in Germany and 176 in Italy by mid-July 2021. We can identify 76 players in Germany and 157 in Italy. Hence, we build our analysis upon the 233 identified players from a sample of 257 positive cases in total. This results in a coverage of over 90 percent. The higher case rates in Italy are likely to be driven by more registered cases in the overall population, and because Italy's Serie A includes more teams (20 compared to 18 in Germany). The high coverage of identified cases should comfortably exceed the knowledge on infections in most industries and allows us to consider our results

[4]`https://www.theguardian.com/football/2020/may/06/bundesliga-set-for-go-ahead-to-resume-season-in-second-half-of-may`, published May 7, 2020, last checked August 16, 2023.

[5]`https://football-italia.net/official-quarantine-rule-softened/`, published June 18, 2020, last checked August 16, 2023.

[6]Injury and national team data is obtained from *transfermarkt*, the largest database on soccer players globally.

to be representative of the dataset at hand. We also conduct our analysis for a subsample of the data, in which we drop the observations of teams with anonymous cases. By doing that, we obtain a dataset of perfectly identified players. Our results are highly similar in this case. To further illustrate the case numbers among footballers relative to the overall population, Figure 5.1 provides information on player infections and 7-day incidences over time – i.e., the number of newly infected persons per 100,000 inhabitants. Infections evolve similarly over time. Figure 5.1 also highlights incidences close to zero in the summer break between the two seasons. This probably would have been the only period in which clubs could have kept an infection secret without the media recognizing the absence of a player. As the overall incidences were very low during this time, we suspect the number of non-identified but infected players to be, if anything, very low.



The plots show the seven-day incidence for Italy (LHS) and Germany (RHS) over time (left y-axis). The seven-day incidence counts all cases over the last seven days and scales them on 100,000. Also, cases among players are given (right y-axis). Source country incidences: Ritchie et al. (2021, data downloaded: 16.07.2021)

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Figure 5.1: General Incidences and Player Infections

The 257 infections among 1,406 players imply that by mid-July 2021, 18 percent of all players had been infected. This exceeds the general incidence of cases in the age group of young adults in both countries. It is likely a consequence of persistent testing and extensive traveling. Additionally, both leagues implemented rigid rules for club and player behavior. The Bundesliga set in place compulsory testing once or twice a week and before a match.[7] The Serie A required a PCR test before a match.[8] Hence, we are confident that we have a true picture of the overall infections.

For player and match statistics, we apply data from *Opta Sports*. The company is one of the leading firms for statistics in sports and has an official partnership with the Bundesliga and the Serie A.[9] The company tracks every player and all of his actions during a match using software that analyzes video records. Every action on the pitch is recorded and registered with the coordinates showing where it happened. We were able to gather information on which players participated in each match of the 2019/2020 and the 2020/2021 seasons, and how these players performed in a match. Hence, we are confident that we have the best data available to track the productivity of all the players.

Our dataset consists of 72,938 records from 1,406 players ranging over both seasons and leagues. These data encompass all players who played on at least one matchday of a season. Among these observations, 40,607 records track players who played in a certain match, i.e., we can construct within-match work performance for them. The remainder covers players who were not nominated or substituted on the pitch at a particular match. Their observations will be included in the analysis at the extensive margin, i.e., whether a player plays. Table 5.2 in the appendix provides descriptive statistics.

We further extend this already rich dataset with information on wages for Italy. Here, we build upon data collected by the 'La Gazzetta dello Sport,' the largest Italian daily newspaper. It provides data on the seasons 2019/2020 and 2020/2021

[7] https://www.dfb.de/fileadmin/_dfbdam/226090-Task_Force_Sportmedizin_Sonderspielbetrieb_Version_3.0.pdf, published August 26, 2020, last checked August 16, 2023 – one or two PCR tests per week depends on the severity of the infection process. One PCR test per week was only allowed in case the region or district of a club had a 7-day incidence < 5 per 100,000 people, which was hardly ever the case during the seasons. PCR testing is the most accurate form of testing for a virus with almost 100 percent sensitivity (Guglielmi, 2020).

[8] https://www.figc.it/media/123076/circolare-quarantena-calcio-def-2.pdf, published June 18, 2020, last checked August 16, 2023.

[9] https://www.statsperform.com/team-performance/leagues-federations/, last checked August 16, 2023. There also exists some literature that validates the quality of the data from Opta, see Liu et al. (2013).

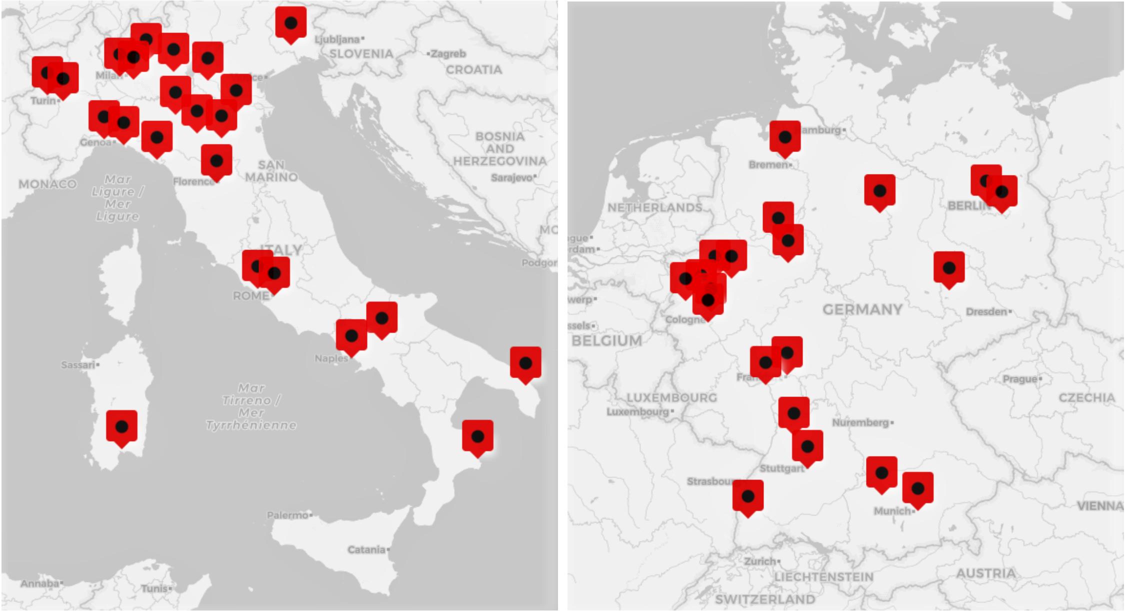
for all teams in the Serie A. For the earlier season, we fill missing information on teams with the salary reports for the season 2018/2019. In total, we are able to cover 78 percent of the player×season observations with salary data. We are not the first to use this source, Principe and Ours (2022) have used 'Gazzetta' salary data as well. This source tends to truncate weaker players, but we still consider the data as valid and highly valuable for our analysis.

With an increasing likelihood of soccer players being or getting vaccinated in the season 2021/22, our analysis of the previous two seasons brings two advantages: First, we can track the unbiased effect of infections without the 'distortion' due to vaccinations, as the Serie A and the Bundesliga started vaccinations only after the end of the 2020/2021 season.[10] Second, vaccinations are likely to increase the degree of self-selection into treatment if some players prefer to remain non-vaccinated. Moreover, our still relatively short treatment period of 15 months (since the beginning of the pandemic) enables us to disregard sample selection issues, for example, that severely hit players may drop out of the top leagues. Contract rigidity in elite soccer ensures that most players remained with their clubs for the whole period.[11]

The comparison between infected – treatment group – and non-infected players – control group – is relevant in our setting. We match both groups and their characteristics with each other in Table 5.1. While the infection timing is arguably random for each player, there might be some selection into who gets infected or not. Indeed, we find some disparities in the performance measures. Players from Italian clubs are slightly over-represented in the sample of infected players. This might be due to the overall incidences, which have been much higher in Italy compared to Germany (as shown in Figure 5.1). Case numbers were particularly high in Northern Italy, where most of the clubs in the Serie A are located (see Figure 5.2). Furthermore,

[10] See for the Serie A: `https://football-italia.net/figc-wants-serie-a-and-serie-b-players-to-get-vaccinated/`, published July 19, 2021, last checked August 18, 2023; and the Bundesliga (in German): `https://www.kicker.de/dfl-empfiehlt-impfung-der-profis-fan-rueckkehr-realistisch-806070/artikel`, published May 28, 2021, last checked August 16, 2023.

[11] Older work by Frick (2007) reports an average contract length of 3 years in the Bundesliga.



The maps give the clubs' location (left: Italy, right: Germany). The maps capture clubs being part of the respective league in one or both seasons. Underlying maps by `www.openstreetmap.org`.

Figure 5.2: Location of the Leagues' Clubs in the Dataset

infected players seem to have played more often and longer prior to the treatment. They also performed better in terms of passes and touches per minute. There are no significant differences in age or other demographics, which might be important for the severity of the symptoms. Concerning positions, it seems that midfielders are over-represented.

We address the differences between the treatment and control groups by controlling for the player- and position-specific effects later on. We then also restrict samples to similar levels of quality to avoid weaker players in the control group biasing our findings and we find our results to be robust for using solely the subsample of treated individuals. Eventually, we perform propensity score matching to create a comparison group that is statistically indifferent to the group of infected players based on observables. All approaches lead to comparable results, so we are confident that sample selection does not drive our findings.

Statistic	Units	Non-Infected	Infected (Pre-Infection)	Δ (p-value)
Match Involvement/Performance				
Played at all	yes/no	0.539	0.659	0.000***
if played...				
Minutes Played	min	66.340	71.509	0.000***
Played Full-time	yes/no	0.484	0.564	0.001***
Passes/min	#/min	0.511	0.546	0.023**
Ball Recoveries/min	#/min	0.057	0.057	0.778
Touches/min	#/min	0.681	0.713	0.042**
Possession/min	#/min	0.491	0.526	0.017**
Dribbles/min	#/min	0.019	0.019	0.402
Aerials/min	#/min	0.038	0.033	0.021**
Shots/min	#/min	0.015	0.017	0.103
Demographics				
Age	years	26.550	26.886	0.351
Height	cm	183.350	184.268	0.049**
Weight	kg	77.273	77.754	0.339
Body Mass Index (BMI)	kg/m^2	22.966	22.879	0.376
Others				
Italian League	yes/no	0.541	0.636	0.013**
Goalkeeper	yes/no	0.043	0.052	0.567
Defender	yes/no	0.223	0.251	0.281
Midfielder	yes/no	0.150	0.225	0.001***
Striker	yes/no	0.077	0.087	0.519
Substitute	yes/no	0.506	0.384	0.000***

Columns (1) and (2) show the means of the respective variable for all observations of non-infected players and infected players (pre-infection). Column (3) reports the p-value of a two-sided t-test. Significant differences are indicated by stars as follows: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$. The significance of differences between infected and non-infected players is obtained from simple regressions of each outcome on an intercept and a dummy for infected players pre-infection. Standard errors of these regressions are clustered on the player level. Variables which give performance per minute omit observations with zero minutes on the field which make up 232 out of 36,671 observations (approximately 0.6%).

Table 5.1: Descriptive Statistics for Non-Infected and Infected Players

5.3 Empirical Strategy

Infections can be modeled as a staggered treatment across players. To disentangle the effect of the infection from other shocks that may limit work performance, we compare infected players' performance before and after their positive test results with the evolution of outcomes of non-infected players. Hence, we apply a difference-in-differences estimation that controls for variation over time and across individuals.[12]

For this setting to be valid, several assumptions need to hold. Within our simple difference-in-differences setting, we need parallel trends of the treatment and control group in the absence of the infection. We have no reason to question this because there is no conceivable cause for the diverging evolution of productivity without COVID-19. Within the dynamic event study setting outlined later on, this corresponds to the requirement that treatment cannot predict outcomes before treatment. As our event study plots will show flat pre-trends, we consider the parallel trends assumption as not violated.

There may exist endogenous drivers of the individual infection risk, for example matches of the national teams that require more and particularly international traveling. The same holds for continental tournaments such as the UEFA Champions League or the UEFA Europa League. In both cases, for obvious reasons, stronger players are more affected than weaker ones. Furthermore, there might be higher exposure to infected people depending on the individual's social predilection for attending parties or public events. Hence, the risk of infection might not be completely idiosyncratic. However, random selection into infection is not necessary for identification, as identification is drawn from the timing of the infection. This should

[12]For this methodology a rapidly developing literature has emerged, which mainly addresses the distortions arising from staggered treatments in plain two-way fixed effects settings (see, e.g., the recent survey by Chaisemartin and D'Haultfoeuille (2022) on this literature). The main critique is that treatment effects at a certain relative point of time to the treatment event might change with heterogeneity in real time. Related to the critique we later show that there is no significant change in the treatment effect between early and recent infections.

be exogenous for all types of players and unanticipated in the short run. Potential differences between treated and non-treated players will nevertheless be addressed in our robustness checks.

For reliable estimations in the simple difference-in-differences setting, we need no variation in the effect size of the treatment over time. This would not be true if, for example, a new medication had been developed that would have changed the impact of an infection. In general, there is no reason to believe that the work performance effects of an infection are constant over time, so we will analyze dynamic patterns in event studies.

Eventually, a difference-in-differences estimator requires that the treatment only causes partial equilibrium effects, or else we need the stable unit treatment variable assumption (SUTVA) to be fulfilled. As we find spillover effects within a team, there might be some confounding, which collides with the SUTVA. This does not invalidate but strengthens our empirical findings. In theory, it is a priori unclear whether a deterioration in a player's performance either causes an overall lower performance of the team or leads to an (over-)compensation of this deterioration. Indeed, we find strong evidence of the former. An increasing number of recovered players on the pitch decreases their team's performance disproportionately. This implies a negative effect on the control group. Hence, our estimates underestimate the true effect in absolute terms.

As we consider the identifying assumptions as fulfilled and the spillovers as innocuous, our model allows us to extract the treatment effect. We implement the regression setup

$$\text{Performance}_{pm} = \beta \, \text{Post-Infection}_{pm} + X'_{pm}\gamma + Z'\zeta + \epsilon_{pm} \, . \tag{5.1}$$

Performance_{pm} on the LHS refers to a set of performance or involvement measures of player p in match m. In our setting, this is, for example, a dummy capturing whether a player played at all, or the exact number of passes (in logs).

We use the number of passes as the main productivity measure for the intensive margin estimations. Individual performance in soccer depends on various physical health measures such as acceleration, condition, and endurance, but also the cognitive capability to position oneself optimally on the pitch. The number of passes is related to all of these measures and thereby suitably proxy the involvement of players in a match. Former papers on work performance in sports have also exploited the number of passes as a measure of interest (for example Carmichael et al., 2001; Lichter et al., 2017; Oberstone, 2009). Descriptive statistics on this measure can be found in Figure 5.16 in the appendix. Results on other measures (e.g., touches and possession) – which also account for slightly different behavior – are provided later on as a robustness check. Hence, cross-validation with different measures should give a thorough picture of player performance.[13]

Post-Infection$_{pm}$ is the treatment dummy that takes the value 1 for all observations of a player after he has tested positive. Hence, β is our coefficient of interest. To account for variation in the cross-section and over time, we control for a large set of covariates X_{pm} and fixed effects (FE) Z. The vector X_{pm} contains a player's age, the plain and squared number of minutes played to capture non-linearities in time on the pitch, a dummy variable for a home match and one that distinguishes matches before and after the interruption of the leagues in Spring 2020. The vector Z includes player fixed effects, team-season, and opponent-season fixed effects as well as matchday fixed effects and an FE capturing variation before and after the interruption in Spring 2020. Doing this, we control for a general underlying performance effect during the COVID-19 pandemic for all players, as Santana et al. (2021) find a worse running performance after the restart in 2020 but an improved passing accuracy. Also, the exclusion of fans might have impacted player behavior during this period (Bryson et al., 2021). All of these FEs shall capture performance

[13]Running performance would be another natural measure to study as COVID-19 is a respiratory infection. However, Lichter et al. (2017) find running and pass performance to be highly correlated. We obtain player-match level running data for the Bundesliga and measure a correlation of 0.64. Later on, we use a variable similar to running performance that shows a significant drop after an infection as well.

differences unrelated to an infection.[14] ϵ_{pm} is the idiosyncratic error term. We use heteroskedasticity-robust standard errors clustered on the player (i.e., the treatment) level to account for correlated residuals across a player's observations. To test the identifying assumption of parallel trends absent a treatment and to understand the dynamic nature of effects, we also apply an event study setting as a dynamic model:

$$\text{Performance}_{pm} = \sum_{\tau=\underline{k},\tau\neq 0}^{\bar{k}} \beta_\tau \text{ Post-Infection}_{pm,\tau} + X'_{pm}\gamma + Z'\zeta + \epsilon_{pm}\,. \tag{5.2}$$

This leads to several β_τ coefficients of interest. Subscript τ is the running index of leads and lags. We bin these one-day binary variables to group dummies of 75 days. Endpoints are binned and hence include all observations which lie beyond the second-last bins on either side (Schmidheiny & Siegloch, 2020). Our results are robust to different specifications of the effect window size. We mainly plot bins up to 225 days before and after infections and bin all observations beyond these thresholds in the outer bins to have sufficiently many observations in each bin. As infections are hardly anticipated and voluntary precautions are only possible with limitations in the world of professional soccer, we do not struggle with a number of identification challenges that have been addressed in the context of COVID-19 studies, such as voluntary precautions, anticipation, and variation in policy timing (see, e.g., Goodman-Bacon & Marcus, 2020).

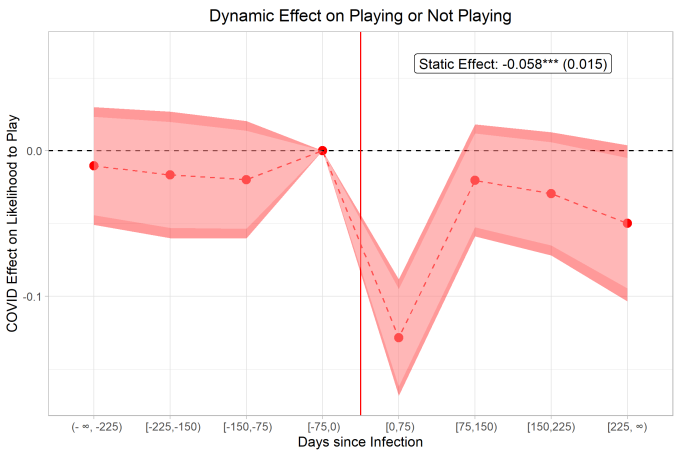
5.4 Results

Our analysis takes two steps. First, we investigate whether a COVID-19 infection has a short- or long-term impact on the participation of players. Subsequently, we look at within-match performance after an infection. The intensive margin could underestimate the persistent effects of a COVID-19 infection as players hit the most

[14]We experimented with several reasonable FE combinations. All results go in the same direction. A battery of FE combinations is discussed later on in the paragraph on robustness checks. Plots are provided in Fig. 5.25 in the appendix.

might not play at all. Hence, analyzing both effects is indispensable and might offer some intuition on performance-related mechanisms. While the main measure of interest is within-match work performance, the effect at the extensive margin helps to understand the severeness of the post-infection work performance drops.

5.4.1 Extensive Margin

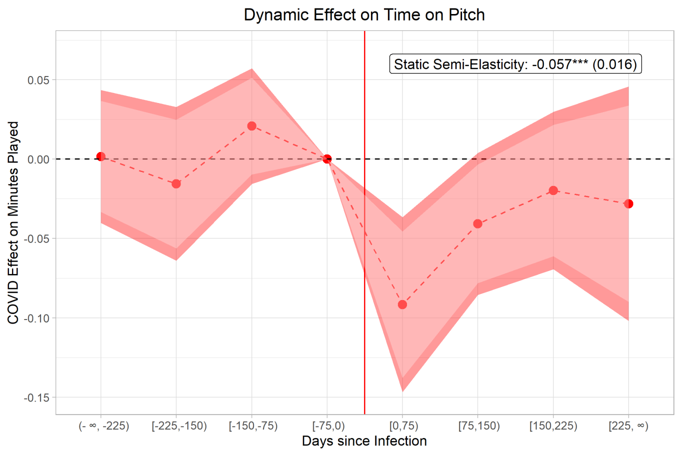


This figure plots the OLS (LPM) estimated coefficients β_τ of the event study regression following Equation (5.2). The reference time period is one to 75 days before treatment. An equivalent plot with a 30-day bin size (for a better description of short-run effects) can be found in Figure 5.18 in the appendix. Standard errors are heteroskedasticity-robust and clustered at the player level. 90 and 95% confidence intervals are given by the red-shaded areas. The dependent variable is a dummy indicating whether a player played or not.

Figure 5.3: Dynamic Effect on Likelihood to Play

First, we analyze the effect of a COVID-19 infection on the probability of playing and the number of minutes played. Figure 5.3 reports the corresponding estimates. From the simple effect, in the upper right of the plot, we infer that players have a 5.7 percentage-point lower probability of playing. However, effects appear to be mechanical, mainly driven by the initial weeks after an infection, when quarantine breaks do not allow a player to participate in a match. The observed drop in playing frequency becomes quickly insignificant again, but does not fully return to its former level. These results indicate that players marginally experience persistent effects on their likelihood to play. A flat pre-trend validates our finding.

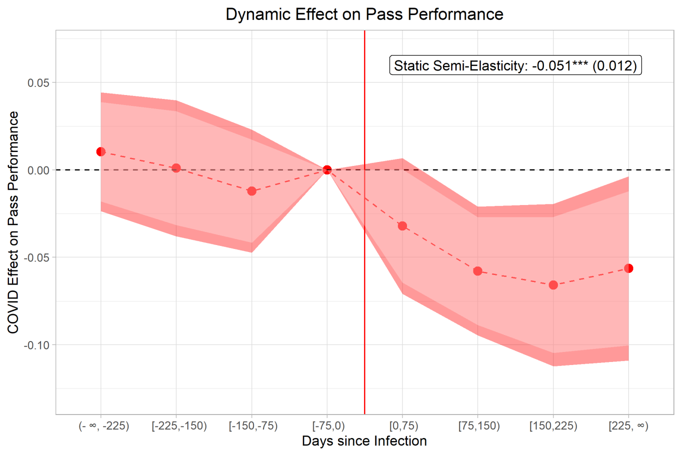
Figure 5.4 shows the corresponding effect on minutes played by players who play. Immediately after the infection and his return on the pitch, a player spends an average of six minutes less on the field than before – this corresponds to a decrease of almost ten percent. It indicates that several players might have been used only as substitutes leaving the pitch earlier or entering it later. This might point to a general fitness problem of the players and make it more likely that work performance effects at the intensive margin might be underestimated as the player might be substituted off before the severest effects kick in. The effect is visible right after an infection but is quite long-lasting. Only after approximately 150 days or five months of play does fitness return to a level that does not significantly differ from pre-infection match times.



This figure plots the OLS estimated coefficients β_τ of the event study regression following Equation (5.2). The reference time period is one to 75 days before treatment. An equivalent plot with a 30-day bin size (for a better description of short-run effects) can be found in Figure 5.18 in the appendix. Standard errors are heteroskedasticity-robust and clustered at the player level. The 90 and 95% confidence intervals are given by the two red-shaded areas. The dependent variable is ln(minutes played) conditional on having played.

Figure 5.4: Dynamic Effect on Minutes Played

Our findings at the extensive margin are confirmed by an increasing likelihood of being substituted on and off the pitch after an infection. On average, players play for a shorter time which may signal insufficient fitness to participate for 90 minutes. The respective event studies can be found in Figure 5.17 in the appendix. In general, our results on the effects at the extensive margin indicate a return to initial levels of infected players over time. Either the players return to the former work performance levels or badly performing players re-enter the subsample of players on the pitch. This would shift the treatment effect from the extensive to the intensive margin, such that worse work performance effects should be observed over time in within-match data.



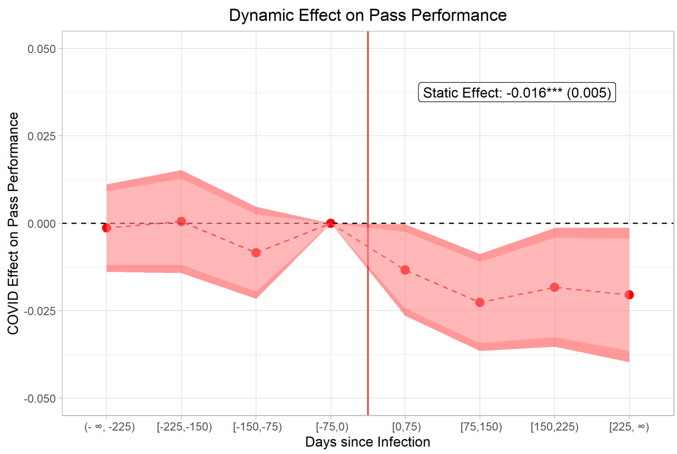
These figures plot the OLS coefficients β_τ of the event study regression following Equation (5.2). The reference time period is one to 75 days before treatment. Standard errors are heteroskedasticity-robust and clustered at the player level. The 90 and 95% confidence intervals are given by the two red-shaded areas. The dependent variable is ln(passes). Additional work performance measures can be found in figures 5.20 and 5.21 in the appendix.

Figure 5.5: Dynamic Effect on Within-Match Work Performance

5.4.2 Intensive Margin

We next take a nuanced look at a player's performance conditional on being on the pitch. As previously outlined, the main building block of our productivity analysis is the number of passes, as shown in Figure 5.5. Besides that, we provide results on two related performance measures, possession and touches in Figure 5.19 in the appendix. Figure 5.5 presents the corresponding event study providing the dynamic estimates of a COVID-19 infection on within-match performance. This plot, as well as the additional measures in the appendix, show rather flat pre-trends. We find a highly significant simple difference-in-differences effect of -5.1 percent. Thus, we can precisely identify deterioration in productivity following a cured COVID-19 infection. This effect is not transient but remains notably negative over the course of time. We consider this as causal evidence of COVID-19 infections causing long-lasting productivity drops for infected individuals.

This finding is surprisingly coherent with medical research from Switzerland that finds 'long COVID' symptoms to be persistent over seven to nine months for a third of all infected persons in the analyzed sample population (Nehme et al., 2021). As an alternative outcome measure, Figure 5.6 presents the effect over a COVID-19 infection on an observation's rank in the pass distribution. It is apparent that the persistent deterioration combined with flat and insignificant pre-trends remains in place. A player slides down in the relative productivity ranking by 1.6 percentiles.



These figures plot the OLS coefficients β_τ of the event study regression following Equation (5.2). The reference time period is one to 75 days before treatment. Standard errors are heteroskedasticity-robust and clustered at the player level. The 90 and 95% confidence intervals are given by the two red-shaded areas. Instead of ln(passes), this regression uses the ln(rank) of the amount of passes played by a player during a match.

Figure 5.6: Dynamic Effect on the Ranking of Infected Players

Interestingly, we also see work performance partly fall over time, while the effect stabilizes after some months post-infection. This gives rise to two remarks: First, players do not return to their former level within the period of observation. Second, the reduction over time also captures the return of infected players to the pitch as there is more involvement at the extensive margin after several months. It may be possible that players who still suffer from weakened performance, eventually return to the pitch and negatively affect the treatment effect over time.

Furthermore, Table 5.1 provides some evidence on differences between infected and non-infected players. To ensure that the intensive margin effect is not driven by this, we address the potential issue of sample selection with a propensity score matching procedure. We do this by nearest neighbor matching without replacement. The matching takes place within matchday and position, i.e., for a midfielder infected right before matchday 16 in season 20/21, we look for a midfielder equivalent on matchday 15 of this season. We include team×season, matchday×season and position FEs in the matching probit regression. We do not allow a matching of infected players with those players who become infected at a different point in time or with players who have not played a single minute up to the respective matchday.

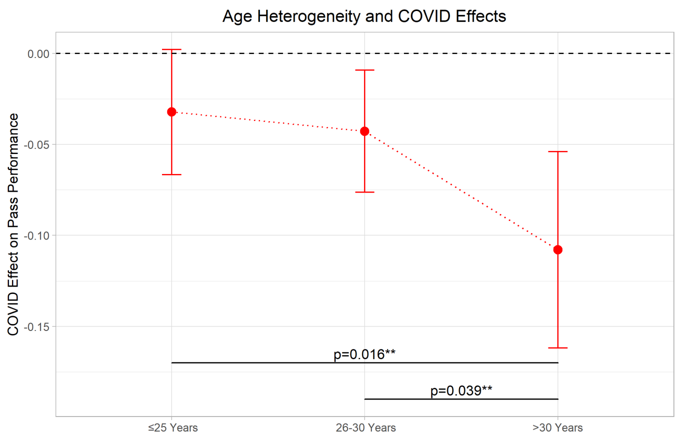
Table 5.3 in the appendix shows that the generated control group does not differ in any observable dimension from the treatment group. Re-estimating our main regression for the intensive and the extensive margin using the fully balanced sample, we get results that are very similar to our baseline results (as shown in Figure 5.31 in the appendix).[15]

5.4.3 Effect Heterogeneity

We do not only find a significant and persistent deterioration in work performance but also heterogeneity in several dimensions. While an infection's effect on the underlying health status should be quite homogeneous in the homogeneous group of players, the consequences of changes in health might impact player performance differently. First, throughout the COVID-19 pandemic, age has been one of the main determinants of how likely an infected person is to develop symptoms or to

[15]Figure 5.31 also provides the extensive margin effect on the likelihood to play.

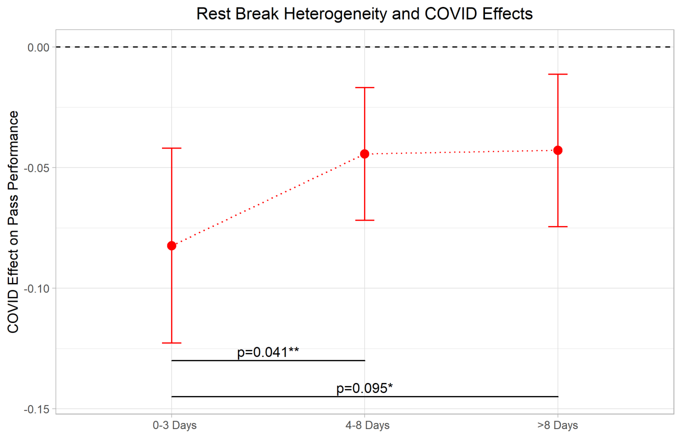
even die (e.g., Gallo Marin et al. (2021)). It seems natural to investigate whether older players also suffer more from an infection. Even though professional athletes in their thirties cannot be compared to the overall elderly population, their recovery may take longer and symptoms may be more persistent. Figure 5.7 provides some intuition that especially players aged 30 and over face the strongest performance drops of over 10 percent. In comparison, younger players up to 25 years of age are only affected marginally. Both effects are statistically significantly different from each other as the Wald test provided in Figure 5.7 highlights.



The plot displays OLS interaction effects between the post-infection dummy and age groups included in equation (5.1). Dependent variable: ln(Passes). SEs: Heteroskedasticity-robust and clustered at player level. The 95% confidence bands are given. p-values: Wald tests for difference between the respective estimates.

Figure 5.7: Effect Heterogeneity: Age Effects

Second, COVID-19 infections are often associated with additional fatigue. Therefore, we investigate whether players need more time to recover from a match post-infection. Put differently, it may be that post-infected players perform worse if the rest break between two matches they played in is insufficient. We compare the treatment effect for different lengths of rest breaks. In Figure 5.8, we show that the treatment effect is especially strong for short breaks of up to three days. The shortest breaks are in terms of the productivity effect statistically significantly worse than longer gaps from four days onward. Our results indicate that post-infected players perform better - though not as well as before the infection - if there is enough regeneration time.



The plot displays OLS interaction effects between the post-infection dummy and different recovery breaks included in equation (5.1). The length of a break is calculated on the player level, i.e., the number of days between two matches the player has played in. Dependent variable: ln(Passes). SEs: Heteroskedasticity-robust and clustered at the player level. The 95% confidence bands are given. The first observation of every player is dropped as no recovery break to previous matches can be calculated. p-values: Wald tests for difference between the respective estimates.

Figure 5.8: Effect Heterogeneity: Recovery Break Effects

We also analyze heterogeneity with regard to positions, team and player strength, and infection timing. Results are intuitive as we find stronger effects for more enduring positions or weaker players.[16] Also, there seems to be no difference between effects from early or late COVID infections, i.e., there tends to be no treatment effect heterogeneity in real time. All plots can be found in Figure 5.23 in the appendix. There, we also provide equivalent heterogeneity analyses for the extensive margin (Fig. 5.24). The results are very similar.

Lastly, we study the impact of the severity of symptoms on the subsequent performance. As we cannot measure or observe the exact symptoms infected players suffered from when being isolated at home, we use the length of the break between a player's infection and his return to the *squad* as an approximation. This is because

[16]The heterogeneity analyses on positions also show that the treatment effect is not driven purely by substituted players but also by starters.

the return to the squad (without necessarily having played) implies that the player is able to play again, i.e., free of symptoms.[17] Moreover, there is less of a quality-based selection issue into the squad than into the players on the pitch. We also restrict the sample to those players who could actually play a match soon after the official end of quarantine. In addition, we drop players who suffered a different injury right after their infection.

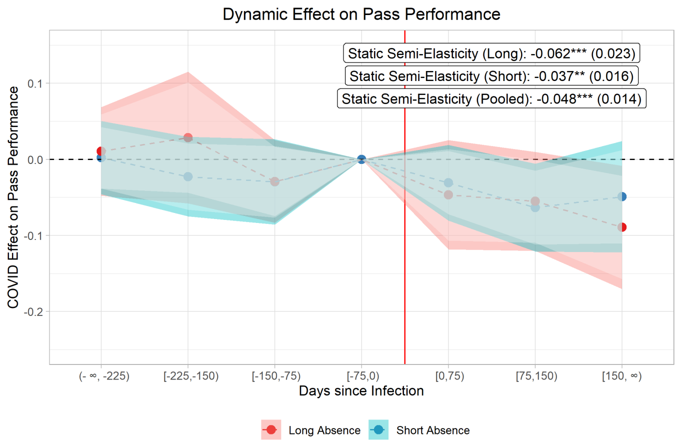
Finally, we drop the 'worst' 10 percent of the remaining, infected players with regard to playing time as for them selection into the squad might have been an issue. We conducted several tests to ensure that there is no strong linkage between player quality and return velocity in the remaining squad. As one would expect, the point estimate for players with a longer break is higher in absolute terms than for shorter absences, as Figure 5.9 shows.[18]

5.4.4 Comparison to Other Injuries

It may be that players and teams treat COVID-19 just like any other injury – a player rests for a while and returns to team practice afterward. If COVID-19 infections have performance effects beyond typical injuries and illnesses, this would emphasize the relevance and uniqueness of this particular virus infection. We investigate this by analyzing the work performance effects of all other injuries which happened during our sample period. They range from muscle and ligament injuries to simple colds. In Figure 5.10, we distinguish the effects of a COVID-19 infection from both short and long injury breaks. We split the data at the median injury duration (2

[17]A squad typically consists of 20-22 players (depending on the league) and, hence, encompasses more players than actually play (typically the starting line-up (eleven players) plus 0-5 substitutes).

[18]Though, the point estimates are not significantly different from each other in this reduced sample (Wald test: $p = 0.345$). Also note that the effect of the pooled sample differs from the effect shown in Fig. 5.5 as we exclude those players who suffered an injury directly after the infection, never played again after being infected or got infected directly before the long interruption in Spring 2020, a summer or a winter break as this would distort the approximation. Hence, we only consider players who actually had the chance to play in a match in the two weeks after the end of quarantining.



The plot displays the intensive margin effect of an infection depending on the severity of an infection, approximated by the length of the interval between infection and return to the squad. Sample split at the median. $N = 140$ infected players. The last two bins $[150, 225)$ and $[225, \infty)$ are pooled to a joint endpoint due to the otherwise very small number of observations. Dependent variable: ln(Passes). SEs: Heteroskedasticity-robust and clustered at the player level. The 90 and (darker) 95% confidence bands are given.

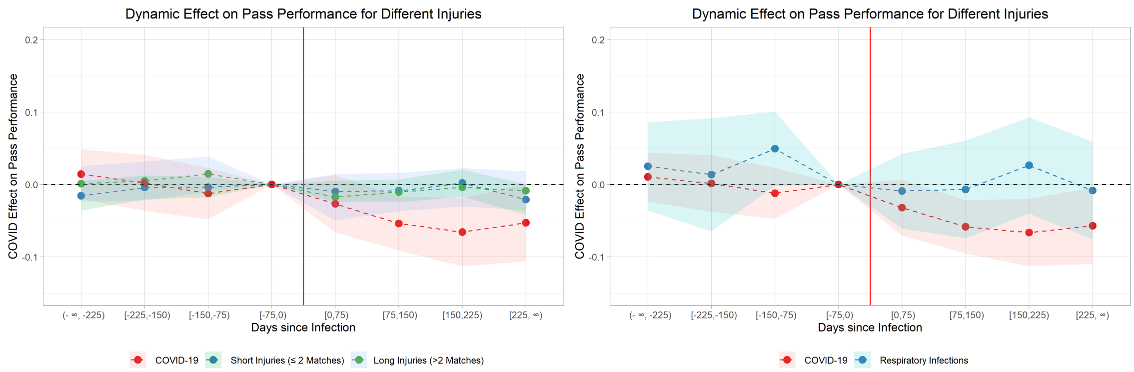
Figure 5.9: Effect Heterogeneity: Severity of an Infection

matchdays) to investigate heterogeneity in injury length. Unlike for COVID-19, we find no comparable work performance effects for other injuries – neither after short nor long ones.

Moreover, we are able to exploit information on the exact type of injury in our sample. We specifically identify absences that are related to similar diseases and infections like COVID-19, such as colds, influenza, and respiratory ailments.[19] This gives us some 100 occasions where players are absent owing to such reasons. The right plot of Figure 5.10 provides the comparison between the COVID-19 repercussions and the respective effect of other respiratory infections over the course of time. Again, it is evident that the COVID-19 infection causes more severe productivity effects than sicknesses affecting similar parts of the organism.[20] This corresponds to

[19]Note that COVID-19 infections are sometimes free of any symptoms, while the other respiratory infections are likely to be symptomatic as there has not been any testing.

[20]To test for such differences for the long-run effect, we conduct Wald tests on the pooled last two bins in each event study regression. As there can be multiple injuries per players, so that the treatment is non-absorbing, we cannot compare simple difference-in-differences coefficients. For the differences between a COVID-19 infection and short and long injuries (as shown on the LHS of Fig. 5.10), the pooled estimates of the last two bins differ significantly from each other for COVID-19 infections in comparison to short injuries ($p = 0.031$) and to long injuries ($p = 0.039$). On the



The plots give the time-specific COVID-19 or injury effects on the number of passes on the player and match level estimated by OLS. SEs: Heteroskedasticity-robust and clustered at the player level. We take the first match missed by an injured player as the starting date of the injury. The regression set-up follows equation (5.2). Under 'respiratory injuries' we subsume colds, influenza infections, pneumonia, and bronchitis (data from `www.transfermarkt.de`). We report 95% confidence bands in both plots.

Figure 5.10: Time-Specific COVID-19 and other Injuries' Effects on Performance

earlier research of Keech et al. (1998), who find a work performance deterioration for influenza-like sicknesses after returning to the workplace only over 3.5 days on average.

Eventually, our findings also support the external validity of our setting. There are no effects among professional soccer players subsequent to an 'ordinary' respiratory infection and hardly any among the general labor force. Hence, the singularity of professional athletes does not immediately imply differences in productivity outcomes. In turn, the significant deterioration following a COVID-19 infection may not reflect an overstatement of the effect in the general population due to players' stronger dependence on their respiratory system.

5.4.5 Within-Match Mechanism

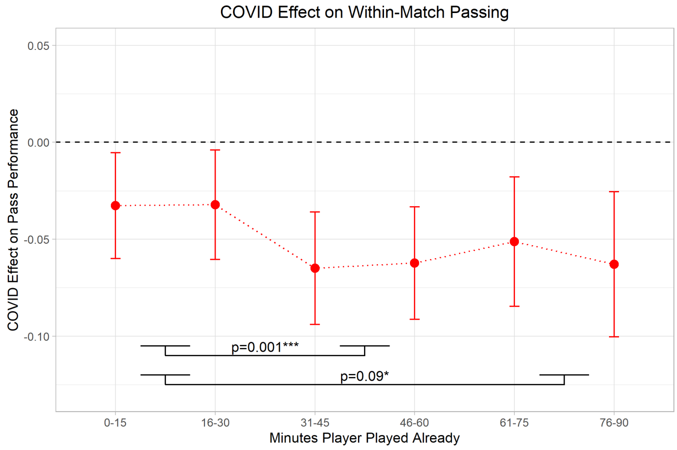
We can identify substantial and persistent effects of a COVID-19 infection on player performance. Our granular data allow us to study not only outcomes by matchday

RHS of Fig. 5.10, the long-term difference between the effect of COVID-19 and other respiratory infections is significant with $p = 0.075$.

but also performance within a match. As COVID-19 is a respiratory infection and soccer requires endurance in physical activity, it may be likely that players perform worse in the later stages of a match. We investigate this in Figure 5.11, in which we plot time-specific COVID-19 effects by decomposing the match of a player into a maximum of six parts of 15 minutes in length each.

The results show decreased physical work performance from the first minute on the pitch onward in cases in which a player has recovered from an infection. Furthermore, performance declines throughout a match. While the effect seems to be stable at around -3 percent in the first 30 minutes, post-infected players face a deterioration of some additional 3 percentage points in later phases. This deterioration is statistically significant on the 1% level for the second 30 minutes relative to the first 30 minutes played and significant on the 10% level for the last 30 minutes relative to the first thirty minutes played by an individual previously infected. Such a downward trend would be in line with COVID-19 affecting the player's endurance. Note that Figure 5.11 shows relative time. Hence, especially the first two bins capturing match time up to 30 minutes also encompass players that have been substituted *on* the pitch in the second half of a game. Even though they play for a shorter length of time and know this in advance, i.e., they do not need to manage their physical energy to last the full 90 minutes, their performance is lower compared to their non-infected peers.

Again, this emphasizes that we are likely to estimate a lower threshold of the treatment effect in absolute terms and that a COVID-19 infection causes a non-negligible deterioration in performance. Additionally, it addresses the external validity of this study, as fewer minutes played might better correspond to 'real world' occupations. Also, players who perform worse during later parts of a match might be substituted off earlier, so that their negative contribution at the end of matches might not be observable. Hence, the extensive margin effects might hide an even steeper downward trend throughout the match.



The plots show the time-specific COVID-19 effects on ln(passes). The x-axis shows the number of minutes a player has already been on the field. The y-axis documents the effect on the outcome variable. Standard errors are heteroskedasticity-robust and clustered at the player level. The 95% confidence bands are given. The regression setup is very similar to (5.1) estimated via OLS except for additional interactions of the COVID-19 term with the 15-minute time slots, which also results in up to six observations per player and match (for each time category if on the field) instead of one aggregate observation. p-values: Wald tests for difference between the respective estimates. The upper p-value compares the first 30 minutes (bins 1 and 2) with the second thirty minutes (bins 3 and 4). The lower p-values compares the first 30 minutes with the last 30 minutes (bins 5 and 6).

Figure 5.11: Time-Specific COVID-19 Effects on Within-Match Performance

5.4.6 Spillovers on Team Performance

Essential in team collaborations is the aggregate outcome of all individuals while the aggregate performance may differ from the sum of its components. A crucial question is whether the deteriorated productivity of post-infected players creates spillover effects on other players on the pitch. Hence, is a player's performance also affected by others' health shocks? We noted that treated players make fewer passes. Hence, teammates might be less involved in the match as well. Alternatively, they could compensate for the decreased performance of infected fellow players by taking more responsibility and being more involved in the match.

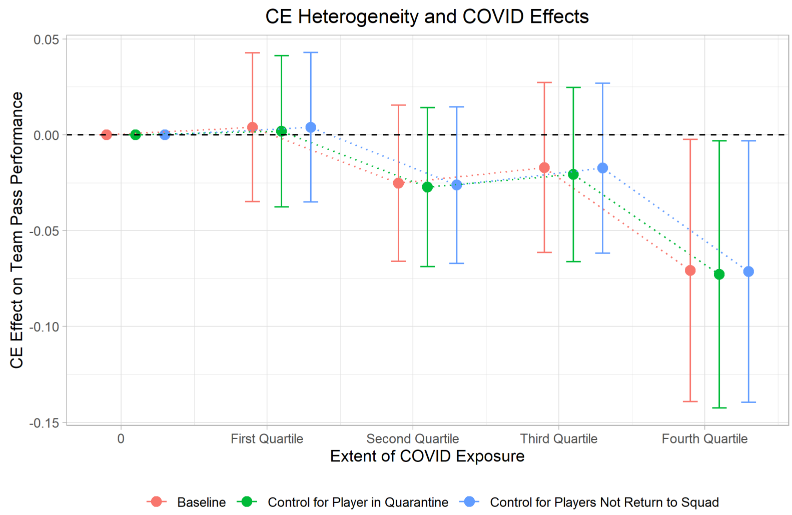
Stepping back, this issue directly corresponds to economic research on team production or else how a worker's effort affects her coworkers. To only name a few papers, Ichino and Falk (2005) find positive spillovers in a lab experiment, Mas and Moretti (2009) confirm the magnitude of this effect in a quasi-experimental setting with field data. They particularly investigate the arrival of highly productive coworkers. In contrast, Azoulay et al. (2010) as well as Waldinger (2011) study negative productivity spillovers. The former analyze the sudden deaths of academic 'superstars,' the latter investigates the forced expulsion of Jewish researchers in Nazi Germany. While Azoulay et al. (2010) find a significant negative effect in quality-adjusted publication rates, Waldinger (2011) does not. In the field of sports, Guryan et al. (2009) investigate the impact of a golf player's performance on the partner he plays with but do not find a sufficient impact. However, this is no team but rather rival performance. Our paper adds to this literature a large-scale natural experiment for workers that need to collaborate strongly in a high-stakes environment with large group sizes.

Technically, we address the spillover issue by analyzing a team's performance depending on its exposure to COVID-19 infections. More specifically, we proxy a team's exposure to COVID-19 by the number of players recovered from an infection as a share of the overall team size (at any point in time before a match) at the match level. We construct the variable 'COVID-19 Exposure' as:

$$CE_{tm} = \frac{\sum_{p \in t} \text{Post-Infection}_{pm}}{\#\text{Players}_{tm}} \,. \tag{5.3}$$

The numerator is the number of infected players of team t on matchday m. The denominator is the overall number of players of team t at match m, i.e., the squad size. Figure 5.28 in the appendix displays the distribution of the positive values of this variable. In almost half of the team-match observations, recovered players were involved.

Figure 5.12 displays the simple reduced-form effect of CE on the logarithmic cumulative pass performance of a team (red). We separate CE into four equal quartiles for $CE > 0$. The decline in performance is increasing but is only significant for the last quartile – $CE \in [0.241, 1]$, which encompasses an exposure of, on average,



The plot shows the effect of CE on team performance measured in ln(passes) estimated by OLS. We compare teams with $CE = 0$ to an exposure in four quartiles, which have the intervals $(0, 0.077)$, $[0.077, 0.130)$, $[0.130, 0.241)$, and $[0.241, 0.500]$ empirically or else $[0.241, 1]$ theoretically. The means are $\overline{CE}_{(0,0.077]} = 0.050$, $\overline{CE}_{(0.077,0.130]} = 0.096$, $\overline{CE}_{(0.130,0.241]} = 0.191$, and $\overline{CE}_{(0.241,1]} = 0.352$. The red bars capture the baseline CE effect on team performance, the green bars additionally control for #players currently in quarantine, the blue bars control for #players not being part of the squad following a COVID-19 infection. All regressions includes controls for home/away matches, ghost matches, the opponent's COVID exposure (transformed by the inverse hyperbolic sine transformation) and team-season FE, opponent-season FE and matchday FE.

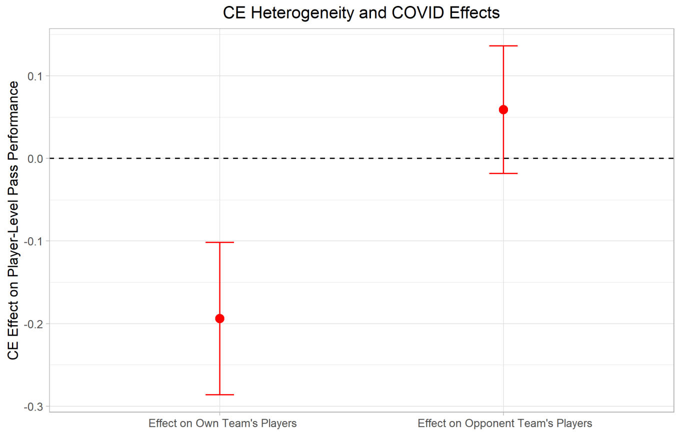
Figure 5.12: Effects of COVID-19 Exposure (CE) on Team Performance

35.2 percent recovered players (out of an average team size of 26.58 players). Hence, one additional infection does not have a constant marginal effect. This could be relevant for other industries relying on collaboration in team tasks, too. Research on the direct health effects of COVID-19 typically does not consider such indirect mechanisms. Observable deterioration in team performance in the largest quartile corresponds to the finding that performance losses due to sickness absenteeism of employees exceed their wages (Pauly et al., 2002; Zhang et al., 2017).

Other than in that research, our treated individuals are not necessarily absent but are often on the pitch. In the blue and green case, we also control for different measures of how many players missed a match due to quarantining. This should isolate the spillover effects of infected players on the pitch from pure composition effects due to missing quarantining players. If anything, the green and blue-colored estimates in Figure 5.12 highlight that the negative team spillovers are most likely related to productivity decline, when infected players are on the pitch, and are not caused by a composition effect. The latter captures a potential decline caused by the pure absence of players and their replacement with weaker substitutes. As we control for the number of players currently in quarantine (green) and not being part of the squad following a COVID-19 infection, one can see that the estimates hardly change. Overall, it might be the case that a team can compensate for small declines in their team members' contributions but not for larger ones. The deterioration in performance for a $CE \in [0.241, 1]$ amounts to 7.08 percent, while the mean exposure in this interval is only roughly one-third. This is strong suggestive evidence of spillover effects well beyond the individual effect.

Our variable definition allows us to capture the direct effect of weaker performance *on* the pitch as well as performance deterioration due to missing players because they have been hit severely by the infection. Hence, CE captures both extensive and intensive effects. As we argue that non-infected players perform worse due to their under-performing teammates, we have an affected control group, which confounds the estimates of a difference-in-differences setup. However, this only implies that our results on individual effects should be interpreted as lower bounds as there might be performance drops related to the treatment in the control group as well. Interestingly, previous research on performance deterioration on the soccer pitch due to external influences did not find such spillover effects (Lichter et al., 2017). These effects might be unique to COVID-19.

Figure 5.13 reports the elasticity of an increase of the own team's COVID exposure to own player and opponent player performance. Individual-level productivity is negatively affected by the own team's exposure while opponents' performance is not significantly changed. An increase of about 4 percent COVID exposure (corresponds roughly to one additional infection in an average squad) again reduces player performance by about 0.8 percent. The individual-level COVID effects of infected players remain unaffected by the inclusion of the exposure measures in the individual-level data.



These figures plot the OLS coefficients β_τ of the simple difference-in-differences regression following Equation (5.1). The 95% confidence intervals are given. The dependent variables are ln(passes). The logarithmic specification excludes observations with zero passes. The independent variables added to the standard model used above is the hyperbolic sine transformation of the players' team COVID exposure.

Figure 5.13: Basic Effect on Team-Level Performance

The results over the course of a match for individual players displayed beforehand in Figure 5.11 hold for the aggregate team performance as well. Figure 5.29 in the appendix provides estimates of the time-specific effect of higher exposure to COVID-19 infections within a team on pass performance. We again find that the effect of more post-infected players on the field especially arises in later stages of the game, even though the simple semi-elasticity for the own team is only significant at the 10% level. The marginal effect of -0.199 describes a hypothetical change of $\Delta CE = 1$ and corresponds to the basic effect in Figure 5.13. Overall, Figure 5.29 also provides no evidence of relevant spillover effects on the performance of the opponent team. Jointly with the basic difference-in-differences results that confirms spillover effects to only occur within a team. As we find a negative effect of COVID-19 across teams, this is additional evidence of the individual-level findings since the SUTVA applies at the team level.

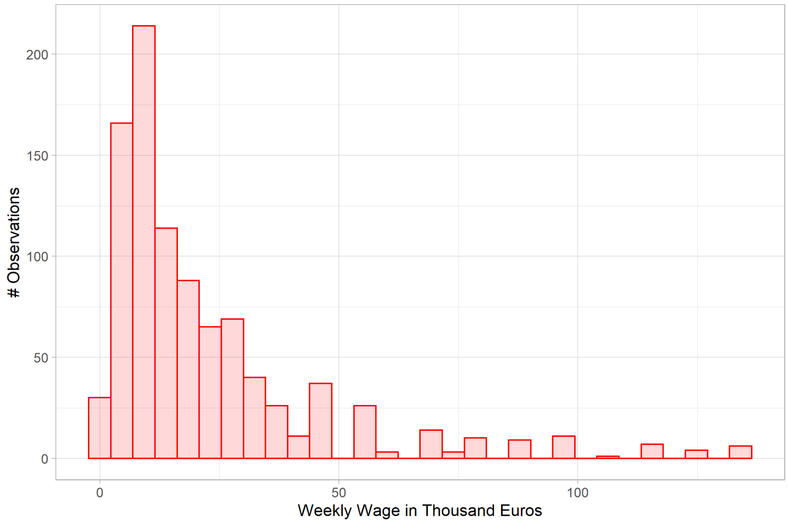
5.4.7 Relevance of Productivity Measures

While our performance measures such as the time on the pitch and the number of passes are highly relevant for soccer players, it is less clear to which extent a change in these measures corresponds to more general labor market outcomes. The most prominent criterion is wages. On the one hand, they reflect worker productivity. On the other hand, they essentially determine living standards. We subsequently provide evidence that the number of passes (intensive margin), as well as the likelihood to play (extensive margin), correlate with players' wages.

To do this, we use wage data of Serie A players at the player-season level from the 'La Gazzetta dello Sport', Italy's most prominent sports newspaper. We assemble annual wages for 78% of all player-season observations in the Italian league. The relationship between wages and passes per minute or the likelihood to play is positive and statistically significant conditional on player demographics, player-specific measures such as his position, or the share of matches a player was part of the starting line-up. Figure 5.26 in the appendix visualizes these correlations.[21] A 5.1% decrease in the pass performance (as in our baseline results) translates into around a 10% decrease in wages across teams and a 2% decrease in wages within a team (including team fixed effects). To address potential non-linearities in the wages of soccer players, we show that the conditional correlations also hold for a nonlinear specification.

Given the correlation between our main productivity measures and players' wages, we investigate how the decline in performance would translate into monetary terms. As wages are typically rigid, this is a rather hypothetical exercise, but it helps to quantify the effect accordingly. Figure 5.14 shows the distribution of weekly wages for the Italian Serie A. The median *weekly* wage is EUR 15,384.60 but wages are widely dispersed – the 25th and 75th percentiles are EUR 7,692.30 and

[21] We can additionally show that the conditional correlation remains significant after including team fixed effects (as shown in Fig. 5.27). Thus, passes per minute continue to be a significant predictor of wages even within a team.



The plot shows the logarithmic weekly wages of players per season in the Italian Serie A as reported by the La Gazzetta dello Sport up to the 99% percentile.

Figure 5.14: Weekly Wages of Serie A Soccer Players

EUR 28,846.20, respectively. At the median, a downgrading of 1.6 percentiles in the productivity ranking as in Figure 5.6 approximately corresponds to a 6.25 percent decrease in the actual wage. This is in the range of our estimates from the partial correlation analysis above and would relate to a weekly loss of EUR 961.54 or EUR 50,000 per annum.

5.4.8 Generalization of Results

A concern regarding our novel findings is that elite soccer players may differ from the general population. They are younger and fitter than the average worker, however, they work in a physically demanding occupation. While the former might imply more severe productivity effects of an infection for the average – less fit – individual, the latter might imply a milder effect. Therefore, assessing whether our findings constitute a lower or an upper bound for the population effect seems inaccurate. Yet, we carefully lay out why the existing differences may not have a sizable impact on observed workplace productivity.

Our paper studies long-run effects of COVID-19 infections after recovery. The prevalence of *post-acute* COVID-19 symptoms, as well as their development over time across age groups and fitness, thus, might be more relevant than differences between athletes and average workers during the actual infection. We focus on research that is not purely based on hospitalized patients, since these patients are a very selective sample of mostly elderly people who had left the workforce (Halpin et al., 2021; Huang et al., 2021; Nalbandian et al., 2021; Evans et al., 2022). Research particularly conducted among athletes reveals that such well-trained individuals can also suffer from post-acute, persistent, non-cognitive symptoms (Brito et al., 2021; Hull et al., 2022; Ribeiro Lemes et al., 2022). More general studies on the long-run impact on non-hospitalized patients cannot identify a monotone relation between age and the prevalence of symptoms in a sample of symptomatic and asymptomatic cases (Whitaker et al., 2022). Similarly, Bliddal et al. (2021) and Tran et al. (2022) do not find a difference in the level and process of symptoms over time between individuals below and above 40 years. Blomberg et al. (2021) and Moreno-Pérez et al. (2021) do not find a robust relation between age, fitness, and persistent symptoms either. Thus, the evidence on non-hospitalized patients does not support that the prevalence of 'long COVID' robustly differs with age and fitness.

While not every job is as physically demanding as the occupation of soccer, there still exists a wide range of industries that rely on physical work as well. The construction sector alone employs millions of workers, 1.8 million in Germany, 1.3 million in Italy. The physically demanding sector of health and social work encompasses 4.9 million or 15 percent of all German jobs.[22] Despite technological advances, these sectors continue to be vastly fueled by the physical labor input of their employees that cannot be easily substituted by machines.

We carefully conclude that professional soccer players are not as different from the average population regarding COVID-19 infections and their consequences as one might expect. If anything, the consequences of a COVID-19 infection may be

[22]See Statistisches Bundesamt (Destatis) (GENESIS database #13111-0003 2021); Italian National Institute of Statistics/Istituto Nazionale di Statistica (Istat) (2021).

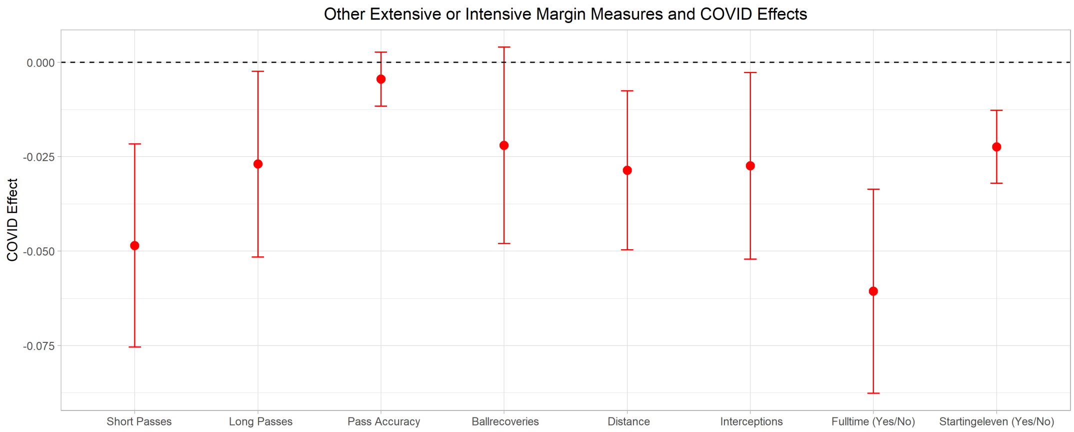
similar – in qualitative terms – for soccer players and the average worker. One should keep in mind that our sample differs from most medical papers as we observe all *asymptomatic* infections. Given that the level of acute infections impacts post-acute symptoms (Peter et al., 2022; Whitaker et al., 2022), we estimate a real population effect for our sample which differs from results based on samples with unobserved infections or selection on symptoms.

5.4.9 Robustness Checks

We find similar results for several other performance measures at both the extensive and intensive margin. Figure 5.15 reports significant performance drops also for measures like running distance or the number of interceptions and reveals that performance effects are neither purely driven by short or long passes. Moreover, clear effects on the likelihood to play full-time or be part of the starting line-up are evident.

Moreover, even though passes appear to be a reasonable and strong measure for productivity, one might wonder about the effect of COVID-19 on goals as this is the ultimate purpose of all match effort. However, the average amount of goals scored per team and match in our dataset is $\bar{x}_{tm} = 1.54$. This value is not only low, other than in most industries, a larger supply of input factors or higher productivity does not necessarily translate directly into higher output, i.e., into scoring more goals. A more appropriate measure is the number of shots. Figure 5.30 in the appendix shows for the individual level that an infection leads to fewer shots for strikers and a higher COVID exposure leads to a significant decrease in shots at least for high values of CE. Insignificant effects for defenders and midfielders are not an objection to our results. If anything, these two types of players mainly try to serve the strikers with good passes to enable them to make shots from a promising position. Again, the number of passes is the ultimate driver of productivity and outcomes.

Our dependent variable is the logarithm of passes such that observations with



The plot shows the OLS estimates of the post-infection dummy included in the baseline regression (5.1) for different performance measures. Dep. variable: given on the x-axis of the plot (always in hyperbolic sine transformation form – partly due to a high number of zero observations - except for the dichotomous variables ‘full-time’ and ‘starting eleven’). SEs: Heteroskedasticity-robust and clustered at player level. Confidence bands of 95% are shown.

Figure 5.15: Robustness Check: Other Performance Measures

zero passes etc. are dropped. This relates to players who participated for just a few minutes of the match and hence make up around 0.5 percent of the observations. To demonstrate that our results do not depend on the functional form, in the appendix we provide results for a specification in levels (Figure 5.20) and for using the inverse hyperbolic sine transformation for the dependent variable (Figure 5.21).[23] Our results do not change. Also, note that our results are not driven by one specific league. Figure 5.22 presents extensive and intensive margin effects for both leagues separately. Next, we test whether performance changes are induced by adapted coaching and changing tactics by inserting detailed information about the main team formation at the match level. Formation in this context means how the squad on the pitch is ‘arranged’, i.e., how many players act as defenders, how many as midfielders and so on. In our data, we observe nineteen different formations. By replacing the team FE with an interacted team × formation FE, we take formation-specific performance patterns into account. As one can see in Figure 5.25, the static

[23]The hyperbolic sine transformation of x is $sinh(x) = ln(x + \sqrt{x^2 + 1})$ and approximates a logarithmic transformation of the variable in a way that zero values do not get lost.

semi-elasticity is again slightly lower but remains significantly negative. The event study pattern remains the same.

As additional robustness checks, we vary the vector of fixed effects (Z). First, we replace the matchday fixed effect with an augmented matchday×season fixed effect. The static semi-elasticity is slightly lower (-4.4% instead of -5.1%) but remains highly significant. Second, we replace the player fixed effect with an interacted player × position fixed effect, which also captures the respective position of a player (goalkeeper, defender, midfielder, striker). Again, the static semi-elasticity is slightly lower (-4.5% instead of -5.1%) as the augmented fixed effect captures a bit more variation but is still highly significant. However, the event study estimates are highly similar to those from the baseline regression in Figure 5.5 as one can see in the plots provided in Figure 5.25 in the appendix.

5.5 Conclusion

This paper analyzes the causal effect of a COVID-19 infection on the productivity of high-performance workers, utilizing a uniquely granular panel data set of elite soccer players. We are the first to quantify COVID-19-related productivity effects at the individual level and find a persistent deterioration of about 5 percent. This does not diminish swiftly but remains prevalent over months. As hundreds of millions were infected around the globe, this is not a problem for a handful of people but is likely to accumulate to an effect size that could be felt by the economy in total. Additionally, we find some mutually reinforcing effects among groups.

This is a novel and thought-provoking result as our findings correspond directly to recent policy debates. A ‘zero COVID’ strategy that aims for complete elimination of the virus in a country or region has been suggested, for example, by Aghion et al. (2021). Bianchi et al. (2020) and Helliwell et al. (2021) highlight the indirect long-run effects of lockdowns on unemployment and health outcomes. Particularly, the latter group finds that rigid NPI strategies leading to zero transmission rates have had superior outcomes in more dimensions than just case rates and mortality. We relate our research to this debate with direct effects.

We are confident that our findings are fairly robust and generalizable. Still, we are aware that our analysis has limitations. That professional soccer players are only a subsample of society has already been discussed. Even though it is ambiguous whether the effects for the 'average' individual might be even worse, it would be helpful if our analyses were more diverse. Future research should, therefore, address gender, a broader age range, and various job profiles. Also, we are not capable of distinguishing the different variants of the virus or the severity of symptoms, since we are fully reliant on test results, which is a dichotomous outcome. Additionally, our results are based on non-vaccinated people. Even though our event study methodology encompasses a fairly long time horizon, for obvious reasons we cannot account for the effects over several years. It would be interesting to re-examine this setting in a few years.

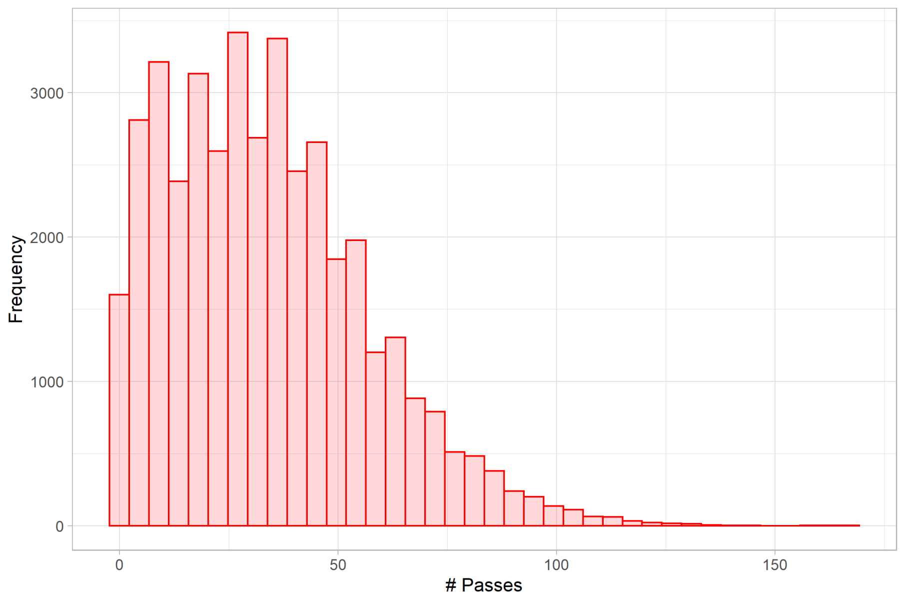
Eventually, our back-of-the-envelope computations for wages of the median worker in Western countries suggest that there may be non-negligible long run costs for individuals infected with COVID-19. Even though these are preliminary considerations, COVID-19 may become not only endemic in a epidemiological sense but also in economic terms.

5.6 Appendix

Table 5.2: Descriptive Statistics on Players and Matches

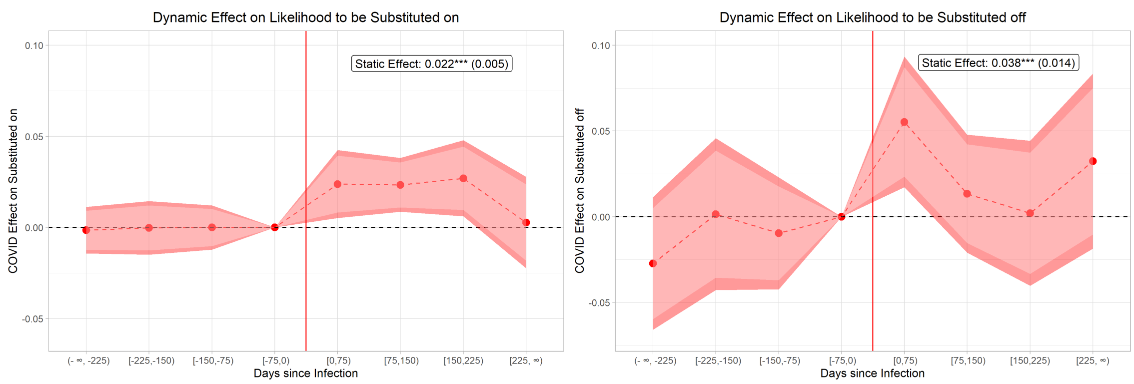
	N	Mean	St. Dev.	Min	Max
Treatment Indication					
Infected Player	72,938	0.195	0.396	0	1
Post-Infection	72,938	0.086	0.280	0	1
Player Characteristics					
Height (in cm)	72,938	183.502	6.139	163	202
Weight (in kg)	72,938	77.322	6.460	58	101
Age	72,938	26.596	4.683	15	43
COVID Game (Yes/No)	72,938	0.659	0.474	0	1
Matchday	72,938	18.592	10.515	1	38
Home (Yes/No)	72,938	0.500	0.500	0	1
Extensive Margin: Player Involvement					
Played (Yes/No)	72,938	0.557	0.497	0	1
Injured (Yes/No)	68,577	0.126	0.332	0	1
Suspended (Yes/No)	68,577	0.024	0.154	0	1
Substituted Off (Yes/No)	40,607	0.257	0.437	0	1
Substituted On (Yes/No)	40,607	0.257	0.437	0	1
Played Full-time (Yes/No)	40,607	0.488	0.500	0	1
Starting Eleven (Yes/No)	40,607	0.743	0.437	0	1
Minutes on Field	40,607	66.741	30.122	0	90
Intensive Margin: Player Performance					
1. General Measures					
Passes	40,607	33.884	22.946	0	167
Passes (Successful)	40,607	26.976	20.313	0	165
Short Passes	40,607	24.212	18.532	0	158
Short Passes (Successful)	40,607	19.624	16.629	0	157
Long Passes	40,607	9.672	6.867	0	50
Long Passes (Successful)	40,607	7.351	5.617	0	41
Distance Covered	40,607	1,175.5	642.9	0	3,878.6
Possession	40,607	32.541	22.152	0	167
Touches	40,607	43.916	26.027	0	177
Aerials	40,607	2.124	2.495	0	28
Aerials (Successful)	40,607	0.022	0.155	0	3
2. Defensive Measures					
Ball Recoveries	40,607	3.581	2.915	0	23
Defensive Aerials	40,607	1.063	1.543	0	17
3. Offensive Measures					
Shots	40,607	0.888	1.290	0	14
Shots (on Target)	40,607	0.311	0.654	0	7
Offensive Aerials	40,607	1.062	1.735	0	26

Figure 5.16: Distribution of #passes per match



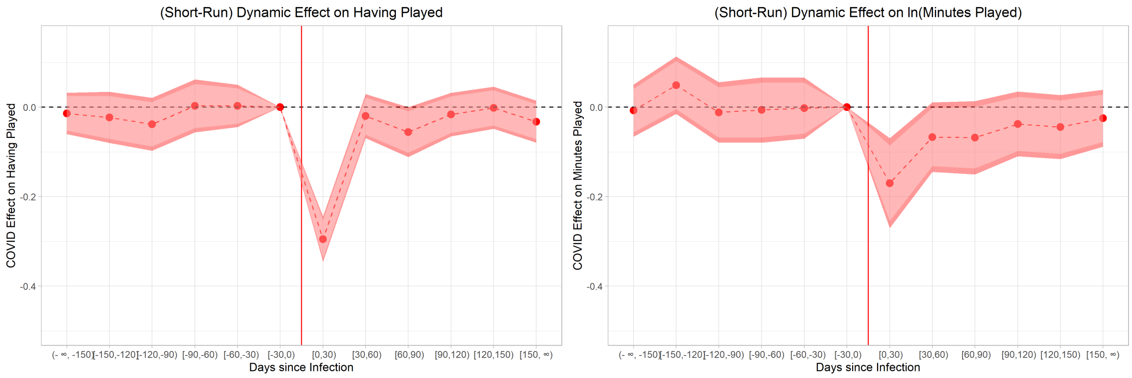
This figure plots the absolute frequency of passes played by a player during his individual time on the pitch in a particular match. Mean = 33.88, median = 31, first quartile = 16, 3rd quartile = 48,
minimum = 0, maximum = 167.

Figure 5.17: Dynamic Effect on On and Off Substitutions



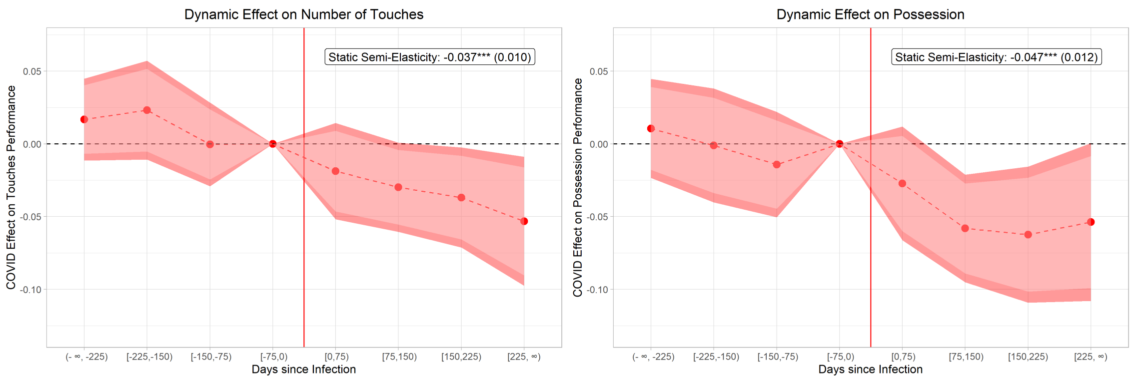
These figures plot the OLS (linear probability model) estimated coefficients β_τ of the event study regressions following Equation (5.2). The reference time period is one to 75 days before treatment. Standard errors are heteroskedasticity-robust and clustered at the player level. The 90 and 95% confidence intervals are given by the two red-shaded areas. The dependent variables is a binary variable indicating to be substituted on (LHS) or off the field (RHS).

Figure 5.18: Dynamic Effect on the Likelihood to Play and Minutes Played: Smaller Bin Size



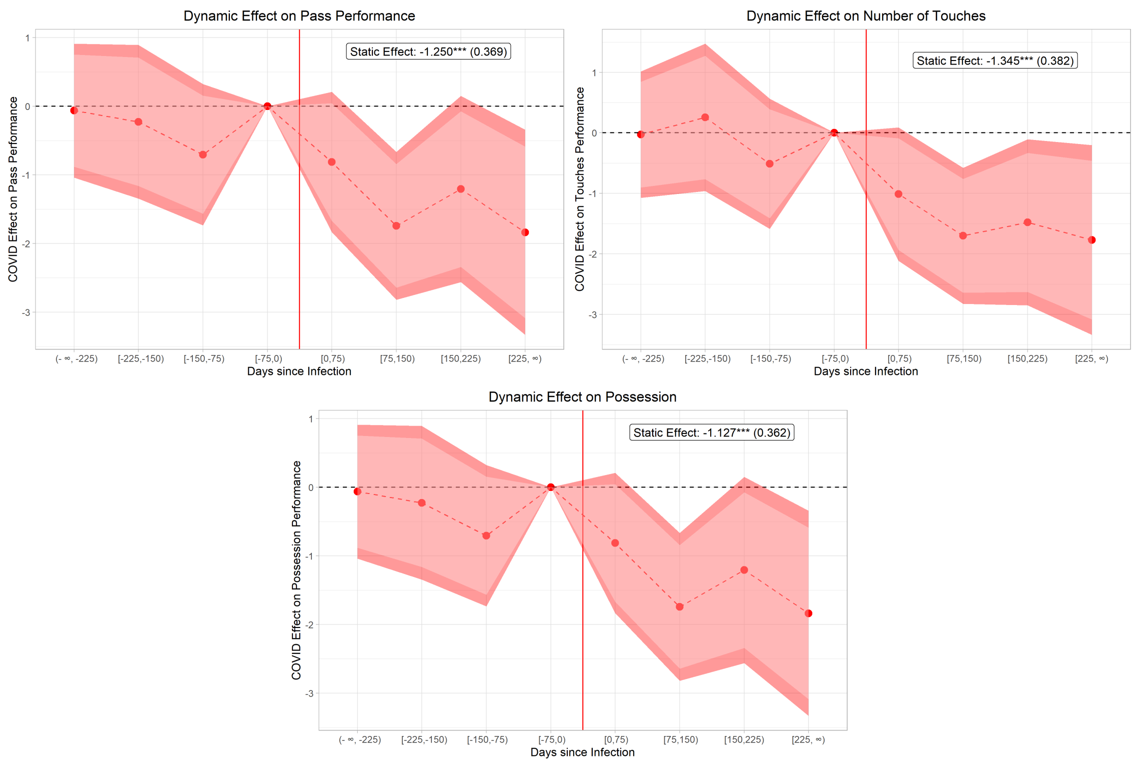
This figure plots the OLS estimated coefficients β_τ of the event study regression following Equation (5.2). The bin size is now 30 days, i.e. one month. The reference time period is one to 30 days before treatment. Standard errors are heteroskedasticity-robust and clustered at the player level. The 90 and 95% confidence intervals are given by the red-shaded areas. As the bin size is much smaller compared to the baseline setting in figures 5.3 and 5.4, this affects the confidence bands. Due to much fewer observations within one bin, we severely lose statistical power, which leads to mostly insignificant results at a 5% significance level. Dependent variable LHS: A dummy indicating whether a player played or not. Dependent variable RHS: ln(minutes played) conditional on having played.

Figure 5.19: Dynamic Effect on Within-Match Performance: Additional Work Performance Measures



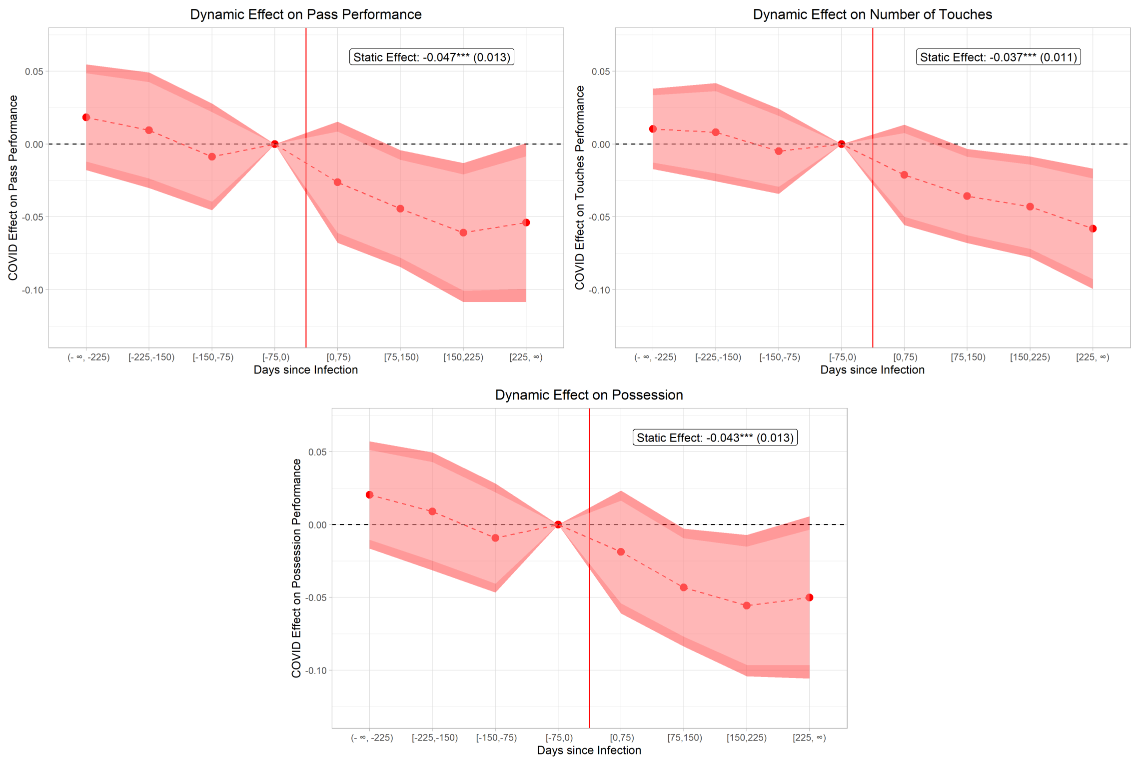
These figures plot the OLS coefficients β_τ of the event study regression following Equation (5.2). The reference time period is one to 75 days before treatment. Standard errors are heteroskedasticity-robust and clustered at the player level. The 90 and 95% confidence intervals are given by the two red-shaded areas. The dependent variables are ln(touches) as ln(possession) as additional work performance measures. The logarithmic specification excludes observations with zero touches or possessions. Robustness checks in figures 5.20 and 5.21 show that these results also hold for settings taking zero values into account.

Figure 5.20: Event Studies for Different Outcome Specifications: *Levels*



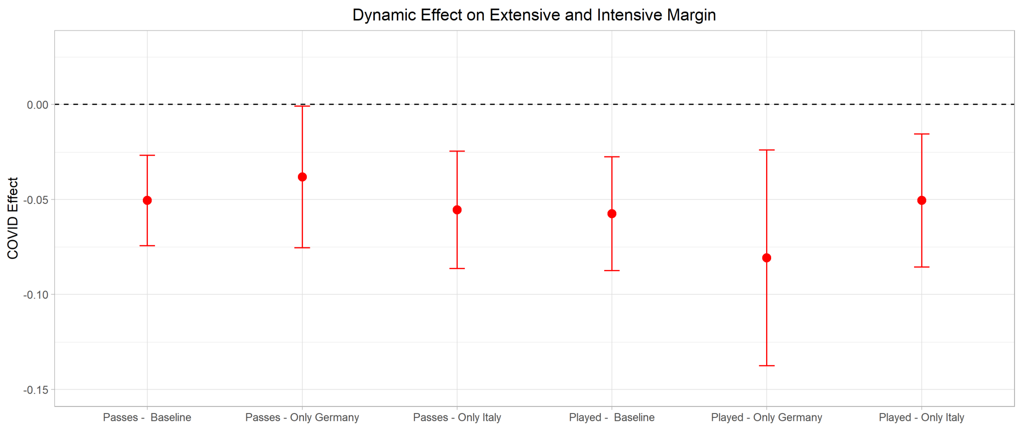
These figures plot the OLS estimated coefficients β_τ of the event study regression following Equation (5.2). The reference time period is one to 75 days before treatment. Standard errors are heteroskedasticity-robust and clustered at the player level. Outcomes winsorized at the 5 and 95% level. The 90 and 95% confidence intervals are given by the two red-shaded areas. The dependent variables are passes, touches and possession in their level specification.

Figure 5.21: Event Studies for Different Outcome Specifications: *Inverse Hyperbolic Sine Transformation*



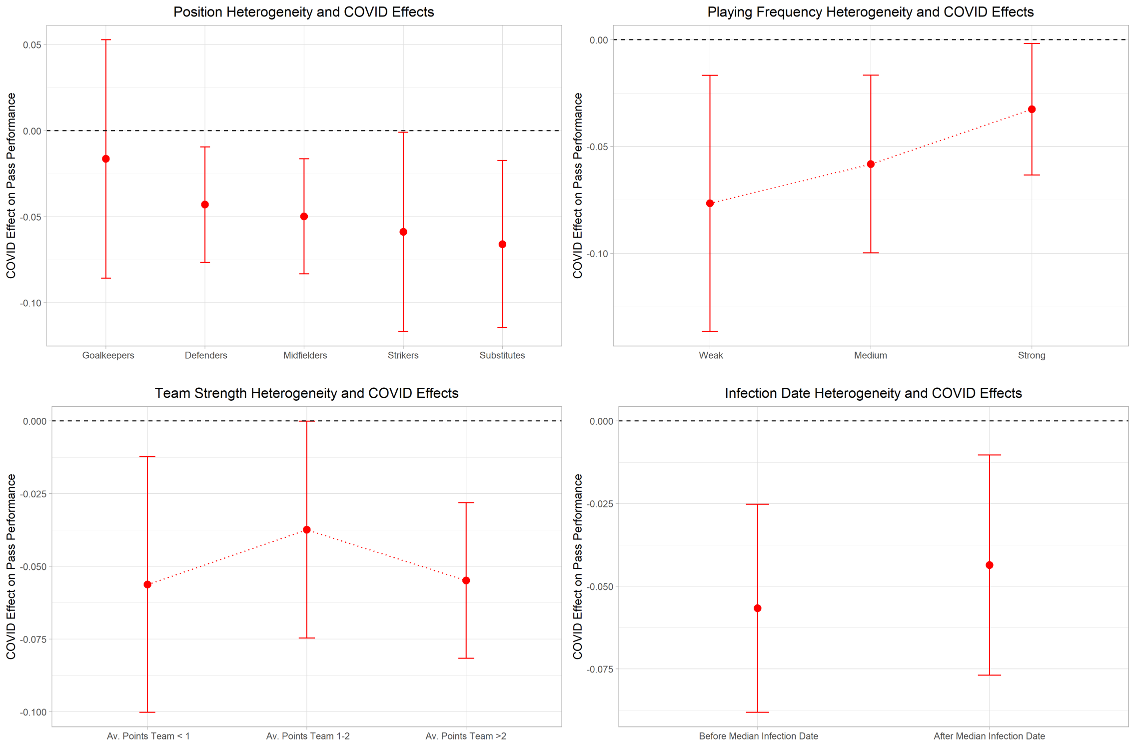
These figures plot the OLS estimated coefficients β_τ of the event study regression following Equation (5.2). The reference time period is one to 75 days before treatment. Standard errors are heteroskedasticity-robust and clustered at the player level. The 90 and 95% confidence intervals are given by the two red-shaded areas. The dependent variables are the variables passes, touches and possessions transformed via the inverse hyperbolic sine transformation to account for zero-values in the dependent variables. See for a critical assessment of this technique, e.g., Bellemare and Wichman (2020).

Figure 5.22: League-Specific Effects



The plot shows the effects of the post-infection dummy included in the baseline equation (5.1) for the extensive and intensive margin estimated by OLS. The x-axis gives more precise information on the choice of the control group. Dep. variable: ln(passes) and a dummy which takes the value 1 if a player has played. SEs: Heteroskedasticity-robust and clustered at the player level. The 95% confidence intervals given.

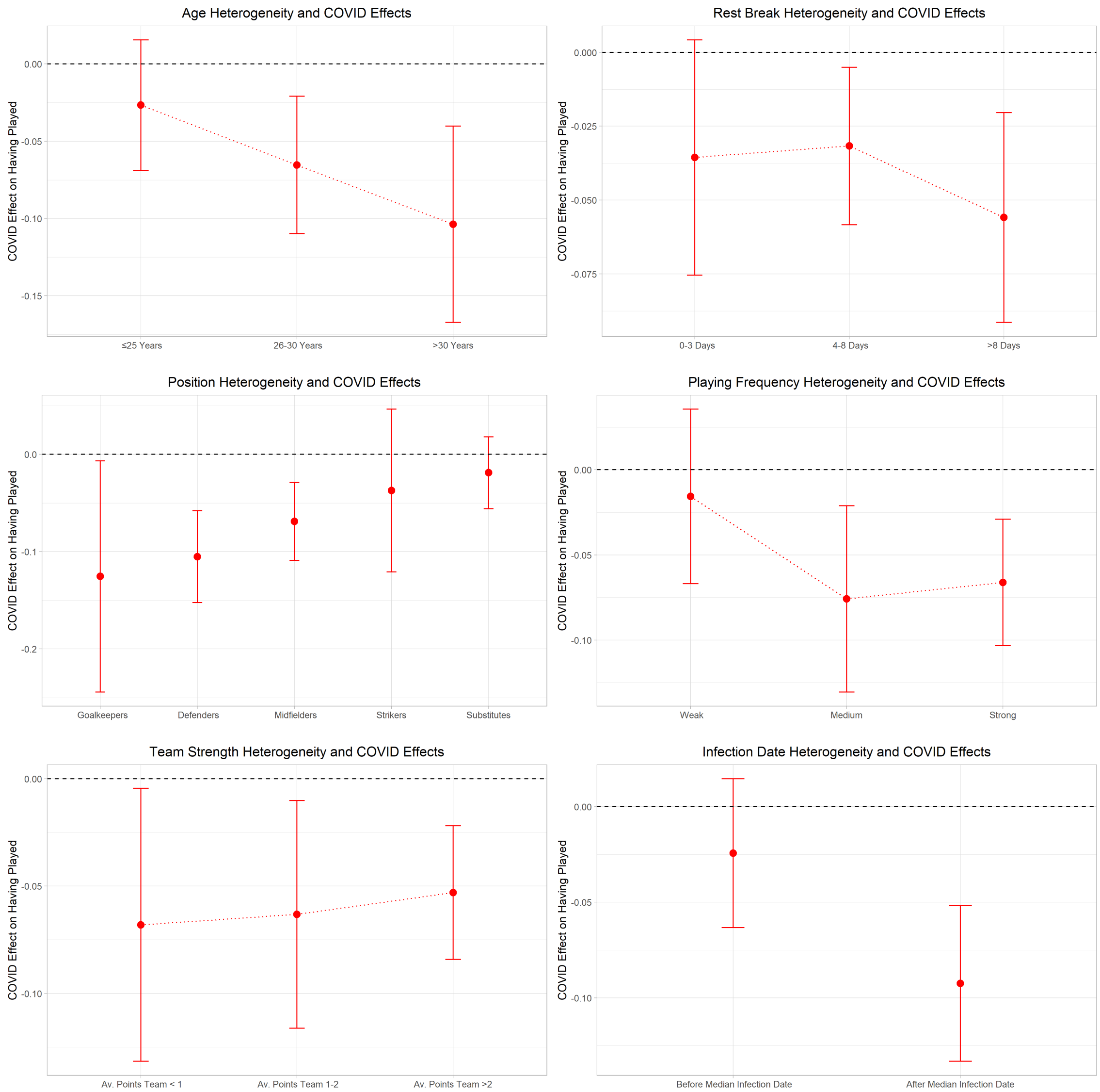
Figure 5.23: Additional Heterogeneity Analyses for Intensive Margin Effects: Position on the Pitch, Playing Frequency, Team Strength, and Infection Timing



These figures plot the OLS estimated heterogeneous semi-elasticity of a COVID-19 infection on pass performance. Standard errors are heteroskedasticity-robust and clustered at the player level. The 95% confidence bands are displayed.

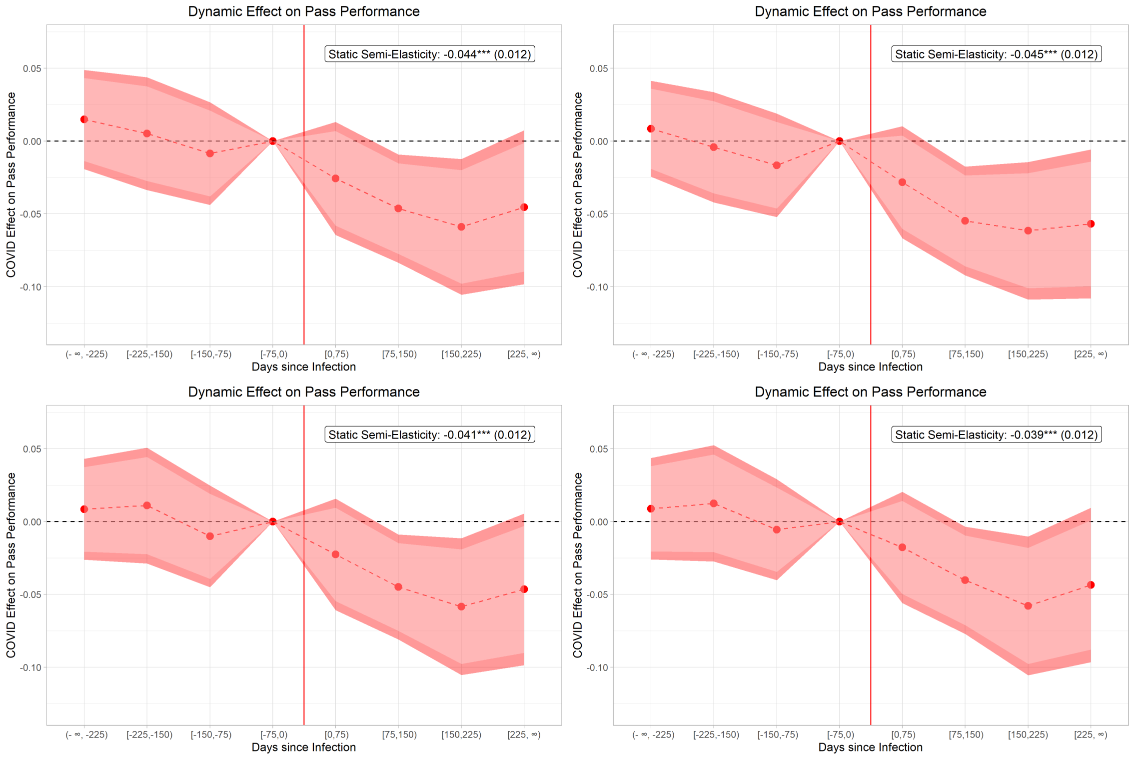
- *Position* addresses the effects on different types of positions a player might have on the pitch. 'Substitutes' captures all players that did not play from the beginning but have been substituted on the pitch during a match.
- *Playing Frequency* addresses differences in a player's quality and significance for his team. To capture this, we calculate the share of available matches a player played in before his infection took place and construct three groups for different terciles (from weak to strong) of this match-share distribution.
- *Team Strength* is the equivalent calculation at the team level. Better teams might have better medical support available while also allowing recovering players to not take on full responsibility immediately. Contrary, above-average teams might perform on a level which is harder to come back to again. We test this relationship by looking at heterogeneous treatment effects for teams which earned a different number of points up to a certain match in a season. Teams can earn zero (defeat), one (draw), or three points (victory) per match, so we group them into clusters of low-performing (average points < 1), medium (average points $1 - 2$) and well-performing teams (average points > 2).
- *Infection Timing* tests whether early infected players show different work performance effects than players who got infected later during the pandemic. The plot at hand shows two groups of infected players which have been divided at the median infection date. One can see that the work performance effect is significant for both groups and not statistically different from each other.

Figure 5.24: Heterogeneity Analysis for Extensive Margin Effects



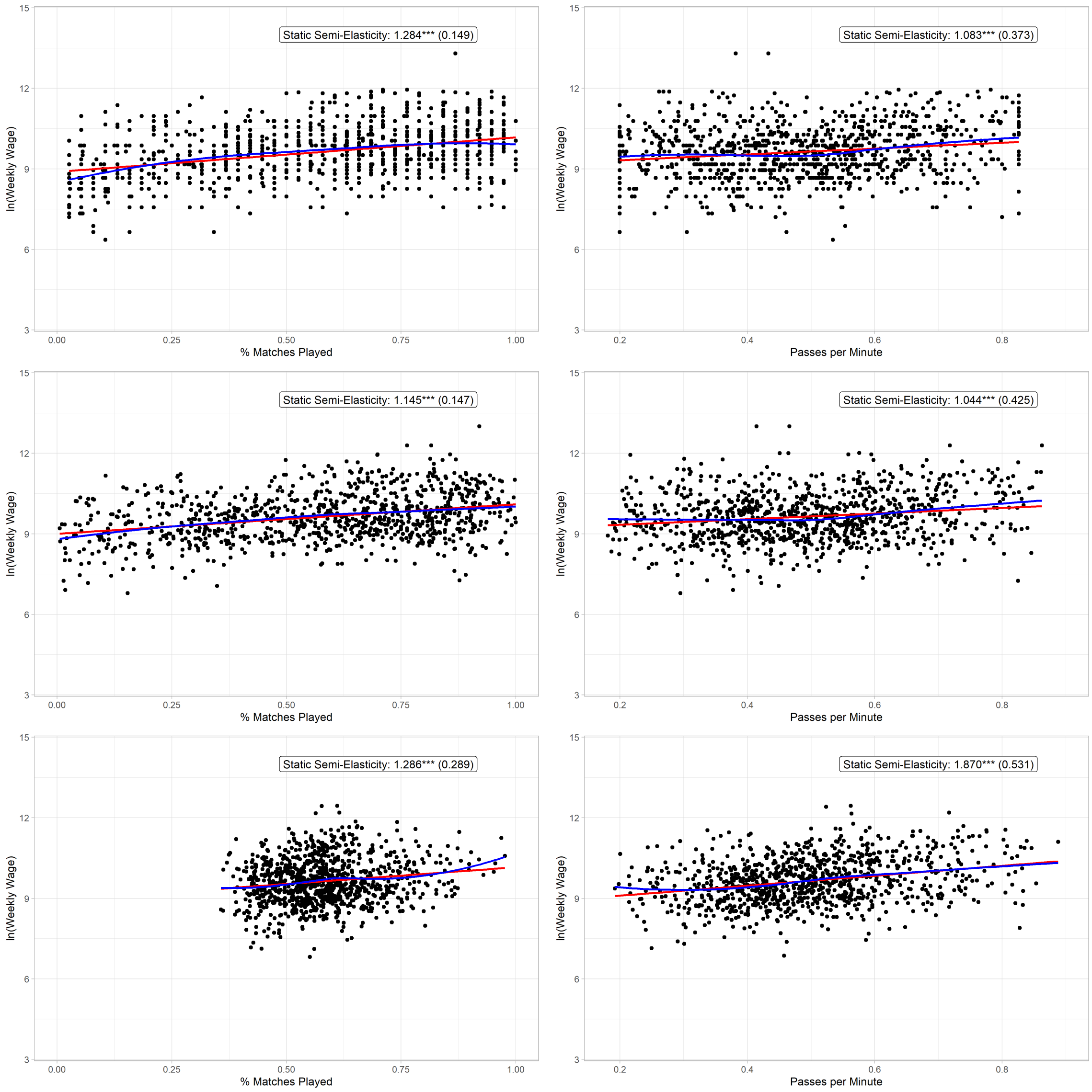
These figures plot the OLS (partly linear probability model) estimated heterogeneous semi-elasticity of a COVID-19 infection on pass performance. Standard errors are heteroskedasticity-robust and clustered at the player level.The 95% confidence bands are displayed.
Heterogeneity in Age and Rest Breaks correspond to the intensive margin effects shown in figures 5.7 and 5.8. Position Heterogeneity, Playing Frequency, Team Strength and Infection Timing correspond to the intensive margin effects shown in Figure 5.23 above. The difference in Infection Timing is driven by technical reasons as for late infections there exists much fewer observations after the infection happened compared to early infections, such that missed matches have a higher weight causing a significant estimate.

Figure 5.25: Dynamic Effect on Within-Match Work Performance using different sets of fixed effects.



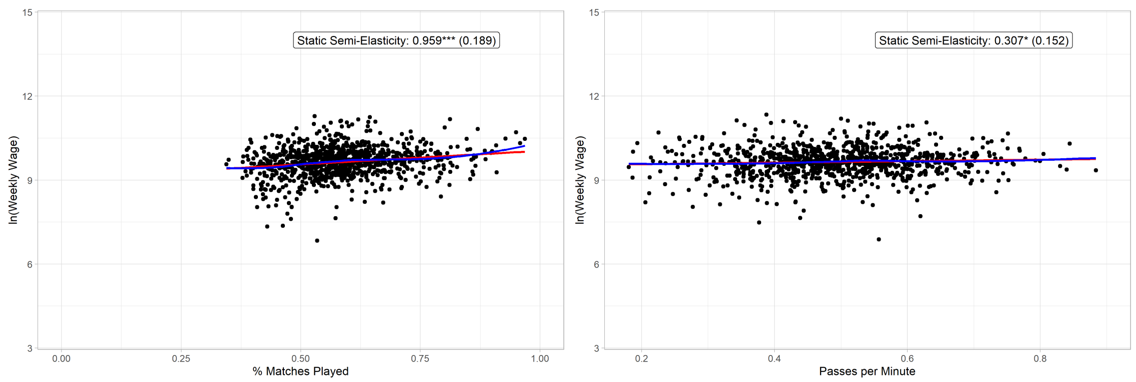
These figures plot the OLS coefficients β_τ of the event study regression following Equation (5.2). The reference time period is one to 75 days before treatment. Standard errors are heteroskedasticity-robust and clustered at the player level. The 90 and 95% confidence intervals are given by the two red-shaded areas. The dependent variable is ln(passes).
The plot on the upper LHS shows the event study results using a matchday × season FE instead of the matchday FE used in the baseline regression (shown in Fig. 5.5). The plot on the upper RHS shows an event study specification that uses a player × position FE instead of the player FE used in the baseline regression.
Both lower plots show a variation in the team FE. The plot on the lower LHS shows event study results using a team × formation FE instead of the plain team FE. The lower RHS shows results using not only the team × formation FE applied on the LHS, but also an opponent × formation FE.

Figure 5.26: Partial Correlation Analysis



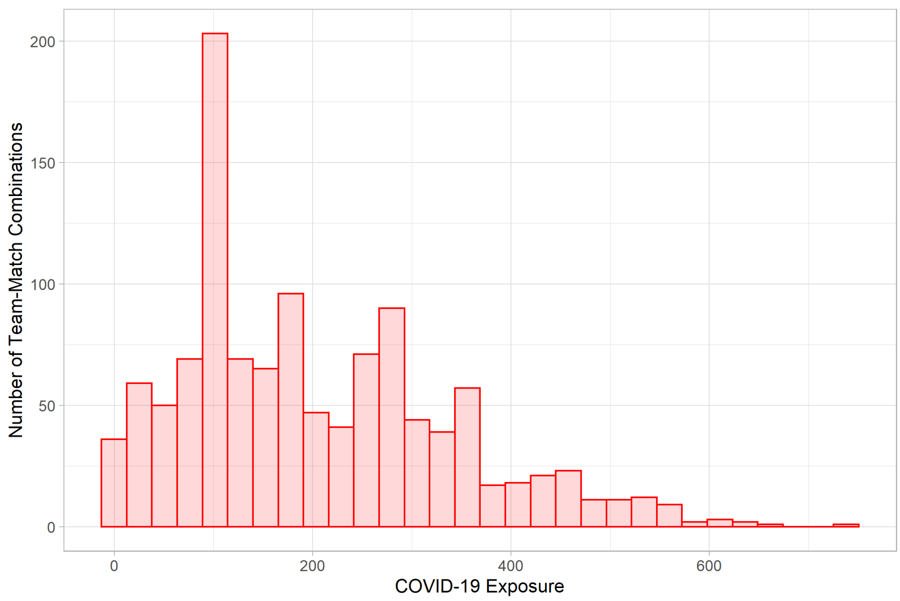
The left column plots the correlation between wages and the share of matches played, the right column plots the correlation between wages and passes/min. Top row: Pure correlation, middle row: correlation controlled for age, weight, and height. Bottom row: additional controls for position, share of starting eleven, share of fulltime matches. The variable passes/min is winsorized at 2.5 and 97.5% to correct for outliers. Standard errors are heteroskedasticity-robust and clustered at the team level. Data from the La Gazzetta dello Sport. We report a linear regression fit (red) and a fit from a local polynomial estimator (blue). *p<0.1; **p<0.05; ***p<0.01.

Figure 5.27: Partial Correlation Analysis Including Team Fixed Effects



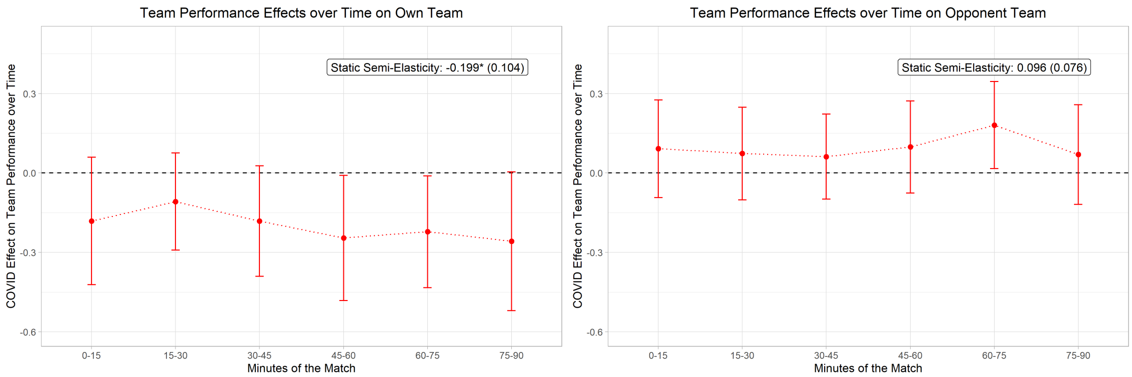
The left column plots the correlation between wages and the share of matches played, the right column plots the correlation between wages and passes/min. Different to Fig. 5.26, we additionally include team fixed effects. *p<0.1; **p<0.05; ***p<0.01.

Figure 5.28: Distribution of COVID-19 Exposure (CE) for $CE > 0$



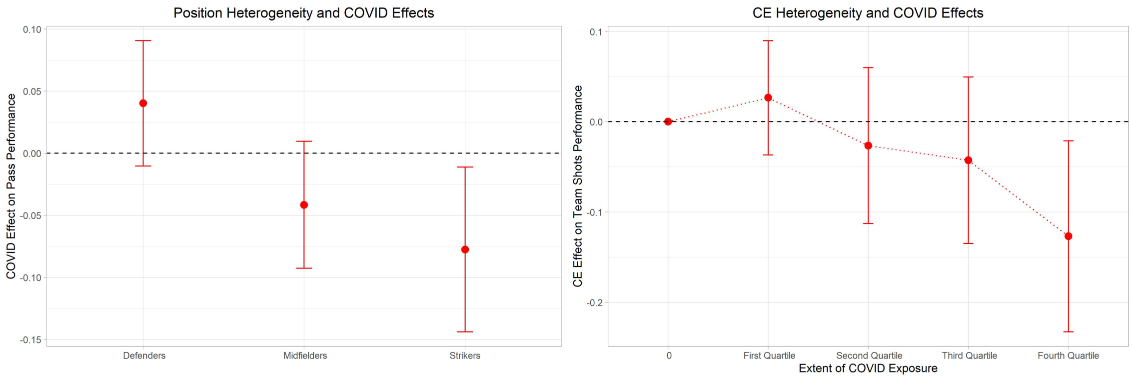
This figure plots the absolute frequency of COVID-19 Exposure (CE) realizations for $CE > 0$ – as defined in eq. (5.3) – observed in the team data.

Figure 5.29: Effects on Within-Match Team Performance (Own vs. Opponent Team)



The plots show the time-specific COVID-19 effects on ln(Passes) on team and match level of CE on own (LHS) and opponent team (RHS) performance estimated by OLS. SEs: Heteroskedasticity-robust and clustered at the team level. The 95% confidence bands are given. The regression set-up is equivalent to (5.1) except for additional interactions of the COVID-19 term with 15-minute time slots, which results in up to six observations per player and match. In contrast to Fig. 5.11, the time slots capture overall match time and not the minutes a player has been on the field. The regression includes controls for home/away matches, ghost matches and team-season FE, opponent-season FE and matchday FE, and time category FEs.

Figure 5.30: Effect of COVID-19 on Shots (Individual and Team Level)



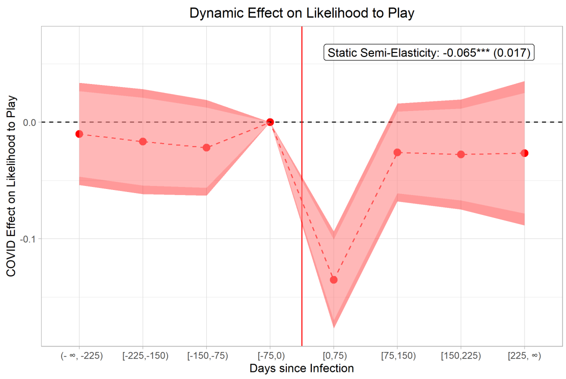
The plots show the effect of a COVID-19 infection on goals on the individual (LHS) and the team level (RHS).
The plot on the left displays OLS interaction effects between the post-infection dummy and age groups included in equation (5.1). Dependent variable: shots, transformed via the inverse hyperbolic sine transformation to account for zero-values. SEs: Heteroskedasticity-robust and clustered at player level. Goalkeepers are omitted due to the position-specific importance of shots.The plot on the right shows the effect of CE on team performance measured in the logarithm of shots estimated by OLS. We use the hyperbolic sine transformation due to zero shots observations at the team level. We compare teams with $CE = 0$ to an exposure in four quartiles, which have the intervals $(0, 0.077)$, $[0.077, 0.130)$, $[0.130, 0.241)$, and $[0.241, 0.500]$ empirically or else $[0.241, 1]$ theoretically. The means are $\overline{CE}_{(0,0.077]} = 0.050$, $\overline{CE}_{(0.077,0.130]} = 0.096$, $\overline{CE}_{(0.130,0.241]} = 0.191$, and $\overline{CE}_{(0.241,1]} = 0.352$. The regression includes controls for home/away matches, ghost matches, the opponent's COVID exposure (transformed by the inverse hyperbolic sine transformation) and team-season FE, opponent-season FE and matchday FE. In both plots, 95% confidence bands are given.

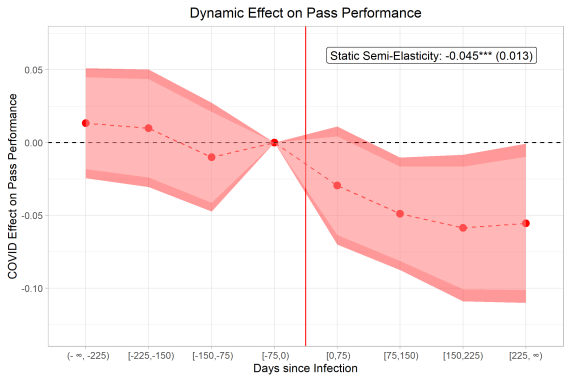
Table 5.3: Test of Balancing Condition

	Before Matching			After Matching		
	Treated	Non-Treated	p-value	Treated	Non-Treated	p-value
Propensity Score	0.034	0.003	0.000***	0.034	0.032	0.534
Recent Match Involvement						
Played	0.702	0.612	0.005***	0.702	0.746	0.321
Played×Fulltime	0.351	0.296	0.102	0.351	0.405	0.264
Played×Starting Squad	0.546	0.452	0.007***	0.546	0.590	0.371
Past Match Involvement						
Played	0.664	0.598	0.000***	0.664	0.643	0.444
Minutes if Played	67.073	61.798	0.000***	67.073	65.191	0.357
Fulltime if Played	0.514	0.450	0.005***	0.514	0.480	0.287
Distance/min	19.038	18.724	0.643	19.038	18.616	0.586
Passes/min	0.516	0.492	0.098*	0.516	0.513	0.884
Ballrecovery/min	0.052	0.056	0.030**	0.052	0.054	0.347
Possession/min	0.497	0.472	0.077*	0.497	0.494	0.879
Touches/min	0.676	0.657	0.182	0.676	0.680	0.857
Shots/min	0.015	0.014	0.556	0.015	0.013	0.309
Aerials/min	0.035	0.037	0.828	0.035	0.034	0.780
Demographics						
log(Height)	5.214	5.210	0.102	5.214	5.211	0.322
log(Weight)	4.344	4.344	0.955	4.344	4.334	0.259
log(Age)	3.273	3.284	0.359	3.273	3.253	0.214
Others						
1[NT COV19]	0.483	0.322	0.000***	0.483	0.463	0.693
1[NT COV19] ×ln(# Matches)	0.782	0.500	0.000***	0.782	0.776	0.947

*p<0.1; **p<0.05; ***p<0.01. p-value report coefficients from two-sided t-tests. 1[NT COV19] = part of the national team during COVID-19. The matching regression also includes team×season, matchday×season and position FE. The matching is conducted within position-matchday cells. We do not match infected players who ever got infected after our sample period or for who we do not observe the last match before the infection. We also drop these players from the final estimation sample. 'Before Matching' compares those observations which are treated in the probit regression, with all other included observations of non-infected players. 'After Matching' compares the matched observation-couples. All variables in the subgroup 'Past Match Involvement' give cumulative averages up to the matchday of the observation for a respective player.

Figure 5.31: Dynamic Effects Using a Fully Balanced Control Group





These figures plot the OLS coefficients β_τ of the event study regression following Equation (5.2). The reference time period is one to 75 days before treatment. Standard errors are heteroskedasticity-robust and clustered at the player level. The 90 and 95% confidence intervals are given by the two red-shaded areas. The dependent variables are the logarithm of the likelihood to play (LHS) and the logged number of passes (RHS). These regressions use a fully balanced sample based on propensity score matching as described in Table 5.3. $N = 205$ infected-counterfactual pairs.

Declaration of Contribution

I hereby declare that the chapter “What cannot be cured must be endured: The long-lasting effect of a COVID-19 infection on workplace productivity” is coauthored with Kai Fischer and J. James Reade. All authors contributed equally to the chapter.

Signature of coauthor Kai Fischer: ______________________

Signature of coauthor J. James Reade: ______________________

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A.M.D.G.

Eidesstattliche Erklärung

Ich, Herr Wolfgang Benedikt Schmal, versichere an Eides statt, dass die vorliegende Dissertation von mir selbständig und ohne unzulässige fremde Hilfe unter Beachtung der “Grundsätze zur Sicherung guter wissenschaftlicher Praxis an der Heinrich-Heine-Universität Düsseldorf” erstellt worden ist.

Düsseldorf, 28. August 2023

Wolfgang Benedikt Schmal