Corporate Innovation

The Role of Scientific Discoveries,

Taxation and Antitrust

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Dissertation, Munich 2021

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The Role of Scientific Discoveries,

Taxation and Antitrust

Inaugural-Dissertation

zur Erlangung des Grades

Doctor oeconomiae publicae

(Dr. oec. publ.)

an der Volkswirtschaftlichen Fakultät der

Ludwig–Maximilians–Universität München

vorgelegt von

Felix Pöge

2021

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Promotionsabschlussberatung: 14. Juli 2021

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Acknowledgments

As it takes a village to raise a child, an entire scientific community is involved in educating a young researcher. Over the years of my dissertation work, this community has been the team at the Max Planck Institute for Innovation and Competition and, more widely, the Munich Graduate School of Economics and LMU Munich. I am deeply indebted to and thankful for this wonderful group of scientists and friends.

First and foremost, I would like to thank Dietmar Harhoff, who supported, pushed and inspired me in his role as my first supervisor and co-author. His entrepreneurial spirit will never cease to amaze me. He convinced me to join the MPI and to go all-in on innovation research, and I have never looked back.

Further, I would like to thank Fabian Waldinger, my second supervisor. His fervent enthusiasm for great ideas and data as well as his emphasis on deep, careful study left me impressed and keen. With his guidance, I was able to turn what was an interest in history into a professional passion.

Thanks go out to all the coauthors who have taught and guided me, our collaboration has been a highlight of this dissertation time. This includes Stefano Baruffaldi, Matthias Dorner, Fabian Gaessler, Dietmar Harhoff, Karin Hoisl, Andreas Lichter, Max Loeffler, Ingo Isphording, Daniele Pacifico, Sebastian Siegloch and Thu-Van Nguyen.

I also thank the international community that all young researchers aspire to become part of for welcoming me. Having profited from so many, some stand out. Jeff Furman hosted me at Boston University, and I thank him for both enjoyable banter and deep insights. My thanks also go out to Serguey Braguinsky, for his advice and for hosting my virtual exchange at the University of Maryland. Finally, I thank Matt Marx, for spirited conversations and for testing me in the dissertation defense.

Without support and infrastructure, research is unthinkable. In particular, the MPI administration and IT teams deserve highest praises. I also thank the many research assistants that supported me over the years for their great work. I gratefully acknowledge financial support from the German Science Foundation through grants and the CRC TRR 190 “Rationality and Competition”. The Max Planck Digital Library and the LMU-ifo Economics & Business Data Center provided important data access. The many libraries in Munich, but in particular the library of the German Museum, made some research projects possible in the first place. Finally, the Institute for Employment Research (IAB) in Nürnberg, in particular Jörg Heining, has my deepest gratitude for outstanding support under difficult – Corona – circumstances.

Together with friends at MPI and LMU, the dissertation time was twice as enjoyable and half as tough. For exciting times between desk, Eisbach and beer garden, I thank Stefano Baruffaldi, Dennis Byrski, Hungni Chen, Marina Chugunova, Carsten Feuerbaum, Svenja Frieß, Fabian Gaessler, Corinna Hartung, Jonas Heite, Timm Opitz, Zhaoxin Pu, Dimche Risteski, Michael Rose, Cristina Rujan, Brendan Shanks, Stefan Sorg, Magdalena Streicher, Lucy Xiaolu Wang, and Rainer Widmann.

Most of all, I thank my wife Ling as well as my parents for their love, support and trust.

Progress in science and technology cannot be stopped. They are in many ways akin to art. One can persuade the one to halt as little as the others. They drive the people who are born for them to activity.[1]

(Carl Bosch, Nobel Laureate and CEO of IG Farben)

Preface

Research activities by large firms have been instrumental to the development of science and technology as we know it. From the first corporate research laboratories in the late 19th century to today's digital giants spans a thread of frontier research and development (R&D), ceaselessly pushing the confines of human knowledge and ability. Yet, these successes would have been impossible without antecedents of and interactions with societal institutions (Murmann, 2003). In the pursuit of fostering innovation and filling the gap between private returns and benefits to society (Nelson, 1959; Arrow, 1962), governments have introduced supporting institutions, such as intellectual property protections (Nordhaus, 1969), national science systems (Bush, 1945) or antitrust regimes (Lamoreaux, 2019). This thesis sheds light on a selection of governmental policies, the resulting opportunities and challenges for corporate innovation and how firms rise to meet them.

Next to the in-house innovations, commercializing discoveries of outside inventors has played an important role for corporate innovation for a long time. Not only in the sciences (Iaria, Schwarz, and Waldinger, 2018), but also for industrial innovation, access to frontier scientific knowledge was of crucial importance. The Haber-Bosch process for nitrogen fixation, revolutionizing the basis of both agriculture and warfare, is a prime example. Here, Haber's academic invention at laboratory scale was followed by Bosch's industrial research into scale towards commercialization (Hager, 2008). The science-industry relationship has however shifted over time. In the 1940s, the US government increased spending on basic science and R&D and replaced university funding by firms, whilst a changed antitrust regime made corporate in-house R&D more attractive (Mowery, 1995). Towards the end of the 20th century, basic research by US corporations declined again (Arora, Belenzon, and Patacconi, 2018), leading to renewed reliance on external knowledge sources, in particular government-funded research (Fleming, Greene, et al., 2019).

Chapter 1 of this dissertation explores the relevance of high-quality science for innovation. Chapter 2 goes on to discuss corporate knowledge access strategies in the modern frontier technologies in computer science. Scientific conferences provide a direct access channel for the most innovative companies to embed themselves into scientific communities.

Investment in basic research and the provision of other public good is vital for corporate innovation, but is also costly. Member countries of the Organisation for Economic Co-operation and Development (OECD) spend on average 0.6% of GDP on R&D, adding to three times that spent by businesses. On average a third of R&D-performing companies received innovation support from their government, but these subsidies only amount to 0.2% of GDP. Governmental support of basic science and R&D needs to be financed. In the mix of tax instruments of OECD countries, corporate taxes only constitute around 13% of total tax

[1] Cited in Hayes (1987) as excerpt from a 1932 speech. Bosch was discussing the role of the Haber-Bosch nitrogen fixation process for prolonging the first world war and concludes with the quote that such considerations were moot.

revenue. Still, the resulting 3% of GDP easily support the government expenditures on R&D (OECD, 2019; OECD, 2020; OECD, 2021). As Nelson (1959) pointed out, government support for R&D relies on an argument of large positive externalities of such investment relative to other types of investment. As such, financing governmental R&D support with taxes on R&D-related profits increases the wedge between private returns and benefits to society and strengthens disincentives. As a consequence, much of the government support for R&D comes through tax subsidies. For optimal policies, knowledge of the effects of corporate taxes on R&D and innovation is important.

Chapter 3 of this dissertation contributes to this question by analyzing the effect of the German local business tax on corporate R&D and patent applications.

At times, corporations need to be reined in as well to protect the competitiveness of markets and to allow disruptive innovation (Federico, Morton, and Shapiro, 2019). Competition policy for innovation is wedged between the theoretical approaches of Schumpeter (1942) and Arrow (1962). In Arrow's "replacement effect", firms earn profits in imperfectly competitive markets, reducing incentives to replace old profits from existing technologies by introducing new innovations. In contrast, Schumpeter argues that in imperfectly competitive markets, firms can easier appropriate returns from their innovations, raising incentives to innovate. The endogeneity of market structure towards innovation makes an empirical evaluation difficult, but available evidence points towards a positive effect of competition on innovation (Gilbert, 2020).

Mergers constitute events with particular effect on the competitive situation, but with distinct features making evaluation more difficult than general innovation policy. Mergers require different theoretical approaches relative to standard models of competition and innovation. Instead of, for example, changing the number of competitors in a market, everything else equal, mergers leave available assets and production facilities fixed while unifying control rights for some of them in one actor (Federico, Morton, and Shapiro, 2019). Problems of empirical evaluation are worsened as the occurrence of merger events is endogenously chosen by the merging parties. Further, merger litigation by antitrust authorities is selective based on predicted merger effects (Carlton, 2009). Empirical evidence obtained in spite of these challenges finds mixed evidence with either positive or no effect of competition on innovation (Gilbert, 2020).

Chapter 4 contributes to this literature by analyzing the breakup of the leading German chemical company, IG Farben, following the Second World War. The breakup was unexpected before the war and executed by external actors (the Allied powers). It was based on political economy considerations rather than antitrust analysis and largely followed a geographical structure. Exploiting this, the analysis can circumvent the limitations of empirical analyses outlined above.

In fact, the histories of the science-industry relationship and the development of modern antitrust regimes have been linked (Mowery, 1995; Lamoreaux, 2019). In the US, the first establishment of corporate research laboratories is linked to emboldened enforcement of the Sherman Act, as companies sought new ways of securing their competitive position. Similarly, the shift of US firms towards in-house R&D in the post-WW2 period has been linked to stricter antitrust enforcement (Mowery, 1995). Historically, the IG Farben breakup is placed early in Germany's transition from a weak antitrust regime closer towards a US role model (Murach-Brand, 2004). While Germany is perceived to have featured high levels of industrial research already before the stronger antitrust regime (Mowery, 1995), the regime shift might have further contributed to this outcome.

Summarizing, corporate innovation thrives in the context of a multitude of institutions providing incentives. This thesis touches on a selection, investigating the role of scientific discoveries, taxation and antitrust. The remainder of the preface summarizes the four self-contained chapters.

Chapter Overviews

Chapter 1 provides a descriptive analysis of the relationship between science and innovation.[2] In joint work with Dietmar Harhoff, Fabian Gaessler and Stefano Baruffaldi, we explore in particular the role of the quality of science for innovation. Previous research had shown that where science and innovation are closely connected, at the frontier between the two domains, both science and innovation are especially impactful and valuable (Ahmadpoor and Jones, 2017). We reinforce this finding by showing that the science-innovation relationship is moderated though the quality of the cited science.

The literature on science-based patents draws conclusions from references from patents to scientific articles. Accordingly, we start from patents with front-page references to scientific articles, so-called scientific non-patent literature references (SNPL). For this purpose, we employ a large, custom dataset connecting United States Patent and Trademark Office (USPTO) and European Patent Office (EPO) patent references to publications listed in Web of Science and Scopus (Knaus and Palzenberger, 2018). Since the publication of the chapter, the availability of large, open-source datasets covering both front-page (Marx and Fuegi, 2019) and in-text non-patent literature references (Marx and Fuegi, 2020) has advanced the literature even further.

We provide evidence that the quality of scientific publications – as commonly assessed in science via citations – is a strong predictor of their relevance for and impact on technology development as documented in patents. We document two main results. First, publications with high scientific quality are vastly more likely to be cited in patent documents, and cited at a higher rate (Hicks et al., 2000). Second, among patents directly building on science, the value of patents increases monotonically with science quality.

These results generalize to patents beyond the science-technology frontier - specifically, to patents linked indirectly to a scientific publication via references to other patents. Patents for which the shortest path in the citation network is longer are said to be more distant from the science-technology frontier. We find that the correlation between patent value and SNPL science quality largely propagates to patents at higher distances to the frontier. Science of high quality spurs technological progress of high value far beyond the science-technology frontier.

Results remain stable when accounting for contextual information such as organizational boundaries, timing and for alternative measurement. The science quality-patent value correlation holds for patent-science links with and without self-references on the inventor or institution level. High-quality science is linked to high-value technology also beyond the organizational boundaries within which it is developed. Similarly, results hold when using organization fixed effects to restrict to within-institution variation, albeit with reduced magnitude. Follow-on patents with shorter time lags between science publication and patent application are always associated with higher patent value. The correlation with SNPL science quality remains strongly positive for all levels of citation time lag, but is stronger for patents with short

[2]This chapter is published as Felix Poege et al. (2019). "Science quality and the value of inventions." In: *Science Advances* 5.12, eaay7323 in a slightly modified version, reprint under creative commons license CC-BY: https://creativecommons.org/licenses/by/4.0/.

lag. Finally, results are robust for different measurements of patent value, such as variations of forward citations (from USPTO or EPO patents), monetary values from abnormal stock returns (Kogan et al., 2017) and inventor surveys (Giuri et al., 2007) or using patent claim lengths (Kuhn and Thompson, 2019).

Our results are descriptive and the exact causes of the strong correlation will have to be analyzed in future work. Leaving aside the exact causal links, our results provide intriguing evidence for the governance system of science, e.g. at universities and public research organizations, as well as for funding agencies and science policy-makers. The current science system steers researchers to strive for success measured in terms of citations and impact. According to our findings, the outcomes of such a system are well-aligned with later stages of technology development and translation of science results. Our study does not provide evidence on the optimality of the alignment. However, it clearly contradicts the notion that the application of bibliometric criteria in science funding decisions would lead researchers to engage in exercises that are of little value to society at large.

Chapter 2, joint work with Stefano Baruffaldi, focuses the science-innovation relationship to the question of knowledge access and diffusion.[3] While firms invest considerable resources on R&D, the origin of ideas often remains outside of their institutional boundaries – in particular, in scientific communities. Science is often perceived as a source of accessible knowledge by virtue of the norms of publication and knowledge sharing. On the other hand, seminal contributions suggest that knowledge spillovers mainly arise at close proximity (Audretsch and Feldman, 1996; Jaffe, Trajtenberg, and Henderson, 1993) and firms have to make specific investments to absorb external knowledge (Cohen and Levinthal, 1989). Firm participation, defined as authorship of papers or sponsorship of conferences, is such a specific investment. This chapter investigates the extent to which firms participate to scientific communities and to what extent such participation leads to knowledge diffusion.

Benefiting from scientific communities might require active participation of firms, alternatively passive observance might suffice. If knowledge flows freely via the publication process, participation should at the extreme be irrelevant. Moderate firms investments in participation may suffice to abate search costs in the increasing body of codified knowledge (Fleming and Sorenson, 2004; Jones, 2009). We add and juxtapose the notion that, in the absence of market mechanisms, a reputation-based system governs scientific communities. Knowledge diffusion is embedded in a process of socialization which is facilitated by temporary proximity, but ultimately requires the ability to establish personal connections. Active and intense participation is necessary to gain reputation and comply with norms that ease social relationships (Merton, 1973; Crane, 1974; Dasgupta and David, 1994; Stephan, 1996). Scholars such as Rosenberg (1990), Hicks (1995), and Cockburn and Henderson (1998), lead the way in this line of investigation but specific evidence has been rare.

Computer science (CS) conferences are a relevant and suitable context to study firm participation to scientific communities and knowledge diffusion. The large economic relevance of industries around CS make it an interesting setting (Brynjolfsson and Hitt, 2003; Tambe et al., 2020). Anecdotally, firms play a large role in the process of scientific and technological advance, field visits and practitioner interviews deepen this impression. More importantly, the specific norms within CS generate favorable

[3]A pre-print working paper version is available as Stefano H. Baruffaldi and Felix Poege (2020). *A Firm Scientific Community: Industry Participation and Knowledge Diffusion*. en. SSRN Scholarly Paper ID 3644106. Rochester, NY: Social Science Research Network.

conditions for empirical investigations. Conference presentations and the resulting proceedings are more closely comparable to short journal publications than pure occasions for knowledge exchange as in other disciplines. Consequently, a highly detailed “paper trail” closely captures the universe of conference presentations. Conferences require scientists to attend. Similar to journals, scientists cannot submit the same paper to multiple conferences, so that they have to choose.

We study how knowledge flows towards firms change with exposure to science at CS conferences. Participation in a scientific community is a choice for firms and scientists, which may be endogenous to other determinants of knowledge flows. To establish causality, we focus on the choice problem of participation for scientists outside of a focal firm. Our econometric models isolate exogenous variation in participation arising from the availability of direct flights, as a proxy of general costs of transportation (Giroud, 2013; Catalini, Fons-Rosen, and Gaulé, 2020). For a given sample of proceedings at a conference A where a firm participates, we consider a counterfactual group of proceedings from another comparable conference B (in the same year, the same field, comparable quality and size). We then instrument the exposure of the firm at A to A/B proceedings with direct flight connections of authors of the A/B proceedings to the conference location A. Different levels of fixed effect controls (FE) rule out variation that may correlate with both the availability of direct flights and the dependent variables.

Identification relies on the assumption that there is exogenous variation in flight connectivity between scientists and conference venues, unrelated with the probability of knowledge flows between scientists and firms. A key virtue of our setting is that the instrumental variable (IV) operates at the firm-conference-scientists triad level. In the large majority of cases, the connectivity between firms and scientists via the conference will not coincide with their direct connectivity. A first concern arises from localized scientific communities. In this case, participants to any conference would be better connected to the corresponding venue as well as between each other. In our model we can fully account for this concern, introducing FE at the firm and scientists region pair level. A second concern is the possibility that time variation at the firm or scientists location level may correlate both with direct flights and the pair level probability of knowledge flows. Increasing regional scientific strength might induce higher connectivity as well as citations. Since our analysis and in particular the IV is defined at the triad level, we can fully account for this variation by introducing region-year FEs. For analogous concerns at the firm level, we include firm-year FE. While we cannot test the IV assumptions directly, we undertake several ancillary analyses. In a placebo test, we show that citations from firm patents and publications published before the focal conference remain unaffected in IV specifications, different from the standard OLS results. We study the dynamics of the effect in the first stage, showing that direct flight connections lead to participation of scientists to the conference series only in or after the year of the conference. We show the robustness of the models to the inclusion of several additional FEs.

We test the hypotheses in a large and global data set with more than 7,000 conferences and 5,000 firms, between 1996 and 2015. In this time span, firm participation to conferences is frequent and at a constant rate. Firm tend to contribute in the conference series of the highest quality and firm proceedings are, on average, highly cited. Firm participation frequency and intensity are highly skewed, with the top firms being responsible for the majority of the observed firm participation.

We find that firm citations to scientific papers that the firm was exposed to are much more likely relative to citations to papers the firm was not exposed to. For patent citations, we find no increase in the citation probability. Firm citations towards previous papers by authors present at the conference increase for both science and patent citations. This already suggests that actual interactions with scientists matter. For more direct evidence, we analyze future scientific collaborations between the firm and scientists present at the conference and observe an increase in the collaboration probability. Contrary, we do not find an average effect on hiring as captured through scientist careers, although recruiting was a stated objective of many participating firms. The knowledge flows we observe are largely explained by interactions and collaborations with scientists that remain external to the firm.

Finally, we study the heterogeneity of the effects by the intensity of the participation of the firm. We find that all effects increase greatly with the intensity of firm participation. For firms that both sponsor the conference and author several proceedings, citation probabilities increase most strongly. We find similar patterns using an indicator of firms' research investments size, based on the number of firms' active scientists.

Our results relate to the literature on corporate science (Rosenberg, 1990; Hicks, 1995) and the external knowledge sources of firm innovation (Cassiman and Veugelers, 2002; Cassiman and Veugelers, 2006; Simeth and Raffo, 2013). Despite declining firm investments in basic research, scientific knowledge remains a primary driver of technology and firm value (Simeth and Cincera, 2016; Arora, Belenzon, and Patacconi, 2018; Fleming, Greene, et al., 2019). Our results offer novel evidence to understand the relevance of participation in scientific communities and the rationale for corporate science, more broadly.

Second, we add to the literature on firms' absorptive capacity (Cohen and Levinthal, 1989). Investments in science can be compatible with firms' objectives because connections with scientific communities help to access external scientific knowledge (Cockburn and Henderson, 1998). Firm participation in a conference is a relatively accessible investment, but can be subject to steep increasing returns to investments (Gittelman and Kogut, 2003; Gittelman, 2007).

Finally, we contribute to the literature on knowledge diffusion (Audretsch and Stephan, 1996; Jaffe, 1989; Eeckhout and Jovanovic, 2002). Extant work has demonstrated that temporary proximity can have large effects (Boudreau et al., 2017; Campos, Lopez de Leon, and McQuillin, 2018; Chai and Freeman, 2019; Lopez de Leon and McQuillin, 2020; Lane et al., 2020). We bring attention to the role of organizations' investments as antecedents to such opportunities. Specifically, firms put in place non-market strategies to acquire external knowledge, based on investments to position themselves within external scientific communities. The evidence is compatible with an important role of reputation and prestige of organizations and the centrality of social relationships within communities (Akcigit, Caicedo Soler, et al., 2018; Haeussler, 2011; Haeussler et al., 2014), that go beyond the effect of proximity and search costs, and yield increasing returns on participation investments. An uneven pattern of diffusion emerges, that more plausibly leads to divergence and concentration, rather than convergence, in firms' innovative productivity (Andrews, Criscuolo, and Gal, 2015; Autor, Dorn, Katz, et al., 2020).

Chapter 3 is joint work with Ingo Isphording, Andreas Lichter, Max Löffler, Thu-Van Nguyen and Sebastian Siegloch.[4] The chapter investigates whether and how public policy, particularly tax policy, can foster or impede firms' innovation activities. Given the role of innovation for economic growth (Romer, 1990), which policies are effective at impacting innovation has been an important question for policymakers (Bloom, Van Reenen, and Williams, 2019).

In this chapter, we study the German business tax system's impact on R&D and innovation. We exploit variation in the local business tax, a profit tax set at the level of the municipality. German municipalities can annually alter the local business tax rate, while the definition of the tax base is fixed at the federal level. We exploit variation in tax rates induced by around 7,300 local tax reforms over the period from 1987 to 2013. As the given profit tax applies to nearly all German plants, we can study policy effects across the entire population of R&D-active plants. Our estimations rely on survey data targeting all R&D-active plants in Germany. We complement this dataset with information on granted EPO patents as well as financial data from Amadeus/Orbis.

We apply an event study design and complementary difference-in-differences regressions to estimate the causal effect of tax changes on plants' innovation activities. Our preferred empirical specification regresses plant-level outcome variables on leads and lags of tax changes, conditional on plant and municipality fixed effects, sector × year fixed effects, as well as flexible and finely-grained region (e.g., commuting zone) by year fixed effects. The latter set of fixed effects accounts for unobservable time-varying confounders at very disaggregated geographical levels. Effects dynamics around the date of treatment as well as additional robustness checks do not point to the presence of confounding effects. For example, we do not detect local economic conditions, population movements or government expenditures to coincide with a given change in the local profit tax.

Theoretically, we expect an increase of the profit tax rate to have a negative effect on plants' R&D spending and innovation. First, the tax-induced decrease in profits lowers plants' expected post-tax returns on R&D expenditures, which should lower the level of R&D expenses in turn. Second, we expect a tax increase to particularly affect those expenses that are financed by equity – given that only the costs of debt-financing are deductible from a plant's tax base. The nature of R&D activities suggests that this is particularly true for expenses on research and development: unfinished R&D projects have little residual value, lack collateral and face a high risk premium by debt-holders. Moreover, R&D investments are highly uncertain and potential returns generally realize with substantial time lags. Hence, we expect R&D investments to respond more to an increase in profit taxation than overall investments. This should particularly hold true for young and credit-constrained firms, where the lack of collateral is usually particularly pronounced (Brown, Fazzari, and Petersen, 2009; Thakor and Lo, 2017). Lastly, a reduction in R&D spending should eventually translate into reduced innovation output as measured via the number of filed patents (see, e.g., Griliches, 1990, for the assumed input-output relationship).

We find a negative, statistically significant effect of a profit tax increase on plants' total R&D expenditures and patent applications. For R&D expenditure, we estimate a long-term elasticity of -1.25, which is lower than estimates reported in the context of targeted R&D tax credits or subsidies. The tax-induced reduction in R&D spending is entirely driven by internally- rather than externally-conducted R&D spending. If firms conducting the external R&D are located in a different municipality and subject to different tax rate

[4] See also Ingo Isphording et al. (2021). "Profit Taxation, R&D Spending and Innovation." mimeo.

changes, this is plausible. Further, the increased transaction costs of external R&D as well as its role in knowledge acquisition may render it less elastic. We observe tax-induced reductions in innovation output, in raw numbers of patents but also when accounting for quality differences by weighting with forward citations. The effect materializes with some temporal lag of around four years.

Average effects mask heterogeneity by plant liquidity and size. Reductions in R&D spending are particularly strong among more credit-constrained plants. In contrast, we detect no notable differences along the plant size distribution. This somewhat questions common practice that R&D tax credits and subsidies are often size-dependent, with policy makers implicitly assuming small- and medium-sized firms to be more responsive to a given level of support (Gonzales-Cabral, Appelt, and Galindo-Rueda, 2018).

We extend the scope of the analysis beyond the plant level by assessing the role of innovation for economic growth, as well as quantifying the importance of tax policy in this relationship. We first show that local innovation has a positive and lasting effect on local growth, while an increase of the local business tax substantially reduces growth. Using the estimated elasticity of filed patents with respect to the tax rate, we further back out that around 40% of the total negative effect of profit taxation on local growth is due to tax-induced reductions in innovation.

We contribute to the small and recent literature that exploits variation in sub-national tax policy settings to study the effect of (corporate) taxation on innovation (Moretti and Wilson, 2017; Mukherjee, Singh, and Žaldokas, 2017; Akcigit, Grigsby, et al., 2018). The analysis of German municipalities provides an analysis on a smaller geographic level with substantially more variation in treatment. Official survey data targeting the universe of R&D-active plants further enable us to study detailed plant-level responses to changes in the local business tax rate, both in terms of innovation input and output (e.g., the effects of a tax increase on internal vs. external R&D spending, or process vs. product innovations). Moreover, we use the rich plant-level data to point to mechanisms that underlie the overall effects.

The chapter further speaks to the literature that estimates the effects of targeted R&D tax credits, deduction possibilities and subsidies (e.g. Bronzini and Iachini, 2014; Dechezleprêtre et al., 2016; Guceri and Liu, 2019; Chen, Jiang, et al., 2019; Agrawal, Rosell, and Simcoe, 2020). We estimate an R&D spending elasticity with respect to the user cost of capital that is well in the range of these studies. This set of studies provides clean causal evidence by exploiting policy cut-offs to establish quasi-experimental research designs. At the same time, the estimates are clearly local in nature, referring to firms around the respective thresholds. The proposed identification strategy in this chapter enables us to estimate treatment effects along the full distribution of R&D-active plants. Hence, we are able to identify average treatment effects but also test for heterogeneous effects along various plant characteristics. For instance, we show that effects are homogeneous across the plant size, which questions the rationale for size-based innovation policies to some extent.

Finally, we connect to a large literature that is concerned with market failures that reduce private R&D activities below socially desirable levels (Nelson, 1959; Arrow, 1962). The benefit to society from innovation are generally well above the private return (Griliches, 1992; Jones and Williams, 1998). At the same time, expected knowledge spillovers as well as uncertainty about marketability may lead to private under-investments into R&D (Czarnitzki and Toole, 2011). Taxes on firms may further lower the private returns to R&D, while the benefits to society remain unaffected. This, in turn, widens the gap between actual and socially desired levels of R&D in an economy (Klenow and Rodriguez-Clare, 2005).

Chapter 4, single-authored, studies how the breakup of a leading innovative company affects competition and innovation.[5] Mergers in innovative industries, such as Dow-DuPont (2017) and Bayer-Monsanto (2018), have renewed the interest in the effect of competition on innovation. This effect is hard to determine empirically, as exogenous variation in market structure is rare. Merger analysis in particular suffers from endogenous selection into mergers as well as enforcement conditional on expected merger outcomes. To overcome these limitations, I exploit the 1952 breakup of Germany's leading chemical company, IG Farben. The breakup was imposed because of IG Farben's importance for the German war economy outside of standard antitrust procedures. In technology areas where the breakup increased competition, patenting strongly increased. Fine-grained information on suppliers and prices of chemical substances allows auxiliary product-level analysis. The results suggest large positive breakup effects without short-run trade-offs.

Before the breakup, IG Farben was one of the most innovative German companies. Three of its scientists won Nobel prizes, and IG Farben produced more than 16% of German-invented patents in chemistry. After the Second World War, the victorious Allies saw IG Farben's economic influence combined with its crucial relevance for the German war machine as undue political potential. IG Farben's crimes, such as its major involvement at the Auschwitz concentration camp, fueled this negative perception. However, political differences between the occupying powers delayed action and the looming cold war altered views on IG Farben. The Allies, now supporting the IG constituents in their respective occupation zones, decided on a breakup largely following this structure. The breakup created three large successors, BASF, Bayer and Hoechst, as well as a dozen smaller businesses (Stokes, 1988; Stokes, 1994).

The breakup of IG Farben, from creation via merger to breakup, closely relates to considerations relevant to today's merger and potential breakup decisions. In merger analysis, antitrust authorities consider the trade-off between potential efficiencies with disincentives arising from reduced competition. In the IG Farben case, historical sources cite both organizational synergies and scale as reasons for IG Farben's 1925 creation via merger (e.g. ter Meer, 1953; Plumpe, 1990; Abelshauser, 2003). A priori, the welfare effects of the initial merger or the eventual breakup are unclear.

The breakup of IG Farben differs in important aspects from standard (de)merger cases, offering advantages for empirical analysis. Business considerations of the afflicted company were not the primary motivations of the breakup, as in merger cases and corporate demergers. Standard economic antitrust considerations, where markets or technologies with potential harm motivate merger litigation, were not the primary causes of the breakup. Both would lead to selection in observable mergers and merger litigation. Rather, the breakup was rooted in contemporary political economy considerations and executed by an external force, with idiosyncratic geographical factors playing a large role. Consequently, effects of this breakup are closer to causal than previous analyses.

As the main result, innovation in areas impacted by the IG Farben shock increases strongly and persistently compared to other areas of chemistry. This conclusion results from a comparison of chemical patents exposed to or unaffected by the IG Farben shock in a difference-in-differences analysis. Breakup exposure

[5] See also Felix Poege (2021). "Competition and Innovation: The Breakup of IG Farben." mimeo.

is the concentration change implied by considering IG Farben as one or as separate successors. IG Farben research facilities were geographically spread out. As the breakup was largely geographical in nature as well, the post-breakup structure can be backdated to the pre-breakup, pre-war time. This avoids contamination by wartime events and post-breakup adjustments. The development of patenting in exposed and unaffected technologies is parallel before 1952. After the breakup, the two increasingly diverge. Results are similar when counting only patents without IG Farben association. Results are also similar when modifying exposure measures to isolate the concentration change caused by the geographical breakup structure. IG Farben itself is difficult to analyze causally as the number of successors is small and appropriate control firms are missing. Descriptive analysis suggests strongly increasing patent output by IG successor firms at high but constant R&D intensity.

The antecedent of innovation effects are changes in product space. Historical product catalogs, matched to prices, allow a detailed analysis. In fact, the breakup led to horizontal, product-level competition. After the breakup, 40% of IG Farben-supplied products are offered by two or more successors. Ten years after the breakup, IG successors still compete at comparable, albeit moderately lower levels. Increased competition could crowd out other competitors, but it could also remove barriers to entry (Aghion and Bolton, 1987). Price effects of the breakup could counteract or exacerbate the innovation effect (Gilbert, 2020). In fact, the number of suppliers increases where the breakup led to competition. The increase is driven by non-IG firms, which suggests additional entry. The IG Farben breakup leads to moderate price declines for products with competition between successor companies, but not for products with only one successor.

The interpretation of the results needs to consider the limitations arising from the historical context. However, historical factors can only influence the results insofar as they differentially affect breakup-exposed sectors within chemistry. However, the breakup effects are concentrated in products and technologies where the geographic structure of the breakup created technology and product-level competition, rather than in areas with IG Farben exposure per se. Some historical factors such as war destruction, Allied occupation policies or tariff changes are measurable. When included in statistical analysis as control variables, they do not materially affect the conclusions. Other historical factors are difficult to quantify, but their potential influence can be judged based on historical research.

The results emphasize the importance of a strong antitrust regime and merger control to maintain competitiveness of markets.

This chapter contributes to the empirical literature on competition and innovation, in particular towards the topic of mergers. A merger (reversal) was effected by an intervention external to the market participants themselves. Previous empirical evidence predominantly relies on merger retrospectives, raising problems with firms' self-selection into merging and selective enforcement (Carlton, 2009). Researchers collect data on a large number of merger cases, use matching to generate control cases and DiD analysis to estimate effects (Ornaghi, 2009; Szücs, 2014), combined with instruments (Haucap, Rasch, and Stiebale, 2019). Another strand of literature relies on structural estimation (Goettler and Gordon, 2011; Igami and Uetake, 2020) In this chapter, effects are instead estimated within one event which differentially affected a large number of technologies and products.

Much of the previous literature on mergers and innovation has focused on direct effects on the merging parties (Haucap, Rasch, and Stiebale, 2019, as an exception). This chapter instead focuses on aggregate breakup effects, combining reactions by the IG Farben successors with reactions from other competitors. Aggregate effects are relevant from a welfare perspective, as it is unclear whether the responses of successors and competitors are in parallel. For prices in markets for homogeneous products this is likely the case. In contrast, innovation decisions of competitors might also be strategic substitutes or complements (Bulow, Geanakoplos, and Klemperer, 1985; Bloom, Schankerman, and Van Reenen, 2013a; Gilbert, 2020, p. 89). Then, competitor responses may exacerbate or offset the change of innovation output by the IG successors. In fact, the IG Farben breakup seems to have increased patenting both by IG Farben as well as other firms.

This chapter relates to the literature on the history of antitrust, in particular towards breakups of large corporations. Such government action is rare and the literature has focused on seminal US cases such as Standard Oil or the Bell system (Lamoreaux, 2019). Yet, cases are few and far between. The IG Farben case adds by broadening the view to a new industry structure in an industry where innovation can be quantified well and broadly.

This chapter also contributes in making novel data available, either newly or much improved. For one, product catalogs offer fine-grained product information that approaches market definitions more closely than the typically used firm- or industry-level data (Affeldt et al., 2021). Comparable detailed product and price data was previously unavailable for this time period and industry. German patent data is processed in greater detail and over a longer time-span than before. Intensive use of machine learning and image processing make it possible to recover applicant, inventor and technology class information previously unavailable at a comparable scale.

1

Science Quality and the Value of Inventions

Abstract *Despite decades of research, the relationship between the quality of science and the value of inventions has remained unclear. We present the result of a large-scale matching exercise between 4.8 million patent families and 43 million publication records. We find a strong positive relationship between quality of scientific contributions referenced in patents and the value of the respective inventions. We rank patents by the quality of the science they are linked to. Strikingly, high-rank patents are twice as valuable as low-rank patents, which in turn are about as valuable as patents without direct science link. We show this core result for various science quality and patent value measures. The effect of science quality on patent value remains relevant even when science is linked indirectly through other patents. Our findings imply that what is considered "excellent" within the science sector also leads to outstanding outcomes in the technological or commercial realm.*

1.1 Introduction

The relationship between science and technology has been subject to intense discussions for centuries. Science was largely funded via patronage during the Renaissance, and a separation of public funding for fundamental research and private, industrial funding for applied research and commercial innovation efforts only emerged in the 19th century (Scotchmer, 2004; Mokyr, 2002). Since the aftermath of World War II, policy-makers have relied on the notion that science helps to generate knowledge and information which will ultimately contribute to the emergence of new technical and organizational capabilities, improvements in the quality of life and economic growth (Bush, 1945). Vannevar Bush's vision of a publicly funded science system that feeds into privately organized innovation channels became the blueprint for most of the Western national systems of science funding, R&D and innovation. This notion has recently come under scrutiny again as voters increasingly demand evidence on the benefits of science spending. For policy-makers and scientists alike, it is tantamount to improve the understanding of the impact of science on technical progress and innovation.

The most pertinent form of output delivered by the science sector are publications, which are known to vary widely in quality. While some scientific publications will reach and inspire large numbers of researchers, others are never read or referenced. Measures of science quality, such as citation counts or impact factors, are used to make this heterogeneity visible and have become increasingly important in the governance of the science sector. Science governance and science funding seek to promote excellent over more mediocre science output by allocating resources to those researchers and institutions from which outstanding results can be expected.

But it has been argued that this logic does not take tangible results from technology transfer and commercialization into account. Science is inward-looking according to these voices. This raises the question to what extent science output that is considered "excellent" within the science sector can lead to outstanding outcomes in the technological or commercial realm. This paper seeks to contribute new insights toward the understanding of this nexus.

We provide evidence that the quality of scientific publications – as commonly assessed in science via citations – is a strong predictor of their relevance for and impact on technology development as documented in patents. We document two main results. First, publications with high scientific quality are vastly more likely to be cited in patent documents, and cited at a higher rate. Second, among patents directly building on science, the value of patents increases monotonically with science quality. These results hold across scientific disciplines, technology areas and time.

1.2 Data

Our analysis starts from the universe of scientific publications in Web of Science (WoS) from the year 1980 onwards, corresponding to approximately 43 million scientific publications. In terms of patents, we consider a sample of more than 4.8 million patent families, comprising all patent families from the database DOCDB with at least one grant publication at the European Patent Office (EPO) or the United States Patent and Trademark Office (USPTO), with first filing date between 1985 and 2012, included.

Subsequently, our unit of analysis is the patent family, to which we also interchangeably refer as patents. The patents protect inventions in developed countries with in total more than one billion inhabitants.

Patents reference various types of documents which relate to the protected material either by determining novelty (prior art) or by explaining the content of the underlying invention. These documents include foremost other patents, but frequently also non-patent literature (NPL). (Callaert et al., 2006) A subset of the latter are references to scientific articles, which we dub Scientific Non-Patent Literature (SNPL).

To link patents to publications, we leverage a highly precise and comprehensive match of NPL references in patents with scientific publications in WoS.[1] The NPL references in patents that were successfully link to scientific publications comprise our set of *S*NPL references. Around 0.9 million patents were linked to at least one scientific publication via a total of about 7.0 million SNPL references. Out of all scientific publications, about 2.2 million figure in this list of SNPL references.

In our core set of analyses, we rely on established measures of scientific quality and patent value. The quality of scientific publications is measured by the number of citations from other scientific publications over a period of three years from publication. We define a patent's SNPL science quality as the quality of the patent's SNPL references. A patent can reference zero, one or several scientific articles, in the same way as a scientific article can be referenced by zero, one or many patents. Figure 1.1a illustrates this setup. When more than one SNPL reference is present, we consider by default only the publication with the highest quality. Patent value is measured by the number of forward patent citations over a period of five years from the patent's first filing date. We use citations by US patents as our first measure of patent value. Our results are robust to alternative choices. We replace citations as science quality measure by the journal impact factor. We replace our aggregation method of the quality of multiple SNPL references with several other options. We replace US patent citations as value measure by a host of alternatives. [2]

1.3 Results

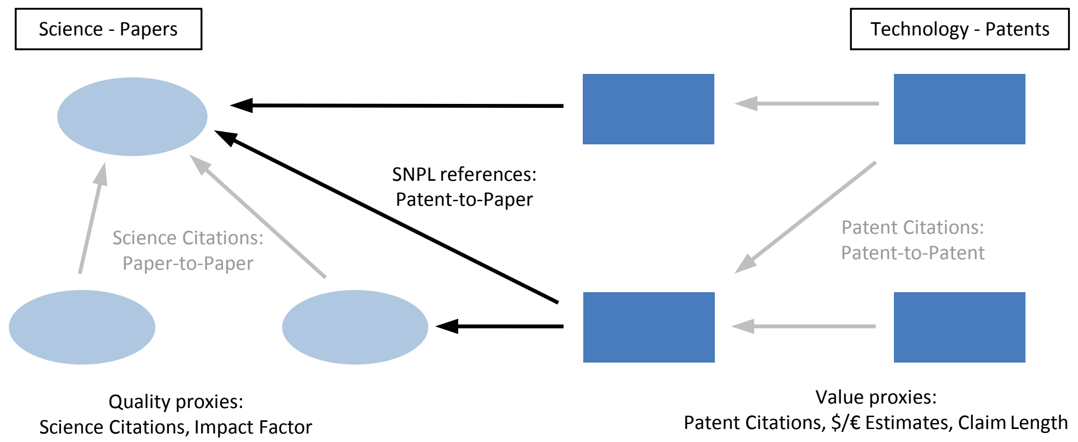
As a first-order question, we explore the selection of scientific publications into the patent realm, i.e., the relationship between science quality and the likelihood that a scientific publication is referenced in a patent (Hicks et al., 2000). We look at the probability and intensity of referencing, i.e., if any and how many patented inventions refer to a given scientific contribution. We present results for publications below the median (all receiving 0 science citations), for publications between the median and the 70th percentile, and at the percentiles 80, 90, 95, 99 (top 1 percent), 99.9 (top 1 permille), and 99.99 (top permyriad) of scientific quality. Figure 1.1b presents these results; the line plots the share of scientific publications appearing as SNPL reference in at least one patent, and the size of the circles indicates the average number of times they appear as SNPL references.

We find a remarkably strong positive selection of scientific publications of high scientific quality into SNPL references. Below the median, scientific publications are almost never SNPL references. This number increases up to 40% at the top 1% of publications by scientific quality. A staggering majority

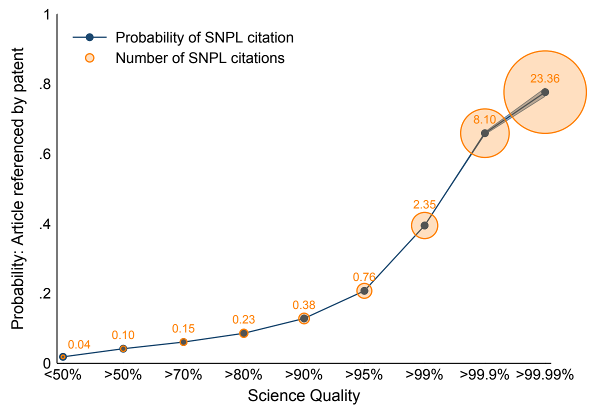
[1]The match is based on a methodology documented in detail in Knaus and Palzenberger (2018) and summarized in the supplementary material.

[2]The supplementary material provides further detailed information on data sources, discusses the use of citations as indicators of relatedness between technology and science and elaborates on alternative measures of patent value as well as scientific quality that we use for robustness analyses.

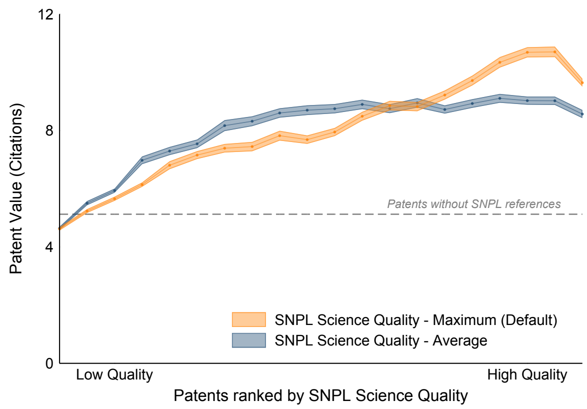
of publications at the top 1 permille (>60%) and beyond the top 1 permyriad (80%) are referenced in patents. The average number of times they appear as SNPL references in distinct patent families is 7.7 and 21.9, respectively. We emphasize that these results are not due to a feedback from important patents to citations of the underlying science. By restricting our measure for scientific citations to the first three years after publication, we have effectively excluded this bias.



(a) Setting: The domains of science (left), technology (right) and patent-paper references



(b) SNPL references by science quality



(c) Patent value by SNPL science quality

Figure 1.1: Setting and main results

Notes: Science quality is the 3-year citation count from other scientific publications. b) The patent count is not conditional on appearing as a SNPL reference. Blue shaded areas show 95% confidence intervals around the mean. N = 42,962,463. c) SNPL science quality is the quality of publications referenced by a patent. When there are multiple patent-paper references, we by default use the highest-quality reference (orange). In comparison, we use the average quality (blue). Patent value is measured as the 5 year count of patent forward citations by US patents. Patent value and science quality are residualized using technology field $\times$ first filing year FEs. Shaded areas show 95% confidence intervals around the respective means. N = 4,767,844 patents (948,006 with SNPL references).

We move on to our main analysis and investigate the extent to which SNPL science quality is a predictor of patent value. The main figures account for level differences across technology fields and over time. We estimate econometric models that absorb variation across these dimensions with pair-level fixed-effect (FE) controls and graphically present the resulting residual values. In effect, we transform deviations from the technology field and year specific mean to deviations from the overall mean. This ensures that structural differences across technological fields and over time do not drive the results. The relationships discussed are backed up by econometric models that allow quantifying their average magnitude, assessing their statistical significance, and controlling for a full set of confounding factors.

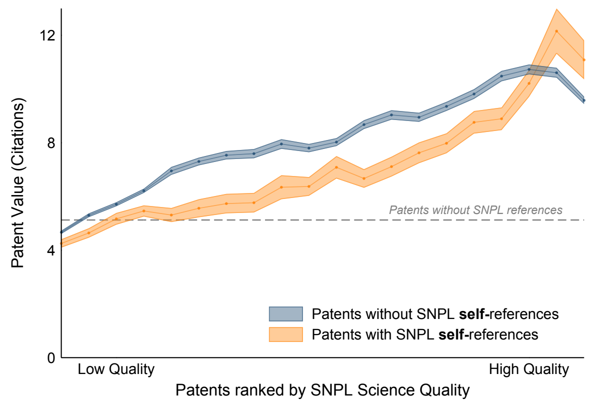
The relationship between SNPL science quality and patent value is depicted in figure 1.1c. We plot the average patent value across the distribution of SNPL science quality. As a first measure of patent value we use the number of patent citations from US patents. We later on consider alternatives. As a benchmark level, the figure shows the average value of patents without any SNPL reference (dashed line). We contrast two possible aggregation methods of SNPL science quality. When a patent references multiple scientific articles, we in a first variant use highest-quality reference as our measure (orange). Here, we juxtapose a second variant, where we consider the average quality of all references. Clearly, top science matters much more, considering scientific material beyond the best one dilutes the science quality-technology value relationship. In the supplementary material, we show that this extends to using other aggregation methods which focus on the top of the quality distribution. Consequently, we continue by only considering the highest-quality SNPL reference.

Previous studies have encountered a higher value of patents with SNPL references or references to other technical literature, in limited samples or specific fields (Branstetter, 2005; Harhoff, Scherer, and Vopel, 2003). We are able to confirm this finding, on a large scale, in our data: the value of patents with SNPL references is higher, or equal than the value of patents without SNPL references, for any level of SNPL science quality except the very bottom.

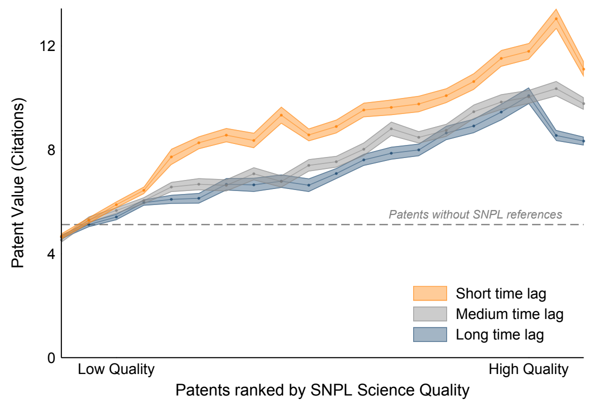
Notably, SNPL science quality fully explains the difference in value between patents with and without SNPL references. Patent value increases rapidly, and almost monotonically, for a higher level of SNPL science quality. Patents with SNPL references at the bottom of the SNPL science quality distribution are on average as valuable as patents without SNPL references. Compared to this group, patents at the top of the SNPL science quality distribution receive more than twice as many forward patent citations. This core result suggests that scientific activities of high quality lead to the development of highly valuable technologies.

Possibly, high quality research and technology development are undertaken by the same individuals or organizations, which may drive the result. Companies, startups, inventors and academic scientists can perform scientific activities that may lead directly to both scientific and technological outcomes (Gittelman and Kogut, 2003). Therefore, we complement this finding exploring how our results vary considering separately SNPL self-references. Figure 1.2a describes the corresponding results. The line in orange indicates the patent value of patents with SNPL self-references. The line in blue describes the value of patents excluding SNPL self-references. The latter presents close to identical results to those obtained in figure 1.1c. Note that part of the SNPL science quality distribution, with the exception of the very top, patent value is higher if patents with SNPL self-references are excluded. In fact, the share of SNPL self-references is roughly similar and, if anything, tends to decrease for higher levels of SNPL science quality. Overall this is supportive of the idea that high-quality science is linked to high-value technology also, and especially, beyond the organizational boundaries within which it is developed.

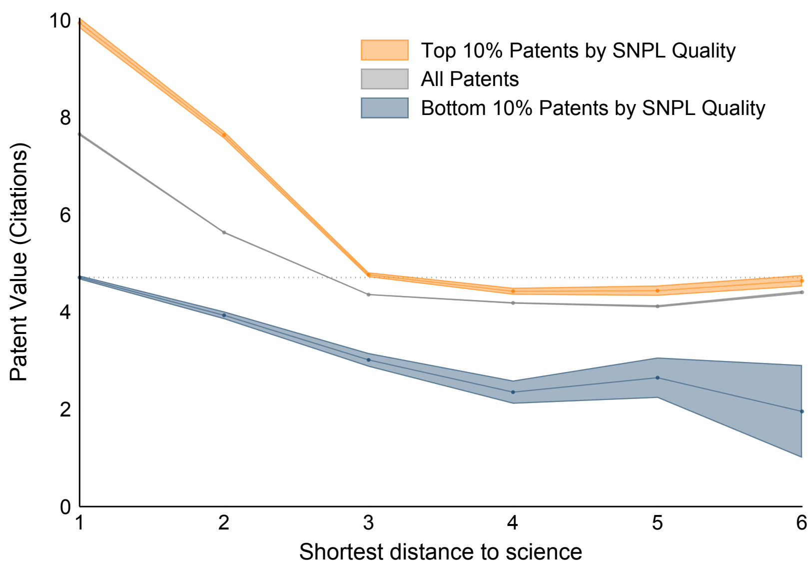
Our analysis so far has focused on patents at the frontier with science, i.e., linked directly to a scientific publication via an SNPL reference. To generalize our findings, we also consider patents connected to scientific publications indirectly via references to other patents. Patents for which the shortest path in the citation network is longer are said to be more distant from the science-technology frontier. Recent



(a) SNPL self-references



(c) Patent value by SNPL science quality and time



(b) Patent value by distance to the scientific frontier and SNPL science quality

Figure 1.2: Additional results

Notes: SNPL science quality is the maximum 3 year citation count across scientific publications appearing as SNPL references in a patent. Patent value is measured as the 5 year count of patent forward citations by US patents. Patent value and science quality are residualized using technology field × first filing year FEs. Shaded areas show 95% confidence intervals around the respective means.
a) SNPL self-references of the highest-quality SNPL reference are considered. N = 4,767,844 patents (948,006 with SNPL references).
b) The distance to the science frontier (x-axis) is measured as the shortest path to a patent with SNPL references in the patent references network. For patents not at the science frontier, SNPL science quality is the maximum SNPL science quality in patents at the frontier to which they are linked. N = 3,816,176
c) Time-distance is measured as the lag between the first filing year of the patent and the publication year of the scientific publication in SNPL references with the the highest science quality. N = 4,767,844 patents (948,006 with SNPL references).

studies have used this concept of distance between science and technology, and demonstrate that the value of patents monotonically decreases for higher distances to the science frontier (Ahmadpoor and Jones, 2017). In Figure 1.2b we consider this dimension and describe the value of patents at different levels of distance from the science-technology frontier. We distinguish patents linked (directly or indirectly) to SNPL references at the top 10% and bottom 10% of quality. We also report the average value of all patents, at different distances. Patents linked to more than one patent with SNPL references at the same distance are assigned to the patent with the highest-quality SNPL reference.

We find that the correlation between patent value and SNPL science quality largely propagates to patents at higher distances from the science-technology frontier. The increase in patent value for a change from the average patent to patents at the top 10% (patents citing other patents with SNPL references to scientific publications of high quality) is approximately equal to the increase in patent value for a step closer to the frontier. For instance, patents at one step of distance from the top 10% have the same value than the average patent at the frontier with science. Patents at any distance from the top 10% always have higher values than patents at the bottom 10%. The difference persists also at a high distance from the frontier, approximately constant and equal to about a 3 times higher value. Regression results in the supplementary material confirm that the positive correlation between SNPL science quality and patent value starts fading only after a degree of distance higher than 6. We can conclude that science of high quality spurs technological progress of high value far beyond the science-technology frontier.

In figure 1.2c we also consider time as a related dimension to distance from science. Time is measured as the lag between the first filing year of a patent family and the publication year of the highest-quality SNPL reference. We study how patent value varies along the SNPL science quality distribution and for different levels of time lag. Interestingly, shorter time lags are always associated with higher patent value. The correlation with SNPL science quality remains strongly positive for all levels of time-distance, but is stronger for patents with short time-distance. As a consequence, at high levels of SNPL science quality, patent value is high, on average, but increases also sharply for shorter time lags. Conversely, at low levels of SNPL science quality the marginal effect of time-distance is small.

So far, we have measured patent value with US forward patent citations. However, the results are robust across a broad set of alternative measures of patent value. First, we consider the count of citations from the EPO. Second, we adopt two indicators of monetary value, available for a subsample of patents. We use estimates from Kogan et al. (Kogan et al., 2017), who propose a measure based on abnormal stock market returns at the patent's grant event as a proxy for its private value. We further obtain inventor survey-based value estimates of patented inventions from the PatVal survey (Giuri et al., 2007). These two measures are only available for a limited sample of patents of about 899k and 11k, respectively. Third, we measure patent scope by the length of the text of the first independent claim. This relies on evidence showing that longer descriptions of the claimed invention implies more narrow legal protection and, therefore, a lower patent value (Kuhn and Thompson, 2019). We consider separately, and when available, the length of the first independent claim in the patent grant publication at the USPTO or the EPO. Table 1.1 reports descriptive statistics based on the average of all these alternative patent value indicators for patents without SNPL references, and for patents in the top 10% and bottom 10% of SNPL science quality. We replicate regression results for all these alternative measures of patent value in the supplementary material.

Table 1.1: SNPL science quality and alternative measures of patent value

	No SNPL	Bottom 10%	Top 10%
US citations			
Mean	**5.125**	**4.928**	**10.175**
Standard Error	(0.004)	(0.022)	(0.058)
N	3,471,621	84,406	84,808
EP citations			
Mean	**0.947**	**0.750**	**2.078**
Standard Error	(0.001)	(0.012)	(0.016)
N	3,471,621	84,406	84,808
Kogan et al. (2017) (USD)			
Mean	**13.326**	**12.517**	**16.704**
Standard Error	(0.044)	(0.625)	(0.469)
N	700,613	8,866	13,811
PatVal (EUR)			
Mean	**11.929**	**8.277**	**24.450**
Standard Error	(0.451)	(3.226)	(4.992)
N	8,507	349	227
US claim length			
Mean	**185.532**	**179.467**	**178.012**
Standard Error	(0.082)	(0.456)	(0.496)
N	1,956,651	65,921	69,939
EP claim length			
Mean	**143.905**	**140.782**	**129.188**
Standard Error	(0.084)	(0.335)	(0.456)
N	1,159,049	42,534	29,972

Notes: The table presents descriptive statistics for all considered measures of patent value. It reports average values for patents without SNPL references, with SNPL references in the bottom 10% and in the top 10% of science quality. Patent value and science quality are residualized using technology field × year FEs. Elasticities from corresponding regression analysis are available in the supplementary material.

1.4 Conclusions

The quality of scientific contributions is often measured in terms of their impact within the scientific community. Yet, scientists also need to gauge and acknowledge of their contributions for society and future technical and social advancements. The fact that science quality is practically defined within the realm of science itself, contributes to a perception of science as being an independent upstream activity, at times detached from technological progress, with an indirect and delayed impact on society at best.

To the contrary, our study suggests that such an interpretation of the relationship between science quality and technology would largely be a misconception. We show that excellent science is directly linked to inventions of particularly high value. More specifically, our findings demonstrate that there is a robust and strong relationship between the scientific quality of a publication referenced in a patent and the patent's impact and commercial value.

Our results are descriptive, and the exact causes of the strong correlation will have to be analyzed in future work. At this point, it seems most reasonable to presume that industrial users of scientific insights scan the science sector for novel results and employ the ones that are most promising for applications in their industrial fields. We doubt that they do so merely on the basis of science citation counts or impact measures. Rather, we expect that they apply their own complex logic and assessments, and that they may even avoid using the classical metrics of the science sector altogether. Commercial investments are unlikely to be made on the premise the citation-measured interest in the scientific community was sufficiently high. Hence, the high correlation between quality measures used in the science sector and those used in the commercial (patent) realm are fortuitous. They are highly unlikely to reflect a spurious selection result.

Leaving aside the exact causal links, our results provide intriguing evidence for the governance system of science, e.g. at universities and public research organizations, as well as for funding agencies and science policy-makers. The current system steers researchers to strive for success measured in terms of citations and impact. According to our findings, the outcomes of such a system are well-aligned with later stages of technology development and translation of science results. Our study does not provide evidence on the optimality of the alignment. However, it clearly contradicts the notion that the application of scientific criteria in science funding decisions would lead researchers to engage in exercises that are of little value to society at large. Quite to the contrary, science quality (as measured by scientists) is a strong predictor of applicability and practical value of the technologies developed as the fruits of scientific endeavor. Paradoxically, when making commercial investment decisions, taking academic measures such as citation counts or impact factors into account may not be a bad idea.

2

Firm Participation and Knowledge Diffusion in International Science

Abstract *We study the diffusion of knowledge from scientists to firms within scientific communities. We look at conference proceedings as "paper trail" of scientific communities activities with a unique dataset of almost all relevant conference series in computer science since 1996. More than 5000 firms appear as conference sponsors or as affiliations in proceedings. Their participation is concentrated in the highly ranked conferences and their scientific contributions are on average highly cited. We exploit direct flights as an instrumental variable for the participation of scientists in conferences where a firm participates and other similar conferences. The participation in the same conferences has positive effects on knowledge diffusion to the firm's scientific and inventive activities. Knowledge diffusion may result from lower search costs, requiring presence but otherwise minimal investments. Conversely, firms may need to invest in intense and active participation to gain reputation, show reciprocity, and set off effective knowledge sharing interactions. Testing this, we show that collaboration with external scientists appears a key mechanism of diffusion. Moreover, the effects are remarkably stronger the larger the firm's investments.*

2.1 Introduction

The Advances in Neural Information Processing Systems (NeurIPS) is a leading academic Machine Learning (ML) conference. In 2017, Google featured as an official sponsor and, with 75 published proceedings, was, by far, the most represented affiliation of scientists. Other firms such as Microsoft, IBM or Tencent follow not too distant.[1] Science is a driver of innovation (Fleming, Greene, et al., 2019) which, at least in the common imagination, is often conceived as a source of accessible knowledge by virtue of the publication norms. Firms active participation may merely respond to marketing objectives. Most management and economics literature would dismiss such perspective. R&D (Cohen and Levinthal, 1989), hiring (Almeida and Kogut, 1999), location (Alcacer and Chung, 2007; Audretsch and Feldman, 1996), and collaborations (Almeida, Hohberger, and Parada, 2011) decisions concurr to access external knowledge. In surveys, conferences score highly in importance as channels of knowledge diffusion (Cohen, Nelson, and Walsh, 2002). However, the extent to which firms participate in scientific communities' activities and the resulting dynamics of knowledge diffusion remain largely unexplored. As firms fund increasingly less basic research internally (Arora, Belenzon, and Patacconi, 2018), further understanding the interface between science and industry gains importance.

The participation in scientific communities is endemic to modern science (Mokyr, 2002). Moderate firms investments in participation may, in principle, suffice to abate search costs in the increasing body of codified knowledge (Jones, 2009; Fleming and Sorenson, 2004). To this theory, we add and juxtapose the notion that, in the absence of market mechanisms, a reputation-based system governs scientific communities. Knowledge diffusion is embedded in a process of socialization which is facilitated by temporary proximity, but ultimately requires the ability to establish personal connections. Active and intense participation is necessary to gain reputation and comply with norms that ease social relationships (Stephan, 1996; Dasgupta and David, 1994; Crane, 1974; Merton, 1973). Scholars such as Rosenberg (1990), Hicks (1995), and Cockburn and Henderson (1998), lead the way in this line of investigation but specific evidence has been rare.[2]

In this paper, we study how knowledge diffuses from scientists to firms that participate in the same scientific communities. We leverage information from conference proceedings as a "paper trail" of the participation of scientists and firms to distinct scientific communities. We assemble a unique dataset of all most relevant scientific conferences in Computer Science (CS), worldwide, from 1996 to 2015. CS is an ideal setting for our study, additional to its economic relevance (Brynjolfsson and Hitt, 2003; Nelson, 1962). Detailed information on conference series is captured in well-curated datasets, due to the importance of conference proceedings for scientists (Franceschet, 2010). In our main specifications, we capture knowledge diffusion using citations from firms' publications or patents to scientists' proceedings. Our setting is analogous to studies on the effect of proximity on knowledge spillovers (Jaffe, Trajtenberg, and Henderson, 1993; Singh and Marx, 2013), but proximity of firms and scientists is given by the

[1]Further information available at https://nips.cc/Conferences/2017.

[2]There are notable exceptions: Gittelman and Kogut (2003) and Gittelman (2007) look at the trade-off between scientific and innovation performance of firms in biotechnology; Cassiman and Veugelers (2002), Cassiman and Veugelers (2006), and Simeth and Raffo (2013) study how the relevance of academic knowledge influence firms' strategy; Vlasov, Bahlmann, and Knoben (2016) look at the association between small and medium-sized firms' innovation and the knowledge diversity of conferences where they participate; (Foerderer, 2020) looks at the effect of the participation to a developers conference on firms app development and collaborations.

intensity of participation of firms and the participation of scientists to different scientific communities, rather than geography.

We are mainly interested in and able to observe participation defined as the active contribution to scientific conferences, as opposed to passive attendance. We capture the two possible forms of firms' participation: the authorship of proceedings by firms' affiliated scientists and the sponsorship of conferences. Firms' scientists as authors of proceedings participate in a similar way as academic scientists do. Sponsorship entails a specific financial contribution to the conference and gives in turn additional opportunities to promote the reputation of the firm, space to expose its research and for hiring activities, and additional entrance tickets. Our understanding of this phenomenon is enriched by interviews with participants at two important conferences.[3] We discuss in the paper how both authorship and sponsorship closely reflect a significant investment and engagement. A higher number of proceedings and sponsorship decisions serve also as a proxy of the intensity of participation of the firm, implying larger investments and stronger presence at a conference.

For a given sample of proceedings at a conference where a firm participates, we consider a counterfactual group of proceedings from another comparable conference (in the same year, the same field, comparable quality and size). Participation is then defined as the participation of other scientists, revealed by the actual presence of a proceeding in the same conference. To establish causality, our econometric models isolate exogenous variation in the probability of this participation arising from the availability of direct flights, as a proxy of general costs of transportation (Giroud, 2013; Catalini, Fons-Rosen, and Gaulé, 2020). As a safeguard against the risk of violation of the exclusion restriction, the connectivity of firms and scientists to conferences seldom coincide with their direct connectivity. By means of FEs controls at different pair-levels of analysis, we can rule out the most plausible concerns to identification. This includes, for instance: the still possible correlation between direct flights to the same conferences and the connectivity between participants (e.g. if scientific communities are geographically localized); time-invariant and time-variant shocks to the innovation potential of the regions of origin of scientists and firms (Giroud, 2013).

Our data portray 7298 conferences from 1042 conference series and 5470 participating firms. Firms' authorship occurs in 88.3% of the conferences and sponsorship in 26.6%, at a rather constant rate over time. Firm participation is concentrated in the conference series of the highest quality. Firm authored proceedings are a relatively small share of the total (10.9%) but are, on average, highly cited. The participation frequency and intensity are highly skewed, with the top firms being responsible for the majority of the observed participation.

The probability of knowledge diffusion from scientists to both scientific and inventive activities of firms is significantly higher after participation in the same conferences. These effects are large, of the same order of magnitude of the correlation with indicators that reveal the direct relevance of a scientist´s research for firms (e.g. previous citations, research similarity), which add as covariates. The probability of citation after the conferences increases also for previous publications of authors at the same conferences. Since these are publications already public long before the conference, this suggests that actual interactions matter. To provide direct additional evidence, we show that these effects are paralled by a large increase in the probability of collaboration. At the same time, interestingly, we find a rather null effect on hiring,

[3]We visited the ECCV conference 2018 in Munich and the NeurIPS conference 2019 in Vancouver.

measured by changes in affiliation, on average. Indeed, this and further robustness analyses demonstrate that the knowledge flows and collaborations we observe are largely explained by scientists that remain external to the firm.

According to our theory of reference, firms that make it to the top of the prestige distribution would face ever increasing returns. Stronger contributions would increase reputation, and further ease knowledge sharing and the establishment of collaborations. Conversely, moderate levels of participation may suffice if only the access to information presented matters. We find that all effects increase greatly with the participation intensity of firms. The effects are weak or null for firms with only one proceeding presented and or exclusively sponsoring the conference. At the opposite extreme, firms that are both sponsor and author several proceedings, participation has the strongest effects, being positive and significant on all outcome variables, including hiring.

In robustness analyses we replicate all results using a measure of text-similarity of scientific publications, other variants of citations-based measures, and test alternative FE specifications. In appendix, we show event study analyses for the effect of direct flights on participation, in a setting where observations are not conditional on the participation of scientists to any specific conference. We also study the effect of participation on yearly-constructed outcome variables, using documents published before the conference to build pre-period measures. We find no sign of anticipation of the effects, providing confidence in the causal interpretation of the results. Finally, standard errors can be clustered at various levels of analysis, and including two-way clustering, with no implications for the significance of the results.

Our study makes two main contributions. First, we offer novel evidence to understand the relevance of participation in scientific communities and the rationale for corporate science, more broadly (Arora, Belenzon, and Sheer, 2020; Simeth and Raffo, 2013). Second, recent work demonstrates that opportunities for temporary proximity can have large effects on knowledge spillovers (Campos, Lopez de Leon, and McQuillin, 2018; Lopez de Leon and McQuillin, 2020; Chai and Freeman, 2019; Boudreau et al., 2017). We bring attention to the role of organizations' investments as antecedents to such opportunities. Our findings reinforce the evidence on the role of temporary proximity but support a theory that defy the notion of knowledge diffusion as free and evenly distributed spillovers, even in these contexts (Breschi and Lissoni, 2001). An uneven pattern of diffusion emerges, that more plausibly leads to divergence and concentration, rather than convergence, in firms innovative productivity (Andrews, Criscuolo, and Gal, 2015; Autor, Dorn, Katz, et al., 2020).

2.2 Participation and Knowledge Diffusion

A fundamental tenant in the economics of innovation is that knowledge does not flow spontaneously (Akcigit, Caicedo Soler, et al., 2018; Jaffe, Trajtenberg, and Henderson, 1993). Geographical and technological boundaries, combined with an ever-increasing body of cumulated knowledge, determine high search costs for individuals and firms (Jones, 2009; Bloom, Jones, et al., 2020; Rosenkopf and Nerkar, 2001). Science operates in communities, united by a common interest, that share knowledge systematically (Knorr-Cetina, 1999; Crane, 1974). As such, the structure of science constitutes a way to organize and navigate existing knowledge (Fleming and Sorenson, 2004). This structure is manifested in international conferences, that turn into loci of controlled spillovers of specialized, up to date, and temporary localized knowledge (Bathelt and Cohendet, 2014). Participation helps abating search costs within a body of knowledge which is defined along some dimension (e.g. scientific area), but otherwise scattered in a variety of dispersed, and possibly unknown, locations (Maskell, 2014; Bikard and Marx, 2020). Recent literature contributions have found empirical evidence on the effect of temporary proximity at conferences on the probability of knowledge flows and collaborations between scientists (Campos, Lopez de Leon, and McQuillin, 2018; Lopez de Leon and McQuillin, 2020; Chai and Freeman, 2019; Boudreau et al., 2017).

Active participation to the same community is helpful, because the mere physical presence or reading of proceedings at conferences may not naturally translate into knowledge diffusion, which is instead embodied in effective voluntary interactions (Hicks, 1995). Knowledge production within a community is a collective process where active participation and compliance to social norms are expected (Merton, 1973). Reciprocity and the reputation gained from previous contributions become the currency that aligns incentives for knowledge production and sharing (Stephan, 1996; Dasgupta and David, 1994). The willingness to share knowledge may remain anchored to cost and benefits considerations based on expected reciprocity (Bobtcheff, Bolte, and Mariotti, 2017; Stein, 2008; Mukherjee and Stern, 2009). At the same time, sharing decisions are not based exclusively on contingent negotiations but rely on a sense of community and the prestige of individuals and organizations. There is experimental evidence that group identity (Chen and Li, 2009; Charness, Rigotti, and Rustichini, 2007) and status clues (Bhattacharya and Dugar, 2014) influence cooperative behavior of individuals. Survey evidence also shows that both academic and industry scientists take into account expected reciprocity and the perceived conformity to the norms of science in knowledge sharing decisions (Haeussler, 2011; Haeussler et al., 2014).

This theoretical perspective has implications for the expected role of participation of firms to scientific communities and the understanding of the related mechanisms of diffusion. Firms need to make investments in absorptive capacity to take advantage of external knowledge (Cohen and Levinthal, 1989). As science unfolds as a sequence of inter-independent hypotheses on cause and effect relationships, actively engaging in science is instrumental in making sense of intermediary and external results (Hellmann and Perotti, 2011; Arora and Gambardella, 1994). However, the role of absorptive capacity alone justifies internal research. If the existence of search costs was the only reason, participation in external scientific communities would be subject to evident decreasing returns to investments. The trade-off between positive and negative spillovers due to disclosure would likely remain binding (Cassiman and Veugelers, 2002; Cassiman and Veugelers, 2006; Laursen and Salter, 2006) and excessive focus on science may divert the firm from technological output and profit objectives (Gittelman, 2007; Gittelman and Kogut, 2003).

Conversely, firms may face steep increasing returns to investments in participation. As a consequence of the accumulation of prestige as a form of capital (Matthew effect), social structures of scientific communities are highly skewed, with few individuals and institutions in positions of great influence (Merton, 1973; Stephan, 1996). Scientific meetings become not simply temporary gatherings for knowledge sharing, but also regular occasions to demonstrate participation and commitment and, ultimately, the confrontation fields where these social structures gradually take shape. Individuals and institutions at the far end of the prestige distribution obtain disproportional gains in visibility and are in a privileged position to collaborate and absorb knowledge (Gittelman, 2007; Gittelman and Kogut, 2003). Competition for these positions, and the notion that existing social structures may require substantial effort to be scratched, justify intense active participation. Absorptive capacity, in this sense, becomes a function of the same reputation-based system that governs scientific communities, rather than of pure firm-level cognitive mechanisms (Cockburn and Henderson, 1998).

2.3 Research Context

Computer Science Conference Series

Newell, Perlis, and Simon (1967) defined Computer Science (CS) as “the study of computers and the major phenomena that surround them”. This research area has grown to include a variety of heterogeneous sub-fields spanning the spectrum between basic and applied research, of primary economic relevance of this field of research is undeniable (Brynjolfsson and Hitt, 2003). CS is also an ideal setting for our study for the role of conferences. Conference proceedings constitute a primary outlet (Franceschet, 2010). They are peer-reviewed and the acceptance process is competitive.[4] As a consquence, presenting the same manuscript at several conferences is rare and considered unethical. Also, proceedings and conferences information in CS are better covered in existing databases. This allows observing conferences on a large scale. Importantly, most organizers attempt to ensure the participation of authors, for instance, conditioning the actual publication of proceeding papers on the physical participation of at least one author.[5] This guarantees that proceedings information will largely reflect the actual composition of active participants, at least of one member of a team of coauthors.

In CS interactions and feedback loops between basic scientific advancements and insights from technological applications have been frequent (Nelson, 1962).[6] Scientific contributions are also often cited in patents (Ahmadpoor and Jones, 2017). However, the field is neither an outlier for the importance of scientific conferences for scientists (beyond the specific value of the publication of proceedings), nor for their relevance for the downstream industries (Cohen, Nelson, and Walsh, 2002), nor for the presence of firms at conferences. Cohen, Nelson, and Walsh (2002) find from responses to the Carnegie Mellon Survey that conferences score similarly high across most industries as a channel of knowledge diffusion from public research to corporate R&D. Computers and semiconductors industry figure among them, but, if anything, they fall behind several other industries.[7]

We also explored descriptively the presence of firms at scientific conferences across different fields, in Scopus data. Figure B.8 in the appendix presents the related results. In CS, 7.5% of all proceedings are associated with firms. This share varies by fields: 9.7% in Physics, 11.3% in Engineering, 5.2% in Biochemistry/Genetics, 17.9% in Earth and Planetary Sciences. With this observation in mind, we note that CS is not an outlier when it comes to the involvement of firms with scientific communities at international conferences. Outside of CS however, coverage and availability of complementary information

[4]Based on publicly available information for a subsample of conferences, the acceptance rate is on average 21% at $A^\star$ conferences (N=333) and around 36% at B or lower conferences (N=988). Detailed data available upon request.

[5]For instance, the IEEE recommends the exclusion from or limitation of distribution of papers which were not presented at the conference. https://www.ieee.org/conferences/organizers/handling-nonpresented-papers.html

[6]High levels of industry participation in the field ML, in particular, has been sharply increasing in the last 10 years (Hartmann and Henkel, 2020).

[7]In computers and semiconductors, 37.9% and 48% of respondents indicated conferences to be important, respectively. Only publications and reports scored higher. This percentage is higher, for example, in petroleum (50%), drugs (64%), steel (54.6%), machine tools (45.5%), Aerospace (51%), and it is similar in several others (Cohen, Nelson, and Walsh, 2002).

(e.g. conference rankings) is severely diminished. Due to these practicalities these figures remain purely indicative. For the same reason, we refrain from extending the sample which would come at the cost of lower data quality.

Firms' Participation: Authorship and Sponsorship

To better understand the nature of firm participation activities we gathered information on conference websites and we attended and interviewed participants at two major conferences: the European Conference on Computer Vision 2018 (ECCV, https://eccv2018.org/) in Munich, Germany and the Neural Information Processing Systems conference 2019 (NeurIPS, https://nips.cc/) in Vancouver, Canada. We interviewed more than 50 participants, between scientists and other representatives, of more than 20 firms and about 20 academic scientists. Key findings are reported here and as notes related to quantitative findings later presented.

The authorship of proceedings occurs normally as for other scientists, via submission and peer-review. The appearance of firm scientists as authors of proceedings largely coincides with their presence at the conference. Firm scientists present their work and normally interact with their academic and corporate peers. Almost all scientists of large firms we interviewed (with only one exception) declared to enjoy significant freedom to participate in conferences and that the acceptance of a proceeding constitutes a sufficient condition for all authors to have the support to participate. Scientists from medium to smaller firms also declared to enjoy similar conditions, only with more binding budget constraints. More generally, at the conferences we attended, the presence of firm scientists was staggering. In programs of these and other conferences they also appear in scientific and organization committees, chairs and discussion roles.

For sponsorship, both firms and academic institutions can apply and allocation of slots happens on a first-come-first-served basis. Fees range between a few thousand USD up to $80'000, depending on the conference and sponsorship category.[8] The benefits go from: the exposure of the company logo on the conference website, gadgets or venue; to access to exposition space; the possibility to submit applications for organizing talks, discussion panels, demos or workshops; and recruiting opportunities. Sponsors also have the right to additional entry tickets. In most cases, one or more employees among HR personnel and scientists represent the firm at a booth. Here they provide information and disseminate material on the firm research and careers opportunities. Large sponsors frequently organize workshops, tutorials, receptions and social gathering events.

To conclude, firms' participation, as we observed it, constitutes an actual firm-level investment and an engagement into the activities of scientific communities. The size of investments can vary substantially, and is closely correlated with the number of proceedings presented and sponsorship decisions. [9] Passive

[8]For instance, the NeurIPS conference provides 5 sponsorship categories: Diamond ($80'000), Platinum ($40'000), Gold ($20'000), Silver ($10'000), Bronze ($5'000). In 2017, the conference attracted 84 sponsors with contribution fees totaling $1.76 million, an increase of 31.5% from the previous year, where 64 sponsors contributed for a total of $840'000). Source: https://medium.com/syncedreview/a-statistical-tour-of-nips-2017-438201fb6c8a

[9]As an indicative example of the participation investment of a large firm, we take the participation of Google at the Neural Information Processing Systems (NeurIPS) conference in 2017 for which sponsorship costs are publicly available https://medium.com/syncedreview/a-statistical-tour-of-nips-2017-438201fb6c8a. Google figured as a second-tier sponsor of the conference, which corresponds to a price of $40'000. Seventy-five proceedings presented at the conference were authored by 86 distinct scientists. We can assume that 50% of them participated in the conference. We assume 5 participants from HR

attendance may occur but it did not appear to be the norm. Finally, specific firm-level processes have emerged from our interviews, complementary to the participation, such as the internal knowledge sharing of inputs from the conferences. These and other aspects are further discussed in Appendix B.5.

2.4 Data

We combine various data sources on conference series in CS and their participants between 1996 and 2015. Our primary objective is to cover a highly representative sample of all relevant conference series in CS with information relevant to our analyses. We make specific efforts and leverage data that allow us to reach sufficient disambiguation of conference series, firms and scientists. Table B.1 in appendix lists our data sources with related information. Appendix B.1 offers a visual description of the connections between these datasets.

We obtain the central information on conferences from the Digital Bibliography & Library Project (DBLP). This is a database specialized on proceedings and publications in CS maintained at the University of Trier, Germany. We complement it with information from Web of Science (WoS) and Scopus, on authors' affiliations, conference sponsors, citations and abstracts. The match between the two is highly precise, based on the DOI, when available, or key bibliographic information. We add information on conference series quality and CS research subfields from the Computing Research and Education (CORE) data, curated by the Computing Research and Education Association of Australasia. The CORE data classify, based on experts' assessment, all relevant conferences series in CS into the quality-rank levels $A^{\star}$, A, B and C and subfields. The match with CORE data is also highly precise, being done largely manually, with the support of text similarity indicators. Patent level information is from PATSTAT. One cornerstone in our data is an additional dataset where references to scientific articles in NPL citations are singled out and linked with bibliometric records in both WoS and Scopus (henceforth SNPL data). The construction of this dataset is described in Knaus and Palzenberger (2018) and Poege et al. (2019).

Data on airports and flight connections comes from the International Civil Aviation Organization (ICAO) and the US Bureau of Transportation Statistics (BTS). The ICAO data covers international flights only. Since the US is one of the most important geographic areas in CS and flights are very important for US domestic travel the BTS data are an important complement. Both data sources come with a definition of market regions, usually the name of a city. We geolocalize all conference venues and scientists' affiliation and map them to airport regions. We reach a total of around 1'100 relevant airport regions.

We match affiliations, sponsors as well as patent applicants with a custom database of firm names from ORBIS, the Global Research Identifier Database (GRID) and the EU scoreboards. From ORBIS, we take any patenting firm as well as any firm in the US and Germany (including subsidiaries). This is a convenience sample, designed to identify all possible firms active at the conferences in our sample. The matching is based on a supervised machine learning algorithm that combines, as input, information from

personnel, for a total of 48 participants. The conference took place in Long Beach, California and lasted for 6 days. We assume a travel cost of \$130, a daily cost for accommodation and expenses of \$200 per person, and an average yearly wage of \$120'000, to divide by 260 working days. These assumptions are conservative, and neglect expenses related to the preparation and submission of proceedings and other general costs for the preparation of the material, the booth and conference activities. This sums up approximately to \$260'000. Google, in 2017, participated, with varying intensity, to more than 160 conferences. Moreover, at the NeurIPS conference 2019 the large firms interviewed declared to have from 100 to up to 200 affiliated participants at the conference.

search results for firm names in the search engine Bing (following the approach by Autor, Dorn, Hanson, et al., 2020) and string similarity. We invest substantial additional manual post-processing efforts to further disambiguate firms. We aggregate subsidiaries at the level of the corporate group.

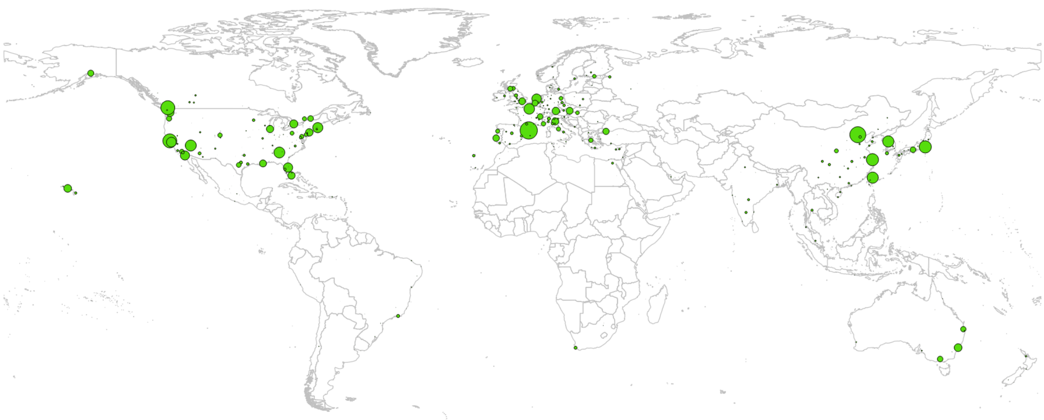
The data are representative of all relevant conferences in CS in our period of observation. The data cover 75% of all conference series listed in CORE and the highest share (80%) of CORE conferences not in our data are of the lowest quality rank, C. This implies that the data cover almost the entirety of top and medium ranked conferences and is biased against small and short-lived conference series of the lowest quality.

The sample is restricted to the period between 1996 and 2010 for practical reasons. Scopus is available to us from 1996 and we allow for a five-year window to observe dependent variables after the conference for a sufficiently long period, without truncation up to 2016 (the last year for which we dispose of citation information). This sample comprises 5470 firms, 7298 conferences pertaining to 1042 conference series, and a total of 612103 proceedings. We cover in greater detail information on data sources and the construction of the dataset in Appendix B.1.

2.5 Descriptives

We present basic descriptive information on our data, with a focus on the characteristics of conferences in our sample and the level of participation of firms. Figure 2.1 shows that conferences in our sample are distributed worldwide with a prevalence of European and North American locations, as expected, but also East Asia. Table 2.1 reports the information on the sample of conferences. High-ranked conference series are fewer, but they are longer-lived and larger. Consequently, the total number of conferences in the sample is lower for A* and A conferences. However, the average number of conferences per conference series is higher for higher ranks.

Figure 2.1: Location of conference events



Notes: Frequency-weighted conference airport regions are shown. The data is based on the estimation sample, counts for the years 1996-2010 for conferences where at least one firm was present are aggregated.

Proceedings receive on average 3 scientific citations on a period of 5 years varying from 1.5 up to 9.8, for C and A* conferences, respectively. Patent citations are relatively rare, on average 0.15 per proceeding. It is perhaps more surprising to encounter also here a more than threefold increase, from 0.08 to 0.29, of citations from C to A* ranked conferences. When restricting to patent citations by firms in our sample, around 5.1% of proceedings are ever cited by a patent. For $A^\star$ conferences, this number rises to 8.2%.

Table 2.1: Conferences information by rank

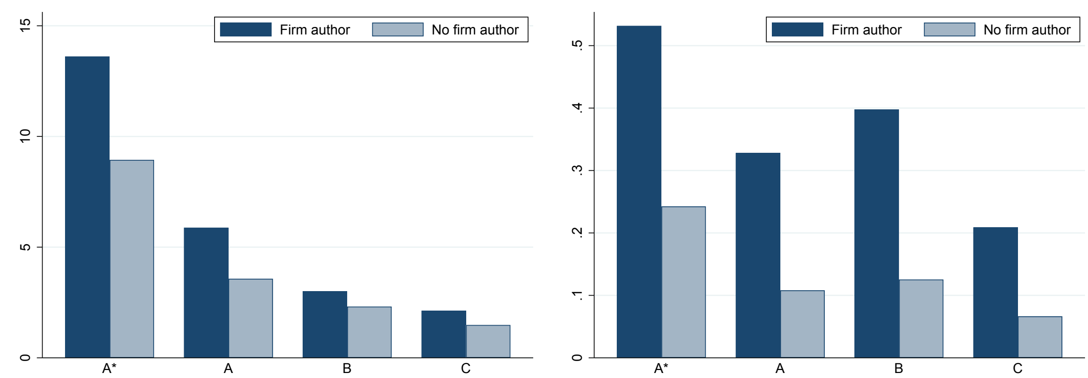
Rank	$A^\star$	A	B	C	Total
Conference series - Total	68	199	355	415	1037
Conference events - Total	824	1875	2597	2002	7298
Firms participating - Avg. n. per conference event	7.81	6.12	6.30	4.15	5.84
Firms sponsoring - Avg. n. per conference event	7.19	5.49	5.68	3.55	5.22
Proceedings - Avg. n. per conference event	89.17	84.93	90.42	72.21	83.87
Share of firm-authored proceedings - (%)	17.68	10.78	10.93	7.43	10.87
Science citations (5y) - Avg. n. per proceeding	9.77	3.82	2.39	1.53	3.45
Patent citations (5y) - Avg. n. per proceeding	0.29	0.13	0.16	0.08	0.15

Notes: Data for years 1996-2010 is used.

Looking at the participation of firms, 88.3% of all conferences have at least one firm author and 10.9% of all proceedings have at least one firm as author affiliation. Also, 26.6% of conferences, corresponding to 29.6% of proceedings, have at least one firm as sponsor. From Table 2.1, the average number of firms participating as authors' affiliations at conferences and as sponsors is 5.8 and 5.2, respectively. It is interesting to note that the intensity of this participation increases strongly with the quality-rank. In percentages, in $A^\star$ conferences, 17.7% of contributions are by firms. At levels A and B, around 10.8% of contributions are by firms and at C-level, only 7.4% of contributions are by firms. Of $A^\star$ conferences, 39.8% are sponsored by at least one firm, down to 22.2% at C-level.

Figure 2.2: Citation counts by type of authors' affiliation and conference rank

(a) Science citations (b) Patent citations



Notes: Counts forward citations by any CS paper or proceeding (2.2a) or by any patent family (2.2b), by conference quality and by author status. A citation window of five years for conference proceedings published in 1996-2010 is used. Cf the overall averages in table 2.1.

While comprising only 10.9% of the total, firm authored proceedings stand out in terms of quality. Figure 2.2 (a) shows the average count of scientific citations to proceedings with and without firm affiliated scientists, by conference rank. Firm proceedings are of exceedingly high quality, for any level of conference rank. For instance, within $A^\star$ conferences, firm proceedings receive on average 5 more forward citations within five years compared to non-firm proceedings, that receive 9 citations on average. The results are purely descriptive but suggest that firms have a scientific impact and gain considerable attention within the scientific communities where they participate. [10] At the same time, figure 2.2 (b) shows that firm authored proceedings are also cited in patents at a much higher rate. The number of patent citations decreases for lower conference ranks, but less sharply than scientific citations.[11]

Table 2.2: Firm information

Variable	Mean	SD	25%	50%	75%	90%	99%
Firm Scientists	18.40	155.70	1	2	6	17	315
Conference participations	8.15	61.20	1	1	3	8	139
Conference sponsorships	0.86	9.87	0	0	0	1	11
Conference proceedings	13.72	163.82	1	1	3	9	205
in collaboration w. academics	7.14	82.13	0	1	2	5	105
in collaboration w. other firm	1.53	14.27	0	0	1	2	25
Firm Patents citing proceedings	8.24	100.02	0	0	0	3	160
Observations	5224						

Notes: Descriptives at the firm level for the 1996-2010 period. Scientist number is the maximum yearly number in this period. Conference participations/sponsorships count firm-conference events. Conference proceedings instead count the total number of proceedings of that firm, first overall and then broken down by type. Lastly, the number of patent families citing a proceeding is counted.

Finally, Table 2.2 presents information at the firm level. On average, about 18 different scientists have worked for a firm in our sample and authored at least one proceeding. They have authored about 13 proceedings presented at 8 distinct conferences. Firms have then sponsored 0.86 conferences and filed 8 patents citing one or more proceedings, on average. Interestingly, a relatively large share of proceedings (7 out of 13.7, on average) is coauthored with academic scientists. On the other hand, collaborations with other firms are rarer (1.5 proceedings). Still, a significant share of proceedings (about 45%) is authored exclusively by scientists affiliated to one single firm. Importantly, the distribution of these firm-level indicators is extremely skewed. Firms below the median had only 2 active scientists or less, they have authored only one proceeding, they never sponsored an event, and have no patents with citations to proceedings. The large share of activity is observed above the median and, even more, at the top quarter, top 10% and top 1% of the sample (roughly 1250, 500 and 50 firms, respectively).

We present in Appendix B.3 the variation over time of some key descriptive statistics. Overall, not surprisingly, the number of conferences and proceedings grew substantially (B.9a). The fields of Artificial Intelligence and, secondly, Information Systems have grown the most. The increase in the number of conferences in non-US locations has been more prominent (B.9b). In 1996, half of all conferences were taking place in the United States, by 2015 it was about a quarter. In terms of composition, the figures discussed above on the level of participation of firms have remained fairly constant over our period of observation. Interestingly, the share of proceedings in collaboration between academia and firms has increased steadily, but not collaborations between firms that instead remained rare and constant in share.

[10] This result resonates well with qualitative evidence from our interviews. Most of the firms declared to have internal peer-review systems to guarantee that the work they present is of above-average quality within the events where they participate in. Some firm scientists have also expressed the idea that, differently to academia, they had no pressure to publish and they would focus on presenting work that they deemed of high impact (from a scientist of a startup in the field of ML: "We are not in a publish or perish mode").

[11] In regression analyses in the appendix table B.8, we test the robustness of these correlations to conference series and also conference events FE. The analysis confirms that also within conference series and single conference events, proceedings authored by firms receive more citations. This is particularly the case when the firm is also a sponsor of the event.

2.6 Econometric Strategy

2.6.1 Econometric Model

Our empirical analysis focuses on the effect of participation in the same conferences on knowledge diffusion from scientists to firms. A first challenge is that we only observe realized conference participations. Second, both participation of firms and scientists in conferences is determined endogenous firms and scientists preferences. In simple comparison of knowledge flows from scientists to firms participating and not participating in the same conferences it would be impossible to distinguish the effect of participating in the same conferences from the unobserved factors that determine the selection of a conference in the first place. To test our hypothesis we must address this fundamental inference problem.

We work with a dataset of pair-level observations of firms and proceedings. We maintain all firm-proceeding pairs for proceedings presented at conferences where a firm participated. We then add, for each conference, a counterfactual group only of proceedings presented at similar conferences. We create strata of conferences in the same year, rank, subfield, and within the same size and forward-citations count categories.[12] We retain up to two other conferences selected randomly within the same strata. The results are robust to selecting only one or more than two. We verified that the average difference for any observable between matched conferences cannot be distinguished from zero.[13]

The matching process generates variation in the data between proceedings presented and not presented at the conferences where the firms participated. The proceedings not presented at the same conferences also serve as a counterfactual to the observed participation of scientists authors of the proceedings, aligned to their revealed interests. However, the matching procedure does not solve the endogeneity problem. Other unobserved factors, besides those considered, that correlate with both the decision to participate as well as the likelihood of knowledge diffusion, are likely to exist.

To address endogeneity we employ econometric models that isolate exogenous variation in the probability of participation of scientists. The availability of direct flights to the conference venues from the location of scientists provides such variation. Direct flights tend to reduce costs, travel time and eliminate layovers. The presence of direct flights is also more likely associated with airport pairs where competition between airlines is higher, therefore reducing the price also of other options. This affects the general costs of transportation and thus the probability of participation. Flight connections have been used before as proxy for the cost of physical individual interactions within firms (Giroud, 2013), between scientists (Catalini, Fons-Rosen, and Gaulé, 2020) and as a determinant of cities economic growth (Campante and Yanagizawa-Drott, 2018). Our study differs as we observe the endogenous variable - participation in conferences - and use direct flights as an instrumental variable. Figure B.7 in appendix represents our empirical setup graphically in a stylized scenario.

[12]To create these categories, we coarsen the conference size into three categories (≤25%, ≤50%, >50% of the largest conference within the group) and forward citation counts using a median split for A* and A-ranked conferences and quartiles for B and C-ranked conferences.

[13]Results of this analysis are presented in table B.6. Note that each conference in the sample is randomly pared to one or more other conferences within the same sample (and according to the matching strata). Consequently, this analysis is not a test of equivalence of matched conferences. It is purely aimed at excluding a malfunction of the matching algorithm and random selection.

We implement a two-stage regression model. The main endogenous variable of interest is labeled *Participation* and is a dummy variable that takes value 1 if the proceeding p was presented in a conference series c where firm f participated and 0 for proceedings matched to conference c that were actually presented at a conference where firm f did not participate. The first stage equation 2.1 models the probability of *Participation*, so defined, as a function of the existence of a direct flight between the location of the authors of p and the venue of conference c, in the year that p is presented. The variable *Direct Flight* is equal 1 if a direct flight exists. X_{fpc} is a vector FE controls and control variables. The proceedings p are nested in years so that we do not have another index for years.

First stage:

$$P(\text{Participation})_{pcf} = \beta_1 \text{Direct flight}_{pcf} + \beta_2 X_{pcf} + u_{pcf} \tag{2.1}$$

The second-stage equation (2.2) models the probability of knowledge diffusion from the proceeding p and its authors to the firm f as a function of *Participation*. We use as dependent variables a set of indicators of knowledge diffusion, better defined in the next section 2.6.2.

Second stage:

$$P(\text{Knowledge diffusion})_{pcf} = \gamma_1 \text{Participation}_{pcf} + \gamma_2 X_{pcf} + \epsilon_{pcf} \tag{2.2}$$

The identification assumption relies on the exogeneity of the availability of direct flights between scientists' locations and conference venues, with respect to the probability of knowledge flows between scientists and firms. A specific advantage of our setting is that the pairs of scientists and conference locations, in the large majority of cases, do not coincide with the pairs of scientists and firms locations. Possible preexisting relationships between firms and scientists in a given location would hardly influence the connectivity with conference venues, if these do not coincide. Moreover, this feature strengthens the credibility of the exclusion restriction - i.e. the assumption that the instrument affects knowledge flows exclusively via the participation to conferences - because new direct flights to conferences do not imply increased direct connectivity between firms and scientists.

Airlines' and conference organizers' decisions depend on several factors. New airline routes are likely driven by broad market trends and regulations (Campante and Yanagizawa-Drott, 2018). The location decisions of conferences series are driven by the general attractiveness of venues but are likely independent from the specific pair-level probability of interaction between scientists and firms in a specific year. Conference locations are scheduled often years in advance and organizers choices are primarily constrained by budget considerations and the need for adequate venues in terms of size and surrounding facilities. Some conferences are static, while others show rather erratic patterns of mobility, besides the preference for attractive locations. As long as the deciding factors of airlines and conference organizers are unrelated to the pair-level probability of knowledge flows between scientists and firms, they are not a concern. However, (omitted) factors affecting both the existence of direct flights and the probability of knowledge diffusion may still bias the results. In our specification, we can account for these factors at the level of the conference series, the firm and the location of scientists. In this, our approach is akin to previous studies, in particular Giroud (2013).

Levels FE. Without additional controls, we would have to worry, for instance, that the most innovative firms, the most productive researchers, and most highly ranked conferences are likely located in regions with better airline connectivity. The estimates, rather than an effect of participation, would reflect the fact that these firms and scientists will more likely cite (or receive citations) and participate in the best conferences. Accordingly, we include FE controls for all the main levels of observation: conference series, region of origin of researchers interacted with CS sub-fields, years. Conference series, region of origin of researchers FE are necessary to control for any constant characteristics related to these levels of observation that can correlate with access to direct flights. We include the interaction between regions and sub-fields as FE to account for regional specialization. Year FE account for general time trends in the data. Firms FE are also included, nested in firms and scientists location pair-FE, discussed next.

Firm and scientists location pair-FE. Another concern is the possible correlation between direct flights to conference venues and the connectivity between pairs of scientists and firms locations. This would be the case for pairs of firms and researchers in the same or proximate regions. A new airline, for instance, would likely increase the number of direct flights to conference locations, but also between all locations within the same geographic area where it operates. Close firms and researchers will also more likely participate in the same conferences and, at the same time, exchange knowledge directly because of their proximity. We control for firm and scientists origin pair-FE, to control generally for all pair specific features between the firm and the location of scientists. This includes geographic distance, location in the same countries or regions, spoken language similarity between different countries, etc. Moreover, these pair-FE control also for possible specific connections of the firm with the scientists' locations that we cannot observe (e.g. presence of subsidiaries).

Firm-level shocks: firm and year pair-FE. To control for firm-level shocks, we are able to control for firm and year pair-FE. An increase in innovative productivity specific to one or more firms and in a specific period would affect their propensity to participate to conferences, particularly of high quality, and, at the same time, their capacity to absorb external knowledge. If such trends are indeed firm and time-specific, level-FE would not suffice. Firms with increased productivity would suddenly participate in the best conferences that may be better connected via direct flights to scientists' locations. Positive estimates may partly reflect the fact these firms may absorb more likely scientific knowledge, independently from conference participation. Firm and year pair-FE eliminate this type of concerns.

Scientists' location-level shocks - scientists location and year pair-FE. Finally, we account for time-specific shocks at scientists' locations level. Economic and innovation trends may be region and time-specific. Better infrastructures, including transportation networks, would normally follow or precede such trends. In this case, despite the use of time-invariant FE, the presence of direct flights may correlate with the quantity and quality of scientific activities in a region, in specific years. Scientists that increase their participation to conferences, thanks to the higher attractiveness of their regions for airlines and conference organizers, may also be those more likely cited in a given period, regardless of their actual participation. We control for scientists location and year pair-FE to fully absorb this variation.

We model both stages as Linear Probability Models (LPM). The use of LPM eases the interpretation of the coefficients, that can be interpreted as changes in percentage points in probabilities. [14] In our main specification, we cluster standard errors at the level of region of origin of scientists. In section 2.9 and in the appendix, we test additional specifications to address other concerns: additional FE controls;

[14]Non-linear probability models and count models are hardly applicable due to our sample size and the use of high-dimensional FE.

continuous outcome variables; alternative clustering of standard errors; OLS specifications; falsification test analyses.

2.6.2 Definition of Variables

Knowledge Diffusion

Our main dependent variables capture knowledge diffusion from scientists to firms as measured by citations from firms to proceedings (Jaffe, Trajtenberg, and Henderson, 1993; Roach and Cohen, 2013). We look both at scientific publications and patent citations. Naturally, scientific citations represent knowledge diffusion to firm science activities, while patent citations to innovation activities. Patents are imperfect proxies for innovation but represent actual intellectual property assets. All the more, patents with citations to science are consistently found to be highly valuable (Ahmadpoor and Jones, 2017; Watzinger and Schnitzer, 2018; Poege et al., 2019). Science and innovation are also often distinct operations of independent organizational units, especially in medium to large firms. Hence, the importance of looking at both dimensions.

Science cit (present) and *Patent cit (present)*. We denote with the variable name *Science cit.* scientific citations and with *Patent cit.* patent citations. We look first at citations to the proceedings at the conference. Specifically, the label *present* refers to citations to the same proceeding p in the firm-proceeding pair observation. All citation-based variables are defined as dummy variables equal to 1 if at least one citation is observed, and 0 otherwise. To avoid truncation in the latest years, we restrict the sample to conferences up to 2010 and we look at citations within a 5 years time window after each conference.[15] For patents, we count the years of delay based on their priority year.

Science cit (past) and *Patent cit (past)*. We look separately at citations to previous publications of the scientists, labeled as *past*. These are citations to other publications from the same authors of the proceeding p, published in the same or the previous five years. The window of time considered after the conference remains also 5 years, as discussed above.

Control Variables

Conditional on the FE-controls in our models, we deem unlikely that there is any residual correlation between the presence of direct flights and proceeding level or firm-proceeding pair-level variables. We include additional control variables for robustness. We control for indicators of geographic distance between scientists' location and conference venues that, in the first stage, may influence the probability of participation usings: the logarithm of the geographic distance between the scientists and the conference venues (*Conference distance*); a dummy equal one if the conference takes place in the same region of the scientists. At the firm-proceedings level, we control for indicators of the relevance of the research of scientists to the focal firm, as likely predictors of knowledge diffusion. *Science citations (L)* captures the presence of citations from proceedings of the firm in the previous 5 years up to the year of the conference to

[15]For patent citations, this may be insufficient to eliminate truncation. Citations are added to patent families over time in subsequent publications of the patent (e.g. grant publication and international filings), both by examiner and applicants. As grant lags of several years are not uncommon, many citations may remain unobserved for the latest conferences in our sample. We ensured that this is not an issue by running regressions for a sample of conferences up to 2008, finding equivalent results.

previous publications of the authors of the proceeding p. Similarly, *Patent citations (L)* capture citations from patents of the firms. The two variables can be seen as the analogous of lagged dependent variables. We use, instead, a measure of the research similarity of the firm to the focal proceeding p, looking at the text-similarity (title and abstracts) with firms' proceedings in the year before the conference, in the same CS sub-field.

Mechanisms

Collaboration. We cannot capture all forms of collaboration, especially if informal or unsuccessful. However, we can observe scientific collaborations that lead to future coauthored publications. We capture the presence of such collaborations with the variable *Collaboration* which is a dummy equal to 1 if in the 5 years following the conference at least one scientist author of proceeding p is found to co-author at least one publication with the firm. We use this as an additional dependent variable.

Hiring. To capture mobility, *Hiring* is equal one if at least one scientist author of the proceeding p (not affiliated to the firm f at the time p is presented) is found to publish with the firm f as affiliation in the 5 years following the conference. This is an imperfect proxy because hiring is observed exclusively if the scientist publishes. This is likely if the scientist is hired in a research unit but less so in a product development unit. However, this variable serves the purpose to assess whether our results can be explained by hiring, rather than by knowledge flows and collaborations with external scientists. We use also this variable as a dependent variable.

Participation intensity. To study the role of firm participation intensity we first use *N.firm-proceedings* which is the number of proceedings presented at the conference by the firm‘s scientists. Because the variable distribution is highly skewed and particularly sparse we aggregate the values of 3 and 4 proceedings together and we censure the variable at the value of 5 for firms with 5 or more proceedings at a single conference. The median number of papers at a conference is 1, at a mean of 1.6. Only the top 4.9% of our estimation dataset has 5 or more proceedings presented by one same firm. Second, we use *Sponsor*, a dummy equal 1 if the firm is a sponsor of the conference. We distinguish the case of firms that exclusively sponsor a conference, without also having papers presented (*Sponsor-only*) and the case of firms doing both (*Sponsor-Proceedings*). In our estimation dataset, 6.3% of firm attendances come with sponsorship. Of these, in 30.0% of the cases, firms are both participating and sponsoring.

Firm size. Finally, to proxy the size of research investments, we rank firms within each year by the number of active scientists they employ. For this, we build an affiliation panel for each scientist. When there is no publication in a year, but in earlier and later years, we use linear imputations. We use all types of publications to build the panel. We look at the top 5, top 50 and other firms' categories by size, within each year. Overall, 9.6% of firms in our full estimation dataset appears at least for one year in the top five firms and 30.3% in the top six to fifty.

2.7 Main Results

2.7.1 First Stage: Participation

The first stage regression results are reported in Table 2.3.[16] In this and all tables for the main dependent variables, we present a series of 3 specifications adding controls gradually. The first column for each dependent variable includes exclusively level FE controls, for conference series, for scientists locations and subfield, firms and scientists' locations pairs, and years. The second column adds time-specific FE: firm and year pair-FE, scientists location and year pair-FE. The last column is the full specification including all FE controls and the additional control variables described in section 2.6.2.

Table 2.3: First stage - the effect of *Direct flight* on *Participation*

	Science cit (present)		
	(1)	(2)	(3)
Dep. Var.	Participation	Participation	Participation
Direct Flight	0.056***	0.059***	0.030***
	(0.006)	(0.006)	(0.005)
Firm-proceeding controls			
Science citations (L)			0.118***
			(0.003)
Patent citations (L)			0.055***
			(0.004)
Research similarity (L)			0.958***
			(0.025)
Conf. distance controls			
Conference distance			−0.039***
			(0.003)
Same region			−0.160***
			(0.035)
Same state			0.131***
			(0.015)
Conf Ser FE	Yes	Yes	Yes
Origin × Field FE	Yes	Yes	Yes
Year FE	Yes		
Origin × Firm FE	Yes	Yes	Yes
Year × Origin FE		Yes	Yes
Year × Firm FE		Yes	Yes
R^2	0.297	0.323	0.346
Observations	5126376	5126273	5126273
Number clusters	1124	1114	1114
DV cond. mean	0.512	0.512	0.512

Notes: * $p < .1$, ** $p < .05$, *** $p < .01$ Standard errors in parenthesis, clustered at the researcher region level. Conf. distance controls include the distance between researcher and conference location (log), whether that distance is zero and whether the two locations are in the same US state or non-US country. Firm-proceeding level dataset, where some proceedings were actually at the conference (Participation=1) and some were at another conference (Participation=0). Participation is instrumented by the direct flight availability between the researcher location and the conference location. Firm-proceeding controls include whether the firm cited previous work by the authors in the years before the conference (Science/Patent citations L) and the average abstract similarity between proceedings published by the firm in the previous year and the focal proceeding (Research Similarity L). First stage results for other second-stage variables are very similar, results are available upon request.

The results show a strongly significant effect of the instrumental variable *Direct flight* on *Participation*. In all specifications, we find a highly significant and positive coefficient. The magnitude is economically

[16] For the sake of brevity, we present here only the first-stage result corresponding to our first dependent variable, *Scientific cit. (present)*. The sample for different dependent variables varies minimally due to some observations being invariant within FE-controls, but otherwise, the first-stage estimates remain almost identical.

meaningful, implying that the existence of a direct flight leads to an increase in the probability of a proceeding being presented at a conference of about 5.9 p.p. or 3 p.p in the full specification, corresponding to about a 12 or 6% increase in probability, respectively, relative to the sample average 51.2%. The F-test value on the excluded instrument exceeds a value of 20 and is often substantially higher, depending on the specification. The lower coefficient and significance of *Direct flight* is expected in the third specification because direct flights are more frequent at short and medium distances and distance controls (*Same state* in particular) absorb part of the same (possibly exogenous) variation of the instrumental variable. The control variables estimates show rather predictable correlations.

Appendix B.2.2 presents a heterogeneity analysis of the effect of *Direct flight* which is informative to understand the population relevant for the Local Average Treatment Effect (LATE) estimated. We find predictable variation. The effect of *Direct flight* is stronger for medium and low ranked conference series but remains significant for conferences of all ranks. Magnitude and significance also increase with the geographic distance of conferences of any rank. Finally, in Appendix B.2.1 we propose an event study analysis, at the scientists' location - conference series pair level of analysis. Differently from the analyses in the paper, these analyses are not conditional on the participation of at least one paper at a conference and look at participation flows between regions and conference-series pairs over time. This allows to study the dynamic effect on participation of direct flights and to appreciate the lack of pre-trend in the probability of participation of scientists to a conference series prior to the availability of a direct flight.

2.7.2 Second Stage: Knowledge Diffusion

Table 2.4 presents the result of the second stage regressions for *Science cit (present)* and *Patent cit (present)*. The coefficients indicate the change in probability of citation in percentages points (p.p.) for proceedings presented in the same conference where the firm participates (*Participation* equal 1). Columns 1 to 3 present results for *Science cit (present)* as dependent variables, columns 4 to 6 for *Patent cit (present)*. For both dependent variables, we deploy a series of model specifications, as for the first-stage results. Columns 1 and 4 include only main FE (conference series, scientists locations and subfield, firms and scientists' locations pairs, and years), column 2 and 5 add year specific FE (firm and year pair-FE, scientists location and year pair-FE), and columns 3 and 6 add control variables (described in section 2.6.2).

The coefficient magnitude varies minimally between the first and second specification. We find significant results, at the 1% level, for the effect of *Participation* on *Scientific cit. (present)*. The magnitude of the coefficients varies more, from 0.013 in column 2 to 0.021 in the specification in column 3, corresponding to 1.3 and 2.1 p.p. change in the probability of observing a citation. In general we find larger estimates with the IV models as compare to OLS. The difference is moderate for models that do not include distance controls.

Finding larger IV estimates is not unusual. This may be already expected on the basis of the severe measurement error associated with publications and patents data, in particular. Additional measurement error, in our context, is derived from the impossibility to be certain about the physical presence of scientists: the IV model may allow "cleaning" for this measurement error in the *Participation* variable. We also do not necessarily expect a positive bias in OLS estimates, since we compare very similar proceedings for topic and quality. Moreover, participation may be more strongly driven by familiarity to some expected content that would be cited with or without participation, while the IV models isolate the effect of exposure to findings or persons that may otherwise less likely encountered. Finally, we show in appendix B.2.2, that the instrument has a larger effect on low-ranked conferences, particularly when we introduce conference distance control variables. It is plausible that the effect of participation in lower-ranked conferences is larger at the margin, because these conferences would attract less attention without participation.

We use the sample mean average conditional on actual participation as a benchmark (the average probability of citation for proceedings at the conference where the firm participates). Compared to this baseline, 1.0%, the effects are quite substantial. On the contrary, the coefficient for the effect on *Patent cit. (present)* is small and not statistically different from zero: we do not find an effect of *Participation* on the probability that proceedings presented at a conference are cited in patents.

The correlation with the control variables is meaningful, showing that proceedings that are similar to the firm recent research or from authors that have previously been cited by the firm are more likely to be cited again, both in firm publications and patents. We can use these estimates as a benchmark for the effect of *Participation*, noting that it has an almost comparable magnitude to *Scientific citations (L)*: proceedings of authors already cited in the past by the firm are more likely to be cited again, but the effect of *Participation* is of the same order of magnitude as this correlation.

Table 2.5 shows the results on the effect of *Participation* on the probability of citation to previous publications of the authors of the focal proceedings (*Science cit (past)* and *Patent cit (past)*). We again include FE and control variables gradually as just discussed for Table 2.4. Point estimates for the effect on *Science cit (past)* are 6.9 p.p., in column 1 with basic FE control, to 0.057 in column 2, with significance levels at 5%, and 11.3 p.p in column 3 with significance level at 1%. This is again a large increase compared to the conditional sample mean, 15.8%. The effect on patents citations, from the results in column 4 to 6, is now highly significant and large in magnitude, corresponding to 4.7 in the more basic specification (column 4), 4.8 in column 5, and 9.8 p.p in column 6.

The results relative to the control variables are again predictable, implying, for instance, that proceedings previously cited by a firm are more likely to be cited again. To use these as a benchmark, we can say that *Participation* increases the probability that the scientists are cited after a conference by about one-fourth of the probability increase associated with the scientists having been cited already before the conference. This ratio is a bit less than a half comparing the effect of *Participation* on *Patent cit (present)*, relative to the correlation with *Patent cit (present)*.

Overall, the results in table 2.4 and 2.5 demonstrate a strong effect of *Participation* on citations and allow some considerations. While the effect on scientific citations is significant for both proceedings at the conference and previous proceedings of scientists, the effect on patent citations is exclusively significant for the latter. This can derive from the difference between science and innovation activities within firms. Scientists participating in conferences can immediately build on new knowledge inputs in upcoming publications while innovation activities are probably performed by distinct organizational units and require farther development. The lack of significance for patent citations to focal proceedings may also be due to patent citations being a noisier indicator.

For the purpose of our investigation, we note that an effect exclusively on proceedings at the conference would have suggested that the timely screening of information at the conference was the main mechanism

Table 2.4: The effect of *Participation* on citations to proceedings at the conference

	(1) Science cit (present)	(2) Science cit (present)	(3) Science cit (present)	(4) Patent cit (present)	(5) Patent cit (present)	(6) Patent cit (present)
Participation	0.013***	0.013***	0.021***	0.001	0.001	0.001
	(0.004)	(0.004)	(0.007)	(0.001)	(0.001)	(0.003)
Science citations (L)			0.029***			0.003***
			(0.001)			(0.000)
Patent citations (L)			0.008***			0.003***
			(0.002)			(0.001)
Research similarity (L)			0.025***			0.007**
			(0.006)			(0.003)
Conf. distance controls	No	No	Yes	No	No	Yes
Conf Ser FE	Yes	Yes	Yes	Yes	Yes	Yes
Origin × Field FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes			Yes		
Origin × Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year × Origin FE		Yes	Yes		Yes	Yes
Year × Firm FE		Yes	Yes		Yes	Yes
R^2	0.076	0.082	0.083	0.024	0.030	0.031
Observations	5126376	5126273	5126273	5126376	5126273	5126273
Number clusters	1124	1114	1114	1124	1114	1114
DV cond. mean	0.010	0.010	0.010	0.002	0.002	0.002
F (First)	81.1	88.7	31.6	81.1	88.7	31.6

Notes: * $p < .1$, ** $p < .05$, *** $p < .01$ Standard errors in parenthesis, clustered at the researcher region level. Citations of firm science (columns 1-3) and firm patents (columns 4-6) in the subsequent five years towards the focal proceeding are analyzed. The dependent variables are 1 if at least one citation occurred. Firm-proceeding level dataset, where some proceedings were actually at the conference (Participation=1) and some were at another conference (Participation=0). Participation is instrumented by the direct flight availability between the researcher location and the conference location. Firm-proceeding controls include whether the firm cited previous work by the authors in the years before the conference (Science/Patent citations L) and the average abstract similarity between proceedings published by the firm in the previous year and the focal proceeding (Research Similarity L). Dependent variable mean is for actually presented proceedings. Conf. distance controls include the distance between researcher and conference location (log), whether that distance is zero and whether the two locations are in the same US state or non-US country.

explaining citations. It remains possible that proceedings at the conference serve as pointers to previous proceedings of the same authors. However, we argue that the effect on "citations to the *past*" is at least supportive of the hypothesis that actual social interactions with scientists are the central channel of diffusion.

Table 2.5: The effect of *Participation* on citations to previous proceedings of scientists

	(1) Science cit (past)	(2) Science cit (past)	(3) Science cit (past)	(4) Patent cit (past)	(5) Patent cit (past)	(6) Patent cit (past)
Participation	0.069**	0.057**	0.113***	0.047***	0.048***	0.098***
	(0.028)	(0.025)	(0.041)	(0.015)	(0.013)	(0.024)
Science citations (L)			0.394***			0.158***
			(0.006)			(0.003)
Patent citations (L)			0.184***			0.195***
			(0.006)			(0.006)
Research similarity (L)			0.240***			0.012
			(0.043)			(0.024)
Conf. distance controls	No	No	Yes	No	No	Yes
Conf Ser FE	Yes	Yes	Yes	Yes	Yes	Yes
Origin × Field FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes			Yes		
Origin × Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year × Origin FE		Yes	Yes		Yes	Yes
Year × Firm FE		Yes	Yes		Yes	Yes
R^2	0.304	0.318	0.372	0.176	0.187	0.192
Observations	5126376	5126273	5126273	5126376	5126273	5126273
Number clusters	1124	1114	1114	1124	1114	1114
DV cond. mean	0.158	0.158	0.158	0.050	0.050	0.050
F (First)	81.1	88.7	31.6	81.1	88.7	31.6

Notes: * $p < .1$, ** $p < .05$, *** $p < .01$ Standard errors in parenthesis, clustered at the researcher region level. Citations of firm science (columns 1-3) and firm patents (columns 4-6) in the subsequent five years are analyzed. Citations towards publications by the proceeding authors in the five years before the conference are considered. The dependent variables are 1 if at least one citation occurred. Firm-proceeding level dataset, where some proceedings were actually at the conference (Participation=1) and some were at another conference (Participation=0). Participation is instrumented by the direct flight availability between the researcher location and the conference location. Firm-proceeding controls include whether the firm cited previous work by the authors in the years before the conference (Science/Patent citations L) and the average abstract similarity between proceedings published by the firm in the previous year and the focal proceeding (Research Similarity L). Dependent variable mean is for actually presented proceedings. Conf. distance controls include the distance between researcher and conference location (log), whether that distance is zero and whether the two locations are in the same US state or non-US country.

2.8 Exploration of Mechanisms

2.8.1 Collaboration and Hiring

The following analyses address the effect of *Participation* on the *Collaboration* with and *Hiring* of scientists. While we do not capture these dimensions perfectly, an effect on these variables is informative to understand the channels of the knowledge diffusion observed. Both outcomes would be indicative of strong interactions with scientists. At the same time, collaboration and knowledge diffusion may occur from scientists that remain external to the firm, or, in the case of *Hiring*, via the actual mobility of scientists from academia (or other firms) to the focal firms. Table 2.6 presents the related results, from columns 1 to 3 for *Collaboration* and from columns to 4 to 5 for *Hiring*. We again include FE and control variables gradually as discussed for tables in section 2.7.

We find a strong and highly significant effect on *Collaboration*. In our preferred specifications, columns 2 and 3, the estimate implies, respectively, a 2.8 and 7.4 p.p higher probability of scientific collaborations between scientists and firms who participated in the same conference. The magnitude of these effects is large if compared with the variable conditional average of 4.8%. The correlation with the control variable remains as expected and the magnitude of the effect of *Participation* relative to these correlations is meaningful. For instance, *Collaboration* is 15 p.p. higher for scientists having been cited by the firm. The magnitude of the effect of *Participation* is half of that.

We find no significant effect of *Participation* on *Hiring*. *Hiring* is also a rarer event, with a conditional average probability in the sample of 1.1%. However, the control variables show meaningful and significant correlations. For instance, indicating a much higher probability of mobility for scientists that have been previously cited by the firm.

Table 2.6: The effect of *Participation* on collaborations with and hiring of scientists

	(1) Collaboration	(2) Collaboration	(3) Collaboration	(4) Hiring	(5) Hiring	(6) Hiring
Participation	0.032***	0.028***	0.074***	0.002	0.001	0.006
	(0.009)	(0.009)	(0.020)	(0.004)	(0.004)	(0.008)
Science citations (L)			0.147***			0.037***
			(0.004)			(0.001)
Patent citations (L)			0.074***			0.012***
			(0.005)			(0.002)
Research similarity (L)			0.047***			0.021***
			(0.018)			(0.007)
Conf. distance controls	No	No	Yes	No	No	Yes
Conf Ser FE	Yes	Yes	Yes	Yes	Yes	Yes
Origin × Field FE	Yes	Yes	Yes	Yes	Yes	Yes
Origin × Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year × Origin FE		Yes	Yes		Yes	Yes
Year × Firm FE		Yes	Yes		Yes	Yes
R^2	0.177	0.185	0.196	0.075	0.080	0.087
Observations	5126376	5126273	5126273	5126376	5126273	5126273
Number clusters	1124	1114	1114	1124	1114	1114
DV cond. mean	0.048	0.048	0.048	0.011	0.011	0.011
F (First)	81.1	88.7	31.6	81.1	88.7	31.6

Notes: * $p < .1$, ** $p < .05$, *** $p < .01$ Standard errors in parenthesis, clustered at the researcher region level. In columns 1-3, the dependent variable is one if at least one of the proceeding authors has a joint publication with a firm researcher. In columns 4-6, the dependent variable is one if at least one of the proceeding authors becomes a firm researcher. Firm-proceeding level dataset, where some proceedings were actually at the conference (Participation=1) and some were at another conference (Participation=0). Participation is instrumented by the direct flight availability between the researcher location and the conference location. Firm-proceeding controls include whether the firm cited previous work by the authors in the years before the conference (Science/Patent citations L) and the average abstract similarity between proceedings published by the firm in the previous year and the focal proceeding (Research Similarity L). Dependent variable mean is for actually presented proceedings. Conf. distance controls include the distance between researcher and conference location (log), whether that distance is zero and whether the two locations are in the same US state or non-US country.

These results allow us to conclude that actual collaborations with scientists are likely a relevant channel of knowledge diffusion. Also interestingly, the hiring of scientists is less likely to explain our results. It is interesting to note that this finding matches survey evidence from Cohen, Nelson, and Walsh (2002) who find that conferences and personal interactions score considerably higher than hiring as channels of knowledge diffusion from public research to corporate R&D. To be sure, by no means does this implies that hiring is not relevant in general and hiring of scientists to positions where they stop publishing may occur, which would not be observed in our data. Moreover, the following sections demonstrate that for some firms, indeed an effect of *Participation* on *Hiring* exists. However, the findings allow us to conclude that, on average, the observed effect on knowledge diffusion as well as on collaborations is explained by scientists that remain external to the firms.

2.8.2 Firm Participation Intensity and Research Investments Size

We focus on the role of firms' participation intensity and firm size. From a theoretical standpoint, we would expect different results if the prestige of firms within scientific communities were relevant, as opposed to the case where participation exclusively served the purpose of lowering knowledge search costs. Moreover, the role of this dimension has direct implications for firm decisions as well as for the resulting pattern of knowledge diffusion from science to industry.

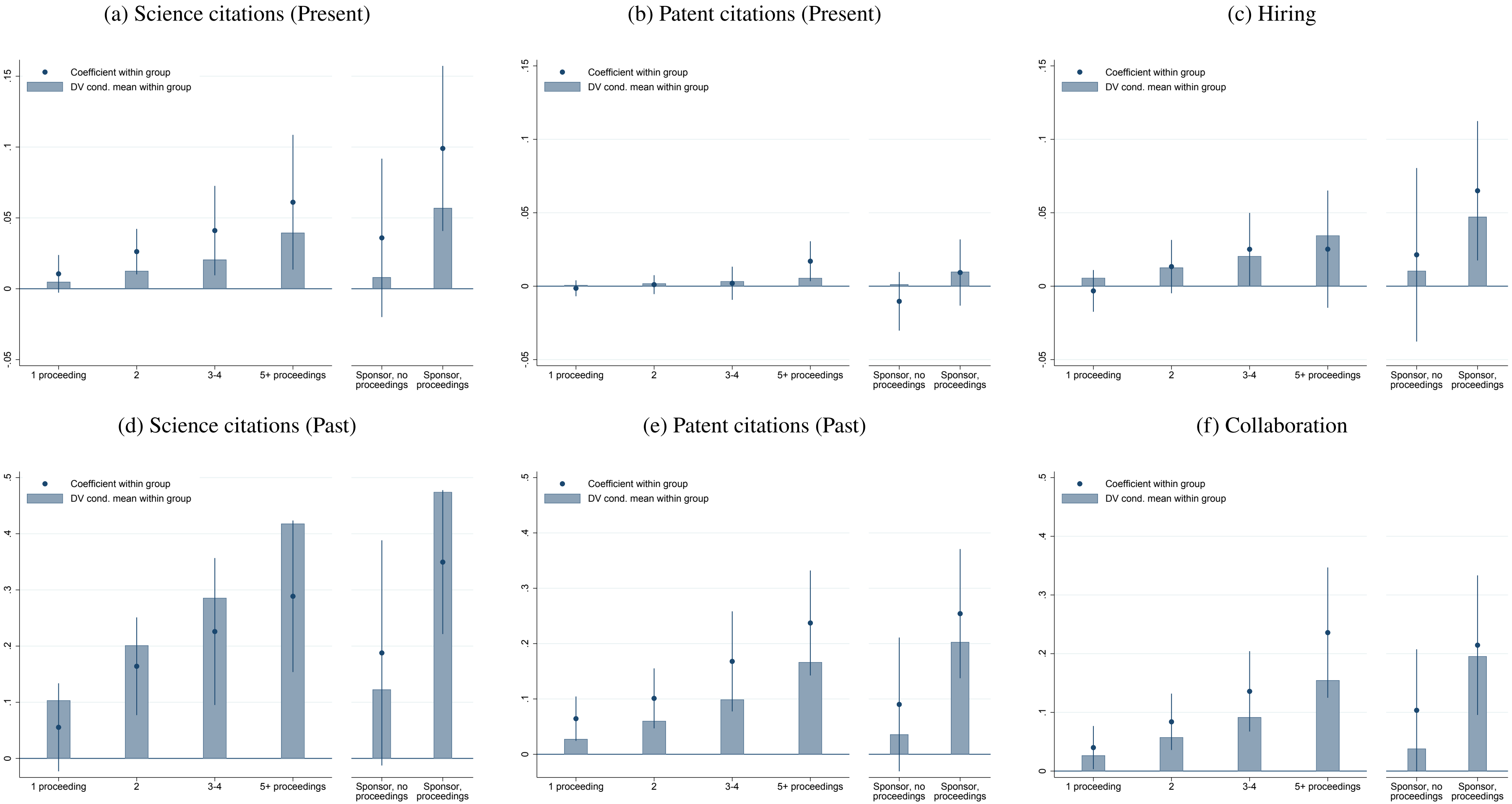
We study how the effect of *Participation* varies for firms that do not sponsor the focal conference and that have numbers of proceedings presented from low (1) to high (up to 5 or more), for firms that exclusively sponsor the conference, and for firms that both sponsor the conference and author proceedings. The underlying regression analyses are reported in Appendix Table B.12. Both the endogenous variable, *Participation*, and the instrument *direct flight*, in the first-stage regressions, are interacted with dummy variables for each subgroup. [17] The results are presented here graphically. Figure 2.3 shows the main coefficient estimates from Appendix Table B.12 from the full model with all controls (similar results are found in other specifications). Each graph, from (a) to (f), represents results for a different outcome variable, reporting the coefficient estimates with bandwidths for the 95% confidence intervals. The bars indicate the within-group average of the dependent variable.

We highlight the most relevant patterns. First, for all variables, we encounter a remarkably stronger effect for firms with a higher number of proceeding presented. Firms that only present one proceeding and that are not sponsors show no significant coefficients, except for the effect on *Patent citations (Past)* and for *Collaboration*, in graphs (e) and (f), respectively. Firms with the largest number of proceedings, on the contrary, show the largest coefficients, and a statistically significant difference. For these firms, we find also an effect on patent citations to proceedings at the conference *Patent citations (Present)*, which was not significant on average for the full sample (see Table 2.4).

Second, sponsorship and authorship appear complementary. Firms that only sponsor a conference show point estimates larger in magnitude and less precise, relative to firms with only a few proceedings and not sponsoring, but never pass the significance threshold. For almost all variables, except for *Patent citations (Present)* in graph (b), firms that both sponsor a conference and author at least a proceeding demonstrate a strong and significant effect. The comparison with the within-groups means of the dependent variables shows that firms with higher intensity of participation also have on average higher levels of the dependent variables. The effect of *Participation* adds to these levels.

[17] We also estimate simplified regression models, where we introduce interactions only for sponsorship (Appendix Table B.9), or using the number proceedings as a single linear interaction variable, rather than four dummies for each subgroup (Appendix Table B.11). The results are a subset of those discussed here for the more complete model and lead to the same conclusions.

Figure 2.3: Heterogeneity of the effect of *Participation* by participation intensity of the firm



Notes: Each panel shows the result from an IV regression. We instrument each subgroup of firm participation intensity interacted with researcher participation is by the interaction of the same firm participation intensity and direct flight availability. For comparison, the dependent variable means in the subgroup of firm participation intensity are shown. Full estimation results are available in table B.12. Control variables and FE are as in other heterogeneity regressions.

We highlight the interesting result of the positive and significant effect on *Hiring* for firms that are both sponsors and present proceedings (graph c). As noted, sponsoring has indeed also hiring objectives. However, sponsorship alone shows no impact. This strongly suggests that the active participation of scientists is fully complementary to sponsoring in hiring activities. This is in line with the idea that interactions among scientists are a vehicle for exchanging information, possibly, in this case, also about scientific positions within firms.[18] Moreover, scientists assign value to the possibility of publishing and doing cutting edge research (Stern, 2004; Sauermann and Cohen, 2010). Firms incapable of signaling such opportunities may not be attractive for scientists. For the scope of our analyses, we maintain that hiring is unlikely to be an underlying mechanism explaining our results for the majority of firms. Instead, it is possible that it contributes to strengthening the effects observed for both sponsoring and authoring firms.

In Table 2.7 we present results for the last analysis of heterogeneity of the effects. We look at the variation of the effects of *Participation* by firm-level investments in research, as measured by the number of active scientists in a year. We present results for all outcome variables and our full specification from column 1 to 6. The results are generally coherent with the evidence from the intensity of participation, indicating that the largest firms have the strongest effects. Smaller firms have significant effects for patent citations to previous publications of scientists, but not for other variables. The estimates sizes for Top 5 firms are multiples of the effect size for smaller firms.

Table 2.7: Heterogeneity of the effect of *Participation* by firms' size of research investments

	(1) Science cit (present)	(2) Science cit (past)	(3) Patent cit (present)	(4) Patent cit (past)	(5) Collaboration	(6) Hiring
Top 5 × Participation	0.097***	0.241***	0.016**	0.278***	0.156***	0.004
	(0.024)	(0.067)	(0.008)	(0.056)	(0.040)	(0.019)
Top 6-50 × Participation	0.023***	0.151***	0.000	0.115***	0.088***	0.005
	(0.008)	(0.047)	(0.003)	(0.029)	(0.023)	(0.008)
Remainder × Participation	0.010	0.050	0.000	0.052**	0.047**	0.006
	(0.007)	(0.039)	(0.003)	(0.022)	(0.020)	(0.007)
Conf. distance controls	Yes	Yes	Yes	Yes	Yes	Yes
Proceeding-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Standard FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.069	0.370	0.027	0.173	0.195	0.087
Observations	5126273	5126273	5126273	5126273	5126273	5126273
Number clusters	1114	1114	1114	1114	1114	1114
DV cond. mean	0.010	0.158	0.002	0.050	0.048	0.011
F (First)	11.0	11.0	11.0	11.0	11.0	11.0

Notes: * $p < .1$, ** $p < .05$, *** $p < .01$ Standard errors in parenthesis, clustered at the researcher region level. Ranks are yearly firms ranks, ordered by the size of the scientific workforce that based on publication information can be attributed to a company, lagged by one year. Individual coefficients of the rank levels are collinear with fixed effects and omitted. Proceeding-level controls include variables on firm citations before the conference towards the authors' previous publications, whether the proceeding is authored by a firm author, whether the authors have previously participated to the conference and the similarity of previous firm publications with the focal conference publication. Standard fixed effects include conference, origin × field, origin × firm, year × origin and year × firm fixed effects.

[18] This interpretation is in line with insights from our interviews. Firms´ scientists declared that they participate in conferences mostly "like any other scientist" and do not normally have the objective of hiring other scientists on the behalf of the firm. At the same time, when firms sponsor the conference, HR personnel is often supported by firms scientists. Several HR representatives declared that exclusively sponsoring conferences turned out ineffective for hiring purposes, as it is difficult to attract the attention of potential candidates without displaying specific competencies and engagement into the scientific community.

2.9 Robustness

Alternative Citation Measures

In this section, we address a list of possible concerns regarding the robustness of our results. We start considering alternative criteria for counting scientific citations and presenting results on a measure of text similarity as an alternative indicator of diffusion. Table 2.8 presents the related results, where the alternative variables are used as dependent variables of our full model specification. Column 1 reports, for reference, the results for our main dependent variable *Science cit.(present)*.

In column 2, we show results for a model where the dependent variable is the probability of observing citations to proceedings presented to the conference but excluding all author-level self-citations. Firm self-citations were already excluded in the main dependent variable. However, citations from scientists that move to the firm, and that were not affiliated to the firm at the moment of the conference, can still occur. As we have shown that hiring is weekly affected by participation, we doubt that this is frequent. Accordingly, the effect size in column two is very similar to the effect size in column one, suggesting that the main results are not driven by author self-citations.

In column 3, we look at citations only from publications where a scientist exclusively affiliated with the firm appears as the first author. This responds to the concern that the effect we observe may be driven by other academic scientists, or by other firms, that by coauthoring with the focal firm introduce citations that would not be otherwise observed. Indeed, co-authored publications are relatively frequent, especially with academics. In these cases, we cannot single out the individual contribution of single scientists of a focal firm to the bibliography list. To obviate this concern, we look at first-authored publications, under the assumption that the first author is the main project leader of a research project (this is indeed the normal practice in CS, also for scientists affiliated to firms). While the effect size (and the dependent variable mean) are somewhat smaller, the coefficient remains positive, statistically significant and large in magnitude.

In columns 4 and 5, we distinguish, respectively, firms' scientists who are authors of proceedings presented at the focal conference, from firms' scientists who are not. In other words, we attempt to distinguish citations from firms' scientists more likely physically present at the conference, from other scientists. As it is expected we find stronger effects for citations from the former group. However, interestingly, also citations from the latter see a significant and sizable effect. This resonates well with the qualitative evidence we collected, about the existence of knowledge sharing processes within firms, often formalized in internal information systems or internal seminar series, dedicated to pass insights from the conference to non-attendees. A more simple explanation is the possibility that some firms' scientists may passively participate without having accepted papers. This especially may play a role when a firm sponsors a conference and other scientists accompany HR personnel.

Text Similarity Analysis

Scientific citations may reflect strategic behavior or salience (Teplitskiy et al., 2020), for instance, if scientists add citations to please other scientists, or simply because the exposure to a proceeding increases the probability that that proceeding is cited rather than another, but without actual influence on the content.

We alleviate this concern by showing that besides the probability of citations, the material content of the research subsequently carried out by the firm changes as well. We do so using a measure of text similarity between the focal proceeding p and future proceedings of the firm, within the same field and in a time window of 3 years. We discuss in detail the construction of the similarity measure in appendix B.6.

Column 6, in Table 2.8, presents the results for the text-similarity measure over the entire 3-year period and for the mean similarity with firm proceedings. Alternative specifications presented in appendices show variants where we distinguish the similarity for each year separately (Table B.20) and using the maximum instead of the mean (Table B.21). The results are broadly consistent across these variants. From column 6, we see that *Participation* has a positive significant effect on the text-similarity measure of about 0.03, against a sample average of the variable of 0.1. This reinforces our finding that participation of firms to the scientific community has real and relevant effects on the firms' scientific activities.

Table 2.8: Robustness - Science citations' alternative measures

	(1) Science cit (present)	(2) Science cit (No self-cit)	(3) Science cit (First author)	(4) Science cit (At conf)	(5) Science cit (Not at conf)	(6) Similarity (Mean, Post)
Participation	0.021***	0.018***	0.014***	0.018***	0.011**	0.030***
	(0.007)	(0.007)	(0.005)	(0.006)	(0.005)	(0.007)
Science citations (L)	0.029***	0.024***	0.016***	0.018***	0.016***	0.006***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Patent citations (L)	0.008***	0.007***	0.004***	0.004***	0.005***	0.004***
	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Research similarity (L)	0.025***	0.023***	0.011***	0.010**	0.013***	0.727***
	(0.006)	(0.006)	(0.004)	(0.005)	(0.005)	(0.008)
Conf. distance controls	Yes	Yes	Yes	Yes	Yes	Yes
Conf Ser FE	Yes	Yes	Yes	Yes	Yes	Yes
Origin × Field FE	Yes	Yes	Yes	Yes	Yes	Yes
Origin × Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year × Origin FE	Yes	Yes	Yes	Yes	Yes	Yes
Year × Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.083	0.076	0.054	0.053	0.059	0.768
Observations	5126273	5126273	5126273	5126273	5126273	5126273
Number clusters	1114	1114	1114	1114	1114	1114
DV cond. mean	0.010	0.009	0.006	0.006	0.006	0.126
F (First)	31.6	31.6	31.6	31.6	31.6	31.5

Notes: Column 1 shows the default specification, counting all citations by firms within five years. Column 2 excludes all author-level self-citation. Firm-level self-citations are always excluded. Column 3 restricts to citations where the first author is affiliated with the firm. Column 4 restricts to citations where at least one author of the citing paper attended the conference. Column 5 restricts to citations where no author of the citing paper attended the conference. Column 6 shows the effect on mean text similarity of subsequent firm papers. * $p < .1$, ** $p < .05$, *** $p < .01$ Standard errors in parenthesis, clustered at the researcher region level. Firm-proceeding level dataset, where some proceedings were actually at the conference (Participation=1) and some were at another conference (Participation=0). Participation is instrumented by the direct flight availability between the researcher location and the conference location. Firm-proceeding controls include whether the firm cited previous work by the authors in the years before the conference (Science/Patent citations L) and the average abstract similarity between proceedings published by the firm in the previous year and the focal proceeding (Research Similarity L). Dependent variable mean is for actually presented proceedings. Column 4 also contains a control variable for the number of publications the similarity is computed for. Conf. distance controls include the distance between researcher and conference location (log), whether that distance is zero and whether the two locations are in the same US state or non-US country.

Pre-period Variables Test and Event Study Analyses

To test the plausibility of our identification assumption, we design an analysis analogous to a falsification test. If the participation at the same conference is, after instrumenting, truly exogenous, then within our models, *Participation* should have no effect in these tests, while non-well identified models should still have a high probability to find significant estimates. First, as dependent variable we use citations from firm publications published before the conference to proceedings authored by the authors of proceeding p. Similarly, the measure of text similarity introduced in the section above should not show an effect for similarity with proceedings of the firm previous to the conference. In other words, we estimate whether there is any correlation between *Participation* and measures that are predetermined at the time of the conference. These measures are analogous to the control variables we use in our models, but we used them here as dependent variables. For patents, we only consider scientific references introduced by patent documents published before the conference year.

Table 2.9 shows the results. We pair the estimations from our IV-model with OLS regressions. In particular, column 1, 3 and 5 show OLS estimates, while columns 2, 4 and 6 estimates from the IV-models. Column 1 and 2 present results for the falsification test based on scientific citations. Column 3 and 4, based on patent citations. Column 5 and 6, based on text similarity. Notably, we can still find statistically significant coefficients, indicating endogeneity issues, in the OLS models. On the contrary, we do not encounter any statistically significant correlation in our IV-models. This increases our confidence that the instrumental variable strategy truly generates exogenous variation in the variable of interest and allows us to estimate causal effects.

In a similar way, we can construct dependent variables that vary for each year, from the year of the conference to the following years. In this setting, for the variables were we find significant results, we find that the marginal probability of knowledge flows increases especially in the first years after the conference, but keep increasing up to the fifth year. Table B.10 shows these results for the main variable on scientific citations (other related analyses are equivalent and available upon request). The event study analysis in Appendix B.2.1, already mentioned in section 2.7.1, provides a similar insight with respect to the first-stage results. If either airlines' or conference organizers' decisions were driven endogenously by specific pair-level increases in participation from certain regions, this would likely emerge as a pre-trend in the probability of participation in the years prior to the change in availability of direct flights. We do not find any support for this concern.

Alternative Model Specifications

Finally, this section discusses additional results showing the robustness to alternative specifications, related to the inclusion of additional and different FE controls, and clustering of errors. The set of FE we include in our preferred specifications is meant to address the most reasonable concerns for our identification strategy. In Appendix, from Table B.13 to Table B.15 we show the robustness of our results to additional FE controls. Since conference locations are determined by unobserved amenities, the results may be driven by such factors. For example, it is evident how frequent conferences in Hawaii, Mallorca or Florida are compared to the resident scientist population (Figure 2.1). Therefore, we show that the results are robust to the inclusion of conference location times year FE (Table B.13). Consequently, the results are not driven by unobserved conference location amenities. Table B.14 shows robustness to the inclusion of origin × conference series fixed effects. This pair-level FE controls address possible concerns about the relative specificity of some conference series for scientists of certain regions. For instance, some conferences may have more national focus. This may influence the probability of participation of scientists regardless of accessibility and have implications also for the consequent interactions between scientists and firms. However, the inclusion of these FE controls does no change the results.

In table B.15, we control for the full interaction of firm, origin and year FE. This set of FE captures every possible variation of connectivity and interactions between firms and particular regions as would be embodied for example in direct-flight connections between firms and the researcher regions, over time. It emphasizes the distinction between direct flights to conference locations (which we study) as opposed to firm regions (which we control for), and excludes any possibility that contemporaneous shocks between the firm and the scientists' regions pairs affect the results. In this model, the residual variation comes from the possibility that firms may be differently exposed to scientists within one same region (for instance in different fields of specialization), due to the different accessibility of these scientists to multiple conferences where the firms participate. Despite this specification is highly demanding, the results remain largely unaffected.

In table B.16, we test the robustness to various other cluster levels. Given the nature of the dataset, arguments can be made to cluster on the firm level (the acting entity in the second stage), the researcher location × conference location level (the level of the instrument) or the proceedings. Along the same lines, two-way clustering is conceivable. We show that the standard errors in the second stage are not strongly affected by the cluster choice. However, the first stage often becomes substantially *more* powerful with alternative clusters.

Table 2.9: Falsification test - citations and similarity *before* the conference

	(1) Science cit (pseudo) OLS	(2) Science cit (pseudo) IV	(3) Patent cit (pseudo) OLS	(4) Patent cit (pseudo) IV	(5) Similarity (Mean, t-1) OLS	(6) Similarity (Mean, t-1) IV
Participation	0.029***	0.022	0.002***	0.010	0.016***	−0.008
	(0.002)	(0.031)	(0.000)	(0.007)	(0.000)	(0.011)
Conf. distance controls	Yes	Yes	Yes	Yes	Yes	Yes
Conf Ser FE	Yes	Yes	Yes	Yes	Yes	Yes
Origin × Field FE	Yes	Yes	Yes	Yes	Yes	Yes
Origin × Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year × Origin FE	Yes	Yes	Yes	Yes	Yes	Yes
Year × Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.211	0.211	0.048	0.046	0.594	0.580
Observations	5126273	5126273	5126273	5126273	5126273	5126273
Number clusters	1114	1114	1114	1114	1114	1114
DV cond. mean	0.066	0.066	0.005	0.005	0.087	0.087
F (First)		28.9		28.9		28.5

Notes: * $p < .1$, ** $p < .05$, *** $p < .01$ Standard errors in parenthesis, clustered at the researcher region level. Conf. distance controls include the distance between researcher and conference location (log), whether that distance is zero and whether the two locations are in the same US state or non-US country.Similarity refers to research similarity, measured by the similarity of abstracts of firm publications in the same subfield with the abstract of the focal proceeding.

2.10 Conclusion

Participation of firms to scientific conferences, perhaps today at unprecedented levels of intensity in fields like Machine Learning (ML), is not a rare phenomenon. Over 20 years, from 1996 to 2010 in our analysis sample (and up to 2015 for the entire initial sample), we find constant and significant participation of firms, in terms of proceedings authored by firms scientists and sponsorship of conferences in the entire field of Computer Science (CS). The key finding of the paper is that participation decisions of firms and scientists to different conferences have a causal effect on firms' innovation outcomes. In other words, firms scientific and technological outcomes rely strongly on knowledge within the scientific communities in which they participate. The effect is not confined to citations to proceedings at the conference, but, especially for patents, extends to citations to previous work of scientists at the conference. We also find a strong effect on the probability of scientific collaborations, but not, on average, on hiring. This suggests that actual collaborations with external scientists are one important mechanism explaining our results. These effects are much stronger and significant for firms capable of highly intense participation, as captured by their sponsorship of conferences, the number of proceedings presented and their research investments size. Participation of a scientist to a conference where a firm is present both as a sponsor and as proceedings' affiliation also leads to a higher probability of hiring. The effect for firms that make only minimal investments in participation is seldom significant.

The effect of active participation implies that physical proximity maintains an important role in the exchange of knowledge and the formation of collaborations. However, from a theoretical standpoint, the rationale for participation appears not confined to the need to abate search costs. Our finding is compatible with the theory that knowledge diffusion within science is shaped by its social norms and structure. The prestige of organizations within communities likely becomes a complementary asset that enables effective knowledge access, beyond the mere role of proximity. This provides the rationale for institutional investments in intense participation in scientific communities. This interpretation of the results is also supported by descriptive evidence and qualitative accounts on the nature of firms participation investments, and on their efforts to ensure that their contributions at scientific conferences are of the highest quality. The role of prestige appears all the more plausible if actual interactions and collaborations with external scientists, rather than the access to proceedings' content, is the main mechanism of diffusion.

These findings make various contributions. Similarly to previous studies, scientific communities appear to transcend organization boundaries, enabling knowledge flows between academia and industry and from science to technology. Also, the results confirm, on a larger scale and for knowledge flows across institutional boundaries, that scientists are strongly influenced by face-to-face interactions. The paper further suggests that active participation and prestige within the scientific community is an important antecedent to both collaborations and of knowledge flows, and may constitute a strategic objective for firms. Absorptive capacity appears not exclusively a function of cognitive ability or face-to-face interactions, but also of the prestige within scientific communities. Consequently, knowledge diffusion is likely channeled towards firms with the highest level of participation.

Implications are substantial. First, the results offer a different perspective on the apparent paradox that investments of firms in research decrease while the relevance of science for firms remain stable, if not higher. Academia and industry interactions emerge to be significant and not unidirectional. Scientific communities remain a platform that generates opportunities for effective interactions. As such, the active participation of industry maintains an important role in the diffusion of knowledge from science to technology. The participation in scientific communities may increasingly be a way to access external knowledge, without internalizing fully its production. The results also challenge the notion that proximity and participation naturally lead knowledge spillovers to spread equally and freely. Institutions capable of intense participation are more likely to absorb knowledge, which in turn reinforces their ability to establish a position within the scientific community, in a process akin to the Matthew effect usually attributed to the accumulation of prestige of scientists or academic institutions. As a consequence, contributions of firms to scientific communities are not necessarily in conflict with firm objectives and may lead to the concentration of innovation capacity. At the same time, this may not be a viable strategy for most firms. Those that are only able of limited and short-lived investments may gain no benefit from interactions with scientific communities, and more limited returns from investing in research in general.

Finally, we highlight some limitations and directions for future research. First, firms' performance and general welfare implications are delicate. Firms capable of large investments in participation may benefit directly from scientific communities and at the same time use the opportunity to guide scientific advancements towards economically valuable applications. On the basis of the evidence on the value of science for firm value and the high value of science-based patents, we posit that connections to scientific communities likely bring great values to such firms. However, strong connections with specific communities may have implications for the direction of research and the diversity of innovation options for firms, with not obvious long-run effects. Moreover, the evidence of concentrated knowledge flows may have implications for the competitive structure of science-based and high-tech industries, potentially increasing inequality. The scope of these considerations is limited by the microeconomic nature of our study and is left to future research. Second, the results suggest that knowledge flows across organizational boundaries are shaped by factors like group identity, reputation and prestige on social interactions that we cannot fully capture at our level of analysis. Related evidence at the individual level exists (Haeussler, 2011; Chen and Li, 2009; Charness, Rigotti, and Rustichini, 2007) which can be however extended to consider the interplay between the individual, organizational and institutional dimension. Finally, our study is limited to CS, although with data that are largely representative for this entire sector. Due to its importance for the economy, we tend to allege that evidence in this context is relevant. By virtue of descriptive evidence on the number of firms participating in conferences, and accounts regarding the relevance of science and conferences (e.g. in chemistry, pharmaceutics, biotechnology, engineering) (Cohen, Nelson, and Walsh, 2002), we also would expect similar evidence in other sectors. Extending this analysis to other contexts would still be recommended.

3

Profit Taxation, R&D Spending and Innovation

Abstract *We study how the taxation of profits affects plants' R&D spending and innovation. Relying on geocoded survey data for R&D-active plants in Germany over the period 1995-2007, we exploit around 7,300 changes in the local municipal business tax rate. Applying event study and difference-in-differences designs, we find a negative and statistically significant effect of an increase in profit taxation on R&D spending with an implied long-run elasticity of* -1.25. *Reductions in R&D are particularly strong among more credit-constrained plants but homogeneous across the firm size distribution. Along with the reduction in R&D spending, higher taxes trigger lagged negative effects on the number of filed patents. Extending the scope of our analysis beyond the plant level, we further show that reduced innovation accounts for up to 40% of the overall negative effect of business taxation on local economic growth.*

3.1 Introduction

Innovation has long been emphasized as a key driver of economic growth (Solow, 1957; Romer, 1990). Firms play a central role in this process by generating and diffusing the vast majority of new technologies and products. Firms' engagement in innovation increases their own performance (Kogan et al., 2017), raises their "absorptive capacity" of competitor knowledge (Aghion and Jaravel, 2015), and exhibits positive aggregate effects on economic growth because innovating firms crowd out the less productive ones (Lentz and Mortensen, 2008). Hence, whether and how public policy – and in particular tax policy – can foster firms' innovation activities is of key interest for economists and policy makers alike (Bloom, Van Reenen, and Williams, 2019).

In this paper, we provide new answers to this old question by exploiting the unique German institutional setting as a laboratory. We exploit variation in the local business tax, a profit tax set at the level of the municipality, to assess the effect of tax policy on plants' R&D activities. German municipalities can annually alter the local business tax rate, while the definition of the tax base is fixed at the federal level. Using survey data targeting all R&D-active plants in Germany, we exploit variation in tax rates induced by around 7,300 local tax reforms over the period from 1987 to 2013. As the given profit tax applies to nearly all German plants, we can study policy effects across the entire population of R&D-active plants.

Information on plants' R&D spending stems from the biennial longitudinal survey dataset *Wissenschaftsstatistik*, collected and administrated by the Stifterverband on behalf of the German Federal Ministry of Education and Research. The dataset serves as a key basis for Germany's official R&D reporting to EU authorities and the OECD. Using information on the plants' name and exact address, we assign treatment, i.e., the applicable local business tax rate in a given year in the respective municipality. We further use the detailed information in the survey to decompose plants' R&D spending along various margins, such as expenses on internally- vs. externally-conducted projects or personnel vs. non-personnel R&D spending.[1] In addition, we test for heterogeneous treatment effects along various plant characteristics (size, liquidity and R&D intensity). Last, we link the surveyed plants to administrative data from the European Patent Office. Thereby, we test to what extent tax-induced changes in R&D spending translate into differences in innovation output as measured by the (citation-weighted, i.e., quality-adjusted) number of filed patent applications.

We apply an event study design and complementary difference-in-differences regressions to estimate the causal effect of tax changes on plants' innovation activities. We separately consider tax increases and tax decreases in the empirical analysis.[2] Our preferred empirical specification regresses plant-level outcome variables on leads and lags of tax changes, conditional on plant and municipality fixed effects, sector $\times$ year fixed effects, as well as flexible and finely-grained region (e.g., commuting zone) by year fixed effects; the latter set of fixed effects accounts for unobservable time-varying confounders at very dis-aggregated geographical levels. Effect patterns around the date of treatment as well as additional robustness checks do not point to the presence of confounding effects, such as varying local economic

[1] We use the terms spending and expenditures as synonyms throughout the paper.

[2] As effects of tax increases and decreases do not have to be symmetric, we shy away from estimating effects in a joint regression framework. In Section 3.2, we show that more than 90% of all tax changes during the observation period were tax increases. We detect no clear pattern and face little statistical power when analyzing tax decreases. Therefore, we primarily focus on the effect of tax increases throughout the paper.

conditions, population movements or government expenditures coinciding with a given change in the local profit tax (see Fuest, Peichl, and Siegloch, 2018, for earlier evidence along this line).

Theoretically, we expect an increase of the profit tax rate to have a negative effect on plants' R&D spending and innovation. First, the tax-induced decrease in profits lowers plants' expected post-tax returns on R&D expenditures, which should lower the level of R&D expenses in turn. Second, we expect a tax increase to particularly affect those expenses that are financed by equity – given that only the costs of debt-financing are deductible from a plant's tax base. The nature of R&D activities suggests that this is particularly true for expenses on research and development: unfinished R&D projects have little residual value, lack collateral and face a high risk premium by debt-holders. Moreover, R&D investments are highly uncertain and potential returns generally realize with substantial time lags. Last, R&D projects usually come with information asymmetries between the innovator and financial backers (Hall and Lerner, 2010; Hall, 2002; Bakker, 2013). Using data on publicly-traded firms in the United Kingdom, Aghion, Klemm, et al. (2004) show that the use of equity finance indeed increases with firms' R&D intensity among the groups of firms engaged in innovation. Hence, we expect R&D investments to respond more to an increase in profit taxation than overall investments. This should particularly hold true for young and credit-constrained firms, where the lack of collateral is usually particularly pronounced (Brown, Fazzari, and Petersen, 2009; Thakor and Lo, 2017). Lastly, a reduction in R&D spending should eventually translate into reduced innovation output as measured via the number of filed patents (see, e.g., Griliches, 1990, for the assumed input-output relationship).

We produce five sets of empirical results that confirm these hypotheses. First, we find a negative, statistically significant effect of a profit tax increase on plants' total R&D expenditures. We estimate a long-term elasticity of -1.25, which is lower than estimates reported in the context of targeted R&D tax credits or subsidies. Second, we find the tax-induced reduction in R&D spending to be entirely driven by internally- rather than externally-conducted R&D spending. Plants appear to reduce R&D at their own research facilities in response to a tax increase, while outsourced research contracts remain unaffected.[3] Moreover, tax-induced reductions on non-personnel R&D expenses are larger than on R&D staff – a finding consistent with varying adjustment costs for these two production factors. Third, average effects mask heterogeneity by plant liquidity. Reductions in R&D spending are particularly strong among more credit-constrained plants. In contrast, we detect no notable differences along the plant size distribution. This somewhat questions common practice that R&D tax credits and subsidies are often size-dependent, with policy makers implicitly assuming small- and medium-sized firms to be more responsive to a given level of support (Gonzales-Cabral, Appelt, and Galindo-Rueda, 2018). Fourth, we observe tax-induced reductions in innovation output – both in raw numbers of patent applications but also when accounting for quality-differences by weighting each patent according to the number of citations it receives. The effect materializes with some temporal lag of around four years. The estimated long-term elasticity of patent applications with respect to the tax is -0.9, an estimate close to the findings of Akcigit, Grigsby, et al. (2018). Fifth, we extend the scope of the analysis beyond the plant level by assessing the role of innovation for economic growth, as well as quantifying the importance of tax policy in this relationship. We first show that local innovation has a positive and lasting effect on local growth, while an increase of the local business tax substantially reduces growth. Using the estimated elasticity of filed patents with

[3] On average, around 9% of the total R&D budget is spent on R&D activities outside the respective plant.

respect to the tax rate, we further back out that around 40% of the total negative effect of profit taxation on local growth is due to tax-induced reductions in innovation.[4]

Our baseline results are robust to a wide range of alternative specifications and modeling assumptions. Summary estimates and event study patterns do not change notably when controlling for broader or finer-grained region-by-year fixed effects . Moreover, effects are basically unchanged when including potential confounders at the municipality or county level, such as local population, unemployment or GDP per capita. Effects are also robust to different functional forms of the outcome variable and alternative ways of drawing inference.

Related literature. Our results contribute to various strands of the literature. First, we add to the small and recent literature that exploits variation in sub-national tax policy settings to study the effect of (corporate) taxation on innovation. Moretti and Wilson (2017) provide evidence on the geographic mobility of "star scientists" in response to tax policy variation at the level of the U.S. states. They find that star inventors are quite responsive to tax incentives, with long-run mobility elasticities amounting to around 1.8 (-1.7) for personal and corporate income taxes (tax credits). Akcigit, Grigsby, et al. (2018) use U.S. state-level panel data on corporate and personal income tax rates as well as on patents over the entire 20th century to study the effect of tax policy on innovation. They find that higher taxes reduce the quantity and quality of innovations and affect the geographic spread of innovative activities. Corporate inventors are found to react more to changes in (corporate) taxes than individual inventors. Exploiting the same variation in U.S. state-level tax rates over a shorter time period, Mukherjee, Singh, and Žaldokas (2017) offer similar evidence in showing that increases of the corporate tax rate reduce R&D investments and patenting. We add to these studies by looking at the German case, which offers unique variation in profit taxation at the level of roughly 11,000 municipalities. Official survey data targeting the universe of R&D-active plants further enable us to study detailed plant-level responses to changes in the local business tax rate, both in terms of innovation input and output (e.g., the effects of a tax increase on internal vs. external R&D spending, or process vs. product innovations). Moreover, we use the rich plant-level data to point to mechanisms that underlie the overall effects.

The paper further speaks to the literature that estimates the effects of targeted R&D tax credits, deduction possibilities and subsidies. Guceri and Liu (2019) exploit a 2008 reform in the UK's corporate tax scheme that increased R&D-related deduction possibilities for medium-sized firms relative to larger ones. Based on a difference-in-differences model, the authors estimate a positive and statistically significant impact of these tax incentives on R&D spending; the respective elasticity with respect to the user cost of capital amounting to -1.59. Dechezleprêtre et al. (2016) look at the same institutional setting but exploit asset-based thresholds for tax subsidy eligibility by means of a regression discontinuity design (RDD). They also find positive effects on R&D spending and patenting, with an implied user-cost elasticity of -3.0. Chen, Jiang, et al. (2019) show that a Chinese tax policy that awarded corporate income tax cuts to firms with R&D investments over a certain threshold substantially stimulated R&D activity. Using bunching techniques, the authors report a user cost elasticity of -2.0, which shrinks to -1.27 when the substantial relabeling of general to R&D expenditures is accounted for. Last, Agrawal, Rosell, and Simcoe (2020) exploit a 2004 reform of the Canadian R&D tax credit scheme for small firms. Using a difference-in-

[4] Note that tax policy can affect growth through other channels than innovation, e.g., by reducing investments not related to R&D, lowering wages, or triggering the re-location of businesses.

differences approach, they estimate a large positive effect on R&D activity among eligible firms, yet report rather wide bounds for the user-cost elasticity in the range between -0.71 to -4.57. In terms of R&D subsidies, Bronzini and Iachini (2014) evaluate a 2003 reform in Northern Italy, which introduced R&D subsidies for certain industrial research projects. Using a regression discontinuity design around the eligibility cut-off, they find no significant impact on R&D investments on average. However, the average effect masks heterogeneous responses: small firms significantly increased their R&D investments in response to the subsidy, whereas larger firms remained unresponsive. We estimate an R&D spending elasticity with respect to the user cost of capital of -2.66, that is well in the range of the discussed studies.[5]

This set of studies provides clean causal evidence by exploiting policy cut-offs to establish quasi-experimental research designs. At the same time, the estimates are clearly local in nature, referring to firms around the respective thresholds. The proposed identification strategy in this paper enables us to estimate treatment effects along the full distribution of R&D-active plants. Hence, we are able to identify average treatment effects but also test for heterogeneous effects along various plant characteristics. For instance, we show that effects are homogeneous across plant size, which questions the rationale for size-based innovation policies to some extent. We are further able to rule-out mis-reporting effects as shown in Chen, Jiang, et al. (2019), given that our policy instrument, the local business tax rate, does not specifically target plants' R&D spending.

Finally, we connect to a large literature that is concerned with market failures that reduce private R&D activities below socially desirable levels: R&D embodies characteristics of a public good (Nelson, 1959; Arrow, 1962), such that the social rate of return to innovation is generally well above the private return (Griliches, 1992; Jones and Williams, 1998). At the same time, expected knowledge spillovers as well as uncertainty about marketability may lead to private under-investments into R&D (Czarnitzki and Toole, 2011). Taxes on firms may further lower the private returns to R&D, while social returns remain unaffected. This, in turn, widens the gap between actual and socially desired levels of R&D in an economy (Klenow and Rodriguez-Clare, 2005).

The remainder of the paper is structured as follows. Section 3.2 describes the institutional background of German profit taxation and documents the policy variation we exploit for identification. Section 3.3 describes the plant-level survey data, as well as the matching of patent information and additional financial variables to the surveyed plants. In Section 3.4, we set up our empirical research design and test the plausibility of the set-up's underlying identifying assumptions. Section 3.5 presents the empirical results. We start by presenting average and heterogeneous treatment effects on R&D spending (Section 3.5.1), before turning to the effects on patenting (Section 3.5.2). In Section 3.5.3, we extend our analysis beyond the plant level by assessing the importance of innovation for local economic growth as well as identifying the role of tax policy in this relationship. Section 3.6 concludes.

[5]Our estimate of the R&D spending elasticity with respect to the user cost of capital is at the upper bound of recent studies that look at overall investment responses to tax-induced changes in the user cost of capital. See, for example, Yagan (2015), Zwick and Mahon (2017), Ohrn (2019), Maffini, Xing, and Devereux (2019), Chen, Jiang, et al. (2019), and Moon (2020).

3.2 Institutional Background

In Germany, business profits are taxed along two different margins. First, at the national level, profits are either subject to the corporate or personal income tax depending upon a firm's legal status. In addition, both corporate (*Kapitalgesellschaften*) and non-corporate firms (*Personengesellschaften*) are subject to the local business tax (*Gewerbesteuer*), which is levied at the municipality level.[6] Our analysis will exploit within-municipality variation in local business tax rates for identification.

The local business tax (LBT) serves as municipalities' most important source of revenue. Municipalities have considerable discretion over the size of the LBT rate. It is derived as the product of the basic federal tax rate (*Steuermesszahl*) and a local scaling factor (*Hebesatz*), which acts as a municipality-specific multiplier:

$$Local\ Business\ Tax\ Rate = Basic\ Federal\ Rate \times Municipal\ Scaling\ Factor.$$

This scaling factor serves as municipalities' sole margin of adjustment. Municipal councils annually decide whether and how to adjust the scaling factor for the upcoming year. In contrast, the basic federal tax rate is set by the national government and uniformly applies to all municipalities.[7] This is also true for the calculation of the tax base: rules are defined at the national level and cannot be altered by state or municipal governments. The definition of the tax base remained unchanged throughout our observation period from 1995-2007.[8]

Figure 3.1 illustrates the spatial and temporal variation in local scaling factors across West Germany.[9] Panel A plots, as an example, the 1995 LBT rates for each West German municipality. We observe substantial differences across Germany, with tax rates varying between zero and 45 percent (1% percentile: 12.5%; 99% percentile: 22.5%).[10] In addition, we see a fair amount of spatial clustering; in particular at the level of the federal states. The latter can be reconciled with varying fiscal equalization schemes across federal states, a feature we account for by controlling for state × year fixed effects in our preferred empirical specification (see Section 3.4.1 for details). Panel B highlights the substantial amount of variation in LBT rates within municipalities over time. On average, municipal councils decided to alter their local business tax rate 3-4 times between 1987 and 2013 — the first and last year of tax data used in our baseline analysis.[11] Around 10% of the sampled municipalities did not adjust their scaling factor during this time span.

Panel A of Figure 3.2 further shows that more than 90% of all tax changes during this time period were tax increases. The average tax increase amounted to around one percentage point, or 5 percent relative to the mean. Figure 3.2 further illustrates that there is meaningful variation in (long-run) tax policies across West German municipalities. One might worry that municipalities adopt different reform strategies (e.g.,

[6]Note that most firms from the agricultural sector, non-profit organizations as well as self-employed individuals in liberal professions (such as accountants, journalists or architects) are exempt from this tax.

[7]The basic federal rate was 5.0% until 2007. It was reduced to 3.5% in the course of the 2008 German business tax reform.

[8]The tax base was defined as firms' operating profits net of 75% of their costs of debt financing and the local business tax expenditures themselves.

[9]Many municipal borders in East Germany were redrawn during the 1990s and 2000s. As we cannot assign the exact LBT rate for affected jurisdictions, we decided to discard East Germany from the analysis.

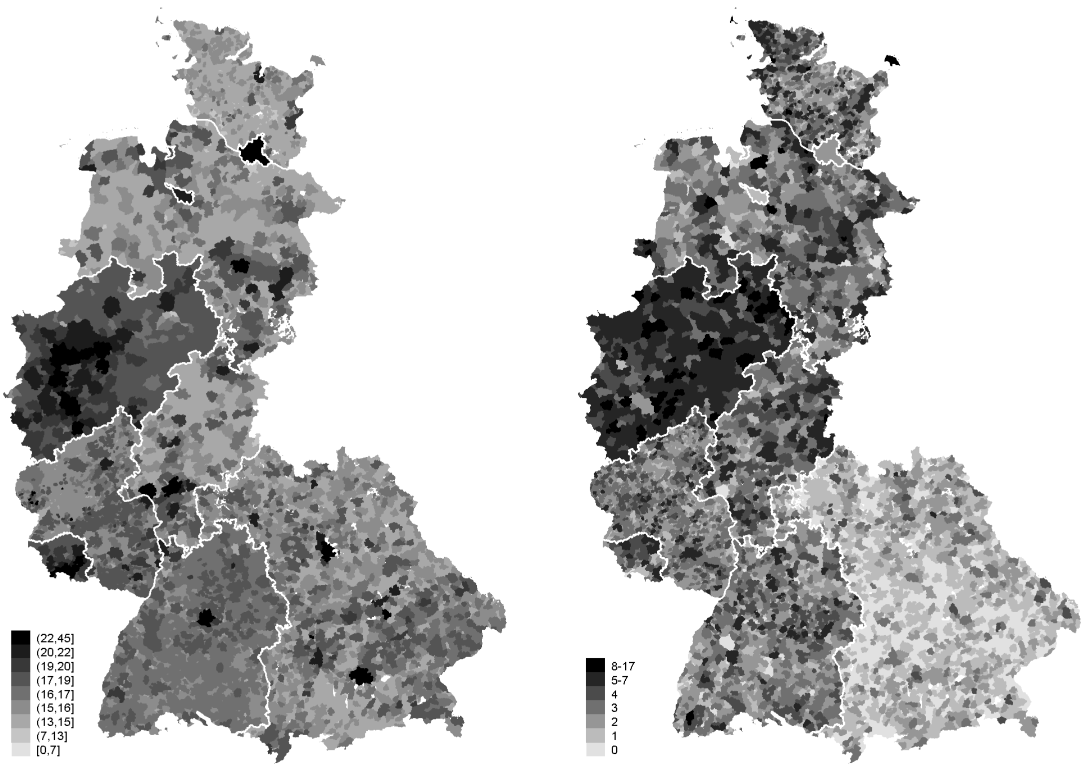
[10]Note that municipalities in our sample have strictly positive local business tax rates throughout the effect window.

[11]We focus on outcomes between 1995 to 2007 and estimate a dynamic event study specification with a lag of eight and a lead of six years in our baseline specification; see Section 3.4.1 for details.

many small reforms vs. a few large) that eventually all lead to the same change in tax rates over the long run. However, as displayed in Panel A, we find the majority of tax changes to be rather

Figure 3.1: Spatial and Temporal Variation in Local Business Tax Rates

(a) The Local Business Tax Rate in 1995 (in %) (b) Number of Tax Changes (1987–2013)



Notes: This figure illustrates the spatial and temporal variation in the local business tax rate across West German municipalities. In Panel A, the 1995 local business tax rate is plotted for each municipality. Darker colors indicate higher levels of the LBT. In Panel B, for each municipality the number of total LBT changes over the period 1987-2013 is plotted. Darker colors indicate a larger number of tax changes in a given municipality. Thick white lines indicate federal state borders. The maps are based on shapefiles from ©*GeoBasis-DE / BKG 2015.*

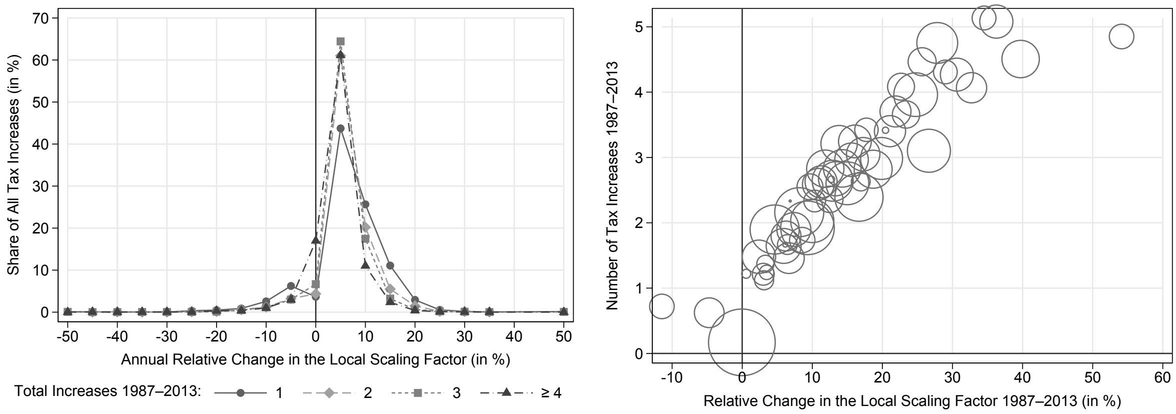
small LBT increases, irrespective of how often municipalities alter their local scaling factor during the effect window. This finding is corroborated by Panel B, which points to meaningful differences in the long-term tax policies of municipalities. Between 1987 and 2013, changes in the LBT scaling factor varied substantially. Moreover, we detect a positive relationship between the number of tax changes and the total change in the scaling factor.

The institutional features of the LBT allow us to base identification on a large number of very local tax changes while flexibly controlling for common shocks at the federal state or commuting zone level (see Section 3.4.1 for details on the empirical strategy pursued). In addition, and in contrast to most other OECD countries, Germany offered no direct or indirect tax subsidies for firms' R&D spending during the time of this analysis (in fact until January 1 2020). This makes the country an ideal laboratory for the research question of interest because no other tax policies need to be accounted for.[12] Moreover, and despite this institutional feature, Germany ranks among the world's most innovative countries; see, e.g., the annual Bloomberg Innovation Index. During the period from 1995 to 2007, the country's total R&D

[12]Note that Germany has long used direct R&D subsidies as a policy instrument to promote firm-level innovation.

Figure 3.2: Variation in LBT Scaling Factors - All West German Municipalities

(a) Distribution of Annual Scaling Factor Changes (b) Tax Changes over the Sample Period



Notes: This graph illustrates the variation in the LBT rate changes across all West German municipalities. Panel A illustrates the distribution of annual scaling factor changes for municipalities with varying numbers of total tax changes throughout the effect window (1987-2013). Panel B illustrates the municipality-level relationship between the total change in the LBT rate and the number of tax changes throughout the period 1987–2013.

expenditures amounted to around 2.35% of its GDP on average, which is close to U.S. levels (2.54%) and much higher than the EU-28 average of 1.65%.[13]

[13]Own calculations based on the OECD's Main Science and Technology Indicators database.

3.3 Data

R&D Plant-Level Survey Data. Our main data source is the biennial longitudinal survey dataset *Wissenschaftsstatistik*, collected and administrated by the Stifterverband on behalf of the German Federal Ministry of Education and Research. The survey targets all German plants engaged in R&D[14] and forms one key basis of Germany's official reporting on its entrepreneurial R&D activities to EU authorities and the OECD. The survey contains detailed information on plants' overall R&D spending, their R&D expenses by subcategories (internally- vs. externally conducted R&D; personnel vs. non-personnel R&D spending) and their R&D staff. Moreover, it offers information on plant size, industry classification and plants' organizational structure. By special agreement with the Stifterverband, we also gained access to each plant's legal name and exact address (postal code, street and house number) in a given year, which allows us to precisely assign the applicable LBT.

Our baseline observation window spans the period from 1995 (the earliest year of the survey) to 2007. We do not cover years beyond 2007 for two reasons. First, we bypass potential R&D effects due to the Great Recession in 2008-2009. Second, a major tax reform in 2008 altered institutional features of the LBT, lowering the federal tax rate from 5.0 to 3.5 percent and broadening the tax base. Besides this restriction, we constrain our baseline sample among two additional margins. First, we discard 649 plants (6% of the total sample) that report R&D activities not only for their own site but for the entire company (at different locations). By applying this restriction, we make sure to compare local changes in the LBT to local plants' responses. Second, we drop 285 plants who moved during the survey period to exclude variation that is due to potentially endogenous mobility decisions.[15]

Ultimately, our baseline sample contains 31,648 unbalanced plant-year observations from 10,056 plants in 2,442 different municipalities. In total, these plants spent around 37 billion EUR per year on R&D, which accounts for around three-quarters of Germany's total R&D expenditures during this period.[16] Manufacturing plants account for around 94% of these expenses.[17] In Panel A of Figure 3.3, we illustrate the spatial distribution of R&D-active plants (as of 2007) across West German municipalities. We find R&D activity to be widespread across the country: around one-fourth of all West German municipalities have at least one R&D active plant. However, we also detect some R&D clusters, in particular among centers of German industry alongside the rivers Rhine and Ruhr, as well as in the states of Baden-Wuerttemberg, Rhineland-Palatinate, and North Rhine Westphalia.

At the level of the individual plant, we detect substantial differences in the size of annual R&D spending. Panel A of Appendix Table C.1 shows that the total annual amount of plants' spending on R&D varies from around 11,000 EUR (1% percentile) to around 91 million EUR (99% percentile). The survey further allows the disaggregation of plants' total R&D expenses along two margins. First, information on plants' expenses for internally- vs. externally-conducted R&D projects is given. Internal and external R&D are usually seen as complementary innovation activities, where the marginal return of one activity

[14]Data Appendix C.1 provides detailed information on the underlying survey methodology.

[15]We find very similar effects when including these 285 plants and assigning them the corresponding tax rates in their first observed municipality of residence over all years (see below).

[16]The vast majority of the remaining R&D volume can be referred to the 649 plants in the data that report R&D activities not only for their own site but for the entire company.

[17]In the baseline sample, 81% of all plants are from the manufacturing sector. Notably, this composition is at odds with the overall German industry structure, where the share of manufacturing plants is substantially lower (at around 9%).

increases with the intensity of the other (Cassiman and Veugelers, 2006). In this regard, external R&D is typically used as a strategy to acquire knowledge, either by engaging in licensing and outsourcing or starting strategic alliances. While the former allows firms to exploit economies of specialization and scale, strategic cooperation generally aims at the development of new technological capabilities (Bönte, 2003; Lokshin, Belderbos, and Rene, 2008). However, the search for and coordination of external contractors and collaborations also comes with sizable (transaction) costs that may prevent some firms from engaging in external R&D activities (Berchicci, 2013). In our baseline sample, half (49.7%) of the covered plants outsource parts of their R&D activity at least once during the sampling period. On average, external R&D accounts for around 9% of plants' total R&D (20% if we consider plants with non-zero external spending only). Second, we can distinguish internal R&D spending on personnel from non-personnel expenses (i.e., for materials and investments). On average, two-thirds of a plant's internal expenses accrue to its scientific staff.

Plant-Level Patent Data. To measure innovation output, we link administrative information from the European Patent Office (EPO) on plants' patenting activities to the survey data; see the Data Appendix C.1 for a detailed description of the matching procedure. From 1995 to 2007, the covered plants filed 151,862 patents, which accounts for around 60% of all patents filed by German applicants at the EPO during this period.[18] Panel B of Figure 3.3 shows the spatial distribution of patent activity across West Germany. The spatial pattern is in line with the regional prevalence of R&D plants. One quarter of all covered plants filed at least one patent during our sampling period. In our baseline specifications, we explicitly account for the large number of zeros in the plant-level patent data by applying an inverse-hyperbolic-sine transformation to the outcome variables. In the appendix, we investigate the relationship between R&D spending and patenting in detail. We find that (i) patenting predominantly occurs in manufacturing and (ii) the number of patents convexly increase in R&D spending (see Appendix Figure C.1).

A simple count of plants' number of filed patents may only imperfectly capture the true value of innovation output if patent quality varies (Scherer, 1965; Hall, Jaffe, and Trajtenberg, 2005). To this end, we construct a second measure of plant-level innovation activity that accounts for a patent's number of citations to infer innovation quality. Previous evidence (see, e.g., Harhoff, Scherer, and Vopel, 2003; Kogan et al., 2017; Moser, Ohmstedt, and Rhode, 2018) has shown that such citation-weighted measures of patents correlate well with other proxies of innovation quality (such as profitability). We construct our measure by weighting each patent by the number of citations it receives from other EPO patents within five years of a given patent's first registration. As shown below, we obtain very similar results when using citations from patents filed at the United States Patent and Trademark Office (USTPO).

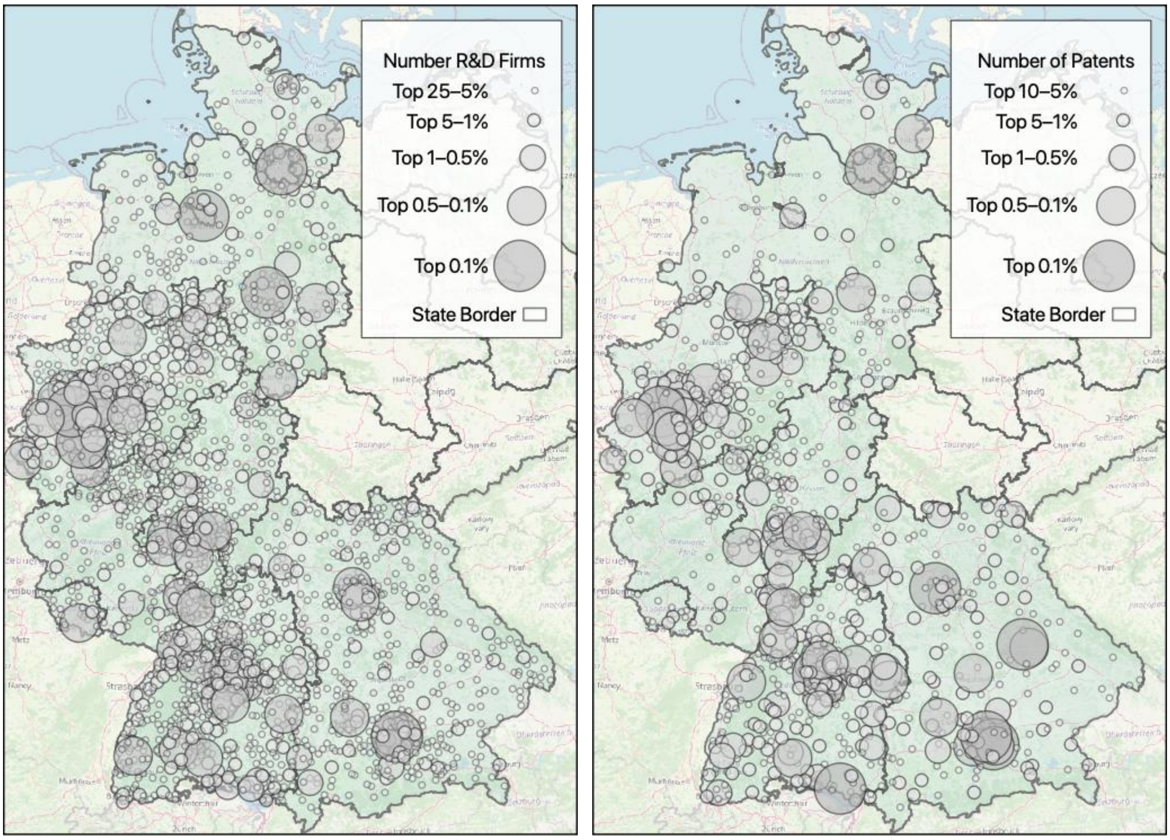
Using detailed textual information from the patent application files, we further distinguish *product* from *process* innovations.[19] Product innovations generally relate to new or substantially-altered products that may lead to high social returns. However, these innovations can be easily appropriated by rival firms and face high market uncertainty, which renders private returns uncertain (Hellmann and Perotti, 2011). In

[18] By definition, we do not capture patents filed by the government, public universities or individual inventors. Moreover, not all plants that file a patent during the observation period are covered in the Stifterverband data and our baseline sample, respectively. This is especially true for plants with very little or infrequent patent activity throughout the observation period.

[19] More precisely, we follow Danzer, Feuerbaum, and Gaessler (2020) and consider patents as process innovation in case keywords such as "method", "process", or "procedure" appear in the respective patent's claims text. Note that 12% of all patent applications do not provide the necessary information to classify an innovation accordingly.

Figure 3.3: Spatial Distribution of R&D Firms and Patenting in West Germany

(a) Distribution of R&D Plants (b) Distribution of Patents



Notes: Panel A illustrates the distribution of plants in the R&D plant-level dataset *Wissenschaftsstatistik* as of 2007 across West German municipalities. Larger circles illustrate more R&D active plants in a given municipality. Panel B plots the spatial distribution of patenting across Germany. Larger circles indicate that more patents were filed in a given municipality throughout our observation period from 1995-2007. The maps are based on shapefiles from © *GeoBasis-DE / BKG 2015* and *OpenStreetMap* contributors and show Germany's jurisdictional borders as of December 31 2010.

contrast, process innovations, i.e., improvements of a given production process, are commonly considered as the more incremental ones that yield lower social returns but also bear lower risk (Klepper, 1996). We will test below whether an increase of the local profit tax affects both types of innovations to a different extent.

Panel B of Appendix Table C.1 provides descriptive statistics on plants' patenting activities. The average plant files 0.84 patents per year, which receives one citation over the following five years on average. Around 60% of all patents can be categorized as product innovations. However, and as stated before, these average descriptive statistics mask substantial heterogeneity.

Additional Plant Data. While the *Wissenschaftstatistik* offers detailed information on plants' R&D behavior, little information is given on plants' financial situation. Theoretically, we may, however, expect liquidity-constraint firms to react more to a tax increase because costs of debt financing can be deducted from the tax base while those of equity financing cannot. To proxy plants' financial situation, we therefore add information from the Bureau van Dijk's *Amadeues* and *Orbis* databases to the plant-level survey; in particular, information on plants' level of non-current liabilities. Unfortunately, the detailed information on plants' financial accounts is available for public firms only. Thus, we can only supplement 40% of the surveyed R&D plants with information from this additional data source.[20]

Municipality- and County-Level Data. Last, we complement the plant-level data with annual information on local business tax rates as well as other regional, i.e., municipality- and county-level information. This includes data on municipalities' annual public expenditures, population levels, unemployment level or county-level GDP. We will use these variables to test whether economic developments at the local level may simultaneously determine municipalities' tax setting and plant activities. Panel C of Appendix Table C.1 provides the corresponding descriptive statistics. On average, innovation predominantly occurs in urban, industrialized regions with relatively little unemployment. Municipalities in our sample are larger (27,663 vs. 7,680 inhabitants) and have a slightly higher GDP per capita (27,656 vs. 24,087 EUR) than the average West German municipality.

3.4 Empirical Strategy

To estimate the causal effect of changes in the LBT on plant-level R&D expenses and innovation, we exploit all available changes in the tax rate within a municipality over time in a dynamic generalized difference-in-differences framework with staggered treatment (Suárez Serrato and Zidar, 2016; Fuest, Peichl, and Siegloch, 2018; Akcigit, Grigsby, et al., 2018). In Section 3.4.1, we describe the empirical implementation. We discuss the identification of causal effects in our model in Section 3.4.2.

3.4.1 Event Study Design

We base our analysis on an event study setup that treats each tax reform as an independent event. This allows us to exploit all available variation in local tax rates across municipalities and years. More precisely, we regress a given outcome Y_{it} of plant i in year t. A plant belongs to a sector s (manufacturing, services, and other) and is located in a municipality m, which is nested in a commuting zone z. We regress the outcome Y_{it} on leads and lags of the treatment variable T_{mt}^{k} (the number respectively the size of tax changes, as further defined below):

$$Y_{it} = \sum_{k} \beta_k T_{mt}^{k} + \mu_i + \lambda_m + \zeta_{st} + \theta_{zt} + \varepsilon_{it}. \tag{3.1}$$

We transform outcomes – R&D spending, the number of patents, and various subcategories of the two – using the inverse hyperbolic sine (IHS) transformation.[21] To control for unobserved time-invariant confounders at the plant and municipality level, we include fixed effects at the respective levels (μ_i and λ_m). Moreover, state × year and commuting zone × year fixed effects, both included in term θ_{zt}, as well sector × year fixed effects, ζ_{st}, control for regional and sectoral time-varying confounders, respectively.

[20] However, baseline effects remain unchanged when using this smaller set of plants (see Section 5).

[21] For any outcome $\tilde{y}$, the inverse hyperbolic sine transformation is defined as: $y = \ln(\tilde{y} + \sqrt{\tilde{y}^2 + 1})$. This transformation comes with the advantage of being well-defined for zero values in the outcome variable. This is particularly relevant for the plant-level patent outcomes in the context of this study. For larger values, the IHS transformation is almost identical to the canonical log transformation. In Section 3.5, we show that the transformation of outcome variables does not drive our estimates.

We calculate cluster-robust standard errors that account for potential correlations across plants, years, and sectors within municipalities.

We adjust this generic event study outline in three dimension to fit our empirical setting. First, we account for the biennial structure of the plant-level R&D survey. To harmonize estimation samples across outcomes, we restrict the analysis for all outcomes to the subset of years $t = 1995, 1997, \ldots, 2007$. Leads and lags of the treatment variable, T^k_{mt}, thus sum tax changes in two consecutive years to account for tax reforms in even-numbered years as well. In our preferred specification, we restrict the effect window to six years before and eights year after a tax reform, i.e., three leads and four lags in the given two-year structure of the data, $k \in [-6, -4, \ldots, 8]$.

Second, we account for multiple tax changes per municipality by binning the endpoints of the effect window (McCrary, 2007). Hence, the first lead of the treatment variable, T^{-6}_{mt}, and the last lag, T^8_{mt}, take into account all tax reforms that will happen six or more years into the future from period t onward or happened eight or more years in the past, respectively. The implicit underlying assumption is that (pre)-treatment effects are constant beyond these endpoints (Schmidheiny and Siegloch, 2020). Formally, we define the leads and lags of the treatment variable in the following way:

$$T^k_{mt} = \begin{cases} \sum_{j=-\infty}^{-6} D_{m,t-j} & \text{if } k = -6 \\ D_{m,t-k} & \text{if } -6 < k < 8 \\ \sum_{j=8}^{\infty} D_{m,t-j} & \text{if } k = 8, \end{cases} \tag{3.2}$$

where $D_{m,t}$ is the actual tax reform indicator denoting treatment in year t or $t-1$. In our empirical specification, we normalize the last pre-treatment coefficient, β_{-2}, to zero, i.e., all effects are relative to two years before treatment.

Third, we use two alternative definitions of treatment, i.e, variation in the LBT rate τ_{mt}: (i) the number of tax changes during the past two years (Equation 3.3), and (ii) a continuous treatment variable capturing the size of the two-year change in the local business tax rate (Equation 3.4):

$$D^{inc}_{m,t} = \mathbb{1}(|\tau_{mt}| > |\tau_{m,t-1}|) + \mathbb{1}(|\tau_{m,t-1}| > |\tau_{m,t-2}|) \tag{3.3}$$

$$D^{cha}_{m,t} = |\tau_{mt}| - |\tau_{m,t-2}|. \tag{3.4}$$

Implied Elasticities. While event study estimates inform about dynamics of treatment effects, it is useful to derive one central take-away elasticity. Our baseline summary measure is the elasticity as implied by the estimates of the last lag in the event study regressions, $\widehat{\beta}_8$, which measures the long-run effect more than seven years after the tax reform. The event study model is able to identify this effect in the case of multiple treatments as long as there is enough variation in the rhythm of subsequent tax changes across municipalities (cf. Figure 3.2). We compare these implied long-run elasticities to alternative summary measures in Section 3.5.

3.4.2 Identification

To estimate causal effects, we relate within-plant changes in R&D activities to changes in LBT rates while absorbing common shocks to federal states, commuting zones and economic sectors. To interpret estimates $\widehat{\beta_k}$ as causal effects, we have to assume that tax changes are not systematically correlated with other time-varying local factors within the same federal state, commuting zone and economic sector that also affect plants' expenses or innovative activities. Small and insignificant pre-treatment coefficients in the event study would support this assumption, as most confounding effects that violate the identifying assumption would show up as diverging pre-trends. If reverse causality was an issue – i.e., if local policy-makers would adjust LBT rates because of changes in plants' innovative activities – we should also observe diverging trends in R&D investments *before* treatment. As shown below, we find hardly any support for this concern.

Another concern for identification are confounding shocks that coincide with the tax change, but have no visible effect before treatment. Whether such shocks are able to impede the identification of causal effects depends on the geographical level at which they arise. Our preferred specification includes state-by-year fixed effects, which control for any change in state policies or varying electoral cycles. However, shocks might also occur below the state level. To this end, we also account for time-varying economic or political shocks at the level of the 204 West German commuting zones (*Arbeitsmarktregionen*, henceforth CZ) in our baseline specification.[22]

To test the sensitivity of our results with respect to the potential presence of regional shocks at varying geographical levels, we deviate from this baseline model and replace the CZ-by-year fixed effects with coarser or finer regional controls below. If systematic local shocks were to violate our identifying assumption, we would expect results to differ alongside these changes in the exact specification of the event study model. We absorb common shocks at the level of the 28 administrative districts (*Regierungsbezirke*, NUTS II), the level of the 71 statistical planning regions (*Raumordnungsregionen*, ROR), and the 272 counties (*Kreise und kreisfreie Städte*) in West Germany, respectively. Appendix Figure C.2 illustrates these different jurisdictions for the example of the federal state of Bavaria. As shown in Section 3.5, we find a stable dynamic treatment effects when moving between specifications, both pre and post-treatment. If anything, we see post-treatment effects to become larger (in absolute terms) when controlling for shocks at finer regional levels.

We further acknowledge that economic or political shocks may also occur at the municipality level and, potentially, coincide with the (level of the) tax change itself. In this regard, one might particularly worry that local economic developments at the municipal level simultaneously determine municipal tax setting and plant behavior. We address this concern twofold. First, we show that socioeconomic indicators at the municipality level (population, the share of unemployed among the population, public expenditures, and public revenues) do not display any systematic pre- or post-trend when used as dependent variables in the event study model as set-up in Equation (3.1); a finding in line with previous studies (Fuest, Peichl, and Siegloch, 2018; Blesse, Doerrenberg, and Rauch, 2019).[23] Second, we sacrifice some econometric

[22]On average, there are eight municipalities and 25 plants per commuting zone in our baseline sample.

[23]In Section 3.5.3, we investigate the effect of an increase of the LBT rate on local GDP in detail. Besides establishing flat pre-trends, we show that local GDP declines in response to a tax rate increase (see Panel A of Figure 3.11).

rigor and include lagged socioeconomic indicators in our baseline event study model. As shown below, estimates remain very much unaffected by the inclusion of these controls.

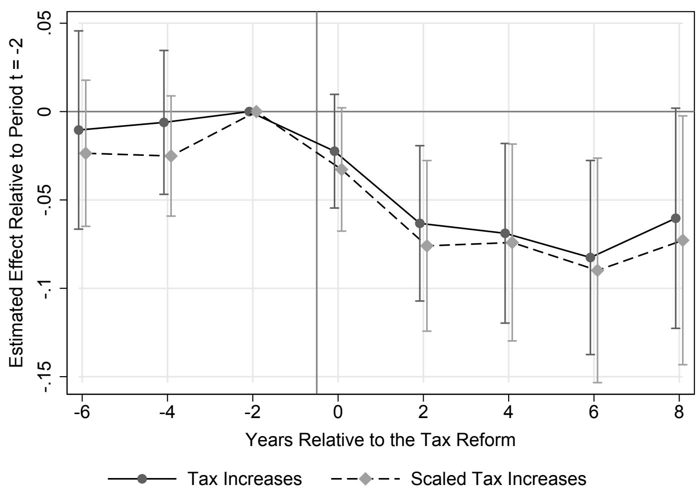
3.5 Empirical Results

We next present our empirical findings. In Section 3.5.1, we first focus on the effect of a change in the local business tax rate on plants' total R&D spending and various sub-margins thereof. Second, we derive an estimate of the user-cost-of-capital elasticity and relate this to the implied elasticities from previous studies. Last, we test for heterogeneous treatment effects along different plant-level characteristics. In Section 3.5.2, we assess the corresponding effects of a change in the LBT rate on innovation output as measured by filed patents. Finally, in Section 3.5.3, extend the analysis beyond the plant level by exploring the importance of innovation for local economic growth and assessing the role of tax policy in this context.

3.5.1 Effects on R&D Spending

Main Results. Figure 3.4 presents the estimated dynamic effects of an increase in the LBT rate on plants' total R&D spending using the event study models as defined in Equations (3.1)–(3.4). First, we see that pre-trends are reasonably flat and statistically insignificant. Post treatment, we estimate that an increase in the LBT rate exerts a substantially negative and statistically significant effect on plants' total R&D spending. This effect builds up over the first two to three years and levels off thereafter. Estimates

Figure 3.4: The Effect of a Business Tax Increase on Total R&D Spending



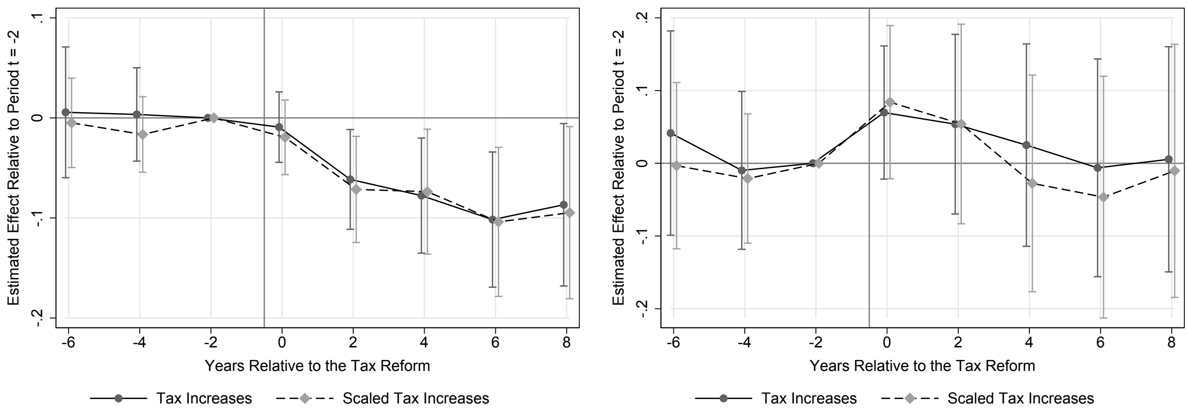
Notes: This graph plots the point estimates, $\widehat{\beta_k}$ ($k \in [-6, -4, \ldots, 8]$), and corresponding 95% confidence intervals of the event study models as defined in Equations (3.1)–(3.4). The dependent variable is a plant's annual (inverse hyperbolic sine transformed) total R&D spending. For the treatment group, the business tax change occurred in year $t = 0$ or $t = -1$. The regressions include plant, municipality, state × year, commuting zone × year, as well as sector × year fixed effects. All municipalities that experienced a tax decrease during the event window period are excluded. Standard errors are robust to clustering at the municipality level.

are very similar when using the number of tax increases or the actual size of the tax rate increase as treatment. This reflects the fact that the average tax rate increase is almost equal to a one percentage point increase (cf. Section 3.3). Our baseline summary measure of the implied effect size is the long-term elasticity as given by the last post-treatment estimate, $\widehat{\beta_8}$, of the dummy variable specification of the event study model (cf. Equation (3.3)). For total R&D spending, this elasticity amounts to -1.25. Below, we report alternative summary estimates of the effect size.

When assessing the effect of a decrease of the local business tax on plants' total R&D spending, we detect no clear post-treatment effect pattern and face little statistical power (cf. Appendix Figure C.4). However, in light of the small number of tax decreases in the data, this finding is not surprising. Hence, the remainder of this subsection focuses on the effect of tax increases only.

Figure 3.5: The Effect of a Business Tax Increase on Internal and External R&D Spending

(a) Internal R&D Spending (b) External R&D Spending



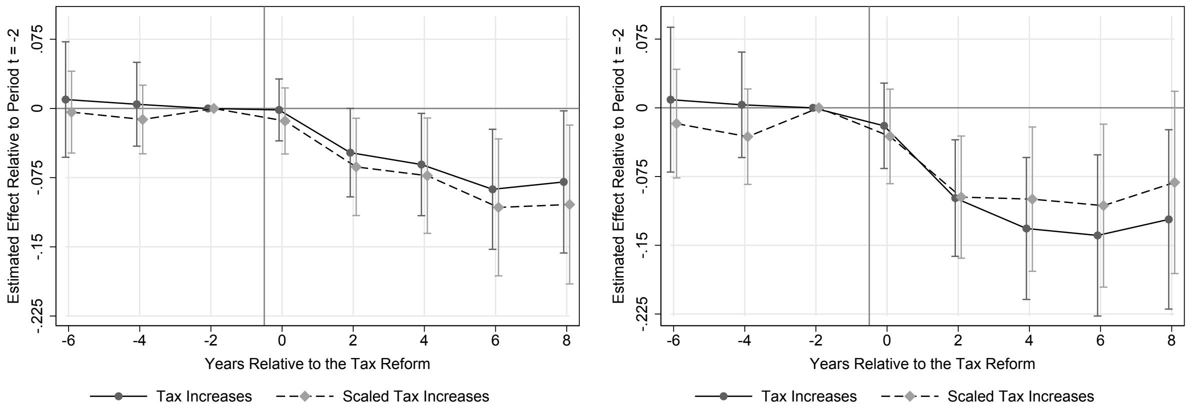
Notes: This graph plots the point estimates, $\widehat{\beta_k}$ ($k \in [-6, -4, \ldots, 8]$), and corresponding 95% confidence intervals of the event study models as defined in Equations (3.1)–(3.4). The dependent variable is a plant's annual internal R&D spending in Panel A, and a plant's annual external R&D spending in Panel B. Both outcomes are inverse hyperbolic sine transformed. For the treatment group, the business tax change occurred on in year $t = 0$ or $t = -1$. The regressions include plant, municipality, state × year, commuting zone × year, as well as sector × year fixed effects. All municipalities that experienced a tax decrease during the event window period are excluded. Standard errors are robust to clustering at the municipality level.

We next test whether spending adjustments in response to an increase in the LBT rate differ with regard to internally- vs. externally-conducted R&D projects. As discussed above, previous research suggests that both types of R&D activities should be seen as complements in plants' innovation behavior (Cassiman and Veugelers, 2006), where external R&D serves as a strategy to acquire missing knowledge through licensing, outsourcing or the start of strategic alliances. Figure 3.5 presents the corresponding estimates for internal and external R&D spending. We first note that pre-trends become even flatter when disaggregating plants' total R&D expenditures along this margin. Moreover, we find the effect on total R&D to be entirely driven by reductions in plants' internal R&D expenditures (see Panel A of Figure 3.5). In contrast, we detect no effect on external R&D expenditures (see Panel B). We suggest that this asymmetry may be due to the type of R&D projects typically conducted inside and outside a given plant, as well as reflective of the substantial transaction costs associated with the outsourcing of R&D to external partners.

Last, we distinguish plants' internal spending on R&D personnel – accounting for around two-thirds of plants' internal R&D expenditures – from non-personnel R&D expenses and test for differential effects along this margin. We expect responses on non-personnel spending to be more pronounced because adjustment costs shall be lower. Figure 3.6 corroborates expectations. Again, we find very flat and statistically insignificant pre-trends for both outcomes. Both panels also display a quite similar effect pattern after treatment. However, effect sizes differ substantially: the implied long-term elasticity for plants' spending on R&D personnel amounts to -1.63, the corresponding elasticity for non-personnel R&D spending to -2.44.

Figure 3.6: The Effect of a Business Tax Increase on Personnel vs. Non-Personnel R&D Spending

(a) Internal Spending on R&D Personnel (b) Internal Non-Personnel R&D Spending



Notes: This graph plots the point estimates, $\widehat{\beta_k}$ ($k \in [-6, -4, \ldots, 8]$), and corresponding 95% confidence intervals of the event study models as defined in Equations (3.1)–(3.4). The dependent variable is a plant's annual internal R&D spending on personnel in Panel A, and a plant's annual internal non-personnel R&D spending in Panel B. Both outcomes are inverse-hyperbolic-sine transformed. For the treatment group, the business tax change occurred on in year $t = 0$ or $t = -1$. The regressions include plant, municipality, state $\times$ year, commuting zone $\times$ year, as well as sector $\times$ year fixed effects. All municipalities that experienced a tax decrease during the event window period are excluded. Standard errors are robust to clustering at the municipality level.

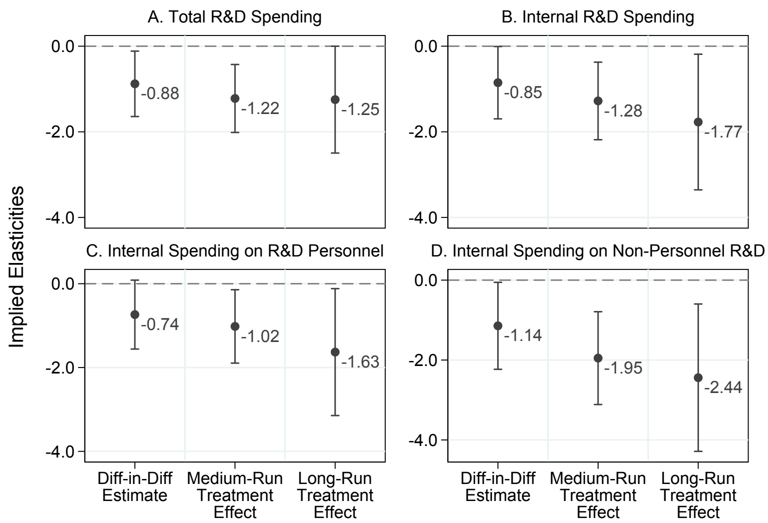
Sensitivity Analysis. We conduct several sensitivity checks to test whether the presented estimates are robust to varying specifications of the event study model. To simplify the comparison, we present robustness checks for the tax increase indicator specification of the event study model, as given in Equation (3.3), only. As discussed in Section 3.4.2, the biggest concern for identification are local shocks that take place along with the tax reforms. Our baseline specification includes state-by-year and CZ-by-year fixed effects (θ_{zt}), which should capture potentially confounding local shocks to a large extent. However, we also re-estimate the event study model with more or less fine-grained region-by-year fixed effects (at the county, the ROR, and NUTS II level) and monitor potential changes in the event study coefficients in order to assess the importance of local shocks as a source of bias. Appendix Figure C.5 shows that effects are statistically significant irrespective of the type of region-by-year fixed effects, but become stronger, i.e., more negative, when controlling for local shocks at a finer geographical level. This suggests that local shocks lead to an upward bias of the estimates, driving estimated coefficients towards zero.

We further explicitly test for the impact of time-varying confounders at the municipality or county level. Sacrificing some econometric rigor, we include time-lagged variables on municipalities' annual level of population, a county's unemployment rate, as well as county-level GDP per capita as controls in some specifications of the event study model. Point estimates remain literately unaffected by the inclusion of these observable confounders (see Appendix Figure C.6). While we take this coefficient stability as suggestive evidence against omitted variable bias, we further follow the approach in Oster (2019) to more explicitly assess potential biases due to unobserved time-varying confounders at the municipal or county level. Appendix Table C.2 presents bias-adjusted long-term elasticities as bounds for our baseline summary estimates. Estimates are very close to the baseline results.

In Appendix Figure C.7, we further demonstrate that a $\log(y+1)$-transformation of the outcome variables yields almost identical results as the inverse hyperbolic sine transformation in our baseline specification. Moroever, estimates remain statistically significant when allowing for correlation in the error term at broader regional levels than the municipality (see Appendix Figure C.8). In Appendix Figure C.9, we further show that the use of different event window specifications does not render the observed effect patterns. Pre-trends remain flat when extending the number of leads. Post treatment, estimated effects on R&D spending level off around six years after treatment. Last, Panel A of Figure C.10 shows that we obtain very similar results when adding those plants that change their location of residence during the observation period to the estimation sample.[24]

Alternative Summary Measures. We next compare the implied long-run elasticity to alternative summary measures of the treatment effect. First, we estimate a simple difference-in-differences (Diff-in-Diff) model that regresses a given spending outcome on the log business tax rate and the full set of plant, municipality, sector × year, and region × year fixed effects. Second, we take the mean over the four post-treatment event study coefficients $\hat{\beta}_t$ $\forall t = 0,..,6$ to derive a medium-run elasticity with regard to treatment. For each outcome, we observe a very similar pattern. Simple Diff-in-Diff elasticities

Figure 3.7: Alternative Implied Elasticities – R&D Spending



Notes: This graph displays implied elasticities for four different R&D spending variables with respect to a change in the local business tax rate. For each panel (outcome), we display the corresponding elasticity when (i) estimating a simple Diff-in-Diff model with the log LBT rate as the explanatory variable, (ii) taking the mean over the four post-treatment coefficients $\hat{\beta}_t$ $\forall t = 0,..,6$ of the event study model defined in Equation 3.4 to derive a medium-run elasticity with regard to treatment, or (iii) taking the last treatment effect (β_8) from the same event study model. The first two elasticities indicate average treatment effects, the third one captures the long-term effect of the tax increase.

are notably smaller (in absolute terms) compared to elasticities derived from the post-treatment event study coefficients. This finding suggests that a Diff-in-Diff strategy fails to recover a reasonably weighted average of the treatment effect in the presence of unit and time fixed effects and staggered treatment

[24]We assigning those plants the prevailing tax rates from their initially observed location of residence in the spirit of an intention-to-treat effect.

(Borusyak and Javarel, 2018). Moreover, and in line with the respective effect patterns displayed above, we find the long-term elasticities to be larger than the medium-run elasticities in absolute terms.

Investment and the Cost of Capital. We further transform our long-run summary measure into an elasticity with respect to the cost of capital. This key parameter assesses the spending response in percent relative to a one-percent increase in the user costs of capital. Following Yagan (2015), we calculate this elasticity as $\varepsilon^{Inv}_{CoC} = \varepsilon^{Inv}_{\tau}/\varepsilon^{CoC}_{\tau}$. The numerator ε^{Inv}_{τ} refers to the spending elasticity with respect to the business tax rate, which is given by the long-run elasticity of -1.25 presented in Figure 3.4. The denominator ε^{CoC}_{τ} is defined as the elasticity of the cost of capital with respect to the tax rate. In the German setting, the user cost of capital are given by $CoC = r/(1-\tau)$ with the pre-tax rate of return $r = 0.07$ and the total profit tax rate $\tau = 0.32$ (including local business and federal corporate taxes). It follows that $\varepsilon^{CoC}_{\tau} = 0.47$. Thus, the implied elasticity with respect to the user costs of capital thus amounts to -2.66. This estimate is very close to the elasticity in Moon (2020), who analyzes a capital gains tax reform in South Korea. In contrast, Zwick and Mahon (2017) and Chen, Jiang, et al. (2019) find somewhat lower elasticities of -1.6 for the United States (ranging between -0.8 and -3.3 depending on plant size) and -1.27 for China (when controlling for the mis-classification of R&D expenses).[25]

Heterogeneous Effects. In a final step, we use the indicator variable specification of our event study design (cf. Equation (3.3)) to test for heterogeneous treatment effects by plant-level characteristics. First, we test for heterogeneity between single- and multi-plant firms by estimating treatment effects separately for both groups of plants. If the tax-induced reductions in R&D spending as implied by Figure 3.4 are only due to changes in the spending behavior of multi-plant firms, estimated effects might be due to the spatial reallocation of R&D activity across plants within a given firm and not necessarily point to an absolute reduction in innovative activity. However, Appendix Figure C.11 shows that both types of plants respond similarly to an increase of the LBT.[26]

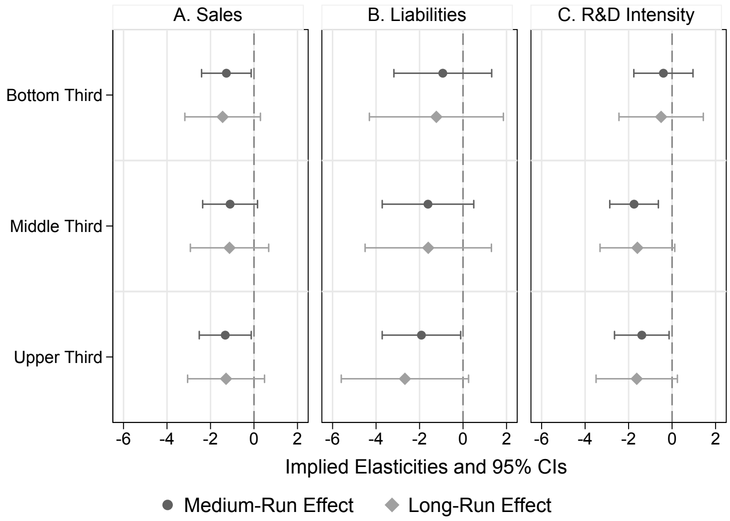
Second, we test for heterogeneous treatment effects by plant size, liquidity and R&D intensity. To this end, we create for each dimension of heterogeneity three equally-sized bins and interact these with all leads and lags of the treatment variable. In addition, we include bin $\times$ year fixed effects to allow for flexible differential trends across groups. Panel A of Figure 3.8 provides the corresponding medium- and long-run elasticities by plant size (as measured via sales).[27] Ex ante, we expect smaller plants to react more to changes in the local business tax rate than larger ones. Small plants are usually assumed to have less access to external funding, and may hence benefit less from the possible deduction of the costs of debt financing from the tax base. Indeed, a number of OECD countries – such as the UK, France, Australia, and Canada – offer targeted R&D tax incentives to small- and medium-sized enterprises (SME), assuming SME's to react (benefit) more to (from) R&D tax incentives than larger plants (Gonzales-Cabral, Appelt, and Galindo-Rueda, 2018). However, we find the effect of an increase in the LBT on R&D spending to be rather independent of plant size.

[25] However, note that the three studies look at general investments rather than R&D spending. In line with prior evidence, we may thus expect stronger responses for R&D given that these expenses are disproportionately equity-financed.

[26] While point estimates are very similar for both groups, standard errors are considerably larger for multi-plant firms - partly reflecting the fact that only one-third of the covered plants belong to a firm.

[27] We plot the corresponding event study graphs in Appendix Figure C.12.

Figure 3.8: The Effect of a Business Tax Increase on Total R&D Spending – Heterogeneous Effects



Notes: This graph plots the implied medium-run R&D spending elasticities and corresponding 95% confidence intervals as derived from the event study model defined in Equations (3.1)–(3.3) when allowing for heterogeneous treatment effects by plant characteristics. In Panel A, we distinguish plants by size (in terms of sales). In Panel B, we assess heterogeneity by the level of non-current liabilities (as a proxy for liquidity). In Panel C, we allow elasticities to differ by R&D intensity. The regressions include municipality, plant, state × year, commuting zone × year, as well as sector × year fixed effects. We exclude all municipalities that experience a tax decrease during the observation period. Standard errors are robust to clustering at the municipality level.

To further explore the assumed relationship between taxation, R&D spending, and plants' degree of debt financing, we next assess treatment effects by plants' level of non-current liabilities relative to their sales. We consider this variable as a suitable proxy of plants' costs of external financing, given that a higher relative level of liabilities should lead to higher interest rates and, correspondingly, a larger share of equity financing. As the survey data at hand do not provide any insight on plants' finance structure, we add information from the Bureau van Dijk's *Amadeus* dataset to the survey (see Section 3.3 for details). Unfortunately, we can only do so for around 40% of the plants covered in the R&D survey and, therefore, need to restrict this analysis to a rather small subset of plants.[28] Nevertheless, we detect an indicative effect pattern (see Panel B): in line with expectations, the effect of a local business tax increase on plants' total R&D spending becomes larger (in absolute terms) with the relative level of non-current liabilities. While large standard errors warrant extra caution when interpreting this finding, we take the effect pattern as suggestive evidence in line with theoretical priors. Moreover, results are in line with Zwick and Mahon (2017) and Moon (2020), who both detect stronger overall investment responses to tax incentives for financially-constrained firms.

Last, we test whether the effect of a local business tax increase on R&D spending differs among plants with varying R&D intensity, defined as the share of R&D staff over plants' total workforce. As R&D is disproportionally financed via equity (Brown, Fazzari, and Petersen, 2009; Thakor and Lo, 2017), more R&D intensive plants should react stronger to a given tax increase. Panel C of Figure 3.8 provides suggestive evidence along this line of argument. Estimated elasticities are larger (in absolute terms) for more R&D-intensive plants.

[28] In Panel B of Appendix Figure C.10, we show that overall effects for this smaller sample are close to our baseline results.

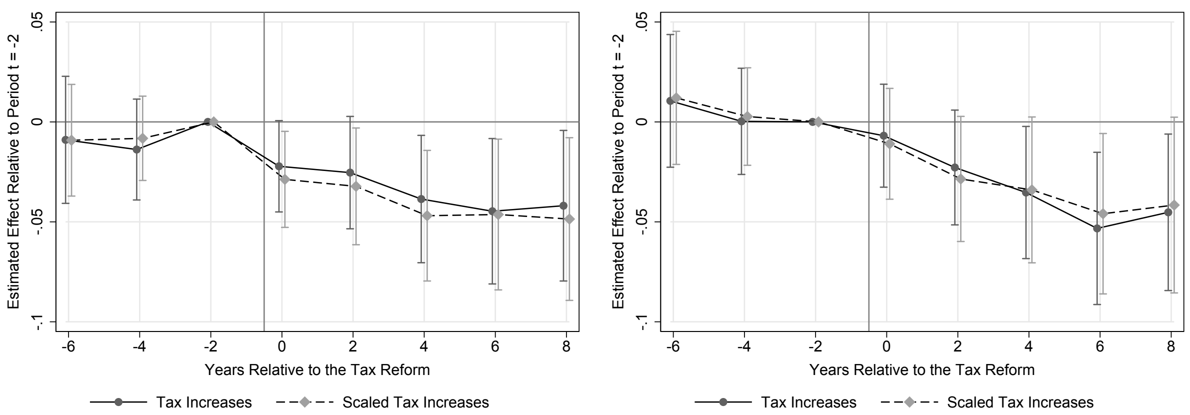
3.5.2 Effects on Innovation

We next investigate whether the tax-induced reduction in R&D spending results in less innovation. To this end, we estimate the effect of a local business tax increase on the number of filed patents using the two specifications of our event study model as set-up in Equations (3.1)–(3.4).

Main Effects. We first investigate the effect of a tax increase on plants' (inverse hyperbolic sine transformed) annual number of filed patents. Panel A of Figure 3.9 displays the corresponding event study estimates. Pre-trends are flat and statistically insignificant. Post treatment, we detect a negative and statistically significant treatment effect that materializes around four years after the tax increase. Effects on innovation thus appear to be lagged by around two years relative to plants' R&D spending response (cf. Figure 3.4). The implied long-run elasticity is -0.87, which is close to the elasticities reported in Akcigit, Grigsby, et al. (2018). We next account for the fact that patent quality has been shown to vary

Figure 3.9: The Effect of a Business Tax Increase on Patent Applications

(a) Number of Filed Patents (b) Number of Citation-Weighted Patents



Notes: This graph plots the point estimates, $\widehat{\beta_k}$ ($k \in [-6, -4, \ldots, 8]$), and corresponding 95% confidence intervals of the event study models as defined in Equations (3.1)–(3.4). In Panel A, the dependent variable is the plant-level annual number of filed patents. In Panel B, the dependent variable is the number of citation-weighted patents. Both outcomes are inverse hyperbolic sine transformed. For the treatment group, the business tax change occurred on in year $t = 0$ or $t = -1$. The regressions include plant, municipality, state $\times$ year, commuting zone $\times$ year, as well as sector $\times$ year fixed effects. All municipalities that experienced a tax decrease during the event window period are excluded. Standard errors are robust to clustering at the municipality level.

substantially (Scherer, 1965; Hall, Jaffe, and Trajtenberg, 2005), such that the simple count of plants' number of filed patents may not measure the true value of innovation output in a correct way. Moreover, if plants only abandon marginal R&D projects in response to a tax increase, we might see a reduction in the plants' quantity of patents but no effect on innovation quality. To this end, we additionally estimate effects on the citation-weighted number of patents, scaling patents according to the number of citations they receive by EPO patents within five years of a patent's own first registration. We detect very similar effects on the citation-adjusted number of filed patents – the long-run elasticity amounting to -0.94.[29]

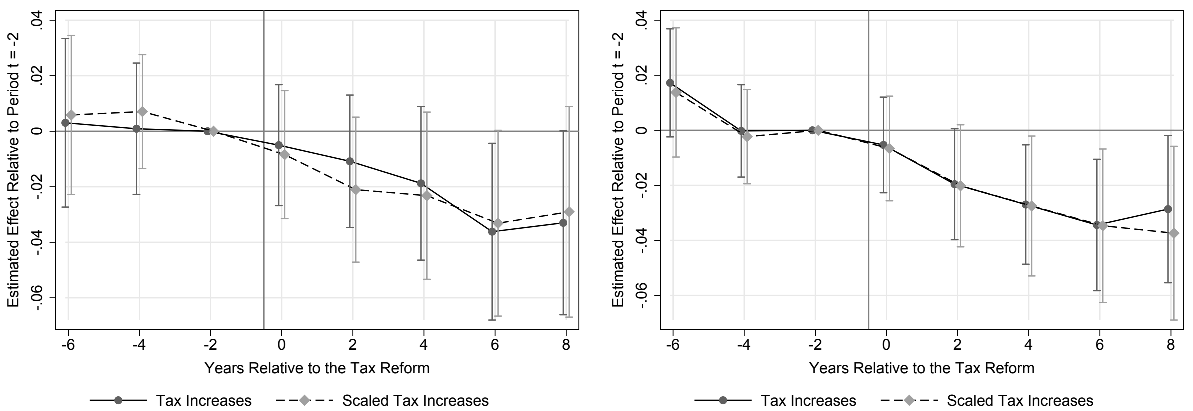
Last, we further investigate whether plants abandon particular types of R&D projects in response to a tax increase. To this end, we use information from each patent's claims text that allows for the distinction

[29]When investigating the impact of tax decreases on the citation-weighted number of applications, treatment effects are small and statistically insignificant (see Appendix Figure C.13).

of product from process innovations. While R&D spending for the development of new products is associated with substantial risks that may in turn open up or revolutionize a market and may thus also yield substantial social returns, process innovations are generally considered as the more incremental ones with limited social returns (Klepper, 1996). However, as displayed in Figure 3.10, we find no heterogeneous responses along this margin. Effects on process innovations materialize earlier, but overall responses are very similar in quantitative terms.

Figure 3.10: The Effect of a Tax Rate Increase on Product and Process Innovations

(a) Citation-Weighted Number of Product Patents (b) Citation-Weighted Number of Process Patents



Notes: This graph plots the point estimates, $\widehat{\beta_k}$ ($k \in [-6, -4, \ldots, 8]$), and corresponding 95% confidence intervals of the event study models as defined in Equations (3.1)–(3.4). In Panel A, the dependent variable refers to a plant's annual number of patents referring to product innovations. In Panel B, the dependent variable is the number of citation-weighted patents referring to process innovations. Both outcomes are inverse hyperbolic sine transformed. For the treatment group, the business tax change occurred on in year $t = 0$ or $t = -1$. The regressions include plant, municipality, state × year, commuting zone × year, as well as sector × year fixed effects. All municipalities that experienced a tax decrease during the event window period are excluded. Standard errors are robust to clustering at the municipality level.

Sensitivity Analysis. We conduct several robustness checks to test whether estimated effects on patents are robust to alternative specifications of our event study models. First, we show that effects on the citation-weighted number of patents are very similar when using citations from patents registered at the USPTO rather than the EPO (see Appendix Figure C.14). Second, we show that estimates become larger (in absolute terms) when controlling for local shocks at finer geographical levels (see Appendix Figure C.15). When using broader regional controls, such as NUTS II × year fixed effects, some post-treatment estimates become statistically indistinguishable from zero. In contrast, fine-grained geographical controls at the county level hardly affect our estimates. The same is true when including time-varying controls at the municipality or county level or calculating biased-adjusted long-term elasticities in the spirit of Oster (2019); see Appendix Figures C.3 and C.16.

We further explicitly test the sensitivity of our baseline results with regard to different transformations of the outcome variable. We consider this test to be particularly important, given that the plant-level distribution of patents is substantially skewed to the right and many plants do not file a patent in every year (or even at all). Indeed, there is a recent debate about the "correct" functional form of the outcome variable when dealing with patent counts that include many zeros (Campbell and Mau, 2020; Bloom, Draca, and Van Reenen, 2020). In Appendix Figure C.17, we first show that effects remain literally unchanged when using a $\ln(y+1)$ instead of our baseline inverse hyperbolic sine transformation. Moreover, treatment effects are also similar when using the simple level of each outcome variable.

In Appendix Figure C.18, we further show that estimates remain statistically significant when allowing for clustering at broader regional boundaries than in the baseline specification. Last, we find that the use of different event window specifications does not affect the observed effect patterns (see Appendix Figure C.19). Prior to treatment, coefficients are close to zero and statistically insignificant. Post treatment, effects materialize around four years and level off around six years after treatment.

Implied Elasticities. In Appendix Figure C.20, we compare our long-run baseline elasticity to alternative summary measures: (i) using the simple Diff-in-Diff approach with the log business tax rate as explanatory variable, and (ii) averaging over the first four post-treatment estimates, $\widehat{\beta_k} \forall k = 0, .., 6$ to derive a medium-run elasticity of patenting with regard to an increase in the LBT rate. Again, we detect a common pattern. First, and in line with our findings in Section 3.5.1, Diff-in-Diff elasticities are notably smaller (in absolute terms) than estimates from the event study models. Second, and in line with the dynamic effect patterns shown in Figures 3.9, the long-term elasticity is considerably larger than the medium-run elasticity in absolute terms.

3.5.3 Business Taxes and Economic Growth

In the final part of our empirical analysis, we widen the scope of our investigation beyond the plant level and investigate the consequences of tax-induced reductions in R&D spending and innovation for local economic growth. Among others, Lentz and Mortensen (2008) and Kogan et al. (2017) highlight the role of firm-level innovation for economic growth by raising average firm productivity. We explore this suggested mechanism in more detail, proceeding in three steps: First, we assess the impact of innovation as measured by the citation-weighted number of patents on economic growth at the municipality level. In a second step, we estimate the total effect of an increase in the LBT rate on local GDP, acknowledging that business taxes may affect economic performance through other margins than innovation – e.g., via lower wages (Fuest, Peichl, and Siegloch, 2018) or employment (Dustmann, de Stefano, and Schönberg, 2020). Third, we combine the different empirical estimates from our study to simulate the tax-induced decline in local economic growth that is due to reduced innovation of local plants.

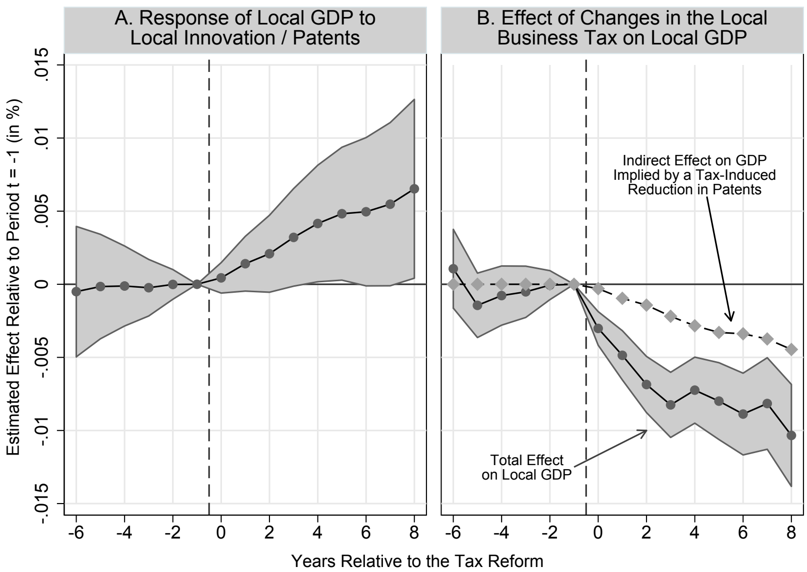
Local Innovation and Growth. To assess the importance of local innovation for economic growth, we adapt the empirical set-up of Kogan et al. (2017) and estimate a distributed lag model that relates changes in the municipality-level citation-weighted number of patents to local GDP. Specifically, we estimate the following equation:

$$\ln GDP_{mt} = \sum_{k=-6}^{8} \gamma_k Patents_{m,t-k} + \mu_m + \zeta_{rt} + \varepsilon_{mt}, \tag{3.5}$$

where GDP_{mt} refers to municipality m's GDP in year t.[30] The explanatory variable $Patents_{m,t-k}$ denotes the (inverse hyperbolic sine transformed) citation-weighted number of patents filed in municipality m at

[30]As official statistics provide measures of GDP at the county level only, we multiply a county's GDP per capita with a municipality's population.

Figure 3.11: Business Taxes, Local Innovation, and Economic Growth



Notes: This graph illustrates the relation between business taxes, local innovation, and economic growth. In Panel A, we plot the dynamic effect of changes in municipalities' number of (citation-weighted) patents on local GDP based on Equation (3.5). In Panel B, we plot the total effect of a change in the local business tax rate on local GDP (in black), see Equation (3.6). The dashed gray line in Panel B indicates how much of the total effect of a tax increase on local GDP is due to tax-induced reductions in local innovation. This mediation analysis is based on the estimated effects from Panel A and the event study coefficients from Panel B of Figure 3.9. Depicted coefficients are based on the cumulated estimates, $\widehat{\gamma_k}$ and $\widehat{\delta_k}$, respectively (always normalized relative to $k = -1$). Gray shaded areas indicate 95% confidence intervals, standard errors are robust to clustering at the municipality level. See Appendix C.1 for details on all variables.

time $t - k$. As both the outcome and the regressor of interest are observed on an annual basis, we specify the event time in years (rather than two-year blocks), and restrict the event window from six years before to eight years after a tax reform. Municipality and state × year fixed effects, μ_m and ζ_{rt}, account for time-(in)variant confounders. Standard errors are robust to clustering at the municipality level.

Panel A of Figure 3.11 plots the estimated dynamic effects of a change in the local (citation-weighted) number of patents on local (log) GDP. We detect a clear dynamic pattern suggesting a lagged positive GDP response to local innovation shocks, with a flat pre-trend in economic growth in the years before. When interpreting the notable structural break at the point of innovation in the spirit of a Granger causality test, these estimates suggest that innovation precedes higher levels of local economic growth, while economic growth does not trigger local innovation. This pattern at the municipality level is very much in line with evidence by Kogan et al. (2017), who find similar time-series evidence at the national level for the United States.

Local Business Taxes and Growth. In a second step, we estimate the effect of changes in the local business tax rate on local GDP. Several contributions have analyzed the impact of corporate taxation on growth before, such as Gemmell, Kneller, and Sanz (2011) for OECD countries and Ferede and Dahlby (2012) for Canadian regions. We bring this idea to the municipality level and relate leads and lags of the local business tax rate τ_{mt} to local GDP:

$$\ln GDP_{mt} = \sum_{k=-6}^{8} \delta_k \tau_{m,t-k} + \mu_m + \zeta_{rt} + \varepsilon_{mt}, \tag{3.6}$$

where we again account for municipality and state × year fixed effects.

The corresponding estimated effects are summarized in Panel B of Figure 3.11. In line with previous evidence, we find a pronounced negative effect of business taxation on economic growth that builds up over time and levels off around three years after the tax change. Quantitatively, our results imply that an increase of the local business tax rate by one percentage point decreases local GDP by around 1% eight years after the tax reform. Small and insignificant point estimates for the pre-treatment period support a causal interpretation of this effect.

Last, we trace out how much of the overall negative effect of local business taxation on local GDP can be linked to tax-induced reductions in plant-level innovation. To this end, we combine the estimated effect of local innovation on municipal GDP as shown in Panel A of Figure 3.11 with the treatment effects of an increase in the local business tax rate on plants' citation-weighted number of patents as given in Panel B of Figure 3.9. Our simulation, displayed by the dashed gray line in Panel B of Figure 3.11, suggests that the implied effect of a tax-induced reduction in plants' patenting activity on local GDP is sizable, amounting to around 40 percent of the total negative effect of local business taxation on economic growth.

3.6 Conclusion

In this paper, we exploit the unique German institutional setting, where municipalities can independently set profit tax rates, to assess whether and how tax policy can foster firms' innovation activities. Using official survey data on the universe of R&D-active German plants, we exploit 7,300 municipal business tax reforms to show a negative, statistically significant effect of a profit tax increase on plants' total R&D expenditures. The corresponding long-term elasticity amounts to around −1.25. The negative effect on R&D spending is entirely driven by cuts in internal R&D spending. Furthermore, we demonstrate that decreases in R&D spending are accompanied by tax-induced reductions in the (citation-weighted) number of filed patents. The patent effects materialize with some temporal lag of about four years. We estimate a patent elasticity with respect to the tax of around −0.9. Last, our estimates imply that around 40% of the total negative effect of business taxation on local growth are due to reduced innovation.

4

Competition and Innovation: The Breakup of IG Farben

Abstract *The effect of competition on innovation is hard to study causally. To this end, this study exploits the 1952 breakup of Germany's leading chemical company, IG Farben. The Western Allies occupying Germany restructured one of the worlds' largest chemical companies along their three occupation zones. The breakup was imposed as a consequence of IG Farben's importance for the German war economy and led to wide variation of competition increases across innovation space. In technology areas with large competition increases, patenting strongly increases as well. Effects are driven by domestic, non-IG firms. Fine-grained product-level information on suppliers and prices allows auxiliary analysis. The breakup induced long-run product-level competition between the IG Farben successors. In affected product areas, additional suppliers entered and prices declined. The results suggest large positive breakup effects without short-run trade-offs.*

4.1 Introduction

Most economists now agree that market concentration has increased in relevant sectors of the economy (Affeldt et al., 2021). Disagreement remains whether efficiency and superior technology of superstar firms (e.g. Crouzet and Eberly, 2019; Autor, Dorn, Katz, et al., 2020) or market power and lax antitrust enforcement (e.g. Gutiérrez and Philippon, 2018; Grullon, Larkin, and Michaely, 2019; De Loecker, Eeckhout, and Unger, 2020) explain this tendency. Fearing negative effects on innovation, some political leaders have even advocated to reverse mergers and to break up dominant firms (Warren, 2019). Theoretical arguments indeed highlight how mergers and concentration can negatively affect innovation and market dynamics (Federico, Morton, and Shapiro, 2019). Empirically, however, the effect of competition on economic outcomes is hard to determine, as exogenous variation in market structure is rare. Merger analysis, central to antitrust enforcement, is further complicated by endogenous selection into mergers and enforcement conditional on expected merger outcomes.

This paper exploits the breakup of the largest German chemical company in 1952 by the Allied Powers outside of standard antitrust practice. The breakup target, IG Farben, was one of the most innovative German companies.[1] Three of its scientists won Nobel prizes, one of them for the world's first antibiotic. It had an outsized role in the German innovation system, responsible for 5.9% of all patents by German inventors, 16.8% in chemistry. But IG Farben also directly or indirectly produced most of Germany's explosives, synthetic fuel and rubber. The victorious Allies saw this economic influence combined with IG Farben's crucial relevance for the German war machine as undue political potential. IG Farben's crimes, such as its major involvement at the Auschwitz concentration camp, fueled this negative perception. However, political differences between the occupying powers delayed action and the looming cold war altered views on IG Farben. The Allies, now supporting the IG Farben constituents in their respective occupation zones, decided on a breakup largely following this structure. Three large successors, BASF, Bayer and Hoechst, as well as a dozen smaller businesses, were created.

From its creation via merger to breakup, the story of IG Farben closely relates to considerations relevant to today's merger and potential breakup decisions in innovative industries. In merger analysis, antitrust authorities consider the trade-off between potential efficiencies with disincentives arising from reduced competition. In the IG Farben case, historical sources cite both organizational synergies and scale as reasons for IG Farben's 1925 creation via merger.[2] A priori, the welfare effects of the breakup are unclear. Next to the overall conflict between competition and merger efficiencies, the breakup might entail trade-offs between the short-term and long-term consequences or between different economic outcomes. For example, the increased competition between the IG Farben successors might remove other companies from the affected areas and so reduce the breakup impact. On the other hand, the breakup might also remove entry barriers and lead to new entry. The breakup might also stimulate innovation at the expense

[1]Stokes (1988) and Stokes (1994) are the key historical references for the breakup and the story of its successors, Hayes (1987) and Plumpe (1990) for the history of IG Farben until the breakup.

[2]Despite being engaged in cartels and profit sharing agreements before the merger, significant synergies such as a singular sales organization could not be realized due to holdup problems (ter Meer, 1953, p. 23). After the merger, IG Farben doubled its stock capital to finance former BASF's innovative research and production projects in high-pressure chemistry (Abelshauser, 2003, p. 228). Additionally, benefits to appropriation can be cited as an advantage of a joint IG firm. Similarity of technology and production profile between the IG Farben constituents implies that R&D projects can gain more widespread application (Plumpe, 1990, p. 137) and internalizing of intrafirm spillovers - spillovers between merger participants - might yield increased incentives for innovation (Gilbert, 2020, p. 90).

of prices. Reactions of other market participants and new entrants might exacerbate the breakup effects, so that aggregate effects are relevant (Gilbert, 2020).[3]

Consequently, the primary outcome of interest is the overall innovation activity in Germany. This consists of innovation by IG Farben, but also of innovation by other firms. For the analysis of trade-offs, decompositions of innovation, for example according to innovation quality, are relevant. In addition, economic outcomes such as long-run market structure - entry or exit of market participants - as well as prices capture additional dimensions of breakup effects.

The breakup of IG Farben differs in important aspects from standard (de)merger cases and offers advantages for empirical analysis. Business considerations of the afflicted company were not the primary motivations of the breakup, as in merger cases and corporate demergers. Standard economic antitrust considerations, where markets or technologies with potential harm motivate merger litigation, were not the primary causes of the breakup. Both would lead to selection in observable mergers and merger litigation. Rather, the breakup was rooted in contemporary political economy considerations and executed by an external force, with idiosyncratic geographical factors playing a large role. Consequently, the effects of this breakup are closer to causal than previous analyses.

Two data types enable the analysis. Data on German patents allow high-level views on innovation outputs in technologies and by firms. Fine-grained data on suppliers and prices of chemical substances allow an analysis of product markets. Starting from scanned grant documents, patent data is collected using machine learning and image processing. Technology classes relevant to the chemical industry are selected based on contemporary classifications. As standard measures for heterogeneity between patents such as forward citations (Harhoff, Narin, et al., 1999) are unavailable, quality measures based on full text analysis are introduced. Analogous to citations, they measure the importance of a patent for subsequent patents, relative to previous patents (cf. Kelly et al., 2018). Digitized supplier catalogs provide product data. The catalogs list thousands of common chemical products and the companies that currently sell them on the market. The analysis restricts to chemical substances. In economic terms, these are homogeneous intermediate products. The digitized catalogs cover the pre-war (1939), post-breakup (1952) and long-run situation (1961). The product-company pairs listed by the catalogs inform about the market competition for a given product and the portfolio for individual firms - especially for IG Farben. Product-level price data from industry journals complement the product data.

IG Farben's enormous size and diversified product portfolio led to impacts across large parts of the civilian chemical industry. IG Farben in 1939 was involved in the supply of 40.5 % of chemical substances in the analysis sample. Each such product was potentially affected, but the realized impact depends on the structure of the breakup. In fact, the breakup led to horizontal, product-level competition. After the breakup, 40% of IG Farben-supplied products are offered by two or more successors. The likelihood of IG Farben successors to compete in the same markets decreased only slightly over the next ten years.

[3]Bloom, Schankerman, and Van Reenen (2013b)'s framework is a possible theoretical foundation. There, competitor responses are driven by technology and product market spillovers of an initial shock. In the short-run, the IG Farben breakup introduces rivalry between the IG Farben successors, but leaves the structure of technology and product market spillovers between IG Farben and other firms intact. The breakup might induce the IG Farben successors as neck-and-neck rivals to increase R&D and knowledge production to compete for shared markets (Aghion, Bechtold, et al., 2018). According to Bloom, Schankerman, and Van Reenen's results, the reaction of knowledge production by competitors depends on whether positive technology spillovers dominate negative product market spillovers. This is in addition to the direct effect of increased within-IG Farben competition on competitors, which depends on the relative technological position of the firms (Aghion, Bechtold, et al., 2018).

As the main result, innovation in areas impacted by the IG Farben shock increases strongly and persistently compared to other areas of chemistry. This conclusion results from a comparison of chemical patents exposed to or unaffected by the IG Farben shock in a difference-in-differences (DiD) analysis. Breakup exposure is the concentration change implied by considering IG Farben as one or as separate successors. IG Farben research facilities were geographically spread out. As the breakup was largely geographical in nature as well, the post-breakup structure can be backdated to the pre-breakup, pre-war time. This avoids contamination by wartime events and post-breakup adjustments. With a measure of the concentration change, the technology classes can be separated into an exposed and an unaffected group. Before the war, IG Farben applied for 12.3% of all chemical patents, 38.8% in technology classes strongly affected by the breakup. The development of patenting amount in exposed and unaffected technologies is parallel before 1952. After the breakup, the two increasingly diverge. Results are similar when counting only patents without IG Farben association. Results are also similar when modifying exposure measures to isolate the concentration change caused by the geographical breakup structure. Heterogeneity analysis provides insights into how the breakup changed patenting. A short-run quality-quantity trade-off is introduced. Average quality decreases immediately after the breakup, but slowly normalizes. Patenting in Germany by foreign applicants increases after the war, but the increase seems unrelated to the breakup. IG Farben itself is difficult to analyze causally as the number of successors is small and appropriate control firms are missing. Descriptive analysis suggests strongly increasing patent output by IG Farben successor firms at high but constant R&D intensity.

The antecedent of innovation effects are changes in product space. The chemical industry offers an advantageous setting as it contains a large number of markets with comparable structure. Chemical substances have well-defined properties, so that companies compete in a large number of markets for homogeneous, intermediate products. Markets can be cleanly defined. The effects of the horizontal, product-level competition resulting from the IG Farben breakup remain to be investigated.

Increased competition could crowd out other competitors, creating a trade-off to the innovation effect. On the other hand, incumbents in intermediate good markets can contract to restrict entry (Aghion and Bolton, 1987). Multiple incumbents could struggle to coordinate accordingly, opening the market for entry. Similarly, the breakup could increase knowledge spillovers or ease technology licensing and so facilitate entry. DiD analysis in a panel of chemical substances investigates market structure effects regarding the overall number of suppliers per product. Changes between 1939 and 1952 cannot be attributed to the breakup, as it is hard to isolate the effect of other events and a pre-breakup measurement is unavailable. Reassuringly, after controlling for major shocks, the number of suppliers stays similar in most specifications. Between 1952 and 1961, however, the number of suppliers increases where the breakup led to competition. The increase is driven by non-IG firms, which is suggestive evidence that an initial increase in competition can induce further entry.

Price effects of the breakup could counteract or exacerbate the innovation effect. In fact, in areas where the IG Farben breakup created effective competition, prices drop by 5.0% compared to products without IG Farben involvement. Products where only one successor is active instead show price increases compared to products where the IG was not active. These results are in line with economic intuitions of market competition and price setting (e.g. Ashenfelter, Hosken, and Weinberg, 2013), but could understate benefits to consumers if price decreases spill over to downstream markets (Basso and Ross, 2019). On average, products with any IG Farben involvement show similar price developments as products not offered by IG Farben. Similarly, when considering 1939 IG Farben, effect estimates are small.

The results need to be interpreted considering the limitations arising from the historical context. Historical factors influence the results insofar as they differentially affect breakup-exposed sectors within chemistry. Confounding factors would need to correlate closely to the geographic structure of IG Farben across occupation zones, which drives the variation in technology and product-level competition. For some historical factors related to war destruction, Allied occupation policies or German post-war policies this can be assessed quantitatively. When included in statistical analysis as control variables, these factors do not materially affect the conclusions. Other historical factors are difficult to quantify, but their potential influence can be judged based on historical research. Further, alternative analysis of innovation effects based on a firm panel instead of a technology class panel yields similar results.

This study contributes to the empirical literature on competition and innovation, specifically the literature on mergers. A set of studies has relied on merger cases and matching methods combined with DiD analysis to estimate effects. The evidence is mixed, with either no effects (Danzon, Epstein, and Nicholson, 2007) or negative effects (Ornaghi, 2009; Szücs, 2014; Haucap, Rasch, and Stiebale, 2019) of mergers. However, merger cases and litigation by antitrust authorities are selective (Carlton, 2009). In this study, effects are instead estimated within one event, which differentially affected a broad range of technologies or products.

Much of the literature on mergers and innovation has focused on direct effects on the merging parties (Haucap, Rasch, and Stiebale, 2019). This study analyzes aggregate breakup effects, combining reactions by the directly affected IG Farben successors with reactions from competitors. In addition, this study tries to identify potential trade-offs created by the breakup. Aggregate effects are relevant from a welfare perspective, as it is unclear whether the responses of successors and competitors are in parallel. For prices in markets for homogeneous products this is likely the case. In contrast, innovation decisions of competitors might also be strategic substitutes or complements (Bulow, Geanakoplos, and Klemperer, 1985; Bloom, Schankerman, and Van Reenen, 2013b; Gilbert, 2020, p. 89). Then, competitor responses may exacerbate or offset the change of innovation output by the IG Farben successors. This study finds that patenting in areas affected by the breakup increases relative to unaffected ones, both overall and for firms unrelated to IG Farben. Descriptively, the IG Farben successors also increased their patent output. Effects on other outcomes could also counteract the positive innovation effect of the breakup. Increased competition could change the propensity to patent, inducing a quantity-quality trade-off. This seems to be the case, at least temporarily. Similarly, policymakers might worry about increased foreign entry following the weakening of domestic incumbents. While foreign patenting in Germany increased following the Second World War, this tendency does not drive the breakup effect. In auxiliary analysis, this study also tests for effects in product space and contributes to the literature on entry and price effects after mergers. The breakup led to the entry of new firms into existing product markets and to moderate price declines within them. The breakup did not induce trade-offs in these additional welfare-relevant dimensions.

This study relates to the literature on the history of antitrust, in particular towards breakups of large corporations. Such government action is rare, and the literature has focused on seminal US cases such as Standard Oil or the Bell system (Lamoreaux, 2019). However, cases are few and far between. The IG

Farben case adds by broadening the view to a new industry, where innovation can be quantified well and broadly.

This study also contributes in making novel data available, either newly or much improved. For one, product catalogs offer fine-grained product information that approaches market definitions more closely than the typically used firm- or industry-level data (Affeldt et al., 2021). Comparable detailed product and price data was previously unavailable for this time period and this industry. German patent data is processed in greater details and over a longer time-span than before. Intensive use of machine learning and image processing make it possible to recover applicant, inventor and technology class information so far unavailable at a comparable scale.

Section 4.2 discusses the empirical literature on competition and innovation. Section 4.3 provides an overview of the breakup of IG Farben. Section 4.4 introduces data sources. Section 4.5 provides measurements of the breakup and descriptive analysis of IG Farben and successors. Section 4.6 discusses the empirical strategy and section 4.7 presents the results, effects in innovation and effects in product space. Section 4.8 discusses limitations arising from contemporaneous events, and section 4.9 concludes.

4.2 Literature: Competition and Innovation

Research on competition and innovation has a long tradition (Reviews include Gilbert, 2006; Cohen, 2010; Gilbert, 2020). Conceptual arguments go back far, at least to Schumpeter (1942) and Arrow (1962). According to Shapiro (2011), the core of their views can be summarized in a framework of ex ante and ex post market structure, relative to an innovation. Greater ex ante competition implies low ex ante profits, which encourages innovation (Arrow). Greater ex post competition reduces the profits reaped from the innovation and discourages innovation (Schumpeter). The literature on mergers differs from the broader competition-innovation literature because mergers, instead of removing one rival from the market, leave R&D and production assets intact and unify control rights for a subset of market participants (Federico, Morton, and Shapiro, 2019). The theoretical effect of mergers on innovation depends on particular modeling assumptions and the balance of merger synergies (Jullien and Lefouili, 2018; Federico, Morton, and Shapiro, 2019). Empirical approaches are discussed below. The IG Farben breakup relates to the literature on mergers in that research assets remain intact, but control changes. Before the breakup, IG held large shares of many markets throughout the chemical industry, so that ex ante competition for a new innovation was low. The breakup increased ex ante competition in some markets so that innovation incentives should increase. Possible counteracting mechanisms include decreased economies of scale or scope, increased difficulties for financing and decreased intrafirm spillovers.

The dynamic nature of the relationship between industry structure (competition) and industry outcomes makes empirical work, especially reduced-form, difficult. Structure likely influences performance and visa versa. With this in mind, the empirical literature has often focused on discrete events that change the competition intensity in an industry. Often these are merger events, but studies have also analyzed cartel breakups or compulsory licensing (Gilbert, 2020). The case of IG Farben relates closely to the study of mergers. One integrated entity is separated into several independent companies, without changing available R&D or production assets. Therefore, in principle, all theoretical and empirical arguments delivered in the question of mergers and innovation apply, but in reverse. In typical merger cases, events are contingent on actions by market participants. This is most clear for mergers, but similarly, cartel breakups are often facilitated by cartel members reporting to the authorities. The actions by antitrust authorities with specific mandates concerning the economic outcome variables also play a role as they decide which mergers proceed (Carlton, 2009).

To overcome empirical challenges, studies have taken approaches of matching combined with DiD or instrumental variables (Danzon, Epstein, and Nicholson, 2007; Ornaghi, 2009; Szücs, 2014; Haucap, Rasch, and Stiebale, 2019). Danzon, Epstein, and Nicholson (2007) study pharmaceutical mergers and conditional on controls do not find differences between merged and matched non-merged entities. In contrast, Ornaghi (2009) find negative innovation effects of pharmaceutical mergers. In the same industry, Haucap, Rasch, and Stiebale (2019) show that after mergers in the pharmaceutical industry, patenting and R&D activity decline. Szücs (2014) analyzes a large number of mergers across industries using matching and DiD analysis. He distinguishes acquirer and target and finds that while targets decrease their R&D expenditure after the merger, acquirers show sharp increases in sales, reducing R&D intensity (but not R&D itself). However, these approaches are faced with identification challenges, as matching might not capture all relevant variables determining the decision to merge and the outcome of the merger. In response, Haucap, Rasch, and Stiebale (2019) use technological and geographical proximity as instruments for M&As. The use of geographical proximity follows Dafny (2009), who instrument exposure to a merger of rivals by their respective geographical proximity and find large price increases.

Other studies relying on observational data without additional identification allow for valuable insights. Cassiman, Colombo, et al. (2005) survey merging firms and argue that technological characteristics are pivotal for the effect of mergers on innovation. When firms with complementary portfolios merge, R&D increases, but a merger of substitutive portfolios decreases R&D. Cunningham, Ederer, and Ma (2021) study drug pipelines of merging firms and find that research activities similar to the acquirer's portfolio are more likely to be discontinued after acquisitions.

Notable exceptions are studies exploiting events credibly exogenous to industry dynamics and industry structure. Watzinger, Fackler, et al. (2020) exploit the government-mandated compulsory licensing of AT&T patents. Baten, Bianchi, and Moser (2017) and Moser and Voena (2012) analyze the confiscation of German patents during the First World War by the US government. All find that ultimately, compulsory licensing has positive innovation effects. Watzinger and Schnitzer (2020) analyze the vertical separation between Bell's research unit and the commercial distribution and operating part of the company in 1984, also finding strong positive innovation effects, with foreign entry playing a major role.

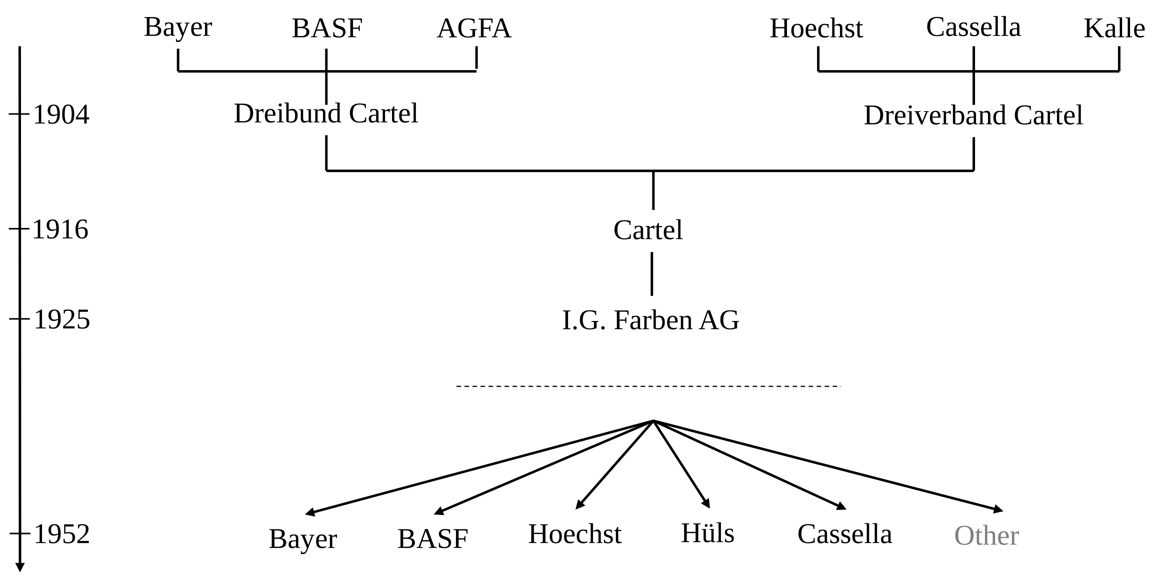
Igami and Uetake (2020) circumvents the empirical identification challenges and estimates a structural model of oligopolistic competition, mergers and innovation. The findings suggest that mergers resulting in fewer than six firms lead to welfare loss, especially drastic for mergers leaving only one or two firms in the market. Goettler and Gordon (2011) analyze the neck-and-neck competition between duopolists Intel and AMD. Due to microprocessors' durable nature, firms need to innovate to gain further sales, leading to large innovation incentives even for monopolists. As a consequence, Goettler and Gordon find that without competition, innovation in the sector would have been higher. However, increased prices would have offset any benefits to consumers.

In the vicinity of the competition-innovation literature built around mergers, there are other notable research trajectories. Authors have analyzed trade-related competition changes, e.g. from liberalization episodes, and find ambiguous innovation results. Shu and Steinwender (2018) survey the literature. A separate channel is the diffusion of knowledge and practices towards acquired firms. Acquisition by more productive, often foreign, firms leads to positive innovation or productivity effects (e.g. Arnold and Javorcik, 2009; Guadalupe, Kuzmina, and Thomas, 2012; Braguinsky et al., 2015).

4.3 Historical Background: IG Farben and Breakup

This section provides a brief history of IG Farben's rise and fall, as far it is relevant to the economic analysis of its breakup. For the historical literature on IG Farben, see Hayes (1987) and Plumpe (1990); for the breakup and the IG Farben successors Kreikamp (1977), Stokes (1988), Stokes (1994), and Stokes (1995). Jeffreys (2010) provides a history focused on IG Farben's crimes in more popular writing. Figure 4.1 gives an overview of IG's timeline as well as the eventual split.

Figure 4.1: The Development of I.G. Farbenindustrie A.G.



Notes: Shows the historical time-line of IG Farben, from preceding cartels, 1925 merger and subsequent breakup using stock transfers. Source: Stokes (1988, p. 12). Does not include smaller subsidiaries as well as close cartels of IG Farben in the explosives industry.

4.3.1 Making IG Farben

IG Farben used to be the largest company in Germany and the largest chemical company in the world. It was one of the most innovative German companies, with three of its scientists winning Nobel prizes. IG Farben had an outsized role in the German innovation system, responsible for 5.9% of all patents by German inventors, 16.8% in chemistry. Contextualizing IG Farben's size, its share among German-invented German patents was three times that of AT&T/Bell among US-invented US patents (2%, see Watzinger and Schnitzer, 2020).

IG Farben was founded as a stock company in 1925 through a merger of some of the largest German chemical companies. All the founding members originated from the dye industry, although their product portfolio was broad and diversified by then. Prior to the merger, the companies were part of an organized cartel of the same name. Before the Second World War, cartels were widespread throughout the German economy. German law guiding such cartels stipulated the possibility for each member to quit unilaterally, so that major inefficiencies remained in the cartel organization. If each member could leave and break the cartel, then giving up one's own sales division or name was inconceivable. Hence, the merger was executed, and the founding companies largely gave up their profile to join the new IG Farbenindustrie AG. Next to holdup problems (ter Meer, 1953, pp. 17–23), easier access to capital is cited as another reason for the integration (Abelshauser, 2003, p. 228). Both are merger efficiencies in today's view.

Whether the IG Farben breakup created competition in a particular product market depends on IG Farben's internal organization, which created both specialization and redundancy. In supervising its complex structure, IG Farben created multiple internal groups (Stokes, 1988, pp. 14–19). Control over production remained with the production groups (Betriebsgemeinschaften). 33 major production complexes were organized in at first four, later five such groups. The groups specialized in certain areas of chemistry, such as Upper Rhine (Ludwigshafen) in high-pressure chemistry, Lower Rhine (Leverkusen) in pharmaceuticals or Berlin in photographic paper, film and artificial silk. However, specialization was limited and "almost all of the central factories produced a broad range of basic chemicals, intermediates and finished products" (Stokes, 1988, p. 18). As Stokes notes, much of the differentiation of the production groups was driven by tradition.

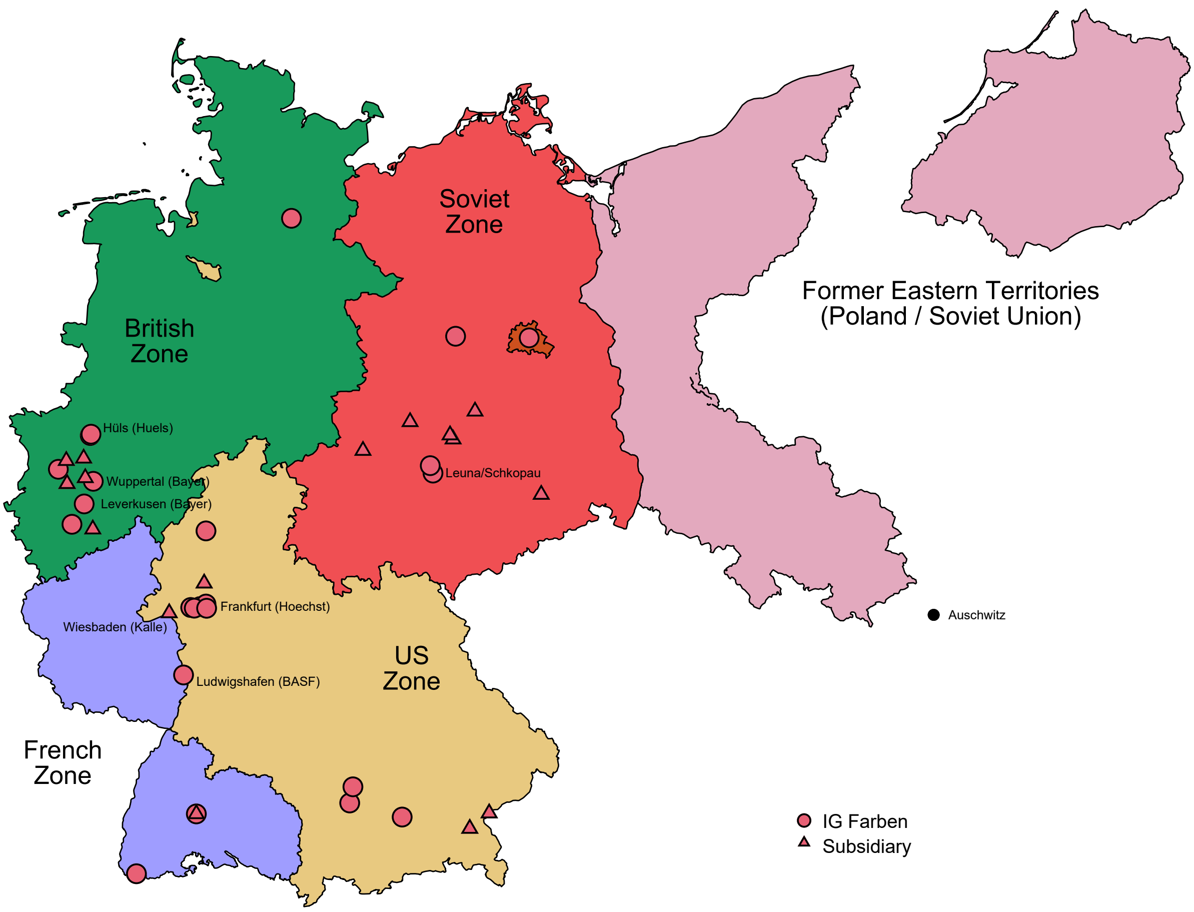
During the 20 years of its existence, IG Farben retained or acquired a dominant position in much of the German organics, plastics and explosives industry. This happened partly through acquisitions and partly through organic growth. IG Farben was further directly or indirectly responsible for much of the production of synthetic fuel and rubber from German coal as substitutes for imports. As part of a wider autarky strategy, they were vital for the start and continuation of the Second World War. Vast resources were devoted to turning coal into gasoline as coal, contrary to oil, was one of the few resources abundantly available to Germany.

IG Farben was not only instrumental to the German war effort but enabled and participated in war crimes and crimes against humanity. In the German-occupied territories, IG Farben conducted extensive acquisitions and was later accused of plundering. As much of the German industry, IG Farben employed forced and slave labor supplied by concentration camps. The most infamous IG investment was in Auschwitz, where one of the most advanced IG facilities was built, yet never completed. An IG Farben subsidiary supplied the Zyklon B pesticide used for murdering more than a million people in Auschwitz and other camps. These three counts, preparing and contributing to wars of aggression, plundering and seizure of plants, and enslavement, deportation and murder, were the base of the IG Farben trials at Nuremberg in 1947/1948. Here, the court indicted 24 IG managers, of whom 12 were sentenced to prison while others were acquitted. While IG's actions before and during the war were and remain a subject of contentious debate, it certainly contributed to the perception of the company as "Hell's cartel" (Jeffreys, 2010).

4.3.2 Breaking IG Farben

IG's importance for the German war machine as well as its crimes resulted in the Allied powers confiscating all of IG's property in 1945, leaving the administration in the hand of the respective zonal government. While the Allies attempted to coordinate the occupation policy, the heightening cold war tensions made this increasingly difficult (Stokes, 1994, p. 71). The subsequent actions differ greatly. While the Soviets quickly began dismantling their IG plants, the Western Allies were more hesitant and grew protective of their respective part of the company. With the Western integration of the US, British and French occupation zone into the bizone and later trizone, the Allied administration of IG Farben was also unified. As a result, Stedman (1950, p. 442) calls the 1945 breakup "largely theoretical" and states that "[t]he individual units today are in closer collaboration than they were then". The breakup question was resolved in earnest only in the early 1950s.

Figure 4.2: Locations of IG Farben manufacturing and research



Notes: Shows IG Farben locations in Germany's 1936 territory, by postwar situation. BASF formed around the Ludwigshafen facilities in the French occupation zone (blue). Bayer formed around facilities in the British occupation zones. Hoechst formed around the facilities in the United States occupation zone (yellow). Some locations (Troisdorf, Marl-Hüls, Wiesbaden) formed smaller successors. The large facilities the Soviet zone (red) in Leuna, Schkopau and Wolfen were restructured as publicly owned enterprises (Volkseigener Betrieb, VEB). The former German areas in the East became Polish or Soviet Union territories after 1945 and did not contain major research-active IG Farben facilities. The IG facilities near Auschwitz, in occupied territories, received large investments during the war, yet never reached completion. Source: Max Planck Institute for Demographic Research: MPIDR Population History GIS Collection, own calculations.

The breakup was not expected or planned for before the war and its structure only determined during the occupation period. IG Farben officials saw the writing on the wall, but eventually, planning for an

Allied victory remained rudimentary (Stokes, 1988, pp. 32–33). Some attempts were made, however. IG officials attempted to transfer ownership of foreign assets to avoid confiscation. Ideas such as a legal separation of war-related factories from civilian production were considered but dropped. In the end, these decisions would be taken by others. Stokes, p. 71, writes: "Although the final outcome of the breakup was not predictable in 1945, zonal policies helped prejudice its general contours. Practically speaking, the major Western successors of I.G. Farben were going to be the three large works units of the old firm, the central factories of which lay in different zones."

Eventually, there are multiple candidates for the exact timing of the intervention. Zonal structure divided effective control among the four Allied Powers in 1945. Then, consolidation of the Western occupation zones consolidated the administration into the Western and Eastern zone by 1948. In August 1950, the Western Allies created the legal basis for separating IG Farben. The announcement of the final structure occurred in 1951, and most of the successors legally incorporated in 1952. While it is possible that specialization processes already happened when returning from war production to civilization production in 1945-1948, most of the focus was on rebuilding, on using known technologies to resume production (Stokes, 1994, p. 73).

The breakup was executed via stock transfers. Each owner of IG Farbenindustrie AG shares received successor shares according to their initial capitalization.

The early postwar time was tumultuous and full of rapid change. After the liberation by the Allied Powers, Germany was occupied. With the occupation came controls of industry and reparations and economic reorganization such as the dissolution of cartels. Events such as the economic and political division of Germany in East and West took effect. These and other aspects of the post-war history are discussed in section 4.8. In most cases, such changes affect all sectors of the German chemical industry, but insofar as effects are differential, they are limitations to the generalizability of the results. For many aspects, it is possible to introduce control variables for statistical robustness checks.

4.4 Data

4.4.1 Patents

This study uses patent data to measure innovation activities. The chemical and pharmaceutical industries are often cited as the two areas where patents are most suitable as a measure of innovation.[4]

To generate the patent data, this study digitizes German patent grant documents between 1910 and 1965. The digitization for this study is complementary and in addition to data available from the German patent office. Appendix D.1.2 discusses the data generation in detail and assesses various quality aspects. Of special note, the German patent office was closed for most of 1945 and all of 1946 and 1947, so that these years are always omitted. War-time applications should be considered with care due to the circumstance of their application, but also because the patent office processed them only in the 1950s. Applicants will selectively pursue prosecution of patents still relevant 5-10 years after the original application.

[4]During the time of interest, the German patent law did not allow product patents in the areas of pharmaceuticals and chemistry. However, processes were patentable. These were effective in deterring entry, as a competitor producing the same product would have to prove that a different process was used (Uhrich, 2010).

For ease of access, patent offices have long classified patents by technology classes. While these are not congruent to individual markets or products, they were considered relevant by technological experts at the time. The German system during the sample period knew more than 500 (sub-)classes, a number comparable to the four-digit level of the present-day Cooperative Patent Classification (CPC). Of those, only a subset is related to chemistry and is the focus of this study. Inspired by Baten, Bianchi, and Moser (2017), these are identified based on descriptions of the group-level technological areas. 135 relevant technology classes remain.[5]

Patent quality is heterogeneous so that adjustment for quality is advisable (Harhoff, Narin, et al., 1999). However, in the historical patent data, information on patent citations is not readily available. Kelly et al. (2018) instead propose alternative quality measure based on patent texts. Kelly et al. calculate similarity scores between patents based on the word counts of the patent text. Patents similar to future patents are called influential, while patents similar to past patents are called derivative. High-quality patents are defined as being influential but not derivative. A quality score results by dividing future similarity by past similarity. This study adjusts Kelly et al.'s methodology by plugging in a more modern approach focusing not on word counts but the text's overall structure (Le and Mikolov, 2014). Appendix D.1.2 discusses this and other adjustments necessitated by the German patent data. The quality measures are normalized on a patent level to have mean three and standard deviation one, making them comparable while excluding negative values.

4.4.2 Products and Prices

Product and price data describe established markets. This is complementary to patent data, which provides insights regarding new products and processes.

Product-firm listings were produced by specialized publishers and provided an industry overview to customers of chemical products. This publication series used here is the most relevant one for the German chemical industry. A review of the 1938 edition in a German chemistry journal doubled down on this book's claim of being an indispensable encyclopedia for the German chemical industry (Bretschneider, 1939). Its tradition started in 1888 and ended only in 2000 when the publication format went digital. The volumes allow users to look up producers for a set of common chemical products and to contact them via mail or telephone. The books are published roughly every three years. With its long publication history before and after the war, it presents the best data source for tracking the German chemical industry.

For this study, three digitized books inform about the situation in mid-late 1939, early 1952 and early 1961, judged by the dates of the editorials (Wegner, 1940; Barth, 1952; Wegner, 1961). These are the volumes last published before and first published after the war, as well as one informing about long-run consequences.

The definition of a product as chosen by industry experts delivers a relevant definition of markets. Clearly, varying degrees of substitutability between products exist, as do vertical relationships. Overall, however, this type of data is substantially more fine-grained than typically used industry definitions (Affeldt et al.,

[5]The CPC's four-digit level is a comparably high-level aggregation. Technology groups, the next-lower level of aggregation in the German classification, is not consistently available. The next-higher top-level grouping has 89 technologies, of which 34 refer to chemistry. Unfortunately, with on average only three subclasses per class, inclusion of class×year FEs does not leave enough variation of exposure within classes.

2021) and, on a large scale, more closely approaches the definition of relevant markets than otherwise attainable.

The books contain a selection of the most relevant products in chemistry. To gather this information, the publisher surveys firms for their product portfolio. For firms, the listing is free of charge, and the operation is financed by sales of the book and advertisement contained therein. For details and examples, refer to Appendix D.1.1. With this, the books describe static competition in the chemical industry and the dynamics of firm portfolios. Entry and exit decisions into the supply of existing products can be captured well. However, the scope of listed products follows the contemporary situation. As an example, consider polyvinyl chloride (PVC), one of today's most widely produced plastics. PVC was discovered in the 19th century, but industrial scale production in Germany started only in the 1930s. IG Farben sold one PVC variant as a branded product, which is listed in the 1939 volume. Also, IG Farben and one subsidiary are listed as producing "vinyl polymer products" (Vinylpolymerisate), a broader group of chemicals also encompassing PVC. Listing under PVC starts in 1952 when plastic production had become more widespread. Then, five IG Farben successors were offering PVC for sale. While the listed products are the most relevant, the development of new chemicals before they reach widespread use has to be tracked with patent data.

Product-level data from supplier lists comes with some disadvantages. No quantity or sales information is available. Many chemical products are traded at the national level so that no local variation is observable. Trade information is unobservable as well. Foreign suppliers are not listed, although trading firms and resale agents are.

The information from supplier lists is enriched using data from Wikipedia and ChemSpider, which maintain lists of chemical substances with common alternative names and chemical properties.

Product-level price data is available in publications of industry journals. The prices represent factory gate prices and are meant to guide readers towards the general price level for a product. Price data becomes available in 1948 when price controls in the occupied areas are relaxed. Before that, prices as of 31.12.1944 were fixed (Fäßler, 2006, p. 42). This resulted in considerable inflation in 1948. At the same time, the price lists are not as comprehensive as in later years. From 1949 until 1954, high-quality data is available, after which the journal only reports foreign prices.[6] Appendix D.1.1 reports details.

4.5 Describing the Breakup

Econometric analysis of the IG Farben breakup and the historical context first requires a quantification of the breakup. The best available measurement is patent data, which describes the technological capabilities of individual firms on a detailed level. Subsection 4.5.1 measures the breakup in innovation space. Historical financial and product data, while less detailed, helps contextualizing the development of IG Farben and its successors. Subsection 4.5.2 provides a descriptive analysis.

[6]From 1945 to early 1948, price controls are in place, possibly similarly during and before the war. Figure D.17 in the appendix shows stable pre-war prices. Stable prices made monthly price updates unnecessary, explaining why price publications commenced in 1948. The reason why the publication of price lists stopped is unknown.

4.5.1 Measurement of Competition in Innovation Space

In innovation space, the distribution of IG patents across research facilities and technology classes characterizes the impact of the breakup. Two approaches are possible. First, the standard Herfindahl-Hirschman index (HHI) provides a description of concentration within technology classes and relates to the previous literature. The change in HHI provides an intuitive description of the concentration change caused by the IG Farben breakup. However, HHI strongly depends on the share of IG Farben-related patents in each technology class. A set of alternative measures removes this dependence by considering only IG Farben-related patents. Starting from this set, alternative HHI can be computed, either by analyzing the breakup of IG Farben across successors or by exclusively analyzing geographical variation across occupation zones.

The change in the HHI implied by the IG Farben breakup is $\Delta\text{HHI} = \text{HHI}^{IG} - \text{HHI}^{\overline{IG}}$, the difference between the concentration with the IG as one group and the IG as separate entities. This defines the concentration change as a positive value, although by definition, HHI decreases. Nocke and Whinston (2020) suggest that between the absolute HHI and ΔHHI, the focus on the latter is more relevant.[7]

The implied concentration change ΔHHI can be examined for the pre-war period (1925-1939) as well as for periods post-war, pre-breakup (1948-1951) or after the breakup (1952-1960). In all three cases, either HHI^{IG} or $\text{HHI}^{\overline{IG}}$ is counterfactual and unobserved. While all three are highly correlated, the pre-war is least likely to be influenced by other contemporary events or endogenous to the breakup itself. Therefore, it is the baseline measure of ΔHHI. After 1952, the successor firms have resumed their separate activities and already responded to the breakup. Between 1948-1951, anticipation and war-related confounders may play a role.

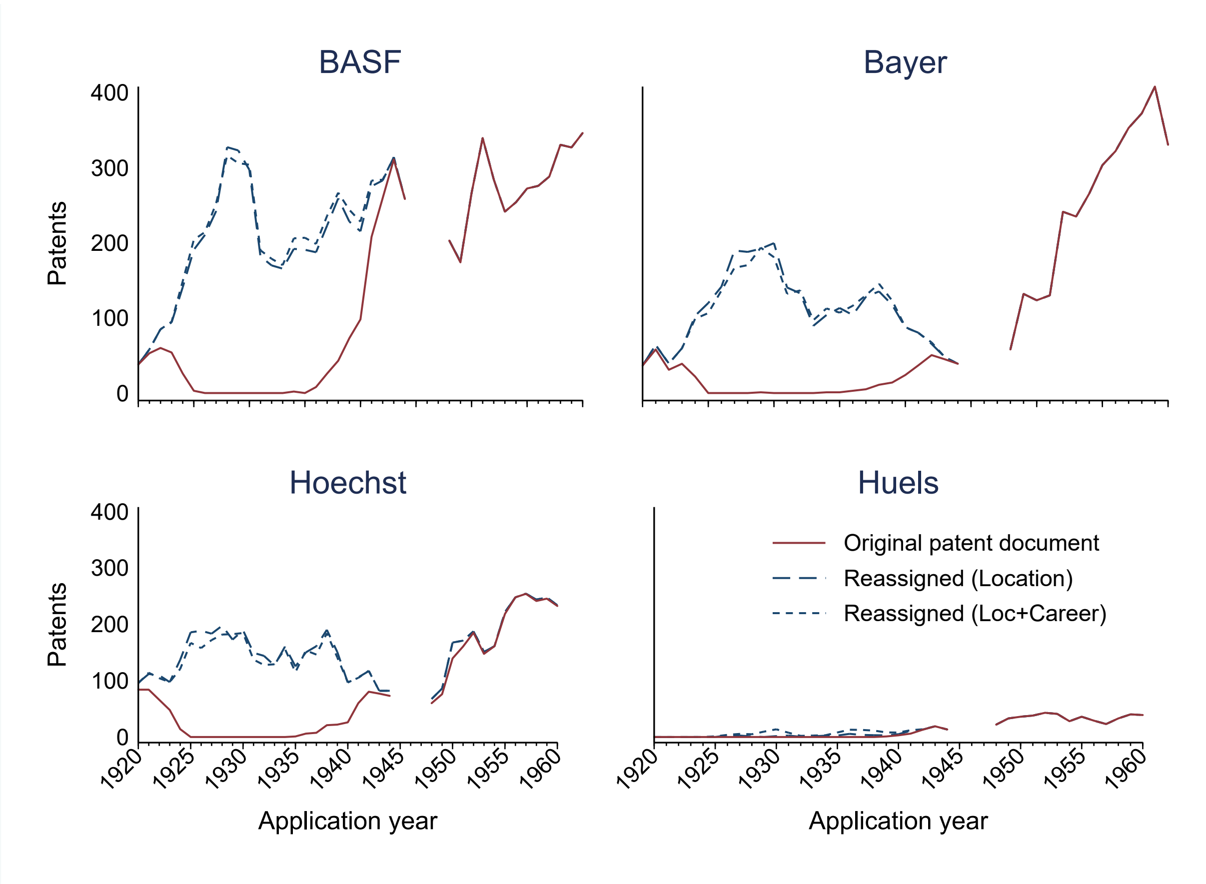
Calculating the implied concentration change ΔHHI in the pre-war period requires assigning pre-war patents to the eventual successor companies. While some successor companies filed patents as separate entities, the bulk of patents was filed as “IG Farben”. Thus, patent applicant information does not inform about the origin of a particular invention. However, with the geographic dispersion of research facilities across Germany, the location of inventors reveals the association to the eventual successor.[8] Fortunately, IG Farben was already listing inventor information on its patents before it became mandatory. Assigning inventor locations according to the nearest research facility or according to pre-merger or post-war employer recovers the patent portfolio of the IG Farben successors.[9] Appendix D.1.3 covers the details of the reassignment. Figure 4.3 shows the result for the largest successors. This process is successful for up to 90% of IG Farben patents. For calculations of ΔHHI, unassigned patents and those of East German IG members are ignored, preserving shares.

[7]Alternatively, measures such as the CR4 share, the share of patents by the four largest applicants, can be analyzed. That measure is less sensitive to incorrect disambiguations for smaller applicants. On the other hand, the IG breakup often replaced the largest applicant with three applicants that are still the largest ones. The CR4 change is then fully determined by the share of the two applicants that were previously in the top 4, which is not a good measure of the implied concentration change. Even so, Table D.5 in the appendix shows the change in the CR4 share.

[8]IG Farben maintained at least 25 research laboratories (Plumpe, 1990, p. 475). Inventive activity was ongoing in all major work units (Haber, 1971, p. 357; ter Meer, 1953, pp. 29–30).

[9]This reassignment rule comes close to the actual post-war reassignment of patents. Contemporary reports indicate that the patents were typically assigned based on the research unit where the patent was originally developed. (“Forschung BASF” 1953) The patent documents do not list ownership changes and subsequent transfers. Especially for the question of post-war ownership of pre-war inventions and of the patent stock of the old IG Farben, such information would be valuable.

Figure 4.3: Patents of successor companies, assigned by inventor locations



Notes: Subsidiaries aside, IG Farben's Frankfurt headquarter is the applicant of all IG Farben patent. However, unlike most companies at the time, almost all patents list the inventors. Due to the geographic spread of IG Farben's research facilities, inventor locations allow the reassignment to eventual successors. Only in some cases, the inventor careers from deduplicated patent applications are more informative. Here, inventors are reassigned to their post-war place of employment. The graph shows the yearly number of granted patent applications for the three large successor companies and the newly independent Huels. Numbers are as listed on the original patent documents (red solid line), as reassigned to eventual successors using location information (blue dash line) and as reassigned to eventual successors using location information and inventor name disambiguation (solid blue line). For the smaller successors and East German patents, see Figure D.9.

The IG Farben dissolution led to large concentration decreases in a set of technology classes, typically equivalent to the reversal of a 4 to 1 merger, with some variance. This equivalence is based on the thought experiment of splitting up the IG Farben conglomerate into N^{Δ} equal parts. N^{Δ} is then a function of the IG Farben share s_{IG} and of ΔHHI, in particular $N^{\Delta} = s_{IG}^2/(s_{IG}^2 - \Delta\text{HHI})$.[10] This idea is closely related to the number equivalent of the HHI first discussed in Adelman (1969). While $N(\text{HHI}) = \text{HHI}^{-1}$ is the number of equal-sized firms corresponding to HHI, N^{Δ} is the equal-sized breakup corresponding to ΔHHI. Aggregating over all patents in chemistry, IG Farben had 12.4% of all patents, split into 3.6% BASF, 2.1% Bayer, 2.3% Hoechst and 4.3% remainder. Considering IG Farben and subsidiaries as one block, overall HHI was 212, split up it was 91, ΔHHI 121. N^{Δ} arrives at 4.8. ΔHHI strongly varies across technology classes, often reaching much higher values than at the aggregate. Table 4.1 lists these statistics for a selection of technology classes and averages over the subsequent groups of high and low breakup exposure. For reference, a merger with an effect of $\Delta\text{HHI} > 100$ or > 200 (Depending on absolute HHI) would be above the FTC screening thresholds. With the 75th percentile cutoff, the

[10] Originally, $\Delta\text{HHI} = \text{HHI}^{IG} - \text{HHI}^{\overline{IG}}$. HHI contributions of non-IG firms are unchanged in this static thought experiment, so that the equal split formally results in $\Delta\text{HHI} = s_{IG}^2 - N^{\Delta}(s_{IG}/N^{\Delta})^2$. Rearrange for N^{Δ}.

Table 4.1: ΔHHI implied by the IG dissolution

Selected technology classes	Patents 1925-1939						48-52
	Count	IG %	HHI^{IG}	$\text{HHI}^{\overline{IG}}$	ΔHHI	N^{Δ}	ΔHHI
8M: Coloring	643	56	3269	787	2481	4.80	1206
12G: Processes (general)	398	26	706	316	390	2.46	149
12K: Ammonium, Cyanides	484	16	384	224	161	2.47	263
22E: Indigo-based dyes	377	77	5988	1459	4529	4.26	2336
29B: Chemical fibers	601	28	851	239	612	4.43	104
30H: Drug development	1048	15	250	94	156	3.58	49
39C: Synthetic plastics	326	51	2693	918	1775	3.09	670
45L: Pesticides	699	31	1071	349	722	3.69	180
Means for ΔHHI > p75 (N=33)	730	0.37	1803	602	1201	3.53	428
Means for ΔHHI ≤ p75 (N=102)	673	0.04	397	375	21	2.46	34

Notes: Concentration change implied by the IG Farben breakup for selected technology classes and by breakup exposure. The columns show the count of granted patents, the share of patents by IG Farben or subsidiaries (IG %), the Herfindahl-Hirschman index considering all as one block (HHI^{IG}) and split up according to the eventual successors ($\text{HHI}^{\overline{IG}}$) as well as the difference, ΔHHI. N^{Δ} is the number equivalent of ΔHHI, the number of equal-sized companies that the change in HHI implies. The first columns consider patents filed between 1925 and 1939, the last column for 1948-1952. Patent counts is rounded from fractional counts. Statistics are calculated by technology class, means across exposed/comparison technology classes in the last two rows. Table D.5 in the appendix shows the change in the CR4 share.

threshold is 160. Averaging over the group of strongly affected technology classes, N^{Δ} is 3.5. A 4:1 merger reversal is a good midpoint for the range of calculated values.

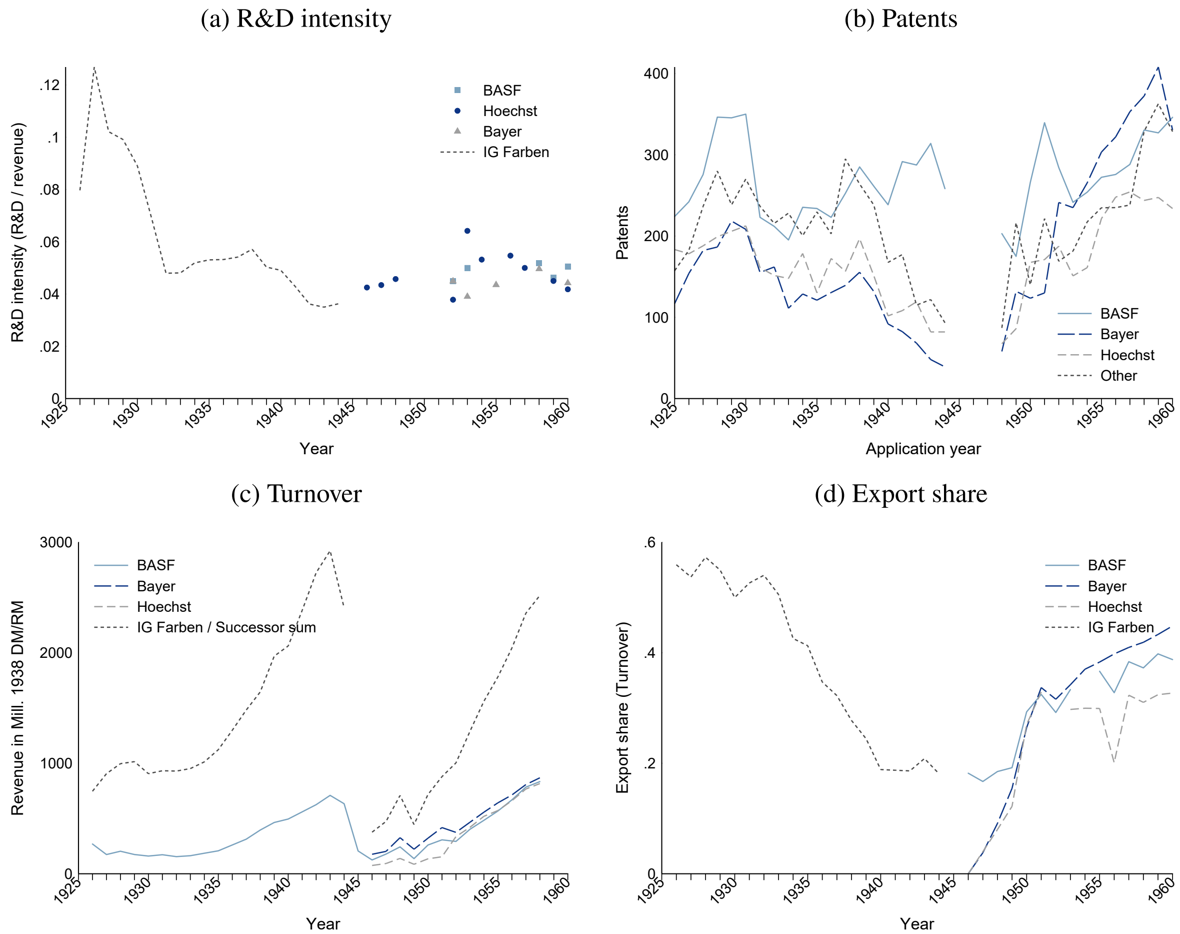
Finally, alternative measurements of ΔHHI that only focus on geographical aspects of the breakup can be calculated. To do so, only IG Farben-related patents are considered. As a consequence, $\text{HHI}^{IG} = 10000$ for all technology classes. $\text{HHI}^{\overline{IG}}$ either follows the structure of the eventual successors, determined as outlined above, leading to $\Delta\text{HHI}_{\text{Within}}$. Alternatively, only the geographical distribution of IG Farben across the occupation zones can be exploited. Instead of successor shares, shares in British, French and US occupation zone form the bases of $\text{HHI}^{\overline{IG}}$. This removes variation introduced from subsidiary structures and leads to $\Delta\text{HHI}_{\text{Occ}}$. In both cases, East German successors or patents are ignored, as are patents unassigned to any successor.

4.5.2 Descriptives on IG Farben

The economic effect of the breakup on IG Farben itself is difficult to study causally, as appropriate control groups are hard to find. The number of successor companies, despite IG Farben's size, remains rather small for statistical analysis. However, descriptive analysis is possible. This section first discusses the aggregate statistics of Figure 4.4 and Figure 4.5, and later dives deeper into IG Farben's product portfolio.

IG Farben was an export-oriented company with high R&D intensity. At the peak of its strength in the late 1920s, R&D spending reached 8-12% of revenue, which in turn was over 50% derived from exports (Figure 4.4). In the context of great depression and Nazi autarky policy, domestic turnover rose during

Figure 4.4: IG Farben and its successors over time



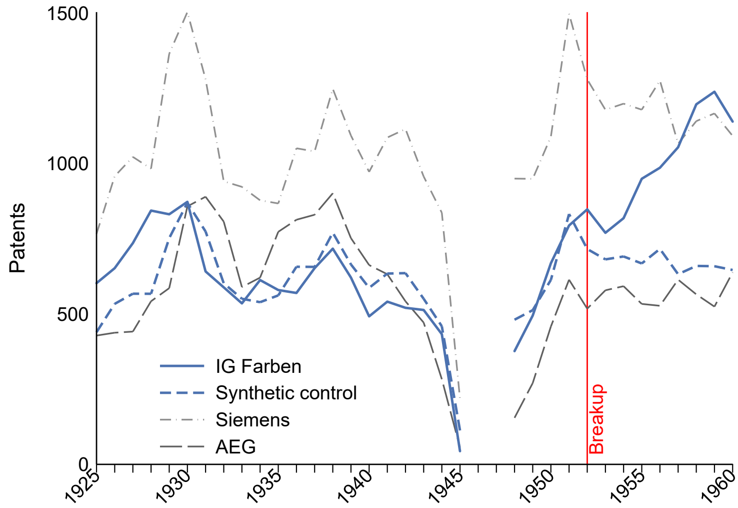
Notes: Data as available from secondary sources. 4.4a: R&D intensity of IG Farben and successors. 4.4b: Patents by IG Farben members. Patents with applicant IG Farben reassigned to successors according to inventor locations and biographies. Unassigned patents are counted towards all four groups, preserving shares. 4.4c: Turnover of IG Farben and successors. IG Farben after 1945 is the successors' sum. Source: ter Meer, 1953 (IG Farben data), Abelshauser, 2003 (BASF turnover), Stokes, 1988 (Exports, turnover), FAZ/ZEIT newspaper archives (various), Statistical yearbooks (Inflation), own calculations following section 4.4.1 (Patents).

the 1930s and 1940s, while export shrank further and further.[11] R&D continued to play an important role, but at more moderate levels than before.

The immediate post-war statistics reflect the economic difficulties, but also the fast return to pre-war levels (Figure 4.4). Turnover collapsed after the war, and export links had disrupted. However, as with the overall economy, recovery was quick enough that by the early 1950s, the Western IG Farben successors could reach turnover and export shares as in the mid-1930s. R&D intensity and patenting levels of the successors initially remained at comparable levels to before the war, with large increases in patenting and high but constant R&D intensity thereafter. Over the course of the next decades, all successors became globally successful corporations.

[11]Especially the synthetic fuel business, which was built on the assumption of an impeding peak oil could only be sustained through government subsidies after the discovery of new oil fields eliminated the economic basis. As such, the policies towards autarky pursued by Nazi Germany were convenient.

Figure 4.5: Comparison to firms in electronics industry (Synthetic control)



Notes: Patenting of IG Farben and its successors compared to firms in the electronics industry. Only patents located in West Germany and Berlin are counted. AEG includes Telefunken and Licentia. Siemens includes Siemens & Halske and Siemens Schuckertwerke. Other firms entering the synthetic control are Bosch, C Lorenz/Standard Elektronik Lorenz, Tenovis and Voigt & Haeffner. The synthetic control procedure (Abadie, Diamond, and Hainmueller, 2010) only fits on the 1925-1944 patent counts, resulting in 65% combined weight for AEG and Siemens. A synthetic control using normalized weights yields similar results, with more balanced shares.

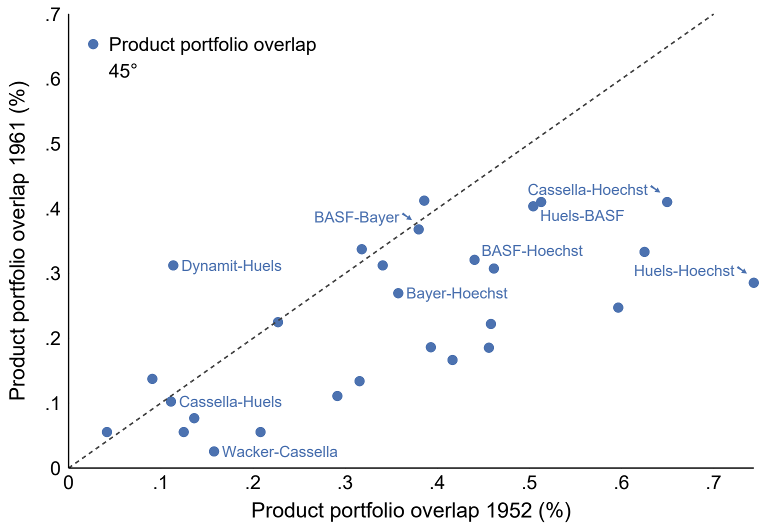
While it is difficult to find appropriate control firms to IG Farben and its successors, the best attempt at a descriptive analysis is the comparison with firms in electronics. The electronics sector was dominated by a duopoly of Siemens and AEG, with some smaller companies like Bosch contributing a smaller share. While Bosch and Siemens were at some point targeted for decartelization measures equivalent to IG Farben, these remained largely without effect. Other candidate sectors drop out as they were also affected by Allied breakups (Heavy industry/Steel) or disproportionately benefited from the war (Automotive engineering). Figure 4.5 shows that patenting by IG Farben successors increased relative to AEG, Siemens or a synthetic control group (Abadie, Diamond, and Hainmueller, 2010) of electronics firms, but this result should be interpreted cautiously.

Fine-grained analysis at the product level allow a better understanding of the breakup structure. With product-level data, it is possible to characterize the portfolios of the IG Farben successors and where they compete with each other. When breaking up companies, it is in principle possible to do so along product lines, as is often attempted in demergers initiated by the companies themselves. Similarly, IG Farben could have been separated between markets and competition would have remained constant.

The extent of actual competition between the IG Farben successors can be measured by the overlap in their product portfolios in product catalogs. Between pairs of IG Farben successors, the overlap is the number of entries offered by both companies. The overlap share is the overlap divided by the maximum possible overlap, which is the smaller company's portfolio size. As no quantities of products sold are available, it is not otherwise possible to quantify the change. Figure 4.6 reports the results for the 1952 and 1961 lists, with the largest successors and some border cases highlighted. The full list is available in Table D.15 in the appendix. There is substantial overlap in the product portfolios. For the largest companies, the overlap share is at 30-40%. Some smaller companies have up to 70% offered by the other, larger company. The intervention served to create within-market competition.

Figure 4.6 also gives an overview of the development of competition over time by serving a comparison with the 1961 issue of the book. Note that this figure compares two cross-sections. The set of products changes between catalogs. Products may be delisted because they are no longer in frequent use, but mostly, products are added as the books became more detailed. This possibly reflects editorial choices as well as the increasing development of the chemical industry. Although the absolute number of products increased, the companies now typically have less overlap between them. The overlap share also decreased. This suggests that companies changed their product portfolios by specializing, but also that the increased product-market competition persisted for at least ten years.

Figure 4.6: Competition changes in product space



Notes: Bilateral competition between key IG Farben successors companies, 1952 and 1961. Two cross-sections are analyzed, showing the overlap share of chemical products between pairs of successors. Overlap shares are the number of products both companies are offering divided by the product portfolio size of the smaller of the two companies.

In analyzing the development of product portfolios over time, not only cross-sectional but within-product views are relevant. Table 4.2 focuses on products with supplier data from 1939, 1952 and 1961. Also, only products with price information are analyzed. The table lists the number of IG Farben suppliers over time. In the first half of the table, products are split by IG Farben supplier status in 1939, in the second half by IG Farben successor supplier status in 1952. Two developments become clear. For one, there is some shift in the IG Farben portfolio between 1939 and 1952. Some products are newly offered; some products are no longer in supply. Especially the share of products offered by more than one IG Farben member has now increased. While this situation was already present in 1939, for example if a product could be supplied by a subsidiary of IG Farben and the main company itself, by 1952 it is exacerbated.

With the product data, it can be concluded that the breakup created competition in relevant markets. The horizontal nature of the breakup lasted, and while the successors specialized, long-term competition was stable.

Table 4.2: Number of IG Farben suppliers by year and by 1939/1952 IG exposure

Panel A: IG Farben suppliers of products conditional on 1939 IG Farben status

	IG Farben Products (1939)				Other Products (1939)			
	Share No IG	Share One IG	Share Multiple	Mean IG Farben	Share No IG	Share One IG	Share Multiple	Mean IG Farben
1939	0.00	0.83	0.17	1.17 (0.37)	1.00	0.00	0.00	0.00 (0.00)
1952	0.30	0.35	0.34	1.32 (1.31)	0.83	0.13	0.04	0.22 (0.54)
1961	0.34	0.28	0.38	1.28 (1.29)	0.87	0.09	0.04	0.17 (0.49)

Panel B: IG Farben suppliers of products conditional on 1952 IG Farben status

	IG Farben Products (1952)				Other Products (1952)			
	Share No IG	Share One IG	Share Multiple	Mean IG Farben	Share No IG	Share One IG	Share Multiple	Mean IG Farben
1939	0.27	0.58	0.15	0.89 (0.64)	0.80	0.18	0.01	0.21 (0.44)
1952	0.00	0.58	0.42	1.73 (1.08)	1.00	0.00	0.00	0.00 (0.00)
1961	0.23	0.35	0.41	1.45 (1.24)	0.93	0.05	0.03	0.10 (0.38)

Notes: Describes the number of IG Farben successors, split by whether products were offered by IG Farben-connected companies in 1939 (Panel A) or 1952 (Panel B). Rows tabulate the status in 1939, 1952 and 1961. The first set of columns looks at products offered by IG Farben in 1939/1952 and shows shares by current supplier status. The second set of columns looks at products only offered by other companies in 1939/1952, again showing shares. The last two columns show means and, in brackets, standard deviations. The data in this table only covers products with data from 1939, 1952 and 1961 and where at least one price information is available. First half: 566 Products, of which 229 were produced by IG Farben in 1939. Second half: 566 Products, of which 218 were produced by IG Farben in 1952.

4.6 Empirical Strategy

The analysis is based on a standard DiD analysis, comparing observations with high exposure to the IG Farben breakup to such with low exposure. The former is labeled exposed, the latter comparison group. Equation 4.1 fixes notation.

$$Y_{it} = \alpha_i + \beta_t D_i + \delta_t + X_{it} + \epsilon_{it} \tag{4.1}$$

The regressions include unit fixed effects α_i and time fixed effects δ_t. Exposure to the IG Farben shock D_i is typically binary or categorical, but is also a continuous variable for some analyses.

Details depend on the outcome variable so that the discussion in this section mostly focuses on the innovation analysis. The three outcome variables are innovation (patent counts), market structure (firm counts) and prices. The unit of observation and the time structure is different for each so that unified exposition is difficult and details are covered in the respective part of section 4.7.

The breakup exposure variable measures the concentration change of the IG Farben breakup along the lines of the successor companies. Concentration levels within technology classes HHI can be calculated from patent counts by applicant, see section 4.5.1. By considering IG Farben either as one unit or separate as the eventual successors, the difference in concentration levels ΔHHI measures the breakup exposure.[12]

[12] In product-level analysis, the catalogs allow the comparison between products offered by IG Farben and other products. In particular, products where IG Farben successors competed with each other can be compared to such where only one successor is active. With this, the analysis can be directly linked to product market competition.

In the main analysis, ΔHHI follows the literature by calculating HHI from shares of IG Farben successors towards the overall set of patents in a technology class. Alternatively, measures solely focusing on the breakup within the set of IG Farben patents can be calculated, thereby isolating variation introduced by the breakup across occupation zones.

With the breakup taking place in the early 1950s, there are two pre-periods. One long pre-period covers the time before the war, where IG Farben was one company. The main interpretation of the pre-war period is to establish long-run parallel trends. The war time itself sometimes also comes with observational data but is less reliable. After the war follows the post-war, pre-breakup period, 1948 to the early 1950s. This period informs about new post-war levels. Finally, in 1952 most successors had incorporated, and the breakup had taken effect. The post-period reaches until the 1960s.

Identification Assumptions Considering the IG Farben breakup as a previously unanticipated event and predominantly geographically executed, the analysis can rely on two different assumptions for causal identification.

First, investments in technology and production capacity are long-term and did not take the subsequent breakup into account because it was not foreseeable. This motivates a comparison between technology and product areas strongly affected by the shock to those only lightly affected. While variation of IG Farben investments across technology areas are not random, this variation is unrelated to the eventual breakup. Without the breakup, so the assumption, there would have been no differential development.

A second argument relies on the geographic structure of the IG Farben breakup. In the first place, research and production are not randomly distributed across IG Farben facilities. However, the distribution is chosen independently of the geography of the Western occupation zones, which historically strongly impacted the breakup structure. With different zonal structures, e.g. BASF and Hoechst or BASF and Bayer in the same zone, the breakup might have been structured differently. For example, had France not insisted on its own area of influence, BASF could have been part of the US zone, changing the initial structure for breakup considerations. A breakup along production lines instead of geography would have been a theoretical possibility as well. Summarizing, the second possible identification assumption is that areas with and without redundancy along occupation zones would have, absent the shock, developed similarly.

The first approach can be realized in all analyses, the second one only where sufficiently fine-grained data is available. For example, in product-level regressions, it is possible to compare product areas with IG Farben activity, separated by whether the breakup created competition or not. In innovation analysis, measures focusing on within-IG variation breakup exposure implement the second argument.

A fundamental assumption regarding the historical context is that the IG Farben shock can be separated from other contemporary changes. The effects of war destruction, dismantlement, or the German separation should be distinguishable from effects of the IG Farben breakup. Many parallel events are quantifiable, and the timing of the IG Farben breakup allows for a discussion of this assumption. All related arguments are collected in section 4.8.

4.7 Results

4.7.1 Effects of the Breakup in Innovation Space

The main outcome variables relate to the overall patenting activity in a technology class or to patenting by non-IG Farben firms. While the theoretical literature as well as antitrust litigation focuses primarily on direct effects on the merging parties, for an economic analysis of the breakup, an aggregate view is crucial. While only descriptive, section 4.5 suggests increased patent output of the IG Farben successors following the breakup. Competitors responses might counteract of exacerbate this pattern.

Table 4.3: Descriptive statistics for IG/non-IG exposed technology classes

Comparing 1925-1939 tech classes: High vs low breakup exposure					
N=33 (T) 102 (C)	Exposed	Comparison	Difference	(SE)	p-value
Granted patents (p.a.)	48.65	44.88	−3.78	(21.30)	0.860
- Domestic	35.82	33.02	−2.80	(16.94)	0.869
- Foreign	9.80	8.78	−1.02	(3.34)	0.762
- Quality-weighted	143.83	134.57	−9.26	(62.64)	0.883
Matched to firm (%)	0.60	0.30	−0.31	(0.03)	0.000***
- IG Farben (%)	0.37	0.04	−0.32	(0.02)	0.000***
- Other (%)	0.24	0.26	0.02	(0.03)	0.531
HHI (IG together)	1802.86	396.70	−1406.15	(248.57)	0.000***
HHI (IG separate)	601.76	375.47	−226.29	(185.14)	0.224
Domestic East (%)	0.17	0.18	0.01	(0.02)	0.624
Domestic East/Berlin (%)	0.24	0.31	0.08	(0.02)	0.000***
War destruction (%)	0.33	0.33	−0.01	(0.01)	0.343
Dismantle (%)	0.40	0.13	−0.27	(0.02)	0.000***
Dismantle (No IG, %)	0.08	0.09	0.01	(0.01)	0.660

Notes: Shows difference between technology classes with high and low breakup exposure. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All data refers to patents applied for in 1925-1939. Patents counts are annual. Domestic and foreign patents are identified using inventor locations if available, applicant locations otherwise. Patents are weighted according to forward text similarity divided by backward text similarity, on patent-level normalized to mean three and standard deviation one. The share of matched patents refers to patents matched to the firm dataset described in section 4.8. HHI is calculated first assuming all IG Farben members to be one entity, then separately according to their post-1952 split-up. The location of patents is first described by the share applied for from the Eastern, Soviet sector. Berlin is handled separately due to its special, divided status. War destructions refers to the share of flats destroyed between 1939 and 1945, weighted by the patent locations in a technology class. Dismantlement on the technology class level is calculated as the share of patents by firms targeted by dismantlement. As the exposed group is strongly selected towards IG Farben patents, it is also shown considering only non-IG firms.

With quality-weighted patent counts and measures of breakup exposure across technology classes, DiD regressions in a technology-year panel are possible. The computation of quality measures and breakup exposure is explained in sections 4.4 and 4.5.1. Only technology classes in chemistry are considered. The subsequent results follow a two-way fixed effect regression with patent class and application year fixed effects and an interaction of the application year with the breakup exposure indicator, which cuts at the 75th percentile of the exposure score. Subsequently, continuous interactions are analyzed. Standard errors in the regressions are clustered at the technology class level (Bertrand, Duflo, and Mullainathan, 2004).

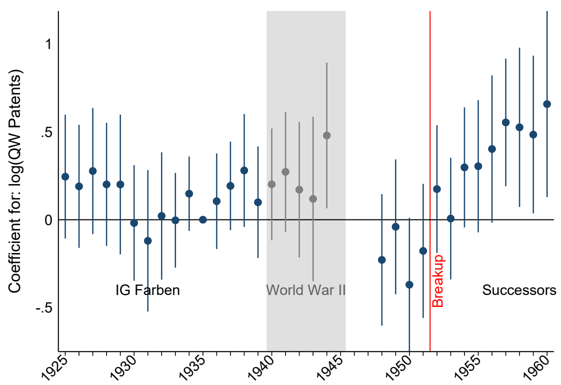
The two sets of technology classes exhibit similar pre-war descriptives. Table 4.3 reports that technology classes with high and low breakup exposure have similar pre-war patent counts, also in terms of patenting by foreigners and by East German firms and inventors. They differ only in terms of exposure to the

IG Farben shock. Applicants are more likely matched to firms in dismantlement lists and product catalogs, but the difference is fully driven by IG Farben. Classes with high breakup exposure are more concentrated before the war, but IG Farben fully drives this difference as well. The higher overall share of dismantlements is also driven by IG Farben, which is discussed in detail in section 4.8.

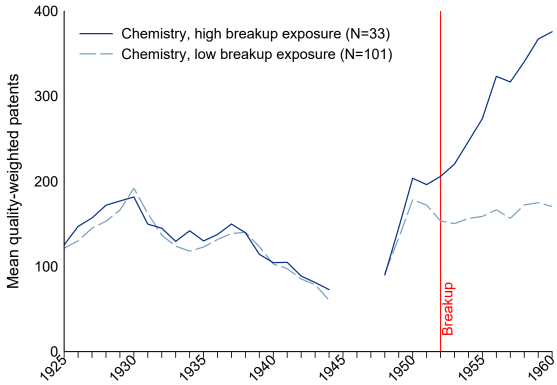
A first set of results shows specifications exposing the full dynamics of pre-trends and post-breakup differences. Figure 4.7 distinguish four periods. First, in the pre-war period, IG Farben was one company. Then, the patent applications during the Second World War are grayed out. Wartime applications were only processed in the 1950s. As firms will only pursue applications still valuable under the new breakup situation, patent counts are subject to selection bias. After the war follows the post-war, pre-breakup period from 1948 to 1951. Finally, in 1952 most successors had incorporated, and the breakup had taken effect. The baseline regressions with quality-weighted patent counts, Figure 4.7a, shows flat pre-trends before the war and before the breakup and long-run increases in patent count after the breakup. Panel 4.7b plots raw averages. The delayed start of the effect in both panels is characteristic of real innovation processes, where R&D investments may take some time to materialize as patents.

Figure 4.7: Technology class-level DiD regressions: Quality-weighted counts

(a) Quality-weighted patent count: Regression



(b) Quality-weighted patent count: Descriptives



Notes: Descriptives and regressions comparing technology classes with high and low exposure to the IG Farben breakup, as defined by the 75th percentile of ΔHHI (160). Exposure is measured using pre-war (1925-1939) data, but the breakup is finalized and effective around 1952. Shows quality-weighted counts of granted patents, with average patent quality winsorized and rescaled to have average three and standard deviation one to exclude negative values. 4.7a shows OLS regressions of log quality-weighted patent counts in technology classes with and without pre-war IG Farben breakup exposure. Shows 95% confidence intervals. 4.7b shows average quality-weighted patent counts in the two groups. The graphs correspond to mean(log y) (left) and mean(y) (right), explaining the difference. The German patent office closed in 1945-1947. Wartime patent applications are largely prosecuted post-war. The coefficients are set in gray to indicate possible bias.

In subsequent analyses, the detailed dynamics are no longer considered and instead, grouped DiD coefficients are reported for a larger set of dependent variables. $\beta_{1948-1951}$ and $\beta_{1952-1960}$ group the respective years, showing differences to the baseline period 1925-1944. The main coefficient of interest is $\beta_{1952-1960} - \beta_{1948-1951}$. While pre-war and post-war levels of the outcome variables are often comparable (so $\beta_{1948-1951} = 0$), the war could have resulted in level shifts, making the individual β comparisons uninformative about the breakup. Table 4.4 reports coefficients for a set of alternative dependent or breakup exposure variables. Results are robust to estimation using Poisson regression (Table D.6) and the inclusion of control variables (Table D.12). The DiD coefficient is 0.58. With average patent-level quality normalized to three, this amounts to 14.9 excess patent grants per patent class and year.

Table 4.4: Effects in Technology class-level DiD regression

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Exposure: ΔHHI 1925-1939						1930-1939	1925-1935	1948-1952
log(Patents)	All (Quality)	Non-IG (Quality)	All (Count)	Non-IG (Count)	Domestic (Quality)	Foreign (Quality)	Non-IG (Quality)	Non-IG (Quality)	Non-IG (Quality)
48-51× High ΔHHI	−0.354***	−0.097	−0.287**	−0.073	−0.716***	0.384***	−0.044	−0.067	0.064
	(0.121)	(0.117)	(0.120)	(0.113)	(0.127)	(0.135)	(0.114)	(0.116)	(0.107)
52-60× High ΔHHI	0.226*	0.366***	0.240*	0.358***	0.114	0.520***	0.325**	0.350**	0.353**
	(0.129)	(0.137)	(0.132)	(0.135)	(0.120)	(0.151)	(0.144)	(0.147)	(0.135)
{52-60}-{48-51}	0.579***	0.463***	0.527***	0.431***	0.830***	0.137	0.369***	0.416***	0.288**
	(0.111)	(0.113)	(0.111)	(0.113)	(0.120)	(0.106)	(0.126)	(0.127)	(0.120)
Tech FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Classes	135	135	135	135	134	135	133	135	134
Dep. var. mean	4.047	3.913	2.950	2.822	3.678	2.861	3.923	3.913	3.918
Adj. R-Square	0.794	0.788	0.821	0.821	0.759	0.730	0.786	0.788	0.788
Observations	4223	4192	4243	4210	4146	3626	4178	4192	4184

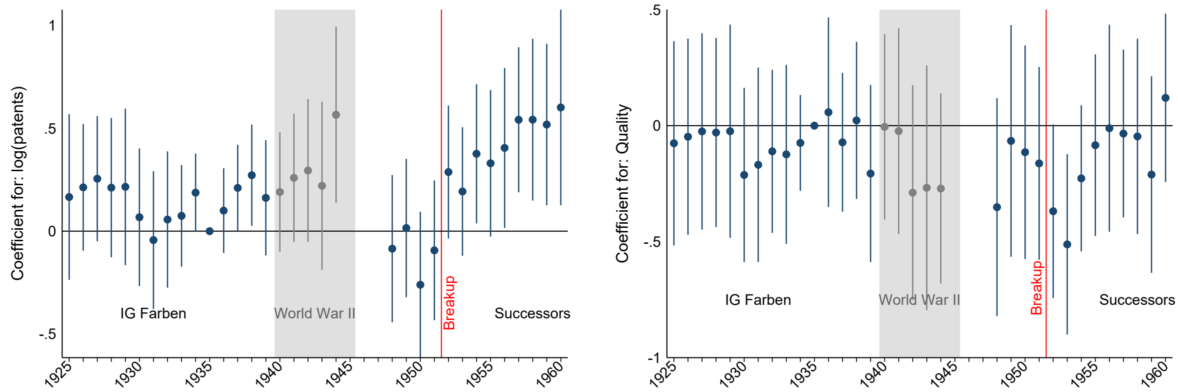
Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ Standard errors clustered on the technology class level in parentheses. ΔHHI is the difference between technology-level concentration, considering IG Farben as one block or as broken up according to the 1952 successors. High ΔHHI refers to the concentration change in the top 25% of the distribution, threshold 160. The DiD coefficients in turn compare patent counts in 1948-1951 and 1952-1960 with the pre-war period. The main effect is the difference between these two coefficients, tabulated in row {52-60}-{48-51}. The dependent variables are quality-weighted patent counts, except columns (3) and (4) with simple patent counts. Quality weights are normalized to mean three, standard deviation one. The columns restrict patents by applicants, either all (columns 1, 3) or applicants unconnected to IG Farben (columns 2, 4, 7-9). Columns 5-6 restrict patents by location, where inventor location is preferred if available. Domestic patents refer to patents with a German location, foreign patents to patents with a foreign location. The number of observations differs if for some technology-year cells, no non-zero patent counts are available. For estimation with continuous breakup exposure variable, see Table 4.5. In the appendix, Poisson regression results are available in Table D.6, estimates with control variables in Table D.8.

Decomposition: Quantity and Quality The breakup could have increased the propensity to patent by the IG Farben successors as well as their competitors. After the breakup, the successors could no longer access each other's patents and research results. With this, the value of possessing patents increased. An increased propensity to patent by some market participants could spill over to others as they are now also confronted with the increased need to claim their stakes.

With an increased propensity to patent, differential effects across quality and quantity are possible. Raw patent counts, as well as average yearly patent quality, allow further investigation. Figure 4.8 presents the results of DiD regressions for both raw counts, Panel 4.8a, and for average quality, Panel 4.8b. The very sharp and fast increase in the raw patent count after 1952, together with the drop in the average patent quality, suggests that an initial quantity-quality trade-off is in play. The sudden increase in patents is unlikely to reflect an increase in innovation but instead rather points to a change in the propensity to patent.[13] Adjusting for quality attenuates the initial increase, and the overall results are consistent between raw and quality-adjusted patent counts.

Figure 4.8: Technology class-level DiD regressions

(a) Patent count: All patents (b) Average quality: All patents



Notes: 4.8a shows OLS regressions of log patent counts in technology classes with and without pre-war IG Farben exposure. 4.8b corresponding regressions for average patent quality within classes. The German patent office closed in 1945-1947.

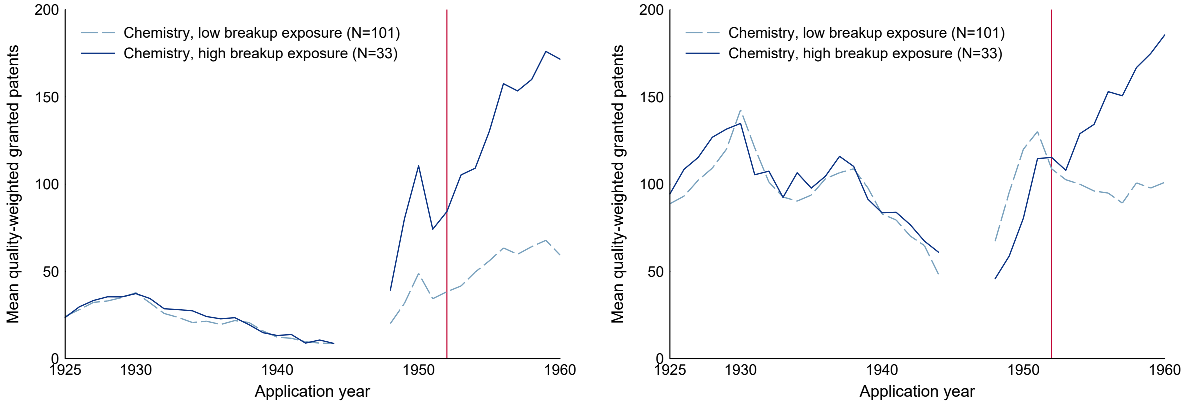
The analysis of inventor counts yields similar results. The number of inventors listed on a patent is a more classic but also an imprecise measure of investment in a particular project. Despite costing more, larger teams have been shown to yield better results in both scientific and technological endeavors (Wuchty, Jones, and Uzzi, 2007). The number of distinct inventors active in a technology class as well as the average number of inventors listed on patents in a technology class are the corresponding dependent variables. Figure D.15 in the appendix reports results. The number of unique inventors in IG-exposed classes follows a similar pattern as the patent count. Increases are driven by new inventors, rather than by established ones. The average number of inventors per patent has no initial jump in 1952, but a slight positive long-run tendency. This evidence also points towards short-run increases in the propensity to patent and long-run increases in innovation effort.

[13] Alternatively, it is possible that strategic delay plays a role here. Firms might hold back patent applications in the 1948-1951 time frame because of uncertainties over IG Farben's future. If the firms expected that some extent of compulsory licensing would be imposed between the successors, such behavior would be rational. However, this observation is not consistent with the large and immediate drop in quality, as incentives to delay important patents are larger. Further, there is no large spike in patenting by the IG Farben successors themselves, see Figure 4.4b.

Decomposition: Domestic and Foreign When thinking about the breakup of a leading company, policymakers worry about foreign entry. Possibly it is better to retain a national champion if it prevents foreign competition, even at the expense of welfare and innovation. In the context of the IG Farben breakup, this is a distinct possibility. With the end of the Second World War, the integration of Germany into the Western alliance system started. However, it is unclear whether the IG Farben breakup further facilitated this process.

Figure 4.9: Technology class-level descriptives of foreign and domestic patenting.

(a) Foreign patents (b) Domestic patents



Notes: Shows quality-weighted counts of granted patents, averaged over technology classes with and without pre-war IG Farben exposure. Patent location is defined by inventor location if available, applicant location otherwise. 4.9a focuses on foreign, 4.9b on domestic patents. The German patent office closed in 1945-1947, so that no data is available for these years.

Immediately following the war, the number of patents by foreign applicants and inventors jumps up distinctly, following a long-run decline before and during the war. Figure 4.9a shows that technology classes exposed to the IG Farben shock experience a specifically large increase. However, the increase happens immediately, and the timing seems unrelated to the IG Farben shock. Subsequently, the number of foreign patents increases both for patents exposed to the shock and unaffected patents. While the absolute increases differ in Figure 4.9a, Table 4.4 shows that in percentage (log) terms, the increase is similar. There is a strongly positive and statistically significant increases in patent counts in both 1948-1951 and 1952-1960 relative to the pre-war period, but the difference between both in percentage terms is small.

Domestic patents show a different development, with much larger relative increases for technologies exposed to the IG Farben shock. Immediately after the shock, levels in the comparison group increase relative to the breakup exposure group, so that the 1948-1951 coefficient is strongly negative in Table 4.4. Visual inspection in Figure 4.9b shows that this is not due to differential trends but differential levels. After 1952, the trends diverge, with the comparison group staying constant and the breakup exposure group strongly increasing. As a consequence, the difference between the early and late DiD coefficient is very large in Table 4.4.

Summarizing, foreign patenting plays an important role after the Second World War. In the context of the IG Farben shock, it plays a smaller role than domestic patents.

Table 4.5: Effects in Technology class-level DiD regression (continuous)

	(1)	(2)	(3)	(4)	(5)	(6)
	Exposure: ΔHHI 1925-1939					
log(Patents)	All (Quality)	Non-IG (Quality)	All (Count)	Non-IG (Count)	All (Quality)	Non-IG (Quality)
48-51× log ΔHHI	–0.035*	0.011	–0.019	0.020		
	(0.021)	(0.019)	(0.020)	(0.018)		
52-60× log ΔHHI	0.052**	0.075***	0.057***	0.078***		
	(0.020)	(0.020)	(0.020)	(0.020)		
48-51× log ΔHHI Adj					–0.038***	–0.010
					(0.014)	(0.013)
52-60× log ΔHHI Adj					0.028*	0.043***
					(0.015)	(0.015)
{52-60}-{48-51}	0.087***	0.064***	0.077***	0.057***	0.066***	0.052***
	(0.023)	(0.023)	(0.022)	(0.022)	(0.015)	(0.015)
Tech FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Classes	104	104	104	104	135	135
Dep. var. mean	4.235	4.071	3.143	2.987	4.047	3.913
Adj. R-Square	0.802	0.801	0.821	0.827	0.794	0.789
Observations	3350	3320	3358	3326	4223	4192

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ Standard errors clustered on the technology class level in parentheses. ΔHHI is the difference between technology-level concentration, considering IG Farben as one block or as broken up according to the 1952 successors. log ΔHHI Adj replaces the $\log(y)$ with the observed minimum where ΔHHI = 0. ΔHHI is strongly right-skewed, but its logarithm is not. The DiD coefficients in turn compare patent counts in 1948-1951 and 1952-1960 with the pre-war period. The main effect is the difference between these two coefficients, tabulated in row {52-60}-{48-51}. Columns 1, 3 and 5 count all patents, columns 2, 4 and 6 only non-IG patents. In columns 3-4, raw patent counts are used, all other columns use quality-adjusted counts. The number of observations differs if for some technology-year cells, no non-zero patent counts are available.

Continuous Breakup Exposure Table 4.5 uses a continuous breakup exposure variable instead of the binarized version. ΔHHI is highly right-skewed, but in log terms more closely resembles a normal distribution, so columns 1-4 use this transformation. Some technology classes have ΔHHI = 0, so that they drop out. Columns 5 and 6 replace these values with the smallest actually observed $log(\Delta\text{HHI})$ and results remain qualitatively unchanged, albeit smaller in magnitude. The results are similar to the binarized version. Counting all patents, strongly exposed technologies first have fewer patents relative to the control group and the pre-war time period, but this reverses after the breakup. The magnitude of the difference arrives at an elasticity of 0.07-0.09. Focusing on patents by applicants not associated with the IG Farben, there is no initial drop, and the difference coefficient has a magnitude of 0.05-0.06.

Alternative Exposure Variables While intuitively appealing, ΔHHI does not directly conform to the identification justification of idiosyncratic breakup of IG Farben along the occupation zones. ΔHHI used in the previous analysis has the advantage of its close relationship to the previous literature in industrial organization. On the other hand, ΔHHI strongly depends on the share of IG Farben in a particular technology and only some part of the variation is driven by the breakup along the occupation zones. An ideal comparison would be between technologies with similar involvement of IG Farben but variation in breakup intensity driven by geographic structure.

Two alternative breakup measures focus on variation within IG Farben. First, $\Delta \text{HHI}_{\text{Within}}$ considers only patents associated with IG Farben and its subsidiaries for the calculation of the HHI. Second, $\Delta \text{HHI}_{\text{Occ}}$ disregards the subsidiary structure and considers only IG Farben's geographical structure across occupation zones for the calculation of HHI. Both approaches remove the amount of IG Farben investment in a particular technology from the analysis. Section 4.5.1 explains details of the calculation. Both exposure measures are standardized to mean zero and standard deviation one.[14]

Table 4.6 uses a continuous breakup exposure exposure variable instead of the binarized version. Only technology classes with non-zero IG Farben share enter the regression. A concentration decrease by one standard deviation increases patenting by between 15-20% on average over the 1952-1960 period, relative to 1948-1951. As before, the dynamic effect shows a gradual increase with little pre-war trend, see Figure D.14 in the appendix.

Robustness Checks Various robustness checks to the innovation analysis are collected and discussed in section 4.8. Among them is a different estimation strategy focusing on a firm panel (Appendix D.3) and control variables for various effects of war and postwar policy. However, the robustness checks apply similarly the market structure and price analysis in the following paragraphs. Also, collecting them in a separate section allows for the required deep discussion of the historical context and measurement approaches. More immediately, estimates of the main technology class-level in a Poisson regression are in Figure D.12 and Table D.6 in the appendix.

4.7.2 Effects of the Breakup in Product Space

Next to the analysis of innovation effects, an auxiliary analysis of outcomes in product space is helpful for three reasons. First, the antecedent of innovation effects are changes in product space, so that the analysis in innovation space alone would be incomplete. Second, in product space the level of analysis is closer to a definition of relevant markets. Finally, the product-level analysis informs about potential trade-offs to the positive innovation effect. As section 4.5 has shown, the IG Farben breakup resulted in product-level competition between IG Farben successors. The reactions of other competitors might strengthen or counteract this tendency. Increased competition could crowd out other competitors, creating a trade-off to the innovation effect. On the other hand, the breakup could have reduced barriers to entry erected by IG Farben, leading to more entry. Finally, price effects of the breakup could counteract or exacerbate the innovation effect.

Market Structure The IG Farben breakup led to large changes in individual product markets. The analysis in section 4.5.2 has focused on the IG Farben portfolio itself and found that the breakup introduced horizontal product-level competition. This section formalizes and extends the analysis to a measure of overall competition in the market, measured by the number of active firms.

[14] In contrast to the previously used ΔHHI, the alternative exposure measures are substantially less skewed. In fact, the log transformation increases the skewness, so that it is not applied.

Table 4.6: Effects in Technology class-level DiD regression (Alternative exposure)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Exposure: ΔHHI 1925-1939 - Alternative measurement							
log(Patents)	All (Quality)	All (Quality)	Non-IG (Quality)	Non-IG (Quality)	All (Quality)	All (Quality)	Non-IG (Quality)	Non-IG (Quality)
48-51$\times$ Std ΔHHI_{Within}	−0.092	−0.094	−0.034	−0.046				
	(0.057)	(0.058)	(0.056)	(0.055)				
52-60$\times$ Std ΔHHI_{Within}	0.112*	0.103	0.131**	0.120*				
	(0.064)	(0.068)	(0.066)	(0.070)				
48-51$\times$ Std ΔHHI_{Occ}					−0.047	−0.047	−0.001	−0.016
					(0.066)	(0.069)	(0.064)	(0.064)
52-60$\times$ Std ΔHHI_{Occ}					0.134**	0.132**	0.170***	0.159**
					(0.059)	(0.059)	(0.062)	(0.061)
{52-60}-{48-51}	0.203***	0.197***	0.165***	0.166***	0.182***	0.179***	0.171**	0.175***
	(0.060)	(0.062)	(0.060)	(0.061)	(0.069)	(0.067)	(0.067)	(0.066)
Tech FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls		Yes		Yes		Yes		Yes
Classes	112	112	112	112	114	114	114	114
Dep. var. mean	4.214	4.214	4.059	4.059	4.192	4.192	4.040	4.040
Adj. R-Square	0.791	0.793	0.790	0.792	0.795	0.796	0.793	0.795
Observations	3628	3628	3601	3601	3675	3675	3645	3645

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ Standard errors clustered on the technology class level in parentheses. ΔHHI_{Within} is the difference between technology-level concentration among IG Farben-related patents, considering IG Farben as one block or as broken up according to the 1952 successors. ΔHHI_{Occ} breaks up the IG Farben block by occupation zones, ignoring subsidiary structures. In both cases, patents from the Soviet occupation zone are ignored, see section 4.5.1 for details. Both ΔHHI are standardized to mean zero and standard deviation one. The DiD coefficients in turn compare patent counts in 1948-1951 and 1952-1960 with the pre-war period. The main effect is the difference between these two coefficients, tabulated in row {52-60}-{48-51}. Columns 1-2 and 5-6 count all patents, columns 3-4 and 7-8 only non-IG patents. Controls include the share of non-IG firms targeted for dismantling, the share of patents located in East Germany or Berlin and war destruction, proxied by the share of destroyed flats in the city of patent inventor or applicant. For details see section 4.8 and Table D.8. For dynamic estimates, see Figure D.14.

In considering product-level effects of the IG Farben breakup, market structure itself is the first relevant outcome. The recent literature in industrial organization has predominantly discussed endogenous product variety within markets for differentiated products (Crawford, 2012, for a survey). Within such markets, business stealing externalities between firms and self-cannibalization are relevant for the number of products relative to the social optimum (Fan and Yang, 2020). For homogeneous products, as discussed here, such considerations do not play a role. For firms, product variety is exclusively about the number of markets they are active in. In principle, increased price pressure from the breakup should make production unprofitable for marginal producers and reduce the number of firms in the market. However, market concentration can serve as a barrier to entry. Licensing of necessary underlying technologies might become easier for newcomers if instead of one dominating incumbent, multiple potential licensors are available. Previous literature has also argued that in intermediate good markets, incumbents can erect entry barriers by contracting with downstream customers (Aghion and Bolton, 1987). In fact, IG Farben did pursue arrangements with other chemical companies about limitations of production portfolios (Haber, 1971, p. 288).

With a panel of chemical substances covering the years 1939-1952-1961, it is possible to investigate market structure effects in terms of the overall number of suppliers per product. Chemical substance lists in historical product catalogs were created and consolidated by domain experts. Synonyms and closely related products are linked to each other and clustered. As such, the market definition is relevant, but narrow.[15] Changes between 1939 and 1952 cannot be attributed to the breakup, as it is hard to isolate the effect of other events and a pre-breakup measurement is unavailable. However, the number of suppliers stays similar in most specifications. Between 1952 and 1961 the number of suppliers increases, driven by non-IG firms, suggesting additional entry. This is particularly pronounced for products where more than one IG Farben successor was active and the shock had increased competition. This is suggestive evidence that an initial increase in competition can induce further entry.

Table 4.7 regresses the number of firms on exposure to the IG Farben shock measured in 1939 or 1952. The regression is based on a panel of product occurring in the price lists and for which data from 1939, 1952 and 1961 is available. These restrictions focus on products already well-known and relevant in the chemical industry in 1939. Standard errors are clustered at the product level (Bertrand, Duflo, and Mullainathan, 2004). Unit fixed effects are at the same level. Time fixed effects are at the product catalog level. In pairs of columns, the first one only controls for product and year fixed effects, while the second introduces a set of controls for events between 1939 and 1952. The overall number of firms increases for products with IG Farben exposure. Especially when measuring exposure in 1952, the increase is focused - necessarily - on areas with multiple IG Farben successors. However, the coefficient for 1961 further increases, suggesting crowding in of additional suppliers for products where the IG Farben breakup created competition. Focusing on non-IG firms in the right half of the table, it becomes clear that IG Farben successors themselves drove a large part of the increases in the overall number of firms in 1952. The additional increase in 1961, however, relies more heavily on additional entry.[16]

[15]In principle, the market definition could be enhanced by incorporating relationships of substitutability between individual substances. Such data could is available from databases of modern chemistry. The drawback that the level of knowledge has significantly advanced since the period of study could be circumvented by focusing on input-output relationships described in the actual patent documents. This remains for future research.

[16]Similar results can be obtained when leaving out the 1939 data, so that only the 1952-1961 difference is analyzed. In this result, trading firms play a role. Trading companies, according to the product catalogs, are to some extent representatives of foreign companies selling products in Germany. They are only listed in the 1939 and 1961 book, so that their exclusion in Table

Table 4.7: Number of suppliers by product, as a result of IG Farben exposure

	Number of firms				Number of non-IG firms			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$IG_{1939} \geq 1 \times 1952$	−0.531*	0.364			−0.436	0.403		
	(0.295)	(0.320)			(0.277)	(0.282)		
$IG_{1939} \geq 1 \times 1961$	1.166*	1.701**			1.245**	1.699***		
	(0.603)	(0.667)			(0.564)	(0.629)		
$IG_{1952} = 1 \times 1952$			0.369	0.704***			−0.115	0.165
			(0.297)	(0.251)			(0.288)	(0.248)
$IG_{1952} = 1 \times 1961$			1.141*	1.167**			0.885	0.860*
			(0.597)	(0.518)			(0.567)	(0.499)
$IG_{1952} \geq 2 \times 1952$			1.403***	2.845***			−0.469	0.867**
			(0.509)	(0.423)			(0.485)	(0.369)
$IG_{1952} \geq 2 \times 1961$			5.950***	5.629***			4.678***	4.302***
			(1.072)	(1.057)			(1.030)	(1.026)
Product, Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Type × Year FE		Yes		Yes		Yes		Yes
Controls		Yes		Yes		Yes		Yes
Products	566	566	566	566	566	566	566	566
Adj. R^2	0.528	0.618	0.560	0.640	0.505	0.607	0.534	0.622
Observations	1698	1698	1698	1698	1698	1698	1698	1698

Notes: Considers only products with data from 1939, 1952 and 1961 where at least one price information is available. IG_{1939} is the count of firms associated with IG Farben offering the product in 1939, pre-war and pre-breakup. IG_{1952} is the number of IG Farben successors offering the product in 1952, immediately after the breakup. The number of firms is the number of suppliers of the product according to the product catalog of the respective year, winsorized at the 99% level. In columns 5-8, IG firms or successors are excluded from the count. Control variables include the count of firms headquartered in East Germany or Berlin in 1939, the count of cartels in 1939 and the count of firms slated for dismantlement in 1939, each interacted with year dummies. Table D.16 contains estimates for control variables. See also the discussion in section 4.8. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Price Effects Innovation effects are key measures for dynamic consequences of the IG Farben breakup, but next to the long-run considerations, short-run effects may play a role as well. Price changes directly influence consumer welfare, possibly occur in a shorter time-frame. As the product analysis is limited to homogeneous goods markets, an increase in competition should lead to a decline in prices. Case-study evidence from merger retrospectives would suggest as much (Ashenfelter, Hosken, and Weinberg, 2013, p. 240). For some products, IG Farben and its subsidiaries maintained production capacity in multiple locations, eventually assigned to multiple successors. Therefore, the breakup will effectively induce competition only in some product markets, while firms in other product markets at most face potential competition. Short-run price effects are likely concentrated in such markets with effective competition, i.e. multiple IG Farben successors.

Confirming this hypothesis strengthens the understanding of the setting but also offers additional benefits. Unlike for patents, where the patent application measures research activity with temporal noise, the timing of prices is clear. Prices are valid for the exact point when they are posted so that higher-frequency

D.17 is a mixture of robustness check and heterogeneity analysis. Results stay qualitatively similar, but with smaller magnitudes. Entry by foreign suppliers seem to play some role, but does not explain the full effect.

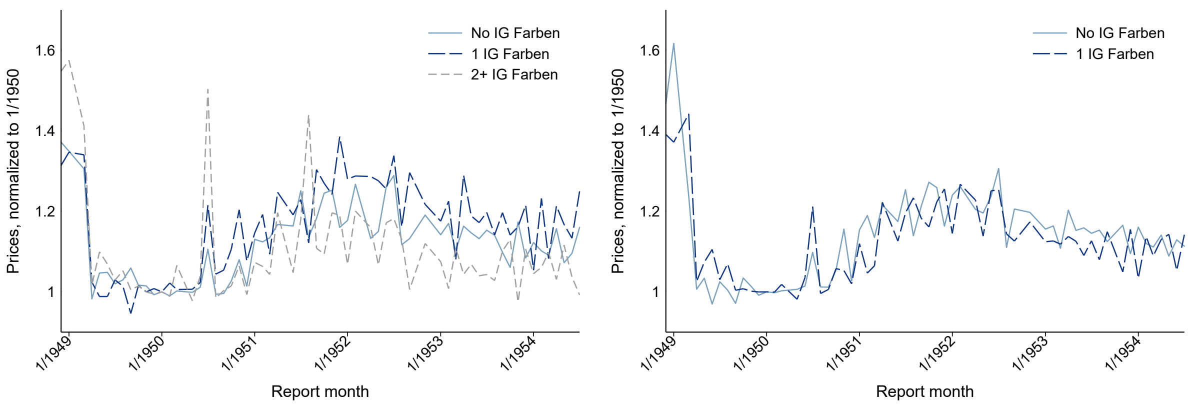
analysis is feasible. With pre-breakup price data, analysis a before/after analysis in the product domain is feasible. Finally, price data allows for additional robustness checks. Here, it is possible to test whether the changes in tariff levels in 1951 had a confounding effect.

The analysis of the pricing effect relies on factory prices for chemical substances from industry journals. Price reports start in 1948 after price controls are relaxed. They list products on a rolling basis, so that in a typical month 30-40% of all prices are reported. For the price effect analysis, only products with prices both before Q2/1950 and after Q2/1952 are considered. The product prices are linked to supplier lists that reveal which IG Farben members (1939) and how many successors (1952) offer the product in the market. For details on data construction, see section 4.4 and Appendix D.1.1.

Figure 4.10 displays average prices by IG supplier status over time. Products not sold by any IG Farben successor (as measured in 1952) behave similarly to products where only one IG Farben successor was present. If at all, price trajectories are above the 'no IG' group. However, products where the IG Farben breakup created product-level competition are on a different trajectory. While prices overall increase substantially in 1951, they do not participate in that price increase to the same extent. The IG Farben status as measured in 1939 is a mixture of the two groups identifiable in 1952, so that their price trajectories are only marginally below products without IG Farben involvement. Visually, the start of divergence of the price trajectories seems to occur earlier than the breakup finalization (early 1952). Instead, it coincides with the breakup announcement and the enactment of its legal basis in August 1950.[17] In subsequent regressions, the first year of the post-period is accordingly set to the third quarter of 1950.

Figure 4.10: Raw price data descriptives

(a) Prices by 1952 IG Farben status (b) Prices by 1939 IG Farben status



Notes: Average prices reported in a month. Prices are normalized to the closest data relative to January 1950. In panel 4.10b, 10/1953 is removed. There, a very short price list leads to a very large outlier. Products are grouped by the occurrence of IG Farben (or subsidiaries and successors) as suppliers in 1939 or 1952. In 1939, it is not possible to distinguish which eventual successors produce a product.

Table 4.8 formally reports regression coefficients on the price data. Standard errors in the regressions are clustered at the product level (Bertrand, Duflo, and Mullainathan, 2004). When a product has multiple

[17]Law 35 of the Allied High Commission ("Disperson of Assets of I.G. Farbenindustrie A.G."), dated 17th of August 1950. The executive order finalizing the process is dated 17th of May 1952. The legal documents are retrievable at http://deposit.d-nb.de/online/vdr/rechtsq.htm.

Table 4.8: DiD estimates for price effects

log(price)	(1) All	(2) $> 1949Q1$	(3) $> 1949Q1$	(4) All	(5) $> 1949Q1$	(6) $> 1949Q1$
Post × IG_{1939} = 1	–0.015	–0.026	–0.040			
	(0.026)	(0.025)	(0.025)			
Post × IG_{1952} = 1				0.069**	0.068**	0.065**
				(0.030)	(0.028)	(0.031)
Post × IG_{1952} ≥ 2				–0.069***	–0.050**	–0.057**
				(0.025)	(0.024)	(0.025)
Product, Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Type × Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Cartel × Month FE			Yes			Yes
Δ Tariff × Month FE			Yes			Yes
N Time series	464	464	401	516	516	443
N Chemicals	363	363	308	400	400	336
Within R-Square	0.000	0.001	0.007	0.012	0.009	0.013
Observations	8129	7953	6878	9030	8854	7593

Notes: Shows difference in difference estimates for a assumed event time in 1950Q3. Columns 1-3 show effects based on the 1939 structure of IG Farben. Columns 4-6 differentiate products by whether there was one or more than one IG Farben successor active. The baseline is always the group of products with no IG Farben involvement. Chemical types are organic, inorganic, metals, pharmaceuticals and plastics. Products with involvement of at least one sales cartel in 1939 are considered as cartelized ("Cartel"). Changes between the previous special tariff and the subsequent ad valorem tariff after the 1951 tariff adjustment are the Δ Tariff control variable. The difference is winsorized at the 1% and 99% level. Both tariffs are calculated as percentages and Δ Tariff is the difference. When information about quality grades (e.g. 'pure') is available, multiple time series per product can exist. Standard errors clustered on the product level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

time series due to quality grades, clustering remains at the higher product level. Unit fixed effects at the product grade level are present in all regressions. Time fixed effects are at the month level (prices). Columns 1-3 show price tendencies using pre-war information on IG Farben portfolios as well as post-war information about the general IG Farben portfolio. Here, negative coefficient estimates are statistically indistinguishable from zero. Columns 4-6 show that coefficients focusing on areas with ex-post IG Farben competition are negative and statistically significant. Prices for products with ex-post IG Farben competition fall relative to prices of other products.

The price analysis enables several robustness checks, namely the role of sales cartels, regulations concerning specific product groups and tariff changes. Section 4.8 discusses the role of sales cartels. These were widespread and officially supported in pre-war Germany but dissolved in the early post-war period. Table D.11 shows that accounting for cartels listed in the 1939 supplier lists does not qualitatively change results. Further, Allied economic policies and restrictions may have interfered with prices. Table D.12 shows that product type × month fixed effects or excluding the specifically impacted group of plastics does not change the results. Finally, with product-level tariff data, the effects of Germany's 1951 GATT accession can be estimated. Results in Table D.13 remain unchanged.

With information about the nature of the chemical substances, detailed comparisons of the groups are feasible. Tables D.9 and D.10 in the appendix reports descriptive statistics between products produced by IG Farben and such not produced by IG Farben. The respective section discusses the differences in detail. In general, IG Farben products were simpler by molar mass and heaviest element and, per kg, cheaper.

IG Farben products were much more likely in the areas of pharmaceuticals and plastics and less likely in inorganic chemistry. This is in line with historical narratives, suggesting that other chemical firms were especially active in specialty chemicals. Yet, the only area of chemistry that IG Farben dominated completely were plastics.

Propensity score matching takes observable differences between chemical substances into account. Table D.14 reports estimates from DiD analysis following the matching. The matching incorporates detailed chemical characteristics, pre-breakup price levels, as well as the competitive situation. Estimates confirm the results from the analysis without matching and tend to show even larger negative effects.

4.8 Robustness to Historical Factors

This section discusses important historical factors and parallel events surrounding the IG Farben breakup. As the breakup happens during one of the most turbulent episodes of German history, the core question is whether the end of the Second World War set off a complete renewal ("Hour Zero") or was rather characterized by continuity. This question was subject of intense debate in post-war German society. Both for society and for the economy, historians emphasize continuity and reject notions of a radical divergence (e.g. Morsey, 2010).

When analyzing the effects of the war, the three main themes are the direct impact of the war, such as bombing, Allied occupation policies and the separation of the Soviet occupation zone as well as the German postwar policy and recovery. Insofar as these effects impact both IG Farben-related areas of chemistry as well as unrelated areas, they are a part of the parallel trends assumption justifying the difference in difference analysis. While in general untestable, in some cases, it is possible to appraise their effect by constructing appropriate control variables. Most control variables can be introduced directly in the regressions on technology class or product level. Oster (2019) bounds allow an explicit assessment of biases by unobservable confounders. For an additional robustness check of the central innovation result, a firm-level panel offers a different view and yields similar results. Its construction and results are discussed in Appendix D.3.

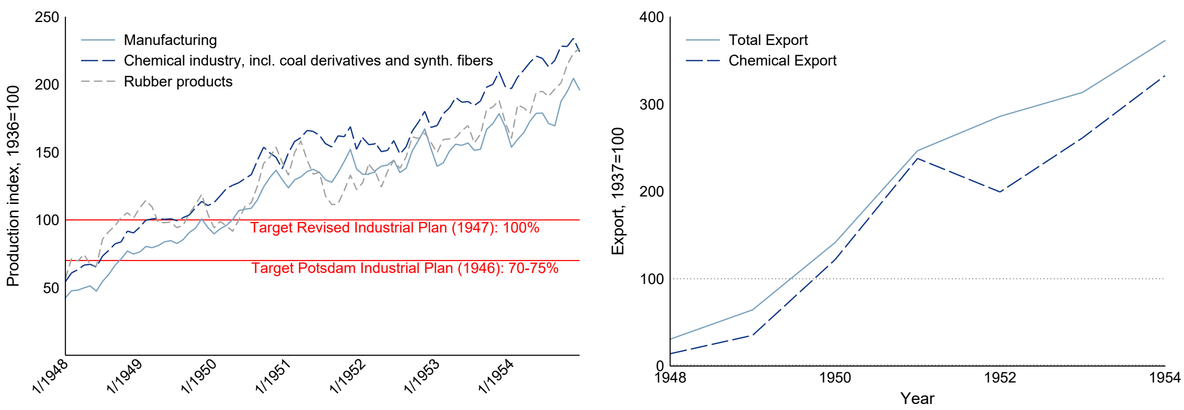
War Damages The war damages to German cities were extensive, but according to the historical literature, the effect on the German industry was smaller than often thought. For example, the US Strategic Bombing Survey conducted after the war concluded that of Germany's war industry, at most 20% had been destroyed (Jeffreys, 2010, p. 295). Overall, the German economy recovered quickly and could return to pre-war levels of export by 1950 (Figure 4.11b). Due to its central role for war-related industries such as synthetic fuels and explosives, IG Farben facilities were likely the primary targets of Allied air campaigns. As an example, the Leverkusen plant was hit by 14 aerial attacks since 1944.[18] Yet, the machines were left rather intact, with only 15% of the factory beyond repair (Jeffreys, 2010, p. 295). To the extent that IG Farben facilities were specifically targeted and destroyed, the damages could result

[18] The Leverkusen example represents a middle ground for the IG story. Of the West German plants, the BASF facilities were hardest hit by the war, while the Hoechst facilities were spared. On the other hand, the Hoechst facilities suffered from underinvestment. The strongest attacks against IG plants targeted the East German synthetic fuel plants at Leuna, which were completely destroyed.

in negative effects on innovation by IG Farben successors, i.e. in smaller estimates.[19] Systematic data on the war-time destruction of companies is not available. However, an indirect proxy is the devastation of the city's housing stock. Robustness checks based on Kästner (1949) and Hohn (1991) match patents to the closest city within 10 km and assign the destruction ratio of that city. Table 4.3 shows that the destruction ratio does not vary between technology classes differentially exposed to the IG Farben shock. In technology-level (Tables D.8), firm-level (Table D.19) and product-level regressions (Table 4.7), the inclusion of war destruction as a control variable does not alter results.

Figure 4.11: Zero Hour: Germany's economy

(a) Production recovery (b) Trade recovery



Notes: 4.11a) Monthly German production index with reference level 1936. 4.11b) Yearly German chemical and total exports with reference level 1937. Source: Statistical yearbooks for West Germany.

Allied Economic Policies and German Recovery In the initial period after the war, with the economy in disarray and the population's basic needs unmet, the Allies assumed direct control over the the economy. With this initially came a set of production restrictions. These were especially targeted towards the dismantlement of all war-related capacity, discussed in detail below, and the restriction of strategic goods. Table D.20 in the appendix gives a detailed account of the relevant products, industries and the development of the regulations over time. According to the 1946 Potsdam Industrial Plan, the German economy was to be limited to 70-75% of the pre-war 1936 level. The ceilings were never reached before the 1947 Revised Industrial Plan increased figures to 100% of the 1936 level. By mid-1950, also this restriction was lifted. After mid-1950, restrictions were still placed on war-related chemicals and some parts of the plastics value chain. These were only relaxed in 1951.

For the empirical strategy, relaxations in the 1950s are of the largest concern. If these relaxations would differentially affect production areas with IG Farben activity, they constitute parallel events of concern. In particular of concern is the plastics industry, where relaxation only occurred by 1951. Robustness checks thus repeat analysis while disregarding technology areas (Table D.8) or products (Table D.12) relevant for the plastics industry. The consistency of results is also reassuring given the dominance of IG Farben in these fields. The removal of restrictions towards the civilian industry by the Petersberg Industrial

[19] Yet, Waldinger (2016) exploits bombing damage to universities and does not find long-run effects on research output. Likewise, Baruffaldi and Gaessler (2021) find that the loss of research infrastructure has little effect on research output over ten years. Renewal of obsolete infrastructure might even have a positive effect.

Plan, effective in late 1950, are unlikely to have large confounding effects. First, the restrictions did not regulate individual products within broad class of chemicals. Fixed effect controls for the broad chemical class are available. Second, the removal of restrictions did not lead to an immediate, marked increase in production. Figure 4.11a shows the output of the German manufacturing and chemical industry relative to the pre-war level. The chemical industry does not show a strong output increase in mid-1950, indicating that the policy was either not binding or that not much additional capacity was available. This is consistent with the historical literature (Morsey, 2010, p. 5).

The brisk German economic recovery and the economic boom starting in the early 1950s could themselves possibly confound the IG Farben shock. In fact, the number of granted patents in technology classes not exposed to the breakup does stay constant, both within and outside of chemistry, see Figure D.11. On the level of technologies, it is difficult to think of a comparison that alleviates these concerns. If the economic shocks driving the recovery were global (e.g. the Korean war), they would similarly affect counts of, e.g., US patents. Even if they were specific to technologies in Germany, affected German companies' patenting activities would likely spill over to other patent systems as well. On a more practical side, matching technologies or patents across patent systems in historical time comes with considerable difficulties, see the online appendix of Baten, Bianchi, and Moser (2017). A better argument is that in product-level regressions, effects can be traced particularly to such products where multiple IG Farben successors were active. It is unlikely that macroeconomic shocks driving recovery and boom exactly correlate with the micro-structure of the IG Farben successors' product portfolios.

Dismantlement of Factories After the war, the Allies sought to limit Germany's war potential, but also to recuperate some of their own the economic losses. The extent and impact of these policies can be captured using data given by Harmssen (1951, pp. 98–126). Harmssen prints the official dismantlement targets for the Western Zones as of 1947 and reported dismantlements for the Soviet zone. There are almost 2000 factory entries, pertaining to some 1700 firms. However, only 100 firms of the chemical industry actually occur in the dismantlement lists, consistent with the over 80% of entries classified as aerospace, defense, machinery or mining. The actual list of dismantled plants is much smaller as the Western allies adjusted the lists. For the Western zones, the list of dismantlement targets starts with 1500 entries and is halved by 1949 (Wallich, 1955, p. 369). Table 4.9 shows that even of the most productive firms in chemistry, only a minority is affected. For a technology class analysis, the share of patents by firms slated for dismantlement can be calculated. Table 4.3 shows that among non-IG Farben firms, around 8% of pre-war patents were applied by targeted firms, balanced between classes by IG shock exposure. Controlling for this variable leaves results unchanged, see Table D.8. The firm-level regressions in Table D.19 show that the patent output of firms exposed to dismantlement permanently suffers, but the estimates for IG Farben exposure remain unchanged.

Next to the effect of dismantlements on the wider chemical industry, the effect on IG Farben is important. IG Farben was a primary target, and all factories were contained in the lists. On a technology class level, this mechanically leads to a strong correlation between breakup exposure and dismantlement share. Studying the issue in more details, it is unlikely that damages to the IG Farben successors through dismantlement drive the effect. Looking more closely at the dismantlement lists, some plants were to be fully disassembled or destroyed. Yet, most of the time, only parts of listed plants were intended for dismantling. For example, IG Farben in Leverkusen was set to lose production facilities for seven types of chemicals, a small subset of its portfolio.[20] In West Germany, whole plants were slated for dismantlement only in the French zone. Abelshauser (2003, pp. 349–350) discusses their history. After much controversy, dismantlements only affected synthetic fuels and plastics, crucial other plants could be saved. If dismantlements had been realized as originally intended, they would have implied considerable damages for the recovery of BASF. In the end, they never affected the supply of in-house production or other industries. While lacking counterfactuals, it is notable that the IG Farben successors could recover quickly to pre-war levels of economic activity, as discussed in 4.5.2.

Table 4.9: Breadth of dismantlement and Eastern Zone exposure in firm population

(%)	Firms with dismantled plants					East HQ	Firms
Book	Any	US	UK	FR	SU	1939	Top 10% (All)
1939	9.2	3.1	2.3	2.7	3.5	38.5	260 (2373)
1952	11.1	3.8	2.8	3.1	3.1	8.3	289 (2734)
1961	5.3	1.3	1.9	1.9	1.3	6.9	318 (3101)

Notes: Dismantlement statistics for top 10% firms by chemical products offered in 1939/1952/1961 respectively. East HQ refers to the later German Democratic Republic but also includes firms in today's Poland and Czech Republic.

The Soviet Sector and German Separation Quickly after Germany's liberation and the division into occupation zones, the Soviet sector started to develop on a diverging path. Here, the harshest reparation policies were introduced. Large parts of the surviving industry were dismantled and brought to the Soviet Union. As in the Western zones, the Soviets took direct control of the IG Farben plants, even before nationalization efforts were begun in earnest.[21] Latest with the currency reforms in East and West, the integration of West and East German industry began to decline. In the supplier lists of 1952, no East German chemical firms are listed. Figure D.16 shows the importance of interzonal (East-West) trade by comparing it with overall trade. The initial importance of interzonal trade is visible, as well as the quick drop in the 1950s and the lack of recovery. Table 4.9 shows that before the Second World War, a large share of chemical companies was headquartered in East Germany or Berlin. Of those, some were able to relocate their operations and are still active in West Germany in 1952. For inventive activity, it is possible to control for the pre-war share of inventive activity taking place in the Soviet sector. This can either occur on the firm or the technology class level. For firms, it is also possible to introduce controls for the headquarter location in the East. Dismantlement targets for the Soviet sector are available from Harmssen (1951).

Robustness checks can account for differential exposure to the Soviet sector. For innovation, analysis on the technology class level and the firm level is feasible. Table 4.3 shows that patents in technologies with and without exposure to the IG were located in East Germany with the same rate. However, the share of patents located in Berlin is higher for IG-exposed technologies, consistent with some IG plants located there. Explicitly controlling for the share, Table D.8 finds estimates unchanged. In analyzing supplier numbers, the share of firms located in East Germany before the war leaves the core results intact, see

[20]Listed were for example a drug against Malaria, some plastics and substances relevant as rocket fuel. All substances listed in the 1952 product listing were still offered by Bayer. See D.21 for the 1947 dismantlement entries related to IG Farben.

[21]For the history of IG Farben in East Germany after 1945, see Stokes (1995).

Table 4.7 and D.16. Firm-level regressions in Appendix D.3 explicitly introduce control variables and show the robustness of the innovation analysis.

Allied Competition Policy Before the war, the German laws regulating cartels were anti-competitive, as considered from today's perspective. Maintaining high prices to strengthen industry was a policy objective. Cartels were allowed, and their general form was regulated by law, to the extent that Germany's cartel court's was largely arbitrating grievances between cartel members. Early during the Allied occupation, in 1947, such cartels were dissolved. However, Germany itself did not introduce competition regulation until 1958 (Murach-Brand, 2004)

As cartels were publicly supported institutions, cartel membership was often public information. The 1939 product catalog details which products are to be procured from cartel organizations directly. Similarly, for listed companies, firm directories also include information about cartel memberships. Combining this information, the effect of the dissolution of cartels can be directly accounted for. In the data, there are 48 cartels supplying 143 products, among which 52 have price information available. Of those, 20 are offered by cartels with IG Farben association. Cartels with IG Farben membership have a particular role in this study. The 1939 data contains 9 such cartels, most prominently the "Stickstoff-Syndikat" (Nitrogen syndicate), which was dominated by the IG Farben group. Products supplied by these cartels are considered to be supplied by IG Farben.

Whether the 1947 dissolution of cartels affected the innovation activities of chemical companies is unclear, but for example Kang (2020) suggests a negative effect. In principle, areas with IG Farben activity (see for example Stokes, 2016, p. 174) and such without were affected, and cartels were frequent throughout the economy. Nevertheless, since IG Farben was the dominant force in its areas of activity, the effect in non-IG areas would likely be stronger. Therefore, if patenting activity in non-IG areas drops more strongly immediately following the war, this could be a reason. Table D.11 shows that controlling for cartels does not strongly influence the results.

Tariff Changes from GATT Accession Effective October 1951, Germany entered the General Agreement on Tariffs and Trade (GATT), the precursor of today's World Trade Organization. Germany's tariff system was thoroughly reformed, and many tariffs changed. To capture the effect, tariff levels based on the pre- and post-reform schedules (Lang, 1939; Bundesministerium der Finanzen, 1951) are matched to individual products. The previous schedule had not undergone major reforms since 1902 and was, with only short interruptions and exceptions, still in effect in 1951 (Jerchow, 1979). The 1902 system imposed a specific tariff that leveraged fixed amounts per unit of imported goods. In 1951, structure, level and type of tariff were changed. Afterwards, a largely ad valorem tariff, leveraging fixed percentages of imported goods' values, was introduced. Knowing the prices per volume, the two systems can be compared. The change increased tariffs, reportedly to - unsuccessfully - gain leeway for GATT negotiations, and put Germany in an intermediate position compared to other European countries (Wallich, 1955, pp. 257–258). In the covered chemical products, tariffs changed from an average of 9.1% to 14.9%. In price regressions, controls for dynamic effects of the tariff changes do not strongly influence the estimates for the IG Farben shock, see Table D.13.

Other Factors Next to the previously discussed factors, others elude measurement attempts. Direct expropriation and exploitation of German intellectual property and tacit knowledge took place during and after the war. German IP in foreign countries was confiscated, and survey groups by the Allies took stock of the technology level of German firms. Scientists - especially in war-related fields such as rocketry and chemical weapons - were recruited (Jacobsen, 2014). Overall, it is difficult to quantify the effect of these policies. Historians who tried to judge their economic impact determined it to be large and significant (Gimbel, 1990). On the other hand, confiscated technical specifications often required additional tacit knowledge (Stokes, 1991, p. 15) or were about to be obsolete due to new technological developments (Murmann and Landau, 2000, p. 61). To the extent that civilian research was concerned, contacts between US and German scientists might have helped to facilitate post-war collaboration. The results of Baten, Bianchi, and Moser (2017) suggest that such policies have a positive effect on subsequent innovation, resulting in a possible upwards bias. Whether such a bias materializes depends on whether the policies more strongly targeted fields with IG Farben activity. Yet, Allied technical survey efforts covered a broad set of targets.[22] Possible confounding effects that come through labor-related channels are beyond the scope of this paper. This is, for one, the loss of life during the war, but also the relocation of East-German inventors (Dorner et al., 2020). On the management level, the loss of experienced personnel due to war crime trials is another possible factor. However, the number of convicted managers is small, and their sentences were short (Jeffreys, 2010). Oster (2019) bounds allow an explicit assessment of biases by unobservable confounders. Table D.7 shows corresponding results.

4.9 Conclusion

This paper studies the effect of the 1952 IG Farben breakup on innovation, market structure and prices. The breakup created competition in technology classes and product areas, driven by the horizontal splits of the different R&D locations of IG Farben. Patent grants in technology classes affected by the breakup strongly increase. Innovation effects incorporate short-run quantity-quality trade-offs and are driven by changes in domestic patenting. Foreign patenting in Germany increases, but the differential increase in technologies with breakup exposure does not explain the overall increase. The breakup facilitated subsequent entry and led to moderate short-run price declines. The latter effects can be traced particularly to products where the breakup created horizontal competition between IG Farben successors.

Naturally, the historical context of the IG Farben breakup is fraught with potential confounding factors. As such, any analysis remains afflicted by limitations. However, it is possible to analyze the historical context for how strong the confounding factors possibly are. The influence of some can be quantified, others can be understood more closely. By studying the timing of the estimates or by limiting to narrow product areas, it is possible to alleviate concerns. Eventually, it is unlikely that a single factor from the historical context can explain the set of observed effects better than the IG Farben breakup itself. For example, it is possible that wartime damages to IG Farben or political action immediately after the war damaged Farben's ability to compete. This might have led to entry or weakened IG's ability to control markets, leading to reduced prices. However, since the observed price drops are exclusively driven by markets where the IG separation actually increased competition, this is unlikely. Robustness analyses in

[22]Gimbel (1990, pp. 64–67) details the cases of chemical companies Merck, Degussa and Linde next to IG Farben and its subsidiary Wacker. Overall, the survey teams worked on a list with 20000 targets, later narrowed to 400.

turn introduces control variables for effects of war destruction, Allied occupation and competition policy, and the Soviet sector.

The results might be lower bound estimates. The IG Farben successors likely did not engage in all-out competition. This might be due to interlinkage of production chains, but also IP rights. Each IG Farben shareholder received stock of every successor. While ownership of IG Farben was widely distributed, this might facilitate considerations related to common ownership. Further, in Germany, commercial banks typically exercise voting rights of shares in their customers' portfolios, leading to interlinked choices. Outright cartelization is also not unfathomable, in fact the IG Farben successors were often indicted by European antitrust authorities (Kovacic, Marshall, and Meurer, 2018).

The historical setting of the IG Farben breakup is very relevant today. The industry structure of the German chemical industry of the early 1950s is not unlike the situation today. Large corporation with strong investments in in-house research continue to drive technological developments. Scale effects are key to success. Mergers such as ChemChina-Syngenta, Dow-DuPont or Bayer-Monsanto have focused attention on competition and innovation. On the other hand, whether similar findings apply for platform industries with their pronounced network effects remains for future research.

The implications for policy remain subtle. The results highlight the importance of competitive markets and strong antitrust policy. On the other hand, the Allied occupation and mandated breakups are credibly one-shot events that are unlikely to foretell future breakups. The German government had agreed to introduce formal competition legislature largely following the US role model. With regards to outright breakups, it had remained conservative and did not plan to extend similar policies to other firms formerly on candidate lists. Consequently, dynamic incentives remain unchanged. Firms would not have to fear further breakups as a consequence of their business success. For an evaluation of the effect of competition on outcomes such as innovation, this is helpful as it removes a channel. Channels of effects are restricted to changed firm size and changed competitive situation, for non-IG firms only the latter. As a policy of breakups would change dynamic considerations, such extrapolations have to remain cautious.

A
Appendix to Chapter 1
Science Quality and the Value of Inventions

A.1 Data

In the following we briefly introduce the scientific literature and patent data. Table A.1 provides details on the structure of the merged dataset. Figure A.2 shows descriptive statistics over time on the samples of patents and SNPL references.

Scientific Literature Data

Scientific literature data comes from 43 million scientific publications, corresponding to all research articles indexed in the Thomson Reuters Web of Science (WoS) database that were published between 1980 and 2016. WoS is the largest bibliographic database of scientific literature and provides all main information for each scientific publication, including authors, affiliations, research field and citations.[1]

Patent Data

The main source of patent data in our study is the database DOCDB, a database maintained and updated on a weekly basis by the European Patent Office (EPO).[2] It includes records from more than 90 patent offices. We base our study on a sample of more than 4.8 million patent families in DOCDB, comprising all patent families with at least one grant publication at the European Patent Office (EPO) or the United States Patent and Trademark Office (USPTO), with first filing date between 1985 and 2012, included. We include references generated during the search and examination phase of patents filed at the EPO, USPTO or the World Intellectual Property Organization (WIPO). Note that at the WIPO, there is no grant procedure and WIPO examinations are typically conducted by the EPO.

DOCDB contains all information digitally available on these patents. An advantage with respect to non-patent literature (NPL) citations data, as compared to other databases, is the availability of enriched xml text comprising separate fields for title, authors, year, journals title, pages, volume and number. This allows matching this information separately with bibliographic scientific literature information, substantially improving the quality of the match (see section A.2).

Whenever we refer to technology field, we use the classification of IPC patent codes in the 34 technology fields provided by WIPO.[3]

Data Transformation

Whenever we use logarithmic transformations on variables with natural zero values (e.g. citation counts), we use a $\log(x + 1)$ transformation. When unifying patent attributes at the patent family level, several decisions have to be taken. For technology fields, we use the modal technology field of member patents. In case of ties, we use the numerically lowest field. When no field classification is available, we drop the patent family. When multiple patent value estimates from Kogan et al. (2017) or PatVal (Giuri et al., 2007) are available, we use the highest one. Some variables with extreme values are winsorized.

[1]More extensive information on the WoS is available at www.webofknowledge.com.
[2]More extensive information on DOCDB is available at www.epo.org/searching-for-patents/data/bulk-data-sets/docdb
[3]WIPO Classification: https://www.wipo.int/edocs/mdocs/classifications/en/ipc_ce_41/ipc_ce_41_5-annex1.pdf

Table A.1: Structure of the dataset

Scientific publications (1980-2012)	Total	Excluding social/humanities	Excluding self-references
Scientific publications	42,962,463	35,874,824	
Scientific publications in SNPL references	2,248,563	2,203,035	2,079,713
Scientific publications in SNPL references (within five years)	1,627,872	1,597,426	1,465,312
Patent families (1985-2012)	**Total**	**EPO**	**USPTO**
Patent family - SNPL reference combinations	6,962,239	1,009,481	6,177,977
Unique SNPL references	2,229,658	575,637	2,017,694
Patent families	4,767,844	1,960,772	4,442,742
Patent families with SNPL references	948,006	488,270	917,179

Notes: Observation counts in the dataset. Discrepancies originate from the different views on the data. The first part of the table also considers SNPL citations from the 1980-1984 range, whereas the second part does not.

Backward reference counts, the number of times a patent references to other patents, are winsorized at the 95th percentile. The same is applied for the number of inventors and SNPL references. Lengths of the first independent claim are winsorized at the 1st and 99th percentile. When assigning scientific fields to scientific publications, in case of multiple fields, we retain the scientific field whose codes are first in the alphabet. We restrict our sample to SNPL citations where the publication year of the scientific article was at or before the first filing year of the patent family.

A.2 Methods

Linking Scientific and Patent Literature Data

"Science" usually refers to the creation and organization of knowledge, often in the form of testable hypotheses and predictions regarding natural phenomena. In a stark simplification, academic scientists (who are mostly employed in the public sector) live in a world governed by the quest for making pioneering contributions to knowledge, hence striving for novelty of insight and for a better understanding of fundamental issues (Merton, 1968; Merton, 1973). According to this view, scientists also follow norms of disclosing newly generated knowledge and information in scientific publications. The societal or private benefit from applications is considered less important, but also hard to assess directly. In principle, the science could thus be decoupled from the economic pursuit of wealth and monetary gain.

Conversely, "technology" refers to the realm of the artificial and to artifacts which may have, or may have not, been constructed with the help of scientific insights. Technology is defined in the OECD Frascati Manual as the collection of techniques, skills, methods, and processes used when producing goods and services. Applications of new insights are largely brought about by engineers (Allen, 1977). Engineers (who mostly work in the private sector) are governed by rules and incentives that are very different from those guiding the behavior of scientists. They seek to contribute new technologies, use secrecy to protect the market positions of their employers and are involved in strategic considerations of market rivalry. Engineers thus turn knowledge into marketable products which then generate monetary returns for owners. This by now classical view of the relationship between science and technology is described, inter alia, by Allen (1977) and Brooks (1994).

Initially understood as two distinct and independent realms, science is now viewed to directly facilitate the application of new knowledge (Stokes, 2011), and that science and technology may follow a process of co-evolution (Murray, 2002). Science has also been described as a kind of map used in the process of devising new technologies (Fleming and Sorenson, 2004). This new view acknowledges that the realms of science and commercial technology development overlap and that their relationship is not necessarily a linear one. While universities mostly generate knowledge, they also file patent applications and license intellectual property. And corporate entities mostly seek to commercialize new products and services, but also engage in basic research not immediately tied to product development and in publication of research results.

SNPL References as a Measure of Knowledge Input

We use non-patent literature references to scientific publications (SNPL) as an indicator of relatedness of a technology, as described in a patent, to scientific contributions, as reported in scientific publications. Numerous studies have proposed patent citations as an indicator of knowledge flows (Jaffe, 1986; Jaffe, 1989). While some authors have raised concerns on the validity of this approach for general patent citations (Thompson and Fox-Kean, 2005; Alcacer and Gittelman, 2006), SNPL references have been consistently found to be more related to actual knowledge flows than other types of references (Roach and Cohen, 2013). In the context of our study, it is not necessary to interpret SNPL references as a direct indicator of knowledge flows: we assume more broadly that a cited scientific paper contains relevant information for the understanding and the development of a technology.

SNPL Matching Methodology

The dataset we adopt to link patents to cited scientific publications is a full match of DOCDB patent data with bibliographic information included in WoS. The matching process is documented in detail in Knaus and Palzenberger (2018). Here we present a brief overview.

The matching consists of three steps, target selection, search and quality control. In the target selection step, cleaning steps are undertaken to exclude NPL strings which are no scientific articles or are outside of the available WoS data. For the remaining entries, a search engine was employed to look up NPL full-text strings in a full-text index of the complete WoS or Scopus content. The search engine returns a ranked list of match candidates. During the quality control stage, the topmost candidate is examined and the match quality is judged according to a field-based scoring. Only high-quality matches are considered valid matches for the final dataset.

The matching procedure is applied on a first set of roughly 37 million NPL references. 27 million (71.8%) entries were selected as a potential target and linked to WoS entries. However, not all of these constitute a valid match after taking the quality of the match into account. The quality of a match is judged by six quality indicators (year, volume, page(s), first author, journal title, article title). Each of these indicators equals one if the information from the matched scientific article can be found in the non-patent literature citation string. The quality score is the sum of the indicators and ranges from zero to six.

To validate the matching quality, random subsamples of 1,000 NPL references each were drawn. An NPL string is considered a valid target if it could be found in the WoS using a manual search. Figure A.1 plots precision and recall, where precision is computed as the share of correct matches out of all matches delivered by the algorithm. Recall is the share of all targets which could be recovered successfully. The graph reveals that when accepting a quality score of three and higher as high-quality matches, precision scores of 0.99 and recall scores of 0.96 (EPO) and 0.92 (USPTO) can be achieved.[4] Table A.2 shows the final quality achieved.

We, therefore, restricted the sample to matches of quality equal to or higher than three. Out of the 27 million references retained as valid targets, 13 million (47.1%) satisfied this quality requirement. Our units of analysis are DOCDB patent families which typically include multiple references.

While the precision and recall scores are high, they only refer to what could have been matched – the content of the Web of Science. Clearly, not all scientific publications that can be referenced in patents are covered in this database. We assess the extent of this issue and consider the subset of NPL references which could not be matched to WoS. We attempt a match to an alternative publication database, Scopus, which has a larger coverage. This exercise generates 113,340 additional SNPL links to 49,254 Scopus items for publication years 1996-2016. Given that this is less than 2% of the total, for simplicity, we disregard these links in our analysis.

Our final sample contains 948,006 DOCDB patent families with at least one grant publication and at least one matched SNPL reference at any of the patent offices considered here.

This compares well with previous datasets, and in general, constitute a larger number of observations than previously identified in existing studies. Ahmadpoor and Jones (2017) use patent data exclusively at the USPTO between 1976 and 2015 where 759,000 patents were found to be directly linked to at least one scientific publication in WoS via an NPL reference. Jefferson et al. (2018) starts with 11.8 million scientific publications published between 1980 and 2015, of which roughly 1.2 million are cited in 690,000 patent *families* (1.1 million patents). Marx and Fuegi (2019) link US patents from 1926-2018 to scientific papers from 1800-2018, identifying approximately 15.7 million citation links between 1.4 million patents to 2.9 million papers. In comparison, our dataset links 948,006 patent *families* from 1985-2017 to 2,229,658 distinct scientific articles in the time range of 1980-2016.

SNPL Self-references

We single out SNPL references to scientific publications where at least one author also figures among the inventors of the patent and where one affiliation of the SNPL references overlap with the list of applicants in the patent. We refer to these categories as SNPL inventor self-references and applicant self-references, respectively. This type of SNPL references reveals links between patents and scientific publications originating from either the same organization or from the same individuals, or both. The first

[4]With a quality cutoff at four, the precision increases even further, but recall suffers to a greater extent so that the quality cutoff at three is preferred when putting equal weight on precision and recall.

Table A.2: Match quality

Office	Precision	Recall
EPO	0.99	0.96
USPTO	0.99	0.92
WIPO	0.99	0.97

Notes: Based on a manual validation exercise of 1000 NPL references per office, as reported in Knaus and Palzenberger (2018). Precision is the share of NPL reference matches that was correct. Recall is, when considering all NPL references that could have been matched, the share that were matched correctly.

analyses rely on the full sample of SNPL references. We present results separately for these categories and excluding them in a later stage.

We consider SNPL inventor self-references those that refer to scientific publications where at least one author has the same name of an inventor on the patent. We consider as SNPL applicant self-references those that refer to scientific publications where at least one affiliation overlaps with the list of applicants in the patent. To match applicants with affiliations we use a list of manually disambiguated organizations (academic institutions and firms) derived from the combination of multiple sources: the Global Research Identifier Database (GRID), the ORBIS database and the EU Scoreboards database. We merge separately applicants in patents and affiliations in scientific publications to these lists using a probabilistic matching algorithm based on training data. We consider an applicant and an affiliation to be the same when they match to the same entity in the list. Note that the two categories of self-references may overlap.[5]

Related Literature on SNPL References

We briefly summarize the literature that has so far discussed the characteristics of SNPL references and their relationship with patent value.

Hicks et al. (2000) look at all scientific articles published between 1993 and 1995 in journals indexed in the Science Citation Index (SCI) with at least one US author. They find that about 6,600 of these publications were cited in 1997 US-invented patents. The probability of a publication being cited as SNPL depends not only on the publication's research field, but also on its scientific impact. If a publication belongs to the top 1% most highly cited publications, it is about nine times more likely to be cited by a US patent than a randomly chosen US publication. In similar vein, Popp (2017) finds in green energy technology fields that scientific articles that are cited frequently by other articles are also more likely to be cited by patents.

Breschi and Catalini (2010) analyze all patent applications to the European Patent Office (EPO) registered in the period 1990 to 2003 within three technology fields (lasers, semiconductors and biotechnology) and find about 44,000 patents with altogether 18,000 SNPL references. SNPL references are more frequent in biotech and lasers than in semiconductors, presumably due to the larger distance between the semiconductor technology field and science.

[5]Figure A.5a presents related descriptive statistics.

Harhoff, Scherer, and Vopel (2003) are among the first to analyze the relationship between the value of such patents and the scientific impact of the underlying scientific contributions. They document a positive relationship between patent value and the number of NPL references. The relationship is particularly strong in the technical area of chemicals and pharmaceuticals. Several other authors have explored the role of NPL references as potential determinants of patent value. Branstetter (2005) uses a random sample of 30,000 US patents from 1983-86, of which about 4,300 include SNPL. Those patents that cite scientific articles are of significantly higher quality (more claims and forward-citations) than those that do not. Sorenson and Fleming (2004) link about 17,300 patents from 1990 with about 16,700 non-patent references. Here, patents that cite non-patent literature receive more citations and are cited more quickly than other patent. They argue this positive relationship between forward-citations and science intensity of a given patent is due to knowledge diffusion through the academic publication. Gittelman and Kogut (2003) explicitly ask “Does good science lead to valuable knowledge?" in biotechnology. They suggest that “(. . .) the evolutionary logics that select valuable scientific publications and valuable patents are different, and because of this, influential publications are not more likely to lead to influential patents than other publications." They employ data on the patent and publication portfolios of 116 biotechnology firms and obtain results that largely confirm their hypothesis.

Suzuki (2011) argues that patented inventions may be assessed with regard to their monetary or their technical quality. The presence of references to the scientific publications has a strong positive effect on the technological value, but a weak negative effect on the commercial value of the patent. The author also points to considerable heterogeneity across technological fields. Fischer and Leidinger (2014) use data from Ocean Tomo auctions between 2006 and 2009 to approximate auction prices as a function of observable value correlates. They find only weak and imprecisely estimated effects for the number of NPL references. As they point out, patents traded at Ocean Tomo auctions are not representative and mostly in the IT and IT-related technical fields. Zahringer, Kolympiris, and Kalaitzandonakes (2017) construct a sample of young life science firms and find that higher-quality academic science is associated with patent citations. This relationship is moderated by the respective firm’s research activities. Veugelers and Wang (2019) use all Web of Science journal articles published in 2001 and all patents from PATSTAT (version 2013b). They find that only about 10% of articles become SNPL. Novel publications are more likely to receive future citations by patents, particularly the 1% highly novel scientific publications. They further find that publications receiving more scientific citations also receive more patent citations.

Sapsalis, de la Potterie, and Navon (2006) use data on 155 patent families with application dates between 1985 and 1999 at the EPO to model the relationship between citations received by patents and characteristics of the underlying science. They find that NPL self-references (i.e. the inventors are also authors on the referenced scientific publication) to the scientific literature are associated with an increase in forward-citations of a patent. The authors argue that in such cases of highly valuable patents, “the inventors master (and contribute to) the related science-base (as witnessed by their own publications) and decide to codify their tacit knowledge into technological inventions" (Sapsalis, de la Potterie, and Navon, 2006, p. 1640).

In the perspective followed by Fleming and Sorenson (2004), invention is interpreted as a process of search for new and useful configurations of technological components. Science serves as a map, pointing inventors to particularly useful configurations of components. Alternatively, science allows inventors to avoid search over less productive solutions. However, these effects are not pertinent across all technologies. Recourse to science may offer little help when inventors work with highly independent components, but should generate high returns when the underlying inventive problem is particularly difficult. Using the population of patents granted by the USPTO in May and June of 1990 (n=16,822 after exclusion of 442 patents without any references), they find that only 2,919 of these patents reference scientific publications. In the empirical analysis, the authors show that references to scientific publications increase forward-citations received by patents with an elasticity of about 10%.

While the results of the studies discussed here are intriguing, they are typically obtained from relatively small samples which are particularly well-suited for the respective studies. An exception is the recent study by Ahmadpoor and Jones (2017), who analyze the network of US patents citing directly or indirectly SNPL references. They hereby introduce the distance to the science frontier as a metric for science-technology intensity. Watzinger and Schnitzer (2018) borrow this metric and provide correlations between the science-technology intensity and the value of patents. Mukherjee, Romero, et al. (2017) emphasize the importance of the age structure of references. The authors study (separately) scientific publications in the WoS database and patents, but they do not link NPL references to WoS entries. Both for publications and for patents they detect a “hot spot" defined by the age structure (of backward references) that is correlated with an increase in citations received by the publication or patent.

Measures of Science Quality

Scientific Citations

Our main variable of interest is the scientific quality of publications cited in patents. We use measures of science quality based on the count of forward-citations to publications. This is an established bibliometric indicator of scientific quality. The use of citations is based on the notion that scientists cite publications they consider influential for their own research. Accordingly, it is possible to assume that highly cited publications have a greater impact on follow-on research and represent a meaningful measure of their scientific quality.

For a given publication, we count the number of citations in a window of three years from publication. This raises the issue that some of these citations may happen later than the filing date of the citing patent. In this case, the number of citations received by a publication may be not independent of the patent itself. In our main specifications, we assume for simplicity that the number of citations to the publication remains indeed independent to the patent citation. In robustness analyses, we verified that the core results remain equivalent when excluding patent citations to publications published in the three years before the filing of the patent.

Journal Impact Factor

An alternative measure of science quality is the impact factor of the journal in which the respective publication is published (JIF). In any given year, the impact factor of a journal is the number of citations, received in that year, of articles published in that journal during the two preceding years, divided by the total number of articles published in that journal during the two preceding years. We use JIF indicators available by the inCite Journal Citations Report. A disadvantage of this measure is that, due to the lack of completeness of the necessary information, the data are available only after 1997. Moreover, the JIF constitutes a retrospective measure of quality of the journal that ignores the possible high variance of publications quality within one same journal and over time. On the other hand, the JIF has the advantage of being predetermined at the time a publication is published, so that it is not subject to concerns about truncation and mechanical correlation with the measure of patent value.[6]

Patent Level Aggregation of SNPL References

In our sample, for patents with SNPL, there are on average 7.2 SNPL references per patent, and a considerable share of 64.0% has references to more than one distinct scientific publication. In our main analyses, we define SNPL science quality as the maximum science quality across publications in SNPL references in a patent. This is based on the notion that the distribution of scientific forward-citations is highly skewed. Consequently, the scientific impact of the most highly cited publication, or the journal with the highest JIF, may be more indicative of SNPL overall science quality than the average across publications. For robustness, we also estimate alternative aggregation operators. This is further discussed in section A.2.[7]

We apply a coherent criterion to aggregate at the patent level the information regarding the presence of self-references: we consider a patent as having a self-reference if the scientific publication with the highest scientific quality among the SNPL references is a self-reference.

Measures of Patent Value

Patent Citations

Our main dependent variable is patent value. In our main specification, we proxy patent value with the number of forward-citations received by the patent. The number of citations is an established, and perhaps the most widely used, measure of patent value, which is highly correlated with other indicators of technological and economic value of patents (Harhoff, Scherer, and Vopel, 2003; Fischer and Leidinger, 2014; Moser, Ohmstedt, and Rhode, 2018). Patent citations differ substantially from citations in scientific literature. Scientific citations constitute recognition of scientists of the relevance of previous contributions for their own work. In contrast, patent citations, particularly to other patents, perform the legal function of documenting the technological relatedness of a patent to existing prior art with the scope of assessing its novelty and patentability (Michel and Bettels, 2001; Roach and Cohen, 2013).

Due to different legal requirements, citations at the EPO and the USPTO differ substantially. EPO patents tend to cite patents that are essential to document the novelty (or lack of novelty) and patentability of the invention; the applicants, in particular, are not required to provide any citation[8]. Applicants at USPTO are expected to report the most extensive list of citations to all possibly relevant patents and examiner complement this list. For this reason we provide analysis where we count EPO and USPTO citations separately. In our main specifications we use USPTO citations.

[6]We use the JIF as variable of interest in table A.6 to show robustness of our results to alternative measures of science quality.
[7]Table A.7 shows the corresponding results.
[8]Indeed, EPO patents are often filed with no initial references, and, when present, the introduction of references by the applicant is arguably more strategic than in other jurisdictions.

We construct the count of citations to a patent from the USPTO over a period of 5 years from the first filing date.[9] In robustness analyses, we use the count of citations from the EPO within 5 years from the first filing date. In case of the EPO citation measure, only examiner-supplied citations are considered.

Patent Scope

As alternative proxy for patent value, we adopt a measure of the patent's scope. The value of a patent is considered proportional to the scope of its protection on a particular technology. The narrower the scope of protection, the lower is its value. The text of patent claims tends to be longer for highly specific and narrow patent protection. In other words, longer descriptions of a claimed invention are associated with more specific features that are actually object of the patent protection (Kuhn and Thompson, 2019). Our measure is defined as the logarithm of the number of words in the first independent claim in patents.[10]

Measures of Monetary Value

Patent citations and patent claim length need to be understood as merely indirect measures of a patent's economic value. Moreover, the number of citations is at times considered to also capture the technological and social value of a patent (Trajtenberg, 1990), which may differ from the private value for the patent owner. Obtaining direct indicators of the monetary private value of patents is a challenging task. Data on this dimension of patent value have limited coverage. To complement the array of indicators of patent value in this direction we adopt two sources of data. First, we use data provided by Kogan et al. (2017) based on estimated stock market returns to the grant of the patent, as a proxy of the private value of the patent grant. Kogan values are only available for patent families with US patent members where at least one applicant was a publicly listed US company. The data cover exclusively a total of 1,029,987 patent families, of which 229,525 come with SNPL references. Second, we use survey-based assessments of patent value from the research project PatVal (Giuri et al., 2007). This is a subsample of 11,061 patent families with at least one EP patent member, of which 2,554 have SNPL references with first filing year mostly in 2003-2005.[11] Descriptive statistics are available in the main publication.[12]

[9]The choice of the time window for the count of scientific and patent citations is motivated primarily by pragmatic considerations: we want to ensure a sufficient period so that the number of citations actually reflects the underlying constructs we are interested in, but we want to limit truncation. The difference between the window considered for scientific publications and for patents is also motivated by the fact that patent applications are not instantly published after filing and – as a result – typically receive few citations within the first years, whereas scientific publications are often cited immediately after publication.

[10]Tables A.5 and A.6 show the corresponding results. Descriptive statistics are available in the main publication.

[11]Tables A.5 and A.6 show the corresponding results.

[12]The Kogan et al. (2017) patent value measures have been in widespread usage since their publication, but in our setting they come with major drawbacks. Much of the private value of the technology will already be incorporated in the stock price, as previous patent publications and grants in other patent systems are informative for investors. The value narrowly captures the additional value of a patent granted in the US patent system. Any information related to the technological capability of the firm that the patent reveals will not be incorporated in that measure. On the other hand, the measures from Giuri et al. (2007) are based on a survey, but the exact phrasing measures much more precisely the concept of private patent value: "Suppose that on the day in which this patent was applied for, the applicant and you had all the information you have today regarding the value of this and the related patents. In case a potential competitor of the applicant was interested in buying the whole set of patents (the patent family including all national patents derived from it), what would have been the minimum price (in Euro) that the applicant should have demanded?".

A.3 Regression Analyses

Regression Models

Selection of Scientific Publications into SNPL References

In a first set of analyses, we consider the probability and frequency in which scientific publications appear in SNPL reference, as a function of their scientific quality.

The regressions take the following forms:

$$y_i = \beta_{cit}\ cit_i + \sum_{ft} \beta_{ft}\ SF_{fi} * T_{fi} + \epsilon_i \tag{A.1}$$

Dependent variable and predictors of interest:

- y_i: The dependent variable is a measure of the probability (or frequency) of a scientific publication appearing among the SNPL references. Respectively, the variable is either a binary or a count variable. Count variables are log-transformed with offset 1. Given the large dataset and the large number of FE groups, nonlinear (count) models are not considered. We employ several variants of these variables.
- cit_i: The main independent variable is a measure of scientific quality. We measure scientific quality at the publication level as the number of citations received over a 3 year period starting from publication (see section A.2).

FEs:

- $SF_{fi} * T_{fi}$: These are FEs corresponding to the combination of scientific fields and publication years. These FEs control flexibly for mechanical differences in scientific quality and SNPL frequency across different scientific fields and over time within each scientific field. In total, there are 252 scientific field codes supplied by the Web of Science.

Science Quality and Patent Value: Residualized Variables

Naturally, usage of SNPL references as well as the quality of cited SNPL varies substantially over technological areas as well as over time. In the regression models below, this is taken into account explicitly with FE control variables. In all figures relating patents to scientific quality, we apply residualization which brings the graphical display in line with the regression outputs.

To do so, we regress both the SNPL science quality variables as well as the patent value variables on the full set of technology area × first filing year FEs. The formal model reads $y_i = \sum_{ft} \beta_{ft} F_{fi} * T_{ti} + \epsilon_i$. This is done in the full sample of patents both with and without SNPL references. Afterwards, we calculate the residual variation as $\hat{\epsilon}_i \equiv y_i - \hat{y}_i = y_i - \sum_{ft} \hat{\beta}_{ft} F_{fi} * T_{ti}$, where $\hat{\epsilon}$, $\hat{y}$ and $\hat{\beta}$ are estimated values. In fact, $\hat{\epsilon}_i = y_i - \bar{y}_{ft}$, where $\bar{y}_{ft}$ is the mean within technology area × first filing year group. Therefore, $E[\hat{\epsilon}_i] = 0$, both overall and within each ft group.

The values plotted in the graphs are $\hat{\epsilon}_i + \bar{y}$, where $\bar{y}$ is the full-sample mean of y. This returns the absolute levels back to what is contextually expected and interpretable.

In plain terms, this strategy removes level effects within technology area × first filing year groups by subtracting the mean y within groups. The overall level is retained by adding the overall y mean. The y variable is transformed. Before, it is a deviation from the *within-group* mean. Afterwards, it is a deviation from the *overall* mean.

Science Quality and Patent Value: Regression Models

In the empirical analysis, we study the relationship between the presence and the quality of scientific publications referenced in patents and the value of patents.

The regressions take the following form:

$$\begin{aligned} y_i = {} & \beta_{hasSNPL}\ hasSNPL_i + \beta_{snplQ}\ snplQ_i \\ & + \sum_{ft} \beta_{ft} TF_{fi} * T_{ti} + \sum_{a} \beta_a A_{ai} + \sum_{n} \beta_n N_{ni} + \sum_{r} \beta_r R_{ri} + \sum_{p} \beta_p P_{pi} + \epsilon_i \end{aligned} \tag{A.2}$$

Dependent variable and predictors of interest:

- y_i: The dependent variable is a measure of patent value. In the main specifications and figures, we use the count of citations from the USPTO within the first 5 years after filing. In alternative specifications, we use: the count of citations from the EPO; indicators of monetary value; patent scope as measured by the length of the first independent claim (see section A.2). All dependent variables are in log-terms with offset 1. Given the large dataset and the large number of FE groups, nonlinear (count) models could not be considered.
- $hasSNPL_i$: A dummy equal to 1 if a patent has at least one reference to a scientific publication
- $snplQ_i$: A measure of SNPL science quality. We measure scientific quality at the scientific publication level as the number of citations received over a period of 3 years from publication. We define SNPL science quality as the maximum scientific quality across SNPL references in a patent when more than one is present.[13]

FEs:

- $TF_{fi} * T_{ti}$: These are FEs corresponding to the combination of technological classes and first filing year. These FEs control flexibly for mechanical differences in patent value across different technological fields and over time within each technological field.
- A_{ai}: These are FEs for the applicant of the patent.
- N_{ni}: These are FEs for the distinct number of inventors listed on the patent.

[13]We test the robustness of the results to alternative aggregation criteria (see table A.7).

- R_{ri}: These are FEs for the number of patent references. We use individual FEs for each number of references up to the 95th percentile and assign one dummy for all patents with a higher number of references.[14]

- P_{pi}: These are FEs for the number of patent references to scientific publications. We use an individual FE for each number of references up to the number corresponding to the 95th percentile and aggregate in one FEs patents with a higher number of references. Note that $hasSNPL_i$ is collinear and therefore dropped when this set of FEs is used.

Regression Results

Selection of Scientific Publications in SNPL References

We present first regression results for the probability that a scientific publication appears in SNPL references as a function of its scientific quality. In the first main specification, table A.3, column 1 and 2, we consider all SNPL references. Second, in column 3 and 4, we consider exclusively SNPL references within five years from the year of publications. Third, in column 5 and 6, we consider references within five years and exclusively if they are the SNPL references with the highest scientific quality. In a fourth variant, column 7 and 8, we consider only SNPL references that are cited for the first time by an applicant, so that each patent applicant-scientific publication pair is counted at most once (*one per applicant*). Finally, in table A.4, we provide regression results excluding academic patents as well as self-references of various types. Figures A.3a and A.3b also show graphically that the exclusion of SNPL self-references is irrelevant to the results. Overall, we consistently find a positive and significant effect of science quality on the selection of scientific articles into SNPL references.

Main Regression Results: SNPL Science Quality and Patent Value

Table A.5 presents regression results for our core findings. It shows elasticity estimates for the main measure of SNPL science quality and each one of the alternative measures of patent value as dependent variable. We include sets of more demanding controls incrementally. All models include the variable $hasSNPL_i$ as a control for the level effect of having at least one SNPL reference. In column 1 to 6, we present results for our base-line specification where we control exclusively for technology field and year pair FEs. In column from 7 to 12, we include all patent level controls as detailed in the above section A.3. In column from 13 to 18, we add applicant FEs. Figure A.4a further highlights the striking differences in the overall distribution of patent citations for patents with and without SNPL references.

Alternative Measures of SNPL Science Quality

As a first variant to these specifications, we test the robustness of the results to alternative measures of SNPL science quality. In table A.6 we use a measure based on the journal impact factor instead of citations. The number of observations is lower because the journal impact factors are only available to

[14]In regressions involving PatVal (EUR) values, the number of available observations is substantially lower. Here, we include only the log-transformed count of backward patent references when estimating the extended specification.

us from 1998 onward. Overall, we find very similar results.[15] In table A.7, we use alternative measures of SNPL science quality derived from different criteria of aggregation at the patent level of the scientific quality of multiple scientific publications, when more than one appear in the NPL-references of a patent. When c_i is the citation count of SNPL reference i, in our main models we consider the maximum. Alternatively, we also consider the sum ($\sum_i c_i$), average ($\frac{1}{n} \sum_i c_i$) and square root of the sum of squares ($\sqrt{\sum_i c_i^2}$). We find similar results irrespective of the aggregation criterion used. Figure A.4b graphically shows the results.

Self-references

Figure A.5a shows the frequency of occurrence of self-references: between 5 and 10% of all patent families include a self-reference. Most self-references are inventor self-references (5-10%), whereas applicant self-references are less frequent with 2-4%. The frequency of self-references tends to decrease with the SNPL science quality (although non-monotonically); this tendency is most pronounced at the top.

In the paper, we consider the possibility that self-references drive the results. On the one hand, from a theoretical standpoint, it is interesting to consider whether high-quality science leads to high-value technologies within or outside the boundaries of the organizations in which it is developed. On the other hand, we want to ensure that the results are not driven by highly productive organizations and individuals that perform scientific and technological activities at the same time.

Figure A.5b replicates the results reported in the paper, separating different categories of self-references. The different groups of self-references behave very similarly. Table A.8 provides regression estimates separately for a sample consisting only of patents with self-references and excluding all patents with self-references. While the magnitude is larger for the sample with self-references, the estimated elasticities are positive and significant in all specifications. We can conclude that self-references do not drive the overall effects.

Technology Fields, Year of Patent Filing, and Applicant Countries

We analyze the heterogeneity of the estimates, first, across technological fields of patents. In table A.9, we run separate regressions by technology main area. In line with previous literature (e.g., Harhoff, Scherer, and Vopel, 2003), we find that effects are particularly strong in Chemistry. However, SNPL science quality also matters for Electrical Engineering, Instruments as well as Mechanical Engineering.

Second, we explore the heterogeneity over time based on the first filing year of patents. We decompose the elasticities calculated in table A.5 (column 1/2, 7/8) over time. Figure A.6 depicts the corresponding point estimates. We find that after marginally increasing between 1985 and 2000, the extent of the relationship decreased substantially. After 2000, there is a substantial decline which is especially pronounced for the US system. The understanding of the reasons for this decline requires further research.

[15] The only exception are the results for the USD values, where the results are unstable in the first two specifications (column 5 and 11) but remain positive and significant in our last and most complete specification.

We consider the possibility that the effect is driven by intense science usage of particular countries. To do so, we split the sample by the first applicant country and consider China, Europe (EU-28), Japan, South Korea and the United States separately. From table A.10, we find that science quality is important in all countries, but particularly so in Europe, the US and Japan. Overall, the results are consistent across different geographic areas.

Interdisciplinarity of SNPL References

We explore the role of interdisciplinarity of science. Previous studies demonstrate the existence of a close connection between novelty and scientific impact and the ability of scientists to successfully recombine knowledge from distinct domains (Mukherjee, Romero, et al., 2017; Wang, Veugelers, and Stephan, 2017; Veugelers and Wang, 2019). In the context of our analysis we are interested in exploring whether this dimension explains the correlation between SNPL science quality and patent value. We proxy the interdisciplinarity of science with the interdisciplinary journals as captured by the classification of journals in scientific fields in WoS.[16]

In figure A.7a we plot the share of patents with SNPL references to interdisciplinary scientific publications and, in figure A.7b, the patent value by SNPL science quality of patents with and without SNPL interdisciplinary references. The share of patents with interdisciplinary SNPL references is highest for intermediary values of SNPL science quality. Indeed, we find that interdisciplinarity is associated overall with higher patent value, with the exception of patents at the top of the SNPL science quality distribution. The correlation with SNPL science quality remains in any case highly positive for both categories. Table A.11 presents the underlying regression results.

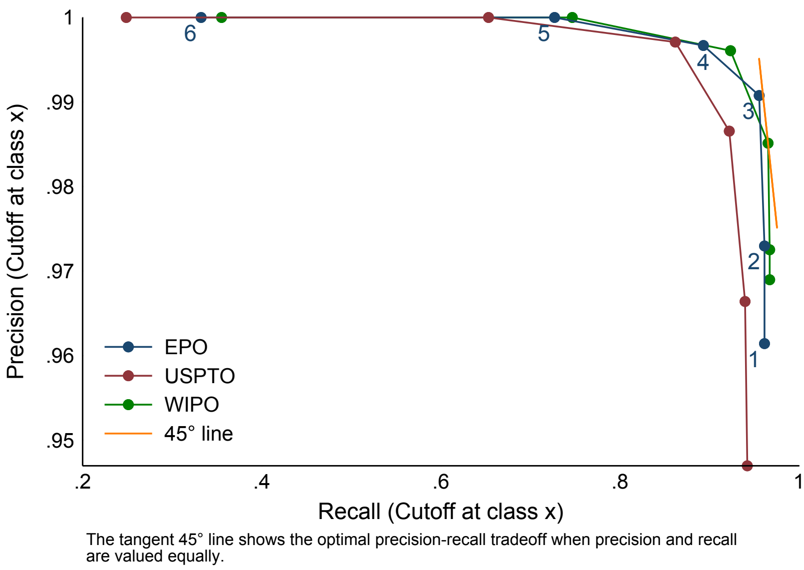
Distance of SNPL References

Finally, we present regression results for the distance and time-distance of SNPL references. The related results are presented graphically in the paper. Table A.12 shows results relative to the interaction between the distance of SNPL references and table A.13 reports the corresponding regression results for the time-distance of SNPL references. Here, we split the time-distance into tertiles. In accordance to what is discussed in the paper, we find that patent families at a short distance, by either dimension, are of particularly high value and tend to show higher elasticities with SNPL science quality as well. The elasticities remain in any case strongly positive and significant also at a relatively high distance.

[16] Some field codes refer directly to multidisciplinary research. These field codes are ah, vj, wu, bq, po, ev, ui, dy, le, ro, if and pm. We tested our results by including journals associated with these codes in the sample of interdisciplinary journals or excluding them. This affects the level estimates of patent value for different values of interdisciplinarity but leaves untouched the correlation with SNPL science quality.

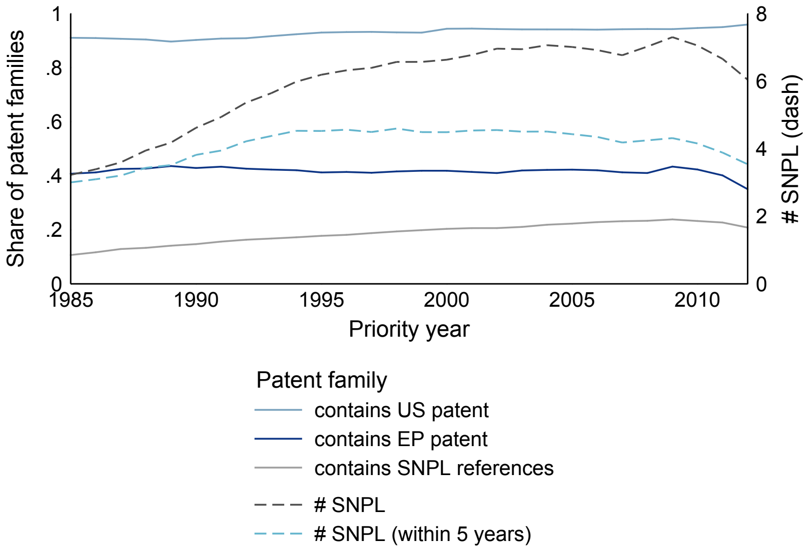
A.4 Supplementary Graphs and Tables

Figure A.1: Precision-recall tradeoff in the WoS match



Notes: Based on manually evaluating 1000 matches for each patent office, as reported in Knaus and Palzenberger (2018). The 45°-line shows the set of points which is of the same quality if precision and recall are weighted equally. This corresponds to a F1-score. The point where this line is intersected is the optimal point, here shown for the WIPO validation. Precision is the share of SNPL reference matched correctly among the matched SNPL references. Recall is the ratio between correct matches identified and all SNPL references that were or should have been matched.

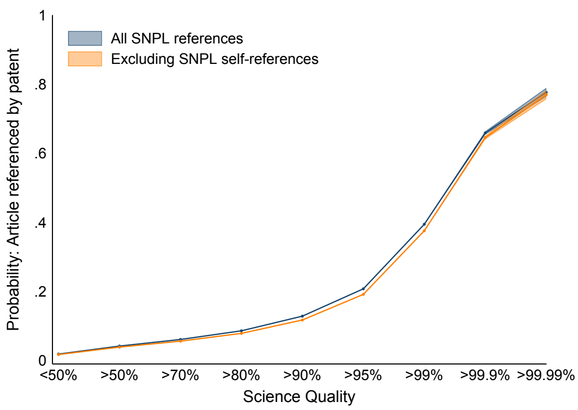
Figure A.2: Sample descriptives over time



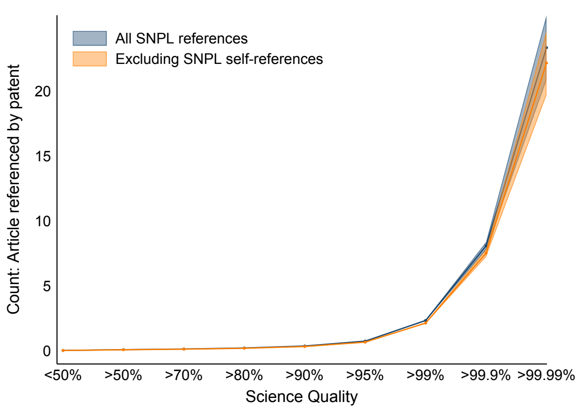
Notes: Shows the composition of the estimation sample. The estimation sample contains US and EP patents from patent families with at least one member patent granted at USPTO or EPO.

Figure A.3: Selection into SNPL by science quality

(a) Probability of patent citation (ext. margin)

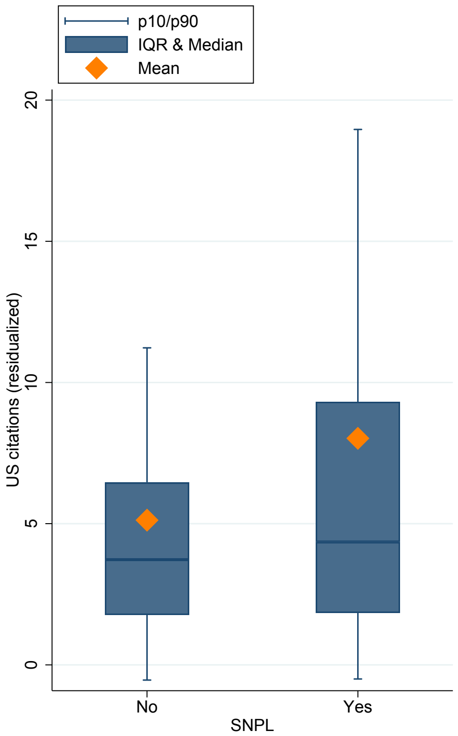


(b) Number of patent citations (int. margin)

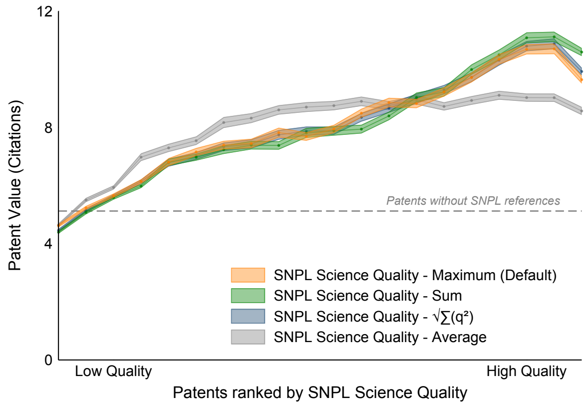


Notes: Probability of being cited as SNPL (left) and the number of SNPL citations (right) by science quality of a scientific publication. Science quality is the 3-year citation count of the scientific publication. Shaded areas show 95% confidence intervals around the respective means.

Figure A.4: Patent Value and SNPL references



(a) Patent value by science reference



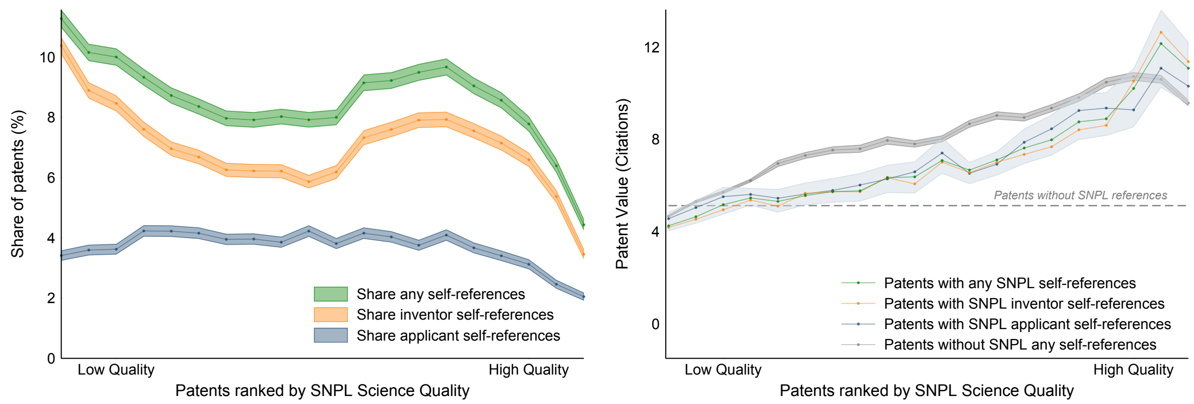
(b) Patent value by SNPL science quality

Notes: Left: Distribution of patent citations for patents with and without SNPL references. Residualized 5-year patent forward-citations by US patents towards patent families are used, see section A.3.
Right: Average patent values by science quality, considering alternative science quality operationalizations. SNPL science quality is the quality of publications referenced by a patent. When there are multiple patent-paper references, we by default use the highest-quality reference (orange). In comparison, the average quality also delivers a positive correlation (gray), but it is more diluted. Other aggregation methods which also focus on the top of the distribution are virtually identical to the maximum. These are the sum (green) and the square root of the sum of squares (blue). Science quality is the 3 year citation count of a scientific publications. Patent value is measured as the 5 year count of patent forward citations by US patents. Patent value and science quality are residualized using technology field × first filing year FEs. The dashed line indicates the average patent value of patents without SNPL references. Shaded areas show 95% confidence intervals around the respective means. N = 4,767,844 patents (948,006 with SNPL references).

Figure A.5: Patent value by science quality, with and without SNPL self-references

(a) Share of SNPL (self-)references by science quality

(b) Patent value by science quality of SNPL (self-)references

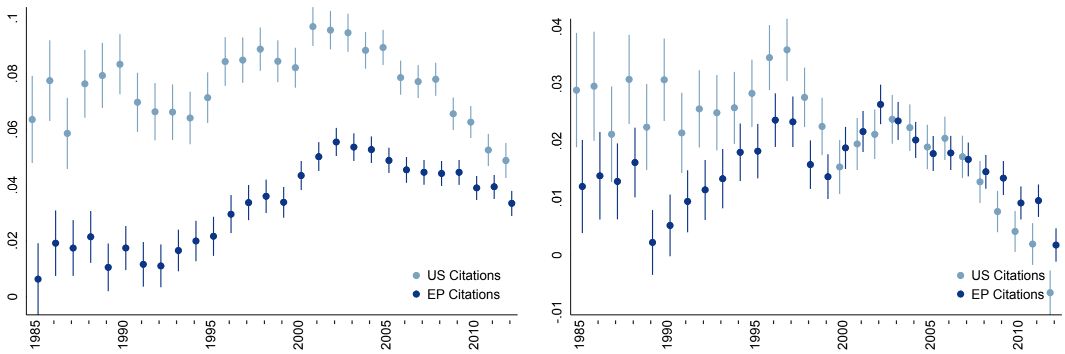


Notes: Left: Share of SNPL self-references by SNPL science quality. Right: Average patent value by SNPL science quality and categories of SNPL self-references. The lines show values for any self-reference (green), inventor self-references (orange) and applicant self-references (blue) or patents without SNPL self-references (gray). SNPL science quality is the maximum 3-year citation count across scientific publications appearing as SNPL references in a patent. Patent value is measured as the 5-year count of patent forward-citations by US patents. Patent value and science quality measures are residualized using technology field-first filing year pair FEs. The gray shaded area shows 95% confidence intervals around the respective means. For visual purposes, the confidence intervals around the means concerning self-references show the maximum extent of the 95% confidence intervals of any of the three underlying measures.

Figure A.6: Patent value-science quality relationship over time

(a) Baseline regressions

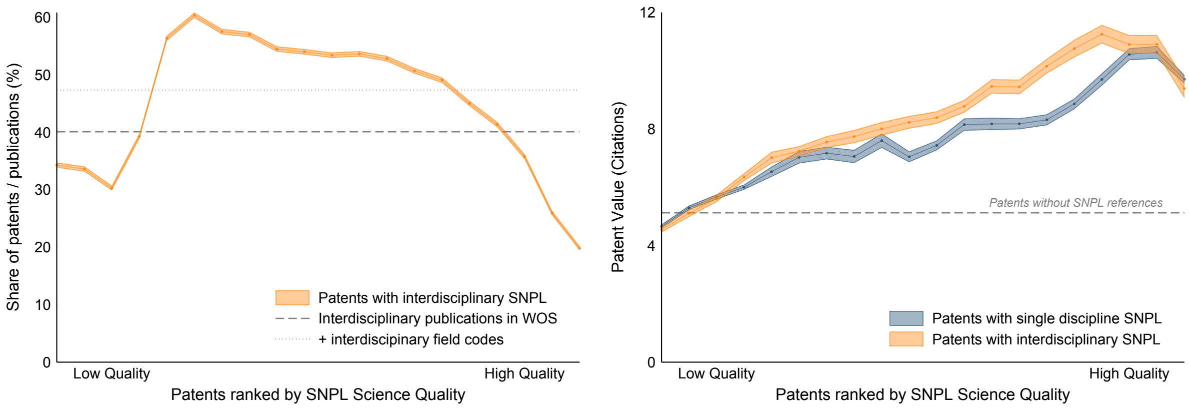
(b) All patent-level controls



Notes: The figure plots the interactions coefficients between each first filing year of patents and SNPL science quality, in a regression with the 5-year patent forward-citations by US patents and EP patents as dependent variables. Science quality is the maximum 3-year citation count of SNPLs of a patent family. Left: Models include technology field and first filing year pair FEs. Right: Models additionally include FEs for SNPL reference counts, patent reference counts and number of inventors. Range indicators show 95% confidence intervals around the respective regression coefficients.

Figure A.7: Patent value by science quality and interdisciplinarity

(a) Share of interdisciplinarity (b) Patent value and interdisciplinarity



Notes: Left: Share of patents with interdisciplinary SNPL by SNPL science quality. Right: Patent value by SNPL science quality and by interdisciplinarity of SNPL references. Scientific articles are considered interdisciplinary if the journal where they are published is associated with at least two WoS field codes. SNPL science quality is the maximum 3-year citation count across scientific publications appearing as SNPL references in a patent. Patent value is measured as the 5-year count of patent forward-citations by US patents. Patent value and science quality measures are residualized using technology field-first filing year pair FEs. Shaded areas show 95% confidence intervals around the respective means.

Table A.3: SNPL and science quality elasticities (intensive and extensive margin, by SNPL definitions)

SNPL definition	All		Within 5y		Within 5y max quality		One per applicant	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
DV: SNPL	(1/0)	Count	(1/0)	Count	(1/0)	Count	(1/0)	Count
3y Cit	0.053	0.068	0.041	0.048	0.012	0.013	0.041	0.043
	(1525.98)	(1551.78)	(1378.07)	(1408.98)	(762.81)	(796.86)	(1371.67)	(1443.36)
Field × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-Square	0.113	0.110	0.090	0.089	0.032	0.033	0.089	0.093
Observations	42259668	42259668	42259668	42259668	42259668	42259668	42259668	42259668

Notes: Values in "0/1"-columns are semi-elasticities, values in "Count"-columns are elasticities. Includes WoS subject code times publication year FEs. The level of observation is at the WoS item. Science quality (*3y cit*) is measured by 3-year forward-citations by other WoS items. Robust standard errors. T-statistics in parentheses.

Table A.4: SNPL and science quality elasticities (probability and frequency, with SNPL restrictions)

SNPL restriction	No academic patents		No applicant self-ref.		No inventor self-ref.		No any self-ref.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
DV: SNPL	(1/0)	Count	(1/0)	Count	(1/0)	Count	(1/0)	Count
3y Cit	0.036	0.046	0.052	0.066	0.050	0.063	0.049	0.063
	(1188.04)	(1195.63)	(1511.65)	(1535.17)	(1476.07)	(1493.49)	(1472.55)	(1489.77)
Field × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-Square	0.082	0.077	0.111	0.108	0.107	0.103	0.106	0.103
Observations	42259668	42259668	42259668	42259668	42259668	42259668	42259668	42259668

Notes: Includes WoS subject code times publication year FEs. The level of observation is at the WoS item. Science quality (*3y cit* is measured by 3-year forward-citations by other WoS items. The baseline category consists of observations with no 3-year forward-citations, approximately 50% of the dataset. Robust standard errors. T-statistics in parentheses.

Table A.5: Patent value and science quality

	(1)	(2)	(3)	(4)	(5)	(6)
DV (log):	5y Cit US	5y Cit EP	US Claim Length	EP Claim Length	USD Values	EUR Values
3y Cit SNPL ref (max)	0.082	0.042	−0.015	−0.059	0.021	0.114
	(123.56)	(85.49)	(−26.83)	(−39.08)	(6.52)	(3.69)
Patent-level controls	Base	Base	Base	Base	Base	Base
Patent applicant FE	No	No	No	No	No	No
Adj. R-Square	0.156	0.067	0.157	0.323	0.113	0.045
Observations	4319660	4319660	2464729	1241154	899351	10844

	(7)	(8)	(9)	(10)	(11)	(12)
DV (log):	5y Cit US	5y Cit EP	US Claim Length	EP Claim Length	USD Values	EUR Values
3y Cit SNPL ref (max)	0.037	0.030	−0.012	−0.038	−0.047	0.089
	(45.97)	(47.64)	(−16.58)	(−18.64)	(−11.46)	(2.88)
Patent-level controls	All	All	All	All	All	All
Patent applicant FE	No	No	No	No	No	No
Adj. R-Square	0.262	0.100	0.160	0.324	0.125	0.065
Observations	4319660	4319660	2464729	1241154	899351	10844

	(13)	(14)	(15)	(16)	(17)	(18)
DV (log):	5y Cit US	5y Cit EP	US Claim Length	EP Claim Length	USD Values	EUR Values
3y Cit SNPL ref (max)	0.027	0.023	−0.010	−0.027	0.004	0.091
	(29.75)	(31.70)	(−11.91)	(−12.85)	(2.21)	(1.61)
Patent-level controls	All	All	All	All	All	All
Patent applicant FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-Square	0.362	0.169	0.253	0.389	0.887	0.113
Observations	3764150	3764150	2099419	1122902	857252	5702

Notes: All reported values are elasticities. *3y Cit SNPL ref (max)* is a measure of SNPL science quality corresponding to the maximum 3-year citation count across scientific publications appearing as SNPL references in a patent. Patent-level controls “Base” include technology fields and first filing year pair FEs. Patent-level controls “All” further include FEs for SNPL reference counts, patent reference counts and number of inventors. Patent applicant FEs are based on the first applicant on the grant publication. Robust standard errors. T-statistics in parentheses.

Table A.6: Patent value and science quality (journal impact factor of SNPL reference)

	(1)	(2)	(3)	(4)	(5)	(6)
DV (log):	5y Cit US	5y Cit EP	US Claim Length	EP Claim Length	USD Values	EUR Values
JIF SNPL ref (max)	0.118	0.087	−0.034	−0.142	−0.008	0.212
	(68.01)	(67.05)	(−26.18)	(−31.72)	(−0.83)	(2.98)
Patent-level controls	Base	Base	Base	Base	Base	Base
Patent applicant FE	No	No	No	No	No	No
Adj. R-Square	0.148	0.064	0.157	0.327	0.112	0.045
Observations	3928677	3928677	2289162	1106544	773983	10253

	(7)	(8)	(9)	(10)	(11)	(12)
DV (log):	5y Cit US	5y Cit EP	US Claim Length	EP Claim Length	USD Values	EUR Values
JIF SNPL ref (max)	0.030	0.056	−0.039	−0.105	−0.056	0.178
	(15.80)	(37.49)	(−25.04)	(−18.81)	(−5.11)	(2.52)
Patent-level controls	All	All	All	All	All	All
Patent applicant FE	No	No	No	No	No	No
Adj. R-Square	0.255	0.097	0.160	0.329	0.124	0.065
Observations	3928677	3928677	2289162	1106544	773983	10253

	(13)	(14)	(15)	(16)	(17)	(18)
DV (log):	5y Cit US	5y Cit EP	US Claim Length	EP Claim Length	USD Values	EUR Values
JIF SNPL ref (max)	0.016	0.044	−0.031	−0.078	0.019	0.143
	(7.15)	(24.40)	(−15.36)	(−12.75)	(3.90)	(1.04)
Patent-level controls	All	All	All	All	All	All
Patent applicant FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-Square	0.359	0.170	0.260	0.394	0.891	0.117
Observations	3385170	3385170	1930366	993031	734430	5353

Notes: All reported values are elasticities. *3y Cit SNPL ref (max)* is a measure of SNPL science quality corresponding to the maximum JIF across scientific publications appearing as SNPL references in a patent. Patent-level controls "Base" include technology fields and first filing year pair FEs. Patent-level controls "All" further include FEs for SNPL reference counts, patent reference counts and number of inventors. Patent applicant FEs are derived from the first applicant on the grant publication. Robust standard errors. T-statistics in parentheses.

Table A.7: Patent value and science quality (alternative science quality indicators)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
DV (log):	5y Cit US	5y Cit US	5y Cit US	5y Cit US	5y Cit US	5y Cit US	5y Cit US	5y Cit US
SNPL science quality indicators:								
3y Cit SNPL ref (max)	0.037	0.027						
	(45.97)	(29.75)						
3y Cit SNPL ref (sum)			0.038	0.027				
			(46.32)	(29.32)				
3y Cit SNPL ref (avg)					0.042	0.030		
					(47.38)	(29.83)		
3y Cit SNPL ref (sq)							0.038	0.028
							(46.65)	(29.96)
Patent-level controls	All	All	All	All	All	All	All	All
Patent applicant FE	No	Yes	No	Yes	No	Yes	No	Yes
Adj. R-Square	0.262	0.362	0.262	0.362	0.262	0.362	0.262	0.362
Observations	4319660	3764150	4319660	3764150	4319660	3764150	4319660	3764150

Notes: All reported values are elasticities. The table present results for alternative criteria of aggregation at the patent level of the science quality of SNPL references. The dependent variable is the 5-year count of patent forward-citations by US patents. Patent-level controls “All” include technology fields and first filing year pair FEs, FEs for SNPL reference counts, patent reference counts and number of inventors. Patent applicant FEs are derived from the first applicant on the grant publication. Robust standard errors. T-statistics in parentheses.

Table A.8: Patent value and science quality (self-references)

Self-references	Excluded		Only	
	(1)	(2)	(3)	(4)
DV (log):	5y Cit US	5y Cit US	5y Cit US	5y Cit US
3y Cit SNPL ref (max)	0.034	0.025	0.040	0.027
	(36.54)	(23.74)	(24.20)	(14.19)
Patent-level controls	All	All	All	All
Patent applicant FE	No	Yes	No	Yes
Adj. R-Square	0.255	0.358	0.235	0.339
Observations	4101699	3556564	3688129	3156722

	(5)	(6)	(7)	(8)
DV (log):	5y Cit EP	5y Cit EP	5y Cit EP	5y Cit EP
3y Cit SNPL ref (max)	0.025	0.018	0.046	0.037
	(35.33)	(22.60)	(34.15)	(23.21)
Patent-level controls	All	All	All	All
Patent applicant FE	No	Yes	No	Yes
Adj. R-Square	0.090	0.160	0.084	0.151
Observations	4101699	3556564	3688129	3156722

Notes: All reported values are elasticities. *3y Cit SNPL ref (max)* is a measure of SNPL science quality corresponding to the maximum 3-year citation count across scientific publications appearing as SNPL references in a patent. Patent-level controls "All" further include FEs for SNPL reference counts, patent reference counts and number of inventors. Patent applicant FEs are derived from the first applicant on the grant publication. Robust standard errors. T-statistics in parentheses.

Table A.9: Patent value and science quality (by technology area of patent)

Technology area	Electrical Eng	Instruments	Chemistry	Mechanical Eng
	(1)	(2)	(3)	(4)
DV (log):	5y Cit US	5y Cit US	5y Cit US	5y Cit US
3y Cit SNPL ref (max)	0.029	0.031	0.059	0.048
	(22.05)	(16.18)	(45.28)	(11.28)
Constant	1.645	1.470	1.048	1.080
	(1758.52)	(984.06)	(524.81)	(1323.79)
Patent-level controls	All	All	All	All
Patent applicant FE	No	No	No	No
Adj. R-Square	0.204	0.232	0.235	0.176
Observations	1542296	714030	779718	953578

	(5)	(6)	(7)	(8)
DV (log):	5y Cit US	5y Cit US	5y Cit US	5y Cit US
3y Cit SNPL ref (max)	0.026	0.025	0.040	0.032
	(18.03)	(10.66)	(26.02)	(6.16)
Patent-level controls	All	All	All	All
Patent applicant FE	Yes	Yes	Yes	Yes
Adj. R-Square	0.316	0.359	0.324	0.309
Observations	1359980	570818	652092	755247

Notes: All reported values are elasticities. *3y Cit SNPL ref (max)* is a measure of SNPL science quality corresponding to the maximum 3-year citation count across scientific publications appearing as SNPL references in a patent. Patent-level controls “All” further include FEs for SNPL reference counts, patent reference counts and number of inventors. Patent applicant FEs are derived from the first applicant on the grant publication. Robust standard errors. T-statistics in parentheses.

Table A.10: Patent value and science quality (by patent applicant country)

Patent applicant country	China	Europe	Japan	South Korea	United States
	(1)	(2)	(3)	(4)	(5)
DV (log):	5y Cit US	5y Cit US	5y Cit US	5y Cit US	5y Cit US
3y Cit SNPL ref (max)	0.021	0.042	0.036	0.014	0.032
	(3.31)	(25.15)	(18.65)	(3.08)	(29.35)
Patent-level controls	All	All	All	All	All
Patent applicant FE	No	No	No	No	No
Adj. R-Square	0.218	0.287	0.171	0.176	0.268
Observations	54067	948340	869126	149395	2032498

	(6)	(7)	(8)	(9)	(10)
DV (log):	5y Cit EP	5y Cit EP	5y Cit EP	5y Cit EP	5y Cit EP
3y Cit SNPL ref (max)	0.005	0.033	0.022	0.018	0.033
	(0.92)	(23.90)	(13.93)	(5.25)	(38.76)
Patent-level controls	All	All	All	All	All
Patent applicant FE	No	No	No	No	No
Adj. R-Square	0.126	0.090	0.072	0.118	0.133
Observations	54067	948340	869126	149395	2032498

Notes: All reported values are elasticities. *3y Cit SNPL ref (max)* is a measure of SNPL science quality corresponding to the maximum 3-year citation count across scientific publications appearing as SNPL references in a patent. Patent-level controls “All” further include FEs for SNPL reference counts, patent reference counts and number of inventors. Patent applicant FEs are derived from the first applicant on the grant publication. Robust standard errors. T-statistics in parentheses.

Table A.11: Patent value and science quality (interdisciplinarity)

	(1)	(2)	(3)	(4)
DV (log):	5y Cit US	5y Cit US	5y Cit EP	5y Cit EP
Interdisciplinary	0.054	0.046	0.041	0.037
	(15.26)	(12.26)	(15.37)	(12.67)
3y Cit SNPL ref (max) × Single Discipline	0.043	0.031	0.036	0.028
	(45.33)	(29.77)	(47.83)	(32.40)
3y Cit SNPL ref (max) × Interdisciplinary	0.034	0.024	0.023	0.018
	(31.47)	(20.68)	(27.86)	(18.73)
Patent-level controls	All	All	All	All
Patent applicant FE	No	Yes	No	Yes
Adj. R-Square	0.262	0.362	0.100	0.169
Observations	4319660	3764150	4319660	3764150

Notes: All reported values are elasticities. *3y Cit SNPL ref (max)* is a measure of SNPL science quality corresponding to the maximum 3-year citation count across scientific publications appearing as SNPL references in a patent. The interdisciplinarity status is taken from the most cited scientific publication appearing as SNPL reference in a patent. Patent-level controls "All" further include FEs for SNPL reference counts, patent reference counts and number of inventors. Patent applicant FEs are derived from the first applicant on the grant publication. Robust standard errors. T-statistics in parentheses.

Table A.12: Patent value and scientific impact (US citations) (by frontier distance)

	(1)		(2)	
DV (log):	5y Cit US		5y Cit US	
Distance to frontier:				
1	0.819	(416.37)	0.740	(316.23)
2	0.605	(340.54)	0.562	(288.90)
3	0.373	(204.97)	0.394	(192.48)
4	0.231	(108.29)	0.262	(100.96)
5	0.135	(46.88)	0.190	(50.51)
6	0.107	(29.23)	0.170	(34.30)
7	0.121	(32.47)	0.204	(40.20)
8	0.119	(36.58)	0.199	(45.92)
9	0.109	(35.64)	0.189	(46.96)
10	0.071	(22.54)	0.149	(35.72)
3y Cit SNPL ref (max)	0.060	(208.43)		
3y Cit SNPL ref (max) × 1			0.091	(144.40)
3y Cit SNPL ref (max) × 2			0.073	(155.18)
3y Cit SNPL ref (max) × 3			0.042	(74.00)
3y Cit SNPL ref (max) × 4			0.032	(32.85)
3y Cit SNPL ref (max) × 5			0.013	(8.03)
3y Cit SNPL ref (max) × 6			0.012	(5.41)
3y Cit SNPL ref (max) × 7			0.002	(0.71)
3y Cit SNPL ref (max) × 8			0.001	(0.28)
3y Cit SNPL ref (max) × 9			−0.001	(−0.74)
3y Cit SNPL ref (max) × 10			0.001	(0.39)
Patent-level controls	Base		Base	
Patent applicant FE	No		No	
Adj. R-Square	0.186		0.188	
Observations	4378579		4378579	

Notes: All reported values are elasticities. Patent-level controls "Base" include technology fields and first filing year pair FEs. *3y Cit SNPL ref (max)* is a measure of SNPL science quality corresponding to the maximum 3-year citation count across scientific publications appearing as SNPL references in a patent. Robust standard errors. T-statistics in parentheses.

Table A.13: Patent value and scientific impact (by time-distance)

	(1)	(2)	(3)	(4)
DV (log):	5y Cit US	5y Cit US	5y Cit EP	5y Cit EP
SNPL time distance:				
Short	0.139	0.115	0.089	0.076
	(33.16)	(25.42)	(27.95)	(21.57)
Medium	0.052	0.040	0.039	0.028
	(12.11)	(8.50)	(12.10)	(7.89)
3y Cit SNPL ref (max) × Short	0.041	0.032	0.034	0.027
	(37.57)	(26.96)	(39.15)	(28.34)
3y Cit SNPL ref (max) × Medium	0.038	0.027	0.027	0.021
	(33.59)	(21.89)	(30.59)	(20.94)
3y Cit SNPL ref (max) × Long	0.031	0.019	0.028	0.018
	(26.98)	(14.83)	(30.25)	(17.53)
Patent-level controls	All	All	All	All
Patent applicant FE	No	Yes	No	Yes
Adj. R-Square	0.263	0.363	0.101	0.169
Observations	4319660	3764150	4319660	3764150

Notes: All reported values are elasticities. *3y Cit SNPL ref (max)* is a measure of SNPL science quality corresponding to the maximum 3-year citation count across scientific publications appearing as SNPL references in a patent. *Short*, *Medium* and *long* time-distance are dummies for the tertiles of time-distance. Patent-level controls "All" further include FEs for SNPL reference counts, patent reference counts and number of inventors. Patent applicant FEs are derived from the first applicant on the grant publication. Robust standard errors. T-statistics in parentheses.

B

Appendix to Chapter 2
Firm Participation and Knowledge Diffusion in International Science

B.1 Data

In this appendix we extend the description of our data sources and data construction, following the section 2.4 of the paper. Table B.1 summarizes the type of information obtained from each source and gives references to the source. The relationships between the data sources are visually documented in figure B.1.

Table B.1: Data sources.

Data source	Variables
DBLP	Conference, conference series information including place, time and presented papers, author disambiguation http://dblp.uni-trier.de/
CORE	Conference series quality ranking, sub-fields classification http://www.core.edu.au/conference-portal
WOS, Scopus	Affiliation information, citations, scientific classifications of articles, sponsorship information
SNPL data	NPL citations from patents to conference proceedings Knaus and Palzenberger (2018) and Poege et al. (2019)
PATSTAT	Patent information, applicant and inventor names and addresses
ICAO, BTS	Direct flight connections, Airport regions https://www4.icao.int/newdataplus https://www.bts.gov/
ORBIS, GRID, EU Scoreboards	Firm names, ownership structure, industry information http://www.grid.ac https://ec.europa.eu/growth/industry/innovation/facts-figures/scoreboards_en

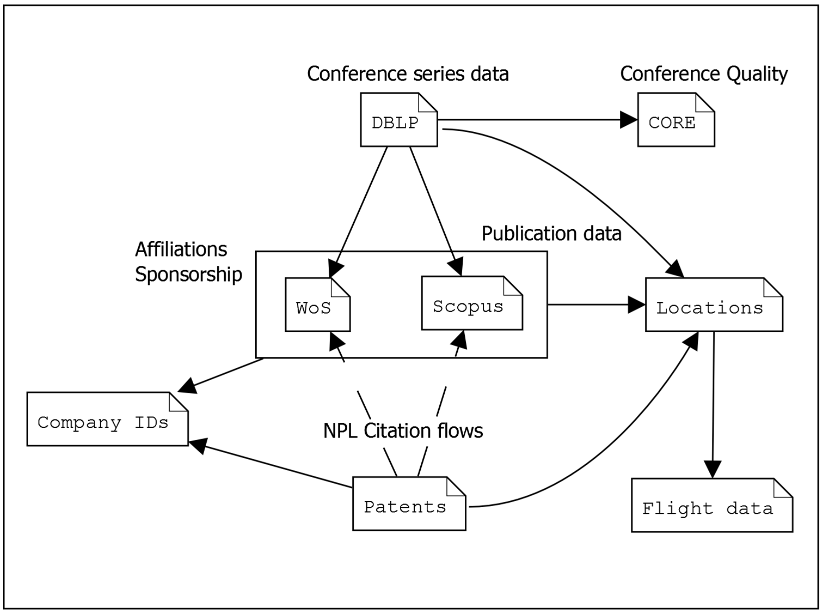


Figure B.1: Structure of the dataset

DBLP has a very broad coverage (Cavacini, 2015) and, compared to other sources, contains more consistent conference and conference series information. Additionally, DBLP supplies a high-quality author name disambiguation (Kim, 2018). DBLP has the highest coverage rate among specialized databases. The data provide an identifier for conference series. Conference event locations and dates are not available as independent fields but can be easily parsed from conference volumes titles. WoS and Scopus have a higher coverage rate, due to the coverage of other fields, but the information in DBLP is more consistent and representative for CS (Cavacini, 2015). Additional information on DBLP is available at dblp.uni-trier.de. A recent discussion of the disambiguation procedures is available at blog.dblp.org/2020/01/08/corrections-in-dblp-2019/.

Other relevant bibliographic information is missing in DBLP, which we obtain from Web of Science (WoS) and Scopus. Since Scopus is available to us from 1996 onwards, we focus our attention to those years. Both WoS and Scopus are widely used bibliometric databases with large coverage of different scientific fields, but possibly with lower coverage of specific fields relative to specialized databases like DBLP. The match between DBLP and the complete WoS and Scopus is done using the DOI and the cleaned title. Matches are verified using page numbers, publication years and author names and only matches showing sufficient overlap are kept. Necessarily, we drop conferences and conference proceedings for which no match is found in WoS or Scopus. We can match up to 90% of the DBLP entries with an item in WoS and/or Scopus.

We add information on conference series quality and CS research subfields from the Computing Research and Education (CORE) data, curated by the Computing Research and Education Association of Australasia. The CORE data classify conference series into the quality-rank levels $A^{\star}$, A, B and C and subfields. The CORE data constitutes an expert-based assessment of conference quality and subfields and is meant to cover a comprehensive set of all relevant conferences in CS. We match CORE to our data manually, partially supported by probabilistic string matching algorithms. We retain exclusively conference series which match with CORE ranking information and drop conference series which are unclassified. We use the latest available version of CORE, which provides the broadest coverage. Consequently, our rank classification is time-invariant. However, by comparing different versions of CORE rankings (2008, 2010, 2013, 2014, 2017, 2018) it is evident that changes in ranks are rare and in most cases minimal.

Table B.2: Observation counts

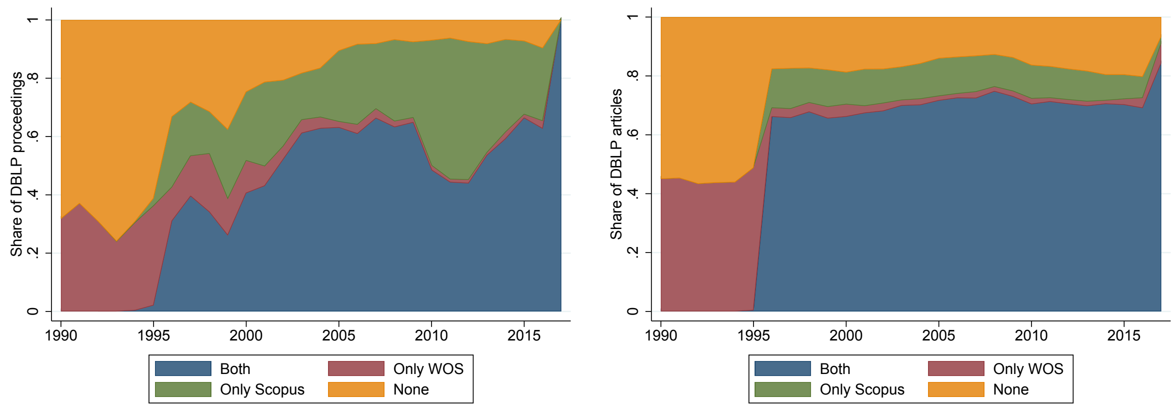
	Observation counts			
	All	WoS/Scopus	With CORE	≤ 2010
Dataset				
Proceedings	1617817	1444813	982548	612103
Conference Events	22404	20361	10973	7298
Conference Series	3767	3505	1087	1042
Firms				
All Firms		9941	7316	5470
Participants		9173	6791	5042
Sponsors		2121	1398	1027

Notes: Observation counts for different matching steps. Fourth column is the estimation sample. Third column from the right is relevant for the descriptive part. First column: All DBLP items. Second column: DBLP items found in WoS or Scopus. Third column: Also restricting to conference series matched with CORE. Last column: Also restricting to 1996-2010.

Table B.2 provides an overview of the number of observations in our data. Merging DBLP with WoS/Scopus and CORE inevitably reduces the number of available observations. Thanks to the combination of both, 90% of DBLP (since 1996) is maintained after matching with WoS and Scopus. The achieved coverage rate of DBLP in Scopus and Web of Science is displayed for proceedings in figure B.2a and for articles in figure B.2b, where after 1996, rates of 70-90% are observed. The full Scopus database is only available to us from 1996 onwards, which explains the lack of coverage before. Clearly, without Scopus, the analysis would lack representativeness, but the WOS adds around 10% in all years. This forces us to restrict our period of analysis to after 1996. Combining DBLP, WOS and Scopus guarantees to obtain the largest possible coverage of bibliographic information in CS in this period of time.

Figure B.2: DBLP items covered by WOS / Scopus

(a) Proceedings (b) Articles



Notes: Shows results of the DBLP-WOS/Scopus match. Match was based on DOI/title, cleaning using page numbers, publication years and author names.

The match with CORE data leads to a more substantial drop in the number of unique conference series and conference events originally covered. However, we verified that these are largely small and less relevant conferences, with few corresponding proceedings each. We still retain 982548 conference proceedings, corresponding to 70% of the initial total (the number of proceedings in DBLP matched with WoS or Scopus).

Most importantly, as noted in the paper, our data cover 75% of all conference series listed in CORE. Eighty percent of conference series listed in CORE and not in our data are of the lowest quality rank, C. This implies that the data cover almost the entirety of top and medium ranked conferences in CORE. In general, the sample is biased against small conference events, short-lived conference series, and conference series of the lowest quality, that are less likely covered in a generic bibliographic database as WoS or Scopus, and are less likely ranked in CORE.

We can claim that the data are largely representative of all relevant conference events in CS in our period of observation. Table B.2 also shows the difference between our estimation sample with years 1996-2010 and our full sample 1996-2015. Citation-based variables require time windows in which the citations can be observed. We choose five-year windows. This truncation issue forces us to limit to 1996-2010 the sample for econometric analyses. The full dataset (up to 2015) consists of a total of 10973 conference events in the 1996-2015 period – pertaining to 1087 conference series and more than one million proceedings. A total of 7316 firms have participated to at least one conference event, either authoring at least one conference proceeding or sponsoring a conference event. The sample up to 2010 comprises instead 5470 firms, 7298 conference events pertaining to 1042 conference series, and a total of 612103 of conference proceedings. In the remainder of the paper, we also present descriptive statistics limited to this sample.

Matching Firms

We generate a list of firm entities that we use as candidates. Our goal is to provide global coverage of the - probably - most important firms which conduct scientific publishing. For this reason, we use the firm names from the EU scoreboards as well as the firm included in GRID. The Scoreboard lists the by R&D expenditure top companies worldwide. In the first year of the list, 2003, 500 EU and 500 global companies are separately listed. Over time, the length of the lists was increased, so that the 2017 Scoreboard lists the top 1000 EU firms and worldwide the top 2500. The 2017 Scoreboard is the last included in our data. All in all, this adds roughly 8300 distinct firm name strings, of which often several refer to the same firm entity. For GRID, use a snapshot from May 2018. GRID, as a curated dataset of research-active entities is a prime candidate for adding firms likely involved in scientific activities. We only add entities labeled as company, which adds another roughly 21,000 match candidates. We further wanted to complement this list by firms who possibly use information from conferences in their technological activities, but do not publish frequently enough to occur in the curated GRID list. Therefore, we add all firm names for firms which in ORBIS were found to be connected to at least one patent. Also, we added firms from the US and DE section of ORBIS to try to capture smaller firms this way. Especially the latter part expands the set of firms too much by too many irrelevant candidates, so we did not further expand to additional countries.

Matching bibliometric information to firms is particularly hard as little additional information besides the affiliation string exists. Location information is often not given, as is the case for sponsor information. When it is available, it often does not refer to the headquarter location but to the particular research lab. Therefore, we try to enrich the affiliation name with contextual knowledge from the Internet, following the approach by Autor, Dorn, Hanson, et al. (2020). We search for the affiliation string in a search engine and retain the first ten results. We disregard very frequent occurrences, where for example many firms are listed on a single website. We also use frequency weighting to put a higher weight on less common entries.

The match uses the software package Dedupe (Gregg and Eder, 2019). Dedupe provides a probabilistic algorithm which, based on manually crafted training data, calculates weights for different input features. These input features are the web search-based similarities, but also traditional string similarity measures. Dedupe also calculates a minimum similarity threshold for which matches are kept. This is done based on a comparison of precision and recall scores. The matching step returns for each affiliation string a set of candidate firm strings which this affiliation string might belong to.

In the next step, we cluster the n:m match provided by Dedupe to group firm strings which belong to the same entity. In GRID, ORBIS and the EU Scoreboards, several possibilities for writing of the same firm name are possible. Additionally, firms may have been renamed, merged, acquired etc. Incidentally, the web search-based algorithm is by itself quite good at picking up these name changes. However, this step required a lot of manual refinement. Whenever multiple entity names were grouped, we validated these choices. When in doubt, the clustering implicitly and our validation explicitly clustered entities in larger groups. So, if two firms merged during a part of the sample time frame, we consider them to be the same entity for our full sample. Also, when the matching algorithm was not able to confidently distinguish subgroups of conglomerates, we grouped them into one entity. This happens with firms like Samsung or LG. The firm clusters yield our firm entities for this study.

Figure B.3 shows the sources of firm observations in the conference dataset. These give an overview over successful matches and of the extent of clustering. The number of firms in each category is weighted by the number of proceedings authored by (blue) and the number of conferences visited (red). Also, an unweighted count is provided. Most individual matches are from ORBIS only, followed by GRID only. However, the most important firms can actually be found in all three databases ("ORBIS+Scoreboard+GRID").

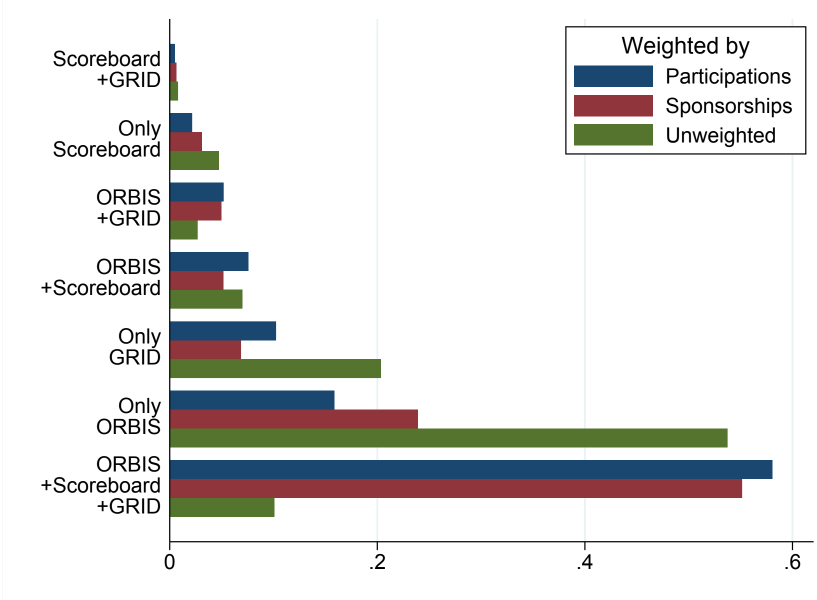
The advantage of our approach is to provide a very global, comprehensive firm dataset. Previous studies have typically only focused on large, listed US companies. Given the degree of internationalization observed in our data, this would substantially underestimate the role of firm science in CS. The disadvantage of not using one consistent firm dataset is that further descriptives at the firm level are difficult to obtain. For example, the GRID data does not contain any further firm-level information, whereas both Scoreboard and ORBIS do. We therefore being with match descriptives based on the Scoreboard only, which provides the most consistent set of firm-level information. Subsequently, we attempt to provide industry classifications for all firms.

We apply our match algorithm for a variety of data sources. At the core of this study is the match between affiliations for conference participants and sponsorship information found in WOS and Scopus. The most relevant of those are for CS conferences also found in DBLP. However, for figure B.8, conferences in other fields are also informative. Further, we use the same matching strategy to match firm applicants from patents citing computer science proceedings or otherwise relevant for computer science (the technology main area 'Electrical Engineering'). Due to this broader match target set, there can be a number of firms which are matched to some affiliation or applicant string but never occur in the computer science dataset.

Table B.3 shows, for the subsample of the 2010 Scoreboard, which share of entities can be matched. We focus on one Scoreboard slice as combining several slices would create a distorted sample. Large companies are always retained, whereas small companies would frequently enter and leave the yearly lists. In the 2010 Scoreboard, we can find 69.5% of the Scoreboard companies in any data source, including conferences, journal publications, conferences outside of CS and relevant patents. However, also 69.5% participated to a conference. The shares are necessarily higher in some sectors and smaller in others. In Telecommunication Services or Telecommunication Hardware and Equipment, more than 70% of all firms ever participate to a conference. However, in all sectors, some companies show some engagement with the academic community.

Intensity of participation and sponsorship also varies substantially across sectors. Table B.3 shows this in columns 4-7. These columns show the share of Scoreboard firms that ever participated or sponsored a CS conference in the 1996-2015 time period as well as the average number of conferences they participated to or sponsored. As some examples, the sector "Software and Computer Services" contains both smaller IT companies as well as the global players of IT. "Leisure Goods" contains some companies involved in

Figure B.3: Sources of firm data



Notes: Shows the data sources of firms in the dataset. Firm data is taken from the EU Scoreboards, ORBIS as well as GRID. The number of firms in each category is weighted by the number of proceedings authored by (blue) and the number of conferences visited (red). An unweighted count is provided as well (green).

electronic entertainment among a greater number of companies unrelated to CS. This explains the low participation share but high average participation intensity.

We can also classify firms outside of the Scoreboards into industries. The industries by firms are taken from the respective data sources. In the Scoreboard, the classification according to the 'Industry Classification Benchmark' (ICB) is taken. In ORBIS, the 4-digit NACE2 classification is translated into the ICB classification. For the 2016 Scoreboard, we have a direct correspondence to ORBIS, from which we construct a probabilistic match between NACE2 and ICB. This one is extrapolated for the remaining ORBIS entries. GRID on the other hand lacks any firm-level properties. The website URL is available, however. We use this and website addresses from ORBIS to attempt a linkage between GRID and ORBIS. Conditional on a matching website, we require a close string similarity for a match. With this, we further get ICB information for GRID-only entities. When for one firm cluster, more than one source of industry information is available, all sources are weighted equally. With this, we can describe the industries present in our estimation sample.

Figure B.4 shows a distribution of firms across business sectors. The number of firms in each category is weighted by the number of proceedings authored by (blue) and the number of conferences visited (red). An unweighted count is provided as well (green). As expected, business sectors traditionally associated with computer science such as "Software and Computer Services" or "Technology Hardware" are very important. However, also firms from a variety of other sectors are occasionally present at conferences.

Constructing Scientist-Firm Biographies

For the collaboration and hiring variables, and for self-citations and scientist counts, we rely on disambiguated scientist profiles and affiliation information combined with firm information. The scientist

Table B.3: Descriptives: Firms at conferences (Scoreboard 2010 subsample)

	All	Ever Matched		Participation		Sponsorship	
	N	N	Share	Ever	Total	Ever	Total
Aerospace & Defense	46	37	0.80	0.54	17.52	0.20	0.76
Chemicals	109	91	0.83	0.17	5.14	0.04	0.29
Construction & Materials	69	38	0.55	0.19	1.17	0.04	0.12
Electrical Components	148	108	0.73	0.37	16.01	0.14	1.14
Engineering & Machinery	272	160	0.59	0.31	5.98	0.07	0.27
Finance	80	37	0.46	0.15	0.34	0.06	0.11
Food Producers	67	43	0.64	0.15	0.43	0.03	0.03
General Industrials	56	31	0.55	0.25	13.29	0.11	0.57
General Retailers	20	10	0.50	0.20	1.45	0.00	0.00
Health Care Equipment	73	56	0.77	0.36	1.01	0.04	0.05
Industrial Metals & Mining	36	29	0.81	0.36	1.03	0.00	0.00
Industrial Transportation	15	9	0.60	0.40	0.93	0.00	0.00
Leisure Goods	28	17	0.61	0.32	30.57	0.18	1.82
Media	17	11	0.65	0.41	4.76	0.12	1.18
Oil & Gas	36	33	0.92	0.47	3.44	0.17	0.44
Personal Goods	60	30	0.50	0.13	0.43	0.05	0.10
Pharmaceuticals & Biotech	229	185	0.81	0.13	0.72	0.05	0.06
Software & Computer Services	207	124	0.60	0.43	39.43	0.22	6.00
Support Services	37	21	0.57	0.32	7.14	0.14	0.68
Technology Hardware	253	214	0.85	0.67	30.16	0.23	2.89
Telecommunication Services	26	23	0.88	0.81	109.31	0.54	5.35
Travel & Leisure	26	13	0.50	0.23	1.62	0.08	0.12
Utilities	58	48	0.83	0.33	2.03	0.07	0.10
Total	1968	1368	0.70	0.34	13.5	0.12	1.3

Notes: Shows the number of firms with conference participation and their activities, exemplary for the 2010 Scoreboard. The first column shows the number of firms, by industry. While the Scoreboard contains overall 2000 entries, in some cases multiple parts of conglomerates are listed separately, for example Samsung. In the matching process, these cannot be distinguished with high accuracy and are joined into one entity. The second column shows the number and share of firms that could ever be matched in any target dataset, including CS conferences and journals, conferences outside of CS and relevant patents. The remaining firms could never be matched. Columns four to seven show the share of Scoreboard firms that ever participated or sponsored a CS conference in the 1996-2015 time period. As almost all firms who sponsor also participate, the 'Ever Participation' shares also show shares of firms found in the conference dataset. Columns five and seven show the total number of conferences attended as well as the number of sponsored conferences.

profiles are taken from DBLP, but for the affiliation information, we rely on data from WOS and Scopus. While for the majority of the data, only a paper-firm link is relevant, here a paper-person-firm link is required. To achieve this, we match at the paper level individual authors from DBLP to individuals authors and corresponding affiliations in Scopus, and if not available, in WoS.

With this, we establish a person-year panel and compute fractional association of individual scientists with firms or academia. Whenever a scientist is associated with a firm on a journal article or conference proceeding, that information is also taken into account. If in a given year a scientist features different affiliations from one or several papers, fractional counts are used. In years where the scientist did not publish, linear interpolations from years before and after are used for the variable on firm size of research investments.

There is a small share of cases where the individual information of affiliation cannot be retrieved. A small part of this issue is due to missing affiliation information. The rest comes from a limitation of WoS, where there is no direct link between the the author list and affiliation list, that they are simply listed uniquely in their order of appearance. For this reason, when available, we prefer information from Scopus, which is essentially complete. We also mitigate this issue as far are possible in WoS: the first

Figure B.4: Industries of firms

Notes: Shows the industries of firms in the dataset. Industry information is taken from the Scoreboards or ORBIS, for GRID via an ad-hoc match with ORBIS. The number of firms in each category is weighted by the number of proceedings authored by (blue) and the number of conferences visited (red). An unweighted count is provided as well (green).

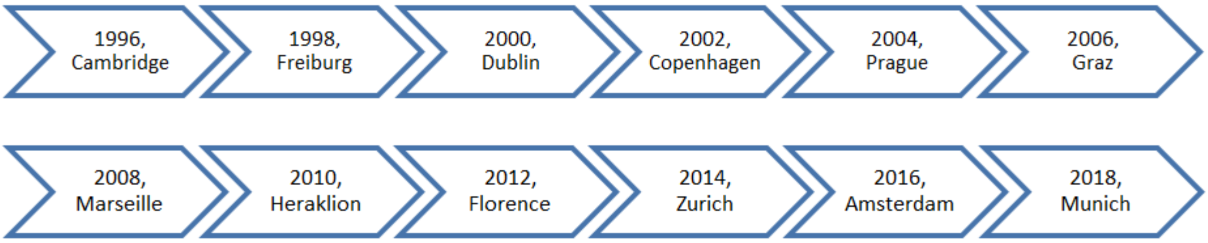
affiliation can always be assigned to the first person or papers with only one affiliation can be assigned completely. Still, some cases remain where the information is missing.

B.2 Investigating the First Stage

B.2.1 Event Study: Direct Flights and Participation

We design an event study on the dynamics of the effect of direct flights on the probability of participation of scientists to conferences. The variation in the availability of direct flights derives from new airlines routes and from the changes in venues of conference series. As an example, figure B.5 shows the case of the European Conference on Computer Vision (ECCV), an A-ranked conference in AI/Machine vision. The conference takes place every two years at varying places in Europe and the locations are decided roughly with 4 years of advance. In our most conservative models, the identifying variations would derive from the possibility to access the conference venues with direct flights, for scientists outside the country where the conference is hosted, after controlling for time-specific FE of their locations, and for their geographic distance. In robustness analyses, we also control for the specific conference FE.

In the event study setting, we can explore, for instance, whether any anticipation in participation exists. We collapse our data and build a panel at the level of scientists locations and conference series pair-level. We use as dependent variables the number of researchers from a region that participate in the conference in a given period and, alternatively, a dummy equal 1 if at least one scientist from that region participates. We construct variables on the change of direct flight availability. If in this period, relative to the previous period, a direct flight connection to the conference series becomes available, the direct flight indicator is 1. If a direct flight connection is no longer available, the indicator is -1. If there is no change, the indicator is 0. Note that some conferences like the ECCV do not occur every year, which is why we are using relative time periods.



Notes: ECCV: European Conference on Computer Vision. We visited this conference in 2018 and discuss findings in section 2.3.

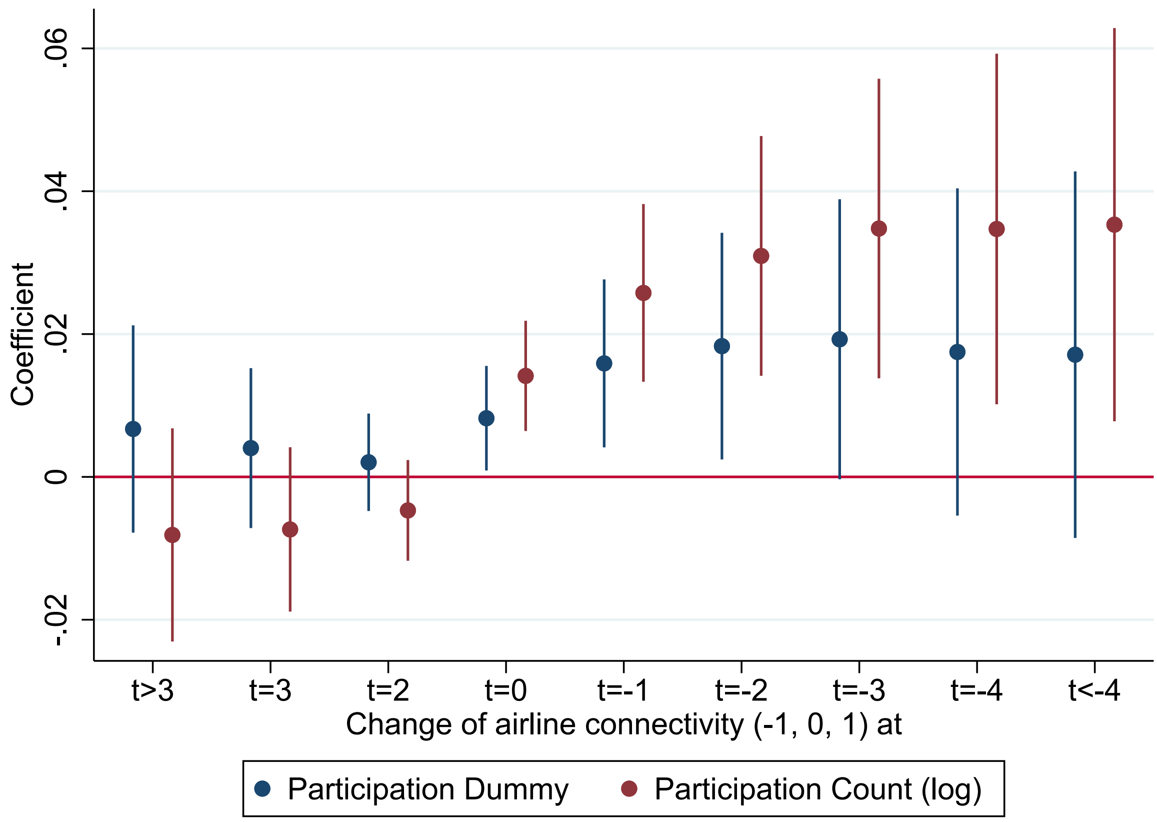
Figure B.5: ECCV locations over time

$$y_{rct} = \sum_{j=-4}^{5} \gamma_j D^j_{rct} + \mu_{rt} + \mu_{rc} + \mu_{ct} + \beta X_{rct} + \epsilon_{rct} \tag{B.1}$$

More formally, we look at a panel of researchers' region r, conference series c by time t in an event-study setting. The endpoints are binned, following the suggestion of Schmidheiny and Siegloch (2020). For each period j, the variable D^j_{rct} takes value 1 if a direct flight is introduced, value -1 if a direct flight is removed, and 0 if no change occurs. The coefficient γ_j captures the effect of a positive change. The period $j-1$ is used as a baseline. Also, we control for FE on the region-year, region-conference series and conference series-year level (single conference event). We add the same control variables on a researcher region-conference-level which are also employed in the main text. We cluster on the conference series-researcher region level.

We restrict the combinations to all region-conference series combinations to various sets of candidate regions. The most narrow set is defined by all regions from which at least one researcher ever attends the focal conference. The results from this specification are shown in figure B.6 and columns 3/6 of table B.4. The second definition considers all location with at least one researcher active in the conference's field. This is shown in columns 2/5. Finally, the third definition considers all possible locations. Here, columns 1/4 of table B.4 are relevant. Note that the notation in the tables and figure displays negative time period values for periods after the event and positive for periods before the event: this reads, the effect of having a direct flight available in, say, 2 years in the future (positive values) or the effect of a direct flight having become available and maintained 2 years in the past (negative values).

Figure B.6 plots the regression results of equation B.1, for log-transformed participation counts and the participation binary dummy variable. Table B.4 lists detailed estimation results for the log-transformed counts. Results for the dummy variable as dependent variable are equivalent and available upon request. There is a relevant and statistically significant increase in the number of participants. This effect starts immediately at the point of the flight introduction and remains present, but no pre-trends can be found. The flight-induced participation seems to be persistent and slightly increasing in subsequent years if a direct flight connection persists. Based on the regression results, pre-trends or anticipation effects are likely not a concern. All other coefficients behave as expected. When the conference is in their home region, researchers are more likely to attend. Researchers are less likely to attend distant conferences.



Notes: Coefficients from linear regression with 95% confidence intervals. FE for region×year, region× conference series and conference events included. Other controls include distance (log), indicator variables for conference being held at the researcher region and for domestic flight connections. Clustering is on region×conference series level.

Figure B.6: Event study estimates of the effect of direct flights on participation (Region-conference series level of analysis)

Table B.4: Event study regression results of the effect of direct flights on participation (Region-conference series level of analysis)

Participants	(1) log(1+)	(2) log(1+)	(3) log(1+)	(4) log(1+)	(5) log(1+)	(6) log(1+)
Domestic (State)	0.012***	0.018***	0.076***	0.012***	0.019***	0.082***
	(0.001)	(0.001)	(0.006)	(0.001)	(0.002)	(0.007)
Same region	0.169***	0.159***	0.044**	0.183***	0.173***	0.048**
	(0.014)	(0.014)	(0.019)	(0.017)	(0.017)	(0.023)
Distance (log)	−0.002***	−0.003***	−0.018***	−0.002***	−0.003***	−0.019***
	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.001)
t>3				−0.001	−0.001	−0.008
				(0.002)	(0.003)	(0.008)
t>2	−0.001	−0.001	0.000			
	(0.001)	(0.001)	(0.004)			
t=3				−0.002	−0.003	−0.007
				(0.002)	(0.002)	(0.006)
t=2	−0.001	−0.001	0.000	−0.002	−0.002*	−0.005
	(0.001)	(0.001)	(0.003)	(0.001)	(0.001)	(0.004)
t=0	0.004***	0.005***	0.010***	0.005***	0.006***	0.014***
	(0.001)	(0.001)	(0.003)	(0.001)	(0.001)	(0.004)
t=-1	0.007***	0.009***	0.017***	0.010***	0.012***	0.026***
	(0.001)	(0.002)	(0.005)	(0.002)	(0.002)	(0.006)
t=-2	0.007***	0.009***	0.018***	0.011***	0.012***	0.031***
	(0.002)	(0.002)	(0.006)	(0.002)	(0.003)	(0.009)
t=-3	0.007***	0.008***	0.018**	0.011***	0.013***	0.035***
	(0.002)	(0.002)	(0.007)	(0.003)	(0.004)	(0.011)
t=-4				0.011***	0.012***	0.035***
				(0.003)	(0.004)	(0.013)
t<-3	0.006***	0.007**	0.014*			
	(0.002)	(0.003)	(0.008)			
t<-4				0.009**	0.010**	0.035**
				(0.004)	(0.005)	(0.014)
Region set	All	Field	Attendance	All	Field	Attendance
Conf. distance controls	Yes	Yes	Yes	Yes	Yes	Yes
Origin × Conf. Ser. FE	Yes	Yes	Yes	Yes	Yes	Yes
Origin × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Year × Conf. Ser. FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.467	0.473	0.505	0.519	0.523	0.534
Observations	4014488	2318856	303520	2600869	1491435	202066
Number clusters	668026	390677	48110	485981	281388	36601

Notes: * $p < .1$, ** $p < .05$, *** $p < .01$ Standard errors in parentheses, clustered at the conference series times researcher region level. Regression results for equation B.1. In columns 3/6 all regions from which at least one researcher ever attends the focal conference are considered. All location with at least one researcher active in the conference's field are considered in columns 2/5. All possible locations are considered in columns 1/4.

B.2.2 First Stage Heterogeneity

We investigate how the strength of the instrumental variable varies in the first stage by interacting it with key characteristics of conferences. We estimate regression B.2 with various sets of heterogeneity dimensions h. Generally speaking, we expect direct flights to matter more when the researchers are otherwise indifferent between similar conferences along with our matching criteria. For example, we expect researchers to spare less to attend $A^{\star}$ or A conferences. For lower-level conferences, the discomfort of traveling might start to play a stronger role. Consequently, we expect the instrument to have less relevance for highly-ranked conferences and more for low-ranked conferences. Similarly, the availability of direct flights should matter more at long distances. We maintain the same observation level than for our main analyses in the paper, detailed in section 2.6.

$$\begin{aligned} D\{\text{p presented at c}\}_{fpc} = & \sum \beta_{1i} D\{\text{direct flight}\}_{fpc} \times h_{pi} \\ & + \sum \beta_{2i} log\,(1 + \text{distance})_{fpc} \times h_{pi} \\ & + \sum \beta_{2i} D\{\text{direct flight}\}_{fpc} \times log\,(1 + \text{distance})_{fpc} \times h_{pi} \\ & + \sum \beta_{3i} h_{pi} + \beta_4 X_{fpc} + u_{fpc} \end{aligned} \tag{B.2}$$

Results are presented in Table B.5. Column 1 corresponds to our main specification. Column 2 shows that the effect of the instrument is larger for conferences at longer distances. Also as expected, it turns out that the quality level of the conference matters. For models without distance controls (column 3), the instrumental variable is significant for all rank levels of conferences, but it is already stronger in magnitude for lower ranked-conferences. In the most conservative models, including distance controls, the coefficient size for $A^{\star}$ and A conferences is comparatively small and weekly significant (column 4). However, the strength of the instrument increases for longer geographic distances and becomes large and significant at long distances for all quality levels. We see this in column 5 where we interact the distance of conferences with the effect of *Direct flight* for conferences of different ranking (triple interactions). In these models, the value of the distance variable is centered at the mean, so that the coefficient on the interacting variables can be interpreted as effects at the mean (corresponding to approximately 3600 km, 8.2 in logarithm).

Table B.5: First stage - Heterogeneity of the effect of *Direct flight* on *Participation*

	(1) Participation	(2) Participation	(3) Participation	(4) Participation	(5) Participation
Direct flight	0.030***	0.033***			
	(0.006)	(0.006)			
$A^{\star}$/A-level × DF			0.039***	0.010*	0.018***
			(0.005)	(0.005)	(0.005)
B/C-level × DF			0.077***	0.049***	0.046***
			(0.009)	(0.008)	(0.008)
log(Distance)	−0.039***	−0.048***		−0.039***	
	(0.003)	(0.004)		(0.003)	
$A^{\star}$/A-level × log(Distance)					−0.039***
					(0.004)
B/C-level × log(Distance)					−0.055***
					(0.005)
Direct flight × log(Distance)		0.023***			
		(0.005)			
$A^{\star}$/A-level × DF × log(d)					0.024***
					(0.006)
B/C-level × DF × log(d)					0.020***
					(0.005)
Same airport	−0.164***	−0.231***		−0.164***	−0.235***
	(0.036)	(0.039)		(0.036)	(0.039)
Domestic (State)	0.132***	0.128***		0.132***	0.126***
	(0.015)	(0.015)		(0.015)	(0.015)
Conf Ser FE	Yes	Yes	Yes	Yes	Yes
Origin × Field FE	Yes	Yes	Yes	Yes	Yes
Origin × Firm FE	Yes	Yes	Yes	Yes	Yes
Year × Origin FE	Yes	Yes	Yes	Yes	Yes
Year × Firm FE	Yes	Yes	Yes	Yes	Yes
Cluster	Origin	Origin	Origin	Origin	Origin
Number clusters	1114	1114	1114	1114	1114
R^2	0.333	0.334	0.323	0.334	0.334
Observations	5126273	5126273	5126273	5126273	5126273

Notes: * $p < .1$, ** $p < .05$, *** $p < .01$ Standard errors in parenthesis, clustered at the researcher region level. The dataset follows the description of table 2.3. The value of log(Distance) is centered at the mean value in the regressions. The sample mean of log(Distance) is about 8.2, corresponding to approximately 3600 km.

B.3 Supplementary Figures

Figure B.7: Stylized empirical setup

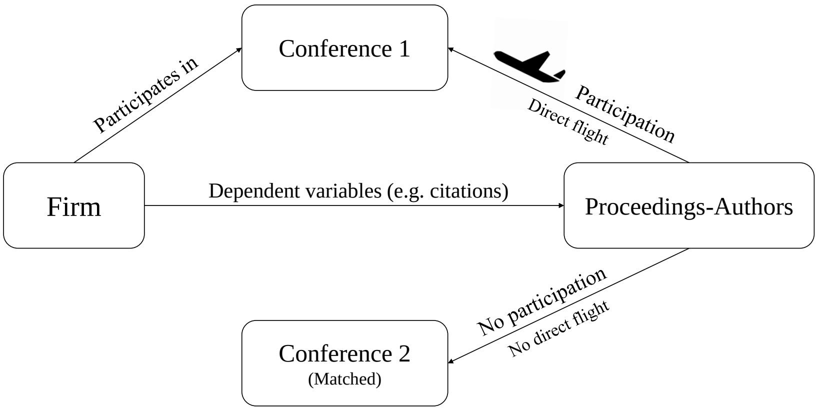
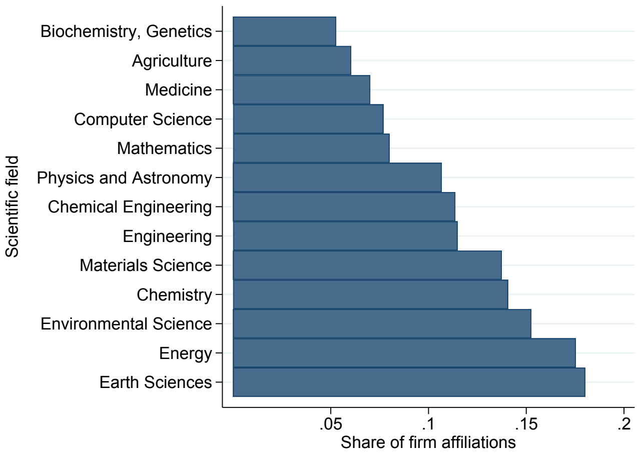
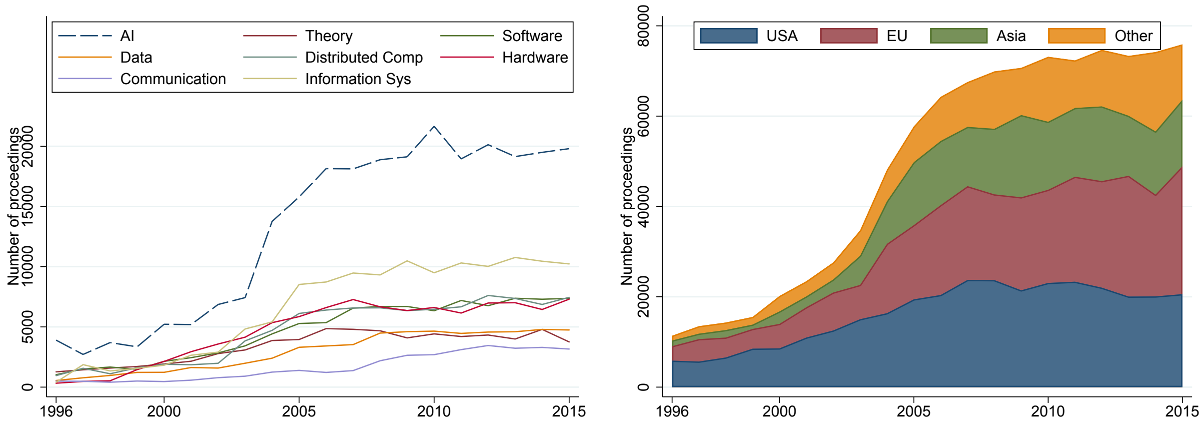


Figure B.8: Share of firms-authored conference proceedings by field



Notes: Fields are identified based on ASJC codes from Scopus between 1996-2015. Largest fields (millions of proceedings) are Engineering (2.19), CS (1.49), Physics (0.53), Mathematics (0.30), Material Science (0.28), Energy (0.18). Smallest fields are Agriculture (0.02), Biochemistry (0.02), Chemistry (0.03), Medicine (0.03), Environmental Science (0.08). Fields with less than 15,000 items or in social sciences or humanities are disregarded.

Figure B.9: Conference proceedings by field and conference locations



(a) Size of different CS fields

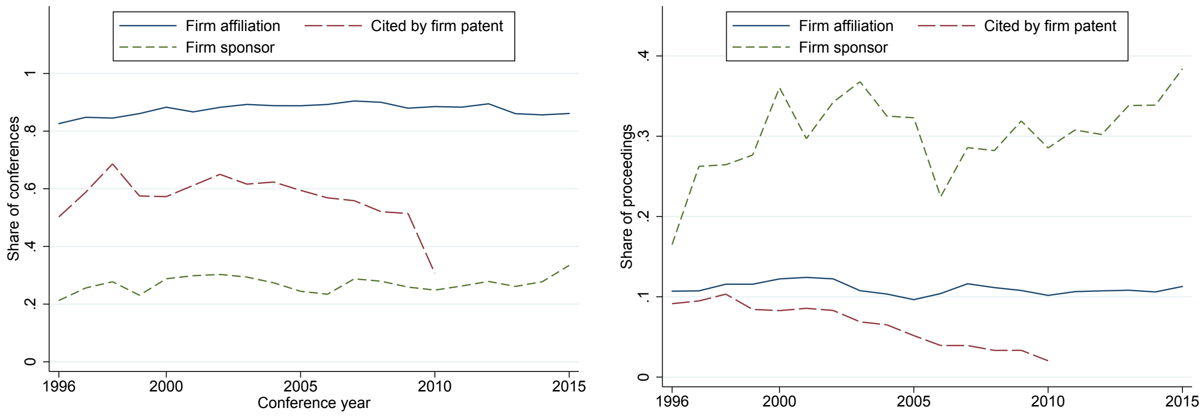
(b) Conference locations

Notes: Only DBLP conferences with matched Web of Science/Scopus articles as well as CORE information are considered. Conferences in panel B.9a are assigned to their first CORE field code.

Figure B.10: Firm participation and patent citations

(a) Share of conferences

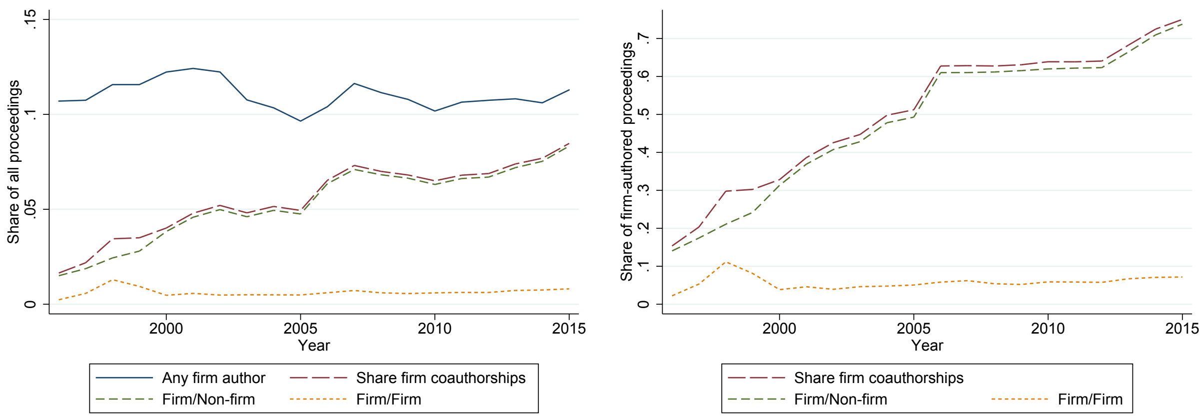
(b) Share of proceedings



Notes: Shows activity of firms on the conference (B.10a) and proceeding (B.10b) level. In B.10b, sponsorship refers to a proceeding at a conference with at least one firm sponsor. Due to truncation we restrict the data based on patent citations to the year 2010 and before. The negative tendency is still likely the artifact of citations data truncation.

Figure B.11: Coauthorships of CS proceedings with a firm

(a) Coauthorships with a firm overall (b) Coauthorships on firm proceedings



Notes: Coauthorships of firms a different firm or institution on conference proceedings. In B.11a, the share of all proceedings with firm authors is decomposed in proceedings with coauthorships and such without, further split up in such with firm-firm and firm-academia coauthorships. B.11b plots the same decomposition conditional on having at least one firm author.

B.4 Supplementary Tables

Table B.6: Covariate balancing table.

	Average value (Actual conf)	Difference (SE) (Counterfactual-Actual)		p-value
Exact matching criteria				
Year	2005.22	0.000	(0.000)	1.00
Rank: A*	0.10	0.000	(0.000)	1.00
Rank: A	0.28	0.000	(0.000)	1.00
Rank: B	0.36	0.000	(0.000)	1.00
Rank: C	0.25	0.000	(0.000)	1.00
Field: General CompSci	0.08	0.000	(0.003)	0.94
Field: General Engineering	0.05	−0.001	(0.001)	0.46
Field: AI / Computer Vision	0.21	0.000	(0.001)	1.00
Field: Computation Theory	0.16	0.000	(0.001)	1.00
Field: Computer Software	0.22	0.000	(0.001)	1.00
Field: Data Format	0.10	−0.001	(0.002)	0.55
Field: Distributed Computing	0.13	0.000	(0.001)	0.80
Field: Information Systems	0.13	−0.002	(0.002)	0.49
Coarsened matching criteria				
Size of the conference	70.40	0.022	(0.563)	0.97
Mean 5-year citations	4.46	0.005	(0.056)	0.93
Untargeted matching criteria				
Conference series age	5.27	−0.042	(0.052)	0.42
Number of fields	1.10	−0.002	(0.006)	0.70
Number of sponsors	1.34	−0.010	(0.018)	0.56
Number of firms	4.95	−0.021	(0.059)	0.72
Observations	5799	10492		

Notes: Covariate balancing for two counterfactual conferences. Shows the average deviation of the counterfactual conference from the actual conference.

Table B.7: CORE fields.

	Share (first)	Count	Share (freq)	Count
Computer Science (general)	7.3	41754	7.1	40476
Engineering (general)	15.1	85837	15.4	87747
Design (general)	0.0	0	0.1	518
Artificial Intelligence and Image Processing	28.8	163843	28.7	163258
Computation Theory and Mathematics	8.2	46612	8.1	46213
Computer Software	10.2	57895	10.1	57468
Data Format	6.4	36332	6.4	36427
Distributed Computing	10.2	58017	9.6	54782
Information Systems	13.8	78812	13.9	79252
Library and Information Studies	0.0	0	0.5	2962
Total	100.0	569102	100.0	569102

Notes: CORE fields as aggregated in the conference-level match are shown. Each conference series is associated with up to three CORE fields. Shares and counts using the first or using equal weighting among the CORE fields is shown. 1996-2015 data is shown.

Table B.8: Scientific and commercial value of corporate proceedings.

log 5y	(1) Science Citations	(2) Science Citations	(3) Science Citations	(4) Science Citations	(5) Science Citations	(6) Science Citations
Firm	0.262***	0.239***	0.063***	0.056***	0.050***	0.044***
	(0.011)	(0.011)	(0.006)	(0.006)	(0.005)	(0.005)
Sponsor	0.088***	0.079***	0.008	0.005		
	(0.023)	(0.023)	(0.014)	(0.014)		
Firm=Sponsor		0.389***		0.125***		0.109***
		(0.044)		(0.028)		(0.024)
Year FE	Yes	Yes	Yes	Yes		
Conf FE					Yes	Yes
Conf Series FE			Yes	Yes		
R^2	0.015	0.016	0.260	0.260	0.326	0.326
Clusters	7298	7298	7295	7295	7217	7217
Obs	612103	612103	612100	612100	612022	612022

log 5y	(1) Patent Citations	(2) Patent Citations	(3) Patent Citations	(4) Patent Citations	(5) Patent Citations	(6) Patent Citations
Firm	0.095***	0.091***	0.069***	0.066***	0.067***	0.064***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Sponsor	0.018***	0.016***	0.012***	0.011***		
	(0.004)	(0.004)	(0.003)	(0.003)		
Firm=Sponsor		0.072***		0.055***		0.038***
		(0.014)		(0.010)		(0.009)
Year FE	Yes	Yes	Yes	Yes		
Conf FE					Yes	Yes
Conf Series FE			Yes	Yes		
R^2	0.026	0.027	0.085	0.086	0.139	0.139
Clusters	7298	7298	7295	7295	7217	7217
Obs	612103	612103	612100	612100	612022	612022

Notes: * $p < .1$, ** $p < .05$, *** $p < .01$ Standard errors in parentheses clustered on the conference level. $log(1 + \text{cit})$ for a window of five years is used as the outcome variable. Outcome variables: Forward citations of DBLP conference proceedings / patent families by DBLP items within five years. Mean science citations: 3.45 . We analyze how citations by proceedings received within five years are different for proceedings authored by firms. Additionally, we test whether proceedings authored by a firm-sponsor receive more citations. We include as regressors a dummy indicating whether at least one author is affiliated to a firm, *Firm*, a dummy indicating whether the conference where a proceeding is presented is sponsored by a firm, *Sponsor*, and one dummy indicating whether the presenting firm is also a sponsor, *Firm=Sponsor*. In all regressions, we control for year FEs. In columns (3) and (4), conference series FE capture time-invariant quality and field differences across conference series. In columns (5) and (6), conference event FE also leave out all variation except within individual conferences. We find that proceedings authored by firms receive on average more citations. The coefficient decreases when controlling for conference series FE and conference event FE, but remains highly significant. This implies that firms tend to present research in conference series of the highest quality, but also within conference series and single conferences, proceedings authored by firms receive more citations. Overall, scientific articles which are associated with at least one firm receive roughly 4.4% more citations than other proceedings in the same conference as seen in column 5. The results on sponsorship suggest that corporate sponsorship is concentrated among high-quality conferences. However, this effect fully stems from firms choosing to sponsor high-quality conference series, rather than individual conferences within conference series (compare columns 1/2 and 3/4). Since sponsorship is defined at the conference level, it cannot be included in columns 5 and 6. When presenting and sponsoring coincide, the proceeding receives especially many citations. Column 6 shows that within a conference, firm citations where the firm is also sponsoring receive 11% more citations compared to proceedings where the firm does not also sponsor. These descriptive results do not imply any causality. Possibly, proceedings receive additional attention through the advertising of sponsorship, so that sponsoring creates an additional halo effect which leads to more visibility and follow-on research. This would constitute a causal mechanism and our observations at conferences suggest such a possibility. However, equally likely firms especially sponsor when they expect to also present strong research at a conference.

Table B.9: Heterogeneity: Sponsorship

	(1) Science cit (present)	(2) Science cit (past)	(3) Patent cit (present)	(4) Patent cit (past)	(5) Collaboration	(6) Hiring
Participation	0.021***	0.112***	0.001	0.097***	0.073***	0.005
	(0.008)	(0.042)	(0.003)	(0.025)	(0.021)	(0.008)
Participation× Sponsor	0.058***	0.190***	0.003	0.108**	0.112***	0.045***
	(0.019)	(0.056)	(0.008)	(0.043)	(0.042)	(0.017)
Sponsor	−0.029***	−0.097***	0.000	−0.049**	−0.051**	−0.022**
	(0.010)	(0.031)	(0.004)	(0.024)	(0.023)	(0.009)
Science citations (L)	0.028***	0.391***	0.003***	0.156***	0.146***	0.036***
	(0.001)	(0.006)	(0.000)	(0.003)	(0.004)	(0.001)
Patent citations (L)	0.008***	0.183***	0.003***	0.194***	0.073***	0.012***
	(0.002)	(0.006)	(0.001)	(0.006)	(0.006)	(0.002)
Research similarity (L)	0.023***	0.234***	0.006**	0.009	0.044**	0.020***
	(0.007)	(0.044)	(0.003)	(0.025)	(0.019)	(0.007)
Conf. distance controls	Yes	Yes	Yes	Yes	Yes	Yes
Conf Ser FE	Yes	Yes	Yes	Yes	Yes	Yes
Origin × Field FE	Yes	Yes	Yes	Yes	Yes	Yes
Origin × Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year × Origin FE	Yes	Yes	Yes	Yes	Yes	Yes
Year × Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.076	0.366	0.031	0.185	0.190	0.084
Observations	5126273	5126273	5126273	5126273	5126273	5126273
Number clusters	1114	1114	1114	1114	1114	1114
DV cond. mean	0.010	0.158	0.002	0.050	0.048	0.011
F (First)	16.6	16.6	16.6	16.6	16.6	16.6

Notes: * $p < .1$, ** $p < .05$, *** $p < .01$ Standard errors in parenthesis, clustered at the researcher region level.

Table B.10: Robustness - Science citations timing

	(1) Science cit ($t = 0$)	(2) Science cit ($t \leq 1$)	(3) Science cit ($t \leq 2$)	(4) Science cit ($t \leq 3$)	(5) Science cit ($t \leq 4$)	(6) Science cit ($t \leq 5$)
Participation	0.001	0.006	0.011**	0.014*	0.016**	0.021***
	(0.001)	(0.004)	(0.005)	(0.007)	(0.007)	(0.007)
Science citations (L)	0.001***	0.009***	0.016***	0.022***	0.026***	0.029***
	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Patent citations (L)	0.000	0.003***	0.004***	0.006***	0.007***	0.008***
	(0.000)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
Research similarity (L)	0.001	0.008**	0.014***	0.020***	0.024***	0.025***
	(0.001)	(0.004)	(0.005)	(0.007)	(0.006)	(0.006)
Conf. distance controls	Yes	Yes	Yes	Yes	Yes	Yes
Conf Ser FE	Yes	Yes	Yes	Yes	Yes	Yes
Origin × Field FE	Yes	Yes	Yes	Yes	Yes	Yes
Origin × Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year × Origin FE	Yes	Yes	Yes	Yes	Yes	Yes
Year × Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.021	0.038	0.054	0.067	0.077	0.083
Observations	5126273	5126273	5126273	5126273	5126273	5126273
Number clusters	1114	1114	1114	1114	1114	1114
DV cond. mean	0.000	0.003	0.005	0.007	0.009	0.010
F (First)	31.6	31.6	31.6	31.6	31.6	31.6

Notes: Columns 1-6 analyze the probability that at least one citation from the firm to the focal proceeding occurred within t years, from $t = 0$ to $t = 5$. For $t = 5$, this corresponds to the default dependent variable. * $p < .1$, ** $p < .05$, *** $p < .01$ Standard errors in parenthesis, clustered at the researcher region level. Firm-proceeding level dataset, where some proceedings were actually at the conference (Participation=1) and some were at another conference (Participation=0). Participation is instrumented by the direct flight availability between the researcher location and the conference location. Firm-proceeding controls include whether the firm cited previous work by the authors in the years before the conference (Science/Patent citations L) and the average abstract similarity between proceedings published by the firm in the previous year and the focal proceeding (Research Similarity L). Dependent variable mean is for actually presented proceedings. Conf. distance controls include the distance between researcher and conference location (log), whether that distance is zero and whether the two locations are in the same US state or non-US country.

Table B.11: Heterogeneity: Participation intensity for citations (linear specification)

	(1) Science cit (present)	(2) Science cit (past)	(3) Patent cit (present)	(4) Patent cit (past)	(5) Collaboration	(6) Hiring
# Firm's proceedings× Participation	0.013***	0.063***	0.003**	0.050***	0.046***	0.007**
	(0.004)	(0.013)	(0.001)	(0.010)	(0.009)	(0.003)
# Firm's proceedings	−0.005**	−0.028***	−0.001	−0.025***	−0.022***	−0.002
	(0.003)	(0.009)	(0.001)	(0.006)	(0.006)	(0.002)
Sponsor× Participation	0.080***	0.151***	0.016***	0.181***	0.172***	0.050***
	(0.017)	(0.028)	(0.006)	(0.033)	(0.028)	(0.013)
Sponsor	−0.041***	−0.074***	−0.008***	−0.091***	−0.085***	−0.025***
	(0.010)	(0.015)	(0.003)	(0.018)	(0.016)	(0.007)
Science citations (L)	0.026***	0.383***	0.002***	0.150***	0.138***	0.034***
	(0.001)	(0.005)	(0.000)	(0.004)	(0.004)	(0.002)
Patent citations (L)	0.006***	0.175***	0.002***	0.189***	0.067***	0.011***
	(0.002)	(0.006)	(0.001)	(0.006)	(0.006)	(0.002)
Research similarity (L)	0.014**	0.210***	0.002	−0.002	0.017	0.011*
	(0.007)	(0.031)	(0.002)	(0.020)	(0.018)	(0.006)
Conf. distance controls	Yes	Yes	Yes	Yes	Yes	Yes
Conf Ser FE	Yes	Yes	Yes	Yes	Yes	Yes
Origin × Field FE	Yes	Yes	Yes	Yes	Yes	Yes
Origin × Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year × Origin FE	Yes	Yes	Yes	Yes	Yes	Yes
Year × Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.075	0.373	0.026	0.193	0.186	0.083
Observations	5126273	5126273	5126273	5126273	5126273	5126273
Number clusters	1114	1114	1114	1114	1114	1114
DV cond. mean	0.010	0.158	0.002	0.050	0.048	0.011
F (First)	34.6	34.6	34.6	34.6	34.6	34.6

Notes: Linear specification version of B.12. The number of firm proceedings is winsorized at five. * $p < .1$, ** $p < .05$, *** $p < .01$ Standard errors in parenthesis, clustered at the researcher region level. Standard fixed effects include conference, origin × field, origin × firm, year × origin and year × firm fixed effects.

Table B.12: Heterogeneity: Participation intensity for citations

	(1) Science cit (present)	(2) Science cit (past)	(3) Patent cit (present)	(4) Patent cit (past)	(5) Collaboration	(6) Hiring
1 × Participation	0.011	0.055	–0.001	0.064***	0.040**	–0.003
	(0.007)	(0.040)	(0.003)	(0.020)	(0.019)	(0.007)
2 × Participation	0.026***	0.164***	0.001	0.101***	0.084***	0.013
	(0.008)	(0.044)	(0.003)	(0.028)	(0.024)	(0.009)
3, 4 × Participation	0.041**	0.226***	0.002	0.168***	0.136***	0.025**
	(0.016)	(0.067)	(0.006)	(0.046)	(0.035)	(0.013)
5+ × Participation	0.061**	0.289***	0.017**	0.237***	0.236***	0.025
	(0.024)	(0.069)	(0.007)	(0.048)	(0.057)	(0.020)
Sponsor, no proceedings × Participation	0.036	0.188*	–0.010	0.090	0.104*	0.021
	(0.028)	(0.102)	(0.010)	(0.062)	(0.053)	(0.030)
Sponsor + Proceedings × Participation	0.099***	0.350***	0.009	0.254***	0.214***	0.065***
	(0.030)	(0.065)	(0.011)	(0.060)	(0.061)	(0.024)
2	–0.005	–0.040***	–0.001	–0.012	–0.015*	–0.007**
	(0.003)	(0.014)	(0.001)	(0.009)	(0.009)	(0.004)
3, 4	–0.011	–0.068**	–0.001	–0.048**	–0.041**	–0.014**
	(0.008)	(0.027)	(0.003)	(0.022)	(0.019)	(0.007)
5+	–0.020	–0.099***	–0.011**	–0.089***	–0.101***	–0.010
	(0.017)	(0.038)	(0.005)	(0.029)	(0.037)	(0.013)
Sponsor, no proceedings	–0.016	–0.073	0.004	–0.016	–0.034	–0.013
	(0.013)	(0.046)	(0.005)	(0.029)	(0.025)	(0.014)
Sponsor + Proceedings	–0.040**	–0.128***	–0.003	–0.079**	–0.067*	–0.032**
	(0.017)	(0.038)	(0.007)	(0.033)	(0.035)	(0.014)
Conf. distance controls	Yes	Yes	Yes	Yes	Yes	Yes
Proceeding-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Standard FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.083	0.371	0.028	0.196	0.196	0.085
Observations	5126273	5126273	5126273	5126273	5126273	5126273
Number clusters	1114	1114	1114	1114	1114	1114
DV cond. mean	0.010	0.158	0.002	0.050	0.048	0.011
F (First)	5.9	5.9	5.9	5.9	5.9	5.9

Notes: Reports the coefficients underlying table 2.3 in section 2.8. * $p < .1$, ** $p < .05$, *** $p < .01$ Standard errors in parenthesis, clustered at the researcher region level. Standard fixed effects include conference, origin × field, origin × firm, year × origin and year × firm fixed effects.

Table B.13: Conference location × year fixed effects

	(1) Science cit (present)	(2) Science cit (past)	(3) Patent cit (present)	(4) Patent cit (past)	(5) Collaboration	(6) Hiring
Participation	0.019**	0.095**	0.002	0.096***	0.045**	−0.002
	(0.008)	(0.048)	(0.003)	(0.026)	(0.019)	(0.007)
Science citations (L)	0.029***	0.393***	0.003***	0.158***	0.150***	0.037***
	(0.001)	(0.007)	(0.000)	(0.003)	(0.004)	(0.001)
Patent citations (L)	0.008***	0.183***	0.003***	0.194***	0.075***	0.013***
	(0.002)	(0.006)	(0.001)	(0.006)	(0.005)	(0.002)
Research similarity (L)	0.027***	0.253***	0.006*	0.015	0.075***	0.029***
	(0.008)	(0.050)	(0.003)	(0.026)	(0.019)	(0.007)
Conf. distance controls	Yes	Yes	Yes	Yes	Yes	Yes
Standard FE	Yes	Yes	Yes	Yes	Yes	Yes
Dest × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.090	0.378	0.033	0.198	0.212	0.088
Observations	5126273	5126273	5126273	5126273	5126273	5126273
Number clusters	1114	1114	1114	1114	1114	1114
DV cond. mean	0.010	0.158	0.002	0.050	0.048	0.011
F (First)	32.1	32.1	32.1	32.1	32.1	32.1

Notes: * $p < .1$, ** $p < .05$, *** $p < .01$ Standard errors in parenthesis, clustered at the researcher region level.Standard fixed effects include conference, origin × field, origin × firm, year × origin and year × firm fixed effects.

Table B.14: Conference series × origin fixed effects

	(1) Science cit (present)	(2) Science cit (past)	(3) Patent cit (present)	(4) Patent cit (past)	(5) Collaboration	(6) Hiring
Participation	0.060**	0.202*	0.010	0.135**	0.162**	0.019
	(0.026)	(0.114)	(0.009)	(0.068)	(0.065)	(0.020)
Science citations (L)	0.025***	0.360***	0.002***	0.147***	0.134***	0.034***
	(0.002)	(0.009)	(0.001)	(0.005)	(0.006)	(0.002)
Patent citations (L)	0.006***	0.169***	0.003***	0.186***	0.067***	0.011***
	(0.002)	(0.006)	(0.001)	(0.007)	(0.006)	(0.002)
Research similarity (L)	−0.001	0.179**	0.000	0.002	−0.007	0.011
	(0.018)	(0.086)	(0.006)	(0.050)	(0.047)	(0.015)
Conf. distance controls	Yes	Yes	Yes	Yes	Yes	Yes
Standard FE	Yes	Yes	Yes	Yes	Yes	Yes
Origin × Conference FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.072	0.383	0.051	0.207	0.165	0.114
Observations	5112327	5112327	5112327	5112327	5112327	5112327
DV cond. mean	1066	1066	1066	1066	1066	1066
Number clusters	0.010	0.158	0.002	0.050	0.048	0.011
F (First)	9.3	9.3	9.3	9.3	9.3	9.3

Notes: * $p < .1$, ** $p < .05$, *** $p < .01$ Standard errors in parenthesis, clustered at the researcher region level. Standard fixed effects include conference, origin × field, origin × firm, year × origin and year × firm fixed effects.

Table B.15: Researcher location × Firm × Year fixed effects

	(1) Science cit (present)	(2) Science cit (past)	(3) Patent cit (present)	(4) Patent cit (past)	(5) Collaboration	(6) Hiring
Participation	0.037***	0.149***	0.000	0.139***	0.116***	0.014
	(0.014)	(0.058)	(0.005)	(0.034)	(0.034)	(0.013)
Science citations (L)	0.028***	0.398***	0.004***	0.158***	0.146***	0.036***
	(0.001)	(0.008)	(0.001)	(0.004)	(0.005)	(0.002)
Patent citations (L)	0.007***	0.181***	0.003***	0.196***	0.073***	0.012***
	(0.002)	(0.006)	(0.001)	(0.006)	(0.006)	(0.002)
Research similarity (L)	0.014	0.217***	0.008	−0.020	0.015	0.015
	(0.011)	(0.057)	(0.005)	(0.033)	(0.029)	(0.012)
Conf. distance controls	Yes	Yes	Yes	Yes	Yes	Yes
Conf Ser FE	Yes	Yes	Yes	Yes	Yes	Yes
Origin × Field FE	Yes	Yes	Yes	Yes	Yes	Yes
Origin × Firm × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.109	0.415	0.079	0.221	0.223	0.135
Observations	4917944	4917944	4917944	4917944	4917944	4917944
DV cond. mean	997	997	997	997	997	997
Number clusters	0.010	0.158	0.002	0.050	0.048	0.011
F (First)	24.6	24.6	24.6	24.6	24.6	24.6

Notes: * $p < .1$, ** $p < .05$, *** $p < .01$ Standard errors in parenthesis, clustered at the researcher region level. Fixed effects on researcher location × firm × year added.

Table B.16: Full-model specification with various cluster levels

	(1) Science cit (present)	(2) Science cit (present)	(3) Science cit (present)	(4) Science cit (present)	(5) Science cit (present)	(6) Science cit (present)
Participation	0.021***	0.021*	0.021**	0.021**	0.021*	0.021***
	(0.007)	(0.012)	(0.008)	(0.009)	(0.012)	(0.007)
Science citations (L)	0.029***	0.029***	0.029***	0.029***	0.029***	0.029***
	(0.001)	(0.005)	(0.001)	(0.002)	(0.005)	(0.001)
Patent citations (L)	0.008***	0.008**	0.008***	0.008***	0.008**	0.008***
	(0.002)	(0.003)	(0.002)	(0.002)	(0.003)	(0.002)
Research similarity (L)	0.025***	0.025***	0.025***	0.025***	0.025***	0.025***
	(0.006)	(0.009)	(0.008)	(0.008)	(0.008)	(0.007)
Conf. distance controls	Yes	Yes	Yes	Yes	Yes	Yes
Conf Ser FE	Yes	Yes	Yes	Yes	Yes	Yes
Origin × Field FE	Yes	Yes	Yes	Yes	Yes	Yes
Origin × Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year × Origin FE	Yes	Yes	Yes	Yes	Yes	Yes
Year × Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	Origin	Firm	Origin-Dest	TW O/D	TW O/F	Paper
R^2	0.083	0.083	0.083	0.083	0.083	0.083
Observations	5126273	5126273	5126273	5126273	5126273	5126273
N clusters	1114	3235	88398			235902
Level 1 (Twoway)				1114	1114	
Level 2 (Twoway)				511	3235	
DV cond. mean	0.010	0.010	0.010	0.010	0.010	0.010
F (First)	31.6	144.5	49.6	14.6	27.8	141.4

Notes: Default specification with varying cluster levels. Columns 1-3 and 6 apply one-way clustering. Column 1 clusters by researcher airport region, column 2 by citing firm. Column 3 clusters by researcher *times* conference airport region. Columns 4 and 5 apply two-way clustering, for origin and destination clusters, and origin and firm clusters, respectively. Column 4 clusters by researcher *and* conference airport regions. Column 5 clusters by researcher airport region *and* firm region. Column 6 applies one-way clustering on the proceeding-level. * $p < .1$, ** $p < .05$, *** $p < .01$ Standard errors in parenthesis, clustered at the researcher region level.

Table B.17: Science citations (OLS/IV)

	(1) Science cit (present)	(2) Science cit (present)	(3) Science cit (present)	(4) Science cit (present)	(5) Science cit (past)	(6) Science cit (past)	(7) Science cit (past)	(8) Science cit (past)
Participation	0.006***	0.013***	0.005***	0.021***	0.059***	0.057**	0.043***	0.113***
	(0.000)	(0.004)	(0.000)	(0.007)	(0.003)	(0.025)	(0.002)	(0.041)
Science citations (L)			0.031***	0.029***			0.402***	0.394***
			(0.001)	(0.001)			(0.004)	(0.006)
Patent citations (L)			0.009***	0.008***			0.188***	0.184***
			(0.002)	(0.002)			(0.006)	(0.006)
Research similarity (L)			0.041***	0.025***			0.307***	0.240***
			(0.003)	(0.006)			(0.012)	(0.043)
Method	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Conf. distance controls	No	No	Yes	Yes	No	No	Yes	Yes
Conf Ser FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Origin × Field FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Origin × Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year × Origin FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year × Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.083	0.082	0.090	0.083	0.318	0.318	0.379	0.372
Observations	5126273	5126273	5126273	5126273	5126273	5126273	5126273	5126273
Number clusters	1114	1114	1114	1114	1114	1114	1114	1114
DV cond. mean	0.010	0.010	0.010	0.010	0.158	0.158	0.158	0.158
F (First)		88.7		31.6		88.7		31.6

Notes: Shows the results of OLS and corresponding IV specifications. Leaving out Year×Origin and Year×Firm FE leaves results qualitatively unchanged. * $p < .1$, ** $p < .05$, *** $p < .01$ Standard errors in parenthesis, clustered at the researcher region level.

Table B.18: Patent citations (OLS/IV)

	(1) Patent cit (present)	(2) Patent cit (present)	(3) Patent cit (present)	(4) Patent cit (present)	(5) Patent cit (past)	(6) Patent cit (past)	(7) Patent cit (past)	(8) Patent cit (past)
Participation	0.001***	0.001	0.001***	0.001	0.019***	0.048***	0.012***	0.098***
	(0.000)	(0.001)	(0.000)	(0.003)	(0.001)	(0.013)	(0.001)	(0.024)
Science citations (L)			0.003***	0.003***			0.168***	0.158***
			(0.000)	(0.000)			(0.003)	(0.003)
Patent citations (L)			0.003***	0.003***			0.200***	0.195***
			(0.001)	(0.001)			(0.006)	(0.006)
Research similarity (L)			0.007***	0.007**			0.094***	0.012
			(0.001)	(0.003)			(0.004)	(0.024)
Method	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Conf. distance controls	No	No	Yes	Yes	No	No	Yes	Yes
Conf Ser FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Origin × Field FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Origin × Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year × Origin FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year × Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.030	0.030	0.031	0.031	0.191	0.187	0.227	0.192
Observations	5126273	5126273	5126273	5126273	5126273	5126273	5126273	5126273
Number clusters	1114	1114	1114	1114	1114	1114	1114	1114
DV cond. mean	0.002	0.002	0.002	0.002	0.050	0.050	0.050	0.050
F (First)		88.7		31.6		88.7		31.6

Notes: Shows the results of OLS and corresponding IV specifications. Leaving out Year×Origin and Year×Firm FE leaves results qualitatively unchanged. * $p < .1$, ** $p < .05$, *** $p < .01$ Standard errors in parenthesis, clustered at the researcher region level.

Table B.19: Collaboration (OLS/IV)

	(1) Collaboration	(2) Collaboration	(3) Collaboration	(4) Collaboration	(5) Hiring	(6) Hiring	(7) Hiring	(8) Hiring
Participation	0.021***	0.028***	0.015***	0.074***	0.005***	0.001	0.003***	0.006
	(0.001)	(0.009)	(0.001)	(0.020)	(0.000)	(0.004)	(0.000)	(0.008)
Science citations (L)			0.155***	0.147***			0.037***	0.037***
			(0.003)	(0.004)			(0.001)	(0.001)
Patent citations (L)			0.077***	0.074***			0.013***	0.012***
			(0.005)	(0.005)			(0.002)	(0.002)
Research similarity (L)			0.104***	0.047***			0.023***	0.021***
			(0.005)	(0.018)			(0.002)	(0.007)
Method	OLS	IV	OLS	IV	OLS	IV	OLS	IV
Conf. distance controls	No	No	Yes	Yes	No	No	Yes	Yes
Conf Ser FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Origin × Field FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Origin × Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year × Origin FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year × Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.185	0.185	0.214	0.196	0.080	0.080	0.087	0.087
Observations	5126273	5126273	5126273	5126273	5126273	5126273	5126273	5126273
Number clusters	1114	1114	1114	1114	1114	1114	1114	1114
DV cond. mean	0.048	0.048	0.048	0.048		0.011		0.011
F (First)		88.7		31.6		88.7		31.6

Notes: Shows the results of OLS and corresponding IV specifications. Leaving out Year×Origin and Year×Firm FE leaves results qualitatively unchanged. * $p < .1$, ** $p < .05$, *** $p < .01$ Standard errors in parenthesis, clustered at the researcher region level.

B.5 Understanding Firms' Attendance of Scientific Conferences

In this paper, we capture firms' conference attendance on a large scale through data on authorship of conference proceedings and on sponsorship of conference events. We collected qualitative evidence to better understand the reality behind these indicators. We attended two large, high-quality conferences to gather evidence in support of our assumptions. The first conference was the European Conference on Computer Vision 2018 (ECCV, https://eccv2018.org/) in Munich, Germany. The ECCV is a large biannual $A^\star$ conference in computer vision, a subfield of AI. In 2018, about 3500 persons participating. The second conference was the Neural Information Processing Systems conference 2019 (NeurIPS, https://nips.cc/) in Vancouver, Canada, with more than 13000 participants. At ECCV and NeurIPS, we interviewed more than 50 in total, between scientists, HR representatives and engineers, of more than 20 firms and about 20 academic scientists. We talked to firms of several countries and of various size and with different levels of participation. We investigated their activities at conferences and the processes taking place before, during and after conferences. Falling short of a full qualitative study, we here report our general impressions from the interviews. This follows from section 2.3 of the paper where we describe the main characteristics of CS conference series and the modes of participation of firms.

In summary, firms activities at a conference can be categorized in (i) scientific activities and, (ii) branding and recruiting activities. These two categories relate to rather distinct underlying dynamics and processes. The former is reflected in the conference participation of scientists who present their work and normally interact with their academic and corporate peers. Generally, firm scientists conveyed the impression of a high degree of autonomy, having considerable freedom in the decision of which conferences to attend and, to a large extent, what to present. Firm-level processes, mostly unknown to academic scientists, play a role mostly in the screening of presented work before and in the follow-up activities after the conference. Interestingly, the screening of work submitted to conferences concerns primarily a selection based on quality: most firms have in place internal peer-review systems (and not of hierarchical approval) to ensure presenting above-average scientific work.

Nonetheless, this also entails guaranteeing the presence of sufficient intellectual property protection. All firm scientists interviewed declared that prior to a conference presentation, firm lawyers would verify whether a patent application is necessary, to protect possible valuable inventions and to avoid compromising the future option of obtaining a patent.[1] However, no one declared this to have ever been an impediment to their participation. It also remains that the work of firms scientists presenting at the conference appeared often not directly related to current product development. Some scientists said that the research closely related to product development is normally maintained secret and performed by different organizational units.

After a conference event, all firms appeared to have in place knowledge sharing processes. Depending on the firm, these may take the form of informal activities, such as the sharing of references among colleagues (also who did not participate to the conference). More often, researchers were expected to write more structured reports or to prepare presentations on the content of the conference to be discussed in internal meetings. In some cases this was supported by an internal IT information system to trace

[1]In most patent jurisdictions, rendering public an invention generates prior art which jeopardizes the novelty of an eventual patent application also if inventors and authors of the publication are the same

the participation of different individuals to different conferences and events and maintain information on their feedback.

Official recruiting and branding activities are mostly carried out by personnel at the conference booths and are directly connected with sponsorship. HR personnel, in particular, advertise job opportunities, mostly for PhDs and young researchers, attend at the booth to all potential candidates and schedule possible follow-up interviews after the conference. The HR units then take care of preparing the material and define the main activities before the conference, and have follow-up meetings to discuss the outcomes and possible improvements after the conference.

Despite being distinct activities, scientific and branding/hiring activities are not disconnected. On the one hand, the firm sponsorship and the personnel employed at the firm's booths also advertise more generally scientific activities of firms scientists and can offer organizational and logistic support to their scientists. The promotion of research activities performed by firm, especially focused on the specific contributions at the focal conference, is at least equally evident than the promotion of job positions. The sponsorship benefits and "infrastructure" of large sponsors, especially at large conferences like NeurIPS, is used to create opportunities for divulging research results, even to offer specialized tutorials and workshops, beyond the presentation of proceedings or organization of workshops that may be part of the normal conference program.

On the other hand, several HR representatives referred to have experimented also participating to conferences without the presence of scientists. This however proved to be ineffective also for hiring objectives, due to the difficulty of engaging with other scientists. The presence of scientists at the booths is planned, in order to facilitate the conversations with potential candidates for job positions that are often interested in discussing in detail research developed by the firm. And the promotion of research at the firm is clearly complementary to engaging possible candidates. Interestingly, most HR representatives we interviewed declared that the decision of the conferences to sponsor often follows the preferences of where scientists want to present their research. Scientists did not seem to care much about whether the firm sponsored or not an event when deciding where to participate, and would very well, and often do, participate without corresponding sponsorship. Informal connections and interactions of scientists at the conference may also constitute a vehicle to reach and engage candidates. The few sponsors we could talk to with a small booth and no parallel scientific activity demonstrate limited interactions with the conference participants and their booths were poorly attended. One of these sponsors' representative (from a large firm) explicitly expressed dissatisfaction for the lack of a more significant investment by the firm, in her/his own words, "to a community that I deem important for our research units".

The evidence discussed here is necessarily anecdotal. In particular, it is based on only two events and a sample of interviewees necessarily selected by the presence at these conferences. Moreover, the level of investment of firms at ECCV18 and NeurIPS19, similarly to other conference series in ML, has risen sharply in the latest years. Nonetheless, we can very well expect that the type of firm activities carried out at other conferences would be equivalent, and, while the level of investment may have varied over time and across subfields, the nature of these activities would likely be the same. Most importantly, this evidence stands as a proof that the participation of firms to conferences constitute a substantial firm-level investment which is well approximated by our empirical quantitative data.

B.6 Similarity Measures

We calculate text similarity scores using the cosine similarity between reduced term frequency–inverse document frequency (tf-idf) values of the cleaned abstracts and titles. In a first step, the abstract is cleaned. Cleaning involves concatenating title and actual abstract, removing copyright statements and replacing special keywords with character strings (2D becomes twod, L2 becomes eltwo, ...). Then, everything which is not a character is replaced with a whitespace. We employ stemming, which reduces flexed forms of words to their stem. We also remove stop words. Of the so-cleaned abstract, we take the 50,000 most frequent tf-idf values of one, two and three-grams. We exclude very frequent terms. We then use a truncated singular value decomposition (SVD) to reduce the dimensionality from 50,000 to 300. This approach is also called latent semantic analysis (LSA). The latter name hints at the purpose - finding dimensions that concisely describe the semantic content of an abstract. Multiple words can have the same meaning and the same word can have several meaning, depending on the context. All in all, this approach generates a procedure which maps an abstract into 300 dimensions. For the tf-idf measure as well as the SVD, it is necessary to take the full body of documents into account in a training stage. For this, we use all 2.6 million DBLP items for which we can find abstracts. Once this training stage is completed, individual abstracts can be analyzed. Finally, the cosine similarity is calculated for two transformed abstracts.

We insert mean and maximum similarity scores as outcome variables into our regression setup from section 2.6. The theoretical range of the similarity scores is between -1 and 1, but observed values are typically between zero and one. In each firm $\times$ year $\times$ field group, we observe several similarities when firms have published more than one paper. Within these groups, we take the average and the maximum. When a firm has not published a paper in a given year $\times$ field group, we set the similarity score to zero.

Table B.20: Mean similarity scores

Mean Similarity	(1) t-1	(2) t	(3) t+1	(4) t+2	(5) t+3
Participation	−0.008	0.043***	0.018**	0.023***	0.015**
	(0.011)	(0.008)	(0.008)	(0.007)	(0.006)
Science citations (L)		0.011***	0.013***	0.012***	0.013***
		(0.001)	(0.001)	(0.001)	(0.001)
Patent citations (L)		0.007***	0.010***	0.009***	0.010***
		(0.001)	(0.001)	(0.001)	(0.001)
Conf. distance controls	Yes	Yes	Yes	Yes	Yes
Conf Ser FE	Yes	Yes	Yes	Yes	Yes
Origin × Field FE	Yes	Yes	Yes	Yes	Yes
Origin × Firm FE	Yes	Yes	Yes	Yes	Yes
Year × Origin FE	Yes	Yes	Yes	Yes	Yes
Year × Firm FE	Yes	Yes	Yes	Yes	Yes
R^2	0.580	0.422	0.597	0.608	0.622
Observations	5126273	5126273	5126273	5126273	5126273
Number clusters	1114	1114	1114	1114	1114
DV cond. mean	0.087	0.151	0.090	0.087	0.084
F (First)	28.5	28.8	29.0	29.3	29.3

Notes: * $p < .1$, ** $p < .05$, *** $p < .01$ Standard errors in parenthesis, clustered at the researcher region level.This table shows mean abstract similarity scores of firm papers in $t + x$ relative to the focal paper. Only papers within the same CS field are compared. When a firm did not publish in $t + x$, the mean similarity score is set to zero. Firm-proceeding level dataset, where some proceedings were actually at the conference (Participation=1) and some were at another conference (Participation=0). Participation is instrumented by the direct flight availability between the researcher location and the conference location. Firm-proceeding controls include whether the firm cited previous work by the authors in the years before the conference (Science/Patent citations L) and the average abstract similarity between proceedings published by the firm in the previous year and the focal proceeding (Research Similarity L). Dependent variable mean is for actually presented proceedings. Conf. distance controls include the distance between researcher and conference location (log), whether that distance is zero and whether the two locations are in the same US state or non-US country.

Table B.21: Maximum similarity scores

Max Similarity	(1) t-1	(2) t	(3) t+1	(4) t+2	(5) t+3
Participation	−0.010	0.075***	0.029**	0.045***	0.022*
	(0.017)	(0.014)	(0.014)	(0.013)	(0.013)
Science citations (L)		0.026***	0.029***	0.026***	0.027***
		(0.002)	(0.002)	(0.002)	(0.002)
Patent citations (L)		0.019***	0.023***	0.022***	0.023***
		(0.002)	(0.002)	(0.001)	(0.001)
Conf. distance controls	Yes	Yes	Yes	Yes	Yes
Conf Ser FE	Yes	Yes	Yes	Yes	Yes
Origin × Field FE	Yes	Yes	Yes	Yes	Yes
Origin × Firm FE	Yes	Yes	Yes	Yes	Yes
Year × Origin FE	Yes	Yes	Yes	Yes	Yes
Year × Firm FE	Yes	Yes	Yes	Yes	Yes
R^2	0.685	0.541	0.697	0.706	0.716
Observations	5126273	5126273	5126273	5126273	5126273
Number clusters	1114	1114	1114	1114	1114
DV cond. mean	0.165	0.245	0.172	0.168	0.162
F (First)	28.5	28.8	29.0	29.3	29.3

Notes: * $p < .1$, ** $p < .05$, *** $p < .01$ Standard errors in parenthesis, clustered at the researcher region level.This table shows maximum abstract similarity scores of firm papers in $t + x$ relative to the focal paper. Only papers within the same CS field are compared. When a firm did not publish in $t + x$, the mean similarity score is set to zero. Firm-proceeding level dataset, where some proceedings were actually at the conference (Participation=1) and some were at another conference (Participation=0). Participation is instrumented by the direct flight availability between the researcher location and the conference location. Firm-proceeding controls include whether the firm cited previous work by the authors in the years before the conference (Science/Patent citations L) and the average abstract similarity between proceedings published by the firm in the previous year and the focal proceeding (Research Similarity L). Dependent variable mean is for actually presented proceedings. Conf. distance controls include the distance between researcher and conference location (log), whether that distance is zero and whether the two locations are in the same US state or non-US country.

C

Appendix to Chapter 3
Profit Taxation, R&D Spending and Innovation

C.1 Data

In this section of the Appendix, we provide additional information on each of the different datasets used in the empirical analysis and provide additional descriptive statistics.

The plant-level R&D survey data. The main data source of this analysis is the biennial longitudinal survey dataset *Wissenschaftsstatistik*, collected and administrated by the Sifterverband on behalf of the German Federal Ministry of Education and Research. The survey targets the universe of research-active plants in Germany. Those are identified via several distinct channels: (i) federal and European datasets on public R&D funding, (ii) patent applications, (iii) plant or firms' annual accounts or business reports, (iv) commercial company databases, (v) the media, and (vi) membership lists of trade associations with a focus on innovative activity. This continuously-updated register of R&D active plants is compared with information from the *Bureau van Dijk* databases to identify plant closures and changes of plant location. To further ensure the comprehensiveness of the plant register, regular surveys are conducted among plants in R&D active industries (in particular: automotive engineering, mechanical engineering, electrical and chemical engineering), which have not been known for their R&D activity so far. Results of these short surveys indicate that a very high share of all R&D-active plants in Germany is indeed covered by the dataset.

The survey covers detailed information on plants' overall R&D spending, its R&D expenses by subcategories (internally- vs. externally conducted R&D, personnel vs. non-personnel R&D spending) and its R&D staff (by age structure, qualification, education). Moreover, it offers information on plant size (in terms of sales and employees), industry classification and plant's organizational structure (single- vs. multi-plant firms). By special agreement with the Stifterverband, we also gained access to each plant's exact address (postal code and location) in a given year, which allows us to precisely assign the applicable LBT (treatment). Panel A of Appendix Table C.1 provides detailed descriptive statistics. The survey forms the basis of Germany's official reporting of its entrepreneurial R&D activities to EU authorities and the OECD.

Patent data. To assess the impact of profit taxation on innovation output, we link administrative information on plants' patenting activity from the *European Patent Office* (EPO, *PATSTAT* dataset as of 4/2016) to the R&D survey. As plants often register the very same innovation at multiple intellectual property (IP) protection institutions, worldwide patent databases focus on "patent families", i.e., pool those inventions that show the very same content and priority date. The latter refers to the date of the first patent application within a patent family at any institution and determines the start of the IP protection period. The focus on patent families effectively rules out the threat of double-counting the very same patented innovation within and across different IP systems. Within the EPO system, double-counting of patents may still occur in cases of parallel or divisional applications. However, these cases are very rare.

To best match the plant-level survey, we limit ourselves to patent families that were first registered between 1995 and 2007 and identify each patent family's initial applicant(s). This is particularly important in the context of our analysis: we want to identify the plant where the initial invention occurred, not the current IP holder. We next drop all patent applications that have not been (co-)filed by a plant (as classified by *PATSTAT*), and geocode all remaining patents. In a final step, we use detailed information on the applicants' name(s) and location(s) of residence to merge the number of filed patents to the plant-level survey by means of a fuzzy matching algorithm. In case multiple actors jointly invented a new product or process, we only assign the respective share of a patent to a surveyed plant. Overall, the surveyed plants account for around 60% of all patents filed by a German applicant during the period from 1995 to 2007.

As the value of patents differs substantially (Scherer, 1965; Hall, Jaffe, and Trajtenberg, 2005), we create a second measure of innovation output that weights each patent family according to the number of citations it receives from other EPO patents that are filed within the first five years after its registration.[1] Citation-adjusted weighted counts are widely used in the literature and have been shown to correlate well with real-world measures of innovation quality such as profitability (see, e.g., Harhoff, Scherer, and Vopel, 2003; Kogan et al., 2017; Moser, Ohmstedt, and Rhode, 2018). As a robustness test, we also weight patents with the number of citations they receive by patents filed at the United States Patent and Trademark Office (USTPO). Relying on data from Danzer, Feuerbaum, and Gaessler (2020), we further distinguish product from process innovations. To group patents along this margin, information from the highly standardized patents' claims texts is used. Patents are classified as process innovation if the claim text of a patent includes terms such as "method", "process" or "procedure". Panel B of Appendix Table C.1 provides descriptive statistics on each measure of plants' innovation output. Note that some patent applications (12%) do not provide enough information to classify a patent accordingly. Excluding these patents from the baseline regressions does not affect estimates.

Additional plant-level data. While the Stifterverband data provide detailed information on plants' R&D activities, the survey offers only few insights on plants' financial situation. To test for heterogeneous effects among more or less cash-constrained plants, we link additional information from the Bureau van Dijk's (BvD) AMADEUS and ORBIS databases to the surveyed plants.[2] The match between the R&D survey and the BvD data has been established by the Stifterverband as part of the survey's implementation strategy. The two BvD datasets offer a variety of financial information at the *company* level. Thus, for plants that belong to a company, we actually proxy their financial situation via the company's one. As the BvD datasets predominantly cover larger and oftentimes stock-listed plants or firms, we can only match around 40% of the surveyed plants to the BvD data.

To prepare the BvD data for the purposes of our study, we predominantly follow Kalemli-Ozcan et al. (2015) and Gopinath et al. (2017). We first combine multiple vintages of the AMADEUS and ORBIS datasets to increase coverage over time. Ultimately, we use vintages of the AMADEUS database from 2001, 2002, 2007 and 2010, as well as the 2016 ORBIS version. When a given plant appears in more than one vintage, we follow Gopinath et al. (2017) and take those information from the most recent vintages. When multiple financial accounts are available for a given plant in a given year, we always refer to accounts with higher quality. Here, we always prefer those accounts that cover the full twelve months of a given year. Moreover, we prefer accounts in accordance to IFRS guidelines over GAAP accounts or those with unknown reporting standards. Last, we choose unconsolidated over consolidated

[1]We show that effects remain unaffected when using citations in the United States Patent and Trademark Office (USPTO) IP protection system, too. Citations counts are quite different in these two institutions as the USPTO requires patent applicants to list all relevant patents prior art, whereas such a requirement does not exist at the EPO. Citation data is taken from PATSTAT 10/2019 to completely rule out attrition in our sample.

[2]The data was kindly made available by the LMU-ifo Economics & Business Data Center (`https://www.ifo.de/EBDC`.)

accounts. In the empirical analysis, we measure plants' liquidity constraints via the level of non-current liabilities; Panel A of Appendix Table C.1 providing the corresponding descriptive statistics.

Municipality- and county-level data. Information on LBT scaling factors (*Realsteuerhebesätze der Gewerbesteuer*) for all West German municipalities were obtained from the Federal Statistical Office and the Statistical Offices of the German States. We construct a balanced panel dataset for the universe of municipalities by combining two different sources. Data for the period from 1987 to 2000 was obtained by filing individual requests to the respective Statistical Offices of the German States. Information for the years from 2001 to 2013 is publicly accessible via annual reports: "*Hebesätze der Realsteuern*", published by the Statistical Offices of the German States.

Information on local GDP per capita is available at the county level only and can be accessed via the Working Group Regional Accounts of the Federal Statistical Office and the Statistical Offices of the German States. The available data cover the period from 1992 to 2014. We use the revision from August 2015 and account for inflation by calculating real GDP per capita in 2010 prices using the consumer price index published by the Federal Statistical Office (*Verbraucherpreisindex*). For some regression, we derive a measure of municipality-level GDP by multiplying the respective county's GDP per capita with a given municipality's level of population.

Data on municipal expenses for all West German municipalities over the period from 1998-2007 were obtained from the Federal Statistical Office and the Statistical Offices of the German States. Since 2001, information on local expenses are publicly available via the annual reports "*Statistik Lokal*", published by the Statistical Offices of the German States. For the period from 1998-2000, we filed a data request to the statistical offices. Again, we account for inflation by using the consumer price index and express expenses in 2010 prices.

Information on population levels are available for the entire effect window (1987-2013) and were taken from the Federal Statistical Office and the Statistical Offices of the German States. We combine two different sources to construct a balanced panel for the universe of West German municipalities. First, data for the period from 1987 to 1999 are based on data requests we filed to the Statistical Offices of the German States. Second, data on population levels from 2000 onwards are publicly available via the annual German municipality register (*Gemeindeverzeichnis*).

Last, we collect information on the number of unemployed individuals per municipality for the period 1998 to 2013 from the annually report *Bestand an Arbeitslosen, Rechtskreise SGB III und SGB II, Insgesamt*, published by the German Federal Employment Agency. In the empirical analysis, we divide this number by the respective municipality's annual population level to proxy local unemployment rates.

C.2 Descriptive Statistics, Correlations and Definitions

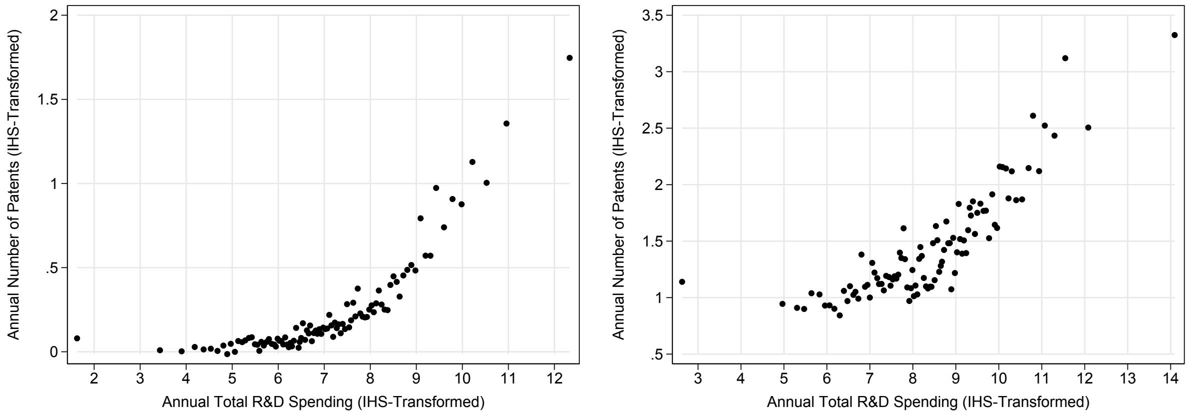
Table C.1: Descriptive Statistics (Baseline Sample)

	Mean	Std Deviation	Min	1% Pctl.	99% Pctl.	Max	Observations
A. Plant-Level R&D Survey							
R&D Spending Levels (in M EUR)							
Total R&D Spending	8,109.77	89,163.33	0	11.00	91,309	4,418,510	31,648
Internal R&D Spending	6,837.73	75,420.28	0	0.00	79,945	4,197,584	31,648
External R&D Spending	1,272.03	21,521.87	0	0.00	11,122	1,119,049	31,648
Internal Spending on R&D Personell	4,152.55	42,238.54	0	0.00	49,834	2,454,123	31,648
Internal R&D Spending on Non-Personell	2,685.16	35,403.09	0	0.00	31,037	2,282,560	31,648
Spending Shares (in %)							
Share of Internal R&D Expenses	90.93	18.36	0	0.00	100	100	31,529
Share of External R&D Expenses	9.07	18.36	0	0.00	100	100	31,529
Share of Internal R&D Expenses for Scientific Staff	67.22	16.54	0	18.44	100	100	31,072
Share of Internal R&D Expenses for Non-Personell	32.78	16.54	0	0.00	82	100	31,072
Other Plant Characteristics							
No. of Employees	536.54	3,035.90	1	3.00	6,214	161,800	31,623
Sales (in MM EUR)	210.95	1,601.39	0	0.00	2,840	84,062	31,445
R&D Spending per Employee (in M EUR)	23.43	347.68	0	0.12	183	35,503	31,623
Non-Current Liabilities to Sales Ratio	0.53	4.48	-0	0.02	3	184	5,879
Manufacturing Sector	0.85	0.36	0	0.00	1	1	31,648
Service Sector	0.10	0.31	0	0.00	1	1	31,648
Other Sector	0.05	0.21	0	0.00	1	1	31,648
B. Patent Data							
Number of Patents	0.84	7.62	0	0.00	15	969	31,648
Citation-Weighted Number of Patents (EP)	0.97	9.96	0	0.00	19	1,082	31,648
Citation-Weigted Number of Patens (With Claims Data)	0.87	8.84	0	0.00	17	1,023	31,648
Citation-Weighted Number of Patents (USTPO)	1.72	19.66	0	0.00	35	2,326	31,648
Citation-Weighted Number of Process Innovations	0.39	4.96	0	0.00	8	538	31,648
Citation-Weighted Number of Product Innovations	0.48	4.53	0	0.00	11	486	31,648
C. Local Characteristics							
Population	27,432.37	76,806.31	177	878.00	284,912	1,689,980	11,403
Log Unemployment Per Capita	3.44	1.34	0	1.27	7	11	8,179
Expenses in M EUR	745.85	3,773.02	-220	12.35	9,772	118,149	8,181
GDP per capita	28,280.75	10,380.23	12,696	16,115.12	81,428	100,807	10,917

Notes: This table shows descriptive statistics for our baseline sample. Panel A provides insights on covered plants' R&D spending in levels, spending shares, and other plant characteristics. Panel B offers information on plants' patenting activities. Last, in Panel C information on municipality- and district-level characteristics are given. See the Data Appendix C.1 for more informaiton on the respective data sources.

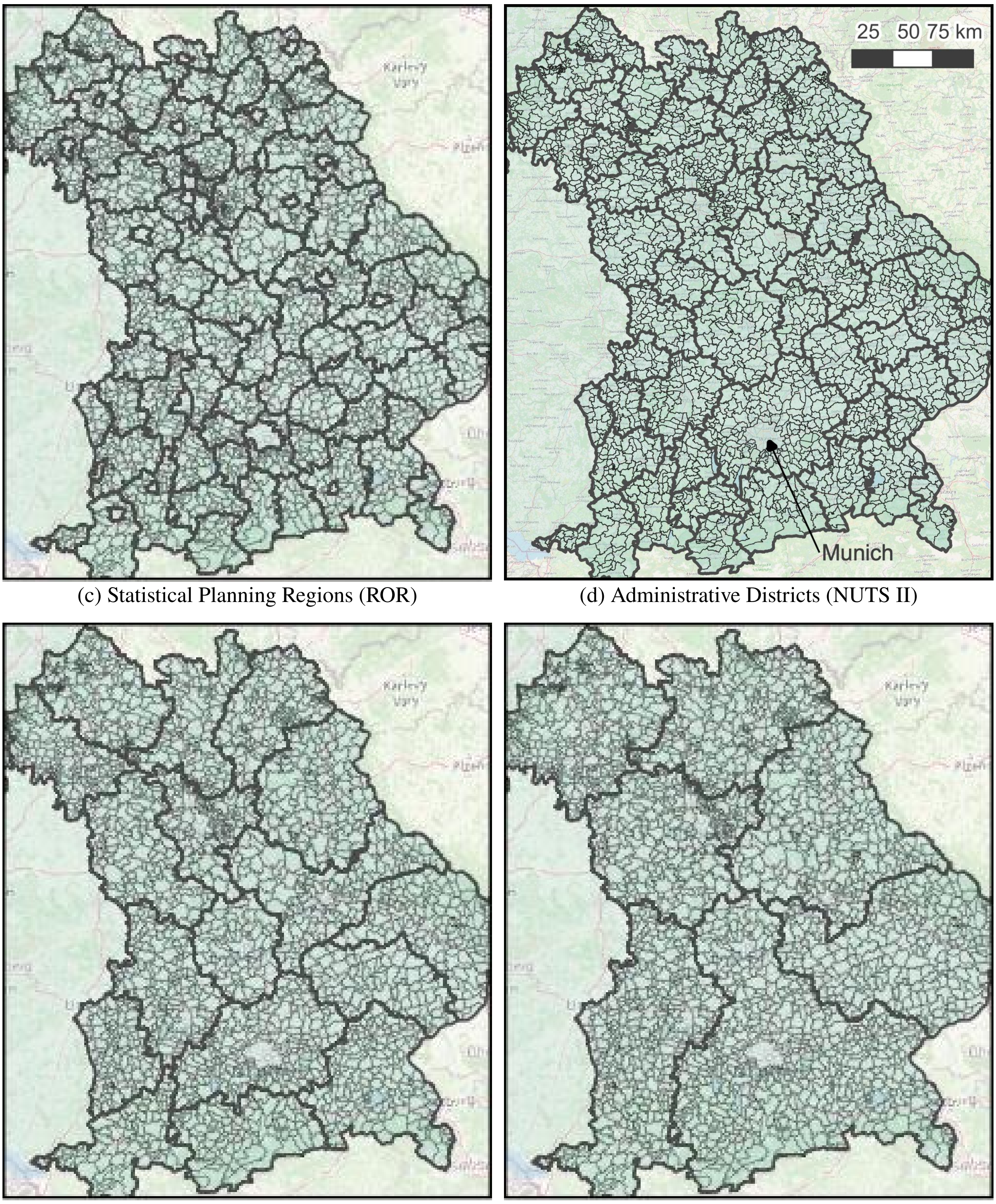
Figure C.1: Assessing the Link between R&D Spending and Patenting

(a) Overall Relationship (b) Internal Margin Effect



Notes: This binscatter plot illustrates the relationship between plants' annual R&D spending and their respective number of filed patents in our baseline sample. An inverse hyperbolic sine transformation is applied to both variables. Year and industry fixed effects are accounted for. We plot the overall relationship in Panel A, and the intensive margin effect in Panel B. Information on plants' R&D spending stems from the *Wissenschaftsstatistik*. Information on plants' patenting activities is taken from the European Patent Office. See Data Appendix C.1 for more information on both datasets.

Figure C.2: Regional Classifications of Municipalities in the Free State of Bavaria



(a) Commuting Zones (CZ)

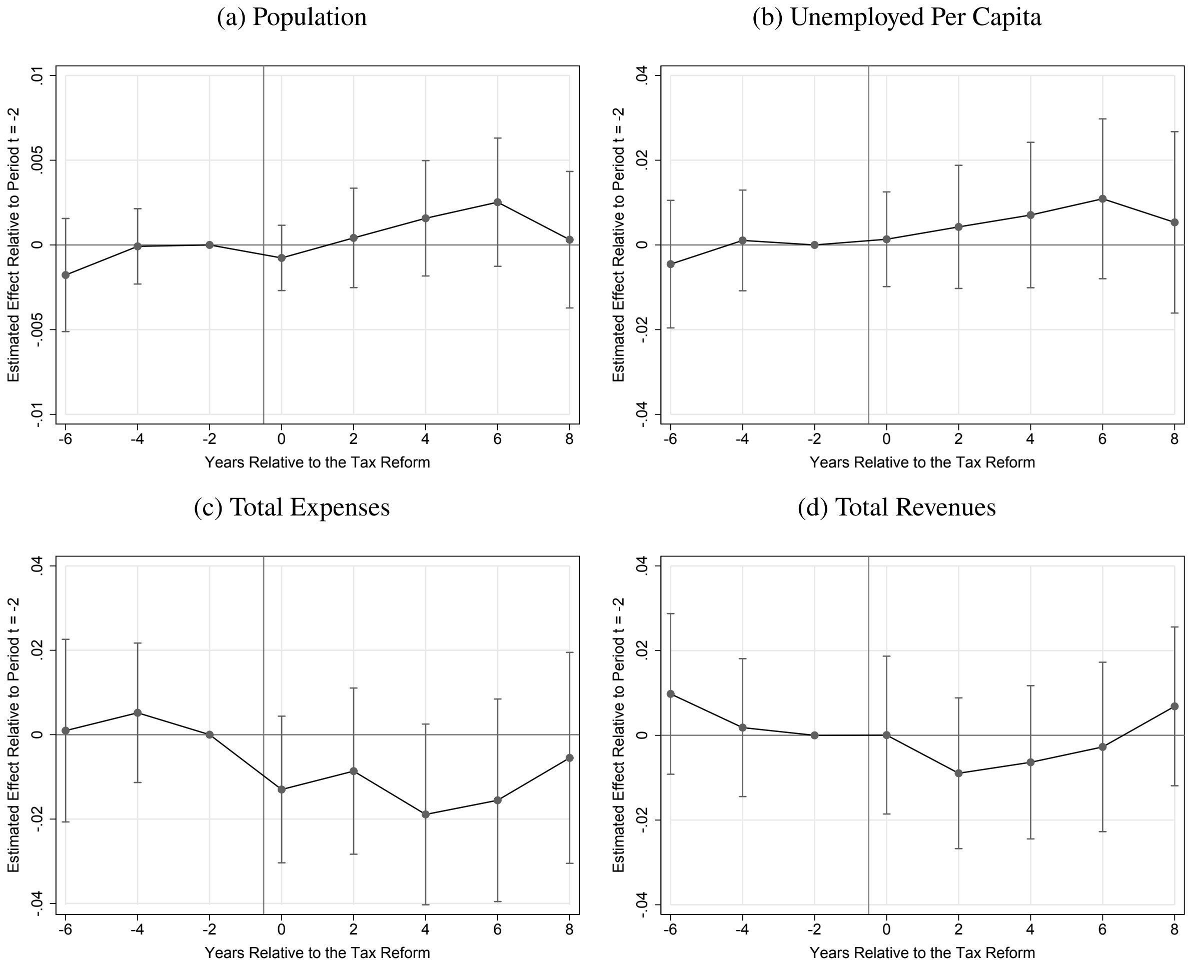
(b) Counties

(c) Statistical Planning Regions (ROR)

(d) Administrative Districts (NUTS II)

Notes: This figure illustrates different regional subdivisions used to control for region-times-year fixed effects in our study, focusing on the 2,056 municipalities in the Free State of Bavaria for the purpose of illustration (thin black lines indicate municipality borders as of December 31, 2010). Panel A plots municipalities along with the 56 commuting zones in Bavaria (thick black lines), which corresponds to our baseline specification. Panel B shows instead the 96 counties and city counties (*kreisfreie Städte*) in Bavaria (nested in commuting zones). Panels C and D show the 18 statistical planning regions (*Raumordnungsregionen*, ROR) and seven administrative districts (*Regierungsbezirke*, NUTS II), respectively, which are geographical aggregations of commuting zones. *Maps:* © GeoBasis-DE / BKG 2015, OpenStreetMap contributors.

Figure C.3: The Effect of a Tax Rate Increase on Municipality-Level Outcomes



Notes: This graph plots the point estimates, $\widehat{\beta_k}$ ($k \in [-6, -4, \ldots, 8]$), and corresponding 95% confidence intervals of the event study model as defined in Equations (3.1)–(3.3). The dependent variable is a municipality's annual population in Panel A, it annual share of unemployed per capita in Panel B, its total annual expenditures in Panel C, and its total annual revenues in Panel D. All outcomes are in logs. For the treatment group, the business tax change occurred on in year $t = 0$ or $t = -1$. The regressions include municipality, state × year, commuting zone × year, as well as sector × year fixed effects. All municipalities that experienced a tax decrease during the event window period are excluded. Standard errors are robust to clustering at the municipality level.

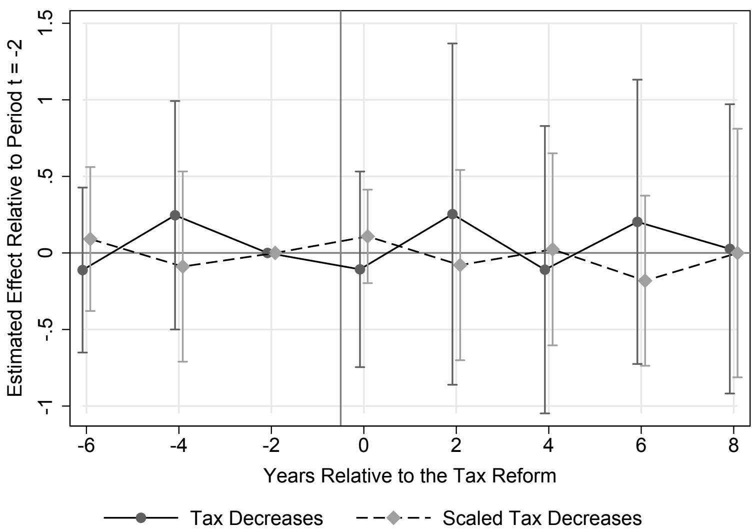
C.3 Additional Results on R&D Spending

Table C.2: Implied Long-Term Elasticities and Oster Bounds

	Total R&D Spending	Internal R&D Spending	Internal Spending on R&D Personnel	Internal Non-Personnel R&D Spending
A. Uncontrolled Estimates				
Point Estimate	-1.25	-1.77	-1.63	-2.44
B. Controlled Estimates				
Point Estimate	-1.19	-1.81	-1.62	-2.49
C. Bounded Oster Estimates				
Point Estimate	-1.16	-1.84	-1.61	-2.63

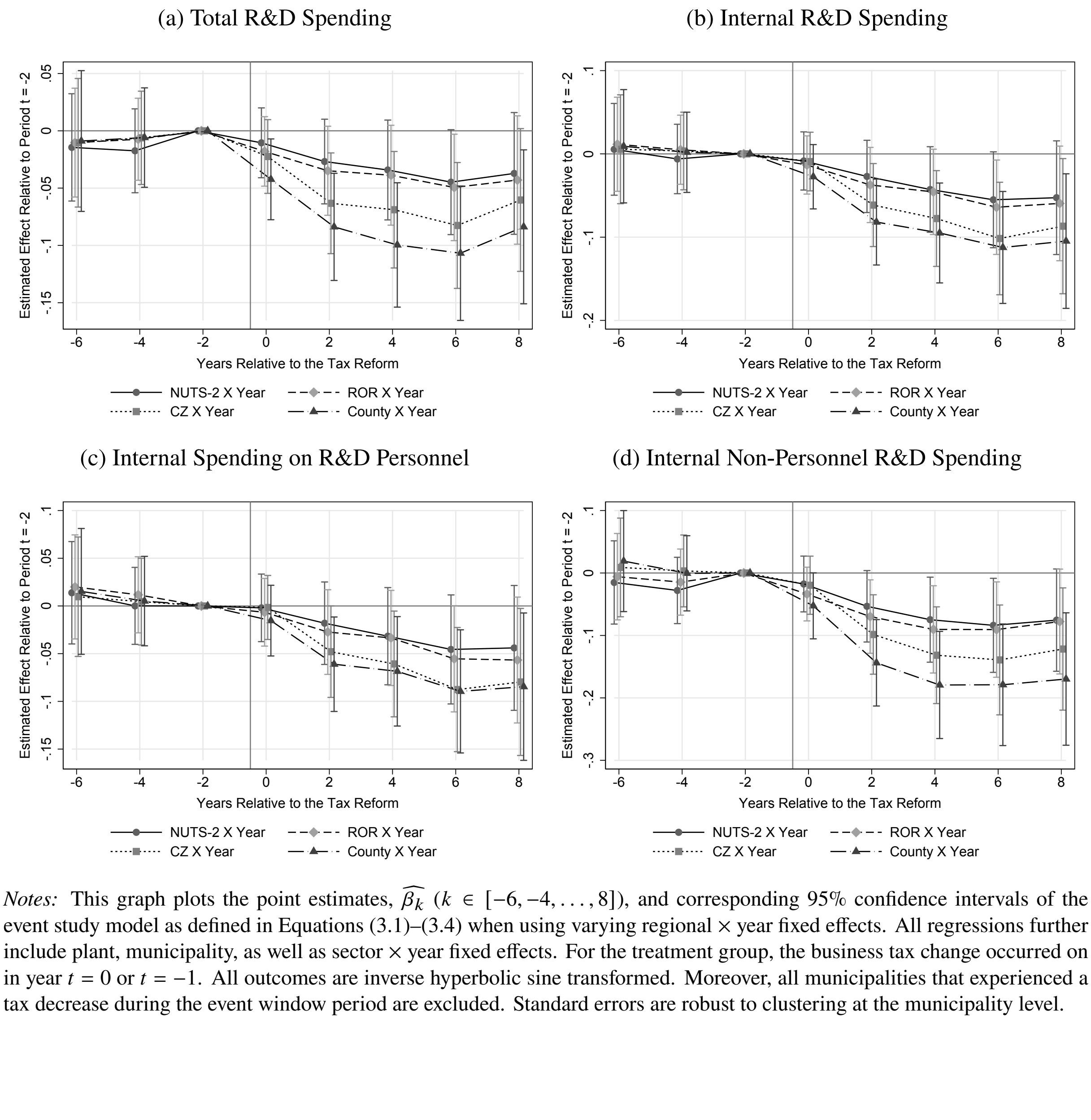
Notes: This table displays the implied long-term elasticity of plants' level of R&D spending (and various sub-categories thereof) with regard to an increase in the LBT rate. In Panel A, we report our baseline elasticities as given in Figure 3.7. In this specification, we do not control for time-varying local confounders at the municipality or county level. In Panel B, we display the corresponding elasticities when accounting for potential confounders at the municipality or county level. Last, in Panel C, we provide bounded estimates of these elasticities in the spirit of Oster (2019). In detail, we calculate the corresponding elasticities via the following formula: $\beta^* = \tilde{\beta} - [\dot{\beta} - \tilde{\beta}] \frac{R_{max} - \tilde{R}}{\tilde{R} - \dot{R}}$, where $\dot{\beta}$ and $\dot{R}$ refer the uncontrolled elasticity and R-Squared and $\tilde{\beta}$ and $\tilde{R}$ to the controlled elasticity and corresponding R-Squared, respectively. Moreover, we set R_{max} to $1.3 \times \tilde{R}$. The framework assumes that coefficient stability with regard to observable confounders may inform about the importance of omitted variable bias if the importance of the controls in explaining the variance of the outcome is accounted for.

Figure C.4: The Effect of a Tax Decrease on R&D Spending



Notes: This graph plots the point estimates, $\widehat{\beta_k}$ ($k \in [-6, -4, \ldots, 8]$), and corresponding 95% confidence intervals of the event study model as defined in Equations (3.1)–(3.4). The dependent variable is a plant's annual (invserse hyperbolic since transformed) total R&D spending. For the treatment group, the business tax change occurred on in year $t = 0$ or $t = -1$. The regressions include municipality, state × year, commuting zone × year, as well as sector × year fixed effects. All municipalities that experienced a tax increase during the event window period are excluded. Standard errors are robust to clustering at the municipality level.

Figure C.5: The Effect of a Tax Increase on Total R&D Spending – Varying Regional Controls



(a) Total R&D Spending

(b) Internal R&D Spending

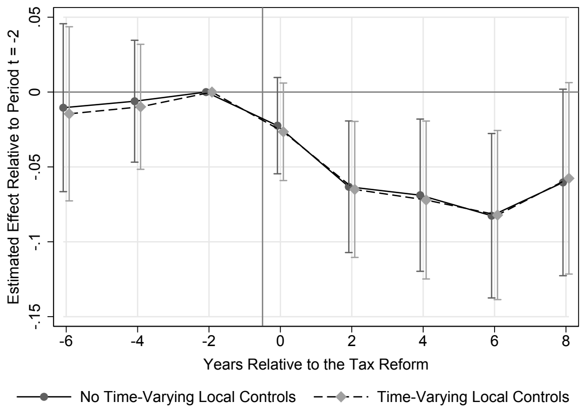
(c) Internal Spending on R&D Personnel

(d) Internal Non-Personnel R&D Spending

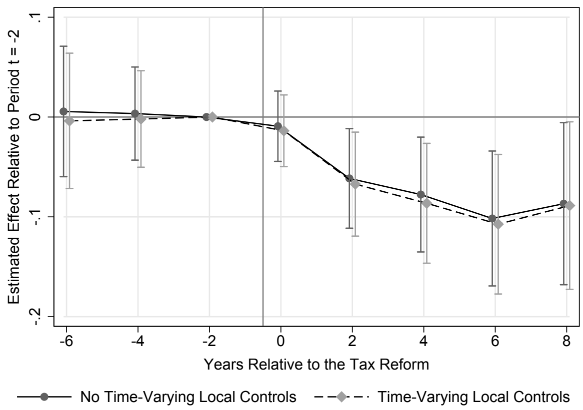
Notes: This graph plots the point estimates, $\widehat{\beta_k}$ ($k \in [-6, -4, \dots, 8]$), and corresponding 95% confidence intervals of the event study model as defined in Equations (3.1)–(3.4) when using varying regional × year fixed effects. All regressions further include plant, municipality, as well as sector × year fixed effects. For the treatment group, the business tax change occurred on in year $t = 0$ or $t = -1$. All outcomes are inverse hyperbolic sine transformed. Moreover, all municipalities that experienced a tax decrease during the event window period are excluded. Standard errors are robust to clustering at the municipality level.

Figure C.6: The Effect of a Tax Increase on Total R&D Spending – Local Time-Varying Controls

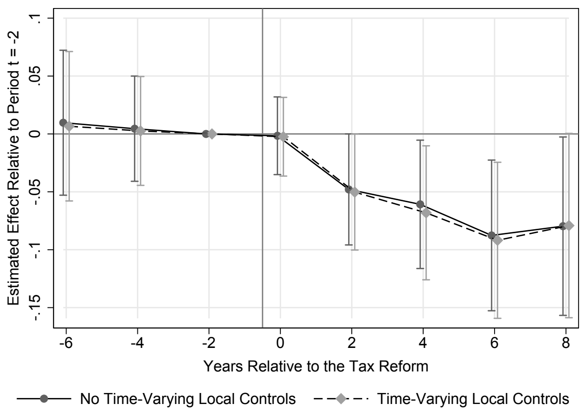
(a) Total R&D Spending



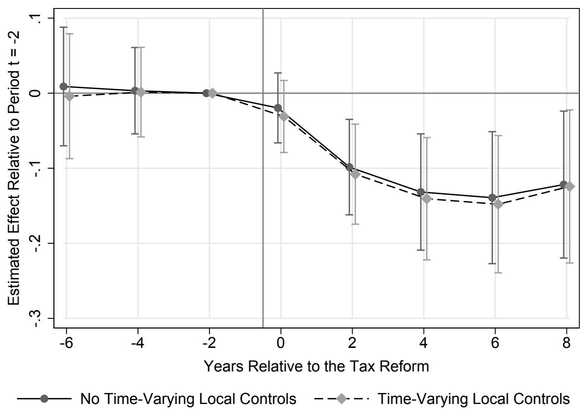
(b) Internal R&D Spending



(c) Internal Spending on R&D Personnel

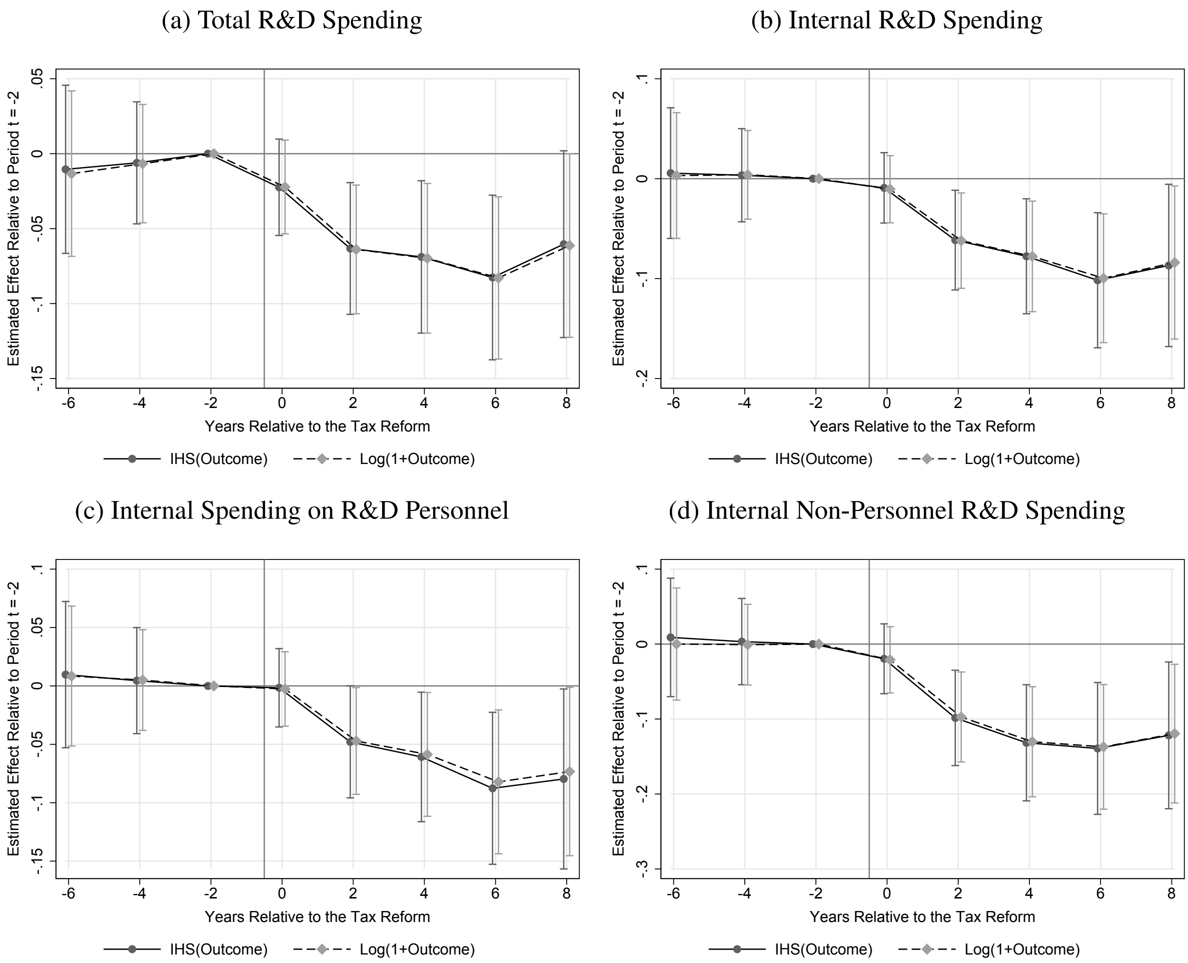


(d) Internal Non-Personnel R&D Spending



Notes: This graph plots the point estimates, $\widehat{\beta_k}$ ($k \in [-6, -4, \ldots, 8]$), and corresponding 95% confidence intervals of the event study model as defined in Equations (3.1)–(3.3) and adding local time-varying controls. In addition to these potential confounders, the regressions include plant, municipality, state × year, commuting zone × year, as well as sector × year fixed effects. All outcomes are inverse hyperbolic sine transformed. Moreover, all municipalities that experienced a tax decrease during the event window period are excluded. Standard errors are robust to clustering at the municipality level.

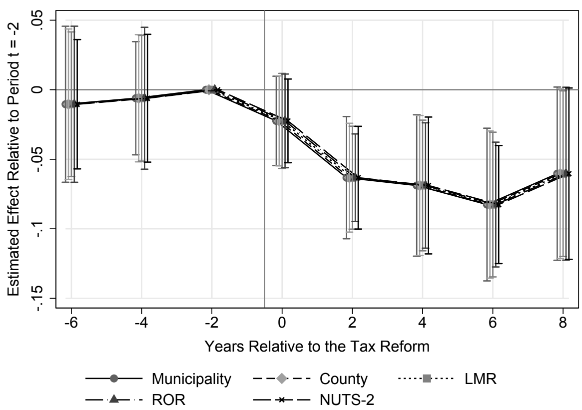
Figure C.7: The Effect of a Tax Increase on Total R&D Spending – Alternative Transformations of Y



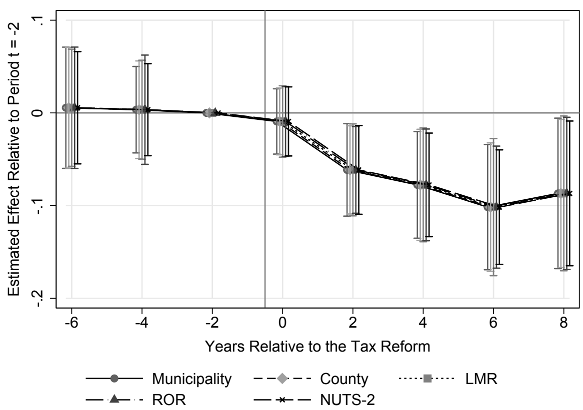
Notes: This graph plots the point estimates, $\widehat{\beta_k}$ ($k \in [-6, -4, \ldots, 8]$), and corresponding 95% confidence intervals of the event study model as defined in Equations (3.1)–(3.3) when using different transformations of the respective outcome variable. All regressions include plant, municipality, state × year, commuting zone × year, as well as sector × year fixed effects. All outcomes are inverse hyperbolic sine transformed. Moreover, all municipalities that experienced a tax decrease during the event window period are excluded. Standard errors are robust to clustering at the municipality level.

Figure C.8: The Effect of a Tax Increase on Total R&D Spending – Alternative Inference

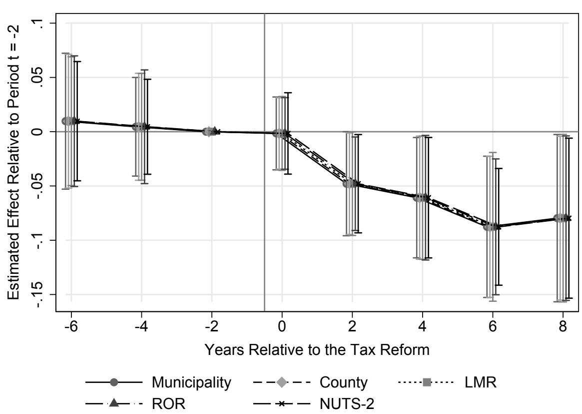
(a) Total R&D Spending



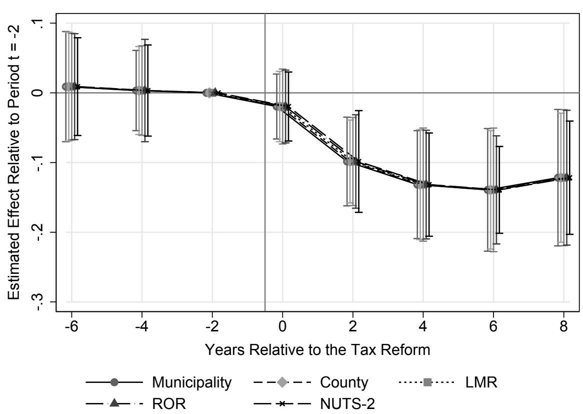
(b) Internal R&D Spending



(c) Internal Spending on R&D Personnel



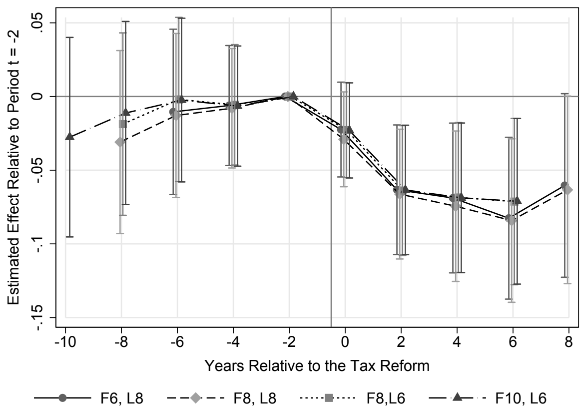
(d) Internal Non-Personnel R&D Spending



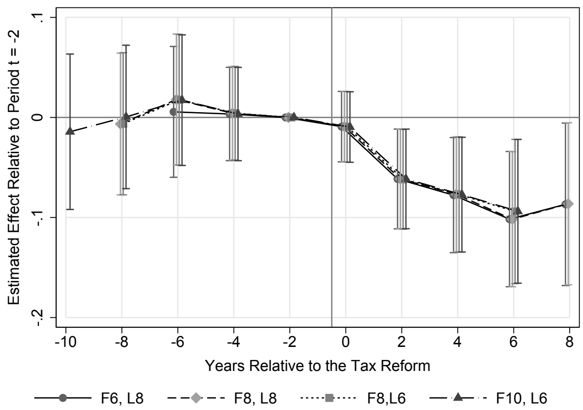
Notes: This graph plots the point estimates, $\widehat{\beta_k}$ ($k \in [-6, -4, \ldots, 8]$), and corresponding 95% confidence intervals of the event study model as defined in Equations (3.1)–(3.3) when using different ways of drawing inference, i.e., clustering standard errors at the indicated spatial jurisdictions. All regressions include plant, municipality, state × year, commuting zone × year, as well as sector × year fixed effects. All outcomes are inverse hyperbolic sine transformed. Moreover, all municipalities that experienced a tax decrease during the event window period are excluded.

Figure C.9: The Effect of a Tax Increase on Total R&D Spending – Alternative Effect Windows

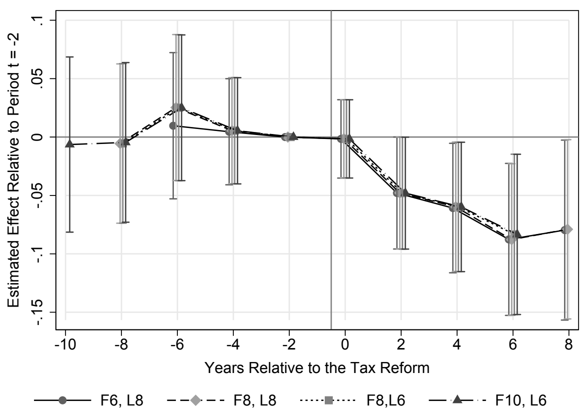
(a) Total R&D Spending



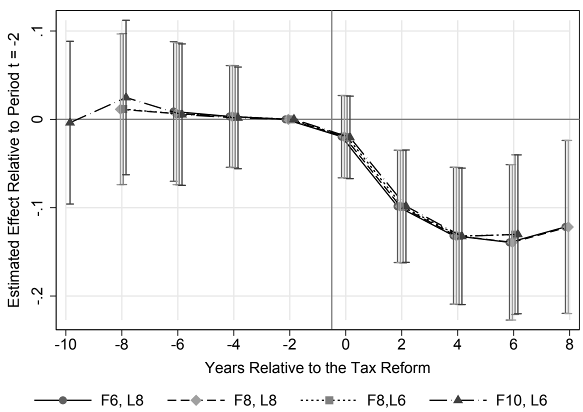
(b) Internal R&D Spending



(c) Internal Spending on R&D Personnel



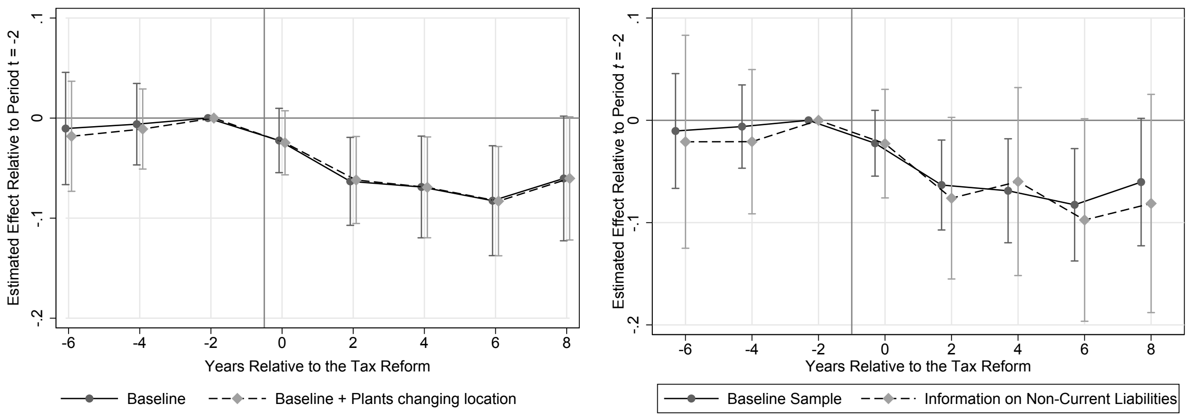
(d) Internal Non-Personnel R&D Spending



Notes: This graph plots the point estimates, $\widehat{\beta_k}$ ($k \in [-10, -8, \ldots, 8]$), and corresponding 95% confidence intervals of the event study model as defined in Equations (3.1)–(3.3) when using different effect windows. All regressions include plant, municipality, state × year, commuting zone × year, as well as sector × year fixed effects. All outcomes are inverse hyperbolic sine transformed. Moreover, all municipalities that experienced a tax decrease during the event window period are excluded.

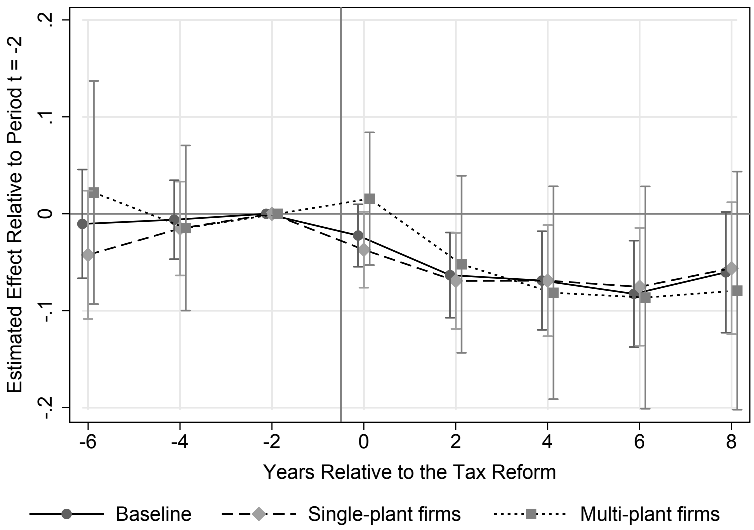
Figure C.10: The Effect of a Tax Increase on Total R&D Spending - Different Samples

(a) Inlcuding Movers (b) Information on Non-Current Liabilities



Notes: This graph plots the point estimates, $\widehat{\beta_k}$ ($k \in [-6, -4, \ldots, 8]$), and corresponding 95% confidence intervals of the event study model as defined in Equations (3.1)–(3.3) on total R&D spending when using different samples. In Panel A, add those plants that change location of residence beyond municipality borders. As treatment, we assign the corresponding tax rates from plants' initially observed location of residence. In Panel B, we limit the sample to those plants where information on non-current liabilities is available. For the treatment group, the business tax change occurred on in year $t = 0$ or $t = -1$. The regressions include plant, municipality, state × year, commuting zone × year, as well as sector × year fixed effects. All municipalities that experienced a tax increase during the event window period are excluded. Standard errors are robust to clustering at the municipality level.

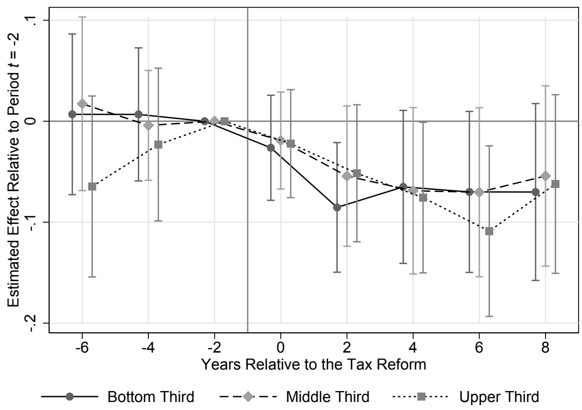
Figure C.11: The Effect of a Tax Increase on R&D Spending - Single- vs. Multi-Plant Firms



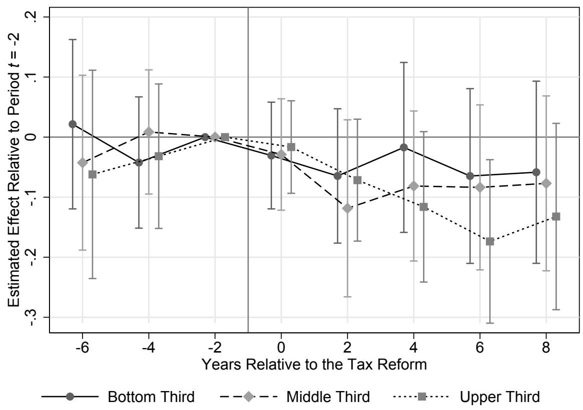
Notes: This graph plots the point estimates, $\widehat{\beta_k}$ ($k \in [-6, -4, \ldots, 8]$), and corresponding 95% confidence intervals of the event study model as defined in Equations (3.1)–(3.3) when allowing for heterogeneous effects for single- vs. multi-plant firms. The dependent variable is a plant's annual total R&D spending. For the treatment group, the business tax change occurred on in year $t = 0$ or $t = -1$. The regressions include plant, municipality, state × year, commuting zone × year, as well as sector × year fixed effects. All municipalities that experienced a tax increase during the event window period are excluded. Standard errors are robust to clustering at the municipality level.

Figure C.12: The Effect of a Tax Increase on Total R&D Spending – Heterogeneous Effects

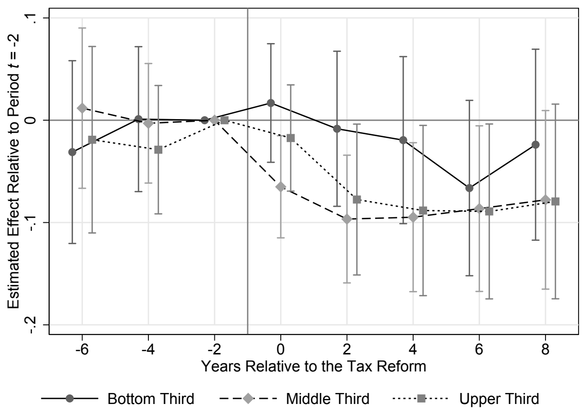
(a) Firm Size (Sales)



(b) Non-Current Liabilities over Sales



(c) R&D Intensity



Notes: This graph plots the point estimates, $\widehat{\beta_k}$ ($k \in [-6, -4, \ldots, 8]$), and corresponding 95% confidence intervals of the event study model as defined in Equations (3.1)–(3.3) when allowing for heterogeneous effects by (a) firm size, (b) the non-current liability to sales ratio, and (c) R&D intensity. In Panel A, we group plants according to their median sales during the observation period. In Panel B, we distinguish plants according to their average non-current liability to sales ratio, and in Panel C according to their R&D intensity (as defined via the share of R&D staff over total employees). All regressions include plant, municipality, state × year, commuting zone × year, sector × year, as well as bin × year fixed effects. The outcome variable is inverse hyperbolic sine transformed. Moreover, all municipalities that experienced a tax decrease during the event window period are excluded.

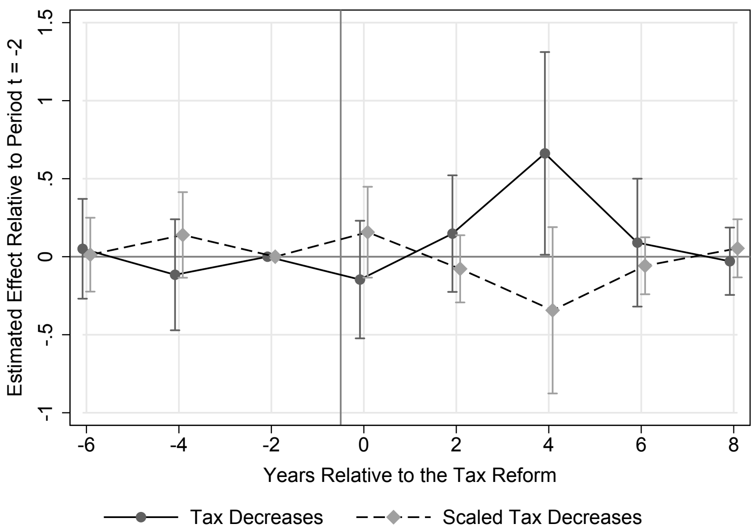
C.4 Additional Results on Patenting

Table C.3: Implied Long-Term Elasticities and Oster Bounds

	Number of Filed Patents	Citation-Weighted Number of Filed Patents
A. Uncontrolled Estimates		
Point Estimate	-0.87	-0.94
B. Controlled Estimates		
Point Estimate	-0.84	-0.85
C. Bounded Oster Estimates		
Point Estimate	-0.81	-0.72

Notes: This table displays the implied long-term elasticity of plants' (citation-weighted) number of filed patents with regard to an increase in the LBT rate. In Panel A, we report our baseline elasticities as given in Figure 3.9. In this specification, we do not control for time-varying local confounders at the municipality or county level. In Panel B, we display the corresponding elasticities when accounting for potential confounders at the municipality or county level. Last, in Panel C, we provide bounded estimates of these elasticities in the spirit of Oster (2019). In detail, we calculate the corresponding elasticities via the following formula: $\beta^* = \tilde{\beta} - [\dot{\beta} - \tilde{\beta}]\,\frac{R_{max}-\tilde{R}}{\tilde{R}-\dot{R}}$, where $\dot{\beta}$ and $\dot{R}$ refer the uncontrolled elasticity and R-Squared and $\tilde{\beta}$ and $\tilde{R}$ to the controlled elasticity and corresponding R-Squared, respectively. Moreover, we set R_{max} to $1.3 \times \tilde{R}$. The framework assumes that coefficient stability with regard to observable confounders may inform about the importance of omitted variable bias if the importance of the controls in explaining the variance of the outcome is accounted for.

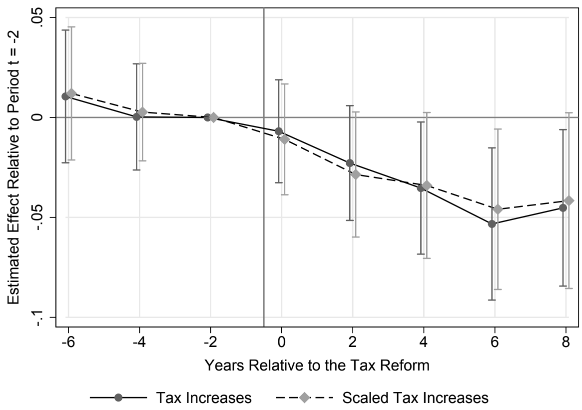
Figure C.13: The Effect of a Business Tax Decrease on Patents



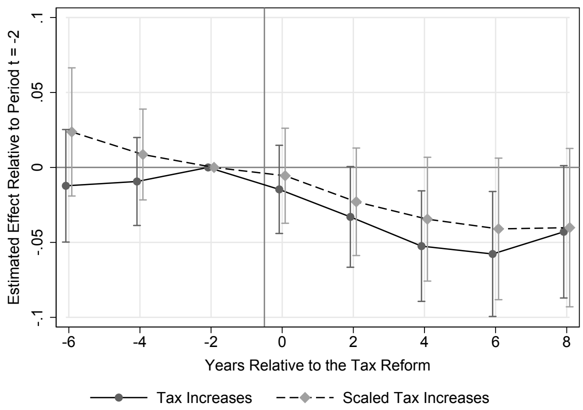
Notes: This graph plots the point estimates, $\widehat{\beta_k}$ ($k \in [-6, -4, \ldots, 8]$), and corresponding 95% confidence intervals of the event study models as defined in Equations (3.1)–(3.4). The outcome refers to a plant's annual citation-weighted number of patents. It is inverse hyperbolic sine transformed. For the treatment group, the business tax change occurred on in year $t = 0$ or $t = -1$. The regressions include plant, municipality, state × year, commuting zone × year, as well as sector × year fixed effects. All municipalities that experienced a tax increase during the event window period are excluded. Standard errors are robust to clustering at the municipality level.

Figure C.14: The Effect of a Tax Increase on Patents - EPO vs. USTPO citations

(a) EPO-Citation-Weighted Number of Patents



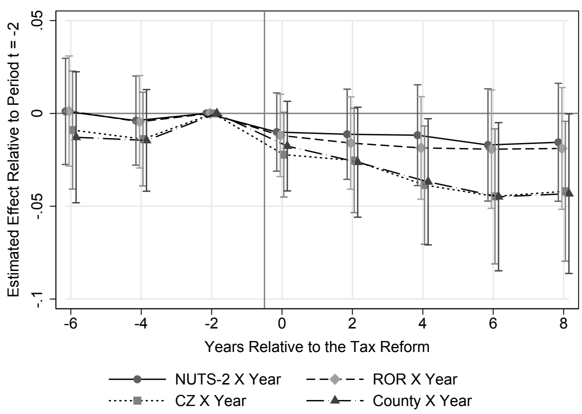
(b) USTPO-Citation-Weighted Number of Patents



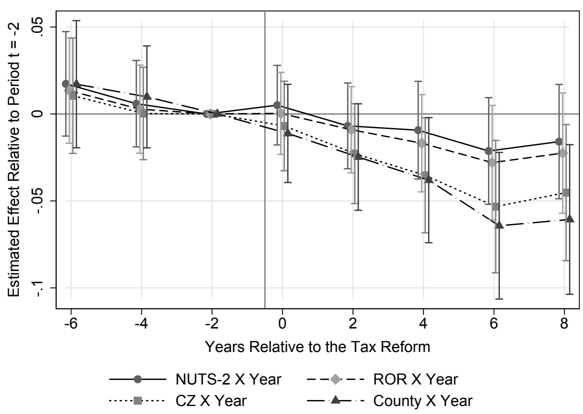
Notes: This graph plots the point estimates, $\widehat{\beta_k}$ ($k \in [-6, -4, \ldots, 8]$), and corresponding 95% confidence intervals of the event study model specifications as defined in Equations (3.1)–(3.4). In Panel A, a given patent is weighted by the number of citations it receives from patents filed at the EPO during the first five years after its registration. In Panel B, weights are calculated according to the number of citations a patent receives in the USTPO within the same time horizon. Both outcomes are inverse hyperbolic sine transformed. For the treatment group, the business tax change occurred on in year $t = 0$ or $t = -1$. The regressions include plant, municipality, state × year, commuting zone × year, as well as sector × year fixed effects. All municipalities that experienced a tax increase during the event window period are excluded. Standard errors are robust to clustering at the municipality level.

Figure C.15: The Effect of a Tax Increase on Patents – Varying Regional Controls

(a) Number of Patents



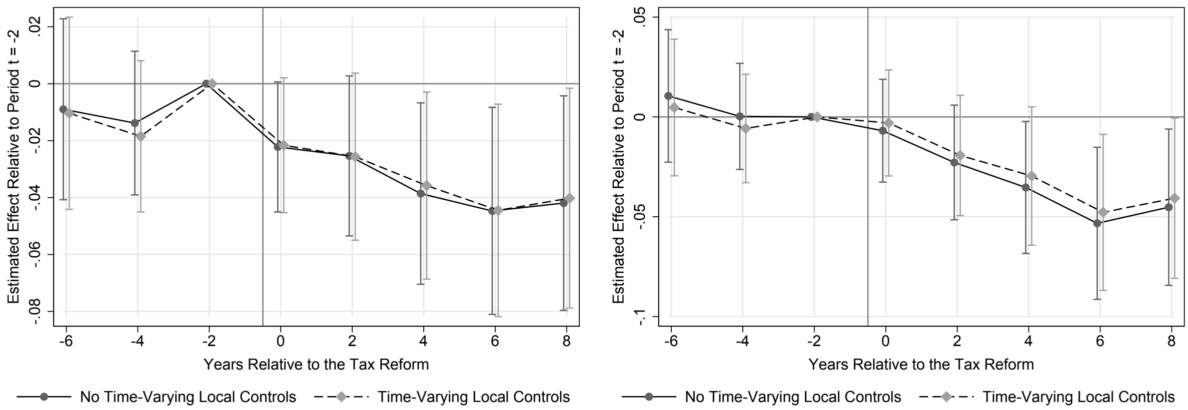
(b) Citation-Weighted Number of Patents



Notes: This graph plots the point estimates, $\widehat{\beta_k}$ ($k \in [-6, -4, \ldots, 8]$), and corresponding 95% confidence intervals of the event study model as defined in Equations (3.1)–(3.4) when using different set of regional × year fixed effects. Moreover, all regressions include plant, municipality, as well as sector × year fixed effects. For the treatment group, the business tax change occurred on in year $t = 0$ or $t = -1$. All outcomes are inverse hyperbolic sine transformed. Moreover, all municipalities that experienced a tax decrease during the event window period are excluded. Standard errors are robust to clustering at the municipality level.

Figure C.16: The Effect of a Tax Increase on Patents – Local Time-Varying Controls

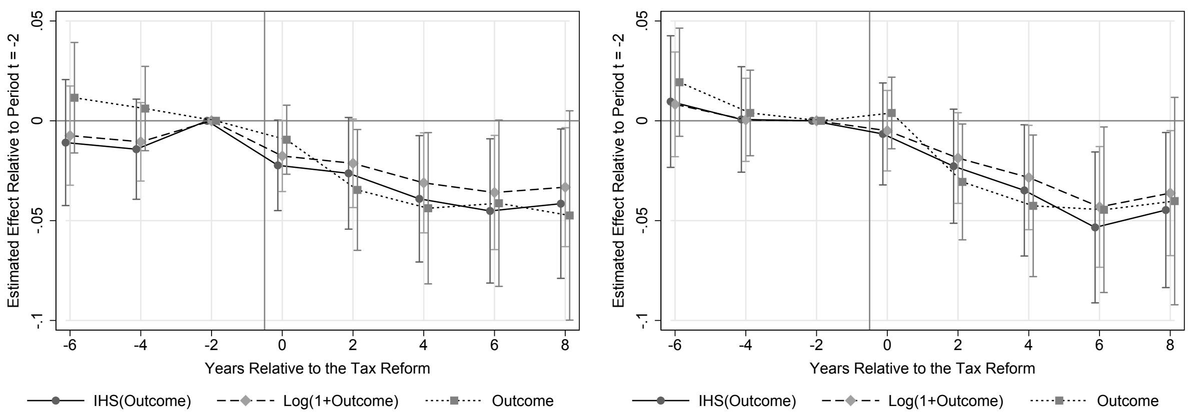
(a) Number of Patents (b) Citation-Weighted Number of Patents



Notes: This graph plots the point estimates, $\widehat{\beta_k}$ ($k \in [-6, -4, \ldots, 8]$), and corresponding 95% confidence intervals of the event study model as defined in Equations (3.1)–(3.4) when (not) controlling for time-varying local characteristics. Moreover, all regressions include plant, municipality, state × year, commuting zone × year as well as sector × year fixed effects. For the treatment group, the business tax change occurred on in year $t = 0$ or $t = -1$. All outcomes are inverse hyperbolic sine transformed. Moreover, all municipalities that experienced a tax decrease during the event window period are excluded. Standard errors are robust to clustering at the municipality level.

Figure C.17: The Effect of a Tax Increase on Patents – Alternative Transformations of Y

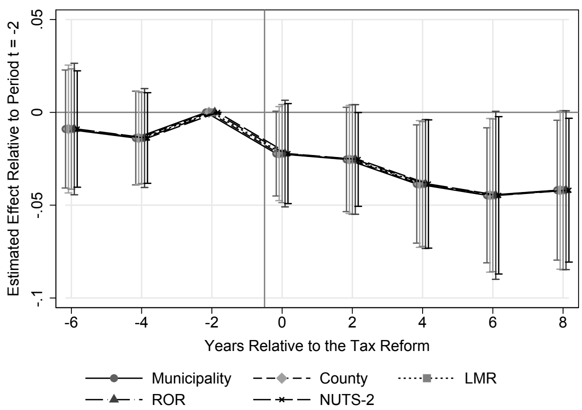
(a) Number of Patents (b) Citation-Weighted Number of Patetns



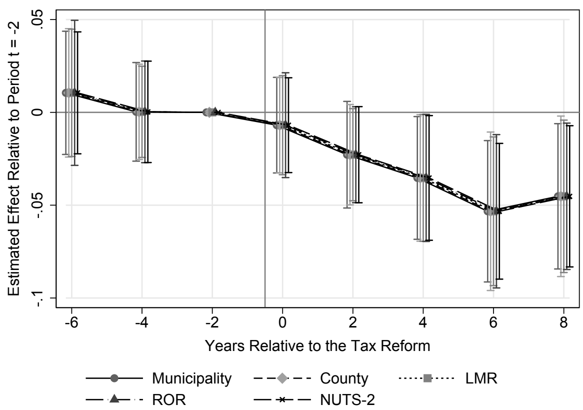
Notes: This graph plots the point estimates, $\widehat{\beta_k}$ ($k \in [-6, -4, \ldots, 8]$), and corresponding 95% confidence intervals of the event study model as defined in Equations (3.1)–(3.4) when using different transformations of the outcome variable. To ease presentation, the level outcome is standardized. All regressions include plant, municipality, state × year, commuting zone × year as well as sector × year fixed effects. For the treatment group, the business tax change occurred on in year $t = 0$ or $t = -1$.. Moreover, all municipalities that experienced a tax decrease during the event window period are excluded. Standard errors are robust to clustering at the municipality level.

Figure C.18: The Effect of a Tax Increase on Patents – Alternative Inference

(a) Number of Patents



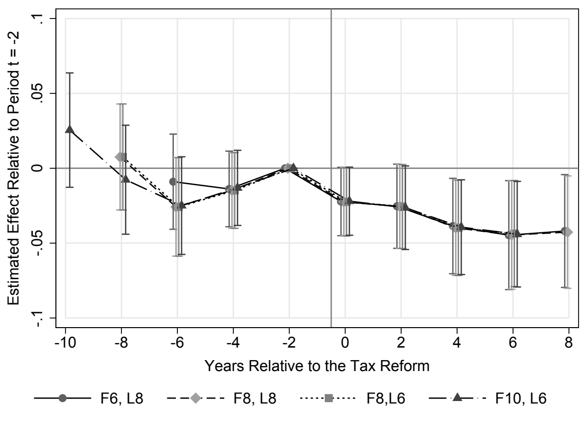
(b) Ctiation-Weighted Number of Patents



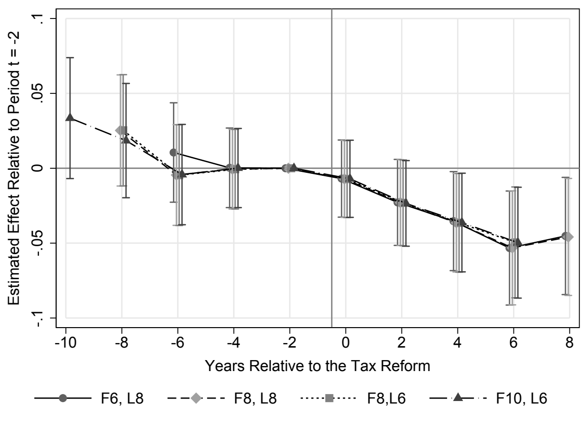
Notes: This graph plots the point estimates, $\widehat{\beta_k}$ ($k \in [-6, -4, \ldots, 8]$), and corresponding 95% confidence intervals of the event study model as defined in Equations (3.1)–(3.3) when using different ways of drawing inference, i.e., clustering standard errors at the indicated spatial jurisdictions. All regressions include plant, municipality, state × year, commuting zone × year, as well as sector × year fixed effects. All outcomes are inverse hyperbolic sine transformed. Moreover, all municipalities that experienced a tax decrease during the event window period are excluded.

Figure C.19: The Effect of a Tax Increase on Patents – Alternative Effect Windows

(a) Number of Patents

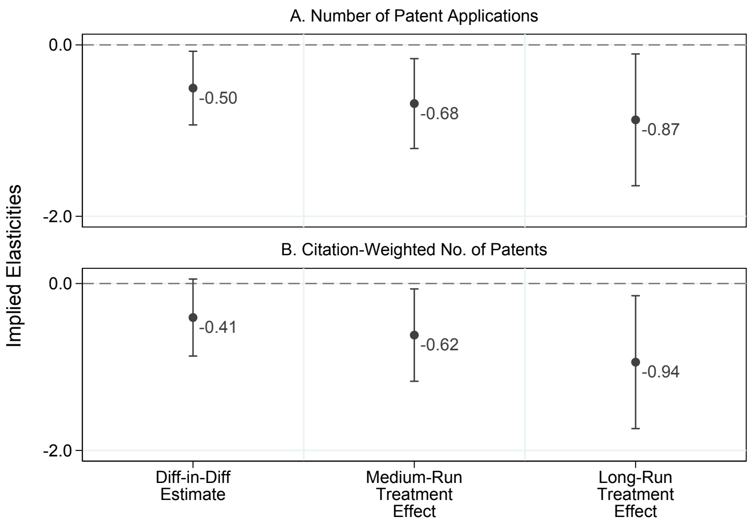


(b) Ctiation-Weighted Number of Patents



Notes: This graph plots the point estimates, $\widehat{\beta_k}$ ($k \in [-10, -8, \ldots, 8]$), and corresponding 95% confidence intervals of the event study model as defined in Equations (3.1)–(3.3) when using different effect windows. All regressions include plant, municipality, state × year, commuting zone × year, as well as sector × year fixed effects. All outcomes are inverse hyperbolic sine transformed. Moreover, all municipalities that experienced a tax decrease during the event window period are excluded.

Figure C.20: Alternative Implied Elasticities – Patents



Notes: This graph displays implied elasticities for our two baseline patent outcomes with respect to a change in the local business tax rate. For each panel (outcome), we display the corresponding elasticity when (i) estimating a simple Diff-in-Diff model with the log LBT rate as the explanatory variable, (ii) taking the mean over the first four post-reform treatment coefficients from the event study specification as defined in Equation (3.3), or (iii) taking the last treatment effect (β_8) from the same event study model.

D

Appendix to Chapter 4
Competition and Innovation: The IG Farben Breakup

D.1 Data

D.1.1 Products and Prices

Both product and price data is derived from publication from and for experts in the chemical industry. They are designed to give an overview over the market situation and as such paint a detailed picture of the contemporary industry structure. They are digitized in a mixture of OCR, automated processing and matching in the von Gaudecker (2019) framework and many manual additions.

Product Data Product catalogs list, for a large number of chemical products, the firms supplying them. The analysis restricts to chemical substances. These are, in economic terms, homogeneous intermediate products. Differentiated consumer products, for example industrial cleaners or paints, are excluded. Figure D.1 shows an example of how product listings look like. Typically, a chemical is given by its German name as well as translations in several other languages. Subsequently follows a list of chemical companies that the chemical can be procured from. A separate part of the book lists contact information for the companies such as address and telephone number.

The introductory remarks in each of the volumes describes the process of their creation and their content. Specifically, the chemical industry is described as producing a myriad of final products from a small set of inputs, which necessitates listing only products usual in trade (Wegner, 1940; Barth, 1952). The total number of potential individual products was given in 1939 as 60,000 and as 100,000 in 1953 (Wegner, 1940; Wegner, 1953). The information is based on information given by the producers themselves and appearance in the volume is free of charge (*Wenzel* 1930). The books finance themselves by featuring advertisements in the books themselves as well as by the sales price. However, if firms object and want to keep information secret, products will not be listed (*Wenzel* 1938). The books also typically do not list foreign suppliers. Until 1932, a parallel publication series tried to keep track of this different set of firms but this effort proved too cumbersome. Specific events impacting the contents of the books are commented on. The 1940 edition for example remarks that war-related changes could not be represented in the book to not delay its publication further, whereas firms from recently occupied areas are covered (Wegner, 1940). As the editorial was written in December 1939, this references the recent invasion of Poland. Henceforth, the volume entitled 1940 is referenced by the date of its publication, 1939. The 1952 edition (Barth, 1952, editorial dated April 1952) describes itself as the first address and product listing of the West-German chemical industry since the end of the war. Turnover of firms between editions is typically high, the 1930 edition drops 1500 firms and adds 600 new ones (*Wenzel* 1930). The 1938 edition claims to have added 3000 new producing and trading firms (*Wenzel* 1938). Based on these remarks, the listed products represent the current supplier status of Germany's chemical industry with respect to a cross-section of common, relevant products.

Individual product-year entries are linked between volumes using alternative names and adjusted string similarity. For example, Phthalic acid is also called Benzene-1,2-dioic acid and is cross-references to Phthalic anhydrate, which is Phthalic acid with one molecule of water removed. Alternative names are mostly sourced from the product books, but also from looking up product names in the German Wikipedia. Linkage using string similarity is added to deal with OCR mistakes. However, similarity between chemical names is treacherous, as for example sulfide and sulfate (German: Sulfit/Sulfat) are

Figure D.1: Product listing examples in 1939 and 1952

(a) ASS, 1939

Acetylsalicylsäure [Acidum acetylosalicylicum], *e.* Acetylsalicylic acis, *f.* Acide acétylsalicylique, *sp.* Acido acetilsalicílico, *i.* Acido acetilsalicilico.
»Bayer« I. G. Farbenindustrie Aktiengesellschaft, Leverkusen-I. G. Werk [DAB. 6].
Chemische Fabrik Aubing, Aubing b. München.
Chemische Fabrik von Heyden A.-G., Radebeul-Dresden.
Gehe & Co. A.-G., Dresden-N 6.
I. G. Farbenindustrie Aktiengesellschaft, Frankfurt a. M.
E. Merck, Darmstadt.
J. D. Riedel—E. de Haën A.-G., Berlin.
Schering A.-G., Berlin N 65.

(b) Phthalic anhydride, 1939

Phthalsäureanhydrid [Acidum phthalicum anhydricum], *e.* anhydrous, *f.* anhydre, *sp.* anhidro, *i.* anidro.
I. G. Garbenindustrie Aktiengesellschaft, Frankfurt a. M.
J. D. Riedel—E. de Haën A.-G., Berlin.

(c) ASS, 1952

62. **Acetylsalicylsäure**
Acetylsalicylic acid
Acide acétyl-salicylique
Acido acetilsalicilico
Chemische Fabrik Aubing Dr. Kurt Bloch, München-Aubing
Chemische Fabrik von Heyden AG, München 23
Farbenfabriken Bayer Aktiengesellschaft, Leverkusen
Farbwerke Hoechst AG, vorm. Meister Lucius & Brüning, Frankfurt/M.-Höchst
E. Merck, Chemische Fabrik, Darmstadt

(d) Phthalic anhydride, 1952

5538. **Phthalsäureanhydrid**
Phthalic anhydride
Anhydride phtalique
Anhidrido ftálico
Badische Anilin- & Soda-Fabrik, Ludwigshafen/Rh.
Dr. Kurt Herberts & Co. vorm. Otto Louis Herberts, Wuppertal
Farbenfabriken Bayer Aktiengesellschaft, Leverkusen
E. Merck, Chemische Fabrik, Darmstadt
Ruhröl GmbH, Bottrop

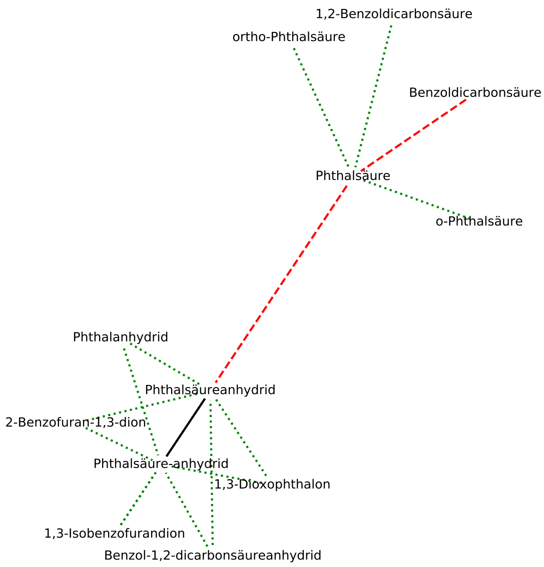
Notes: Entry from 1939 and 1952, where ex-post IG Farben successors competed with each other. Acetylsalicylic acid, better known as Aspirin, is a pharmaceutical product. Phthalic anhydride is an input product to the dyestuffs, plastics and pharmaceutical industry. Acetylsalicylic acid was in 1939 offered by IG Farben (with two listings, one as "Bayer") and several others. In 1952, with Bayer and Hoechst, two IG Farben successors as well as many of the previously active non-IG suppliers offer the product. For phthalic anhydride, BASF and Bayer compete in 1952, after the product was already offered by IG Farben in 1939.

very similar, yet chemically different. To this end, similarity is adjusted by up-weighting the beginning and the end of substance names and by down-weighting common OCR mistakes (e.g. l vs i vs I). A set of training data is used to estimate a regularized logistic regression of a set of string similarity measures, which yields the similarity score. Only candidates with very high similarity are kept. The result of the overall procedure is a network of linked alternative names.

From supplementary sources (Wikipedia and ChemSpider), properties of chemical substances can be extracted. This is in particular the chemical composition. The molar weight gives a first impression of a molecule's complexity. From the formula, the heaviest atom can be identified. In inorganic chemistry, especially for metal salts, this atom can drive a large part of the molar weight.

Price Data Price data is taken from industry journals ("Chemie-Ingenieur-Technik", from 1953 also the insert "Chemiemarkt" to "Chemische Industrie"), where it was part of reports on the general market situation. The price information does not intend to capture the final price paid by customers, which

Figure D.2: Name network for Phthalic acid



Notes: Red dashed links are product catalog cross-references or alternative names, green dotted links alternative names derived from Wikipedia. Black solid links rely on text similarity.

would depend on too many details. Instead, they describe producer prices at the factory gate. When there is substantial variance of price across producers, the lists indicate price spans ("Was bieten unsere Preisberichte" 1952).

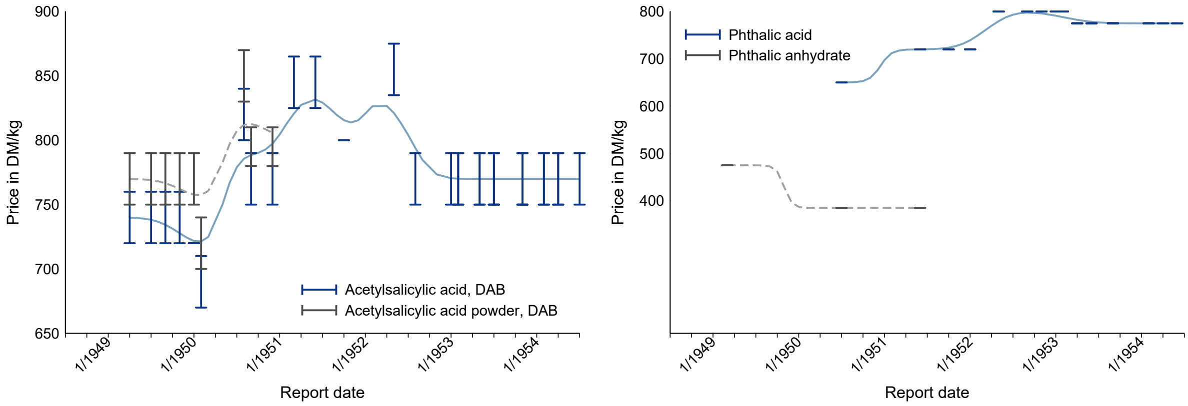
The quality of the price lists improves over time. The chaos of the early post-war period only gradually allowed the production of such lists, as high variance across firms and various disruptions rendered information notoriously unreliable ("Was bieten unsere Preisberichte" 1952). The price lists in 1948/1949 are short and sparse, whereas the later lists are more detailed and comprehensive. Both pattern lead to larger variance listed prices in the early periods. By mid-1949, however, the situation seems to have normalized sufficiently. Figure 4.10 in the main text plots average prices by supplier status, where the partially extreme price levels of 1948/early 1949 are clearly visible. Most analyses will restrict to the later time periods.

Before and during the war, government controls render price information meaningless, but data on innovation activity as well as industry structure allows tracking the German chemical industry until the outbreak of war. More high-level price data covering longer periods is available from the German Statistical Yearbook. Here, price trends before and after the war can be compared. Before the war, lack of changes in prices indicate that rigid price controls were in place, so that such a pre-post comparison is uninteresting. Price controls were maintained after the war. Between 1945 and 1948, prices as of 31.12.1944 were fixed (Fäßler, 2006, p. 42).

The price lists report various purity levels or delivery variants, exemplified in Figure D.3. For example, Acetylsalicylic acid is available in the standard form and as powder - always in pharmaceutical grade (denoted DAB). For other products, the distinction is between in purity grades, for example 'for technical processes' and 'pure' ($\geq 97\%$ product). In other cases, cross-references in the product books group very similar chemical substances together, such as Phthalic anhydrate and Phthalic acid (The former is the

Figure D.3: Reported prices and quality information

(a) Prices for Acetylsalicylic acid (b) Prices for Phthalic anhydrate / Phthalic acid



Notes: Shows prices reported in industry journals for two selected substances. Phthalic anhydrate is Phthalic acid with water removed. The two substances were grouped based on cross-references (alternative names) from the product books. DAB is an abbreviation for the contemporary German pharmaceutical standard ("Deutsches Arzneibuch"). When price information is given as ranges as for Acetylsalicylic acid, midpoints are used in the analysis.

latter with one water molecule removed). In such cases, the time series were inspected manually and separated if different tendencies or price levels exist between purity grades. In the regression, only one time series related to Acetylsalicylic acid would occur as the 'powder' variant has no post-1952 data. The distinction by quality grades is typically not reflected in the product catalogs and price series are matched to the best available fit.

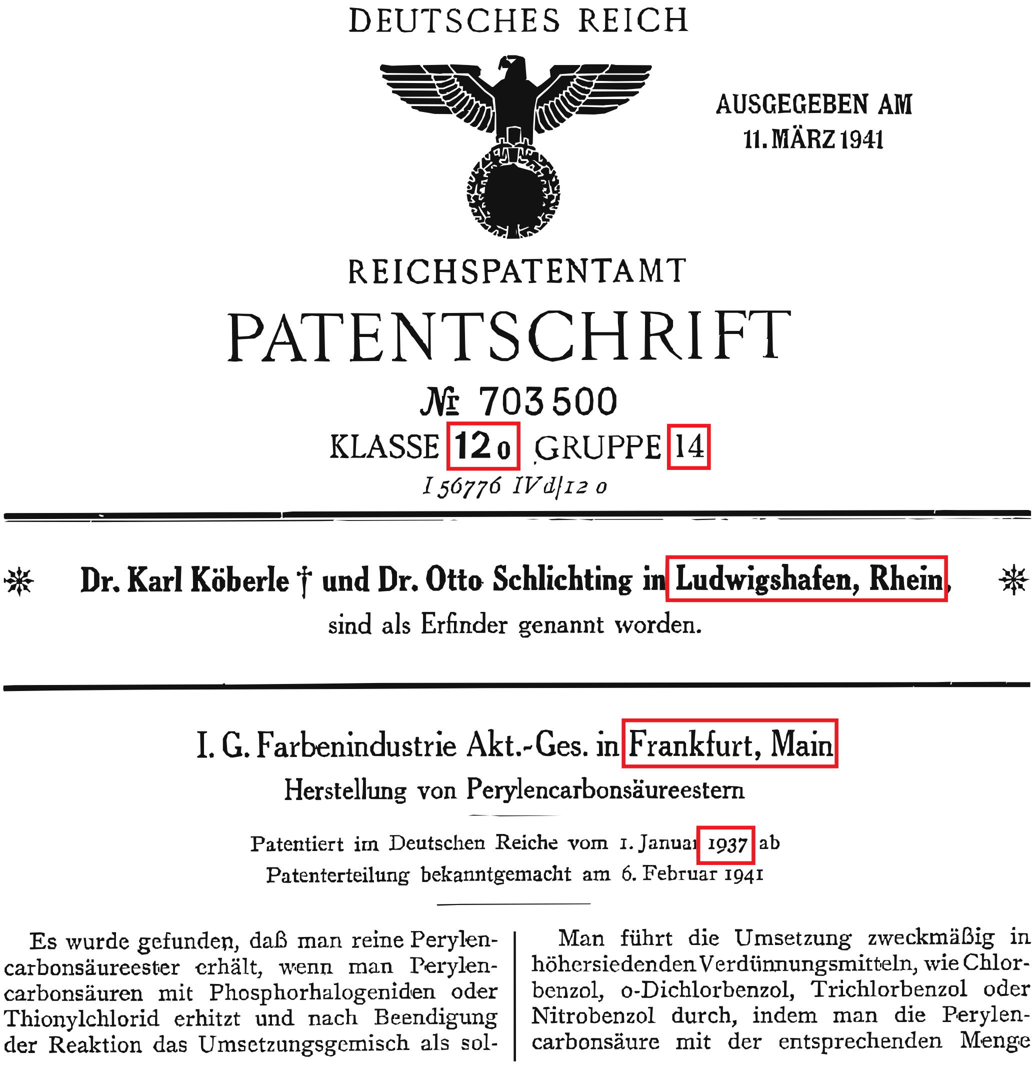
The change in availability of price time series over time requires several adjustment. In a typical month 30-40% of all prices are reported, increasing over time. For the price panel, several cleaning steps are undertaken. First, only prices for products linked to the 1939 or 1952 product catalogs are kept. Price time series must have at least one observation before or in Q2/1950 and after or in Q2/1952, leaving around 560 time series. Time series with large gaps or five or fewer price entries are dropped (Approximately 20). Products with extreme price changes are dropped (Factor > 4 since 01/1950, approximately 10 price series).

D.1.2 Patents

For the analysis, various parts of the information contained on individual patents is required. While some data could be acquired from the German patent office, much of the needed information has to be acquired through image processing or OCR and subsequent text processing. These are especially the technology class, applicant name, inventor location and application year. Here, a largely automated processing pipeline was designed which delivers highly accurate information for almost all patent documents.

Year Information The German patent office was first set up in 1877, although successors existed in the various German states. It handled German IP matters until mid-1945, when it closed. It remained so until 1948, when preliminary offices were established. These accepted patent applications, but processing started only in 1950. By then, also wartime applications were processed. Therefore, patent statistics

Figure D.4: IG Farben patent



DEUTSCHES REICH

AUSGEGEBEN AM
11. MÄRZ 1941

REICHSPATENTAMT

PATENTSCHRIFT

№ 703500

KLASSE 12o GRUPPE 14

I 56776 IVd/12 o

Dr. Karl Köberle † und Dr. Otto Schlichting in Ludwigshafen, Rhein,
sind als Erfinder genannt worden.

I. G. Farbenindustrie Akt.-Ges. in Frankfurt, Main
Herstellung von Perylencarbonsäureestern

Patentiert im Deutschen Reiche vom 1. Januar 1937 ab
Patenterteilung bekanntgemacht am 6. Februar 1941

Es wurde gefunden, daß man reine Perylencarbonsäureester erhält, wenn man Perylencarbonsäuren mit Phosphorhalogeniden oder Thionylchlorid erhitzt und nach Beendigung der Reaktion das Umsetzungsgemisch als sol-

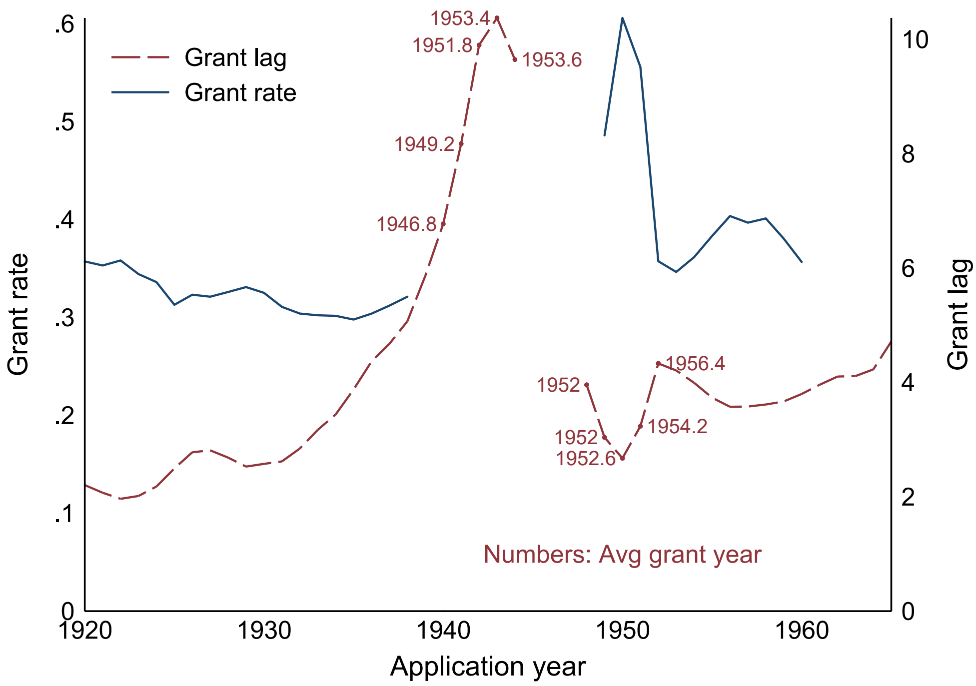
Man führt die Umsetzung zweckmäßig in höhersiedenden Verdünnungsmitteln, wie Chlorbenzol, o-Dichlorbenzol, Trichlorbenzol oder Nitrobenzol durch, indem man die Perylencarbonsäure mit der entsprechenden Menge

Notes: Example patent. Highlighted are technology class (12o) and group (14). Further, inventor location (Ludwigshafen) and application year (1937) are marked.

show a gap in the years 1946 and 1947, but are available from 1948 onwards. Figure D.5 shows the difference between application and grant year for patents where this information is available. Note the strongly increasing grant lag for war-time patents, implying that patents applied for during these years are typically granted when when technological requirements have already changed. As a consequence, applicants might have only selectively pursued these patents, leading to selection issues.

In historical patent records from before 1945, only granted patents (“Patentschriften”) are available. To ensure a correct pre-post comparison, this study therefore disregards applications which were not ultimately granted, even when this data is available. Figure D.5 shows the grant rate by comparing the number of granted patents in the data with the number of applied patents from administrative publications. A comparison of the number of granted patents (completeness of the data) is impossible as the administrative publications list granted patents by their grant year. In the long run, the grant rate remains roughly the same, although a policy of limited novelty checks at reopening yields a temporarily much higher grant rate.

Figure D.5: Patent grant lags



Notes: By application year, shows grant lag and grant rate of German patents. Grant lag is computed as difference between grant and application year, when both information is available. Grant rate is computed as a comparison of the annually filed patent applications as reported in the "Blatt für Patent-, Muster- und Zeichenwesen." 1948 and 1949 are jointly reported and thus collapsed.

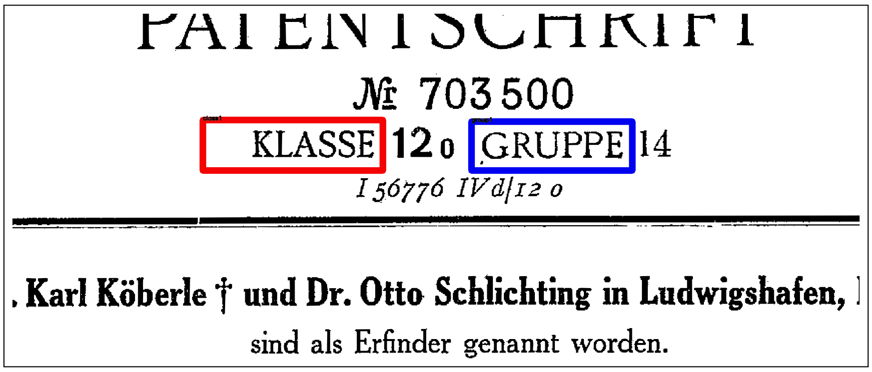
Technology Class Information The German patent office classified technologies into 89 major and roughly 540 minor technology classes. Descriptions of these technology classes from 1910 and 1949 (*Taschenbuch des Patentwesens* 1910; Deutsches Patent- und Markenamt, 1949) show that at this level, the content of the technology classes remains almost always the same.

The descriptions of the technology classes as well as the 1949 technology groups enable classification of the technology classes into such that are directly relevant for the chemical industry. This classification includes classes from health care, photography and agriculture that have a relevant relation to the chemical industry. This yields 135 classes. The paragraph on text quality measures below validates this definition with patent lists featuring significant advances in inorganic chemistry.

While the technology class information is available on patent publications, making them available for data analysis presented a major challenge. Standard OCR (Tesseract) proved to be too unreliable because the technology classes are numbers and letters without context in the middle in the document. Therefore, OCR had to be augmented with a pattern recognition algorithm designed directly for the font used. Figure D.6 demonstrates the process. First, in the relevant subsection of the patent scan, the location of the technology class and group are identified. For this, image templates of the "KLASSE" and "GRUPPE" strings are matched to the scan. Over time, with the layout of the patents the actual font and templates also change. Especially the processing of the letters is often incorrect so that they are matched to a set of templates, based on problems identified in the training data. The letter font also changes over time, requiring multiple sets of templates. All areas that are known to be blank, for example behind the matched latter, are painted white to remove manual markings and other noise. Finally, the remainder of the technology class string is processed using OCR. In addition to this general process, some automatic corrections are applied. For example 3 and 8 are often confused, also a set of rule-based automatic corrections removes technology class letters that, from knowing the technology class list, cannot occur. This process relies on OpenCV (https://opencv.org/) in combination with Tesseract (https://github.com/tesseract-ocr/tesseract).

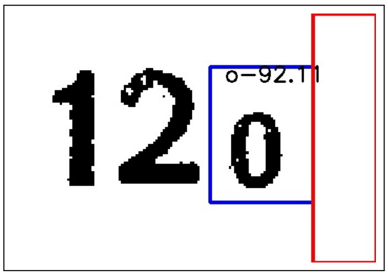
Figure D.6: Technology class extraction

(a) Locate technology class using image templates



(b) Extracted technology class

(c) Match of letter ‘o’



Notes: Process of extracting the technology class, based of the example in Figure D.4. First, the locations of the technology class within the document is identified, to reduce variance from the input documents (D.6a). As a result, the technology class snippet is extracted (D.6b). Based on extracts, the correct letter is identified (D.6c). Standard OCR can identify the remaining numbers sufficiently well.

Based on manual training data, it was possible to retrieve the technology class information with up to 95% accuracy. Most of the cases where the algorithm was unsuccessful, the quality of the underlying image is problematic and manual processing is required. For example, many documents before 1900 were manually reclassified to the level of the minor patent class. These manual additions lead to problems, as Table D.1 shows.

Applicant and Inventor Information Applicant and inventor information is extracted from the OCR using machine learning. First, the precise location of applicant/inventor strings is ascertained using keywords. For example “sind als Erfinder genannt worden” (were named as inventors) signifies that the inventors are named just before. The necessary keywords change with the layout of the patents over time. The second part, the actual processing, is visualized in Table D.2. Here, the applicant/inventor strings are tokenized (split into words) and each token is assigned to a label. These labels signify the meaning of a particular token, for example whether it is part of the applicant name, a location, the patent title or a particular part of the inventor name. With labels such as ‘in_word’ and ‘und_word’ (for ‘and’), the syntactic structure can be captured as well. The label assignment uses conditional random fields, where

Table D.1: Quality indicators for technology class processing

	All		Excluding bad input	
	Count	Correct (%)	Count	Correct (%)
1877-1900	172	77.33	138	93.48
1901-1920	531	92.66	514	95.72
1921-1933	275	98.18	272	98.53
1934-1945	344	97.38	342	97.95
1948-1949	780	97.69	779	97.82
1950-1954	101	98.02	97	100.00
1955-1961	478	93.10	457	95.62
later	67	98.51	66	98.48
Total	2748	94.69	2665	97.00

Notes: Quality indicators by application years of patents, based on randomly selected patent documents. The two rightmost columns exclude patents where bad input data makes correct processing impossible. The predominant reason are manual, handwritten additions (before 1900) or changes of the technology class.

a model based on a set of string features (which are based on the tokens) executes the classification.[1] Features from the current as well as the two preceding and subsequent tokens enter the calculation. The model is trained based on a convenience sample training data, mostly designed to teach the model about strings where weaknesses were observed. The advantage of this method over standard rule-based classification and extraction is twofold. First, rules do not need to be implemented precisely by hand. Second, the algorithm is rather robust to the frequent problems occurring within the OCR.

Before application year 1938, most patents do not have inventor information, but large firms and especially IG Farben do. Figure D.7b shows the share of patents with inventor information for different groups. For some time, supplying inventor information was voluntary, which was only changed with the 1936 reform of the German patent law. Here, the inventors' right to be named on patent applications was first introduced. In the end, the inventor information could be recovered for about 90% of all IG Farben patents. In the remainder, the inventor information was typically intentionally omitted from the document.

Text Analysis and Quality Measures The first step of the text analysis is to find a numerical representation of the documents (patent fulltexts) to compute similarity scores between them. Text analysis is done based on Angelov (2020)'s wrapper of Doc2Vec (Le and Mikolov, 2014). Doc2Vec is advantageous compared to the bag of word (TF-IDF)-based numerical representations that are often used in the economic literature. For one, it is able to take the context of a word into account. Also, it is designed to incorporate the structure of documents. Finally, Doc2Vec has some ability to take into account different writing variants of the same word, which alleviates the necessity for stemming and lemmatization and makes it more robust against OCR errors. The calculation with Doc2Vec results in a set of document vectors D_i, between which the similarity is calculated as the cosine similarity.

$$S_{ij} = cos(D_i, D_j) \tag{D.1}$$

[1]The 'parserator' package in Python does the heavy lifting, see https://github.com/datamade/parserator

Table D.2: Extraction of applicant and inventor details

Header		Inventor	
OCR	Classification	OCR	Classification
DE000000703500A	pn	DE000000703500A	pn
.	trash	IWDLIZ	trash
	trash	O	trash
	trash		trash
I.	name		trash
G.	name	2	trash
FARBENINDUSTRIE	name	ME	trash
AKT.-	name	NN	trash
GES.	name		trash
IN	in_word		trash
FRANKFURT,	location	*	trash
MAIN	location	DR.	title_name
	trash	KARL	first_name
HERSTELLUNG	title	KÖBERLE	other_name
VON	title	F	other_name
PERYLENCARBONSÄUREESTERN	title	UND	und_word
	trash	DR.	title_name
	trash	OTTO	first_name
	trash	SCHLICHTING	last_name
	trash	IN	in_word
	trash	LUDWIGSHAFEN,	location
	trash	RHEIN,	location

Notes: For patent DE000000703500A (Figure D.4), demonstrates the result of the conditional random field parsing of applicant and inventor. Parts of the document were first identified to contain applicant and inventor information. These parts of the documents are then tagged as applicant name, location or different parts of inventor names.

Calculating a vector space for a very large number of patents computationally demanding, but converges in reasonable time for the roughly 250,000 fulltexts of patent grant documents in the timespan of interest for chemical patents. To speed up the execution, multiprocessing is used, i.e. multiple processor cores run the code. This however might introduce slight numerical deviations between every training execution, even after setting seeds. The correlations of quality scores between executions are on the order of 0.98.

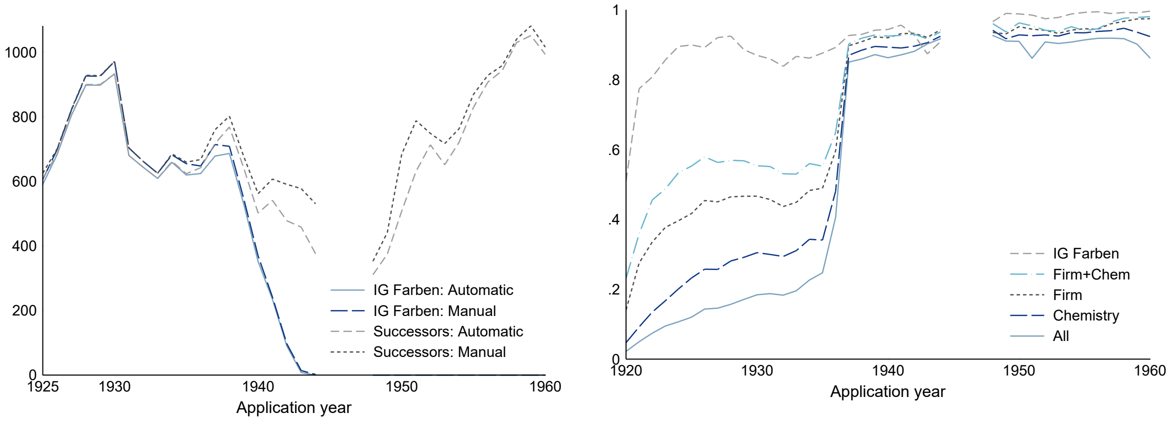
Quality of a patent Q_i is defined as the ratio between the forward similarity FS_i and backward similarity BS_i towards other patents in the same technology class. Forward similarity is seen as a measure of how influential a particular patent was, how much its language is taken up by subsequent patents. Backward similarity in contrast is seen as a measure of derivativeness, how much a patent took up language from previous patents.

$$\begin{aligned} FS_i &= \frac{1}{N(F_i)} \sum_{F_i} S_{ij} \\ F_i &= \{j : t(j) = t(i) + \tau \wedge tc(i) = tc(j)\},\ \tau \in \{1..5\} \end{aligned} \tag{D.2}$$

Figure D.7: Patent processing descriptives

(a) Patent matching algorithm: IG Farben patents based on automatic and manual processing.

(b) Share patents with inventor information



Notes: (D.7a) plots the patent counts of IG Farben and successor companies BASF, Bayer, Hoechst, Huels, Cassella and Agfa. Automatic refers to the processing pipeline above, manual to manual classification of company names based on the DPMA base data. (D.7b) plots the share of patents with inventor information, by groups. Before 1937, listing inventors was optional and was more likely done by large firms such as the IG Farben (top line). Matched firms in chemistry (second line) and matched firms overall (third line) list inventors with decreasing frequency. Patents in chemistry (fourth line) and patents overall (last line) are least likely to list inventors.

$$BS_i = \frac{1}{N(B_i)} \sum_{B_i} S_{ij}$$
$$B_i = \{j : t(j) = t(i) - \tau \wedge tc(i) = tc(j)\}, \;\; \tau \in \{1..5\} \qquad \text{(D.3)}$$

$$Q_i = \frac{FS_i}{BS_i} \qquad \text{(D.4)}$$

$tc(i)$ is the technology class of patent i, $t(i)$ is the application year of patent i. $N(F_i)$ and $N(B_i)$ indicate the cardinality of F_i and B_i, i.e. number of patents j within five years in the same technology class.

For practical purposes, the The so-obtained quality scores are adjusted and normalized. They are winsorized at the 1st and 99th percentile and are standardized to have mean three and standard deviation one. This ensures that there are no negative values in any quality measure (which would occur with mean one) and that results are easy to interpret. Finally, the number of patents in 1945 is very small. For that reason, 1945 is not considered for quality scores. 1946 and 1947 are disregarded as in all other regressions as the German patent office was closed in these years. This gap is skipped for purposes of calculating the previous or next five years in equations D.2 and D.3. So, for a patent in 1950, the previous five years are 1949, 1948, 1944 and 1943.

These measures are inspired by Kelly et al. (2018) but differ in that instead of the total forward/backward similarity, the average forward/backward similarity are used. As long as the number of patents in the previous and subsequent years are the same, there is little difference. However, the number of patent applications at the German patent office changes considerably across years, as Figure D.11 shows. Therefore, not normalizing by the amount of patent applications in consideration would incorporate future and past changes in patent numbers into current quality measures, which is not desirable for event study estimates. Since this measure is calculated within technology classes (also different to Kelly et al.), the past and future development of the size of technology classes would directly enter the quality calculation - but this is itself the base outcome measure on top of which the quality scores are applied. On the other hand, to some extent these concerns apply also to forward citation counts. These are necessarily correlated with the number of future patent applications in the close technology space. Hence, text-based quality measure calculated based on total future similarities likely correlate better with forward citation counts, a key validation target in the study of Kelly et al.

Kelly et al. (2018) account for dynamically changing terminology by adjusting their measure of similarity. Their TF-IDF measures that are separately calculated for each time period, intended to reflect the updated corpus of words. While this adjustment offers an important methodological advantage, it also vastly increases computational complexity. Next to calculating a separate text model for each year, this approach is not easily integrated into the otherwise advantageous Doc2Vec methodology.

A middle ground approach is to calculate the text model based on patents well before the policy change and to extrapolate it to the remaining time period. In a robustness check, only patents between 1920 and 1940 train the Doc2Vec model. This model is then extrapolated to 1941-1965 patents. With this, new words in patent texts after the policy change around 1952 do not influence the underlying similarity scores. As it turns out, regressions based on this alternative approach yield qualitatively very similar results, although the correlation between the quality scores yielded by the different approaches is only around 0.52 (0.66 for pre-war patents). Figure D.13 compares estimates based on the two types of quality scores. Quality scores take only patent grant documents into account, as the availability of application documents after the Second World War would artificially inflate quality scores.

Validating Quality Scores with Lists of Notable Patents The external validity of the quality scores can only be tested with additional data. A separate publication series compiles notable patents in inorganic chemistry from 1877 until roughly 1935 (Bräuer and D'Ans, 1921; Bräuer and D'Ans, 1925; Bräuer and D'Ans, 1930; Bräuer and D'Ans, 1934; Bräuer and D'Ans, 1940). Industry experts first list and then reprint the 4265 patents most relevant to industrial users. As a first test, 97.9 % of the listed patents are covered technology classes in 'Chemistry', as defined above. On the flip side, inorganic chemistry is only a subset of chemistry, but still 50.4 % of 'Chemistry' technology classes contain patents in organic chemistry. For a test of the correlation between quality scores and highlighted patents, only technology class-year pairs with at least ten patents in inorganic chemistry between 1924 and 1935 are considered. After this restriction, 2737 inorganic chemistry patents remain.[2] Table D.3 lists regression results and finds positive and statistically significant semi-elasticities between highlighted patents and their estimated quality. The correct control group would be other patents in inorganic chemistry, but this remains for future research.

D.1.3 Reassigning IG Farben Patents

During the time period in question, journeys to work are typically short. Historical evidence is compiled by Pooley and Turnbull (1999), who collect journey-to-work histories for 1813 British individuals, totaling more than 12,000 individual journeys. In Table 4 therein, they list for the 1920-1939 time period an average workplace distance of 11.1 km (London), 5.6 km (other cities with >100,000 population) and 4.4

[2] 2737 highlighted patents remain after restricting to the 1924-1935 time period. The further restriction is useful as a strong positive correlation should only be expected for technology classes where inorganic chemistry actually plays an important role. Also, some of the selections are due to the digitization of the lists being still in progress.

Table D.3: Validating quality scores

log(Quality)	(1) Doc2Vec for all	(2) Doc2Vec for $t \leq 40$
Featured patent	0.021^{**}	0.011^{*}
	(0.009)	(0.006)
Tech-Year FE	Yes	Yes
Adj. R-Square	0.090	0.131
Observations	18905	18905

Notes: In columns 1 and 2, quality is based on all patents. In columns 3 and 4, quality is based on patents in 1940 and before. Featured patent is a dummy variable for being featured in a publication series listing significant advances in inorganic chemistry. The sample consists of all patents between 1924 and 1935 with at least ten patents featured in the inorganic chemistry list.

km (Towns < 100,000 population). The overall average is 6.8 km. (Pooley and Turnbull, 1999, p. 287) In the (not tabulated) variance around these estimates, inventors are likely on the upper end. Because of this, the upper boundary for reassignment of 30km is chosen. In this light, the travel distances reported in Table D.4 are reasonable. They are slightly smaller, which is due to the coarse measurement of inventor locations (which are available at the city or, for larger cities, city-quarter level).

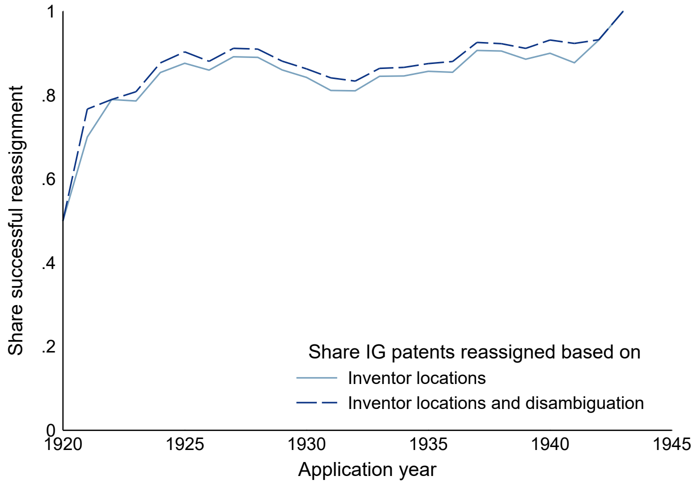
Table D.4: Distance between geocoded inventor and IG plant locations

	Mean distance	Std. Dev.	Min	Max	Total Patents
Agfa	4.12	5.95	0.07	27.32	286.00
BASF	2.58	5.58	0.02	27.81	3366.00
Bayer	1.92	3.12	0.06	24.83	2134.00
Cassella	1.46	0.89	0.01	7.78	317.00
Hoechst	3.03	4.68	0.05	26.34	2481.00
Huels	11.78	10.39	0.03	29.81	36.00
East Germany	8.74	9.28	0.02	28.75	397.00
Overall	2.87	5.24	0.01	29.81	9017.00

Notes: The minimum distance is often zero as inventor and plant locations are coarse and only available at the city-quarter (for large cities) or town level. East Germany subsumes several locations such as Leuna, Schkopau or Premnitz. See also map D.10.

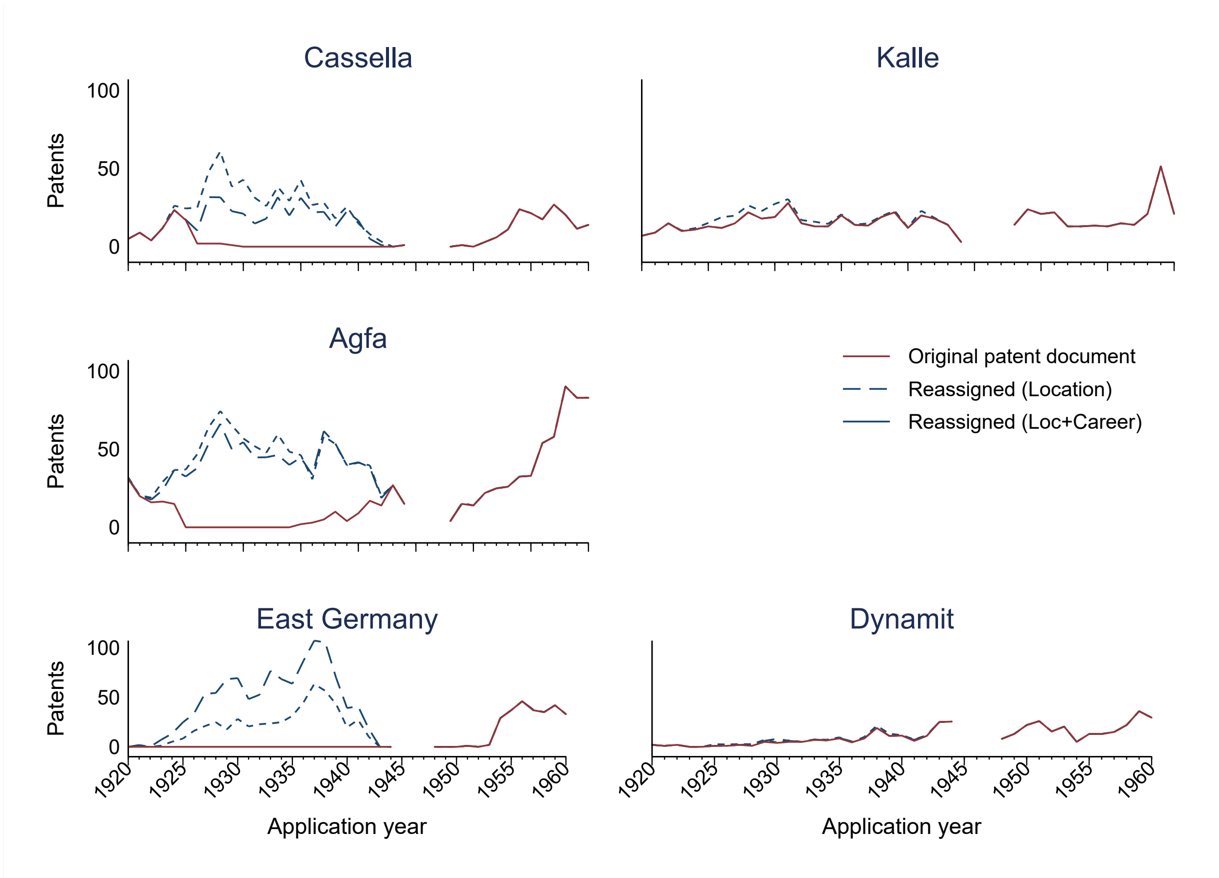
The only subsidiaries where the geographical assignment is challenged are Bayer/Agfa and Cassella/Hoechst. For Bayer/Agfa, Agfa's Leverkusen plant cannot be distinguished from Bayer's Leverkusen plant, in fact they are at the exact same physical location. Therefore, Agfa's Leverkusen operation is subsumed under Bayer's label. Cassella is located in Frankfurt-Mainkur, a suburb of Frankfurt (Main). Hence Hoechst, located in several other parts of Frankfurt (Main), cannot fully be distinguished from Cassella. As far as possible, the deduplication of inventor profiles is used to rectify both problems. Inventors whose patents are subsequently assigned to Agfa or Cassella are also previously assigned to these companies. Map D.10 visualizes the issue.

Figure D.8: Success rate of IG Farben patent reassignment



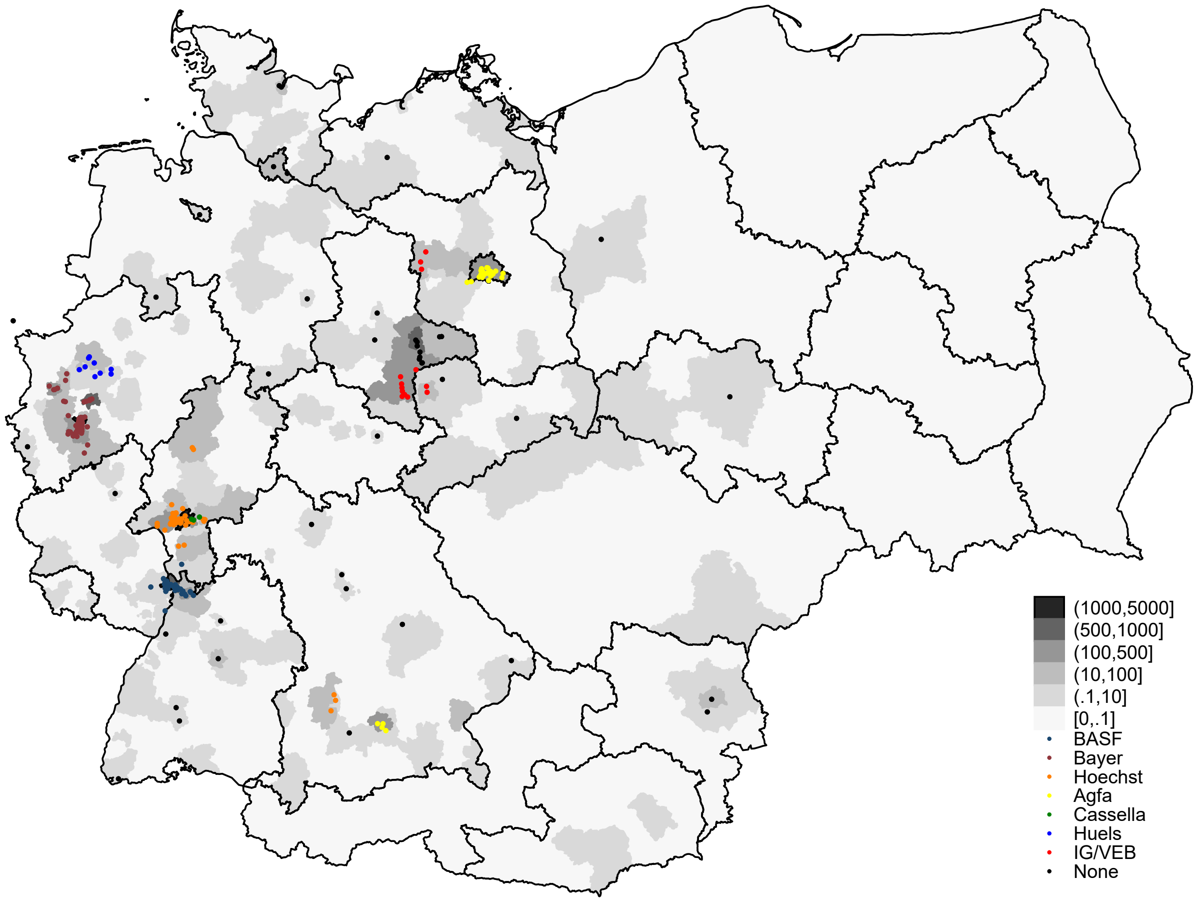
Notes: Share of IG Farben patents that could be reassigned to a successor company. Remaining patents typically have no inventor information. In some cases, inventor locations is not at any successor plant or the inventor could not observed before or after IG's existence.

Figure D.9: Patents of successor companies, assigned by inventor locations (smaller successors)



Notes: The core IG Farben company applied all patents from the Frankfurt headquarter. However, unlike most companies at the time, almost all patents list the inventors. Due to the geographic spread of IG Farben's research facilities, inventor locations allow the reassignment to eventual successors. Only in some cases, the inventor careers from deduplicated patent applications are more informative. Here, inventors are reassigned to their post-war place of employment. The graph shows the yearly number of granted patent applications for the three large successor companies and the newly independent Huels. Numbers are as listed on the original patent documents (red solid line), as reassigned to eventual successors using location information (blue dash line) and as reassigned to eventual successors using location information and inventor name disambiguation (solid blue line). For BASF, Bayer, Hoechst and Huels see Figure 4.3.

Figure D.10: Map: Inventor reassignment locations



Notes: Shows the location of inventors (with number of patents above a threshold) and the successor company that they are assigned to in the location-based reassignment. The background maps shows modern European regional boundaries of Germany, Austria, Poland and Czech Republic, colored with the number of IG Farben patents assigned to NUTS3 regions. Maximum intensity regions are typically not visible below the reassignment location markers. Map source: European Commission.

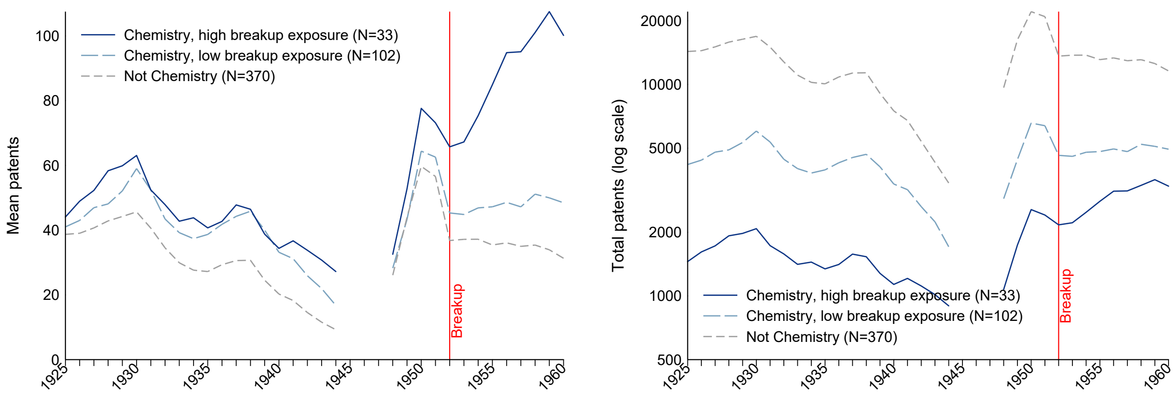
D.2 Supplementary Results

D.2.1 Innovation in Technology Classes

Figure D.11: Patent counts

(a) Mean granted patents

(b) Total granted patents (log)



Notes: Shows the average by technology class (D.11a) and the total amount of granted patents (D.11b) across groups of patents. The first two groups are patents in chemistry, further divided by high and low breakup exposure (defined as 75th percentile of ΔHHI of 1925-1939 patents). The third group is patents outside of chemistry. The patent office was closed for most of 1945, 1946 and 1947, hence no data is available. A graph for quality-weighted patents is part of Figure 4.7 in the main text.

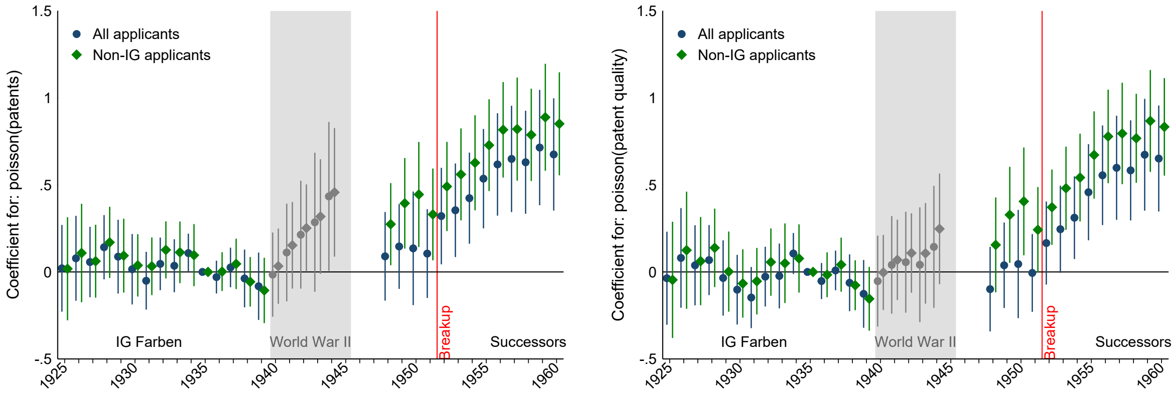
Selected technology classes	1925-1939					1948-1952
	Patents	IG %	$CR4^{IG}$	$CR4^{\overline{IG}}$	ΔCR4	ΔCR4
8M: Coloring	643	0.56	0.72	0.52	0.20	0.10
12G: Processes (general)	398	0.26	0.32	0.24	0.09	0.06
12K: Ammonium, Cyanides	484	0.16	0.28	0.22	0.06	0.10
22E: Indigo-based dyes	377	0.77	0.88	0.67	0.22	0.26
29B: Chemical fibers	601	0.28	0.37	0.25	0.12	0.06
30H: Drug development	1048	0.15	0.21	0.15	0.06	0.03
39C: Synthetic plastics	326	0.51	0.61	0.51	0.10	0.15
45L: Pesticides	699	0.31	0.44	0.33	0.11	0.10
Means for ΔHHI > p75 (N=33)	730	0.37	0.50	0.38	0.12	0.09
Means for ΔHHI ≤ p75 (N=102)	673	0.04	0.24	0.22	0.01	0.01

Notes: Statistics by technology class, means across classes with high and low breakup exposure in the last two rows.

Table D.5: Δ CR4 implied by the IG dissolution

Figure D.12: Event studies: Poisson estimates

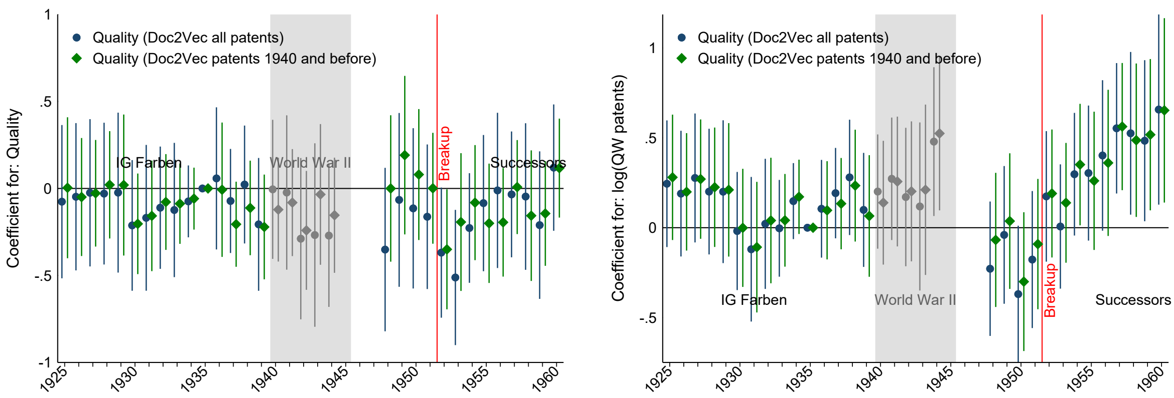
(a) Patent count (b) Quality-weighted patents



Notes: Technology-year panel Poisson regression with 95% confidence intervals. Regressions comparing technology classes with high and low exposure to the IG Farben breakup, as defined by the 75th percentile of ΔHHI (160). Exposure is measured using pre-war (1925-1939) data, but the breakup is finalized and effective around 1952. Shows quality-weighted counts of granted patents, with average patent quality winsorized and rescaled to have average three and standard deviation one to exclude negative values. The German patent office closed in 1945-1947. Wartime patent applications are largely prosecuted post-war. The coefficients are set in gray to indicate possible bias.

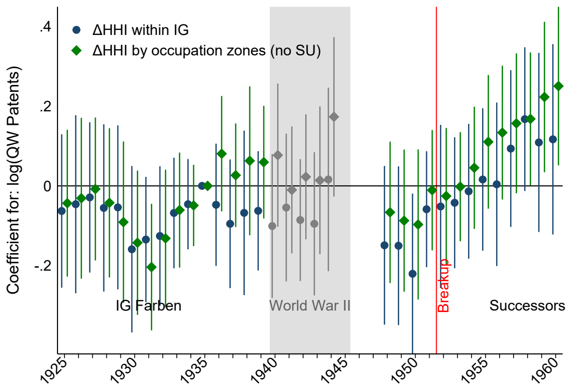
Figure D.13: Event studies: Alternative calculation of quality scores

(a) Average quality (b) Quality-weighted patents



Notes: Technology-year panel regression with 95% confidence intervals. Regressions comparing technology classes with high and low exposure to the IG Farben breakup, as defined by the 75th percentile of ΔHHI (160). Exposure is measured using pre-war (1925-1939) data, but the breakup is finalized and effective around 1952. Round estimate markers rely on quality-scores where the Doc2Vec model was trained on the full corpus of chemical patents. Diamond estimate markers rely on a Doc2Vec model trained only with patents until 1940 and extrapolated for later years. Patent quality is winsorized and rescaled within technology classes to have average three and standard deviation one to exclude negative values. D.13a shows average yearly quality within technology classes as dependent variable. D.13b shows quality-weighted counts of granted patents. The German patent office closed in 1945-1947. Wartime patent applications are largely prosecuted post-war. The coefficients are set in gray to indicate possible bias.

Figure D.14: Event studies: Alternative calculation of ΔHHI



Notes: Technology-year panel regression with 95% confidence intervals. Dependent variable are quality-weighted non-IG patents. Continuous exposure measures are interacted with year indicators. Exposure is measured using pre-war (1925-1939) data, but the breakup is finalized and effective around 1952. The explanatory variables as explained in section 4.5.1 are standardized to mean zero an standard deviation one. The German patent office closed in 1945-1947. Wartime patent applications are largely prosecuted post-war. The coefficients are set in gray to indicate possible bias.

Table D.6: Effects in Technology class-level DiD regression (Poisson)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Exposure: ΔHHI 1925-1939						1930-1939	1925-1935	1948-1952
Poisson(Patents)	All (Quality)	Non-IG (Quality)	All (Count)	Non-IG (Count)	Domestic (Quality)	Foreign (Quality)	All (Quality)	All (Quality)	All (Quality)
48-51× High ΔHHI	0.020	0.280**	0.066	0.294**	–0.405***	0.715***	0.035	0.035	0.074
	(0.103)	(0.124)	(0.110)	(0.129)	(0.095)	(0.152)	(0.100)	(0.101)	(0.097)
52-60× High ΔHHI	0.505***	0.673***	0.505***	0.661***	0.288***	0.794***	0.499***	0.532***	0.528***
	(0.123)	(0.139)	(0.129)	(0.144)	(0.107)	(0.180)	(0.123)	(0.120)	(0.119)
{52-60}-{48-51}	0.485***	0.393***	0.439***	0.367***	0.694***	0.079	0.464***	0.497***	0.453***
	(0.093)	(0.096)	(0.075)	(0.081)	(0.100)	(0.098)	(0.095)	(0.092)	(0.095)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Classes	135	135	135	135	135	135	133	135	134
Dep. var. mean	149.470	131.332	46.971	41.277	101.073	40.096	150.267	149.470	149.776
Pseudo R-Square	0.915	0.914	0.896	0.894	0.904	0.864	0.914	0.915	0.915
Observations	4257	4257	4455	4455	4257	4257	4234	4257	4248

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ Standard errors clustered on the technology class level in parentheses. ΔHHI is the difference between technology-level concentration, considering IG Farben as one block or as broken up according to the 1952 successors. High ΔHHI refers to the concentration change in the top 25% of the distribution, threshold 160. The DiD coefficients in turn compare patent counts in 1948-1951 and 1952-1960 with the pre-war period. The main effect is the difference between these two coefficients, tabulated in row {52-60}-{48-51}. The dependent variables are quality-weighted patent counts, except columns (3) and (4) with simple patent counts. Quality weights are normalized to mean three, standard deviation one. The columns restrict patents by applicants, either all (columns 1, 3) or applicants unconnected to IG Farben (columns 2, 4, 7-9). Columns 5-6 restrict patents by location, where inventor location is preferred if available. Domestic patents refer to patents with a German location, foreign patents to patents with a foreign location.

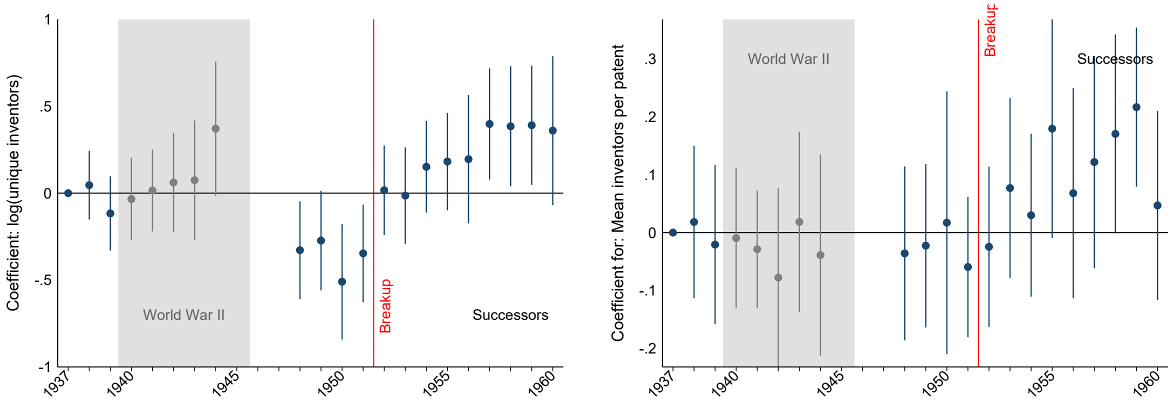
Table D.7: Effects in Technology class-level DiD regression (Robustness)

	(1)	(2)	(3)
	Exposure: ΔHHI 1925-1939		
log(Patents)	Uncontrolled	Controlled	Oster
48-51× High ΔHHI	−0.10	−0.18	−0.21
52-60× High ΔHHI	0.37	0.32	0.31
{52-60}-{48-51}	0.46	0.50	0.52

Notes: Shows coefficients from regression with and without controls as well as resulting Oster (2019) bounds. Dependent variable: quality-weighted non-IG Farben patents. Controls are the share of non-IG firms targeted for dismantling, the share of patents located in East Germany or Berlin as well as war destruction, proxied by the share of destroyed flats in the city of patent inventor or applicant. Control variables are interacted with a full set of year indicators. The DiD coefficients in turn compare patent counts in 1948-1951 and 1952-1960 with the pre-war period. The main effect is the difference between these two coefficients, tabulated in row {52-60}-{48-51}. Bounds are calculated as: $\beta^* = \tilde{\beta} - [\dot{\beta} - \tilde{\beta}]\,\frac{R_{max}-\tilde{R}}{\tilde{R}-\dot{R}}$, where $\dot{\beta}$ and $\dot{R}$ refer the uncontrolled coefficient and R-Squared and $\tilde{\beta}$ and $\tilde{R}$ to the controlled coefficient and R-Squared. R_{max} is set to $1.3 \times \tilde{R}$. The underlying assumption is that reaction of coefficients to observable controls informs about the potential importance of omitted variable bias.

Figure D.15: Regressions based on disambiguated inventors

(a) Number of unique inventors (b) Mean inventors per patent



Notes: OLS regressions comparing technology classes with high and low exposure to the IG Farben breakup, as defined by the 75th percentile of ΔHHI (160). Exposure is measured using pre-war (1925-1939) data, but the breakup is finalized and effective around 1952. Shows 95% confidence intervals. The German patent office closed in 1945-1947. Wartime patent applications are largely prosecuted post-war. The coefficients are set in gray to indicate possible bias. Before 1937, inventor information on German patents is only available for large companies such as IG Farben. See Appendix D.1.2.

Table D.8: Effects in Technology class-level DiD regression (Robustness)

	(1)	(2)	(3)	(4)	(5)	(6)
	Exposure: ΔHHI 1925-1939					
log(Patents)	Default	Excl Plastics	Control Dismantle	Control East	Control Destr	Control All
48-51× High ΔHHI	–0.097	–0.167	–0.104	-0.201^{*}	–0.098	–0.185
	(0.117)	(0.108)	(0.120)	(0.121)	(0.119)	(0.121)
52-60× High ΔHHI	0.366^{***}	0.291^{**}	0.370^{***}	0.352^{**}	0.364^{***}	0.322^{**}
	(0.137)	(0.124)	(0.135)	(0.144)	(0.137)	(0.142)
48-51 × Dismantle (%)			-1.448^{**}			–0.858
			(0.710)			(0.873)
52-60 × Dismantle (%)			1.135^{*}			1.489^{*}
			(0.664)			(0.879)
48-51 × East/Berlin (%)				-1.362^{***}		-1.194^{*}
				(0.506)		(0.683)
52-60 × East/Berlin (%)				–0.192		–0.669
				(0.535)		(0.800)
48-51 × Destruction (%)					0.136	–0.722
					(1.112)	(1.134)
52-60 × Destruction (%)					0.383	–0.157
					(1.101)	(1.286)
{52-60}-{48-51}	0.463^{***}	0.458^{***}	0.474^{***}	0.553^{***}	0.461^{***}	0.507^{***}
	(0.113)	(0.117)	(0.109)	(0.125)	(0.116)	(0.120)
Tech FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Classes	135	132	135	135	134	134
Dep. var. mean	3.913	3.878	3.913	3.913	3.917	3.917
Adj. R-Square	0.788	0.792	0.790	0.789	0.787	0.789
Observations	4192	4097	4192	4192	4187	4187

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ Standard errors clustered on the technology class level in parentheses. Dependent variable: quality-weighted non-IG Farben patents. Column 2 excludes technology classes 39A, 39B and 39C, referring to chemical synthesis plastics and handling of plastics. Column 3 controls for the share of non-IG firms targeted for dismantling. The inclusion of IG Farben in this measure would control directly for the IG Farben share, mechanically highly correlated to the breakup indicator. The more appropriate test for effects of dismantlement is a firm-level regression as described in section 4.8. Column 4 controls for the share of patents located in East Germany or Berlin. Column 5 controls for war destruction, proxied by the share of destroyed flats in the city of patent inventor or applicant. The number of observations differs as for small technology classes, text similarities and quality scores cannot be calculated. The DiD coefficients in turn compare patent counts in 1948-1951 and 1952-1960 with the pre-war period. The main effect is the difference between these two coefficients, tabulated in row {52-60}-{48-51}.

D.2.2 Price Analysis

Tables D.9 and D.10 shows means comparisons between the different groups of IG Farben exposure. As expected, the groups are unbalanced. Overall, the balancing test conveys the impression that IG Farben products are more likely within basic chemicals, whereas other chemical companies in Germany tended to specialize in more niche or specialty chemicals. However, a substantial overlap exists. This impression relies on the main observations that prices per kg for IG products were lower, their chemical weight smaller and the number of competitors larger compared to non-IG products. While IG prices are on average lower, it is important to note that absolute prices crucially depend on the nature of a product. Some products, for example radioactive luminescent colors, are only used and sold in small amounts and thus have exceedingly high per-kg prices. Winsorization is used to contain the effect of such outliers in the balancing tables whereas in regressions, product fixed effects suffice. Molar mass is the weight of a substance sample divided by the number of contained molecules, measured in weight per mole (mol, a standard unit for the number of particules). For the purposes of this analysis, it is only relevant that the molar mass rises with the number and size of atoms contained in a compound. Molecules are heavier if they are more complex (e.g. large organic compounds such as Chlorophyll) or if they contain with heavy atoms (e.g. lead salts). To capture the latter explanation, the atom with the largest atomic mass is identified from the chemical formula. The molar mass reduced by all occurrences of this atom is listed as 'remaining mass'. Overall, substances sold by IG Farben are lighter and with lighter heaviest atom. Note that this is unlikely to be driven by a distinction in organic/inorganic chemistry, where share differences are not large enough to explain the difference. In terms of tariffs, pre-war tariffs on IG products were higher than on non-IG products, but the post-war difference is negligible. This might speak to IG Farben's political influence, which was reduced after the war. Finally, products with IG Farben involvement had consistently more suppliers than non-IG products.

Table D.9: Descriptive statistics for IG/non-IG product portfolios: 1952

	Comparing 1952: IG Farben - No IG Farben				
N=518	IG	No IG	Difference	(SE)	p-value
Price (per kg, log)	5.38	6.33	0.95	(0.15)	0.000***
Quality grades	1.73	1.42	–0.31	(0.07)	0.000***
Has Wikipedia data (%)	0.94	0.95	0.01	(0.02)	0.482
Molar mass (g/mol)	122.02	193.63	71.61	(10.47)	0.000***
- Heaviest element	32.11	47.98	15.88	(3.66)	0.000***
- Remaining mass	74.89	116.50	41.61	(9.91)	0.000***
Anorganic	0.41	0.45	0.04	(0.04)	0.417
Organic	0.36	0.44	0.08	(0.04)	0.061*
Pharma	0.14	0.09	–0.06	(0.03)	0.046**
Plastics	0.07	0.00	–0.07	(0.02)	0.000***
Metal	0.02	0.02	0.01	(0.01)	0.650
Has tariff data (%)	0.86	0.86	0.00	(0.03)	0.877
Pre-war tariff (%)	0.13	0.06	–0.07	(0.03)	0.037**
Post-1951 tariff (%)	0.15	0.15	–0.01	(0.01)	0.274
Tariff difference (%-%)	0.03	0.10	0.06	(0.03)	0.026**
Suppliers (1939)	6.34	4.05	–2.29	(0.43)	0.000***
- Non-IG (1939)	5.00	3.78	–1.22	(0.43)	0.004***
- Cartel (1939, %)	0.15	0.11	–0.04	(0.03)	0.261
- Non-IG (1952)	3.56	2.79	–0.77	(0.25)	0.002***

Notes: Shows difference between the different groups used in the comparisons. Appendix D.2.2 explains the data in details. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table D.10: Descriptive statistics for IG/non-IG product portfolios: 1939

	Comparing 1939: IG Farben - No IG Farben				
N=466	IG	No IG	Difference	(SE)	p-value
Price (per kg, log)	5.44	6.10	0.65	(0.16)	0.000***
Has Wikipedia data (%)	0.95	0.95	−0.01	(0.02)	0.698
Molar mass (g/mol)	133.71	184.62	50.92	(11.15)	0.000***
- Heaviest element	32.38	51.54	19.15	(3.95)	0.000***
Anorganic	0.42	0.48	0.06	(0.05)	0.185
Organic	0.41	0.35	−0.06	(0.04)	0.180
Pharma	0.14	0.13	−0.02	(0.03)	0.566
Plastics	0.03	0.01	−0.02	(0.01)	0.116
Metal	0.00	0.04	0.04	(0.01)	0.010***
Has tariff data (%)	0.87	0.86	−0.02	(0.03)	0.611
Pre-war tariff (%)	0.12	0.04	−0.08	(0.03)	0.002***
Post-1951 tariff (%)	0.16	0.14	−0.01	(0.01)	0.098*
Tariff difference (%-%)	0.04	0.11	0.07	(0.02)	0.002***
Suppliers (1939)	6.30	4.02	−2.28	(0.42)	0.000***
- Non-IG (1939)	4.64	4.02	−0.62	(0.42)	0.140
- Cartel (1939, %)	0.17	0.09	−0.08	(0.03)	0.008***
- Non-IG (1952)	3.47	3.37	−0.10	(0.28)	0.715
IG (1952): Any	0.79	0.23	−0.57	(0.04)	0.000***
IG (1952): 1	0.36	0.17	−0.19	(0.04)	0.000***
IG (1952): 2+	0.43	0.06	−0.38	(0.04)	0.000***

Notes: Shows difference between the different groups used in the comparisons. Appendix D.2.2 explains the data in details. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table D.11: DiD estimates for price effects, considering cartels

log(price)	(1) Base	(2) Post	(3) Dynamic	(4) Base	(5) Post	(6) Dynamic
Post × $IG_{1939} = 1$	–0.026	–0.024	–0.024			
	(0.025)	(0.025)	(0.025)			
Post × $IG_{1952} = 1$				0.068^{**}	0.068^{**}	0.068^{**}
				(0.028)	(0.028)	(0.028)
Post × $IG_{1952} \geq 2$				-0.050^{**}	-0.049^{**}	-0.049^{**}
				(0.024)	(0.024)	(0.024)
Post × Cartel 1940		–0.042			–0.031	
		(0.031)			(0.029)	
Product, Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Type × Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Cartel × Month FE			Yes			Yes
N Time series	464	464	464	516	516	516
N Chemicals	363	363	363	400	400	400
Within R-Square	0.001	0.002	0.003	0.009	0.009	0.010
Observations	7953	7953	7953	8854	8854	8854

Notes: Shows difference in difference estimates for a assumed event time in 1950Q3. Columns 1-3 show effects based on the 1939 structure if IG Farben, columns 4-6 based on the 1952 structure of the IG Farben successors. The baseline is always the group of products with no IG Farben involvement. Products with involvement of at least one sales cartel in 1939 are considered as cartelized "Cartel". When information about quality grades (e.g. 'pure') is available, multiple time series per product can exist. Standard errors clustered on the product level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table D.12: DiD estimates for price effects, chemical types

log(price)	(1) All	(2) All	(3) No Plastics	(4) All	(5) All	(6) No Plastics
Post × $IG_{1939} = 1$	–0.026	–0.036	–0.021			
	(0.025)	(0.025)	(0.025)			
Post × $IG_{1952} = 1$				0.068^{**}	0.070^{**}	0.076^{***}
				(0.028)	(0.029)	(0.028)
Post × $IG_{1952} \geq 2$				-0.050^{**}	-0.046^{**}	-0.046^{*}
				(0.024)	(0.023)	(0.024)
Product, Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Type × Month FE	Yes	No	Yes	Yes	No	Yes
N Time series	464	464	456	516	516	498
N Chemicals	363	363	355	400	400	384
Within R-Square	0.001	0.002	0.001	0.009	0.009	0.009
Observations	7953	7970	7783	8854	8869	8450

Notes: Shows difference in difference estimates for a assumed event time in 1950Q3. Columns 1-3 show effects based on the 1939 structure if IG Farben, columns 4-6 based on the 1952 structure of the IG Farben successors. The baseline is always the group of products with no IG Farben involvement. In columns 3 and 6, products from the group of plastics and adhesives is omitted. When information about quality grades (e.g. 'pure') is available, multiple time series per product can exist. Standard errors clustered on the product level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table D.13: DiD estimates for price effects, considering tariffs

log(price)	(1) Base	(2) Post	(3) Dynamic	(4) Base	(5) Post	(6) Dynamic
Post × $IG_{1939} = 1$	–0.026	–0.042*	–0.043*			
	(0.025)	(0.025)	(0.025)			
Post × $IG_{1952} = 1$				0.068**	0.066**	0.066**
				(0.028)	(0.031)	(0.031)
Post × $IG_{1952} \geq 2$				–0.050**	–0.058**	–0.058**
				(0.024)	(0.024)	(0.025)
Post × Δ Tariff		–0.001			0.009	
		(0.041)			(0.032)	
Product, Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Type × Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Δ Tariff × Month FE			Yes			Yes
N Time series	464	401	401	516	443	443
N Chemicals	363	308	308	400	336	336
Within R-Square	0.001	0.002	0.004	0.009	0.010	0.011
Observations	7953	6878	6878	8854	7593	7593

Notes: Shows difference in difference estimates for a assumed event time in 1950Q3. Columns 1-3 show effects based on the 1939 structure if IG Farben, columns 4-6 based on the 1952 structure of the IG Farben successors. The baseline is always the group of products with no IG Farben involvement. Changes between the previous special tariff and the subsequent ad valorem tariff after the 1951 tariff adjustment are the Δ Tariff control variable. Both tariffs are calculated as percentages and Δ Tariff is the difference. The difference is winsorized at the 1% and 99% level. When information about quality grades (e.g. 'pure') is available, multiple time series per product can exist. Standard errors clustered on the product level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table D.14: Matching+DiD estimates for price effects

log(price)	(1) IG 1952	(2) IG 1952	(3) IG 1952	(4) IG 1952	(5) IG 1952	(6) IG 1952
Post × Treated	-0.099^{***}	-0.101^{**}	-0.134^{**}	-0.156^{**}	-0.093^{**}	0.057
	(0.032)	(0.044)	(0.057)	(0.061)	(0.040)	(0.036)
Post × Δ Tariff				0.016	−0.013	−0.062
				(0.042)	(0.036)	(0.095)
Post × Cartel 1940				0.003	−0.014	0.021
				(0.076)	(0.051)	(0.046)
Treatment group	IG ≥ 2	IG ≥ 2	IG ≥ 2	IG ≥ 2	IG ≥ 2	IG = 1
Control group	IG = 0	IG = 0	IG = 0	IG = 0	IG ≤ 1	IG = 0
Product, Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Pre-treatment price	Yes	Yes	Yes	Yes	Yes	Yes
Chemical properties		Yes	Yes	Yes	Yes	Yes
Competitor count			Yes	Yes	Yes	Yes
N cluster	361	266	259	227	339	332
Adj. R-Square	0.984	0.985	0.986	0.987	0.985	0.985
Observations	6404	4704	4556	4004	5906	5550

Notes: Shows difference in difference estimates for a assumed event time in 1950Q3. Probit model for propensity scores in turn adds includes price (squared), chemical type interacted with log molar mass and maximum atomic mass in the compound as well as indicators for the room temperature state of matter. Finally, the coarsened number of competitors in 1952 is added (0-1, 2-3, 4+). Columns 1-5 use products with two or more IG Farben successors as breakup-exposed group. Column 6 considers products with one IG Farben successor as exposed. Columns 1-4 and 6 consider products without 1952 IG involvement as control. Column 5 includes both products with zero or one IG Farben successor in the control group. When information about quality grades (e.g. 'pure') is available, multiple time series per product can exist. Standard errors clustered on the product level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

D.2.3 Supplier Analysis

Table D.15: IG Farben successor product portfolio overlap

Company	Products		Company	Products		Overlap 1952		Overlap 1961		Δ
	1952	1961		1952	1961	N	%	N	%	
Bayer	747	790	Hoechst	682	830	244	0.36	213	0.27	-0.09
BASF	429	489	Bayer	747	790	163	0.38	180	0.37	-0.01
BASF	429	489	Hoechst	682	830	189	0.44	157	0.32	-0.12
Cassella	117	39	BASF	429	489	54	0.46	12	0.31	-0.15
Cassella	117	39	Bayer	747	790	60	0.51	16	0.41	-0.10
Cassella	117	39	Hoechst	682	830	76	0.65	16	0.41	-0.24
Cassella	117	39	Huels	117	161	13	0.11	4	0.10	-0.01
Huels	117	161	BASF	429	489	59	0.50	65	0.40	-0.10
Huels	117	161	Bayer	747	790	46	0.39	30	0.19	-0.21
Huels	117	161	Hoechst	682	830	87	0.74	46	0.29	-0.46
Wacker	57	97	BASF	429	489	26	0.46	18	0.19	-0.27
Wacker	57	97	Bayer	747	790	22	0.39	40	0.41	+0.03
Wacker	57	97	Cassella	117	39	9	0.16	1	0.03	-0.13
Wacker	57	97	Hoechst	682	830	34	0.60	24	0.25	-0.35
Wacker	57	97	Huels	117	161	18	0.32	13	0.13	-0.18
Dynamit	44	80	BASF	429	489	15	0.34	25	0.31	-0.03
Dynamit	44	80	Bayer	747	790	14	0.32	27	0.34	+0.02
Dynamit	44	80	Cassella	117	39	6	0.14	3	0.08	-0.06
Dynamit	44	80	Hoechst	682	830	10	0.23	18	0.22	-0.00
Dynamit	44	80	Huels	117	161	5	0.11	25	0.31	+0.20
Dynamit	44	80	Wacker	57	97	4	0.09	11	0.14	+0.05
Kalle	24	18	BASF	429	489	11	0.46	4	0.22	-0.24
Kalle	24	18	Bayer	747	790	10	0.42	3	0.17	-0.25
Kalle	24	18	Cassella	117	39	7	0.29	2	0.11	-0.18
Kalle	24	18	Dynamit	44	80	1	0.04	1	0.06	+0.01
Kalle	24	18	Hoechst	682	830	15	0.63	6	0.33	-0.29
Kalle	24	18	Huels	117	161	5	0.21	1	0.06	-0.15
Kalle	24	18	Wacker	57	97	3	0.13	1	0.06	-0.07

Notes: Bilateral overlap between product portfolios of IG Farben successors in 1952 and 1961. Looks at all products (repeated cross-sections), excluding brands. Overlap is calculated as share of the smaller portfolio.

Table D.16: Number of suppliers by product, as a result of IG Farben exposure (control coefficents)

	Number of firms				Number of non-IG firms			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$IG_{1939} \geq 1 \times 1952$	−0.531*	0.364			−0.436	0.403		
	(0.295)	(0.320)			(0.277)	(0.282)		
$IG_{1939} \geq 1 \times 1961$	1.166*	1.701**			1.245**	1.699***		
	(0.603)	(0.667)			(0.564)	(0.629)		
$IG_{1952} = 1 \times 1952$			0.369	0.704***			−0.115	0.165
			(0.297)	(0.251)			(0.288)	(0.248)
$IG_{1952} = 1 \times 1961$			1.141*	1.167**			0.885	0.860*
			(0.597)	(0.518)			(0.567)	(0.499)
$IG_{1952} \geq 2 \times 1952$			1.403***	2.845***			−0.469	0.867**
			(0.509)	(0.423)			(0.485)	(0.369)
$IG_{1952} \geq 2 \times 1961$			5.950***	5.629***			4.678***	4.302***
			(1.072)	(1.057)			(1.030)	(1.026)
$East_{1939} \times 1952$		−0.817***		−0.903***		−1.014***		−1.050***
		(0.122)		(0.109)		(0.097)		(0.094)
$East_{1939} \times 1961$		−0.015		−0.215		−0.139		−0.305
		(0.211)		(0.198)		(0.193)		(0.187)
$Cartel_{1939} \times 1952$		0.097		−0.183		0.126		0.107
		(0.646)		(0.597)		(0.577)		(0.564)
$Cartel_{1939} \times 1961$		6.527***		6.196***		6.263***		6.100***
		(1.287)		(1.316)		(1.231)		(1.272)
$Dism._{1939} \times 1952$		−0.453**		−0.591***		−0.281		−0.230
		(0.206)		(0.161)		(0.184)		(0.149)
$Dism._{1939} \times 1961$		−1.236***		−1.197***		−1.044***		−0.889***
		(0.414)		(0.331)		(0.393)		(0.320)
Product, Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Type × Year FE		Yes		Yes		Yes		Yes
Products	566	566	566	566	566	566	566	566
Adj. R^2	0.528	0.618	0.560	0.640	0.505	0.607	0.534	0.622
Observations	1698	1698	1698	1698	1698	1698	1698	1698

Notes: Considers only products with data from 1939, 1952 and 1961 where at least one price information is available. IG_{1939} is the count of firms associated with IG Farben offering the product in 1939, pre-war and pre-breakup. IG_{1952} is the number of IG Farben successors offering the product in 1952, immediately after the breakup. The number of firms is the number of suppliers of the product according to the product catalog of the respective year, winsorized at the 99% level. In columns 5-8, IG firms or successors are excluded from the count. Control variables include the count of firms headquartered in East Germany or Berlin in 1939, the count of cartels in 1939 and the count of firms slated for dismantlement in 1939, each interacted with year dummies. See also the discussion in section 4.8.

Table D.17: Number of suppliers by product, as a result of IG Farben exposure

	Excluding trading and IG companies				Excluding trading companies			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$IG_{1939} \geq 1 \times 1952$	−0.269	0.479*			−0.351	0.443		
	(0.257)	(0.264)			(0.277)	(0.301)		
$IG_{1939} \geq 1 \times 1961$	−0.475	0.615			−0.565	0.576		
	(0.366)	(0.414)			(0.384)	(0.438)		
$IG_{1952} = 1 \times 1952$			−0.205	0.039			0.271	0.566**
			(0.274)	(0.240)			(0.282)	(0.243)
$IG_{1952} = 1 \times 1961$			−0.205	0.068			0.034	0.355
			(0.378)	(0.318)			(0.402)	(0.327)
$IG_{1952} \geq 2 \times 1952$			−0.270	0.898**			1.594***	2.855***
			(0.453)	(0.367)			(0.479)	(0.416)
$IG_{1952} \geq 2 \times 1961$			0.711	1.867***			1.889***	3.082***
			(0.676)	(0.613)			(0.683)	(0.617)
$East_{1939} \times 1952$		−0.896***		−0.934***		−0.698***		−0.788***
		(0.096)		(0.094)		(0.120)		(0.108)
$East_{1939} \times 1961$		−0.905***		−0.975***		−0.796***		−0.897***
		(0.125)		(0.120)		(0.137)		(0.127)
$Cartel_{1939} \times 1952$		0.249		0.245		0.252		−0.010
		(0.572)		(0.561)		(0.637)		(0.590)
$Cartel_{1939} \times 1961$		2.254***		2.161***		2.545***		2.288***
		(0.828)		(0.830)		(0.879)		(0.868)
$Dism._{1939} \times 1952$		−0.270		−0.196		−0.434**		−0.545***
		(0.180)		(0.149)		(0.200)		(0.161)
$Dism._{1939} \times 1961$		−0.824***		−0.788***		−0.980***		−1.064***
		(0.245)		(0.199)		(0.260)		(0.206)
Product, Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Type × Year FE		Yes		Yes		Yes		Yes
Products	566	566	566	566	566	566	566	566
Adj. R^2	0.450	0.558	0.451	0.564	0.500	0.580	0.508	0.602
Observations	1698	1698	1698	1698	1698	1698	1698	1698

Notes: Considers only products with data from 1939, 1952 and 1961 where at least one price information is available. IG_{1939} is the count of firms associated with IG Farben offering the product in 1939, pre-war and pre-breakup. IG_{1952} is the number of IG Farben successors offering the product in 1952, immediately after the breakup. The number of firms is the number of suppliers of the product according to the product catalog of the respective year, excluding trading firms, winsorized at the 99% level. In columns 5-8, IG firms or successors are excluded from the count. Control variables include the count of firms headquartered in East Germany or Berlin in 1939, the count of cartels in 1939 and the count of firms slated for dismantlement in 1939, each interacted with year dummies. See also the discussion in section 4.8.

D.3 Innovation Analysis in Firm Panel

Robustness analysis can be conducted at varying levels. Some variables directly apply to the product level (cartels, production restrictions) and tests for their relevance can be implemented directly in the respective regressions. Some variables can be collected and aggregated to a technology class level (war destruction, dismantlement, Soviet sector). Such analysis is bound to remain indirect as the shocks affect firms, not technologies. In a firm-level analysis, measurement and control is more direct. In this section, a firm-panel is constructed to offer an additional robustness check for the innovation analysis, leading to comparable results.

Building a Firm Panel The firm panel is constructed by combining various firm data sources. These are supplier lists (see D.1.1), handbooks of listed corporations (Hoppenstedt-Aktienführer, via https://digi.bib.uni-mannheim.de/aktienfuehrer/) and firms slated for dismantlement (Harmssen, 1951) as well as manually collected complementary entries. The firm entries are first matched with each other and the resulting clusters are matched to patent applicants. Appendix D.1.2 discusses details. The subsequent regressions consider patents in classes relevant to the chemical industry. Only firms with patent applications in at least four pre-war (1925-1939) years are part of the panel. With this, the focus is on incumbent firms. However, exposure measures to the IG Farben breakup and other shocks can be calculated with pre-war variables. Overall, more than 350 firms remain in the panel. As the IG Farben successors are special and often outliers in terms of size, they are excluded for the main regressions but included in alternative specifications. The pre-1945 patent count of the eventual successors follows the hypothetical reassignment according to the breakup rules.

Table D.18: Descriptive statistics for IG/non-IG exposed technology classes

	Comparing firms: High vs low breakup exposure				
N=100 (T) 303 (C)	Exposed	Comparison	Difference	(SE)	p-value
Weighted ΔHHI	779.04	61.16	−717.88	(25.08)	0.000***
Quality-weighted patents	162.34	255.25	92.91	(123.92)	0.454
- (log)	4.21	4.09	−0.12	(0.15)	0.445
Foreign (%)	0.19	0.10	−0.09	(0.04)	0.016**
Patents in Soviet sector (%)	0.37	0.35	−0.03	(0.05)	0.577
War destruction (%)	0.27	0.29	0.02	(0.02)	0.425
Any plants dismantled (%)	0.14	0.33	0.19	(0.05)	0.000***
In product list (1939, %)	0.71	0.33	−0.38	(0.05)	0.000***
- product count	41.92	6.82	−35.10	(11.41)	0.002***
- with IG competition (%)	0.28	0.10	−0.18	(0.03)	0.000***

Notes: Shows difference between firms with high and low breakup exposure. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All data refers to patents applied for in 1925-1939. The shock exposure ΔHHI for technology classes is calculated first assuming all IG Farben members to be one entity, then separately according to their post-1952 split-up. A firm's value of shock exposure is weighted according to pre-war patent counts in the respective technology classes. Patents counts are totals. Patents are weighted according to forward text similarity divided by backward text similarity, on patent-level normalized to mean three and standard deviation one. Firm locations follow the predominant patent location, where domestic and foreign patents are identified using inventor locations if available, applicant locations otherwise. Domestic are such located in present-day Germany or Poland, Soviet sector patents all located in present-day East Germany, Berlin or Poland. The inclusion of Poland is a coarse reference to Germany's pre-war territory. War destructions refers to the share of flats destroyed between 1939 and 1945, weighted by the patent locations of a firm. Dismantlement is an indicator for whether the firm occurs in any dismantlement list. The occurrence in a product list and, if so, with how many products is based on the 1939 book. The share of products with IG Farben competition is the pre-war co-occurrence with any IG Farben member or IG Farben-associated cartel.

The firms are grouped by their technology exposure to the IG Farben breakup. For this, the technology class-specific exposure (ΔHHI) is weighted by the pre-war patent portfolio of the firms. Again, firms in the top 25% by exposure are marked as exposed to the breakup, leading to a threshold of 342. In Table D.18, their main characteristics are tabulated and compared with the control firms. They have similar pre-war levels of patenting and are similarly exposed to the Soviet sector as well as to destruction of German cities. They are moderately more likely to be foreign (as measured by patent locations), but substantially less likely to be a target of dismantlement. Notably, they have a high likelihood to be included in the 1939 supplier list, but more so for the firms active in similar technologies as IG Farben. Consequently, firms in this group on average have more products offered in the supplier lists and are more likely exposed to product-market competition by IG Farben.

Table D.19 shows the regression results. The empirical strategy follows the main innovation analysis, with the level of observation shifted to firms. Standard errors in the regressions are clustered at the firm level (Bertrand, Duflo, and Mullainathan, 2004). Unit fixed effects at the same level are included, time fixed effects are at the year level. Firms patenting in technologies with high exposure to the IG Farben breakup strongly increase their patenting output after the breakup, relative to comparison firms. Columns 1-4 individually include the main control variables and columns 5-7 include them all at the same time. All of dismantlement, exposure to the Soviet sector and war destructions predict decreases in patenting in the post-war periods, but the main effect estimates remain unchanged. The effects also remain qualitatively unchanged when excluding IG Farben firms (columns 1-5), considering all firms including the IG Farben successors (column 6) or when excluding foreign firms (column 7). Effect sizes become larger when including the IG Farben successors. The results are smaller in magnitude than the technology-class level regressions of section 4.7.1, hinting towards entry by new innovators playing a role as well.

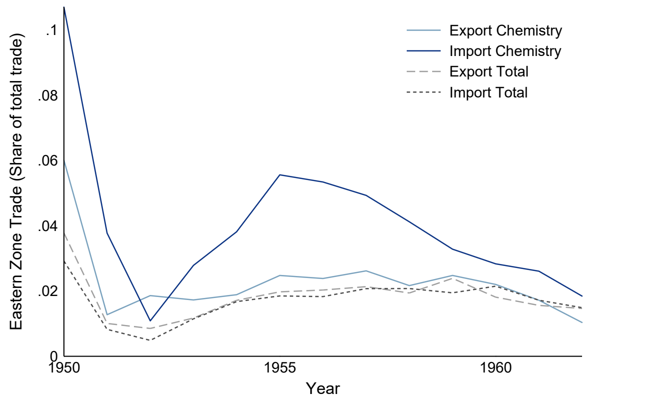
Table D.19: Firm-level regressions controlling for dismantlements and East exposure

	(1) No IG	(2) No IG	(3) No IG	(4) No IG	(5) No IG	(6) All	(7) Domestic
48-51*High ΔHHI	−0.023	−0.074	0.002	−0.028	−0.016	−0.076	−0.159
	(0.137)	(0.135)	(0.126)	(0.138)	(0.126)	(0.119)	(0.105)
52-60*High ΔHHI	0.211	0.136	0.236*	0.202	0.189	0.263**	0.178*
	(0.142)	(0.141)	(0.132)	(0.143)	(0.132)	(0.123)	(0.104)
48-51*Dismantle		−0.266**			−0.047	−0.068	0.007
		(0.114)			(0.102)	(0.097)	(0.097)
52-60*Dismantle		−0.395***			−0.174*	−0.127	−0.035
		(0.112)			(0.103)	(0.099)	(0.099)
48-51*East Pat (%)			−0.908***		−0.931***	−0.902***	−0.736***
			(0.106)		(0.107)	(0.105)	(0.102)
52-60*East Pat (%)			−0.910***		−0.918***	−0.929***	−0.695***
			(0.114)		(0.116)	(0.113)	(0.104)
48-51*Destruction (%)				−0.237	−0.522*	−0.531*	−0.134
				(0.299)	(0.278)	(0.277)	(0.225)
52-60*Destruction (%)				−0.492	−0.757**	−0.756**	−0.140
				(0.312)	(0.296)	(0.293)	(0.219)
DiD: {52-60}-{48-51}	0.234**	0.210**	0.234**	0.230**	0.206**	0.339***	0.337***
	(0.092)	(0.094)	(0.093)	(0.093)	(0.095)	(0.097)	(0.102)
Firm, Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N Firms	403	403	403	403	403	417	368
Adj. R-Square	0.563	0.566	0.580	0.564	0.582	0.617	0.643
Observations	13299	13299	13299	13299	13299	13761	12144

Notes: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ Standard errors clustered on the firm level in parentheses. Dependent variables are inverse hyperbolic sine transformed quality-weighted patents in technology classes related to the chemical industry. In columns 1-5, the sample consists of firms not related to IG Farben. In column 6, all firms are included and column 7 excludes foreign firms. High ΔHHI refers to the top 25% of firms in terms of pre-war-weighted exposure to ΔHHI in technology. Dismantle is a dummy of whether the firm was featured on a dismantlement list. East Pat is the share of pre-war patents in East Germany or Berlin. Destruction is the average war destruction in the German cities, weighted by pre-war patent locations. Poisson regressions or regressions without quality-weighting deliver qualitatively similar results.

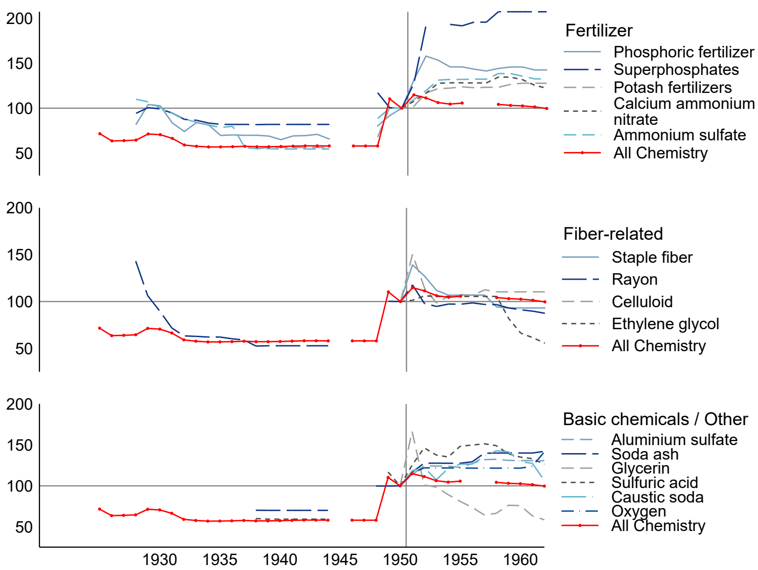
D.4 Historical Context

Figure D.16: East-West trade flows



Notes: D.16: Interzonal trade as a share of total trade, in the chemical industry and total. Earlier numbers are not available from statistical yearbooks. Source: Statistical Yearbooks for West Germany

Figure D.17: Long-run price trends



Notes: D.17: Long-run tendencies of chemical prices. 1950=100 Source: German Statistical Yearbooks, Statistical Yearbooks for West Germany

Figure D.17 shows the long-run development of some chemicals for which the German Statistical Yearbooks reported prices. While the number of prices is not sufficient to draw statistical conclusions about long-run tendencies, it is apparent that price controls strongly restricted the movement of prices before and during the Second World War, at least from 1937 onwards.

Table D.20: Post-war production and capacity restrictions until 1951

Materials	Potsdam Industrial Plan	Revised Industrial Plan	Washington/Petersberg Industrial Plan	Agreement on Industrial Monitoring
Announcement	Mar 46	Aug 47	Apr 49 / Nov 49	Apr 51
Effect	N/A	?	Sept 50	Apr 51
Target level	70-75% of 1936 Dismantle 1500 plants	100% of 1936 859 plants, later 700	Unrestricted Dismantlement stop	Unrestricted
Chemical industry				
Basic chemicals Others chemicals Pharmaceuticals Colors	40% of 1936 70% of 1936 80% of 1936 36k t Export restricted	98% of 1936 97% of 1936 84% of 1936 96% of 1936 Export allowed	Unrestricted	
Synthetic ammonia	Prohibition of production		Post-dismantlement capacity	None
Chlorine	Basic chemicals / Only upon approval		Post-dismantlement capacity	None
Synthetic fuels	Prohibition of production			Monitoring
Plastics value chain				
Styrene	70% of 1936	100% of 1936	20k t	None
Butadiene	Not mentioned		Prohibition of production	
Synthetic rubber, gum	Prohibition of production (ex. small Q)			Monitoring
Synthetic fibers	185k t	Not mentioned	None	
Consumer products	Q Restrictions	Unrestricted	None	
Metals				
Copper, zinc, lead, tin, nickel	ca. 50% of 1936	up to 100% of 1936	None	
Aluminium	Prohibition of production		Capacity restriction	None

Table D.20: Post-war production and capacity restrictions until 1951

Materials	Potsdam Industrial Plan	Revised Industrial Plan	Washington/Petersberg Industrial Plan	Agreement on Industrial Monitoring
Magnesium	Prohibition of production			
Beryllium	Prohibition of production			None
Vanadium	Prohibition of production		None	
War-related products	Prohibition of production			
War material, including explosives, warfare gases, biological weapons				
Firearm propellants, e.g. Nitroguanidine, Nitroglycerin, Diethylene glycol, Nitrocellulose				
Rocket fuels: Hydrogen peroxide (>37%), Hydrazine hydrate, Methyl nitrate				
White phosphorus and other burn agents				

Notes: Summarizes post-war production restrictions until 1951. Not all restrictions laid out came into effect. For example, the Potsdam Industrial Plan had little practical consequence. This was due to a breakdown of coordination among the Allies and changed priorities in the wake of the coming Cold War. Also, the German industry did not reach ceilings before they were adjusted (Morsey (2010, p. 5) and Wallich (1955, p. 369)). Exemplary, with respect to plastics and synthetic ammonia, the Potsdam plan outlawed production, but halted this restriction until sufficient imports were viable. After this, all capital equipment should be removed. Specialized metals are listed as IG Farben subsidiaries were involved in their production. Aluminium, Magnesium, Beryllium and Vanadium are either light metals or ingredients for specialty steel.Butadiene and Styrene - in 3:1 ratio - are ingredients for the synthetic rubber "Buna", among other chemical substances. Styrene was only explicitly regulated in the Washington Industrial Plan, before it was regulated as 'generic chemicals'. With the Washington Agreement, capacity restrictions on civilian production such as cement, paper, textiles and shoes, cars, trains etc. were lifted. Other goods more tightly restricted were steel, heavy machine tools, aircraft, ships and electronic and optical components. Under the agreement on industrial monitoring (1951), industries such as synthetic rubber and synthetic fuels required approval for capacity expansion, but were otherwise free to operate. Source: Harmssen (1951). Factory numbers from Wallich (1955, p. 369).

Table D.21: Dismantling of IG Farben

Successor	Plant	Type	Products / Description
British-American zone			
Bayer	Dormagen	Part	Perlon (en: Nylon)
Bayer	Elberfeld	Part	Cellulose derivatives, artifical resins
Bayer	Holten	Part	1,2-Dichloroethane
Bayer	Leverkusen	Part	Sodium sulfide, "Atebrin" (Mepacrine), polyamides, artificial resins, hydrazine hydrate (Propellant), activated carbon, toluene nitrate (Explosives)
Bayer	Uerdingen	Part	Chloride, causic soda, alkydal artifical resins
Bayer	Zweckel	Part	Diethyl ether, 1,2-Dichloroethane, polyethylene, bleaching powder
Other	Duisburg	Unclear	Liquid oxygen
Anorgana	Gendorf	Part	Bleach und sodium hydroxide, acetaldehyde, glycol
Wacker	Burghausen	Part	No details
Kalle	Wiesbaden	Part	Methyl, ethyl, Cellulose derivatives
Hoechst	Frankfurt/M	Part	"Uresin" (Pastics), acetate, carboresin, black sulfur, solvents, chloride solutions, dinitrobenzene
Hoechst	Griesheim	Unclear	Industrial gases
Other	Kassel	Unclear	Industrial gases
Dynamit	Fürde/Grevenbrück	Part	Explosives, fuses
Dynamit	Schlebusch	Part	Glycerine, toluene nitrate
Dynamit	Troisdorf	Part	Nitrogen, vulcanized fiber, phenol formaldehyde resin, celluloid
Dynamit	Claustal-Zellerfeld	Part	High explosives, grenades
Dynamit	Empelde-Hannover	Full	Ammunition
Dynamit	Near Hamburg	Full	(At Düneburg/Krümel) Explosives
Dynamit	Nürnberg	Full	Bullet casings
Dynamit	Kauferin/Landsberg	Full	Ammunition
Dynamit	Stadeln	Full	Bullet casings
Dynamit	Hamm	Full	Gunpowder
French zone			
Other	Rottweil	Part	Hunting ammunition
BASF	Ludwigshafen	Full	38 plants (unspecified)
BASF	Oppau	Full	11 plants (unspecified)
Other	Rheinfelden	Full	Unspecified
Soviet zone			
IG East	Aken	Full	
IG East	Wolfen	Full	Agfa plants
IG East	Schkopau	Full	Buna plant
IG East	Leuna	Full	Leuna plant
IG East	Piesteritz	Full	Nitrogen plant
IG East	Bitterfeld	Full	
IG East	Coswig	Full	(Former WASAG)

Notes: Dismantlement targets as reported in Harmssen (1951), lists as of 1947. Soviet zone lists actual dismantlements.

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