The London School of Economics and Political Science

Essays in Finance and Innovation

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Declaration

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Chapter 1 and Chapter 3 use the *Inventor Biography Data Linked to Administrative Data of the IAB (INV-BIO ADIAB)* from the German Institute for Employment Research (IAB).

Data access was provided on-site at the Research Data Centre (FDZ) of the Federal German Employment Agency and via remote data access.

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Abstract

This thesis examines which environments foster or possibly constrain innovation, focusing on three settings: the match between inventors and firm type, the regulatory environment surrounding new platform technologies and its effects on local economic prosperity, and the labor markets for inventive talent across countries.

The first chapter asks whether large-scale innovative environments—firms with high patenting activity—are associated with higher *individual* inventor productivity. Using German employer–employee data linked to European Patent Office records, I document sizable inventor-level productivity premia at large-scale innovative firms. Exploiting geographic constraints faced by German apprentices, which generate quasi-random variation in access to large-scale innovative employers, I show that improved access raises early-career patenting by roughly 20%, consistent with a causal link between firm innovative scale and individual productivity. The positive association extends to seasoned inventors. Evidence points to co-worker spillovers and capital–labor complementarities as likely mechanisms.

The second chapter analyzes how regulation shapes the composition of local economic activity around a prominent platform innovation: peer-to-peer short-term rental intermediaries such as Airbnb and Vrbo. Using a staggered difference-in-differences design for U.S. counties from 2010 to 2020, I find limited or no effects on housing prices or tax revenue. By contrast, GDP and personal income per capita in accommodation and food services increase, consistent with reallocation from informal hosting toward hotels. Despite the political controversy surrounding STR platforms, current regulations appear to have only modest aggregate

economic consequences while shifting activity across closely related sectors.

The third chapter compares the labor-market environments for inventors in Germany and the United States using comprehensive employer–employee microdata. Both countries exhibit aging inventor populations, low female participation, growing concentration of inventors in larger and older firms, and declining job mobility. They differ, however, in their ability to attract foreign-born inventors, in the strength of the earnings–productivity relationship, and in the types of firms where inventor output is highest. Taken together, these findings shed light on the possible sources of slowing innovative performance in advanced economies.

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Chapter 1

The Role of Firms' Innovative Scale in Inventor Productivity: Evidence from Germany

Abstract

Do firms with large innovative output simply employ more inventors, or are their innovators more productive? To address this question, I analyse German administrative employer-employee data linked with European Patent Office records. First, I examine early-career patent output. Inventors beginning their careers at firms in the 75th–95th percentile of innovative scale, measured by total annual patent filings, are 40% more productive than those starting at non-patenting firms. Productivity differentials rise to 60% and 90% for those starting at firms in the 95th and 99th percentiles, respectively. Moreover, inventors who, for their first patent, move from smaller- to larger-scale innovative firms achieve similar productivity to those starting and staying at large-scale firms. Using geographic constraints of German apprentices, which result in quasi-random variation in the access to large-scale innovative firms, I show that better access to such firms results in 20% higher early-career

patent output. Finally, I show that the positive relationship between firm innovation scale and individual productivity extends to seasoned inventors, with those moving to larger-scale firms creating more subsequent patents than those joining smaller-scale firms. I provide suggestive evidence that these results are linked to superior co-worker complimentarities and better access to financing at large-scale firms.

1.1 Introduction

[...] education, research, and innovation, I believe we all agree, are essential for sustainable growth, secure jobs, and our prosperity [...]

— Angela Merkel, Federal Chancellor of Germany, 15 April 2010

[...] The first step in winning the future is encouraging American innovation. [...] What we can do – what America does better than anyone – is spark the creativity and imagination of our people.

— Barack Obama, 44th U.S. President, 25 January 2011

Innovation is a critical driver of economic growth (e.g., Romer 1990; Aghion and Howitt 1992). Extensive research has examined factors that influence innovation at various levels—macro, firm, and, more recently, inventor. It is now widely recognized that factors such as team-specific capital, mobility, geographic clusters, and financial incentives affect inventors' patent output. However, there remains limited understanding of the role firms play in shaping the careers and productivity of innovators. Prior evidence shows that large incumbent firms can stifle innovation through poaching innovators from competitors, undertaking "killer acquisitions" of rivals, or engaging in political lobbying.

In contrast, this paper provides evidence that large innovative firms may foster rather than hinder the productivity of individual inventors. Using novel administrative data from Germany that links inventors' social security records with European Patent Office data, I track inventors throughout their careers—from labour market entry to their first patenting job and their subsequent development as experienced inventors.

I find that starting a career at firms with large innovative scale, measured by their total yearly patent output[1], is associated with significantly more future patent applications. Leveraging the limited geographic mobility of school leavers choosing an apprenticeship, I

[1] In this study patent output refers to patent applications.

show that this positive correlation between innovative scale and inventor productivity is not solely due to assortative matching between firms and workers.

I then extend the analysis to examine whether this positive association between large firms and patent output persists for seasoned inventors, employing the matched event study design of Akcigit and Goldschlag (2023). Contrary to their findings, I observe that inventors moving to large-scale innovative firms see an increase, not a decrease, in their patent output[2]. I provide suggestive evidence that worse access to financing by low-scale firms may explain this finding in the German context.

The strong positive correlation between inventor productivity and firms with high innovation output is consistent with two non-mutually exclusive roles that firms may play: They may select talent, or they may contribute to inventor productivity by providing training and resources. Under the first hypothesis, inventors possess inherent, immutable qualities that determine their productivity, regardless of the employer. More innovative firms simply attract more productive inventors because, for example, of their greater ability to capitalize on high-quality innovations. A firm like BMW is better positioned to utilize ideas for new electric vehicle technology than is the local garage, and it can consequently offer a higher wage.

Alternatively, firms may differ in their ability to support and enhance an inventor's productivity through training, collaborations, financial capital, equipment, or administrative support. In this scenario, an inventor who produces few patents at a low-scale innovative firm might generate significantly more at a larger-scale firm.

To examine the relationship between firms' innovative scale and inventor output, I leverage German administrative employer-employee data from the Institute for Employment Research (IAB) that links inventors, firms, and patents from 1980 to 2014 as well as regional data on county-level socio-economic variable such as GDP per capita from the German

[2]Akcigit and Goldschlag (2023) don't categorize firms by innovative scale but by size (number of employees) and age. Using their exact classification, I still find that, post-hire, inventor productivity is larger at incumbent firms (large and old) than at young firms, in contrast to their findings.

Federal and State Statistical Offices. Unlike most of the prior literature, which observes inventor–firm links only through patents (and thus only post–first patent), my data tracks inventors' firm ties even without patents, enabling analysis of early-career (pre-first patent) trajectories.

I classify firms into seven groups. The first group includes all firms with zero patents per year (excluding patents of the focal inventor) at the time the inventor joins. Firms with non-negative patent output are then divided into quartile bins according to their patent output in the year in which the inventor joined the firm. To capture potential nonlinear effects, I further subdivide the fourth quartile: firms in the 75th–95th percentile bins are labelled as Q4*-firms, those in the 95th–99th percentile bins as P95*-firms, and firms in the top percentile bin as P100-firms or "star firms".

The firm where an inventor begins her career is highly predictive of her patent productivity over the next 10 years. Inventors who start at Q4*-firms produce 63% more patents and receive 73% more citations in the first 5 years, and 36% more patents and 30% more citations in the first 10 years, than those who start their careers at non-patenting firms (Q0-firms). This productivity gap is even wider for inventors starting at P95*- or P100-firms: they file for 81% and 128% more patents, respectively, in their first 5 years, and 50% and 77% more over 10 years. There are similar differences in citations, with P95* and P100-starters generating 96% and 127% more in the first 5 years, and 50% and 83% more over 10 years, compared to Q0-starters.

The relationship between firms' innovative scale and young inventors' productivity is convex. Inventors starting at Q1-firms (i.e., firms in the first quartile bin as defined above) produce only 33% more patents and receive 12% more citations in their first 5 years than Q0-starters do, with the difference in citations being statistically insignificant. Over 10 years, Q1-starters do not produce significantly more patents or citations than Q0-starters do.

This evidence is consistent with both interpretations: firms with large innovative scale may be particularly effective at selecting high-ability inventors, or they may provide an environment that actively enhances innovation and boosts individual productivity. Regardless of the underlying mechanism, the evidence suggests that innovation activities at large-scale firms differ from those at small-scale firms.

To further explore the role of firms as either talent selectors or productivity enhancers, I next examine the career paths of inventors who do not patent in their first job ("non-first job inventors").

More than 50% of inventors do not patent in their first job. On average, inventors have held 2.5 jobs when applying for their first patent, which decreases to 1.9 jobs for those starting at Q4*-firms. Regardless of their first firm, the majority of inventors move to firms above the 75th percentile in innovative scale before filing for their first patent. Even among those who begin at already quite innovative Q3-firms, 65% transition to firms above the 75th percentile in innovative scale before their first patent, with over 38% of them joining firms in the top 5% of innovative output.

Given the importance of early-career job moves, I analyse the early-career productivity of non-first-job inventors as a function of the innovative scale of both their first employer and the firm where they file for their first patent. To enhance tractability, I categorize firms into three groups: (1) small- to medium-scale firms ("SM-firms")[3], (2) large-scale firms ("L-firms")[4], and (3) mega firms ("XL-firms")[5], .

Inventors who transition to firms with greater innovative scale than that of their first employer achieve significantly higher ten-year patent and citation outputs, whereas moves to lower-scale firms predict lower productivity. An inventor who starts at a small- to medium-scale firm and moves to a large-scale firm for her first patent filing has 30% greater ten-year patent output and 40% more citations, compared to peers who transition to another low- to medium-scale firm. Moving to a mega firm raises productivity even further, with 88% higher patent output and 105% more citations.

[3]These are firms below the 75th percentile in yearly patent applications, i.e., the Q0–Q3 firms as described above.

[4]These are firms positioned between the 75th and 95th percentiles, i.e., the Q4*-firms.

[5]The firms representing the top 5% in patent applications, i.e., the P95*- and P100-firms.

Conversely, if an inventor who begins at a mega firm moves to a low- to medium-scale firm, her patent output is not significantly higher than if she had started at a low- to medium-scale firm. Additionally, among inventors joining mega firms, productivity differences based on their first employer's innovative scale are minimal.

On the one hand, these results are consistent with larger-scale innovative firms selecting better talent: Non-first-job inventors moving to larger-scale firms may simply be inherently more productive than those moving to smaller-scale firms, leading to a positive correlation between firm scale and individual patent output.

However, the fact that over half of the inventors in the sample move to larger-scale innovative firms for their first patent filing, coupled with minimal productivity differences observed at these firms regardless of inventors' initial employers, challenges a purely selection-based view. While some high-ability inventors may initially be mismatched by starting at small- to medium-scale firms, this alone does not account for why they do not produce any patents at these firms. For instance, the most innovative subset of SM-firms is the Q3-firms, representing those in the third quartile of actively patenting firms. The majority of inventors who start at these Q3-firms do not produce their first patent until after transitioning to larger-scale firms. Even more notably, over one-third of inventors who begin at L-firms—those in the 75th to 95th percentile of actively patenting firms—move to XL-firms before filing their first patent.

This pattern, however, supports the idea that large-scale innovative firms provide an environment that fosters (first-time) innovation. One possibility is that young prospective innovators lack necessary skills and that large-scale firms provide better training. Alternatively, early-career inventors may benefit from co-worker and capital-labour complementarities; smaller-scale firms may lack potential collaborators or specialized, costly equipment.

Consistent with superior co-worker complementarities at large-scale innovative firms, inventors starting at Q3- or better firms have significantly more collaborators over the first 10 years of their careers than do those starting at Q0-firms: inventors who start at Q3-firms have 28% more collaborators, while those at top-percentile firms have 70% more.

While knowledge exchange *within* firms might be important for young inventors, I do not find similar evidence for its importance *across* firms. Specifically, I investigate whether inventors starting their careers in innovation clusters, defined as counties with at least two firms in the top percentile in terms of patent output[6], are more productive in the first ten years of their careers. I find that, controlling for the innovative scale of their initial employer, inventors in clusters are not more productive. Hence, just being close to large innovative firms without being part of them is not associated with higher patent output.

This contrasts with prior U.S. evidence of positive effects of clusters on the patent output of established inventors (e.g., Jaffe et al. 1993; Moretti 2021; Giroud et al. 2024). One reason for the discrepancy could lie in the differing needs of prospective inventors at the start of their career and established inventors in their mid-career. Young inventors might require more training and, with their current level of knowledge, benefit less from free-floating high-level ideas—in contrast to seasoned inventors.

To provide further evidence that the correlation between firms' innovative scale and their inventors' productivity is at least in part causal, I leverage plausibly exogenous variation in the geographic location of those who begin their careers as apprentices. In Germany, instead of attending college or entering the workforce full-time, school graduates can opt for a multi-year vocational training program that combines formal education with hands-on work experience. These programs typically last three years and offer low wages. The vast majority of trainees, a significant portion of whom are minors, are unwilling to move or to even commute beyond 12.5 miles[7] from home. Given that their geographic location is likely shaped by their parents' preferences rather than by their innate ability to innovate, and due to the strong reluctance to move outside the county, this creates a group of career starters

[6]The results are robust to broader definitions of innovation clusters. However, it has been argued that a small number of very large firms often drive initial agglomeration and that it is these firms that shape a cluster's future viability and productivity (e.g., Moretti 2021).

[7]Up to 96% do not want to move and ca. 80% do not even want to commute more than 12.5 miles (Bundesinstitut für Berufsbildung, 2018)

who are quasi-exogenously assigned to a county.

Counties, however, vary significantly in their concentration of firms with high innovative scale. Many counties have none of the firms most strongly associated with high inventor productivity, i.e., firms in the top percentile in terms of patent output ("star firms"), while others have several. Within each county, trainees and firms likely engage in positive assortative matching. Nevertheless, even the most talented prospective inventor is limited to matching with the best firm in her county, which often will not be a star firm. Thus, in counties with star firms, the most capable individuals can match with top-tier employers, while equally talented individuals in other counties must settle for lower-quality firms. If star firms have a causal impact on inventor productivity, we would, on average, expect higher patent output from trainees who begin their careers in counties where these firms are present ("star counties").

Consistent with this, I find that inventors starting their careers in star counties have a 21% higher patent output in the first 8 years and a 16% higher output over the first 10 years of their career. Moreover, comparing the productivity of the ex post 200 most productive inventors in star and non-star counties to get an idea of the effect on potential star inventors, the productivity difference increases to 60% in favour of those starting in a star county.

Next, I explore potential threats to this identification strategy. It could be the case that apprentices in different counties have different human capital due to differences in formal education. However, in Germany the education system is very homogeneous, with over 95% of students visiting public schools. Geographic differences in syllabi are minor and, if at all, exist only at the state but not at the county level, so they should be controlled for by state fixed effects.

I also address the concern that differences in the general socioeconomic environment across counties could affect prospective inventors' ability. For example, more prosperous counties might have both star firms and better educated parents with higher ability children who become higher ability apprentices. To alleviate this concern, I control for county GDP per capita.

Lastly, I run the same analysis but leave out small (in terms of area) counties to mitigate the concern that some trainees might leave the county, which would be more cumbersome the larger the county is. Reassuringly, none of these robustness tests change the outcome of the analysis.

These results suggest that access to star firms at the start of an inventor's career can significantly boost innovative productivity. On the flip side, potentially promising future inventors might see their productivity throttled by limited opportunities to join star firms early in their career.

Finally, I turn my attention to established inventors—those with a proven patent track record. At this stage in their careers, their needs may differ significantly from those of early-career inventors. Seasoned inventors might have already received all the training and formed the networks of co-inventors and colleagues necessary for high productivity, and they might benefit from low bureaucracy in small firms. On the other hand, irrespective of one's career stage, productivity might be elevated by the resources offered by large-scale firms.

Hence, it is an open question whether the innovative output of seasoned inventors is enhanced by the innovative scale of firms. Akcigit and Goldschlag (2023) show that the patent output of experienced U.S. inventors moving to incumbent firms (defined as being large and old) declines by 6 to 11 percent compared to those moving to young firms. They argue that large incumbents poach promising inventors from young firms to avoid being displaced by their disruptive ideas. Rather than implementing these ideas, which comes at a cost, they bench the new inventors while continuing to exploit their incumbent market position using their old technology.

To shed light on the productivity differences of seasoned inventors moving to firms with different innovative scale, I employ a matched event study approach that closely follows Akcigit and Goldschlag (2023). Specifically, I match inventors based on past patenting history, age, and education. I then trace the difference in patent output between inventors who move to firms in the fourth quartile of innovative scale and those who move to firms below this quartile.

I find that inventors who joined Q4-firms have 25% higher patent output than those who joined lower quartile firms. However, since departures from firms and choices of new employers are often endogenous, these results cannot be interpreted as causal.

The results do, however, contrast sharply with the lower patent output observed by Akcigit and Goldschlag (2023) for inventors joining incumbent (large and old) firms, compared to those joining young firms, in the U.S. context. As a robustness test, I apply the same firm classifications used by Akcigit and Goldschlag (2023) and rerun the event study. Consistent with my previous results, seasoned inventors who join incumbent firms, relative to those who join young firms, have higher post-move innovation output.

There are many potential reasons why the effects of firm scale on inventor productivity differ between Germany and the U.S., such as a different sectoral composition of the economy or differential access to financing for young firms.

In the last part of my analysis, I focus on whether the post-move productivity differences of inventors between large-scale and small-scale innovative firms might be due to differences in the ability to finance R&D. Irrespective of firm size, funding R&D is often difficult because most explorative scientific work is not attached to collateralizable assets that could be used to obtain financing. Moreover, unlike the U.S., Germany does not have a well-developed venture capital ecosystem financing high-risk startups. Without sufficient R&D funding, the productivity of even high-ability inventors might be suppressed.

To explore this mechanism in more detail, I first rank industries by their ability to provide collateral, proxied by the ratio of industry fixed assets to industry revenue. The assumption is that small-scale firms that operate in industries that do not require a lot of fixed assets have less collateral and thus an even harder time securing financing for R&D, while larger firms have sufficient cash flow and credit history to fund innovation in these industries. Hence, if financing constraints play a role, the productivity differences of inventors at large-scale and small-scale innovative firms should be even larger in low-collateral industries.

Running the same event study as before, but now adding level and interaction dummies for whether inventors patent in low-collateral industries, I find an even more pronounced productivity premium for seasoned inventors moving to large-scale firms.

This analysis cannot rule out other potential explanations, but it offers a rationale for the post-move productivity differences across firm types in the German context, and for the cross-country differences with the U.S., given the U.S.'s superior ability to fund startups through their more sophisticated (venture) capital markets.

1.2 Related Literature

My findings contribute to several areas of research. First, they add to the literature on innovation output by individual knowledge workers. While most studies in this field focus on factors such as financial and tax incentives (Bell et al. 2019a), teamwork (Jaravel et al. 2018) innovation clusters (e.g. Jaffe et al. 1993; Moretti 2021; Giroud et al. 2024), early exposure to innovation (Bell et al. 2019b) and project funding (Azoulay et al. 2019), the role of firms in driving inventor productivity has received limited attention, with only a few exceptions (e.g., Akcigit and Goldschlag 2023; Di Addario and Wu 2024). In the Italian context, Di Addario and Wu (2024) show that young workers' initial patent filings correlate positively with average firm wages, though this association weakens for experienced inventors. Akcigit and Goldschlag (2023) examine firm size and age, documenting a decline in patent output for established inventors moving to larger, older firms relative to young firms.

Rather than classifying firms by wage, size, or age, this study focuses on firms' innovative scale as measured by patent output. Measuring innovation directly through patent output provides a clearer view of a firm's innovation environment and the potential impact on its inventors than other characteristics do. While many high-paying, large firms are not innovation hubs, highly patent-active firms are typically highly innovative. My results show that large-scale innovative firms are positively associated with inventor productivity across both early-career and mid-career inventors. Unlike Akcigit and Goldschlag (2023) or Di Addario and Wu (2024), I propose an empirical identification strategy to isolate the causal effect of highly innovative firms on inventor productivity.

Second, my findings contribute to the literature examining how firms affect innovation within the broader economy. Recent research suggests that large incumbent firms may suppress innovation through practices such as acquiring rival companies (Cunningham et al. 2021), poaching skilled innovation workers (Akcigit and Goldschlag 2023), lobbying (Akcigit et al. 2023), engaging in strategic patenting (e.g., Shapiro 2003; Jaffe and Lerner 2004; Blind et al. 2009), and operating with inefficient organizational structures (Kerr 2016). In contrast, my results highlight that firms with high innovative scale, which are often large incumbents, are critical environments for nurturing early-career inventors and are correlated with high productivity among established inventors.

Third, my paper speaks to the literature on innovation and labor mobility. Labor mobility is central to innovative activity because it enables the reallocation of human capital and the diffusion of knowledge—both from firms to workers (through learning and training) and from workers to firms (through job-to-job moves).

A foundational insight is that jobs differ in their learning content. Rosen (1972) formalizes a competitive "market for learning," in which workers accept lower wages early in their careers in exchange for steeper subsequent wage growth as they acquire skills through on-the-job experience. Whether these skills translate into mobility depends on their portability. Becker (1962) distinguishes general training, which raises productivity across firms, from firm-specific training, whose returns are lost upon separation and therefore dampen mobility. More recent work refines the notion of portability by emphasizing that skill transfer depends on the composition of tasks and their overlap across jobs (e.g., Lazaer 2009; Gathmann and Schönberg 2010). In the German inventor context, this perspective is particularly relevant: early-career inventors—especially those on the apprenticeship track—often accept relatively low contemporaneous pay while accumulating practical, task-relevant skills that later facilitate moves to higher-paying and more innovation-intensive firms.

Where the previous literature highlights within-firm skill formation, another strand focuses on the worker-to-firm direction: job moves carry knowledge from one firm to another. Almeida and Kogut (1999) show that spillovers do not arise mechanically from colocation; they emerge when inventors actually move across firms. Subsequent studies similarly emphasize that the extent of knowledge transfer depends on characteristics of both the moving inventor and the hiring firm (e.g., Song et al. 2003; Palomeras and Melero 2010).

Consistent with these ideas, I document a life-cycle pattern in which inventors initially move—often repeatedly—toward more innovative firms, where they accumulate capabilities and are more likely to produce their first patent. Later in their careers, inventors more frequently transition to less innovative firms, consistent with a greater role for knowledge diffusion through mobility.

Firms actively manage the trade-off between developing inventors and retaining them. Ma et al. (2023) show that firms can shape the portability of inventor knowledge through project choice—for example, by selecting R&D projects that build more general versus more firm-specific capabilities—thereby mitigating the risk of inventor departures to competitors.

This line of research connects naturally to work on restrictive labor covenants, such as non-compete agreements (NCAs). Early contributions—often contrasting weak enforcement in California with stricter enforcement elsewhere—argue that limited enforceability can foster labor mobility, knowledge spillovers, and the emergence of innovation clusters (e.g., Saxenian 1994; Gilson 1999; Marx et al. 2009).

More recent work emphasizes that mobility restrictions involve a trade-off between investment incentives and talent reallocation. On the one hand, restrictions that reduce mobility can raise firms' willingness to invest in physical capital and other assets that are productive only when paired with key employees' know-how—for example specialized equipment, labs, or proprietary processes—by mitigating hold-up concerns when key personnel can credibly be retained (e.g., Jeffers 2024). Conversely, when restrictions bind, they can impede firms' ability to hire and reallocate inventive talent across firm boundaries. Chen et al. (2021) show that firms may respond by substituting toward alternative legal mechanisms—such as M&A—to access and retain talent when job-to-job mobility is constrained. They further document higher post-acquisition retention where NCA enforceability is stronger, which coincides with higher post-deal profitability.

Although my paper does not study NCAs directly—both because the institutional setting differs and because post-employment restrictions are less central in my data—the implications of tighter mobility frictions in Germany are theoretically ambiguous. On the one hand, greater retention may strengthen firms' incentives to invest in training (including apprenticeship programs, which account for a large share of post-secondary pathways in Germany). On the other hand, restricting early-career mobility could impede the progression to innovation-intensive firms that, in my setting, appears to be a key pathway into first-time inventorship.

Fourth, this paper relates to the literature on firm-level wage and productivity effects surveyed by Card et al. (2018). Although my paper focuses on patent output rather than wages, the underlying question is similar: how much of inventor output reflects inventor type versus firm type.

One part of this literature—often described as "rent sharing"—studies how shocks to firm productivity translate into wages. These studies typically track workers who remain with the same employer and relate their wage changes to within-firm changes in productivity (or rents), and to use instruments for firm productivity to address measurement error and simultaneity. Across settings, this work finds that improvements in firm productivity tend to raise wages, though the implied pass-through is generally modest (e.g., Van Reenen 1996; Guiso et al. 2005; Card et al. 2014).

Another line of work, following Abowd et al. (1999), estimates two-way fixed effects ("AKM") models using matched employer–employee data. Identification comes from wage changes of movers, which allow the researcher to separate persistent worker components from persistent firm components in wages. A robust finding across settings is that firm effects account for a meaningful share of wage dispersion, alongside sorting of high-wage workers into high-wage firms (e.g., Abowd et al. 2003; Card et al. 2013, 2018).

In the same spirit, using matched employer–inventor administrative data, this paper documents that—holding the inventor constant—an inventor's patent output is strongly related to the innovativeness of the employer. I further provide evidence consistent with a causal interpretation: conditional on apprentice inventor characteristics, access to more innovative employers increases subsequent inventive output, suggesting that firm environments shape inventor productivity beyond sorting alone.

Lastly, this paper contributes to the literature on the persistent effects of career starts, which shows that where, when, and how individuals begin their careers can significantly influence long-run career outcomes (e.g., Ryan 2001; Kletzer and Fairlie 2003; von Wachter and Bender 2006). For example, graduating during a recession can have lasting effects on future earnings, job mobility, and employer characteristics (e.g., Oreopoulos et al. 2012; von Wachter 2020), while early-career layoffs from large establishments can similarly impact long-term outcomes (von Wachter and Bender 2006). Moreover, the type of firm where individuals start their careers has significant long-term impacts. Using an instrumental-variable approach, Arellano-Bover (2024) shows that starting at a larger firm significantly and persistently increases lifetime income. My analysis shows that starting at highly innovative firms (which are often large) is associated with higher long-term inventor output. By exploiting the geographic constraints faced by German apprentices, I demonstrate that at least part of this relationship appears to be causal.

1.3 Data

In this section, I first describe my data sources and sample selection. I then define key variables and present summary statistics.

1.3.1 Data Sources

My primary data source is the Linked Inventor Biography Data, developed by the Institute of Employment Research (IAB) and the Max Planck Institute for Innovation and Competition. This dataset integrates inventor and patent information from patent registry data with administrative labour market data on individuals and their employers[8]. The sampling frame for the data includes all inventors residing in Germany who are covered by social security[9] and who are listed in the PATSTAT data on patent applications filed with the European Patent Office between 1999 and 2011. The patent and employment histories of these inventors are tracked from 1980 to 2014. The data are anonymized, preventing the retrieval of additional information on individual inventors, firms, or patents. However, the dataset already provides extensive details, including patent-specific information such as filing dates, co-inventors, technology fields, and forward citations. It also includes firm-level data on industry classification, employee count, wage structure, and firm age, as well as county-level location data. Inventor-specific information covers age, gender, and education.

The second data source is regional data from the German Federal and State Statistical Offices, providing county-level metrics such as GDP, population, and area. Due to extensive redistricting over the past 30 years, particularly following German reunification, county-level information in the IAB data is aligned with district boundaries as of 2016. Accordingly, all county-level data are sourced for that year to ensure consistency.

The third data source is industry-level revenue and investment data from the OECD Analytical Business Enterprise Research and Development (ANBERD) database, which includes information on industry-wide revenue, investment, and R&D expenditures. Industries in this dataset are classified according to the International Standard Industrial Classification of All Economic Activities (ISIC), while the IAB data uses the German Classification of Economic

[8]Throughout this paper, I will use the terms establishments and firms interchangeably. Even though for many applications these two entities are by no means the same, most innovative activities and R&D takes place at the firms' headquarters which provides a good enough approximation for those overall firm characteristics that this paper aims to study.

[9]This group excludes students, self-employed persons, freelancers, and civil servants.

Activities ("Klassifikation der Wirtschaftszweige")[10]. The appendix outlines the process of matching and reconciling these two classification schemes.

1.3.2 Variable Definitions and Summary Statistics

General Firm Characteristics. Throughout the paper, I use firm age, firm size and the average imputed and deflated firm wage as my main control variables. Firm Age is the difference between the year the firm was first recorded in federal employment records and the current period. Firm Size is the total number of employees, and Firm Wage is the deflated mean imputed wage of all full-time employees in 2015 euros.

Apprentices/Trainees. I define "apprentices" broadly to encompass a range of early-career starters in Germany who may undergo similar forms of vocational training, even if formal labour laws classify them differently. Individuals qualify as apprentices if they meet one of two criteria: (1) they hold a school-leaving qualification that does not permit university study ("Haupt-/Realschulabschluss") and begin their first job at age 18 or younger, or (2) they hold a qualification allowing university study ("Abitur") and begin their career by age 22 or younger (which would make it extremely unlikely that these students had gone to university). These age thresholds are based on typical educational pathways in Germany. Parents generally choose to start their child in school at either six or seven years old. Students can complete either 10 years or 13 years of schooling, with an additional possibility of repeating a grade, which is not uncommon in the German school system. For example, a student who starts school at seven and repeats a grade would be 18 years old after 10 years of schooling and 21 years old after 13 years. Moreover, during the period covered by the sample, young men deemed medically fit were required to complete a year of compulsory military service after school, which often delayed career starts.

Firm Patent Output. The variable Firm Patents measures the total number of distinct annual patent filings generated by all inventors in a firm during each year from 1999 to 2011,

[10]Statistisches Bundesamt (2025)

the defined sampling frame. The restriction to 1999–2011 ensures an unbiased representation of firms' patent output. Although patent records exist for earlier years, data coverage is limited before 1999 because it only includes inventors who patent both before and during the 1999–2011 period. This constraint means that for years prior to 1999, some firms' patent output is underrepresented, as it excludes inventors who patented only prior to this period and did not continue patenting thereafter.

Innovative Scale of Firms. For each year, I classify firms into categories based on their patent output in that year to define their innovative scale. Firms with zero patents are grouped into Q0. Among firms with positive patent output, I first categorize them into quartiles: Q1 represents firms in the first quartile of patenting firms, Q2 represents those in the second quartile, and Q3 includes firms in the third quartile. To capture potential nonlinear effects of innovative scale, I further subdivide the fourth quartile: firms inside the 75th to 95th percentile are classified as Q4*-firms, those inside the 95th and 99th percentile as P95-firms, and firms in the top percentile as P100-firms.

As an additional robustness check, I also categorize firms based on (a) the sum of their current patent output and that of the prior year, and (b) the sum of their current output plus outputs from both one and two years prior. Neither approach materially affects the results.

Summary Statistics. Table A.1 presents descriptive statistics for inventor characteristics. The full sample consists of 11,169 inventors whose career start falls between 1999 and 2011. The average number of patents for an inventor is 5.4 but the distribution is skewed—the median is only 2. Inventors are, on average, in the sample for 12.8 years and have 3.3 jobs.

The sample includes a total of 26,885 firms. Table A.2 illustrates that, while yearly patent output differences across firms in the lower quartiles are relatively modest, there is a large disparity at the higher end, particularly among Q4* firms and above. Table A.3 further reveals that even modest differences in patent output among lower quartiles are associated with meaningful distinctions in firm characteristics such as size, age, and average wage. For instance, in 2001, firms in the Zero-Patent category are considerably smaller; the median Zero-Patent firm is less than half the size of the median Q1-firm. Additionally, the median Q2-firm is over 50% larger than the median Q1-firm. These size disparities highlight how varying levels of patent activity correlate with substantial structural differences in firms, even within the lower end of the innovative scale spectrum.

1.4 Inventor Career Starts

In this section, I first describe the types of firms inventors join when they enter the labour market, where they begin patenting, and the firms they move to. I then show how inventors' career characteristics differ based on the innovative scale of their first employer. Next, I demonstrate that starting and/or moving to firms with higher innovative scale is positively associated with early-career individual productivity. Using plausibly exogenous variation in access to large-scale firms, I demonstrate that this relationship is at least partially causal. Finally, I briefly explore potential mechanisms underlying this relationship.

1.4.1 Innovative Firms and Inventor Career Starts

Innovation in Germany is highly concentrated, mirroring patterns seen in countries like the U.S. and Italy (Akcigit and Goldschlag 2023; Di Addario and Wu 2024). For instance, Figure A.1 shows that the top one percent of firms by innovative scale (measured by total yearly patent output) accounted for over one-third of all patent filings in 2001, while the top 5% produced over 55%. These patterns are similar for the years 2006 and 2011.

Table A.4 further shows that more than 22% of prospective inventors entering the labour market in 2001 started at firms above the 95th percentile of patent output, and more than 40% began at firms above the 75th percentile. Conversely, around 44% of inventors joined firms with no patent activity at their time of entry.

Finally, Table A.4 also shows that most inventors do not file for their first patent in their first job. Instead, many inventors transition to more innovative firms before their first filing. While only 22% of inventors start at firms above the 95th percentile of innovative scale, 40% of inventors apply for their first patent at such firms. Similarly, 40% of inventors start at firms above the 75th percentile of innovative scale with 65% of inventors creating their first patent at these firms. Table A.5 shows that even 60% of inventors who start at firms with the lowest innovative scale eventually move to firms in the 75th percentile or above to file for their first patent. By contrast, over 80% of those inventors who begin their careers at firms above the 75th percentile in innovative scale remain there until their first patent filing.

Table A.6, however, shows that, once inventors start patenting a notable share eventually transitions to firms with lower innovative scale. More than 40% of inventors who have filed for their first patent at firms above the 95th percentile of innovative scale later move to firms below the 75th percentile. These findings contrast with Akcigit and Goldschlag (2023), who argue that in the U.S., large incumbent firms poach successful inventors from smaller, younger firms. In Germany, however, large innovative firms primarily recruit prospective inventors who have not yet patented. This suggests two interpretations of the role large firms play: in the U.S., they may drain smaller firms of experienced innovative talent, whereas in Germany, they may help develop innovative talent into inventors who later transition to smaller firms.

1.4.2 Inventor Careers by Innovative Scale of First Employer

Table A.7 shows that inventors' career trajectories depend on the innovative scale of the firm they start at. For instance, inventors who begin at Q0-firms (those with the lowest innovative scale) average 3.3 jobs and 7.6 years before creating their first patent. In contrast, inventors starting at P95*- or P100-firms (those in the 95th–99th and top percentiles of innovative scale) take only 5.3 and 4.7 years, and 1.8 and 1.7 jobs, respectively, to achieve their first patent.

Within the first decade of their careers, inventors who began at P95*- or P100-firms typically hold around 2.6 jobs on average, while those starting at Q0-firms average almost

4 jobs. This links back to the earlier observation that inventors from lower-scale firms often transition before they apply for their first patent. Interestingly, once inventors reach the firm where they file for their first patent, the time spent there before creating the patent is similar across all groups. For example, those initially at Q0-firms take about 2.2 years, while those from P100-firms take around 2.1 years.

These results suggest two possible explanations: Inventors from Q0-firms may have inherently lower productivity, resulting in them being selected by lower-scale firms that often pay lower wages (see Table A.3) and them requiring more job transitions and more time to reach their first patent. Alternatively, differences in productivity may be less about individual ability and more about the environment, where large-scale innovative firms may provide a superior setting that fosters first-time inventors. Once in this conducive environment, inventors take about the same time to produce their initial patent.

The innovative scale of an inventor's first firm is strongly linked to their ten-year career productivity. For example, inventors who start at Q0-firms average about 3 patents within their first decade, while those beginning at Q3-firms produce around 4.4 patents, and those starting at P100-firms achieve 7.5 patents on average. The next subsection will delve deeper into this productivity gap.

1.4.3 Innovation Output by Innovative Scale of First Employer

In this subsection I analyse the relationship between early–career patent output and the innovativeness of an inventor's first employer, measured by its total patent output in the year in which the prospective inventor joins the firm. I estimate the following Poisson regression of inventors' n-year early–career patent or citation output:

$$\mathbb{E}[y_{ijtn} \,|\, \cdot] = \exp\left(\beta_{0n} + \sum_{q=1}^{6} \beta_{qn} \times Q_{jtq} + \mathbf{x}'_{jt}\mathbf{\Lambda} + \mathbf{z}'_{it}\mathbf{\Psi} + \boldsymbol{\eta}\right). \tag{1.1}$$

The n-year cumulative patent count of inventor i starting her career at firm j in calendar year t is denoted by y_{ijtn}. Q_{jtq} is a set of dummy variables for the firm's innovative scale in the hire year t. Specifically, Q_{jt1}–Q_{jt3} are dummies that equal 1 if the firm falls within the first, second, or third quartile of patent output (i.e., Q1–, Q2–, or Q3–firms). Q_{jt4} equals 1 if the firm is in the 75th–95th percentile (Q4*–firm), while Q_{jt5} and Q_{jt6} equal 1 if the firm is in the 95th–99th percentile range or the top percentile (P95*– and P100–firms), respectively. The dummy for non-patenting firms (Q0) is omitted.[11] The coefficients of interest, β_{qn}, can be interpreted as semi-elasticities: they give the percent change in the conditional mean relative to Q0 for bin q.

Firm controls $\mathbf{x}_{jt}$ include firm age, the natural logarithm of the number of employees, and the natural logarithm of average firm wage. Inventor controls $\mathbf{z}_{it}$ include inventor age and level of education. Lastly, I include a set of fixed effects—hire-year, industry, and state—collected in the vector $\boldsymbol{\eta}$.

These covariates control for much of the heterogeneity in early patenting. For instance, ability differences between inventors may already be reflected in their educational choices, while industry and state fixed effects account for regional productivity differences and variations in patenting across industries. Yet it is difficult to rule out the omission of other confounding variables, and thus the resulting estimates should not be interpreted as causal.

Table A.8 shows the coefficients for Equation (1.1), revealing a strong and convex relationship between firm innovative scale and inventor productivity. Inventors who begin at Q4*-firms produce 45% more patents in their first 8 years and 36% more in their first 10 years compared to those starting at Q0-firms. The effect is even larger for inventors at P95*- and P100-firms, where productivity is 59% and 92% higher after 8 years, and 50% and 77% higher after 10 years, respectively. In contrast, inventors starting at Q1- and Q2-firms do not produce significantly more patents over 10 years than those starting at Q0-firms.

Firm innovative scale correlates not only with the quantity but also with the *quality* of individual output, as measured by 4-year citation counts. Inventors starting at Q4*-firms

[11] These are conditional quartiles, i.e., the bins are formed conditional on firms with positive patent output.

achieve 38% more citations in the first 8 years and 30% more in the first 10 years relative to those at Q0-firms. This quality difference is again larger for inventors at P95*- and P100-firms, who have 57% and 90% more citations after 8 years, and 50% and 83% more after 10 years, respectively.

At this point, it is difficult to discern whether large-scale innovative firms primarily act as talent selectors or productivity enhancers. However, given the strong correlation between innovative scale and individual inventor productivity, the analysis indicates that these firms are effective in fulfilling at least one of these roles.

1.4.4 Job Moves before the First Patent

To further explore the relationship between firms' innovative scale and inventor productivity, I expand the analysis beyond an inventor's first employer to include their employment history up to the firm where they file for their first patent. Specifically, the focus is on two milestones: the inventor's initial employer and the employer at which they apply for their first patent.

For tractability, I group firms into three categories: small- to medium-scale innovative firms (SM: Q0–Q3), large-scale innovative firms (L: Q4*), and *mega* innovative firms (XL: P95*, P100). Let $\Theta = \{\mathrm{SM}, \mathrm{L}, \mathrm{XL}\}$. Let $S_i \in \Theta$ denote the type of inventor i's *starting* firm (in hire year t), and let $P_i \in \Theta$ denote the type of the firm where inventor i files the *first patent*. Define the pair indicator

$$D_{i,qq'} \equiv \mathbf{1}\{\, S_i = q \wedge P_i = q' \,\}, \qquad (q, q') \in \Theta \times \Theta.$$

I estimate the following Poisson model for the n-year cumulative output:

$$\mathbb{E}[y_{itn} \,|\, \cdot] = \exp\Big(\beta_{0n} + \sum_{\substack{(q,q') \in \Theta \times \Theta \\ (q,q') \neq (\mathrm{SM},\mathrm{SM})}} \beta_{qq'n}\, D_{i,qq'} + \mathbf{x}'_{st}\boldsymbol{\Lambda} + \mathbf{z}'_{it}\boldsymbol{\Psi} + \boldsymbol{\eta}\Big). \tag{1.2}$$

Here y_{itn} is the n-year cumulative patent count of inventor i (from the hire year t). The vector $\mathbf{x}_{st}$ collects firm-type-by-year controls for the inventor's starting type $s = S_i$ in year t (e.g., firm age, log employees, log average wage), and $\mathbf{z}_{it}$ contains inventor controls (e.g., age, education). The vector $\boldsymbol{\eta}$ bundles fixed effects (hire-year, industry, state) that absorb broad time, sectoral, and regional heterogeneity. The baseline pair $(\mathrm{SM}, \mathrm{SM})$ is omitted, so each $\beta_{qq'n}$ compares the conditional mean to SM→SM. As in the first-employer specification, the $\beta_{qq'n}$ can be interpreted as semi-elasticities relative to the SM→SM baseline.

Table A.9 presents the regression coefficients for Equation (1.2). Regardless of the innovative scale of an inventor's first employer, moving to large-scale or mega firms (i.e., L- or XL-firms) before applying for one's first patent is associated with significantly higher subsequent productivity over 10 years. For instance, inventors who start at SM-firms but create their first patent at L-firms have a 30% higher patent output and 40% higher citations than those who start and patent at SM-firms. The difference is even larger for those who move from SM- to XL-firms. These innovators have 88% higher patent output and 105% higher citations over 10 years.

In contrast, inventors starting their careers at XL-firms but moving to SM-firms to create their first patent do not have a higher productivity than those who began their careers and created their first patent at SM-firms.

Interpreting these results causally is challenging because job moves are likely endogenous. For instance, there may be initial mismatches. Some high-ability inventors may start at SM-firms, while some lower-ability inventors may initially secure positions at XL-firms. Over the first few career years, these inventors may switch jobs to better align with their abilities, resulting in high-ability inventors working at XL-firms and lower-ability ones at SM-firms by the time they file for their first patent.

However, this still raises the question: Why do most inventors move to at least large-scale (L) firms to create their first patent? If high-ability inventors who begin at SM-firms are indeed highly talented, why can't they produce their first patent at SM-firms before moving to larger firms? After all, many SM-firms are also patenting, albeit at a smaller scale.

Before presenting causal evidence, I first briefly explore why and how innovative scale could affect individual inventor productivity. I do so by focusing on a specific mechanism: *knowledge transfer.*

1.4.5 Mechanism: Knowledge Transfer

This subsection examines two channels through which firm innovative scale may affect innovation—*collaboration* and *innovation clusters*—both of which derive their value from knowledge exchange between inventors.

While positive assortative matching between high-quality inventors and firms could mechanically explain the correlation between inventor output and firm quality, knowledge exchange—within firms or across firms in clusters—offers an alternative mechanism through which firms may influence innovation production (e.g., Akcigit et al. 2018; Jaravel et al. 2018; Moretti 2021).

Collaboration. Let $colabs_{ijtn}$ denote the total number of *distinct* collaborators inventor i works with during the first n years of her career, starting in hire year t at firm j. I estimate the following Poisson model for the conditional mean number of collaborators:

$$\mathbb{E}[colabs_{ijtn} \,|\, \cdot] = \exp\left(\beta_{0n} + \sum_{q=1}^{6} \beta_{qn}\, Q_{jtq} + \mathbf{x}'_{jt}\boldsymbol{\Lambda} + \mathbf{z}'_{it}\boldsymbol{\Psi} + \boldsymbol{\eta}\right). \tag{1.3}$$

Here Q_{jtq} are dummies for the firm's innovative-scale bin in hire year t (Q1–Q3, Q4*, P95*, P100; Q0 omitted), $\mathbf{x}_{jt}$ and $\mathbf{z}_{it}$ are firm and inventor controls, and $\boldsymbol{\eta}$ collects fixed effects (hire-year, industry, state). The β_{qn} can be read as semi-elasticities relative to Q0.

The categories for innovative scale and the control variables are defined as in Equation (1.1). Table A.10 shows that inventors who start at larger-scale innovative firms have significantly more collaborators than those at the lowest-scale (Q0) firms. For instance, starting at a Q3-firm is associated with a 28% higher number of collaborators over 10 years compared to starting at a Q0-firm. This effect is even stronger at higher levels: inventors at

Q5-firms have 42% more collaborators, while those at Q6-firms have 71% more.

While this correlation could stem from either firm-facilitated team formation or the natural tendency of high-ability inventors to work in teams (assortative matching), both mechanisms imply value creation. Even if firms simply serve as meeting points where high-skilled inventors—who may have a greater preference for collaboration—can connect with minimal friction, this alone could be useful.

Innovation Clusters. While firms may be the primary locus for idea exchange, innovation clusters are arguably a close second. This raises the question of whether proximity to innovation alone, rather than employment in a highly innovative firm, is associated with higher productivity for young inventors.

To shed light on this question, I categorize German counties into innovation–cluster counties ("IC counties") and non–innovation–cluster counties ("Non-IC counties"). IC counties host at least two firms in the top percentile of patent output ("star firms"), while non-IC counties do not. I then estimate the following Poisson regression:

$$\mathbb{E}[y_{ijtn} \,|\, \cdot] = \exp\big(\beta_{0n} + \beta_{1n}\, IC_i + \beta_{2n}\, SF_j + \beta_{3n}\, (SF_j \times IC_i) + \mathbf{x}'_{jt}\boldsymbol{\Lambda} + \mathbf{z}'_{it}\boldsymbol{\Psi} + \boldsymbol{\eta}\big)\,. \tag{1.4}$$

As before, y_{ijtn} denotes the n-year cumulative patent count of inventor i starting her career at firm j in calendar year t. The dummy IC_i equals 1 if inventor i starts her career in an innovation–cluster county, while the dummy SF_j equals 1 if she starts at a "star firm." Inventor controls $\mathbf{z}_{it}$ and firm controls $\mathbf{x}_{jt}$ are defined as in earlier specifications. Finally, $\boldsymbol{\eta}$ collects fixed effects—hire-year, industry, and state—that absorb broad time, sectoral, and regional heterogeneity.

Table A.11 presents the regression coefficients for Equation (1.4). Consistent with the previous analysis, starting at a star firm is associated with approximately 36% higher patent output in the first 10 years of an inventor's career compared to those who do not start at a star firm. In contrast, starting in an IC county without joining a star firm does not yield significantly different productivity outcomes compared to starting in a non-IC county.

This may appear counterintuitive, as prior studies (e.g., Jaffe et al. 1993; Moretti 2021; Atkin et al. 2022; Giroud et al. 2024) suggest that innovation clusters boost inventor productivity. However, these studies do not consider career stages, and early-career inventors may, for example, require structured training more than access to freely circulating ideas. The latter is arguably more valuable to seasoned inventors. Moreover, if training is key, firms within clusters but outside the star category may hesitate to invest in training for young inventors, fearing poaching by star firms. In contrast, more productive star firms, with their higher wages, can likely retain the talent they invest in, making human capital investments for these firms more profitable.

1.4.6 Causal Evidence

This subsection moves toward a causal interpretation of the positive relationship between firms with large innovative scale and early-career inventor output by focusing on geographically constrained career starters.

Ideally, inventors would be randomly assigned to firms,[12] allowing us to observe if those at highly innovative firms demonstrate higher patent output than those at less innovative firms do, though this is infeasible.

Instead, I examine a group of career starters—trainees—who are quasi-randomly assigned to counties and generally do not relocate. I use cross-county differences in the presence of star firms (i.e., firms that are in the top percentile of patent output) as an exogenous source of variation in the access to these firms. *Within* counties, I expect there to be endogenous matching, with high-ability trainees more likely to join the best firms. However, not all counties have star firms. In these counties, the top local firms do not reach the innovative scale of star firms like Volkswagen or BASF, constraining the opportunity set of trainees.

Under the assumption that the presence of star firms is the only channel through which

[12] Technically, even this might have the issue that innovative scale might be correlated with other firm unobservables. In fact, ideally, inventors *and* innovative scale should be randomly assigned to firms.

star counties (i.e., counties with at least one star firm) enhance trainee productivity, a positive relationship between such counties and trainee productivity would suggest a causal effect of highly innovative firms.

Apprenticeship System in Germany. In Germany, high school graduates can choose between entering the labor market directly, attending university, or pursuing an apprenticeship program. Students opting for apprenticeships are legally permitted to leave high school as early as age 15, and many do so.

Apprenticeships constitute a cornerstone of the German education system. In fact, they represent the most popular form of post-secondary education, with more than 50% of the population having completed an apprenticeship[13].

The German apprenticeship system is often characterized as a "dual system" because it combines two core components over a multi-year period[14]. First, apprentices receive hands-on training and work experience within firms, typically 3-4 days per week. Second, they attend classes at public vocational schools 1-2 days per week.

Both components—firm-based training and vocational schooling—are highly regulated. Curricula and required skills for both settings are determined through consensus among unions, employer associations, chambers of industry, works councils, and state ministries of education. Additionally, training standards for over 400 professions are codified in law.

Not all firms are authorized to offer apprenticeship programs. To qualify, firms must meet numerous standards and employ specially certified instructors known as *Meister* ("masters of a trade"). These instructors themselves must complete additional multi-year training and accumulate substantial work experience beyond their own apprenticeships before earning the right to train apprentices. Compliance with these regulations is actively monitored and enforced by both local chambers of commerce and state education ministries in firms and vocational schools alike. The historical rationale for such stringent oversight was to ensure that apprentices acquire broadly marketable skills applicable across many firms, rather than

[13] Destatis (2025)

[14] The following discussion relies heavily on work by Franz and Soskice (1994).

merely firm-specific competencies.

While the curriculum and standards are strictly prescribed, firms retain considerable flexibility in how they implement the required training. Some provide primarily on-the-job training, while others maintain dedicated training facilities focused exclusively on developing apprentice skills. Thus, the delivery of training varies with firm heterogeneity.

Finally, given firms' substantial investment in apprentice education—with no guarantee that apprentices will remain after program completion—apprentices receive only very modest wages during their training period despite their often considerable labor contributions.

Apprentice Inventors. Apprenticeships are prevalent not only in the general population but also among inventors in my sample. Table A.12 shows that 27% of all inventors and 9% of star inventors have apprenticeship backgrounds. However, this share varies considerably across technology fields: in Chemistry and Pharmaceuticals, 19% of all inventors and 4% of star inventors completed apprenticeships, while in Civil Engineering and Mechanical Engineering (a key driver of Germany's automotive industry), over 30% of all inventors and up to 16% of star inventors began as apprentices.

Selection into Apprenticeship Programmes. The current analysis is conditional on entering an apprenticeship program. In a rational choice framework, the presence of highly innovative star firms in a county may influence both the decision to enter an apprenticeship and the choice of field among those who do. For instance, academically strong high school students who might otherwise attend university could opt for apprenticeships instead, or students already planning apprenticeships might choose technical fields (e.g., chemical or mechanical engineering) over service fields (e.g., banking). Consequently, the effect I measure likely reflects a combination of two mechanisms: a pure training effect, where apprentices receive superior instruction conditional on program entry, and a selection effect, where higher-ability individuals are attracted to apprenticeships in counties with star firms. Importantly, even if selection plays a role, this still represents a causal impact of star firms on regional innovation capacity.

Location Constraints of Apprentices. Given their young age and low wages, it is perhaps unsurprising that the vast majority of trainees remain in their county of high school graduation, often even staying in their parental homes. Surveys indicate that up to 96% of high school graduates are unwilling to relocate, with over 80% unwilling to commute more than 12.5 miles,[15] even within their county, for an apprenticeship.

Location and Apprentice Ability. The location of trainee career starters is likely to be determined by the location preferences of their parents rather than by their innate ability. However, it remains possible that certain regions produce higher-quality human capital through, for instance, superior formal education—either via better secondary schools or higher-quality public vocational training schools during apprenticeships. If true, any positive correlation between star counties and inventor productivity could simply reflect knowledge acquired outside the firm rather than within it.

This concern is mitigated by several institutional features of the German education system. Unlike in the U.S., German secondary education is highly uniform: over 95% of students attended public schools during the sample period, with minor curricular differences existing only at the state level. These state-level variations are absorbed by state fixed effects in my empirical specification.

Similarly, as discussed earlier, vocational schools must adhere to a strict, centrally mandated curriculum that is tightly supervised by state education ministries. While monitoring intensity may vary across states, it is uniform within states. Again, any across-state variation is captured by state fixed effects.

That said, overall apprenticeship quality may still vary by region because a substantial portion of training occurs within firms, and firms retain flexibility in how they deliver the mandated curriculum. Crucially, this variation need not threaten identification. If, for example, certain counties that host star firms offer superior apprenticeships because the firm-based component at these star firms is of higher quality, then this is precisely part of

[15] Bundesinstitut für Berufsbildung (2018).

the treatment effect I aim to measure.

Star firms might also influence apprenticeship quality indirectly through spillover effects on the local training ecosystem. For example, a star firm could donate resources to the local public vocational school, or competing firms in the region might enhance their own apprenticeship programs to attract trainees who might otherwise join the star firm. While I find no anecdotal evidence of direct subsidies to vocational schools, and data limitations prevent me from testing for competitive upgrading effects, such mechanisms—if present—would still constitute genuine treatment effects of star firms, albeit operating through channels beyond the firm's own training program.

Finally, and more broadly, local socioeconomic conditions might also influence inventor ability. For instance, wealthier counties may attract high-ability individuals who then have high-ability children, creating a potentially spurious positive relationship between inventor output and star county status. To address this concern, I include the logarithm of county-level GDP per capita as a control variable to account for wealth disparities across counties.

Movers. Finally, it is important to acknowledge the possibility that some trainees may leave their county of origin. A related concern is that those who move are precisely the individuals who are likely to become inventors. While I cannot entirely dismiss this possibility, I conduct the same analysis excluding the 33% smallest counties. The rationale is that in larger counties, the costs and challenges associated with relocating or commuting are heightened due to the increased distance from parental resources. Consequently, trainees in these larger counties are expected to be even less likely to move.

Empirical Strategy. I classify counties into star and non-star counties based on their number of star firms as described above. I then investigate whether apprentices starting their careers in star counties experience higher future patent productivity by estimating the following Poisson regression:

$$\mathbb{E}[y_{ictn} \mid \cdot] = \exp\big(\beta_{0n} + \beta_{1n} SC_c + \boldsymbol{z}'_{it}\boldsymbol{\Psi} + \boldsymbol{\eta}\big) \tag{1.5}$$

Here, y_{ictn} denotes the n-year cumulative patent applications of inventor i starting her career in county c in calendar year t. The dummy SC_c equals 1 if inventor i starts her career in a star county. Inventor controls, along with state, industry, and hire-year fixed effects, are defined as in previous specifications.

Table A.13 presents the regression coefficients for this specification. Trainees starting in a star county have more than 20% higher patent output in the first eight years of their careers and more than 15% higher output after ten years. Restricting the analysis to larger counties to further reduce the likelihood of inventors switching counties yields even stronger results: inventors starting in star counties experience a 30% higher patent output after 8 years and a 24% higher output after 10 years.

Importantly, these effects represent the productivity premium of star counties for the average inventor. However, innovation and technological progress is often driven by only a small set of individuals. Thus, to explore the productivity differences between high-ability inventors in star and non-star counties, I rerun the analysis but restrict the sample to the ex post 200 most productive inventors ("star inventors") in star and non-star counties and compare their patent output.

Table A.14 reports the results. They are consistent with prior patterns, though the differences are more pronounced: star inventors in star counties exhibit 70% higher patent output after eight years and 60% higher output after ten years than their counterparts in non-star counties do.

These findings suggest that the relationship between the innovative scale of firms and individual inventor productivity has a causal component. They also raise the question how much innovative potential is lost by limited access to the right kind of firms early in an inventor's career.

1.5 Mid-Career Inventor Productivity

This section examines whether the positive relationship between a firm's innovative scale and individual inventor productivity extends into an inventor's later career by analysing job-to-job transitions among experienced inventors. First, I show that inventors moving to larger-scale innovative firms exhibit higher productivity than those transitioning to smaller-scale firms. I then explore whether differential access to financing between large- and small-scale firms could help explain this productivity gap.

1.5.1 Mid-Career Job Moves and Inventor Productivity

The conditions that foster productivity for early-career inventors may differ significantly from those for seasoned inventors. Early in their careers, factors like technical training, mentorship, and access to a broad network of collaborators may play a primary role in driving productivity. Large-scale innovative firms, with their larger R&D teams and capacity to distribute training costs (e.g., Lynch and Black 1998; Knott and Vieregger 2020), may be better positioned to provide these resources. In contrast, established inventors are likely to have already mastered essential skills and built extensive co-inventor networks, possibly across multiple firms. For them, productivity may be hindered by the bureaucracy and internal politics typical of larger-scale firms. Alternatively, large-scale innovative firms could continue to enhance productivity at any career stage due to their superior ability to offer access to costly equipment and other resources.

To estimate the difference in patent output between experienced inventors at large-scale and small-scale innovative firms, I use the matched event-study framework proposed by Akcigit and Goldschlag (2023) who, using U.S. data, study the productivity differences of matched inventors after having been hired by either incumbent (i.e., a firm with a large number of employees that is also old) or young firms.

Following their approach, for each year I identify hire events of inventors and differentiate between those moving to firms with an innovative scale in the fourth quartile and those moving to firms below the fourth quartile. I then create an annual panel of these inventors' patent output four years before and four years after the hire event. To identify close matches between inventors moving to large-scale firms and those moving to small-scale firms, I create patent tertiles for each year preceding the hire event. I then match inventor pairs based on the closest patent output for each year in the pre-hire period, as well as on age, education, industry, and hire-year.

To estimate the post-move difference in inventor productivity between those inventors joining high-scale innovative firms and those joining small-scale ones, I estimate the following equation of patent output, y_{ite}, for inventor i, hire event e and hire year t:

$$y_{ite} = \alpha + \sum_{j=-4,\, j\neq -1}^{4} \lambda_j \, D(j)_{ite} + \beta_1 \, LargeScale_{ie} + \sum_{j=-4,\, j\neq -1}^{4} \eta_j \, D(j)_{ite} \times LargeScale_{ie} + \delta_j + \gamma_k + \psi_i + \varepsilon_{ite}. \tag{1.6}$$

In this setup, $LargeScale_{ie}$ is an indicator variable equal to one if the inventor transitions to a firm with innovative scale in the top quartile. $D(j)_{ite}$ is a dummy representing relative event time, while δ_j, γ_k, and ψ_i are industry, hire-year, and inventor fixed effects, respectively. The coefficient of interest, η_j, captures the post-move difference in patent output between inventors hired by large-scale innovative firms and their counterparts hired by low-scale innovative firms.

Figure A.2 presents the coefficient estimates for η_j, showing that inventors who move to firms with a high innovative scale demonstrate significantly higher productivity after the hire event compared to those joining lower-scale firms. Specifically, each year post-move, hires at large-scale firms produce nearly a quarter more patents than their counterparts at smaller-scale firms. This absolute productivity delta is nearly twice as large as the difference found by Akcigit and Goldschlag (2023), who compare inventors at incumbent firms (defined as $>$ 1000 employees, firm age $>$ 20 years) to those at young firms (firm age $<$ 5 years). Furthermore, Akcigit and Goldschlag (2023) document a *negative* relationship between post-move productivity and employment at incumbent firms relative to young firms.

Although Akcigit and Goldschlag (2023) categorize firms by size and age rather than innovative scale, as we have seen before (Table A.3), firms in the top innovative quartile also tend to be larger and older on average. Given this overlap, it is somewhat surprising that the productivity effects in my analysis run counter to theirs. To test this further, I rerun the event study using Akcigit and Goldschlag's exact firm categories. Figure A.3 shows the resulting estimates for η_j. This time, the magnitude of my coefficients closely matches theirs; however, in my sample, inventor productivity following a move is *higher*, not lower, at incumbent firms compared to young firms.

1.5.2 Event Study with Financial Constraints

There are potentially several factors that could explain why inventors are (1) more productive at large (innovative) firms in Germany but (2) less productive at large firms in the U.S. One such mechanism that would be consistent with both patterns is related to the ability of small-scale innovative firms to finance R&D. R&D funding is inherently difficult to secure due to the risky, non-collateralizable nature of research activities, and small firms with limited cash flow often encounter additional barriers. However, small-scale innovative firms in the U.S. may have better access to financing through more developed (venture) capital markets (Hege et al. 2009), which could support their ability to foster inventor productivity.

To explore this further, I rank industries by their investment-to-revenue ratio, classifying those above the median as high-collateral industries and those below as low-collateral industries. Using the actual level of fixed assets would be preferable, but this data was unavailable. However, in robustness tests focusing on a random subset of firms in these industries and using hand-collected firm-level financial data, I find a strong correlation between *investment* in fixed assets and *levels* of fixed assets, supporting the validity of this proxy. The core assumption here is that firms with a higher fixed-asset-to-revenue ratio can offer more collateral, making it easier for them to secure financing for R&D.

I then adjust the previous event-study framework by including the dummy variable $LoCol_{ie}$, which is set to 1 if the inventor is employed in a low-collateral industry and to 0 otherwise. Specifically, I now compare the productivity differential for inventors joining large-scale innovative firms versus smaller-scale firms within low-collateral industries (relative to high-collateral industries) following a hire event by estimating the coefficient ϑ_j in the following regression:

$$\begin{aligned} y_{ite} &= \alpha + \sum_{j=-4,\, j\neq -1}^{4} \lambda_j\, D(j)_{ite} + \beta_1\, LargeScale_{ie} + \beta_2\, LoCol_{ie} \\ &+ \sum_{j=-4,\, j\neq -1}^{4} \eta_j\, D(j)_{ite} \times LargeScale_{ie} \\ &+ \sum_{j=-4,\, j\neq -1}^{4} \theta_j\, D(j)_{ite} \times LoCol_{ie} \\ &+ \sum_{j=-4,\, j\neq -1}^{4} \vartheta_j\, D(j)_{ite} \times LargeScale_{ie} \times LoCol_{ie} \\ &+ \delta_j + \gamma_k + \psi_i + \varepsilon_{ite}. \end{aligned} \tag{1.7}$$

Figure A.4 shows that, post-move, productivity differences at large-scale innovative firms versus smaller-scale firms correspond to up to 0.085 additional patents per year in low-collateral industries (relative to high-collateral industries). Rerunning the analysis with post-move event dummies collapsed into a single coefficient confirms both the magnitude and statistical significance of the effect.

As an additional robustness check, I examine whether the scale-productivity relationship varies with industries' dependence on external finance. Following Rajan and Zingales (1998), I classify industries by their reliance on external financing to proxy for financial constraints[16].

The intuition is that smaller firms face greater difficulties accessing external capital markets, e.g., due to information asymmetries and higher fixed costs of financing (e.g., Altinkiliç and Hansen 2000; Hadlock and Pierce 2010). In industries with high external finance depen-

[16]Following Rajan and Zingales (1998), I assume that technological differences in industries' demand for external finance, measured using U.S. data, persist across countries.

dence, these frictions may be particularly binding, limiting smaller firms' ability to make the complementary investments in equipment, laboratories, and other research inputs necessary for inventor productivity. By contrast, in industries that generate sufficient internal cash flows, the financing advantage of larger firms should be less pronounced.

I rank industries by their external finance dependence—defined as the difference between investments and cash flow from operations—and classify industries above the median as having high external finance dependence ($HiExtDep$)[17]. Equation (1.8) replaces $LoCol_{ie}$ with $HiExtDep_{ie}$ while maintaining the same interaction structure as Equation (1.7):

$$\begin{aligned} y_{ite} &= \alpha + \sum_{j=-4,\, j\neq -1}^{4} \lambda_j \, D(j)_{ite} + \beta_1 \, LargeScale_{ie} + \beta_2 \, HiExtDep_{ie} \\ &+ \sum_{j=-4,\, j\neq -1}^{4} \eta_j \, D(j)_{ite} \times LargeScale_{ie} \\ &+ \sum_{j=-4,\, j\neq -1}^{4} \theta_j \, D(j)_{ite} \times HiExtDep_{ie} \\ &+ \sum_{j=-4,\, j\neq -1}^{4} \vartheta_j \, D(j)_{ite} \times LargeScale_{ie} \times HiExtDep_{ie} \\ &+ \delta_j + \gamma_k + \psi_i + \varepsilon_{ite}. \end{aligned} \tag{1.8}$$

Figure A.5 summarises the results. The productivity differential between movers to large-scale versus small-scale innovative firms is between 0.148 to 0.388 additional patents per year larger in high external finance dependence industries relative to low dependence industries. This effect is economically meaningful and, if anything, larger than the effects documented using access to collateral.

Thus, in industries where smaller-scale firms face greater challenges in securing R&D financing, the productivity advantage for inventors hired by large-scale firms over those hired by small-scale firms is even more pronounced. While this evidence cannot rule out alternative explanations, it is suggestive of financial constraints more strongly impacting

[17] I use a binary classification rather than the continuous measure due to data constraints in the confidential IAB dataset, where incorporating non-binary external variables presents disclosure challenges.

R&D at smaller-scale innovative firms.

1.6 Conclusion

Inventors are central to innovation and economic growth, yet little is known about how their productivity relates to their allocation across firms. This paper examines the influence of firms' innovative scale on inventor productivity at different career stages.

Using novel German administrative employer-employee data linked with European Patent Office records, I find that inventors beginning their careers at firms in the 75th–95th percentile of innovative scale are 40% more productive than those starting at non-patenting firms. Productivity differentials rise to 60% and 90% for those starting at firms in the 95th and 99th percentiles, respectively. Moreover, inventors moving from low-scale to high-scale innovative firms increase their patent output by as much as 88% relative to those spending their early careers in low-scale firms. Using geographic constraints of German apprentices resulting in quasi-random variation in the access to large-scale innovative firms, I show that better access to these firms results in 20% higher early-career patent output, suggesting that large-scale innovative firms causally *affect* inventor productivity. I further find that seasoned inventors moving to large-scale innovative firms produce more patents than those joining lower-scale firms do, with evidence pointing to financing constraints as a potential reason small firms struggle to fund R&D effectively.

These findings may have important policy implications. Large-scale innovative firms are often scrutinized for using their size and market power to limit competition, which could hinder innovation. As a result, policies like blocking mergers or breaking up large firms are frequently debated. However, it may be this very scale that provides individual inventors with access to resources that boost their productivity. Additionally, the findings suggest that policies enhancing access to financing for smaller firms could support individual-level innovation and, in turn, foster economic growth.

Chapter 2

Taming the Sharing Economy: Short-Term Rental Regulation and Local Economic Prosperity

Abstract

This paper studies the economic effects of short-term rental (STR) regulations. Using a staggered difference-in-differences framework, I compare U.S. counties introducing STR restrictions to similar control counties between 2010 and 2020. I find limited or no effects of the regulations on local housing prices or tax revenue. However, I do find suggestive evidence of increased GDP and personal income per capita in the accommodation and food sector, potentially driven by a reallocation of economic activity away from the informal hosting actvities towards the traditional hotel industry. These findings suggest that while STR platforms are politically contentious, regulating them has only minor aggregate economic consequences.

2.1 Introduction

Technological innovation is a central engine of economic growth. New technologies, however, often present substantial trade-offs. They can expand consumer choice, lower costs, and create income opportunities, but they may also impose concentrated harms—displacing workers, straining communities, or, in extreme cases, posing existential risks. This tension makes the regulation of emerging technologies increasingly urgent: how can policymakers balance innovation's promise against its potential social costs?

This paper contributes to this broader debate by analyzing the economic effects of regulatory responses to one prominent technological development: the rise of short-term rental (STR) platforms such as Airbnb and HomeAway. Between 2010 and 2017, numerous U.S. counties and municipalities adopted restrictions on STRs in response to a range of concerns. Advocates of these platforms argue that they enhance consumer welfare by making travel more flexible, affordable, and personalized, while providing homeowners with valuable supplementary income[1]. Critics, however, highlight reduced housing affordability from increased investor demand, neighborhood disruption, safety and tax compliance issues, and unfair competition with traditional lodging providers[2].

Using a staggered difference-in-differences approach, I evaluate how STR restrictions affect key local economic outcomes—housing prices, personal GDP per capita, income per capita, employment, and county-level tax revenue per capita. I construct a sample that matches treated counties to comparable control counties based on pre-treatment economic characteristics.

I show that these regulations are associated with up to 13% higher GDP per capita in the accommodation and food sector and 8% higher personal income per capita in food and drink services. Simultaneously, the employment share in these sectors decreases by about 12.5%, consistent with a shift from lower-productivity private lodging toward higher-productivity

[1] For example, see New York Times (2016).
[2] For example, see Wired (2019).

professional hotels. I find no or only very limited statistically significant effects on GDP or personal income in construction, real estate and leasing, or arts and entertainment, nor any impact on housing prices relative to control counties.

Identifying causal effects of STR regulations is empirically challenging, as regulatory adoption may correlate with underlying economic trajectories. The local political economy shapes both the likelihood of regulation and economic outcomes: densely populated areas face greater housing affordability pressures and more frequent complaints about noise, parking, and safety issues from STRs, while wealthier communities may be more or less tolerant of STR activity depending on whether residents primarily own investment properties that generate STR income or live in owner-occupied homes affected by nearby rentals. Similarly, local labor market conditions—particularly unemployment rates and educational composition—influence political coalitions that either support regulation to preserve traditional hospitality employment or oppose it to maintain income diversification opportunities. These selection dynamics potentially confound the relationship between regulation and economic outcomes.

To mitigate these endogeneity concerns, I implement a matched difference-in-differences design, using the recently developed staggered adoption estimator by Callaway and Sant'Anna (2021), and pair treated counties with observationally similar controls within the same state. The matching procedure uses personal income per capita, unemployment rates, population density, and college graduation shares. Additionally, I also include these covariates as controls. Event study specifications further validate the design by testing for pre-trends and by revealing dynamic treatment effects. While this approach cannot eliminate all endogeneity, it improves comparability and helps isolate regulatory effects under the assumption that selection is primarily driven by observables.

The DiD results offer insight into how local economies evolve following STR regulation. Before interpreting these effects, however, it is crucial to establish that regulation actually constrained short-term rental supply in the counties I study. Although platforms such as

Airbnb and Vrbo do not provide historical listings data, strong evidence—including academic research using proprietary data[3], as well as newspaper coverage and local government reports[4]—shows that regulations were enforced and that they significantly reduced STR supply in these jurisdictions, often provoking litigation between platforms, hosts, and public authorities.

I first analyse effects on the real estate market. Theoretically, the effect on house prices is ambiguous. Restrictions may reduce the income-generating potential of residential property—for example, by limiting the duration, frequency, or zoning of STR activity. Conversely, in communities where STRs generate negative spillovers such as noise or overcrowding, regulations may improve neighborhood amenities and support property values. Consistent with this ambiguity, I find no significant changes in housing prices following the restrictions across most property types, though there is some suggestive evidence of a modest 5% decline for two-bedroom homes that could reflect reduced investor demand for properties commonly used as STRs. Overall, these results suggest either offsetting effects that cancel each other out, or that STR penetration was insufficient to substantially influence house prices beyond specific market segments.

Next, I analyse GDP, personal income and labor shares in sectors central to the STR debate: accommodation and food services (hotels and restaurants), arts and entertainment (theaters, amusement venues), construction, and real estate, rental, and leasing.

As a first-order effect, restricting STRs raises the cost of supplying short-term rentals, which likely reduces private hosting activity. The net impact on the accommodation and food sector is uncertain. Visitors may substitute toward traditional hotels, leaving aggregate GDP per capita unchanged or even higher if hotels expand capacity, offer higher value-added services or simply charge more. Alternatively, if travelers view hotels as poor substitutes for STRs, they may divert to other destinations, lowering local activity and output. Labor

[3] For example, van Holm (2020); Yeon et al. (2020); Valentin (2021); Koster et al. (2021); Chen et al. (2022); Bekkerman et al. (2023); Jin et al. (2024)

[4] For example, Bloomberg (2024); Miami Herald (2019); Tennessean (2019); CPR News (2017).

market impacts are similarly ambiguous: reduced tourism would directly lower employment, but even if hotels serve as effective substitutes, a shift toward more capital-intensive, higher-productivity establishments could reduce labor demand while raising output per worker.

I show that GDP per capita in accommodation and food services increases by 13.3% in the five years following regulation, while personal income per capita in food and drink services rises by 8.1% in my preferred specification with detailed county-level controls. Meanwhile, the accommodation and food services employment share declines by approximately 13% relative to control counties.

This pattern of increased GDP and personal income per capita alongside declining employment shares in the accommodation sector—suggests a structural transformation in local hospitality markets following STR regulation. Hotels emerge as effective substitutes for STRs, capturing displaced demand while generating higher revenue per capita. This increase could reflect two possible mechanisms: superior service provision that commands price premiums, or market power resulting from the newly constrained accommodation market. Personal income rises by 11% in the accommodation sector (though not statistically significant) and 8.1% in food services (statistically significant), consistent with visitor spending shifting toward traditional hospitality clusters.

The concurrent decline in accommodation employment shares likely reflects compositional changes rather than sector contraction. First, STR regulations particularly affected full-time hosts who had purchased secondary properties for short-term rental purposes, as many jurisdictions restricted hosting to primary residences. These formerly self-employed accommodation sector workers[5] may have transitioned to long-term rental models, reclassifying their employment from accommodation to real estate, or may have sought primary employment elsewhere, no longer relying on property-based income as their main occupation. Hotels, possibly operating with existing capacity slack, could have absorbed the redirected

[5] The BEA data, I use, is based on adjusted Bureau of Labor Statistics (BLS) data that captures both payroll employees as well as the self-employed. In the BLS Household Survey (CPS) receiving income from short-term renting as one's main job would count as being self-employed in the accommodation industry, BLS CPS (2025).

demand without proportional employment expansion.

Second, the employment share metric captures relative rather than absolute changes. The spatial concentration of hotels in commercial districts may generate spillover effects absent in residential STR locations. As visitor spending shifts from dispersed residential areas to hotel districts, complementary sectors[6]—particularly retail—could experience accelerated employment growth to meet increased demand. This reallocation effect, whereby STR regulation stimulates employment in hotel-adjacent businesses more than in hotels themselves, would mechanically reduce accommodation's employment share even if the sector's absolute employment remains stable or grows modestly. The result appears to be a more productive but *relatively* smaller accommodation sector embedded within a broader hospitality ecosystem that may be experiencing employment gains.

Turning to the real estate sector, limiting short-term rentals can shift existing housing supply from visitors toward long-term residents, potentially influencing returns in the real estate and leasing sector. On the other hand, such restrictions may also alter incentives to build new housing stock, for example, they may discourage investment by reducing expected rental yields.

The empirical evidence reveals no robust effects in either the real estate or the construction sector. In construction, point estimates are positive and economically meaningful—GDP per capita is about 25% higher and personal income about 8.3% higher in treated counties—but the estimates are imprecise and not statistically significant. In the real estate and leasing sector, impacts on both GDP and personal income are economically small and statistically insignificant. Employment shares likewise show no systematic change in either sector.

These results suggest that while construction activity may exhibit short-lived or localized responses, restrictions do not generate consistent, measurable effects on broader real estate or construction outcomes.

[6]BEA data suggests that the travel and tourism industry is a significant driver of local shopping activities, BEA U.S. Travel and Tourism Sattelite Accounts (2025).

Other tourism-related industries, such as arts, entertainment, and recreation, could also be affected if changes in lodging options influence local visitor flows. A decline in STR activity might reduce demand for cultural and recreational services, whereas substitution toward hotels could sustain or even increase such demand.

The estimates show large, statistically significant reductions in personal income in this sector following STR regulations. However, pre-treatment personal income per capita grows much faster in the regulated counties than in the control counties. This calls into question the validity of the parallel trends assumption in the post-treatment period and suggests that control counties might not be a credible counterfactual for treated counties in this sector. Sectoral GDP and employment shares remain virtually unchanged.

On balance, the evidence does not support the conclusion that restrictions materially affected the arts and entertainment sector. The apparent income reductions are more plausibly explained by differential trends between treated and control counties rather than by the regulations themselves.

Beyond output, income, and employment, regulatory interventions may also carry fiscal consequences. Policymakers often worry that restrictions could erode county tax revenues, particularly if they reduce property values and thus the property tax base.

Event-study estimates reveal sharp but short-lived effects on property tax revenues. Tax revenue declines of about 23.4% and 18% appear in the first and second year after implementation, respectively, both weakly significant. If taken at face value, this pattern could align with institutional features of property taxation in ”reset-on-sale” states like California, where tax assessments update only upon transaction. The initial decline may reflect reduced property turnover as markets adjust to regulatory change—buyers and sellers pause to understand new rules and price implications—while the subsequent recovery (with revenues increasing 17-27% in years 4-5) captures both accumulated appreciation and resumed transaction volumes at higher prices.

However, these results should be interpreted with extreme caution as borderline significant pre-treatment effects indicate much faster growth of property tax revenue in treated counties suggesting potential violations of parallel trends. The observed revenue declines may simply be a reversal of a particularly profitable period rather than reflect true regulatory impacts. Over a five-year horizon, no statistically significant cumulative reduction in revenues emerges relative to control counties.

These patterns suggest that while restrictions may create substantial fiscal pressures in the immediate aftermath of adoption—whether due to actual market adjustments or reversal of pre-existing trends—such effects do not persist, leaving longer-term county revenues broadly comparable to those in non-regulated areas.

Related Literature. This paper contributes to two connected strands of the literature: the economic evaluation of regulatory interventions, particularly in innovative industries, and the local economic effects of peer-to-peer sharing platforms. The intersection of these literatures is central for understanding how regulation of emerging technologies shapes local economic outcomes.

The regulation and innovation literature shows that regulation can meaningfully affect economic activity, dampening innovation intensity and inducing geographic sorting as firms relocate to avoid regulatory burdens (Aghion et al. 2023; Lerner et al. 2024). However, the main empirical difficulty is constructing credible counterfactuals: regulatory changes are rarely randomized, and resulting estimates of costs and benefits depend strongly on measurement and identification choices (Hazilla and Kopp 1990; Hahn and Hird 1991; Hahn 1998; Bombardini et al. 2025).

The regulation of P2P platforms is not exceptional in this regard: it raises the familiar problem of separating regulatory effects from contemporaneous local shocks. What is distinctive is the economic environment being regulated. By lowering entry barriers, P2P platforms such as Airbnb and Uber enable flexible participation at a scale that traditional intermediaries cannot match (Einav et al. 2016; Filippas et al. 2020). This flexibility benefits a subset of consumers and asset providers, but it also allows resources to reallocate across interconnected markets, redistributing surplus in ways that can affect incumbent firms, workers, and local neighborhoods. Specifically, because participation is tied to access to specific assets (a dwelling for Airbnb, a car for Uber), these platforms broaden earning opportunities for some while excluding those without such assets (Koustas 2019; Buchak 2024). Motivated by these features, this paper employs a matched difference-in-differences design across multiple counties and outcomes to estimate both intended and unintended consequences of STR regulation within a consistent empirical framework.

Short-term rental platforms represent a particularly salient case for studying regulatory trade-offs in innovative markets. The rise of platforms like Airbnb has generated intense debate about their local economic effects, particularly regarding housing affordability and competition with traditional hospitality. A substantial literature documents how STRs reallocate housing from long-term residents to short-term visitors, with neighborhood-level studies finding significant rent increases associated with STR expansion (Horn and Merante 2017; Barron et al. 2021; Calder-Wang 2021). Regulatory interventions produce mixed results: while restrictions can restore housing affordability by reducing STR supply by up to 50% and lowering rents by 2-4% (Koster et al. 2021; Chen et al. 2022), they also impose costs. Bibler et al. (2022) document a 40% increase in foreclosures following STR enforcement as income-dependent hosts lose a financial lifeline, while Valentin (2021) finds 30% property value declines in regulated areas. Moreover, Bekkerman et al. (2023) show that STR restrictions reduce new residential construction permits by 9%, suggesting dynamic effects on housing supply. In contrast to these studies focusing on extremely localized effects, I find no significant association between STR regulations and housing prices at the county level. This divergence may reflect both offsetting heterogeneous effects—reduced investor demand lowering prices in some areas while restored neighborhood quality (less noise, reduced turnover of strangers, fewer parking conflicts) raises prices in others—and measurement differences, as prior work leverages proprietary zip-code or neighborhood-level data capturing granular variation that my county-level analysis aggregates.

Beyond housing markets, STRs fundamentally alter competition in the hospitality sector. Evidence points to substantial substitution between STRs and hotels, particularly at the lower end of the market. While this competition expands accommodation options and improves affordability for visitors, existing evidence suggests it constrains hotels' pricing power, with estimated revenue reductions of 1.6-2.8% and budget hotels experiencing the largest losses (Li and Srinivasan 2019; Yeon et al. 2020; Farronato and Fradkin 2022). Studies of regulatory enforcement find that STR listing reductions (ranging from 16-30% in affected areas) are associated with increased hotel revenues, especially in dense urban areas (Jin et al. 2024). Yet STRs may also expand the market: Basuroy et al. (2022) find positive spillovers to restaurants in residential neighborhoods, suggesting STRs create new tourism demand rather than purely redistributing existing flows. My analysis reveals a previously undocumented productivity dimension to this reallocation: STR regulations increase accommodation sector GDP per capita by 13% while reducing sectoral employment shares by 12%, suggesting a shift from labor-intensive informal hosting to capital-intensive professional hotels. This finding indicates that the competition between STRs and hotels involves not just market share redistribution but fundamental differences in production technology.

While existing work has generated valuable evidence on particular channels of STR regulation, it is difficult to translate these findings into broader policy guidance. First, studies use heterogeneous empirical designs and levels of aggregation—ranging from neighborhood housing panels to city-level synthetic controls and single-jurisdiction datasets—so estimated magnitudes are not straightforwardly comparable across outcomes. Second, many papers focus on jurisdictions where a given outcome is most salient (e.g., high-pressure housing markets or tourism-intensive cities), which is appropriate for identifying mechanisms but leaves open how effects generalize to the broader set of places considering regulation. Third, because outcomes are typically studied in separate samples using different methods, the literature provides limited evidence on how housing, hospitality, employment, and fiscal channels trade off within the same policy setting.

This paper addresses these gaps by estimating multiple sectoral outcomes in a common set of U.S. counties using a unified difference-in-differences design with matched comparison counties. Relative to prior work, the paper studies a markedly broader set of regulating counties, which helps move beyond single-city or highly selected samples. By tracking staggered adoption of STR restrictions over a decade and applying the same specification across outcomes, the analysis produces effect estimates that are directly comparable in magnitude and timing. This framework also helps clarify the channels through which regulation operates: rather than treating "local economic impact" as a single object, it allows housing-market responses, hospitality-sector activity, employment composition, and local public finance to be evaluated side by side within the same treated sample. For example, estimating housing and hospitality outcomes in the same counties and specifications makes it possible to compare sectoral responses on a consistent basis; in my setting, housing prices are largely unchanged while accommodation and food services activity increases, a contrast that is hard to infer from studies that examine these channels separately in different places.

2.2 Data

The empirical analysis combines data on short-term rental (STR) regulations with county-level economic outcomes and demographic characteristics for the United States between 2010 and 2020. This section describes the data sources and the construction of key variables.

Short-Term Rental Regulation

To identify counties that introduced STR restrictions, I manually compile data from publicly available sources, including municipal code ordinances, local government websites, and newspaper reports. I include all counties for which I could verify the implementation of an STR regulation between 2010 and 2020 and that had available data for the outcome variables of interest. Many counties contain several municipalities. I classify a county as regulated if

STR restrictions apply countywide or to its largest municipality/municipalities. The starting point of 2010 reflects the timing of Airbnb's initial national expansion. Although Airbnb was founded in 2008, the platform gained broader adoption and visibility only after 2010. The sample period ends in 2020 to avoid confounding effects from the COVID-19 pandemic, which disrupted the travel and accommodation sectors beginning in early 2020. Table B.1 lists the counties that adopted STR regulations and the year of introduction.

Housing Prices

Data on housing prices come from Zillow's Home Value Index (ZHVI), a county-level measure of typical home values by bedroom count (Table B.2). The ZHVI reflects the estimated value for homes in the 35th–65th percentile of the local price distribution, capturing broad market trends while limiting the influence of outliers. I use separate ZHVI measures for two-, three-, and five-bedroom homes to test whether STR regulations have heterogeneous effects across property sizes.

County-Level GDP and Personal Income

Information on county-level gross domestic product (GDP) and personal income is sourced from the U.S. Bureau of Economic Analysis (BEA) (Table B.2). The BEA reports both aggregate and sector-specific economic activity on an annual basis. I use these data to construct total GDP per capita and personal income per capita, as well as GDP and income measures for the accommodation and food services, real estate and rental, arts and entertainment, and construction sectors.

Sectoral Employment

I obtain annual employment data from the BEA, which includes employment by sector for all U.S. counties (Table B.2). Sectoral employment share is defined as the number of employees in a given sector divided by total county employment. This variable is used to analyze whether STR regulation affects the composition of local employment across industries.

Property Tax Revenue

Annual data on county-level property tax revenue are obtained from the U.S. Census Bureau (Table B.2). These data are used to test whether STR regulation has short-term fiscal consequences for local governments through effects on housing prices.

Socioeconomic Control Variables

Several demographic and economic control variables are used in the construction of the matched sample and in the estimation of treatment effects (Table B.2). The share of the county population with at least a bachelor's degree is obtained from the USDA Economic Research Service. County-level unemployment rates are sourced from the U.S. Bureau of Labor Statistics. Population counts are taken from the BEA, and land area measures are obtained from the U.S. Census Bureau. These variables are included to account for differences in local economic conditions, education levels, and urban density.

2.3 Overview of STR Regulations

Short-term rental (STR) regulations have emerged as a common policy response to the rapid growth of platforms such as Airbnb and HomeAway. These platforms enable homeowners and tenants to rent out properties or individual rooms on a short-term basis, often operating outside the scope of traditional hospitality, tax, and zoning regulations. In response, many local governments have implemented laws intended to balance the perceived economic benefits of STR activity—such as increased tourism and supplemental income for residents—against its potential downsides, including housing market distortions, neighborhood disruption, and uneven regulatory treatment relative to hotels.

While the precise design of STR regulations varies across jurisdictions, a broadly consistent set of regulatory features has emerged. Most commonly, localities impose licensing or registration requirements for STR operators (e.g., San Francisco, Santa Monica, Multnomah, Denver, Travis, Riverside), often paired with obligations related to safety compliance, tax registration, or reporting standards (e.g., Orange, Clark, Davidson, Cook County). A number of jurisdictions further restrict STR activity to a host's primary residence (e.g., Denver, Multnomah, Colorado), seeking to distinguish casual home-sharing from commercial rental activity.

Limits on the intensity or scope of STR use also appear frequently. These include annual caps on rental days (e.g., San Francisco), density restrictions within geographic units such as census tracts (e.g., Travis, Davidson), and zoning-based exclusions (e.g., Miami Beach, Santa Barbara, Sonoma). In several counties, regulations target the structure of stays themselves, imposing minimum stay requirements (e.g., Orange, Teton), occupancy limits (e.g., Riverside, Sarasota), or prohibitions on certain rental formats such as entire-home rentals (e.g., Santa Monica, King County).

Many jurisdictions have also sought to extend or adapt local tax regimes to STR activity, either by applying existing hotel taxes (e.g., Colorado, Cook County) or imposing new per-night surcharges (e.g., King County). In some cases, the regulatory process has included legal or political challenges, particularly where the boundaries between residential and commercial use have been contested (e.g., Santa Barbara).

Taken together, while STR laws differ in technical implementation, they share a common policy logic: to integrate STR activity into existing legal and fiscal frameworks, mitigate its externalities, and rebalance the terms of competition between private STR hosts and the traditional lodging industry.

2.4 Empirical Strategy

I estimate the effects of short-term rental regulations on local economic outcomes by comparing counties that introduce STR regulations with observationally similar control counties that have not yet adopted (or never adopt) such regulations, using the staggered difference-in-differences framework of Callaway and Sant'Anna (2021). This approach avoids the well-documented biases that arise in two-way fixed effects estimators when treatment timing varies and effects are heterogeneous across cohorts or time periods (de Chaisemartin and D'Haultfœuille 2020; Goodman-Bacon 2021). The estimator first computes group-time average treatment effects—the average effect for units treated in period g at time t—then aggregates these into interpretable parameters. Results are shown in two ways: (i) an event study that reports the full dynamic profile, including pre-treatment coefficients for parallel-trends diagnostics; and (ii) period-aggregated effects that average impacts over the pre- and post-treatment windows.

The identification of causal effects requires that the timing of STR regulation adoption is unrelated to potential outcome trends. While recent literature treats STR regulation as plausibly exogenous (Bekkerman et al. 2023), political economy considerations suggest systematic selection into treatment. Counties with higher population density may face different political pressures regarding housing availability and neighborhood externalities from STRs. Wealthier counties, as measured by per capita income, may have different tolerance for STR-related disruptions and different compositions of property owners who benefit from STR income. Counties with lower educational attainment and higher unemployment may have larger shares of workers dependent on traditional hospitality employment, creating distinct political coalitions around STR regulation that could either favor restriction (to protect hotel jobs) or expansion (to create alternative income opportunities).

To address these selection concerns, I condition the parallel trends assumption on four county characteristics measured five years before treatment: population density, per capita personal income, college graduation rates, and unemployment rates. These pre-treatment covariates capture fundamental county characteristics that drive both political economy decisions and economic trajectories. Following Callaway and Sant'Anna (2021), measuring these covariates before treatment avoids conditioning on outcomes potentially affected by regulation or anticipation thereof.

This empirical approach has several advantages. First, it handles treatment timing appropriately. The staggered design explicitly accounts for treatment timing variation (2010-2020), which can lead to biases in traditional two-way fixed effects estimators. The Callaway and Sant'Anna (2021) estimator ensures that each treated cohort is compared only to appropriate controls (not-yet-treated or never-treated), avoiding contamination from already-treated units that would bias estimates in standard difference-in-differences specifications.

Second, the matching procedure systematically addresses selection on observables. By matching on pre-treatment characteristics (population density, income, unemployment, education) and requiring that control counties fall within the same quintile of the outcome variable distribution five years before treatment, I ensure that treated and control counties are comparable on dimensions that drive both treatment adoption and economic outcomes. The outcome quintile requirement is particularly important: it ensures that counties are similar not just in their demographic and economic characteristics, but also in the baseline level of the specific outcome being analyzed (e.g., housing prices, sectoral GDP). This dual matching criterion—on covariates and outcome levels—substantially reduces model dependence and ensures comparisons occur only within regions where treated and control counties have overlapping distributions.

Third, it focuses on the policy-relevant margin. My design answers the question policymakers typically face: "What happens when we introduce a binding STR regulation versus not regulating?" This is arguably more policy-relevant than, for example, exploiting variation in policy intensity across already-regulated jurisdictions, which conflates the extensive margin (whether to regulate) with the intensive margin (how strictly to regulate conditional on regulating). Notably, this approach avoids the difficulties of other popular empirical strategies such as comparing contiguous counties or exploiting differences in regulation intensity and scope.

Contiguous Counties. A natural alternative would be to match treated counties to their geographic neighbors, exploiting the intuition that contiguous counties share similar economic conditions and are subject to common shocks. However, this approach faces several important challenges in my setting.

First, spillovers and contamination of controls. When a large, tourism-intensive county tightens STR rules, nearby jurisdictions become a natural margin of adjustment for both hosts and guests. STR activity and hotel demand can spill over into immediately neighboring counties. Using contiguous counties as controls would thus likely violate the Stable Unit Treatment Value Assumption (SUTVA), biasing treatment effects toward zero by contaminating the control group with treatment spillovers. By matching treated counties to observationally similar controls within the same state but not necessarily on borders, I deliberately trade off geographic proximity for a lower risk of direct spillovers.

Second, comparability and external validity. For many treated counties in my sample, contiguous counties are either (a) economically very different, or (b) themselves treated around similar dates. Contiguous county comparisons rest on the assumption that geographic proximity ensures comparability. Yet U.S. counties are large administrative units with substantial heterogeneity even across borders. Consider several illustrative examples:

- San Francisco County (regulated, 2015) and San Mateo County (unregulated): Despite sharing a border, San Francisco is an ultra-dense urban core (18,000+ people per square mile) with a tourism-driven economy, while San Mateo includes substantial suburban and semi-rural areas with fundamentally different housing markets and economic structures.

- Miami-Dade County (regulated, 2010) and Broward County (unregulated until later): Though contiguous, these counties differ markedly in tourism intensity, housing stock composition, and the role of STR platforms in their economies.

In contrast to settings where contiguous regions share common labor markets, infrastructure, or economic shocks (e.g., adjoining European regions or U.S. commuting zones), county borders in the U.S. often demarcate meaningful economic and demographic discontinuities. Restricting to contiguous pairs would therefore substantially shrink the sample and often worsen pre-treatment covariate balance relative to my matching procedure.

Third, treatment contamination in the control group. Many contiguous counties adopted their own STR regulations at similar times, making them invalid controls. A design requiring untreated geographic neighbors would either severely restrict the sample or force the inclusion of counties that themselves received treatment shortly after their neighbors, further biasing estimates.

Differences in Regulatory Intensity. Another alternative would be to exploit cross-county variation in the "stringency" of STR regulations—for instance, comparing counties with strict day caps to those with lenient ones, or counties with primary-residence requirements to those without. While such variation is potentially informative about intensive-margin effects, this approach faces substantial challenges in my setting.

First, my main research question concerns the average effect of introducing a binding STR regulation on local outcomes, rather than the effect of moving from a "weaker" to a "stronger" regulatory regime. The binary treatment margin—whether to regulate or not—is the fundamental policy decision most jurisdictions face. Understanding intensive-margin effects (how much to regulate, conditional on regulating) is an important downstream question, but it is conceptually distinct from the extensive-margin question my design addresses.

Second, STR regulations are highly multidimensional. In my setting, regulations encompass registration requirements, primary-residence rules, night caps, zoning restrictions, occupancy limits, tax provisions, and more (Section 2.3). There is no natural cardinal ranking of "stringency" across these dimensions, and different combinations of provisions are likely to matter differently in different markets. Any single intensity index would be necessarily ad hoc. Moreover, we lack comparable data on enforcement intensity over time, which is an important dimension of regulatory bite. A design based on such a noisy intensity measure would potentially introduce substantial measurement error.

Third, intensity and scope present additional endogeneity challenges beyond those already present in the binary adoption decision. While both the decision to regulate and the stringency of regulation reflect underlying political economy forces, there is an important distinction. The timing of initial regulation adoption is plausibly more predictable from pre-treatment observables—population density, income, unemployment, and education capture the fundamental county characteristics that make regulation politically salient and economically consequential. In contrast, how strictly a jurisdiction regulates conditional on deciding to regulate depends on additional, harder-to-observe factors: the specific coalition of interest groups that prevailed in local debates, idiosyncratic features of the regulatory process, enforcement capacity constraints, and negotiated compromises between competing stakeholders.

For example, two counties with identical pre-treatment characteristics might both adopt STR regulations, but one implements a 90-day annual cap while the other implements a 30-day cap plus primary-residence requirements plus registration fees. These differences likely reflect bargaining dynamics, the relative strength of hotel lobbies versus homeowner groups, or administrative capacity—factors that are difficult to proxy with standard economic and demographic controls.

Conditioning on observed covariates and treating variation in "how strict" a county regulates as quasi-random therefore requires stronger assumptions than treating the timing of first adoption as conditionally random. My matched staggered DiD approach focuses on the margin where selection is most transparently related to observables, explicitly addressing endogeneity through (i) matching treated counties to comparable controls within the same state and pre-regulation outcome quintile, and (ii) conditioning parallel trends on population density, income, unemployment, and education measured before treatment.

2.4.1 Econometric Framework

For my analysis and the notation in this section, I lean heavily on Callaway and Sant'Anna (2021) and on Baker et al. (2025). Let i index counties, t years and g the time STR regulations are first introduced for a given county. Denote by $Y_{it}(g)$ the (potential) outcome of interest (e.g., log house prices, sectoral GDP, or employment shares). G_i denotes a county's treatment date, with $G_i = \infty$ if it never adopts any STR regulations and $\mathcal{G}$ represents the set of treatment times. $Y_{it}(\infty)$ is the outcome for county i at time t if it were never regulated, and $Y_{it}(g)$ is the outcome if it first became regulated at time g. The relationship between potential and observed outcome satisfies

$$Y_{it} \;=\; \sum_{g \in \mathcal{G}} Y_{it}(g)\, \mathbf{1}\{G_i = g\}. \tag{2.1}$$

The parameter of interest and building block for my analysis, $ATT(g,t)$, is the group-time average treatment effect, at time t, of introducing STR regulations at period g relative to never introducing them, among counties that introduced regulations in period g, i.e,

$$ATT(g,t) = \mathbb{E}[\, Y_{it}(g) - Y_{it}(\infty) \mid G_i = g\,]. \tag{2.2}$$

The challenge is that $Y_{it}(\infty)$ is not observed. Under very strong strong assumptions (e.g., unconditional parallel trends, homogeneous treatment effects) simple two-way fixed effects regressions can produce valid estimates for $ATT(g,t)$. However, if these strong assumptions are violated the resulting estimates become biased.

My analysis relies on significantly less demanding identifying conditions, which does complicate estimation, however. Specifically, I require the following identifying assumptions (see Baker et al. 2025):

(i) No Anticipation. For all counties i that are eventually regulated and all pre-regulation periods t,

$$Y_{i,t}(g) = Y_{i,t}(\infty). \tag{2.3}$$

(ii) Conditional Parallel Trends. For every eventually regulated county group g, not-yet-regulated group g', time periods t such that $t \geq g$ and $g' > t$, and every covariate value X_i,

$$\mathbb{E}_\omega[\, Y_{i,t}(\infty) - Y_{i,t-1}(\infty) \,|\, G_i = g,\, X_i] = \mathbb{E}_\omega[\, Y_{i,t}(\infty) - Y_{i,t-1}(\infty) \,|\, G_i = g',\, X_i]\,. \tag{2.4}$$

(iii) Strong Overlap. For every group $g \in \mathcal{G}$, the conditional (weighted) probability of belonging to a regulation group g, given observed covariates X_i that are determinants of untreated potential outcome growth, is uniformly bounded away from zero and one. That is, for some $\epsilon > 0$ and for every group $g \in \mathcal{G}$,

$$\epsilon \;<\; \mathbb{P}_\omega\big[\, G_i = g \,\big|\, X_i \,\big] \;<\; 1 - \epsilon. \tag{2.5}$$

With these assumptions, Callaway and Sant'Anna (2021) show that the post-treatment $ATT(g,t)$ is identified by the doubly-robust estimand:

$$\mathrm{ATT}_{\mathrm{dr}}(g,t) = \mathbb{E}\Big[\big(w_{\omega,\,G=g}(G_i) - w_{\omega,\,g,t}(G_i, X_i)\big)\big(Y_{i,t} - Y_{i,t=g-1} - \mathbb{E}_\omega[\, Y_{i,t} - Y_{i,t=g-1} \mid X_i,\, G_i > t\,]\big)\Big], \tag{2.6}$$

where $\big(w_{\omega,\,G=g}(G_i)$ and $w_{\omega,\,g,t}(G_i, X_i)\big)$ are defined as

$$w_{\omega,\,G=g}(G) = \frac{\omega\,\mathbf{1}\{G = g\}}{\mathbb{E}[\omega\,\mathbf{1}\{G = g\}]},$$

$$w_{\omega,\,g,t}(G, X) = \frac{\omega\,\mathbf{1}\{G > t\}\mathbf{1}\{G \neq g\}\,p_{\omega,g,t}(X)}{1 - p_{\omega,g,t}(X)} \bigg/ \mathbb{E}\left[\frac{\omega\,\mathbf{1}\{G > t\}\mathbf{1}\{G \neq g\}\,p_{\omega,g,t}(X)}{1 - p_{\omega,g,t}(X)}\right],$$

and

$$p_{\omega,g,t}(X) = \mathbb{E}_\omega[\mathbf{1}\{G_i = g\} \,|\, X,\, \mathbf{1}\{G_i > t\} = 1]$$

denote the (weighted) probability of belonging to group g given covariates X and that the county belongs either to group g—the regulated group for ATT(g,t)—or to the not-yet-regulated group $G_i > t$ (which includes never regulated counties)—the comparison group.

The doubly-robust method is used because we do not observe the counterfactual outcome, $Y_{it}(\infty)$, in Equation (2.2) for treated counties. Callaway and Sant'Anna (2021) therefore propose two methods, the regression adjustment (RA) approach and the inverse probability weighted (IPW) approach to recover ATT(g,t).

The idea behind the regression adjustment approach is to use covariates to predict outcome trends. Specifically, it runs a regression of the change in the outcome variable on covariates only among untreated units. The resulting fitted model generates predicted values for all units in the sample, including treated units. These fitted values can then be used to produce an estimate for the unobserved counterfactuals.

Alternatively, one can use covariates not to predict outcome trends but to predict the conditional probability of being treated. This implies re-weighing observed changes in the outcome variable for the non-treated groups to ensure that treated and non-treated groups are similar on covariates. The doubly-robust method combines these two approaches and leads to the following estimator:

$$\widehat{\text{ATT}}_{\text{dr}}(g,t) = \frac{1}{n}\sum_{i=1}^{n}\Big(\widehat{w}_{\omega,\,G=g}(G_i) - \widehat{w}_{\omega,g,t}(G_i, X_i)\Big)\Big(Y_{i,t} - Y_{i,t=g-1} - \widehat{\mu}_{\omega,\,\Delta,\,G_i>t}(X_i)\Big). \quad (2.7)$$

where,

$$\widehat{w}_{\omega,\,G=g}(G) = \frac{\omega\,\mathbf{1}\{G=g\}}{\dfrac{1}{n}\displaystyle\sum_{i=1}^{n}\omega_i\mathbf{1}\{G_i=g\}},$$

$$\widehat{w}_{\omega,g,t}(G,X) = \frac{\omega\,\mathbf{1}\{G>t\}\mathbf{1}\{G\neq g\}\,\widehat{\pi}_{\omega,g,t}(X)}{1-\widehat{\pi}_{\omega,g,t}(X)} \Bigg/ \frac{1}{n}\sum_{i=1}^{n}\frac{\omega_i\mathbf{1}\{G_i>t\}\mathbf{1}\{G_i\neq g\}\,\widehat{\pi}_{\omega,g,t}(X_i)}{1-\widehat{\pi}_{\omega,g,t}(X_i)}. \tag{2.8}$$

The term $\widehat{\mu}_{\omega,\,\Delta,\,G_i>t}(X_i)$ is an estimate of the counterfactual for regulated counties, $\mathbb{E}_{\omega}[\,Y_{i,t} - Y_{i,t=g-1} \mid X_i,\, G_i > t\,]$, and is a function that relates average outcome trends for unregulated counties to their covariates. Consistent with prior literature, I use a linear model $\mu_{\omega,\,\Delta,\,G_i>t}(X_i) = X_i'\beta_{G_i>t}$ with parameters that are derived from a regression of $(Y_{i,t} - Y_{i,t=g-1})$ on X_i using only the subset of unregulated counties. The fitted model, $\widehat{\mu}_{\omega,\,\Delta,\,G_i>t}(X_i)$, produces predicted values for the entire sample, which includes regulated counties.

The IPW-weight, $\widehat{w}_{\omega,g,t}(G,X)$, depend on $\widehat{\pi}_{\omega,g,t}(X)$ which estimates $p_{\omega,g,t}(X)$, the weighted probability of being part of the regulated counties, i.e. the propensity score of being treated. The IPW-weights shift the weighted distributions of covariates of the control counties to match the distribution of the regulated counties (see Rosenbaum and Rubin 1983; Baker et al. 2025). Following Baker et al. (2025), I choose a logistic model for the propensity score estimation and and use the logit coefficients to obtain fitted values for all observations, i.e, to obtain $\widehat{\pi}_{\omega,g,t}(X)$.

Finally, once I estimate the ATTs for different treatment cohorts and years, I aggregate them by relative event time to produce event study estimates. In a second step, as proposed by Baker et al. (2025), I take a simple average of all available post-treatment event times and report an overall ATT measure.

Specifically, the causal parameter summarising treatment effect dynamics in terms of event time can be written as:

$$\mathrm{ATT}_{\mathrm{es}}(e) = \mathbb{E}_{\omega}[\,\mathrm{ATT}(G,\, G+e)\,|\, G+e \in [1,T],\, G \leq T] = \sum_{g<\infty} w^{\mathrm{es}}_{\omega,g,e}\, \mathrm{ATT}(g,\, g+e), \quad (2.9)$$

with $w^{\mathrm{es}}_{g,e}$ giving the share of a group $G = g$ among treated counties that have been exposed to regulation for exactly e periods (the counties for which we observe event time e), and is formally defined as

$$w^{\mathrm{es}}_{g,e} = \mathbf{1}\{g + e \leq T\}\, \mathbb{P}_{\omega}(G = g \,|\, G + e \leq T,\, G \leq T)\,.$$

Thus, the $\mathrm{ATT}_{\mathrm{es}}(e)$ gives the average treatment effect among the counties that have been exposed to regulation for exactly e periods, conditional on being observed having introduced regulation. Estimating $\mathrm{ATT}_{\mathrm{es}}(e)$ merely requires replacing $\mathrm{ATT}(G,\, G+e)$ with the doubly robust estimates from before, i.e.,

$$\widehat{\mathrm{ATT}}_{\mathrm{es}}(e) = \sum_{g<\infty} \widehat{w}^{\mathrm{es}}_{\omega,g,e}\, \widehat{\mathrm{ATT}}(g, g{+}e), \qquad \widehat{w}^{\mathrm{es}}_{\omega,g,e} = \frac{\frac{1}{n}\sum_{i=1}^{n} \omega_i\, \mathbf{1}\{G_i = g\}\, \mathbf{1}\{G_i + e \leq T\}\, \mathbf{1}\{G_i \leq T\}}{\frac{1}{n}\sum_{i=1}^{n} \omega_i\, \mathbf{1}\{G_i + e \leq T\}\, \mathbf{1}\{G_i \leq T\}}. \quad (2.10)$$

Finally, as Callaway and Sant'Anna (2021) propose, one can generate an overall treatment (and pre-treatment) effect parameter by averaging the $\mathrm{ATT}_{\mathrm{es}}(e)$ parameters. Specifically, with $\mathcal{E}_{\mathrm{post}} := \left\{\, e \in \mathbb{Z} \,\middle|\, e \geq 0 \text{ and } \exists\, g \text{ with } g + e \leq T \,\right\}$ and $\tau_{\mathrm{post}} := |\mathcal{E}_{\mathrm{post}}|$, the aggregated treatment effect can be written as:

$$ATT^{\mathrm{post}}_{es} = \frac{1}{\tau_{\mathrm{post}}} \sum_{e \in \mathcal{E}_{\mathrm{post}}} ATT_{es}(e), \quad (2.11)$$

and

$$\widehat{\mathrm{ATT}}^{\mathrm{post}}_{es} = \frac{1}{\tau_{\mathrm{post}}} \sum_{e \in \mathcal{E}_{\mathrm{post}}} \widehat{\mathrm{ATT}}_{es}(e) \quad (2.12)$$

where pre-treatment "effects" can be summarized by

$$\widehat{\mathrm{ATT}}_{es}^{\mathrm{pre}} = \frac{1}{\tau_{\mathrm{pre}}} \sum_{e \in \mathcal{E}_{\mathrm{pre}}} \widehat{\mathrm{ATT}}_{es}(e), \tag{2.13}$$

with $\mathcal{E}_{\mathrm{pre}} := \left\{ e \in \mathbb{Z} \,\middle|\, e \leq -2 \text{ and } \exists\, g \text{ with } 1 \leq g + e \leq T \right\}$ and $\tau_{\mathrm{pre}} := |\mathcal{E}_{\mathrm{pre}}|$. In other words, $\widehat{\mathrm{ATT}}_{es}^{\mathrm{pre}}$ aggregates how the treated group changes over the pre-treatment period relative to the period right before treatment compared to the estimated counterfactual.

2.4.2 Sample Construction and Matching

I construct a balanced panel from 2008 to 2020 comprising counties that adopted STR regulations during this period (treated) and observationally similar counties that never adopted regulations. To ensure common support and reduce model dependence, I implement a matching procedure before estimation. While the Callaway and Sant'Anna (2021) doubly robust estimator incorporates covariate adjustment, pre-matching ensures comparisons occur only within regions of overlapping covariate distributions, reducing sensitivity to functional form assumptions.

The matching protocol proceeds as follows. For each treated county, potential controls must satisfy three criteria: (i) location within the same state to account for state-level regulatory environments and economic conditions; (ii) placement in the same quintile of the outcome variable distribution five years before treatment, ensuring baseline similarity in economic characteristics; and (iii) proximity in the four-dimensional covariate space. From candidates meeting the first two criteria, I select the two counties minimizing the Mahalanobis distance in pre-treatment covariates.[7]

The matched never-treated counties provide credible counterfactuals for treated units. However, following Callaway and Sant'Anna (2021), the estimation uses the broader set of not-yet-treated counties as the comparison group, which includes both the matched never-

[7] The matching is performed separately for each outcome variable to ensure optimal balance for each specific analysis.

treated counties and counties that have not yet adopted regulation by time t. This approach maximizes statistical power while maintaining comparison credibility through the initial matching on observationally similar never-treated units.

2.5 Results

This section presents staggered difference-in-differences estimates of STR regulation effects on local economic outcomes. I report three complementary sets of results: event-study estimates that trace out the full dynamic path of treatment effects (see Equation (2.10)), aggregated pre-treatment coefficients for parallel trends diagnostics (see Equation (2.13)) and aggregated post-treatment effects that summarize the overall effects of the STR laws (see Equation (2.12)). The analysis examines regulatory impacts across six key outcomes: house prices (Section 2.5.1), accommodation and food services employment (Section 2.5.2), real estate activity (Section 2.5.3), construction (Section 2.5.4), arts and recreation (Section 2.5.5), and property tax revenues (Section 2.5.6).

To isolate plausibly causal effects, the specification includes state fixed effects and county-level socioeconomic controls. State fixed effects account for time-invariant state-level factors such as general regulatory environments, tax structures, and economic conditions that affect both STR regulation adoption and economic outcomes. This is particularly important given that STR regulations, while often adopted at the city or county level, exist within broader state legal frameworks. The specification also incorporates county-level socioeconomic controls—population density, per capita personal income, unemployment rate, and college graduation share—measured five years before treatment. These controls implement the conditional parallel trends assumption discussed in Section 2.4, addressing the political economy considerations that drive both regulatory adoption and economic trajectories. By controlling for both state-level heterogeneity and pre-treatment county characteristics, this specification substantially reduces the influence of confounding factors that affect both treatment assignment and outcomes, though the possibility of remaining unobserved confounders cannot be eliminated.

2.5.1 Housing Market Effects

I begin by examining the impact of STR regulations on residential house prices. The covariate balance statistics (Table B.3) indicate successful matching on most dimensions, with normalized differences below 0.25 for income, education, and unemployment. Importantly, the matching protocol already ensures baseline similarity by requiring control counties to be within the same quintile of the outcome variable five years before treatment (see Section 2.4.2). Population density remains imperfectly balanced despite matching, reflecting the inherent challenge that STR regulations predominantly occur in dense urban markets, for which comparable control counties are scarce. This imbalance persists across all specifications but is addressed by controlling for it explicitly when estimating the ATTs.

Figure B.1 shows event-study estimates relative to the last pre-treatment period, and Table B.8 collapses these into aggregated pre- (*Pre_avg*) and post-treatment (*Post_avg*) coefficients. Pre-treatment coefficients are statistically insignificant for aggregate house prices and for each property-size category, indicating no evidence of differential pre-trends between regulated and non-regulated counties and supporting the plausibility of parallel trends after treatment.

Aggregate house prices show no significant response to STR regulation (*Post_avg* in Table B.8). However, there is some heterogeneity across property types. For example, there is suggestive evidence—even though not statistically significant— that the value of two-bedroom homes decreased slightly in the years following STR introduction with two-bedroom home prices experiencing an aggergate decline of 5% over the 5 years post-regulation, reaching -8.7% by year four post-treatment (see Figure B.1). If taken at face value, this pattern would be consistent with reduced investor demand for entry-level properties previously profitable as short-term rentals, and potentially with decreased affordability for households who previously relied on STR income to finance mortgages. Conversely, larger homes show no (economically or statisitically) significant effects (three-bedroom: 0.019; five-bedroom: -0.022, both insignificant), suggesting the regulatory impact, if at all, concentrates in the lower-tier housing market where STR yields relative to property values are highest.

2.5.2 Accommodation and Food Services Sector

Next, I examine whether STR regulations affected the sectoral GDP and personal income in the accommodation and food services sector. Despite some covariate imbalance (see Tables B.4–B.6)—treated counties have higher baseline income ($52,596 vs. $44,761)— differences between outcome trends in the pre-treatment period are insignificant across specifications, giving some credence to the parallel trends assumption for the post-treatment period (see *Pre_avg* in Tables B.9–B.11).

The accommodation and food services sector exhibits substantial adjustments following STR regulation. Sectoral GDP per capita increases by 13.3% following regulation (Table B.9), with pronounced dynamic effects: negligible impacts initially (0.008 at t=0) but rising to 32.2% by year five (Figure B.2). This temporal pattern likely reflects the gradual implementation and enforcement of STR regulations. Initial compliance may be limited as hosts wait to see if rules will be actively enforced, while enforcement agencies require time to develop monitoring systems, issue warnings, and pursue violations. As enforcement intensifies and non-compliant hosts face penalties or exit the market, demand progressively shifts toward traditional accommodations.

Personal income also shows positive effects across subsectors, with the accommodation sector experiencing an 11.0% increase (though not statistically significant) and food services showing an 8.0% gain (Table B.10 and Figure B.3).

These income gains, together with the GDP increases, are consistent with several mechanisms. Hotels may command higher prices—because they provide superior services relative to STRs or because they can exercise greater market power in a newly constrained accommodation market—and traditional hospitality districts may see more visitor traffic that spills over to nearby food-service establishments.

The 12.5% aggregate decline in the sector's employment share (Table B.11), which accelerates over time – from 10% at t=1 to 20% at t=5 (Figure B.4) and which is measured as the sector's share of total county employment, reveals a more complex structural transformation than simple substitution would suggest.

Several mechanisms likely operate simultaneously: First, STR regulations particularly impact full-time hosts who purchased secondary properties for short-term rental purposes, as many jurisdictions restrict hosting to primary residences. These formerly self-employed accommodation workers may transition to long-term rental models, causing their employment to be reclassified from the accommodation sector to the real estate sector in official statistics, or pursue traditional employment in other sectors.

Second, hotels appear to absorb redirected demand through existing capacity slack rather than employment expansion. Where five separate STR properties might require five self-employed hosts, a single hotel can accommodate the same number of guests using existing staff with minimal or no additional hiring, leveraging economies of scale unavailable to individual property operators.

Third, the employment share captures relative rather than absolute changes. The spatial concentration of hotels in commercial districts may generate asymmetric spillover effects: as visitors shift from dispersed and residential STR locations to hotel clusters, complementary sectors—particularly retail—could experience accelerated employment growth to meet increased demand. This reallocation effect would mechanically reduce accommodation's employment share even if absolute employment remains stable or grows modestly.

Collectively, the findings are consistent with a reallocation from fragmented, informal hosting to more consolidated, professional operations. The sector becomes more productive yet occupies a smaller share of local employment, with spillovers running through spatially clustered hospitality establishments. The persistent and growing effects over five years indicate these changes represent structural shifts rather than temporary adjustments, with traditional hospitality establishments not merely substituting for STRs but potentially capturing value through service differentiation and operational efficiencies unavailable to informal hosts.

2.5.3 Real Estate, Rental, and Leasing Sector

In this section, I examine the effect of STRs on the real estate, rental and leasing sector which is dominated by lessors and property management.

Covariate balance is strong (see Tables B.4–B.6), but Table B.10 reveals diverging trends for personal income (-0.082) in the pre-regulation period. In particular, personal income in the eventually regulated counties grows faster than in the control counties prior to the introduction of STR regulations, potentially biasing results.

Nevertheless, the real estate sector, dominated by lessors and property management, shows little response to STR regulation. Post-treatment effects are economically small and statistically insignificant across all measures: GDP per capita (0.037, Table B.9 and Figure B.2), personal income per capita (-0.024, Table B.10 and Figure B.3), and the employment share (0.018, Table B.11 and Figure B.4) all appear to be unaffected. The absence of positive effects contradicts the policy expectation that STR restrictions would substantially redirect properties to long-term rental markets. This suggests either that STR properties remain vacant or convert to owner-occupation, or that the scale of conversion is insufficient to meaningfully impact sector-wide aggregates.

2.5.4 Construction Sector

Following the real estate and leasing sector, I analyse the potential effects of STR regulations on the construction sector. Matching quality is somewhat weaker for this sector (see Tables B.4–B.6), with larger differences in education and income levels, though the matching requirement that treated and control counties fall within the same quintile of pre-treatment construction outcomes ensures basic comparability. Reassuringly, there are also no statistically significant differences in pre-treatment trends between regulated and non-regulated counties (see Tables B.9–B.11).

Construction activity shows no clear response to STR regulation, though estimates are imprecise. While statistically insignificant, the point estimate for GDP per capita is economically large (25.0% increase, Table B.9, with effects growing over time (Figure B.2)). Personal income and employment shares show negligible changes (Tables B.10–B.11 and Figures B.3–B.4). The large but imprecise positive GDP effect may reflect hotel construction responding to increased demand for formal accommodations, though the wide confidence intervals prevent strong conclusions. Importantly, STR regulations do not appear to depress construction activity, alleviating concerns about negative spillovers to this sector.

2.5.5 Arts, Entertainment, and Recreation Sector

Finally, I investigate whether STR regulations affected the arts, entertainment and recreation sector. Although the matching between regulated counties and their non-regulated controls yields reasonably close covariate balance, pre-treatment trends in GDP per capita and personal income per capita differ markedly. Regulated counties exhibit substantially faster pre-treatment growth in both outcomes, undermining the parallel-trends assumption for the post-treatment period needed for causal interpretation (see Tables B.9 and B.10).

With this caveat in mind, the point estimates indicate no discernible effect of STR regulations on average sectoral GDP per capita or on the sectoral employment share over the five years following adoption (Tables B.9 and B.11; Figures B.2 and B.4). By contrast, the estimates for personal income per capita imply a 41% decline over the post-regulation period (Table B.10 and Figure B.3); however, given the pronounced pre-treatment run-up in personal income in the eventually regulated counties, this pattern likely reflects a reversal of those pre-trends rather than a policy effect.

2.5.6 Property Tax Revenues

Finally, I examine the effects of STR regulations on property tax revenues. Covariate balance shows some disparities in income and education (Table B.7), and pre-treatment trends are very close to being significant (Table B.12), warranting cautious interpretation.

Property tax revenues exhibit complex dynamics following STR regulation. The average five-year effect is a modest 2.18% decline (Table B.12), but this masks substantial temporal heterogeneity. Years 1-3 show significant revenue drops of up to 23%, followed by recovery with 17-27% increases in years 4-5 (Figure B.5). On the one hand, this pattern aligns with institutional features of property taxation in "reset-on-sale" states (notably California's Proposition 13), where tax assessments update only upon transaction. The initial decline may reflect reduced property turnover as the market adjusts to regulatory change, while later increases capture both accumulated appreciation and resumed transaction volumes at higher post-regulation prices.

On the other hand, the initial decrease could also represent a reversal of the borderline significant pre-treatment effects, which show that eventually regulated counties experienced faster pre-treatment revenue growth than their controls. This pre-existing trend, combined with the complex dynamics of property markets and tax assessment systems, makes it difficult to isolate the causal impact of STR regulation on property tax revenues.

2.5.7 Economic Magnitudes of STR Regulations

To contextualize the overall economic impact of STR regulations on affected counties, I conduct a back-of-the-envelope calculation focusing on county GDP per capita. GDP per capita serves as a natural summary measure of economic impact because it captures the total productive output of the local economy on a per-person basis, providing a comprehensive view of aggregate economic consequences. While my analysis examines multiple outcomes—including house prices, employment, and property tax revenue—GDP per capita offers the most holistic assessment of the regulations' overall economic impact. As my earlier results demonstrate, the accommodation and food sector is the primary driver of this GDP effect, with sector-specific GDP per capita increasing by 13.3% following regulation.

The economic importance of this 13.3% increase depends critically on each county's reliance on the accommodation and food sector. Table B.13 presents the sector composition for regulated counties, showing the share of GDP attributable to each analyzed sector (Accommodation & Food, Arts & Recreation, Construction, Real Estate). For the Accommodation & Food sector, the median GDP share across treated counties is 3.53% [IQR: 3.07%–3.85%], indicating that for most counties, this sector represents a modest portion of the local economy. However, substantial heterogeneity exists: tourism-dependent counties such as Clark County, Nevada (Las Vegas) and Teton County, Wyoming (Jackson Hole) derive nearly one-fifth of their GDP from accommodation and food services.

To translate the sector-specific effect into aggregate economic terms, I calculate each county's baseline accommodation and food sector share using the five-year pre-treatment average. I then apply the estimated 13.3% treatment effect to this baseline share to approximate the implied contribution to total GDP growth, under two simplifying assumptions: (i) treatment effects are relatively homogeneous across counties, and (ii) spillover effects to other sectors are negligible, consistent with my finding of no significant effects on other economic outcomes.

Table B.14 presents these calculations. The median effect across treated counties is a 0.47 percentage point increase in total GDP per capita relative to the counterfactual based on control counties. In absolute terms, this translates to a median increase of $2,116 per capita over the five-year post-treatment period (or approximately $423 per capita annually). However, the effect varies considerably with sector dependence: tourism-intensive counties like Clark and Teton experience substantially larger impacts of 2.28 and 2.51 percentage points, respectively, corresponding to per capita GDP increases of $8,474 and $15,634.

These calculations suggest that for the typical regulated county in my sample—where accommodation and food services comprise a relatively small share of the economy—the aggregate economic impact of STR regulation is modest, despite the statistically significant sector-specific effect. However, for counties heavily reliant on tourism and hospitality, the regulations generate substantially larger economic consequences. This heterogeneity underscores that while STR regulations may be politically contentious across diverse jurisdictions, their economic implications vary considerably depending on local economic structure, with meaningful effects concentrated in tourism-dependent communities.

2.6 Conclusion

This paper examines the economic consequences of short-term rental regulations implemented across U.S. counties between 2010 and 2017. Using a staggered difference-in-differences design with matched control counties, I estimate the effects of STR restrictions on housing markets, sectoral economic activity, and local government revenues.

The results reveal that STR regulations seem to trigger sectoral reallocation without generating substantial aggregate economic disruption. In the accommodation and food services sector, regulations are associated with a 13% increase in GDP per capita and an 8-11% rise in personal income, alongside a 12.5% decline in the sector's employment share. This pattern indicates a shift from fragmented, informal hosting toward consolidated, capital-efficient professional hotels. The formal hospitality sector appears capable of absorbing demand previously served by STRs.

Housing market impacts are limited and heterogeneous. While aggregate house prices show no significant response, there is suggestive evidence that two-bedroom homes experience a minor decline, consistent with reduced investor demand for entry-level properties that previously generated attractive STR yields. Larger homes remain unaffected, suggesting regulatory impacts concentrate in specific market segments rather than transforming broader housing dynamics. Notably, the real estate and rental sector shows no measurable response, contradicting policy expectations that STR restrictions would substantially redirect properties to long-term rental markets.

Fiscal impacts (if any) prove temporary. Property tax revenues decline by up to 23% in the first two years following regulation but recover fully within five years. This V-shaped pattern likely reflects institutional features of property taxation systems that reset assessments upon sale, creating short-term revenue volatility as transaction volumes adjust to the new regulatory environment.

These findings contribute to broader debates about regulating emerging technologies and platform economies. The evidence suggests that STR restrictions neither devastate local economies nor deliver transformative housing affordability improvements. Instead, they primarily redistribute economic activity from informal to formal providers, with efficiency implications that depend on one's weighting of consumer surplus, producer welfare, and externalities. While regulations successfully redirect accommodation provision toward traditional establishments, their limited impact on housing availability and prices suggests that STR platforms may be less central to housing market dynamics than the political discourse often suggests. For policymakers confronting similar platform economy disruptions, these results highlight the importance of clearly defining regulatory objectives and recognizing that market responses may concentrate in unexpected dimensions.

While this paper focuses on the arguably more immediate economic effects, several important dimensions of STR regulations remain unexplored. First, regulations may trigger demographic shifts if they improve residential quality of life. Future research could examine whether restrictions affect the number of kindergarten and primary school-aged children in counties, capturing potential "family flight" back to previously touristified neighborhoods or, conversely, continued family departure if regulations prove insufficient to restore residential character.

Second, the mental health consequences of STR activity and its regulation merit investigation. County-level measures of mental health outcomes—such as depression rates, anxiety disorders, or stress-related conditions—could reveal whether the noise, transience, and disruption associated with STRs impose psychological costs on long-term residents, and whether regulations mitigate these effects.

Third, anti-social behavior and crime at the county level represent another understudied outcome. STR concentrations may correlate with property crime, public disturbances, or violations of local ordinances. Examining whether regulations reduce such incidents would illuminate the broader social costs and benefits of platform-mediated tourism.

Fourth, the economic welfare of local small businesses serving residents—particularly services oriented toward neighborhood stability such as grocery stores, pharmacies, and local restaurants catering to residents rather than tourists—deserves attention. If STR activity displaces resident-serving businesses in favor of tourist-oriented establishments, regulations might reverse this substitution, supporting employment among "pink collar" workers and preserving neighborhood commercial ecosystems.

Finally, two broader questions merit attention. First, school quality and educational outcomes could respond to STR regulations if restrictions stabilize residential populations and strengthen community investment in local institutions. Second, the distributional consequences across income groups remain unclear: regulations may disproportionately benefit or harm lower-income residents depending on whether they rely more heavily on STR income opportunities or suffer more acutely from STR-induced displacement pressures. Understanding these equity dimensions would inform more targeted policy design.

Chapter 3

Inventor Demographics and Employment Dynamics: Comparing the U.S. and Germany

Abstract

Using comprehensive employer-employee administrative data covering nearly all German inventors from 1999-2011, I analyze inventor characteristics and employment dynamics and compare them to U.S. inventors. Both countries have aging inventor populations with low female participation, increasing concentration at larger and older firms, and declining job mobility. However, Germany's inventors are overwhelmingly domestic-born, contrasting with substantial foreign-born representation in the U.S., particularly in high-impact fields. While inventors in both countries concentrate in top income groups, the earnings-productivity relationship appears stronger in the U.S. Moreover, while German inventors tend to be more productive in larger firms of any age, U.S. inventors are most productive in young, small firms. These patterns could offer insights into the innovation slowdowns affecting advanced economies.

3.1 Introduction

Innovation drives long-term economic growth, yet three of the world's five most patent-intensive economies—the United States, Japan, and Germany—have experienced stagnating or declining innovation rates for nearly a decade[1]. This raises fundamental questions about the mechanisms of the innovation process and highlights the urgent need to understand the factors constraining technological progress in advanced economies. Fine-grained inventor data linked to firms remains scarce, limiting our understanding of the supply side of innovation: the inventors themselves and the labor markets in which they operate.

This paper provides a comprehensive analysis of German inventor characteristics and employment dynamics, enabling a cross-country comparison with the United States. These two economies rank among the world's most innovative, yet they differ substantially in labor market institutions, demographics, and access to talent and capital. Using employer-employee administrative data covering nearly the universe of German inventors from 1999 to 2011, I examine four key dimensions of the inventor labor market: demographics, employer characteristics, earnings, and employment dynamics. I closely follow Akcigit and Goldschlag's (2025) analysis of U.S. inventors, which allows a direct comparison between the two countries and provides new evidence on how institutional and other differences affect innovation.

The findings reveal both striking similarities and important differences between the German and American inventor labor markets. Both countries have aging inventor populations with persistently low female participation, and inventors in both nations increasingly concentrate at larger and older firms[2], while becoming less mobile across jobs and regions. However, Germany's inventor workforce is overwhelmingly domestic-born, contrasting sharply with the substantial foreign-born inventor population in the United States, particularly from China

[1]See Figure C.1 and, for comparison with China and South Korea, Figure C.2.

[2]The IAB data provides establishment-level information. However, in Germany, R&D activities are predominantly concentrated at firms' headquarters rather than distributed across multiple establishments (Stifterverband 2016).

and India—groups that are disproportionately represented in high-impact fields such as information technology. Moreover, while inventors in both countries are predominantly high earners concentrated at the top of their respective income distributions, the relationship between earnings and inventor productivity, measured by citations, appears stronger in the United States. Another key difference is the relationship between firm age, firm size and inventor productivity: U.S. inventors at young, small firms produce the highest-impact patents, whereas in Germany the peak occurs at medium-to-large-sized, middle-aged firms.

These patterns offer potential insights into the innovation slowdown puzzle, suggesting that both cross-country institutional differences and common trends in labor mobility, immigration policy, and firm dynamics may play crucial roles in shaping innovation outcomes. The results have important implications for understanding how labor market institutions affect the allocation of innovative talent and, ultimately, aggregate innovation performance.

An important determinant of innovative output is the utilization of inputs in the innovation production function. Substantial evidence documents considerable historical misallocation of creative talent (e.g., Alper 1993; Aghion et al. 2017; Bell et al. 2019b; Hsieh et al. 2019). Women are often underrepresented among inventors, and not all economies effectively attract and integrate foreign-born workers into their innovation ecosystems, though those that do often benefit substantially (Akcigit et al. 2017).

Examining the demographic composition of inventors in the U.S. and Germany reveals patterns that illuminate potential sources of talent misallocation. Both Germany and the United States exhibit significant underrepresentation of women among inventors, particularly on a citation-weighted basis, with females accounting for less than 12% of inventors in both nations. In the United States, however, female participation is gradually rising and tends to be higher among young inventors, while Germany exhibits no such improvement—female representation remains stagnant even among younger cohorts. Female inventors in both countries also demonstrate distinct sectoral patterns, concentrating in Health Care and Social Assistance in the U.S. and in Chemical/Pharmaceutical fields in Germany.

The differences in foreign-born inventor representation are even more pronounced. Over 30% of U.S. inventors are foreign-born, with China and India becoming increasingly dominant within this group—rising from 25% of foreign-born inventors in 2000 to 40% by 2016. Foreign-born inventors concentrate heavily in Information, Education, and Professional/Scientific Services—sectors that have become increasingly central to modern innovation. Germany presents a stark contrast: only very few inventors are foreign-born—3.3% in 1999. While this share rose 63% between 1999 and 2009, it declined again from 2010 onwards. Moreover, most foreign-born inventors come from other EU countries. Crucially, German foreign-born inventors cluster in Electrical Engineering and Chemical/Pharmaceutical fields—areas representing Germany's traditional technological strengths rather than the high-growth sectors driving contemporary innovation.

Both countries face aging inventor populations with similar trajectories. Between 2000 and 2011, mean inventor age increased from 41 to 45 years in Germany and from 43 to 45 years in the United States. Similarly, the share of young inventors ($\leq$ 35 years) declined sharply—from 35% to 20% in Germany and from 20% to 14% in the United States. These demographic trends suggest that, while both economies confront comparable challenges in talent utilization, institutional and policy differences may explain why the United States appears more successful at attracting diverse talent and channeling it toward emerging technological frontiers.

Another critical aspect of talent allocation and utilization is which types of firms employ inventors. The rise of superstar firms and dominant incumbents have generated substantial research interest, as these large firms present a complex picture: while typically demonstrating high productivity, they often engage in anti-competitive behaviors ranging from political lobbying to poaching talent from smaller rivals, while inefficiently hoarding labor resources (e.g., Segal and Whinston 2007; Gutiérrez and Philippon 2017; Acemoglu et al. 2018; Autor et al. 2020; Akcigit and Goldschlag 2023; Akcigit et al. 2023).

The allocation of inventors across firm types reveals common trends in both countries that align with broader patterns of increasing market concentration. In both Germany and the United States, inventors increasingly concentrate at older, larger firms, with over 60% of inventors in both countries working at firms older than 20 years and at firms with at least 1,000 employees. This concentration has intensified over time: in the U.S., the share of inventors at incumbent[3] firms rose from 49% to 58% between 2000 and 2016, continuing a trend that appears to extend back to the 1970s, while Germany followed a remarkably similar path over the 2000-2016 period[4].

The flight from young firms is potentially problematic for innovation dynamics. In both countries, the share of inventors working at firms up to five years old fell by almost half during the study period—from 15% to under 8% in the United States and by a similar magnitude in Germany. However, the composition of this shift differs importantly between the countries: in the U.S., the decline was most pronounced among superstar inventors with the most impactful patents—this group became increasingly rare at young firms over time. In Germany, by contrast, the decline was steepest among ordinary inventors, who became less likely to work at young firms.

The relationship between firm characteristics and inventor productivity is markedly different for the two countries. In the United States, firm age dominates as a predictor of patent impact: inventors at young firms (ages 0-10) consistently produce the highest-impact patents across all size categories, with citations declining almost monotonically with firm age. The sweet spot appears to be very young, small-to-medium-sized firms (0-5 years old, 21-250 employees). By contrast, inventors at the oldest firms (21 years and older) generate patents with 30-50% fewer citations, regardless of firm size.

Germany, on the other hand, shows a fundamentally different pattern in which firm size predicts individual inventor productivity much better than firm age. Across nearly all age groups, medium- to large-sized firms (> 250 employees) employ the most productive

[3]Akcigit and Goldschlag (2023) define incumbent firms as firms that are older than 20 years and that have at least 1,000 employees.

[4]The IAB data does not allow me to reliably analyse the time period between 1970 and 1998.

inventors. Moreover, within this size class inventor productivity peaks in the middle-aged (6-20 years old) rather than in the youngest (5 years or younger) firms. Finally, in sharp contrast to the U.S., large, old (1000+ employees, 21+ years old) firms employ more productive inventors than very young, small firms.

One possible explanation for these differences lies in the well-documented financial market constraints that smaller firms face in Europe compared to the U.S.[5] The United States' mature venture capital ecosystem enables even unproven startups to secure funding for ambitious projects that would struggle to attract investment in Germany. Thus, even small (or young) American firms may be able to pursue high-risk, high-reward innovation strategies, while their German counterparts face binding capital constraints that force more conservative approaches to R&D.

These divergent patterns reframe our understanding of the declining share of inventors at young firms. In the United States, where superstar inventors became increasingly concentrated at established firms over time, this shift represents a reallocation of top talent toward less productive environments, potentially dampening innovation. In Germany, however, where ordinary inventors became less likely to work at young firms, this population-level shift may actually concentrate them in more productive settings—particularly medium-sized and large established firms where patent impact remains high. This suggests that the innovation consequences of young firms employing fewer inventors depend critically on national innovation systems and which types of inventors are becoming scarce at young firms.

Innovation and entrepreneurial activity concentrate not only by firm type but also geographically. The dual forces of specialization and knowledge exchange drive the formation of innovation clusters, where innovative firms and personnel in an industry agglomerate in specific locations, generating knowledge spillovers that enhance individual inventor productivity (e.g., Audretsch and Feldman 1996; Jaffe et al. 2000; Thompson and Fox-Kean 2005; Hoisl 2007; Kerr and Kominers 2015; Rosenthal and Strange 2020; Matray 2021; Moretti

[5] see European Investment Bank (2024)

2021). However, when innovative activities concentrate in relatively few locations, talented individuals must be both able and willing to relocate to participate effectively in the innovation process (Gischer 2025). Furthermore, successful clusters and innovation systems depend critically on generating new ventures with fundamentally new ideas—a capacity that appears to have weakened in recent years (e.g., Decker et al. 2014; Akcigit and Ates 2021).

The mobility patterns of inventors in both countries suggest a decline in the dynamism that traditionally underpinned successful innovation ecosystems. While Germany maintains higher absolute levels of inventor mobility across firms than the United States, both nations experienced substantial declines: U.S. job mobility[6] fell from 7.5% to 6.5% by 2016, while it dropped from 16% to 10% in Germany. This reduced labor market fluidity may impede the knowledge transfer and recombination processes that drive innovation within and across clusters.

The entrepreneurial dynamism that creates new ventures has similarly weakened. In both countries, inventors have become significantly less likely to start[7] firms over time. The United States shows a particularly stark pattern among superstar inventors, whose probability of becoming entrepreneurs fell by 57%, while in Germany, the 70% decline in entrepreneurial activity affected ordinary and superstar inventors equally. This reduction in new firm formation may be especially problematic given the importance of young firms in introducing disruptive innovations (Decker et al. 2017).

Geographic patterns reveal different trajectories that may reflect distinct institutional contexts. In the United States, inventor concentration has intensified markedly, with the

[6] Akcigit and Goldschlag (2025) report hire and separation rates rather than direct job-to-job transition rates. While these measures capture all employment transitions—including movements to and from unemployment or labor force exit—I use separation rates as a proxy for U.S. job mobility. For high-skilled workers like inventors, separation rates provide a reasonable approximation because: (1) involuntary separations and unemployment spells are relatively rare for this group, (2) the competitive market for innovation talent means most voluntary separations involve immediate transitions to new positions rather than labor force exits. Though this approach may overstate mobility by including some non-job-to-job separations, it likely captures the broad patterns of inventor mobility across firms.

[7] Akcigit and Goldschlag (2025) employ a more restrictive definition of entrepreneurial activity, making level comparisons less meaningful than trend comparisons across countries. See the Results section for detailed methodological discussion.

share of inventors working in the 20 largest counties by inventor count rising from 39% to over 47% between 2000 and 2016. Germany, by contrast, maintained stable geographic concentration—hovering between 38% and 42% throughout the period, with a temporary dip in the late 2000s. However, both countries exhibit declining willingness or ability to relocate for employment opportunities: the share of U.S. inventors switching employment across state lines fell from a peak of 4.6% in 2006 to 2.6% in 2016, while German cross-state mobility plummeted from 10% in 2000 to just 4% by 2011—a decline that, despite higher initial levels, mirrors the reduced geographic dynamism observed in the United States.

These trends suggest that both innovation systems are becoming less dynamic, potentially undermining the flexible talent allocation and entrepreneurial experimentation that successful clusters require. The combination of reduced job mobility, declining entrepreneurship, and limited geographic mobility may constrain the innovation process precisely when technological and competitive pressures demand greater adaptability.

Finally, innovation output at the individual level may be driven by compensation incentives. Research from the United States and Finland demonstrates that inventors respond strongly to pecuniary incentives, including making location decisions based on tax rates (Akcigit et al. 2016), and that successful invention, particularly high-impact work, receives substantial financial rewards (e.g., Toivanen and Väänänen 2012; Aghion et al. 2018; Kline et al. 2019).

Inventors in both countries are highly compensated, though with notable differences in the strength of performance-based rewards. In the United States, 63% of all inventors and 88% of superstar inventors are in the top 10% of the national income distribution, with almost 8% of all inventors and 19% of superstar inventors in the top 1%. Similarly, the majority of German inventors are located in the highest available income bracket[8], indicating that inventor compensation is substantial in both countries.

However, the relationship between earnings and productivity reveals important cross-

[8]German administrative labor data censors wages, i.e., wages that are above the social security contribution limit are top-coded. The majority of German inventors fall within this earnings category.

country differences in incentive structures. In the United States, this relationship is strong and monotone: inventors in the top 10% of the inventor earnings distribution have average citations per patent about 0.5 log points higher than those in the bottom 10%, and productivity and income moves in lockstep across the entire income distribution. Germany presents a more complex pattern where inventor earnings are less strongly tied to productivity. While the highest-earning German inventors are mildly more productive than those in the lowest income brackets, this relationship does not hold universally across all income levels—for instance, inventors in the second-highest income bracket are not more productive than those in the lowest bracket.

These contrasting patterns may reflect different institutional approaches to inventor compensation and career advancement. The stronger earnings-productivity relationship in the United States suggests more market-driven compensation mechanisms, while the weaker and inconsistent relationship in Germany may indicate compensation structures influenced by factors beyond individual productivity, such as collective bargaining agreements, seniority systems, or different organizational reward structures. Such differences in incentive alignment could have implications for individual inventor effort and, ultimately, aggregate innovation outcomes.

3.2 Data

3.2.1 IAB Dataset

This study uses the linked inventor biography dataset INV-BIO ADIAB 1980–2014, provided by the Research Data Centre of the German Federal Employment Agency (IAB)[9]. The data link inventor and patent information from the European Patent Office (EPO) and the German Patent and Trademark Office (DPMA) to administrative labor market records from Germany's social security system. Using a record linkage and machine-learning procedure,

[9]For more information, see the associated data report FDZ IAB (2018).

the dataset assigns pseudonymized social security identifiers to inventors listed on EPO patent applications between 1999 and 2011, yielding a sample of 152,350 inventors whose careers can be tracked from 1980 through 2014.

The dataset captures both inventive activity and detailed employment biographies. Patent records include approximately 644,000 patent families contributed by the linked inventors, with bibliographic details and technology field classifications that enable measures of inventive output and quality. Labor market data, drawn from the Integrated Employment Biographies (SIAB), provide spell-level information on employment, unemployment, and job search episodes, along with wages[10], occupations, and demographics. Establishment identifiers enable merging with firm-level data on industry, location, and structural characteristics from the Establishment History Panel.

This comprehensive database tracks inventors' longitudinal career trajectories—employer transitions, wage dynamics, and inventive productivity changes. Coverage is limited to inventors in dependent employment subject to social security contributions, excluding the self-employed, freelancers, and civil servants who comprise approximately ten percent of inventors according to survey evidence. Nevertheless, the sample represents about 71 percent of all German patent families during 1999–2011.

To contextualize the German evidence, I compare my results with U.S. findings from Akcigit and Goldschlag (2025), who construct a comparable database linking USPTO patent records to Census Bureau microdata. Their dataset covers 4.5 million inventor records from 2000 to 2016 and, like INV-BIO, combines patent information with administrative employer–employee records to generate longitudinal career histories. While the datasets differ in specific features—U.S. wage data avoid the censoring issues present in German social security registers, whereas German data provide richer establishment-level detail—these differences are minor. The fundamental comparability of both datasets makes them well-suited

[10] Wages in the German social security data are top-coded at the social security contribution limit (Beitragsbemessungsgrenze), meaning all earnings above this threshold are recorded at the ceiling value rather than the actual wage amount.

for parallel analysis of inventor careers and systematic cross-national comparison.

3.2.2 Measuring Innovation Output

Patents

This analysis relies exclusively on patent data, which captures formally protected innovations but might miss those safeguarded through other mechanisms such as trade secrecy (Cohen et al. 2000; Moser 2013). Following Akcigit and Goldschlag (2025), I proxy inventor productivity using forward citations accumulated within four years of the earliest publication date at the German Patent and Trademark Office. This citation-based metric not only facilitates comparison with Akcigit and Goldschlag (2025) but also addresses the well-documented heterogeneity in patent value (Trajtenberg 1990). Results prove robust to alternative specifications in the German data, including substituting patent counts for citation-based measures or using fractional patents to account for collaborative invention.

Superstar Inventors

Building on Akcigit and Goldschlag (2025), I identify superstar inventors dynamically over time using a rolling performance measure. For each inventor-year observation, I calculate the cumulative citations received over the preceding four years for all same-aged inventors in that year[11]. This age-adjusted approach accounts for career-stage effects on productivity. I classify inventors as superstars when they rank in the top 10% of this citation distribution. The four-year window balances two concerns: minimizing end-of-sample censoring while maintaining sufficient temporal coverage for meaningful measurement.

[11] Akcigit and Goldschlag (2025) use a five-year window and quarterly time intervals. Due to a smaller observation period and less reliable quarterly observations, I opt for a four-year citation period and yearly intervals.

3.3 Results

3.3.1 Demographics

Innovation output depends critically on the number and quality of researchers engaged in the innovation process. Ideally, individuals' selection into innovative professions should reflect skill, talent, and inclination rather than demographic characteristics.

Female Inventors

Figure C.3 reveals persistently low female representation among German inventors from 1999 to 2011. Female inventors comprised approximately 5% of all active inventors and generated 3.6% of citations in 1999. Their share increased only marginally over the subsequent decade, temporarily reaching 6% (accounting for 4.2% of citations) in 2009 before declining to 4% (3.3% of citations) by 2011.

Among younger women (35 or under), who exhibit higher labor force participation than older cohorts and were exposed to early interventions like Women in Science Days alongside broader societal encouragement to pursue STEM, female representation doubles to 8% and rises steadily to 12% by 2009 before dropping back to 8% in 2011. Crucially, female inventors not only expand the inventor pool but also allocate differently across technological domains. Figure C.4 demonstrates that over 50% of female patents are in the chemical and pharmaceutical sectors, compared to just 17% for male inventors. Conversely, mechanical engineering—including automotive—accounts for only 15% of female patents versus 37% for males. This sectoral concentration extends to employment patterns: Figure C.5 shows female inventors disproportionately working in chemical manufacturing while being underrepresented in transport equipment, optical equipment, and machinery manufacturing.

The United States exhibits higher female inventor participation with consistent growth. Akcigit and Goldschlag (2025) document an increase from 8% in 2000 to over 11% by 2016, with young female inventors rising from 11% to 16%. These cross-country differences may reflect Germany's historically low female labor force participation or additional barriers within German innovation markets. However, conditional on participation, German female inventors face slightly less citation disadvantage than their American counterparts. The citation gap for female inventors is larger in the United States—where women represent 10% of inventors but generate only 7% of citations—than in Germany, where the corresponding figures are 5.4% and 4%. Both countries show female inventors concentrating in healthcare and social assistance sectors, suggesting field preferences that transcend national contexts.

Foreign Inventors

Attracting foreign-born innovators becomes increasingly crucial when domestic inventor pipelines weaken. Figure C.6 shows that foreign-born inventors represent a small fraction of Germany's inventor population, starting at 3.3% in 1999 (with a 2.9% citation share), peaking at 5.4% in 2009 (4.1% citation share), then declining to 3.9% by 2011 (3.3% citation share). Notably, the citation gap[12] widens as the foreign-born inventor share increases—from a 12% deficit in 1999 to 24% in 2009—suggesting that while Germany attracted more foreign inventors during this period, it struggled to recruit the highest-impact talent.

Figure C.7 reveals that approximately 80% of Germany's foreign-born inventors originate from Europe in both 1999 and 2011. Austria, sharing Germany's language, contributes nearly a quarter of foreign inventors in 1999 and 15% by 2011. Strikingly, Asian inventors—including those from China and India—comprise only 10%, despite these countries' strength in critical fields like information technology and electrical engineering. This pattern suggests that language and cultural barriers substantially limit Germany's ability to attract global talent. Figure C.8 shows that while foreign-born inventors have greater representation in chemical/pharmaceutical (26% versus 18% for natives) and electrical engineering (23% versus 19%), they remain underrepresented in mechanical engineering (29% versus 37%) and related

[12] Defined as $\frac{\text{inventor share} - \text{citation share}}{\text{inventor share}}$.

industries (see Figure C.9).

The United States presents a stark contrast to Germany. Foreign-born inventors not only represent a much larger share—24% in 2000 rising to 33% by 2016—but consistently outperform domestic inventors in citations (Akcigit and Goldschlag 2025). In 2000, foreign inventors generated 1.25 citations for every 1.0 expected based on their population share; by 2013, this ratio had declined to 1.15. While still attracting above-average talent, the U.S. appears to face diminishing returns as it expands its foreign inventor pool, suggesting the most exceptional international talent may be increasingly difficult to recruit (or is perhaps already there).

Inventor Age

Sustaining innovation requires either attracting foreign talent or developing domestic inventors. Germany's inventor age structure reveals concerning trends. Figure C.10 shows mean inventor age rising sharply from 41 in 2000 to 45 by 2011, while young inventors (35 or younger) plummet from 34% to 15%. Interestingly, Figure C.11 indicates minimal technological divergence between age cohorts—with the only difference being young inventors showing slightly stronger preferences for electrical engineering (21% versus 18%) over mechanical engineering (33% versus 36%).

The United States experiences similar aging, with average inventor age rising from 43 to 46 between 2000 and 2016 and young inventors declining from 20% to 16% (Akcigit and Goldschlag 2025). However, young U.S. inventors concentrate more heavily in emerging technologies like IT, and the country's superior ability to attract strong foreign inventor talent may partially offset demographic headwinds—an option less available to Germany.

3.3.2 Employer Characteristics

Another crucial question is what types of firms employ inventors, and how firm characteristics relate to inventor productivity. Do large, established firms or small, young startups better foster innovation?

Firm Age and Inventor Age

Table C.1 and Figure C.12 show that most inventors across all age groups work at established firms—over half are employed by firms more than 20 years old, reaching 64% for some inventor age cohorts. Young inventors (≤25 years) have a slightly higher propensity to work at young firms (15% at firms 0–5 years old) compared to older inventors (≥56 years, 12%). However, Figure C.13 documents declining relative employment at young firms: the share of inventors at young firms fell from 16% in 1999 to 8% by 2011, with superstar inventors showing similar declines (14% to 7%).

These patterns closely parallel U.S. trends. Akcigit and Goldschlag (2025) report 68% of inventors at firms older than 20 years, with employment at young firms declining from 15% to under 8% from 2000 to 2016. The United States shows even stronger age-based sorting, with young inventors nearly twice as likely as older ones to work at young firms (15% versus 8%).

Firm Size and Inventor Age

Table C.2 and Figure C.14 show that 52% of inventors work at large firms (>1000 employees). However, both young (≤25) and older (≥56) inventors are less concentrated in larger firms (43% and 44%) than middle-aged inventors (53–56%). Young inventors have the highest small-firm employment (11% at firms with ≤20 employees), substantially exceeding that of older inventors (5%). Overall, however, small firms employ only 4% of all inventors.

U.S. patterns differ slightly. American inventors are more concentrated at both ends of the firm-size distribution—64% work at large firms and 10% at small firms. Moreover, Akcigit and Goldschlag (2025) find no pronounced age-based sorting by firm size, though middle-aged inventors (26–55) remain most likely to work at large firms. The higher share of U.S. inventors at large firms reflects broader structural differences: U.S. firms are generally larger than their

European counterparts[13]. Germany's economy, by contrast, has historically centered on its "Mittelstand"[14]—medium-sized firms that often dominate global niche markets as "hidden champions". This middle-market focus leaves less room for both small startup-like firms and large multinationals. Indeed, Germany exhibits lower startup rates than the U.S. and even many other European countries[15].

Inventor Productivity by Firm Age and Firm Size

Given the concentration of inventors in large, established firms, it is essential to understand how firm characteristics map into individual productivity. Table C.3 shows that, in Germany, inventors at "middle-aged" (6–20 years), medium-to-large (250–1,000 employees) firms exhibit the highest average productivity, measured by four-year citations per patent, followed by those at the oldest, largest firms. At the other end, inventors at small, old firms are least productive. Among the oldest firms, productivity increases monotonically with firm size.

In the United States, by contrast, the most productive inventors are concentrated in very young, small firms, followed by young, medium- to large-sized firms. Across nearly all size classes, older firms employ less productive inventors. For instance, among very small firms (≤ 20 employees), inventors at the youngest firms are nearly 80% more productive than those at the oldest firms; even among very large firms ($\geq 1{,}000$ employees), inventors at the youngest firms outperform those at the oldest firms by more than 20%—the opposite of the German pattern. Although U.S. patents receive roughly an order of magnitude more citations than German patents (5.665 vs. 0.529 for comparable firms)[16], the *relative* patterns align: in both countries, inventors at "top firms" have about 40% higher productivity than those at "bottom firms." What differs is which firms are "top" and "bottom": in the United States they are young (and often small), whereas in Germany they are larger and more

[13] see U.S. Bureau of Labor Statistics (2025) and Stenkula (2006)

[14] see IfM Bonn (2024)

[15] see OECD (2025)

[16] Part of this difference likely reflects the citation windows: Akcigit and Goldschlag (2025) use five-year forward citations, whereas I use four years (because a slightly smaller sample period).

established.

Financing constraints offer a plausible explanation for this discrepancy. Young, small firms—especially those pursuing novel but riskier products—may face tighter financing constraints in Germany than in the United States[17]. As a result, large, established German firms with robust balance sheets and long banking relationships are more likely to provide the complementary resources and stability that enable inventors to be highly productive. By contrast, in the U.S., young and small innovative firms can more readily access venture financing, allowing them to support—and elicit—high inventor productivity at earlier stages.

3.3.3 Inventor Earnings

While German data censoring limits the analysis compared to Akcigit and Goldschlag (2025), we can still show patterns related to earnings levels and to the broad relationship between earnings and productivity.

Position in the Income Distribution

German inventors earn substantially more than the average German worker. Table C.4 shows that 90% of inventors in 2000 and 97% in 2010 earned more than the average worker. Figure C.15 shows that approximately 52% of inventors are in the top earnings decile in 2000, rising to 66% by 2011. Similarly, Akcigit and Goldschlag (2025) find that over 92% of U.S. inventors are above the 60th earnings percentile, with 63% in the top decile.

Earnings and Productivity

To examine the relationship between inventor earnings and productivity in Germany and enable comparisons with the United States, I adopt Akcigit and Goldschlag's (2025) regression

[17] see, for example, European Investment Bank (2024)

framework of earnings on citations, i.e.,

$$ihs(Cites_{i,t}) = \alpha + \sum_{j=2}^{5} \lambda_j EarnGroup[j]_{i,t} + X + \epsilon_{i,t} \tag{3.1}$$

I measure earnings using year-end wages in the patent application year[18] and group them into five categories[19]: the top category captures all top-coded wages (those at or above the social security contribution limit), while the remaining wages are divided into quartiles (representing the remaining four categories, with category 1 being the reference group). Citations are measured over a 4-year window[20] and inverse hyperbolic sine transformed. Following Akcigit and Goldschlag (2025), I include fixed effects for inventor age, calendar year, industry, firm age, and firm size.

Figure C.16 shows a week relationship between earnings and productivity. Top earners generate patents with only 1% more citations than bottom earners, compared to 50% more citations in the United States. Moreover, Germany's fourth earnings group has no citation advantage over the lowest group. Data limitations notwithstanding, these patterns suggest a fundamentally weaker performance-pay link for German inventors.

3.3.4 Employment Dynamics

Employment dynamics are important determinants of economy-wide productivity and innovation. Job mobility enables sorting between workers and firms while facilitating knowledge spillovers and collaboration. New firm creation often catalyzes emerging technologies.

[18] Akcigit and Goldschlag (2025) use averaged earnings from two quarters before and after the patent application. I use year-end wages as they are more reliably recorded in the IAB data. Robustness checks using their averaging approach yield virtually identical results.

[19] Akcigit and Goldschlag (2025) group earnings into deciles. Given the top-coding in IAB wage data, upper deciles would be indistinguishable, necessitating broader categories.

[20] Akcigit and Goldschlag (2025) use a 5-year citation window. I employ 4 years to maximize sample size given my shorter observation period. Results remain substantively unchanged when using five years.

Job Mobility and Geographic Concentration

Figure C.17 documents steadily declining job mobility, defined by the share of inventors who switch jobs in a given year, among German inventors, from 17% in 2000 to 10% in 2011. The United States experienced similar declines from lower initial levels, with hire and separation rates[21] falling from 7% and 8% to 4% and 6%, respectively (Akcigit and Goldschlag 2025).

Interstate mobility shows even sharper declines. Figure C.18 shows that German inventors' willingness to cross state lines plummeted from above 10% in 2000 to under 4% by 2011. While U.S. declines appear less dramatic (3.5% to 2.5% between 2002 and 2016), Germany's smaller size makes state changes arguably less burdensome, rendering the comparison complex.

Beyond lower overall labor-market dynamism and a declining willingness of inventors to relocate for better matches, reduced interstate job mobility may also reflect increasing agglomeration which would necessitate inventors to stay inside a certain region even when changing jobs. Figure C.19 shows that the top 20 German counties accounted for 38–43% of inventors throughout 2000–2011. In the United States, concentration in the top 20 counties was at a similar level early on but then climbed—from about 39% in 2011 to 47% by 2016 (Akcigit and Goldschlag 2025). The net effect of such concentration on overall innovation levels is ambiguous—it could promote spillovers and economies of scale (Moretti 2021) while potentially curtailing early-life exposure to science and innovation for those outside these clusters, worsening the young inventor pipeline problem (Bell et al. 2019b).

Inventors and Entrepreneurship

New firm creation drives radical innovation (Acemoglu and Cao 2015; Decker et al. 2017; Akcigit and Kerr 2018). To investigate the trajectory of entrepreneurial activity of inventors

[21] Akcigit and Goldschlag (2025) do not report job-to-job mobility but job hire and separation rates. While these do not allow me to recover true job mobility, they provide at least an upper bound of job mobility, i.e., $0 \leq J \leq \min\{H, S\}$ where J are job-to-job moves, N are hires from non-employment and L are separations to non-employment.

in Germany, I build on the approach suggested by Akcigit and Goldschlag (2025). Specifically, I run a regression of a dummy for entrepreneurial activity on year dummies (with 2000 being the reference period) and individual inventor fixed effects, i.e.,

$$Entrep_{i,t} = \alpha + \sum_{k=2001}^{2014} \beta_k D^k + \psi_i + \epsilon_{i,t} \tag{3.2}$$

I define entrepreneurial activity, $Entrep_{i,t}$, broadly as inventors working at firms that are three years old or younger and that are in the top 25th percentile of the firms earning's distribution. This definition slightly deviates from Akcigit and Goldschlag (2025) who classify inventors as founders if they have positive earnings in the first quarter of a firm's operation and are among the top three workers in the firm's first year. However, this potentially undercounts entrepreneurial activity. First, founders often do not pay themselves top salaries in the first year; they prioritize hiring, and their main upside comes from equity.[22] Second, inventors who formally join one or two years after founding may receive sizable equity grants that effectively make them co-entrepreneurs.[23] Finally, non-compete agreements can delay when de facto co-entrepreneurs join the new venture; some must observe a cooling-off ("garden leave") period before starting.[24]

German entrepreneurial activity among inventors fell significantly between 2000 and 2011, falling approximately 70% from 10% to 3% for both superstar and non-superstar inventors (Figure C.20). The United States experienced a similar though less severe decline: Akcigit and Goldschlag (2025) document drops of 41% for non-superstar inventors and 57% for superstars over a comparable period. This convergence suggests that despite methodological variations[25], both economies witnessed a genuine and substantial retreat from entrepreneurship among their most innovative workers.

[22]Fast Company, Startup Founder Salary/CEO Report (2023); Pilot, Founder Salary Report (2023).
[23]Silicon Valley Bank (2025).
[24]See Jeffers (2024) and Epstein Becker Green (2020).
[25]Reassuringly, when replicating my results using their classification, patterns are extremely similar.

3.4 Conclusion

This paper provides a comprehensive analysis of German inventor demographics and employment dynamics using administrative data, enabling direct comparison with U.S. patterns. The findings reveal that while both innovation systems face similar challenges—aging inventor populations, persistent gender gaps, declining mobility, and concentration at incumbent firms—institutional differences shape distinct patterns in talent allocation and productivity.

Three key differences emerge between the countries. First, Germany's innovation system operates with minimal foreign-born talent (under 5% of inventors), contrasting sharply with the U.S. where foreign-born inventors comprise over 30% of the workforce and concentrate in high-growth sectors. This difference appears increasingly consequential as China- and India-born inventors drive U.S. innovation in frontier technologies while Germany's limited foreign talent clusters in traditional industrial strengths. Second, the relationship between firm characteristics and inventor productivity differs fundamentally: U.S. inventors at young firms consistently outperform those at established firms regardless of size, while German inventors' productivity depends more on firm size than age, with medium-sized firms showing highest productivity. Third, the earnings-productivity nexus is markedly stronger in the United States, where citations increase monotonically with income, while Germany shows a weaker and non-monotonic relationship, suggesting different incentive structures shape innovative effort.

Common trends across both countries point to declining dynamism in innovation labor markets. Job mobility fell by approximately 40% in both nations, entrepreneurial activity collapsed—particularly dramatically in Germany where rates fell 70%—and geographic concentration increased while cross-regional mobility declined. The share of inventors working at young firms halved in both countries, but the composition of this shift differs importantly: in the U.S., young firms saw their share of superstar inventors decline most sharply, meaning top talent became increasingly scarce at these firms over time. In Germany, the decline was most pronounced among ordinary inventors. These different patterns of representation at young firms carry distinct implications: the U.S. concentration of superstar inventors at established firms suggests a potential misallocation of top talent toward less productive environments, while in Germany, the increasing concentration of ordinary inventors at medium-sized enterprises may actually place them in more productive settings.

These patterns suggest that innovation slowdowns in advanced economies stem from both common structural trends and country-specific institutional factors. Policy responses should therefore be tailored to national contexts: the U.S. might focus on reversing the concentration of top talent at incumbent firms, while Germany faces the more fundamental challenge of attracting and integrating foreign talent while strengthening its venture capital ecosystem to support young firm innovation. Both countries should address declining labor market dynamism that impedes the knowledge flows and entrepreneurial experimentation essential for technological progress.

While this paper documents meaningful differences in inventor labor market characteristics and dynamics between Germany and the United States, several important questions remain unexplored. First, future research could develop direct, structural measures of wedges in inventor labor markets. Following Hsieh and Klenow (2009), one could estimate differences between a benchmark marginal revenue product of labor for inventor labor and expected total compensation across firm types, regions, and demographic groups, and test whether these wedges are associated with inefficient sorting of inventor talent. Persistent wedges—especially if they correlate with barriers to mobility or predicted reallocation gains—would be consistent with distortions that reduce aggregate innovative efficiency. Cross-country comparisons could then relate institutional differences to the dispersion of wedges and the implied aggregate innovation losses under the maintained model.

Second, identifying the causes of any observed misallocation represents a crucial next step. One promising avenue examines whether non-compete agreements (NCAs) contribute to talent misallocation by restricting inventor mobility. Large incumbent firms may strategically deploy NCAs to prevent high-productivity inventors from joining or founding competing ventures, effectively trapping talent in less productive environments. Exploiting state-level variation in NCA enforcement—particularly natural experiments such as state supreme court rulings or legislative reforms—could reveal whether stricter enforcement reduces mobility from large to small firms and whether this coincides with declining innovation output at young ventures.

Third, the mechanisms linking financial market development to innovative entrepreneurship require deeper examination. Germany's weaker venture capital ecosystem may explain why inventor productivity peaks at medium-sized established firms rather than at young ventures, but we lack direct evidence on how capital constraints shape inventor sorting across firm types. Micro-level data linking inventor movements to financing events—such as venture capital rounds, IPOs, or acquisitions—could illuminate whether financial frictions prevent high-potential inventors from pursuing entrepreneurial opportunities or joining startups.

Finally, the role of immigration policy in shaping innovation outcomes deserves systematic study. Germany's limited success attracting foreign-born inventors, particularly from high-growth regions like China and India, may reflect restrictive visa policies, language barriers, or inadequate integration support. Comparing inventor migration responses to policy changes—such as Germany's skilled immigration reforms or H-1B visa restrictions in the United States—could identify which policy levers most effectively attract innovative talent and whether such talent generates knowledge spillovers that benefit domestic inventors.

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A. Appendix to Chapter 1

A.1 Tables

Table A.1: Summary Statistics for Inventors

	Mean	Median	S.D.
Patent Applications	5.42	2	9.19
Number of Jobs	3.29	3	1.85
Female Share	0.15	0	0.36
German Nationality	0.77	1	0.42
Years in Sample	12.82	14	3.48
Observations		11,169	

Notes: This table shows basic summary statistics for inventors whose career start falls between 1999 and 2011. *Patent Applications* refer to the total number of patents an inventor files while in the sample. *Number of Jobs* refers to the total number of jobs inventors held in the sample period.

Table A.2: Yearly Firm Patents by Innovative Scale

Firm Innovative Scale Groups	Mean Yearly Patents	Min Yearly Patents	Max Yearly Patents	Observations
Year 2001				
Q0	0	0	0	21,796
Q1	1	1	1	2,627
Q2	2	2	2	1,018
Q3	3.37	3	4	845
Q4*	8.99	5	20	928
P95*	36.45	21	74	212
P100	213.57	75	908	56
Year 2006				
Q0	0	0	0	16,536
Q1	1	1	1	2,856
Q2	2	2	2	1,018
Q3	3.36	3	4	875
Q4*	9.05	5	20	1,040
P95*	39.24	21	88	234
P100	200.93	89	654	60
Year 2011				
Q0	0	0	0	19,773
Q1	1	1	1	1,733
Q2	2	2	2	626
Q3	3.36	3	4	535
Q4*	9.37	5	22	697
P95*	41.77	23	98	150
P100	236.89	101	667	37

Notes: This table presents patent thresholds for firm innovative scale groups. Q0 includes all firms with zero patents in a given year. For firms with positive patent output, quartiles are formed: Q1, Q2, and Q3 represent the first, second, and third quartiles, respectively. The fourth quartile is further divided into three groups: Q4* (75th–95th percentile), P95* (95th–99th percentile), and P100 (top percentile).

Table A.3: Firm Characteristics by Innovative Scale

Firm Size						
	2001		2006		2011	
Innovative Scale	Mean	Median	Mean	Median	Mean	Median
Q0	148	46	154	50	169	57
Q1	269	95	229	89	313	113
Q2	362	146	331	121	408	169.5
Q3	478	227	375	177	590	275
Q4*	818	449	823	434	825	386.5
P95*	2145	1431	1874	1337	1809	1266
P100	8053	4040	7630	3271	6859	3357

Average Deflated Imputed Firm Wage						
	2001		2006		2011	
Innovative Scale	Mean	Median	Mean	Median	Mean	Median
Q0	124	116	130	121	144	130
Q1	138	128	139	129	148	136
Q2	147	133	148	135	159	144
Q3	150	139	152	138	167	146
Q4*	157	143	162	148	172	155
P95*	184	163	180	167	189	179
P100	198	187	210	199	225	218

Firm Age						
	2001		2006		2011	
Innovative Scale	Mean	Median	Mean	Median	Mean	Median
Q0	13.5	11	15.4	13	18.3	16
Q1	15.2	15	16.7	14	20.3	19
Q2	15.4	16	16.8	14	21.8	21
Q3	16.0	19	17.4	16	21.9	21
Q4*	16.3	22	19.8	26	23.1	23
P95*	19.5	26	21.4	30	26.1	36
P100	20.7	26	23.8	30	29.5	36

Notes: This table presents firm characteristics by firm innovative scale and year. Q0 includes all firms with zero patents in a given year. For firms with positive patent output, quartiles are formed: Q1, Q2, and Q3 represent the first, second, and third quartiles, respectively. The fourth quartile is further divided into three groups: Q4* (75th–95th percentile), P95* (95th–99th percentile), and P100 (top percentile). *Firm Size* is measured by the number of employees a firm employs. *Average Deflated Imputed Firm Wage* is the average deflated imputed wage the firm pays to all full-time employees (wages are censored and top-coded).

Table A.4: Career Starts and First Invention by Innovative Scale of Firms

	Share of 1^{st} Employment	Share of 1^{st} Invention
Q0	44%	0%
Q1	5%	17%
Q2	3%	8%
Q3	5%	9%
Q4*	20%	26%
P95*	12%	20%
P100	10%	20%

Notes: This table shows where inventors start their careers and where they create their first patent. Q0 includes all firms with zero patents in a given year. For firms with positive patent output, quartiles are formed: Q1, Q2, and Q3 represent the first, second, and third quartiles, respectively. The fourth quartile is further divided into three groups: Q4* (75th–95th percentile), P95* (95th–99th percentile), and P100 (top percentile).

Table A.5: Transition Probabilities: First Employer to Employer of First Patent

	Firm of 1^{st} Patent						
First Firm	Q0	Q1	Q2	Q3	Q4*	P95*	P100
Q0	0%	20%	9%	10%	25%	21%	14%
Q1	0%	30%	9%	12%	27%	13%	10%
Q2	0%	12%	20%	12%	35%	12%	9%
Q3	0%	8%	8%	20%	39%	12%	13%
Q4*	0%	7%	4%	7%	45%	22%	15%
P95*	0%	5%	3%	3%	18%	52%	19%
P100	0%	3%	2%	2%	8%	15%	69%

Notes: This table shows transition probabilities between inventors' first employer and the firms of their first patent application. Q0 includes all firms with zero patents in a given year. For firms with positive patent output, quartiles are formed: Q1, Q2, and Q3 represent the first, second, and third quartiles, respectively. The fourth quartile is further divided into three groups: Q4* (75th–95th percentile), P95* (95th–99th percentile), and P100 (top percentile).

Table A.6: Transition Probabilities: Firm of First Patent to Firm post 1^{st} Patent

	Firm post 1^{st} Patent						
Firm of 1^{st} Pat	Q0	Q1	Q2	Q3	Q4*	P95*	P100
Q0	0%	0%	0%	0%	0%	0%	0%
Q1	53%	9%	5%	6%	14%	7%	6%
Q2	47%	8%	6%	6%	19%	9%	5%
Q3	39%	9%	7%	7%	20%	11%	6%
Q4*	32%	9%	7%	8%	22%	13%	9%
P95*	26%	9%	4%	7%	22%	18%	15%
P100	16%	6%	2%	4%	14%	21%	38%

Notes: This table shows transition probabilities between the firm in which inventors file for their first patent and the next firm they join. Q0 includes all firms with zero patents in a given year. For firms with positive patent output, quartiles are formed: Q1, Q2, and Q3 represent the first, second, and third quartiles, respectively. The fourth quartile is further divided into three groups: Q4* (75th–95th percentile), P95* (95th–99th percentile), and P100 (top percentile).

Table A.7: Inventor Career Characteristics by Innovative Scale of First Employer

	Q0	Q1	Q2	Q3	Q4*	P95*	P100
Jobs until 1st Patent							
Mean	3.25	2.10	2.11	2.00	2.03	1.84	1.74
Median	3	2	2	2	2	2	1
S.D.	1.59	1.31	1.23	1.16	1.14	1.07	1.14
N	6,554	728	527	716	3,113	1,891	1,551
Years until 1st Patent							
Mean	7.60	5.17	5.69	5.47	5.72	5.27	4.69
Median	8	5	6	5	6	5	4
S.D.	2.71	3.16	2.94	2.82	2.80	2.74	2.86
N	6,554	728	527	716	3,113	1,891	1,551
Jobs in first 10 Years							
Mean	3.95	3.22	2.95	2.93	2.92	2.62	2.64
Median	4	3	3	3	3	2	2
S.D.	1.72	1.52	1.48	1.40	1.36	1.34	1.49
N	5,851	544	393	527	2,384	1,358	1,153
Tenure until 1st Patent							
Mean	2.15	1.92	2.21	2.23	2.17	2.23	2.12
Median	2	1	2	2	2	2	2
S.D.	1.94	2.04	2.13	2.06	2.00	2.00	1.94
N	6,554	728	527	716	3,113	1,891	1,551
Pats in first 10 Years							
Mean	2.98	3.88	3.65	4.37	4.76	5.56	7.53
Median	1	2	2	2	2	3	4
S.D.	4.58	6.33	4.90	6.25	7.12	8.49	12.75
N	5,851	544	393	527	2,384	1,358	1,153

Notes: This table shows various early-career statistics of inventors by the innovative scale of their first employer. Q0 includes all firms with zero patents in a given year. For firms with positive patent output, quartiles are formed: Q1, Q2, and Q3 represent the first, second, and third quartiles, respectively. The fourth quartile is further divided into three groups: Q4* (75th–95th percentile), P95* (95th–99th percentile), and P100 (top percentile). *Jobs until 1st Patent* is the number of jobs an inventor holds until she files for her first patent. *Years until 1st Patent* is the number of years from career start to the application for the first patent. *Jobs in first 10 Years* is the number of jobs held in the first ten career years. *Tenure until 1st Patent* is the number of years it takes to file the first patent *conditional* on being employed at the firm of the first patent. *Pats in first 10 Years* is the number of patent applications an inventor creates in the first ten years of her career.

Table A.8: Innovation Output by Innovative Scale of First Employer

Years after Career Start	7 Years	8 Years	9 Years	10 Years
Patent Applications				
Q1	0.2676***	0.1967**	0.1565*	0.1151
	(0.087)	(0.086)	(0.087)	(0.088)
Q2	0.1044	0.0705	0.0944	0.0811
	(0.084)	(0.087)	(0.087)	(0.087)
Q3	0.4256***	0.3973***	0.3715***	0.2872***
	(0.087)	(0.082)	(0.082)	(0.079)
Q4*	0.5129***	0.4512***	0.4024***	0.3580***
	(0.067)	(0.063)	(0.063)	(0.061)
P95*	0.6515***	0.5877***	0.5522***	0.4963***
	(0.075)	(0.071)	(0.071)	(0.070)
P100	1.0392***	0.9187***	0.8779***	0.7714***
	(0.106)	(0.101)	(0.104)	(0.099)
Patent Citations				
Q1	0.1484	0.1043	0.0824	0.0413
	(0.125)	(0.125)	(0.123)	(0.123)
Q2	0.3502**	0.2093	0.1635	0.1383
	(0.157)	(0.156)	(0.148)	(0.147)
Q3	0.4574***	0.3495***	0.2829**	0.1729
	(0.138)	(0.131)	(0.128)	(0.129)
Q4*	0.4976***	0.3807***	0.3125***	0.2973***
	(0.100)	(0.097)	(0.092)	(0.093)
P95*	0.7054***	0.5707***	0.5164***	0.4970***
	(0.118)	(0.114)	(0.109)	(0.110)
P100	1.0047***	0.8993***	0.8592***	0.8292***
	(0.137)	(0.136)	(0.131)	(0.131)
Inventor Controls	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Hire-Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Observations	10,749	10,585	10,191	9,638

Notes: This table shows the Poisson regression estimates for Equation (1.1) examining the relationship between the innovative scale, as measured by yearly patent output, of the firms at which inventors start their careers and their subsequent patent and citation productivity. Q0 includes all firms with zero patents in a given year. For firms with positive patent output, quartiles are formed: Q1, Q2, and Q3 represent the first, second, and third quartiles, respectively. The fourth quartile is further divided into three groups: Q4* (75th–95th percentile), P95* (95th–99th percentile), and P100 (top percentile). Inventor controls include age and education. Firm controls include size (number of employees), deflated imputed average firm wage, and firm age. Standard errors are in parentheses. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.9: Inventor Innovation Output by First Employer and Firm of First Patent

Years after Career Start	7 Years	8 Years	9 Years	10 Years
Patent Applications				
SM → L	0.2421***	0.2606***	0.2379***	0.3033***
	(0.063)	(0.086)	(0.059)	(0.059)
SM → XL	0.7672***	0.8061***	0.8149***	0.8777***
	(0.060)	(0.057)	(0.056)	(0.057)
L → SM	-0.0096	-0.0350	-0.0323	0.0581
	(0.103)	(0.095)	(0.091)	(0.093)
L → L	0.1683*	0.2310***	0.2281***	0.3157***
	(0.087)	(0.084)	(0.085)	(0.087)
L → XL	0.9285***	0.9317***	0.9155***	0.9919***
	(0.082)	(0.078)	(0.077)	(0.078)
XL → SM	0.0503	0.0778	0.0357	0.0838
	(0.118)	(0.122)	(0.120)	(0.121)
XL → L	0.3232***	0.3653***	0.3422***	0.4302***
	(0.104)	(0.100)	(0.100)	(0.101)
XL → XL	0.8859***	0.9236***	0.9772***	1.0757***
	(0.099)	(0.093)	(0.094)	(0.098)
Patent Citations				
SM → L	0.2681**	0.3335***	0.2974***	0.3936***
	(0.114)	(0.118)	(0.114)	(0.112)
SM → XL	0.8609***	0.9267***	0.9490***	1.0491***
	(0.107)	(0.109)	(0.105)	(0.103)
L → SM	-0.3026	-0.2515	-0.2137	-0.0602
	(0.199)	(0.194)	(0.178)	(0.184)
L → L	0.1086	0.2180	0.2108	0.3270**
	(0.172)	(0.163)	(0.159)	(0.158)
L → XL	0.9833***	0.9934***	0.9929***	1.1169***
	(0.140)	(0.140)	(0.135)	(0.135)
XL → SM	-0.0468	0.0899	0.1110	0.2996
	(0.245)	(0.223)	(0.224)	(0.212)
XL → L	0.4664***	0.5590***	0.6064***	0.6837***
	(0.179)	(0.177)	(0.174)	(0.175)
XL → XL	0.9313***	1.0144***	1.0867***	1.2277***
	(0.159)	(0.157)	(0.152)	(0.149)
Inventor Controls	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Hire-Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Observations	7,171	7,215	7,084	6,798

Notes: This table shows the Poisson regression estimates for Equation (1.2) examining the relationship of individual inventor productivity and employment history. Firms are categorized into three groups: (1) *SM*, small- to medium-scale innovative firms including Q0, Q1, Q2, and Q3; (2) *L*, large-scale firms including Q4*; and (3) *XL*, mega-scale firms including P95* and P100. Q0 includes all firms with zero patents in a given year. For firms with positive patent output, quartiles are formed: Q1, Q2, and Q3 are the first, second, and third quartiles. The fourth quartile is split into Q4* (75th–95th percentile), P95* (95th–99th percentile), and P100 (top percentile). The analysis is restricted to inventors who do not patent in their first job. The first firm group (left of the arrow) indicates the firm type where an inventor starts her career; the second firm group indicates the firm type where she creates her first patent. Coefficients are semi-elasticities relative to starting one's career at SM firms and creating one's first patent there. Inventor controls include age and education. Firm controls include size (number of employees), deflated imputed average firm wage, and firm age. Standard errors are in parentheses. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.10: Number of Co-inventors by Innovative Scale of First Employer

Years after Career Start	7 Years	8 Years	9 Years	10 Years
Collaborations				
Q1	0.1122	0.1612	0.1589	0.1464
	(0.112)	(0.115)	(0.114)	(0.114)
Q2	-0.0075	0.0673	0.0771	0.1030
	(0.104)	(0.109)	(0.110)	(0.110)
Q3	0.3156***	0.3031***	0.3070***	0.2829***
	(0.097)	(0.093)	(0.092)	(0.094)
Q4*	0.3529***	0.3568***	0.3422***	0.3282***
	(0.078)	(0.074)	(0.075)	(0.073)
P95*	0.3980***	0.4296***	0.4352***	0.4169***
	(0.094)	(0.088)	(0.087)	(0.084)
P100	0.7961***	0.7900***	0.7694***	0.7081***
	(0.135)	(0.130)	(0.129)	(0.121)
Inventor Controls	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Hire-Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Observations	7,841	8,616	9,080	9,100

Notes: This table shows the Poisson regression estimates for Equation (1.3) examining the relationship between the innovative scale, as measured by yearly patent output, of the firms at which inventors start their careers and the total number of their co-inventors in the first years of their careers. Q0 includes all firms with zero patents in a given year. For firms with positive patent output, quartiles are formed: Q1, Q2, and Q3 represent the first, second, and third quartiles, respectively. The fourth quartile is further divided into three groups: Q4* (75th–95th percentile), P95* (95th–99th percentile), and P100 (top percentile). Inventor controls include age and education. Firm controls include size (number of employees), deflated imputed average firm wage, and firm age. Standard errors are in parentheses. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.11: Inventor Productivity and Innovation Clusters

Years after Career Start	7 Years	8 Years	9 Years	10 Years
Patent Applications				
SF	0.4166***	0.3649***	0.3682***	0.3579***
	(0.093)	(0.082)	(0.079)	(0.083)
IC	0.0195	0.0344	0.0184	0.0093
	(0.046)	(0.043)	(0.041)	(0.040)
SF $\times$ IC	-0.1020	-0.0718	-0.0569	-0.0750
	(0.104)	(0.094)	(0.091)	(0.095)
Inventor Controls	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Hire-Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Observations	10,749	10,585	10,191	9,638

Notes: This table shows the Poisson regression estimates for Equation (1.4) examining the relationship between career starts in innovation clusters and the total number of patent applications in the first years of an inventor's career. *IC* is a dummy variable that equals 1 if the inventor starts her career in an innovation-cluster county, defined as a county with at least two firms in the top percentile of innovative scale (measured by total yearly patent output). *SF* is a dummy that equals 1 if an inventor starts her career at a star firm. Star firms are firms in the top percentile of innovative scale. Inventor controls include age and education. Firm controls include size (number of employees), deflated imputed average firm wage, and firm age. Standard errors are in parentheses. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.12: Share of Trainee Inventors by Technology Class

Technology	Share of trainee inventors among all inventors	Share of trainee inventors among star inventors
Electrical Engineering	27%	10%
Instruments	24%	8%
Chemical/Pharma	19%	4%
Process Engineering/Spec. Equipment	27%	8%
Mechanical Engineering	30%	13%
Civil Engineering	32%	16%
Total	27%	9%

Notes: The table reports the share of trainee inventors within each technology class. Column (1) shows the fraction of all inventors in the class who are classified as trainee inventors; Column (2) shows the corresponding fraction among star inventors.

Table A.13: Access to Innovative Firms and Inventor Productivity of Trainees

	All Counties				Only Larger Counties			
Years after Career Start	7 Years	8 Years	9 Years	10 Years	7 Years	8 Years	9 Years	10 Years
Patent Applications								
Star County	0.2203**	0.2148**	0.1927**	0.1582*	0.3126***	0.3034***	0.3016***	0.2393**
	(0.098)	(0.086)	(0.083)	(0.086)	(0.114)	(0.101)	(0.096)	(0.097)
log(gdp/capita)	-0.0493	-0.0250	-0.0651	-0.0509	-0.2501	-0.2066	-0.1892	-0.1103
	(0.164)	(0.143)	(0.131)	(0.128)	(0.174)	(0.156)	(0.151)	(0.146)
Inventor Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Hire-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,718	2,801	2,781	2,674	1,839	1,883	1,813	1,745

Notes: This table shows the Poisson regression estimates for Equation (1.5) examining the relationship between apprentices' access to star firms (i.e., firms in the top percentile of patent output) and early-career productivity. *Star County* equals 1 if the inventor starts her career in a county with at least one star firm. *log(gdp/capita)* is the natural logarithm of counties' GDP per capita. The first panel uses all counties; the second panel drops the 25% smallest counties to mitigate the risk of leavers. Inventor controls include age. Standard errors are in parentheses. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

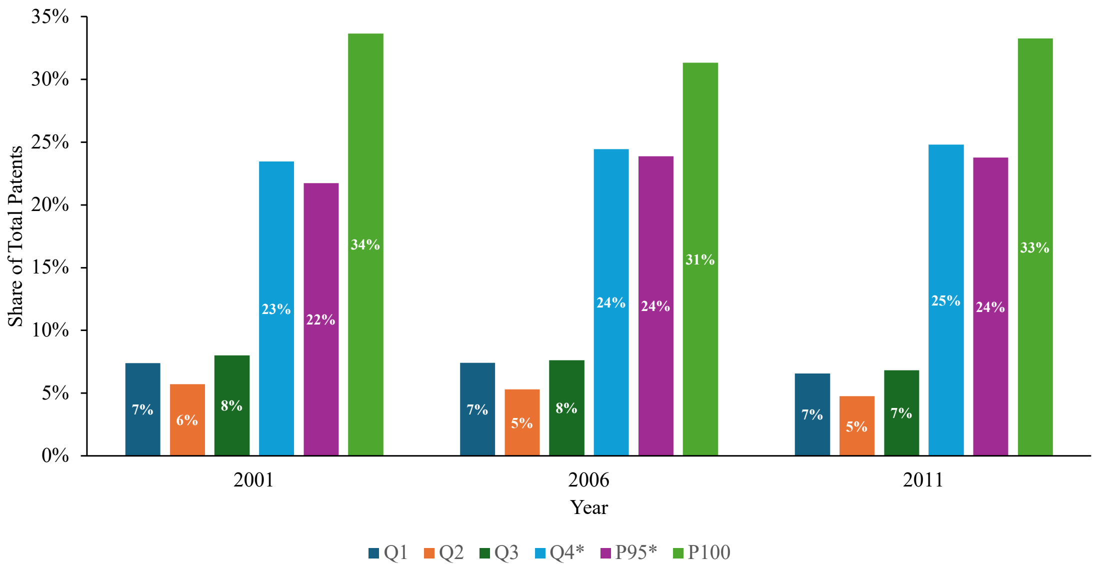
Table A.14: Access to Innovative Firms and Inventor Productivity of Trainees: Star Inventors

Years after Career Start	7 Years	8 Years	9 Years	10 Years
Patent Applications				
Star County	0.7083***	0.7045***	0.6706***	0.5931***
	(0.158)	(0.125)	(0.130)	(0.128)
log(gdp/capita)	0.0040	-0.0293	-0.2468	-0.2825
	(0.244)	(0.230)	(0.239)	(0.231)
Inventor Controls	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes
Hire-Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Observations	400	400	400	400

Notes: This table shows the Poisson regression estimates for Equation (1.5) examining the relationship between apprentices' access to star firms (i.e., firms in the top percentile of patent output) and early-career productivity. The analysis is restricted to *star inventors*, defined as the 200 most successful apprentices (by 7–10 year patent output) in each county type ("star" and "non-star" counties). *Star County* equals 1 if the inventor starts her career in a county with at least one star firm. *log(gdp/capita)* denotes the natural logarithm of counties' GDP per capita. Inventor controls include age. Standard errors are in parentheses. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

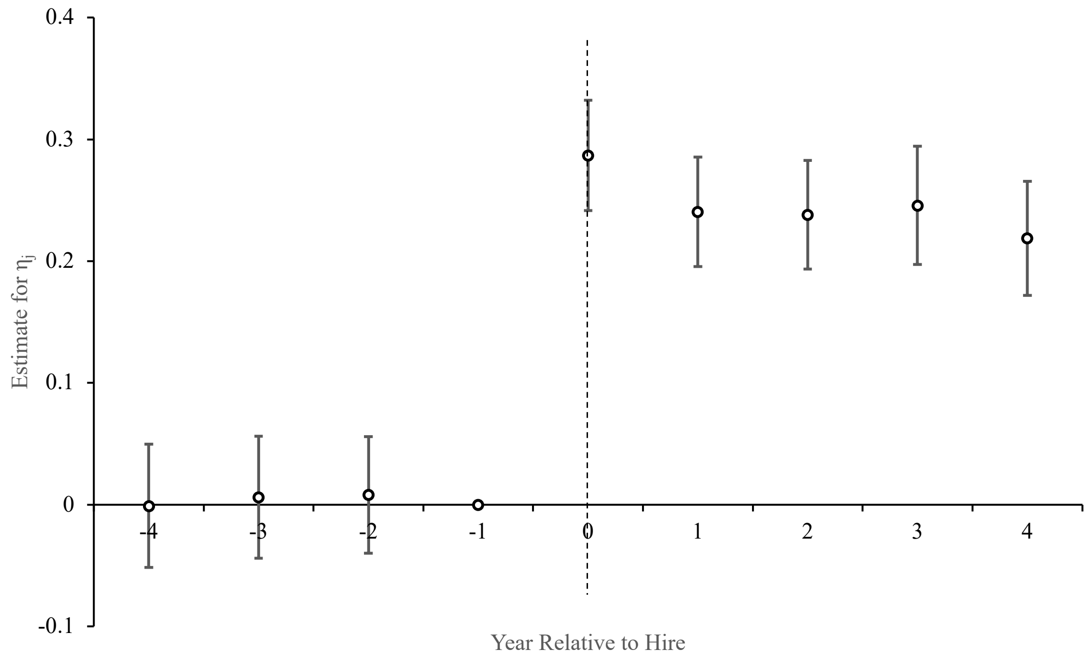
A.2 Figures

Figure A.1: Total Patent Output and Innovative Scale



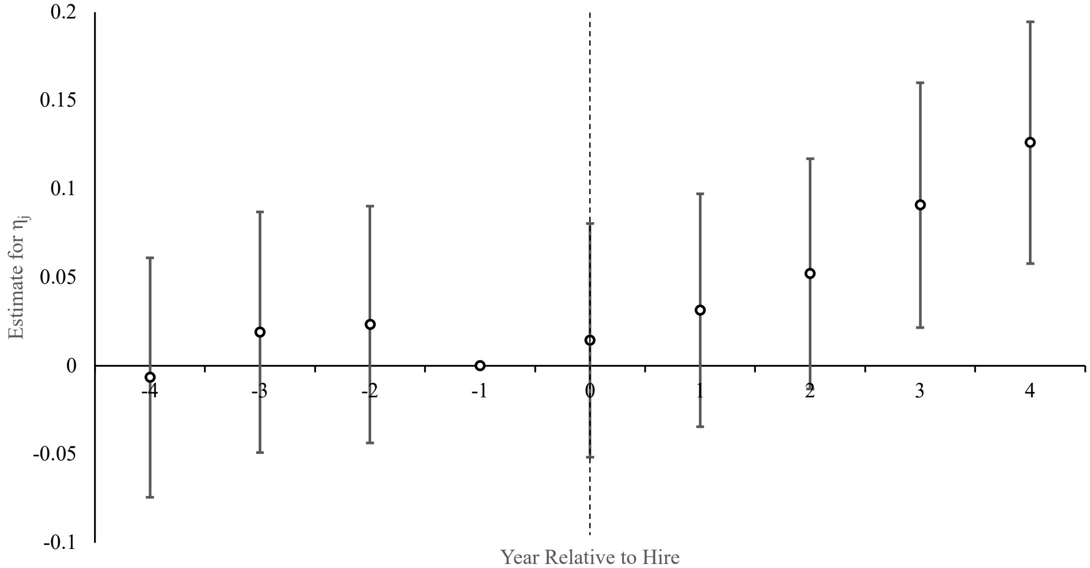
Notes: This figure shows the share in total patent output of firms with different levels of innovative scale. Q0 includes all firms with zero patents in a given year and hence is omitted. For firms with positive patent output, quartiles are formed: Q1, Q2, and Q3 represent the first, second, and third quartiles, respectively. The fourth quartile is further divided into three groups: Q4* (75th–95th percentile), P95* (95th–99th percentile), and P100 (top percentile)

Figure A.2: Large-Scale vs. Small-Scale Innovative Firms – Inventor Productivity Differences after Hire



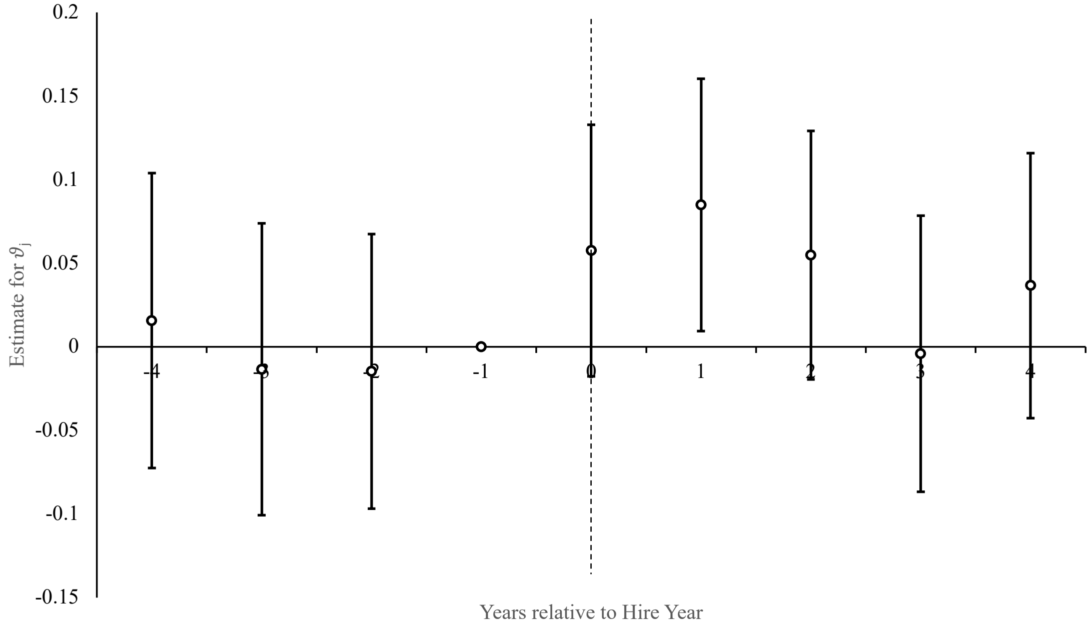
Notes: This figure shows event–study estimates for the differences in patent applications between inventors joining large-scale innovative firms (firms in the 4^{th} quartile of yearly patent output) and low-scale ones ($< 4^{\text{th}}$ quartile). Inventors are matched based on pre-hire characteristics. The event-study design is based on Akcigit and Goldschlag (2023). The figure shows estimates for η_j from Equation (1.6) with point estimates represented by circles and 95%-confidence intervals by vertical lines.

Figure A.3: Incumbent vs. Young Firms – Inventor Productivity Differences after Hire



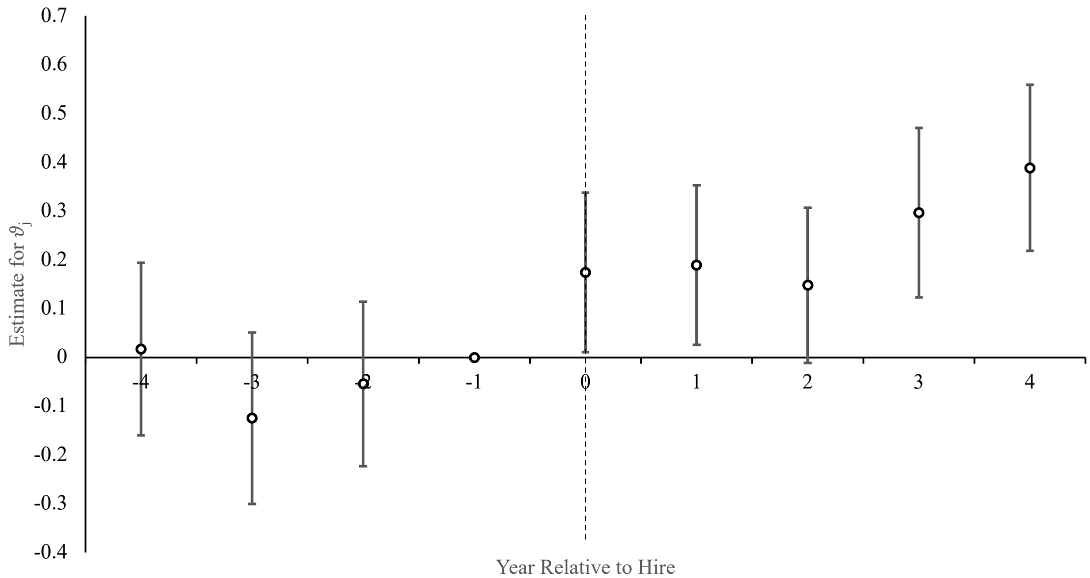
Notes: This figure shows event study estimates for the differences in patent applications between inventors joining incumbent firms (firms with > 1000 employees and older 20 years) and young firms ($\leq$ 5 years old). Inventors are matched based on pre-hire characteristics. The firm categories and event study design are based on Akcigit and Goldschlag (2023). The figure shows estimates for η_j for a regression similar to Equation (1.6) but now analysing moves to incumbent firms rather than large-scale innovative firms. Point estimates are represented by circles and 95%-confidence intervals by vertical lines.

Figure A.4: Innovative Scale, Access to Financing (Collateral) and Inventor Productivity



Notes: This figure shows event–study estimates for the differences in patent applications between inventors joining large-scale innovative firms (firms in the 4$^{\text{th}}$ quartile of yearly patent output) and low-scale ones (< 4$^{\text{th}}$ quartile) in high- vs. low-collateral industries. Inventors are matched based on pre-hire characteristics. The event–study design follows Akcigit and Goldschlag (2023). The figure shows estimates for ϑ_j from Equation (1.7). Point estimates are represented by circles and 95%-confidence intervals by vertical lines.

Figure A.5: Innovative Scale, Access to Financing (External Dependence) and Inventor Productivity



Notes: This figure shows event–study estimates for the differences in patent applications between inventors joining large-scale innovative firms (firms in the 4$^{\text{th}}$ quartile of yearly patent output) and low-scale ones ($<$ 4$^{\text{th}}$ quartile) in industries with high vs. low dependence on external financing (see Rajan and Zingales (1998)). Inventors are matched based on pre-hire characteristics. The event–study design follows Akcigit and Goldschlag (2023). The figure shows estimates for ϑ_j from Equation (1.8). Point estimates are represented by circles and 95%-confidence intervals by vertical lines.

B. Appendix to Chapter 2

B.1 Tables

Table B.1: List of Counties and STR Regulation Introduction Years

County	State	Year of Regulation
Los Angeles	California	2015
Orange	California	2014
Riverside	California	2014
San Francisco	California	2015
Sonoma	California	2017
Boulder	Colorado	2016
Denver	Colorado	2017
Miami-Dade	Florida	2010
Chatham	Georgia	2015
Cook	Illinois	2016
Clark	Nevada	2010
Buncombe	North Carolina	2015
Multnomah	Oregon	2014
Davidson	Tennessee	2015
Travis	Texas	2012
King	Washington	2017
Teton	Wyoming	2015

Notes: The table reports U.S. counties that became subject to short-term rental (STR) regulations, along with the year the rules were first enacted, sourced from local ordinances and municipal records.

Table B.2: Summary of Variables

Sector/Control Variables	Description	Source
Real Estate		
Price of 2-Bedroom Homes	Typical value for 2-bedroom homes in the 35th to 65th percentile range in Dollars	Zillow
Price of 3-Bedroom Homes	Typical value for 3-bedroom homes in the 35th to 65th percentile range in Dollars	Zillow
Price of 5-Bedroom Homes	Typical value for 5-bedroom homes in the 35th to 65th percentile range in Dollars	Zillow
Price of all Homes	Typical value for all homes in the 35th to 65th percentile range in Dollars	Zillow
GDP		
Accommodation & Food Services	GDP per capita for accommodation and food services (in $1,000s)	BEA
Arts, Entertainment & Recreation	GDP per capita for arts, entertainment, and recreation (in $1,000s)	BEA
Construction	GDP per capita for construction sector (in $1,000s)	BEA
Real Estate, Rental & Leasing	GDP per capita for real estate, rental, and leasing (in $1,000s)	BEA
Personal Income		
Accommodation	Personal income per capita in accommodation sector (in $1,000s)	BEA
Food Services and Drinking Places	Personal income per capita in food and drink establishments (in $1,000s)	BEA
Arts, Entertainment & Recreation	Personal income per capita in arts, entertainment, and recreation (in $1,000s)	BEA
Construction	Personal income per capita in construction sector (in $1,000s)	BEA
Real Estate, Rental and Leasing	Personal income per capita in real estate, rental and leasing (in $1,000s)	BEA
County Revenue		
Property Tax Revenue	Property tax revenue per capita of county (in $1,000s)	U.S. Census Bureau
Job Share		
Accommodation & Food Services	#People employed in Accomm. & Food divided by total employment	BEA
Arts, Entertainment & Recreation	#People employed in Arts, Entertain. & Rec. divided by total employment	BEA
Construction	#People employed in Construction divided by total employment	BEA
Real Estate, Rental and Leasing	#People employed in Real Estate, Rental & Leasing divided by total employment	BEA
Control Variables		
College Education (2000)	Share of population with bachelor's degree or higher (2000)	USDA ERS
College Education (2008-2010)	Share of population with bachelor's degree or higher (2008–2012)	USDA ERS
Unemployment Rate	#People unemployed divided by the total labor force	U.S. Bureau of Labor Statistics
Aggregate Personal Income	Total (including all sectors) personal income per capita (in $1,000s)	BEA
Population	County population	BEA
Population Density	Population divided by county land area	U.S. Census Bureau

Notes: This table lists all variables used in the analysis, organized by economic sector. Monetary figures are in current dollars unless otherwise noted. All variable values are at the county level.

Table B.3: Covariate Balance For House Price Regressions Across Home Types

	All Homes			2-Bedroom			3-Bedroom			5-Bedroom		
Variable	Non-Ad.	Adopt	Norm. Diff.	Non-Ad.	Adopt	Norm. Diff.	Non-Ad.	Adopt	Norm. Diff.	Non-Ad.	Adopt	Norm. Diff.
Personal Income per Capita	47231	53492	0.28	47639	53492	0.26	47394	53492	0.28	47981	53492	0.24
Bachelor's Degree (2000)	0.32	0.33	0.06	0.32	0.33	0.03	0.32	0.33	0.04	0.30	0.33	0.22
Bachelor's Degree (2008–2012)	0.36	0.38	0.24	0.36	0.38	0.18	0.36	0.38	0.15	0.35	0.38	0.30
Unemployment Rate	0.05	0.05	0.06	0.05	0.05	0.07	0.05	0.05	0.01	0.05	0.05	0.18
Population Density	686.25	2516.45	0.62	562.30	2516.45	0.66	702.39	2516.45	0.61	613.63	2516.45	0.64

Notes: The table compares pre-treatment covariates for counties that adopt short-term rental (STR) regulations ("Adopt") and their matched controls ("Non-Ad."). Controls are chosen from the same state and must fall in the same quintile of the relevant outcome (home price for All Homes, 2-Bedroom, etc.) measured five years prior to adoption. Normalized difference = $(\text{Mean}_{\text{Adopt}} - \text{Mean}_{\text{Non-Adopt}}) / \sqrt{(\text{Var}_{\text{Adopt}} + \text{Var}_{\text{Non-Adopt}})/2}$. Values > 0.25 in absolute value may indicate imbalance (Imbens and Rubin 2015).

Table B.4: Covariate Balance For GDP Per Capita Regressions Across Sectors

	Accom & Food			Construction			Real Estate			Arts & Rec.		
Variable	Non-Ad.	Adopt	Norm. Diff.	Non-Ad.	Adopt	Norm. Diff.	Non-Ad.	Adopt	Norm. Diff.	Non-Ad.	Adopt	Norm. Diff.
Personal Income per Capita	44761	52596	0.36	43410	53492	0.46	46990	52596	0.26	46440	52596	0.28
Bachelor's Degree (2000)	0.29	0.32	0.22	0.27	0.33	0.67	0.31	0.32	0.08	0.30	0.32	0.11
Bachelor's Degree (2008–2012)	0.33	0.37	0.40	0.30	0.38	0.83	0.35	0.37	0.20	0.34	0.37	0.24
Unemployment Rate	0.06	0.05	−0.04	0.06	0.05	−0.22	0.06	0.05	−0.01	0.06	0.05	−0.08
Population Density	523.57	2382.68	0.64	561.38	2516.45	0.66	593.97	2382.68	0.62	670.13	2382.68	0.59

Notes: The table compares pre-treatment covariates for counties that adopt short-term rental (STR) regulations ("Adopt") and their matched controls ("Non-Ad."). Controls are chosen from the same state and must fall in the same quintile of the relevant outcome (GDP per capita in Accom & Food, Construction, etc.) measured five years prior to adoption. Normalized difference = $(\text{Mean}_{\text{Adopt}}$ - $\text{Mean}_{\text{Non-Adopt}})$ / $\sqrt{(\text{Var}_{\text{Adopt}} + \text{Var}_{\text{Non-Adopt}})/2}$. Values > 0.25 in absolute value may indicate imbalance (Imbens and Rubin 2015).

Table B.5: Covariate Balance For Personal Income Per Capita Regressions Across Sectors

	Accommodation			Food			Construction			Real Estate			Arts & Rec.		
Variable	Non-Ad.	Adopt	Norm. Diff.	Non-Ad.	Adopt	Norm. Diff.	Non-Ad.	Adopt	Norm. Diff.	Non-Ad.	Adopt	Norm. Diff.	Non-Ad.	Adopt	Norm. Diff.
Personal Income per Capita	42908	52596	0.46	45822	52596	0.31	42969	53492	0.49	46542	52596	0.27	44471	52596	0.37
Bachelor's Degree (2000)	0.26	0.32	0.57	0.30	0.32	0.18	0.27	0.33	0.66	0.29	0.32	0.23	0.30	0.32	0.15
Bachelor's Degree (2008–2012)	0.30	0.37	0.72	0.33	0.37	0.35	0.30	0.38	0.85	0.33	0.37	0.35	0.34	0.37	0.32
Unemployment Rate	0.06	0.05	−0.10	0.06	0.05	−0.10	0.06	0.05	−0.16	0.06	0.05	−0.18	0.06	0.05	−0.02
Population Density	485.60	2382.68	0.65	612.23	2382.68	0.61	408.99	2516.45	0.71	554.87	2382.68	0.63	558.95	2382.68	0.63

Notes: The table compares pre-treatment covariates for counties that adopt short-term rental (STR) regulations ("Adopt") and their matched controls ("Non-Ad."). Controls are chosen from the same state and must fall in the same quintile of the relevant outcome (personal income per capita in Accom, Food, Construction, etc.) measured five years prior to adoption. Normalized difference = $(\text{Mean}_{\text{Adopt}}$ - $\text{Mean}_{\text{Non-Adopt}})$ / $\sqrt{(\text{Var}_{\text{Adopt}} + \text{Var}_{\text{Non-Adopt}})/2}$. Values > 0.25 in absolute value may indicate imbalance (Imbens and Rubin 2015).

Table B.6: Covariate Balance For Employment Share Regressions Across Sectors

	Accom & Food			Construction			Real Estate			Arts & Rec.		
Variable	Non-Ad.	Adopt	Norm. Diff.	Non-Ad.	Adopt	Norm. Diff.	Non-Ad.	Adopt	Norm. Diff.	Non-Ad.	Adopt	Norm. Diff.
Personal Income per Capita	41257	52596	0.55	42674	53492	0.46	46915	52596	0.26	45343	52596	0.33
Bachelor's Degree (2000)	0.26	0.32	0.62	0.26	0.33	0.68	0.30	0.32	0.17	0.31	0.32	0.07
Bachelor's Degree (2008–2012)	0.30	0.37	0.80	0.29	0.38	0.91	0.34	0.37	0.35	0.35	0.37	0.23
Unemployment Rate	0.06	0.05	−0.35	0.06	0.05	−0.41	0.06	0.05	−0.03	0.06	0.05	−0.13
Population Density	485.79	2382.68	0.66	464.40	2516.45	0.69	593.32	2382.68	0.62	574.31	2382.68	0.62

Notes: The table compares pre-treatment covariates for counties that adopt short-term rental (STR) regulations ("Adopt") and their matched controls ("Non-Ad."). Controls are chosen from the same state and must fall in the same quintile of the relevant outcome (employment share of Accom & Food, Construction, etc.) measured five years prior to adoption. Normalized difference = $(\text{Mean}_{\text{Adopt}}$ - $\text{Mean}_{\text{Non-Adopt}})$ / $\sqrt{(\text{Var}_{\text{Adopt}} + \text{Var}_{\text{Non-Adopt}})/2}$. Values > 0.25 in absolute value may indicate imbalance (Imbens and Rubin 2015).

Table B.7: Covariate balance for Property Tax Per Capita Regressions

Variable	Non-Adopt	Adopt	Norm. Diff.
Personal Income per Capita	39448	51512	0.53
Bachelor's Degree (2000)	0.23	0.30	0.78
Bachelor's Degree (2008–2012)	0.27	0.36	0.88
Unemployment Rate	0.06	0.06	−0.54
Population Density	327.49	1327.61	0.88

Notes: The table compares pre-treatment covariates for counties that adopt short-term rental (STR) regulations ("Adopt") and their matched controls ("Non-Ad."). Controls are chosen from the same state and must fall in the same quintile of the relevant outcome (i.e., property taxes per capita) measured five years prior to adoption. Normalized difference = $(\text{Mean}_{\text{Adopt}} - \text{Mean}_{\text{Non-Adopt}}) \,/\, \sqrt{(\text{Var}_{\text{Adopt}} + \text{Var}_{\text{Non-Adopt}})/2}$. Values > 0.25 in absolute value may indicate imbalance (Imbens and Rubin 2015).

Table B.8: House Price Effects

	All Homes	2-Bedroom	3-Bedroom	5-Bedroom
Pre_avg	-0.0306	-0.0763	-0.0579	-0.0444
	(-0.81)	(-1.23)	(-1.01)	(-0.99)
Post_avg	0.00772	-0.0514	0.00678	-0.0271
	(0.20)	(-0.95)	(0.09)	(-0.38)
Observations	542	531	537	528
State FE	Yes	Yes	Yes	Yes
Economic Controls	Yes	Yes	Yes	Yes

Notes: This table presents pre- and post-treatment effects aggregated over the respective windows (see Equation (2.12) and Equation (2.13)), estimated via doubly robust DiD (Callaway and Sant'Anna 2021; Sant'Anna and Zhao 2020). The outcome is the logarithm of the average home price (All Homes, 2-Bedroom, etc.), using a sample trimmed to the interquartile range (below the 25th and above the 75th percentile excluded). Covariates are measured at the county level and include the log of personal income per capita, the log of population density, the unemployment rate, and the share of residents with a bachelor's degree. The comparison group comprises not-yet-treated units and never-treated units. Standard errors are heteroskedasticity-robust and computed via influence functions. T-statistics are in parentheses. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.9: Sectoral GDP Per Capita Effects

	Accom & Food	Construction	Real Estate	Arts & Rec.
Pre_avg	-0.0772	-0.114	0.00514	-0.145^{*}
	(-0.98)	(-0.89)	(0.07)	(-1.75)
Post_avg	0.133^{*}	0.251	0.0424	-0.0253
	(1.86)	(1.03)	(0.78)	(-0.21)
Observations	645	596	611	612
State FE	Yes	Yes	Yes	Yes
Economic Controls	Yes	Yes	Yes	Yes

Notes: This table presents pre- and post-treatment effects aggregated over the respective windows (see Equation (2.12) and Equation (2.13)), estimated via doubly robust DiD (Callaway and Sant'Anna 2021; Sant'Anna and Zhao 2020). The outcome is the logarithm of sectoral county-level GDP per capita. Covariates are measured at the county level and include the log of personal income per capita, the log of population density, the unemployment rate, and the share of residents with a bachelor's degree. The comparison group comprises not-yet-treated units and never-treated units. Standard errors are heteroskedasticity-robust and computed via influence functions. T-statistics are in parentheses. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.10: Sectoral Personal Income per Capita Effects

	Accomm.	Food	Construction	Real Estate	Arts & Rec.
Pre_avg	0.282	-0.0984	-0.0135	-0.348**	-0.523***
	(1.62)	(-1.07)	(-0.11)	(-2.08)	(-3.11)
Post_avg	0.110	0.0811**	0.0834	0.00773	-0.406*
	(0.89)	(2.18)	(0.52)	(0.04)	(-1.67)
Observations	650	628	596	591	647
State FE	Yes	Yes	Yes	Yes	Yes
Economic Controls	Yes	Yes	Yes	Yes	Yes

Notes: This table presents pre- and post-treatment effects aggregated over the respective windows (see Equation (2.12) and Equation (2.13)), estimated via doubly robust DiD (Callaway and Sant'Anna 2021; Sant'Anna and Zhao 2020). The outcome is the logarithm of sectoral county-level personal income per capita. Covariates are measured at the county level and include the log of personal income per capita, the log of population density, the unemployment rate, and the share of residents with a bachelor's degree. The comparison group comprises not-yet-treated units and never-treated units. Standard errors are heteroskedasticity-robust and computed via influence functions. T-statistics are in parentheses. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.11: Sectoral Employment Shares Effects

	Accom & Food	Construction	Real Estate	Arts & Rec.
Pre_avg	0.0481	0.0104	0.0552	−0.0131
	(1.64)	(0.18)	(0.71)	(−0.36)
Post_avg	−0.125***	0.0246	−0.0203	0.0477
	(−2.75)	(0.33)	(−0.39)	(0.59)
Observations	628	579	650	611
State FE	Yes	Yes	Yes	Yes
Economic Controls	Yes	Yes	Yes	Yes

Notes: This table presents pre- and post-treatment effects aggregated over the respective windows (see Equation (2.12) and Equation (2.13)), estimated via doubly robust DiD (Callaway and Sant'Anna 2021; Sant'Anna and Zhao 2020). The outcome is the logarithm of sectoral county-level employment shares. Covariates are measured at the county level and include the log of personal income per capita, the log of population density, the unemployment rate and the share of residents with a bachelor's degree. The comparison group comprises not-yet-treated units and never-treated units. Standard errors are heteroskedasticity-robust and computed via influence functions. T-statistics are in parentheses. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.12: Property Tax per Capita Effects

	Property Tax per Capita
Pre_avg	-0.150
	(-1.63)
Post_avg	-0.0218
	(-0.21)
Observations	458
State FE	Yes
Economic Controls	Yes

Notes: TThis table presents pre- and post-treatment effects aggregated over the respective windows (see Equation (2.12) and Equation (2.13)), estimated via doubly robust DiD (Callaway and Sant'Anna 2021; Sant'Anna and Zhao 2020). The outcome is the logarithm of county-level property tax revenue per capita. Covariates are measured at the county level and include the log of personal income per capita, the log of population density, the unemployment rate, and the share of residents with a bachelor's degree. The comparison group comprises not-yet-treated units and never-treated units. Standard errors are heteroskedasticity-robust and computed via influence functions. T-statistics are in parentheses. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.13: Sectoral GDP per Capita Levels and Shares in Treated Counties (Pre-Treatment Averages)

	Accommodation & Food		Arts & Recreation		Construction		Real Estate		Total GDP
County	Absolute	Share	Absolute	Share	Absolute	Share	Absolute	Share	Absolute
Los Angeles, CA	1,430.95	2.45%	1,782.96	3.05%	1,164.94	1.99%	8,943.85	15.35%	58,342.07
Orange, CA	1,875.28	3.07%	807.14	1.32%	2,664.97	4.35%	10,849.46	17.74%	61,097.79
Riverside, CA	1,057.54	3.74%	258.87	0.92%	1,550.43	5.48%	6,095.04	21.60%	28,252.84
San Francisco, CA	5,077.60	3.84%	2,571.23	1.94%	2,856.69	2.13%	16,807.27	12.62%	133,187.50
Sonoma, CA	1,837.65	3.59%	596.43	1.17%	3,102.90	6.06%	9,029.95	17.77%	50,938.84
Boulder, CO	1,855.06	2.64%	612.04	0.87%	1,479.69	2.10%	9,498.32	13.57%	70,116.23
Denver, CO	3,255.74	3.46%	1,743.13	1.84%	2,950.79	3.11%	10,689.74	11.39%	93,894.71
Miami-Dade, FL	1,785.06	3.86%	509.99	1.10%	2,833.99	6.11%	7,104.85	15.34%	46,253.80
Chatham, GA	2,277.28	4.40%	352.83	0.68%	1,567.62	3.03%	6,475.54	12.55%	51,631.36
Cook, IL	2,147.79	3.26%	855.33	1.29%	1,779.47	2.69%	8,956.61	13.63%	65,683.42
Clark, NV	8,474.51	17.17%	1,549.48	3.13%	5,099.29	10.29%	7,954.33	16.14%	49,321.03
Multnomah, OR	1,903.87	3.10%	797.71	1.30%	2,011.52	3.28%	7,396.06	12.08%	61,264.40
Davidson, TN	3,011.48	3.83%	4,151.88	5.28%	2,043.49	2.60%	7,107.06	9.04%	78,538.16
Travis, TX	2,083.67	3.06%	452.54	0.66%	2,924.54	4.29%	6,980.98	10.24%	68,195.58
King, WA	2,844.84	2.63%	1,063.70	0.99%	3,170.31	2.92%	11,595.69	10.77%	107,659.72
Teton, WY	15,634.69	18.90%	3,871.06	4.67%	5,914.99	7.11%	13,552.45	16.38%	82,693.01

Notes: All entries are five-year pre-treatment averages for treated counties. "Absolute" columns report GDP per capita in U.S. dollars (USD per capita). "Share" columns report each sector's GDP per capita as a share of total county GDP per capita (percent). "Total GDP" reports total county GDP per capita (USD per capita).

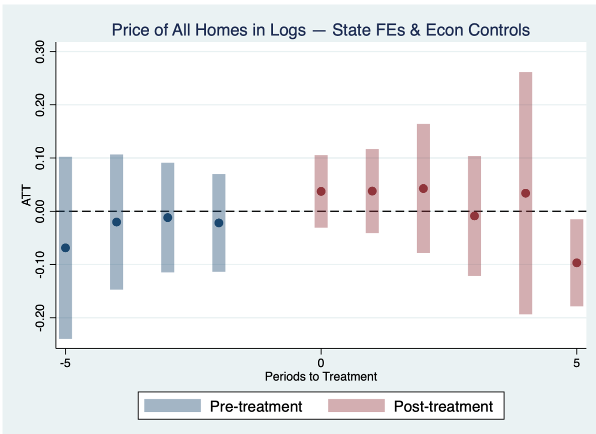
Table B.14: Economic Magnitudes of STR Regulation Effects

County	(1) GDP/cap (Abs.)	(2) Growth (Abs.)	(3) GDP/cap (Share)	(4) Growth (Share)
Los Angeles, CA	1,430.95	190.32	2.45%	0.33%
Orange, CA	1,875.28	249.41	3.07%	0.41%
Riverside, CA	1,057.54	140.65	3.74%	0.50%
San Francisco, CA	5,077.60	675.32	3.84%	0.51%
Sonoma, CA	1,837.65	244.41	3.59%	0.48%
Boulder, CO	1,855.06	246.72	2.64%	0.35%
Denver, CO	3,255.74	433.01	3.46%	0.46%
Miami-Dade, FL	1,785.06	237.41	3.86%	0.51%
Chatham, GA	2,277.28	302.88	4.40%	0.59%
Cook, IL	2,147.79	285.66	3.26%	0.43%
Clark, NV	8,474.51	1,127.11	17.17%	2.28%
Multnomah, OR	1,903.87	253.21	3.10%	0.41%
Davidson, TN	3,011.48	400.53	3.83%	0.51%
Travis, TX	2,083.67	277.13	3.06%	0.41%
King, WA	2,844.84	378.36	2.63%	0.35%
Teton, WY	15,634.69	2,079.41	18.90%	2.51%

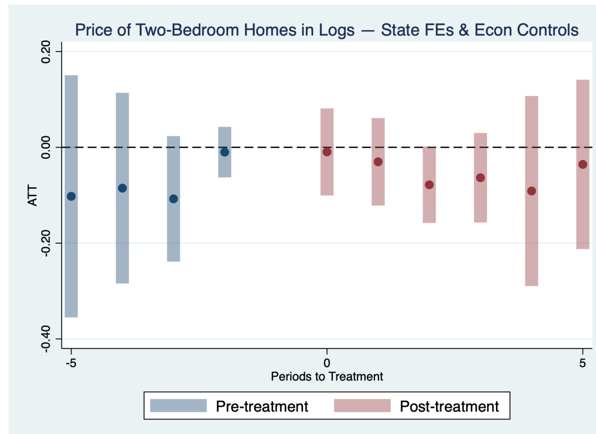
Notes: This table provides a back-of-the-envelope estimate of the economic magnitude of the STR regulation effect. Column (1) reports the five-year pre-treatment mean of county GDP per capita in Accommodation and Food Services among treated counties (USD per capita). Column (2) reports the implied treatment-induced increase in sectoral GDP per capita (USD per capita), obtained by applying the estimated 13.3 percent effect from Section 2.5.2 to the pre-treatment mean in Column (1). The calculation assumes homogeneous treatment effects and abstracts from other general-equilibrium adjustments. Column (3) reports the pre-treatment sector share of total county GDP per capita (percent). Column (4) reports the implied contribution of Accommodation and Food Services to total GDP per capita growth (percent), computed as the pre-treatment share in Column (3) multiplied by 13.3 percent.

B.2 Figures

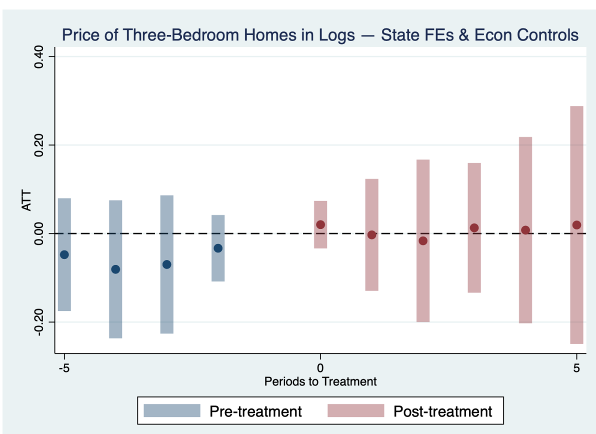
Figure B.1: Event-Study Estimates of House Price Effects



a) All Homes



b) 2-Bedroom

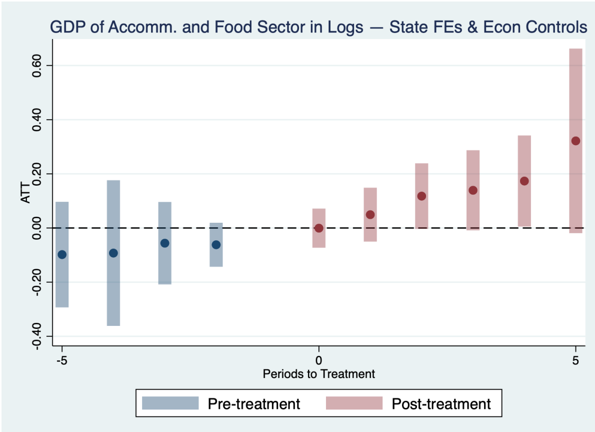


c) 3-Bedroom

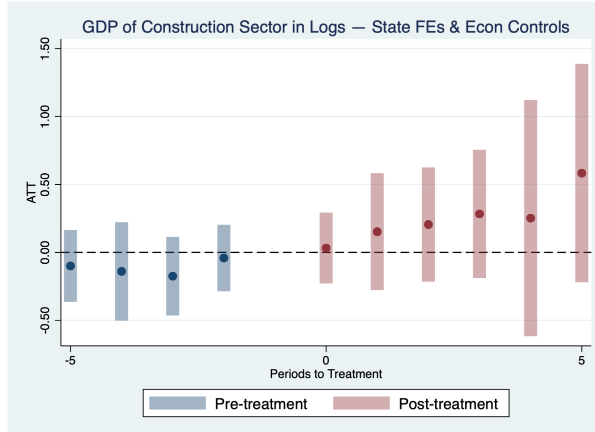
d) 5-Bedroom

Notes: This figure reports pre- and post-treatment event study estimates (see Equation (2.10)) with all coefficients normalized relative to $e = -1$ (the period immediately preceding STR regulation). The estimation procedure follows the doubly robust DiD approach proposed by Callaway and Sant'Anna (2021) and Sant'Anna and Zhao (2020). The outcome is the logarithm of county-level house prices. Covariates are measured at the county level and include the log of personal income per capita, the log of population density, the unemployment rate and the share of residents with a bachelor's degree. The comparison group comprises not-yet-treated units and never-treated units. The shaded areas represent 95% confidence intervals based on heteroskedasticity-robust standard errors computed via influence functions.

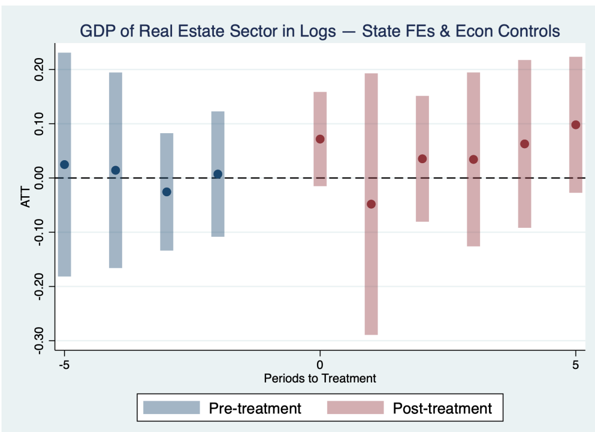
Figure B.2: Event-Study Estimates of Sectoral GDP Effects



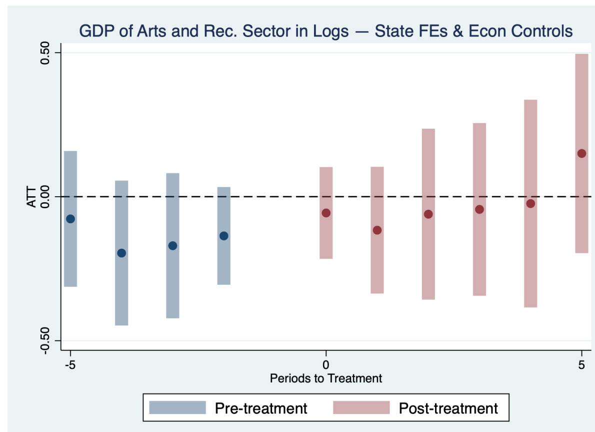
a) Accomm & Food



b) Construction



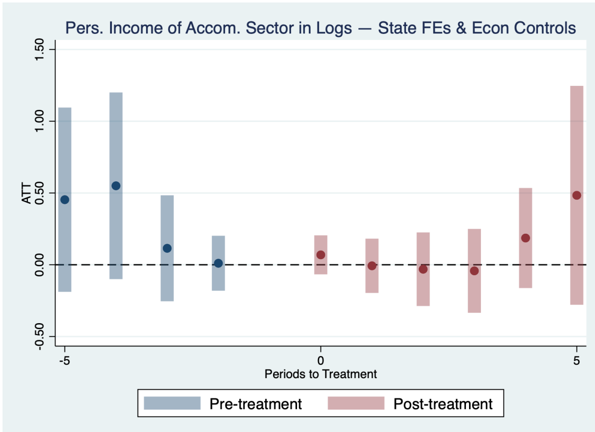
c) Real Estate



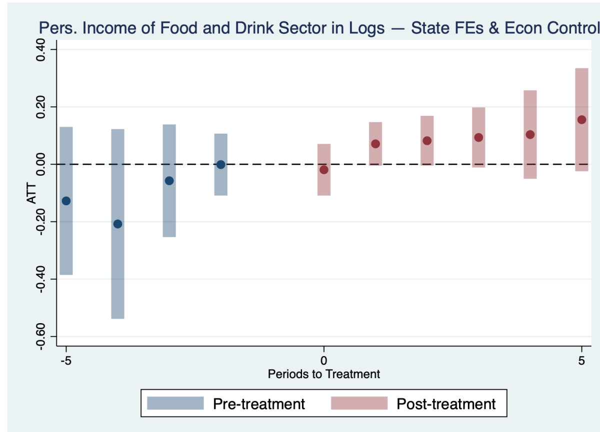
d) Arts & Rec

Notes: This figure reports pre- and post-treatment event study estimates (see Equation (2.10)) with all coefficients normalized relative to $e = -1$ (the period immediately preceding STR regulation). The estimation procedure follows the doubly robust DiD approach proposed by Callaway and Sant'Anna (2021) and Sant'Anna and Zhao (2020). The outcome is the logarithm of sectoral county-level GDP per capita. Covariates are measured at the county level and include the log of personal income per capita, the log of population density, the unemployment rate, and the share of residents with a bachelor's degree. The comparison group comprises not-yet-treated units and never-treated units. The shaded areas represent 95% confidence intervals based on heteroskedasticity-robust standard errors computed via influence functions.

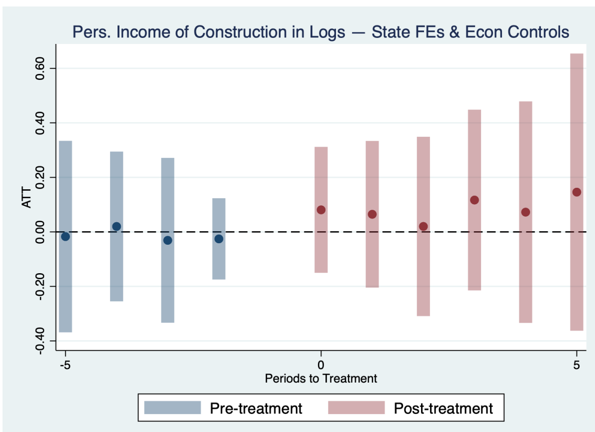
Figure B.3: Event Study Estimates of Sectoral Personal Income Effects



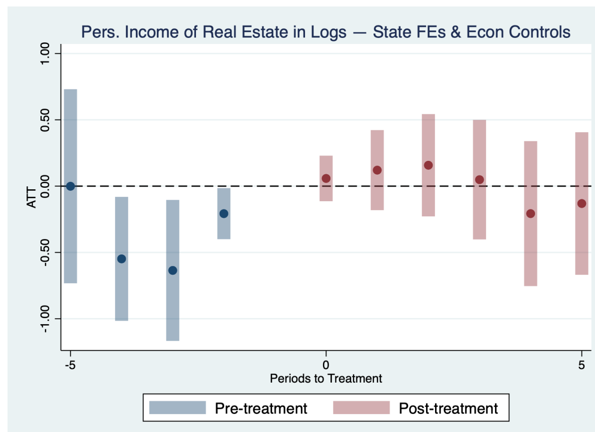
a) Accommodation



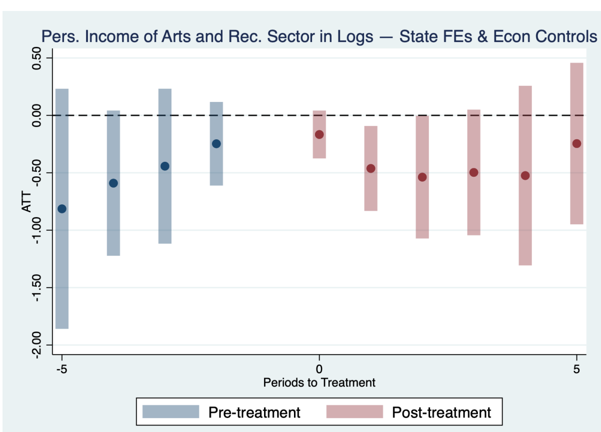
b) Food



c) Construction



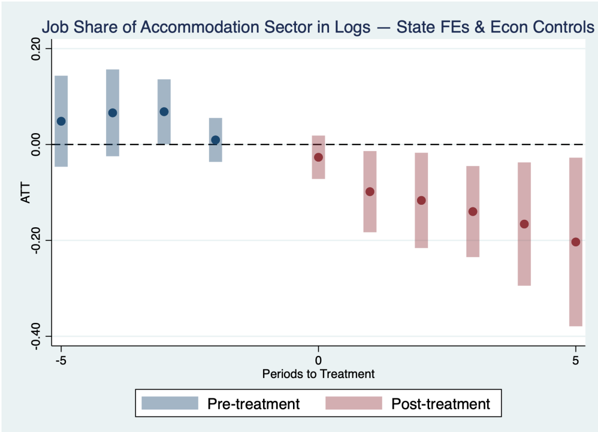
d) Real Estate



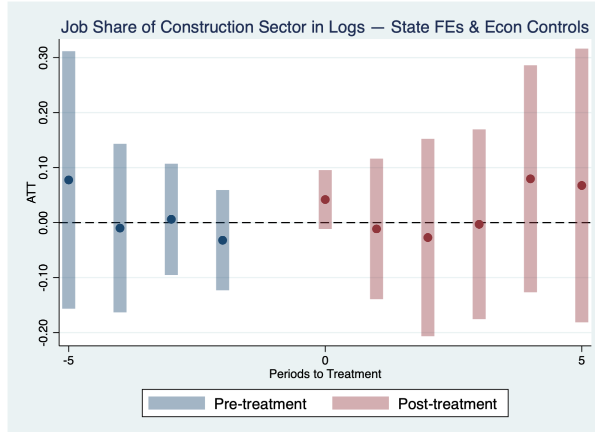
e) Arts & Rec

Notes: This figure reports pre- and post-treatment event study estimates (see Equation (2.10)) with all coefficients normalized relative to $e = -1$ (the period immediately preceding STR regulation). The estimation procedure follows the doubly robust DiD approach proposed by Callaway and Sant'Anna (2021) and Sant'Anna and Zhao (2020). The outcome is the logarithm of county-level sectoral personal income per capita. Covariates are measured at the county level and include the log of personal income per capita, the log of population density, the unemployment rate, and the share of residents with a bachelor's degree. The comparison group comprises not-yet-treated units and never-treated units. The shaded areas represent 95% confidence intervals based on heteroskedasticity-robust standard errors computed via influence functions.

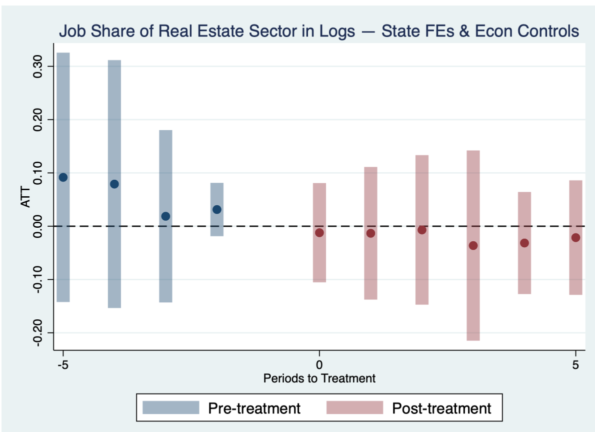
Figure B.4: Event-Study Estimates of Sectoral Employment Shares



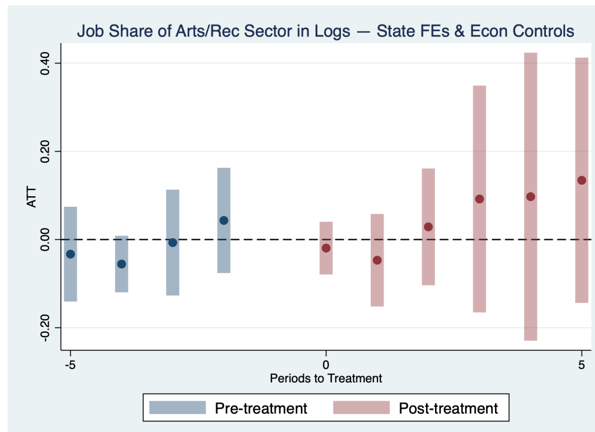
a) Accomm & Food



b) Construction



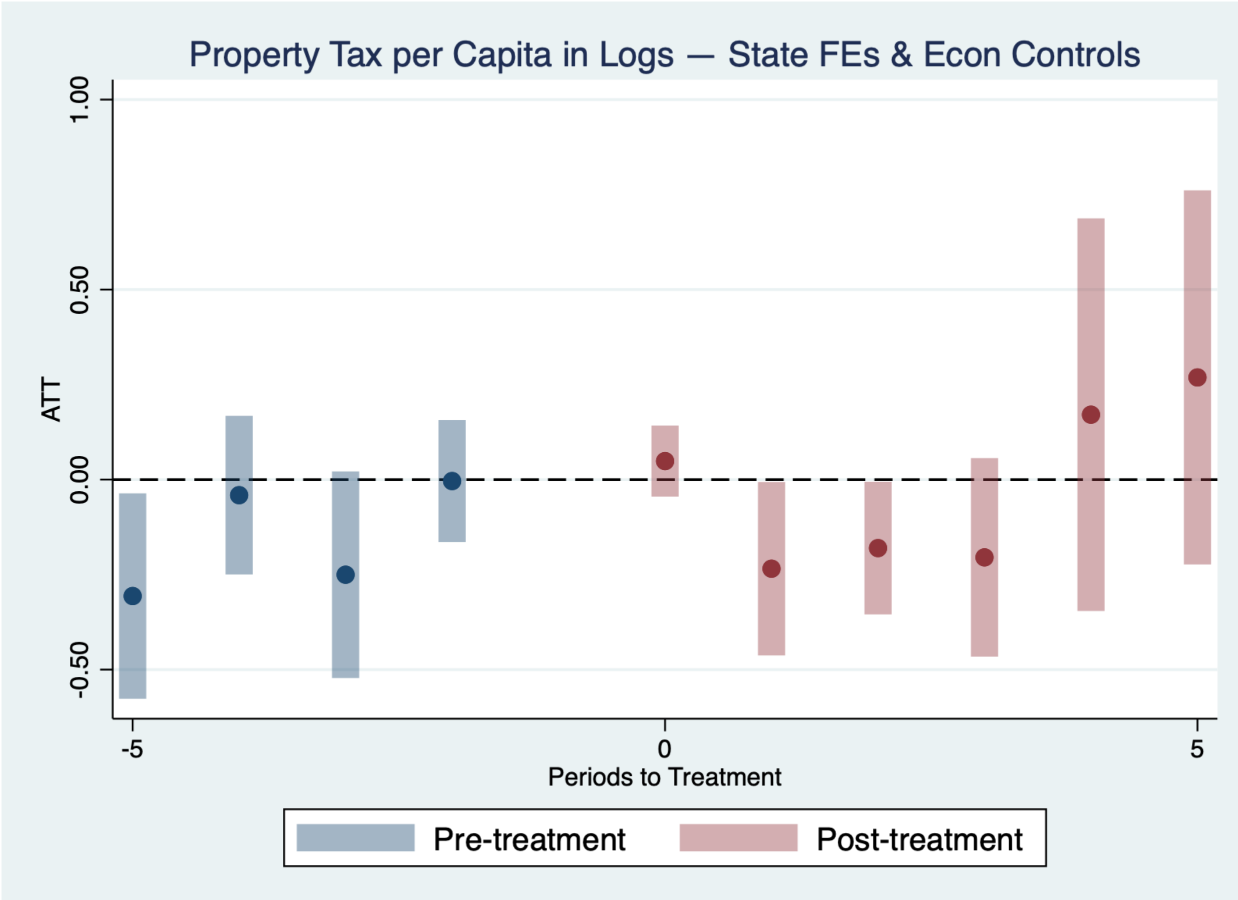
c) Real Estate



d) Arts & Rec

Notes: This figure reports pre- and post-treatment event study estimates (see Equation (2.10)) with all coefficients normalized relative to $e = -1$ (the period immediately preceding STR regulation). The estimation procedure follows the doubly robust DiD approach proposed by Callaway and Sant'Anna (2021) and Sant'Anna and Zhao (2020). The outcome is the logarithm of county-level sectoral employment shares. Covariates are measured at the county level and include the log of personal income per capita, the log of population density, the unemployment rate and the share of residents with a bachelor's degree. The comparison group comprises not-yet-treated units and never-treated units. The shaded areas represent 95% confidence intervals based on heteroskedasticity-robust standard errors computed via influence functions.

Figure B.5: Event-Study Estimates of Property Tax Revenues



Notes: This figure reports pre- and post-treatment event study estimates (see Equation (2.10)) with all coefficients normalized relative to $e = -1$ (the period immediately preceding STR regulation). The estimation procedure follows the doubly robust DiD approach proposed by Callaway and Sant'Anna (2021) and Sant'Anna and Zhao (2020). The outcome is the logarithm of county-level property tax revenue per capita. Covariates are measured at the county level and include the log of personal income per capita, the log of population density, the unemployment rate, and the share of residents with a bachelor's degree. The comparison group comprises not-yet-treated units and never-treated units. The shaded areas represent 95% confidence intervals based on heteroskedasticity-robust standard errors computed via influence functions.

C. Appendix to Chapter 3

C.1 Tables

Table C.1: Inventor Share by Firm Age by Inventor Age

	Inventor Age					
Firm Age	All	≤ 25	26–35	36–45	46–55	56+
0 to 5	13.7	15.2	15.7	13.7	12.4	12.1
6 to 10	11.1	11.8	11.1	11.3	10.5	11.5
11 to 20	12.8	15.4	11.6	12.7	13.4	14.4
21+	62.4	57.6	61.6	62.3	63.7	62.0

Source: FDZ IAB
Notes: This table shows the share of inventors (in percent) by firm age group (rows), conditional on inventor age group (columns).

Table C.2: Inventor Share by Firm Size by Inventor Age

	Inventor Age					
Firm Size	All	≤ 25	26–35	36–45	46–55	56+
1 to 20	3.7	10.6	4.1	3.3	3.2	5.1
21 to 100	8.3	13.1	8.4	8.1	7.9	9.7
101 to 250	10.8	11.1	9.7	10.6	11.2	12.8
251 to 500	11.7	10.6	10.1	11.7	12.4	13.6
501 to 1000	13.2	11.5	11.8	13.3	14.1	14.6
1000+	52.3	43.0	55.8	52.9	51.1	44.3

Source: FDZ IAB
Notes: This table shows the shares of inventors (in percent) by firm size group (rows), conditional on inventor age group (columns).

Table C.3: Firm Age, Firm Size, and Cites per Grant

	Firm Size						
Firm Age	1 to 20	21 to 100	101 to 250	251 to 500	501 to 1000	1000+	Total
0 to 5	0.567	0.576	0.549	0.604	0.516	0.547	0.559
6 to 10	0.493	0.550	0.545	0.647	0.649	0.533	0.562
11 to 20	0.529	0.514	0.584	0.646	0.652	0.592	0.594
21+	0.474	0.465	0.530	0.553	0.586	0.602	0.585
Total	0.536	0.529	0.547	0.588	0.596	0.592	0.580

Source: FDZ IAB.
Notes: This table shows the average citations per patent, within the first 4 years of application, by firm age and firm size.

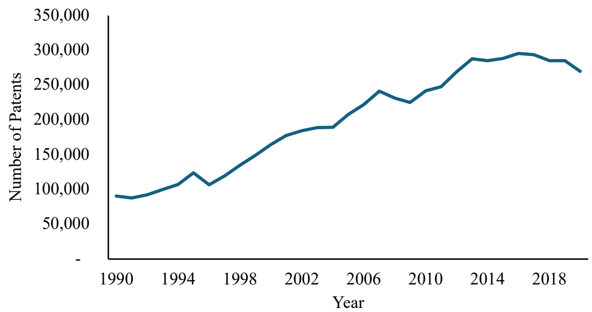
Table C.4: Share of Inventors Above Earnings Thresholds

Yearly Earnings in EUR	Percent of Inventors Above in 2000	Percent of Inventors Above in 2010
65,000	54.6	72.3
60,000	61.4	82.8
55,000	71.9	87.4
50,000	78.2	91.1
45,000	82.5	93.6
40,000	85.6	95.3
35,000	87.6	96.3
30,000	89.1	97.1
25,000	91.6	97.8
20,000	92.7	98.2
15,000	93.5	98.5
10,000	95.6	98.7
0	100.0	100.0

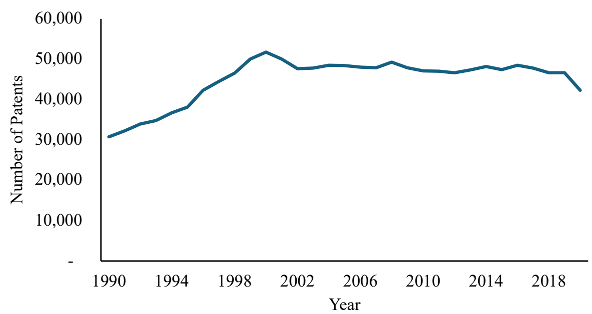
Source: FDZ IAB.
Notes: This table reports the share of inventors whose annual earnings exceed each threshold, separately for 2000 and 2010. All earnings and thresholds are expressed in constant 2015 euros (CPI, 2015=100). For reference, average CPI-adjusted wages in Germany were €31,436 in 2000 and €30,436 in 2010 (Institut Arbeit und Qualifikation der Universität Duisburg-Essen 2023). In 2000, the IAB right-censored wages above €66,063; in 2010, above €70,184. Roughly 10–13% of German full-time employees are above the censoring threshold in a typical year (IAB Methodenreport 2023).

C.2 Figures

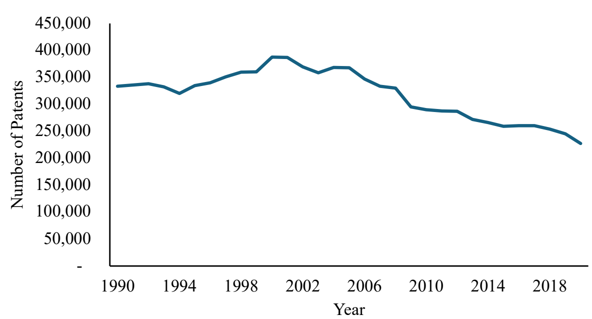
Figure C.1: Patent Trends by Historically Innovative Countries



(a) United States



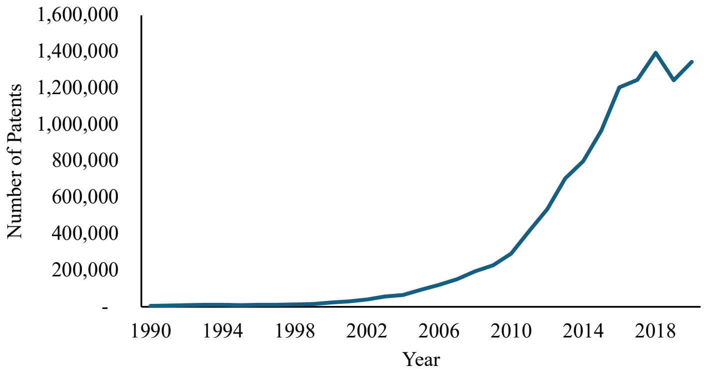
(b) Germany



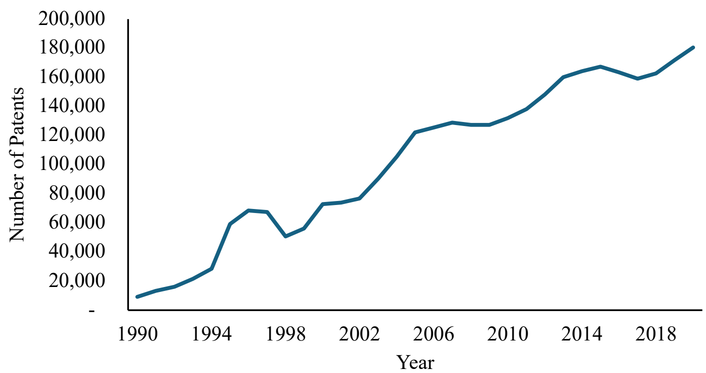
(c) Japan

Notes: This figure shows long-run patent trends of the most innovative and mature economies from 1990 until 2020. Panel (a) shows data for the United States, (b) for Germany and (c) for Japan.
Source: World Bank

Figure C.2: Patent Trends by Emerging Innovative Countries



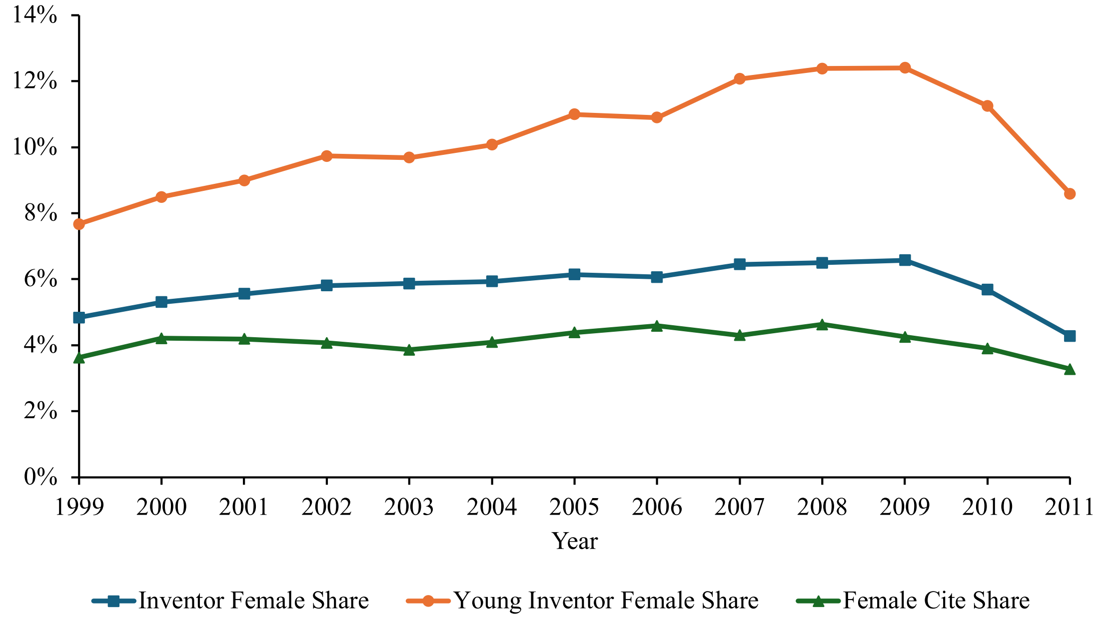
(a) China



(b) South Korea

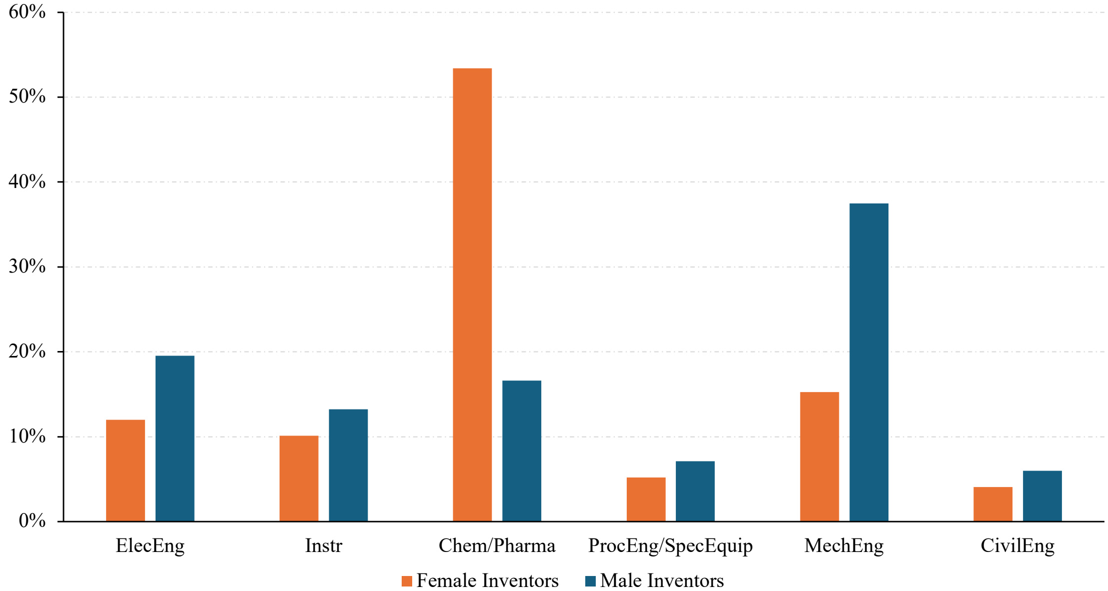
Notes: This figure shows long-run patent trends of newly emerging innovative countries from 1990 until 2020. Panel (a) shows data for China, and (b) for South Korea.
Source: World Bank

Figure C.3: Inventor Female Share



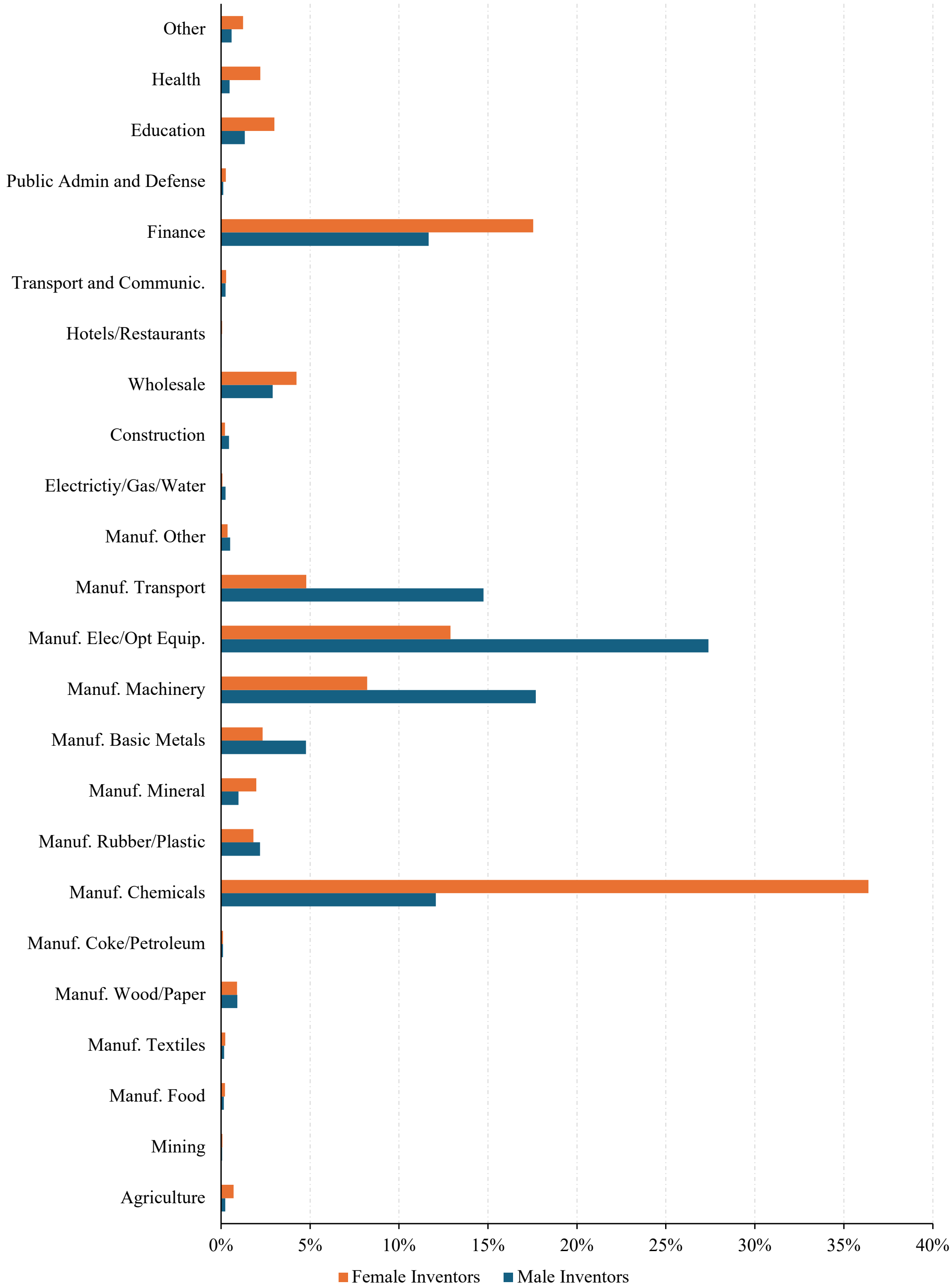
Notes: This figure shows the share of female patent applicants per year. Young inventors are 35 years old or younger. Female cite share is the female inventors' share of total patent citations. Classifications follow Akcigit and Goldschlag (2023).
Source: FDZ IAB

Figure C.4: Technology Classes and Female Inventor Patents



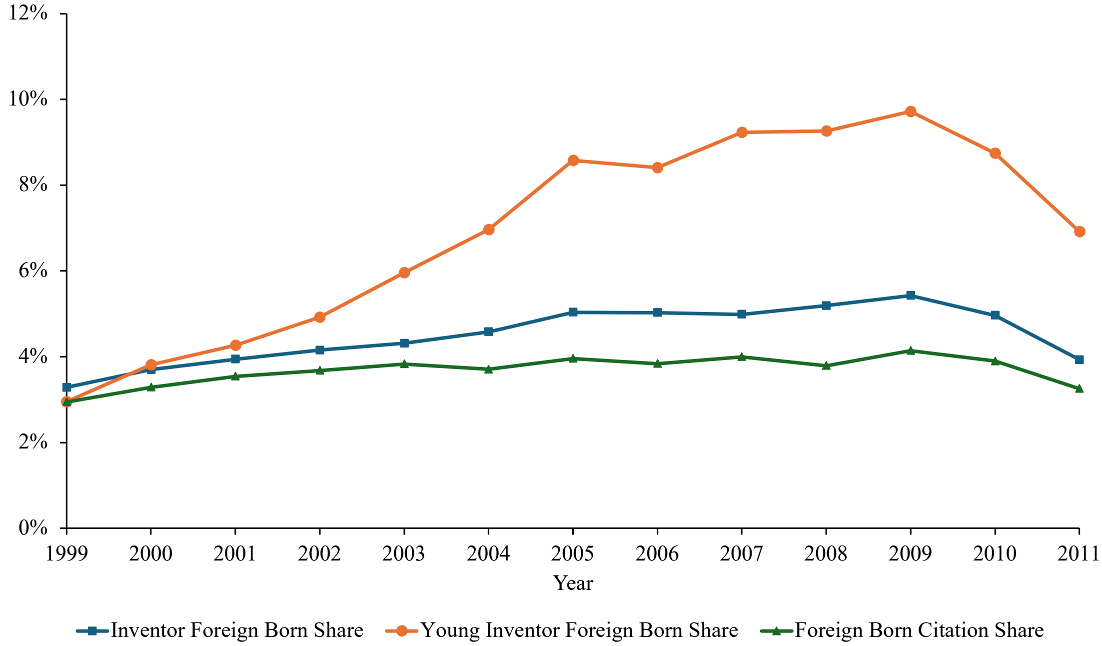
Notes: This figure shows the share of female inventor patents by patent technology class. *Source: FDZ IAB*

Figure C.5: Industry Sectors and Female Inventor Employment



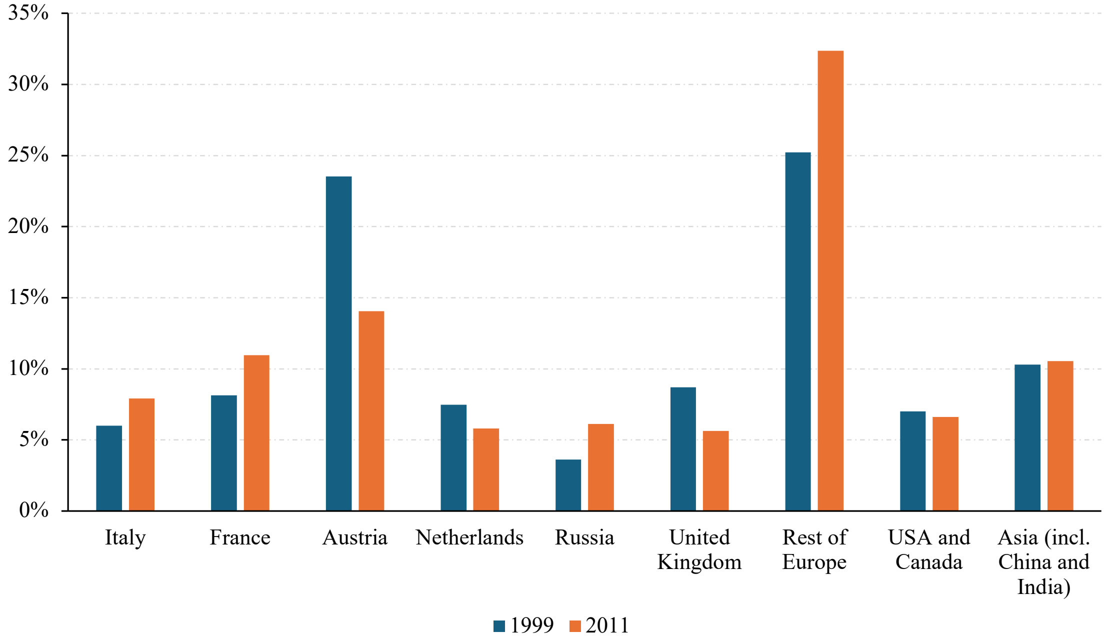
Notes: This figure shows the share of female inventor employment by industry.
Source: FDZ IAB

Figure C.6: Inventor Foreign Born Share



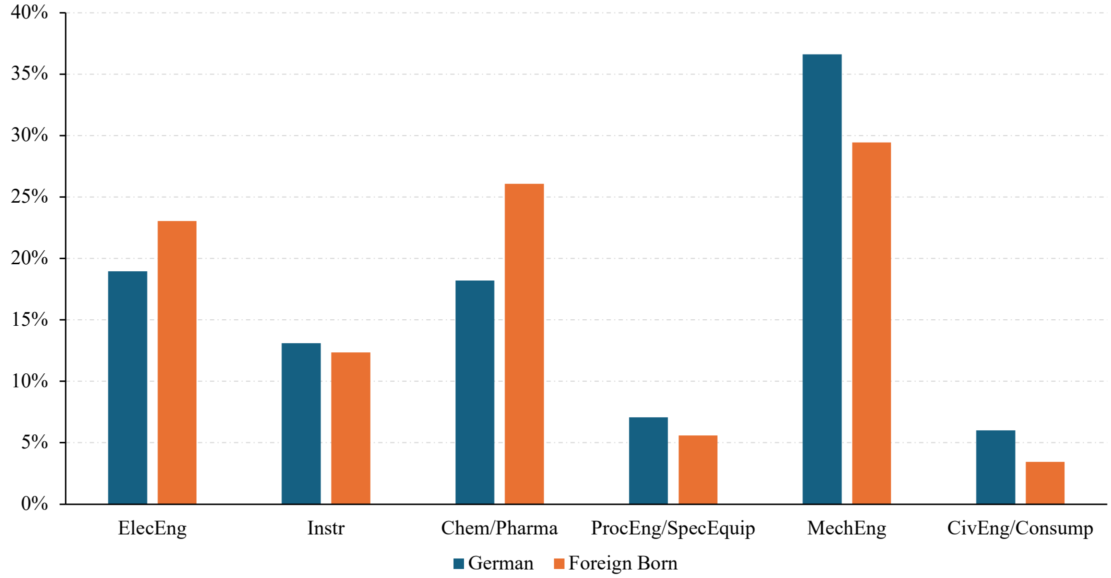
Notes: This figure shows the share of foreign born patent applicants per year. Young inventors are 35 years old or younger. Foreign born cite share is the share of patent citations of foreign born inventors. Classifications follow Akcigit and Goldschlag (2023).
Source: FDZ IAB

Figure C.7: Nationalities of Foreign Born Inventors



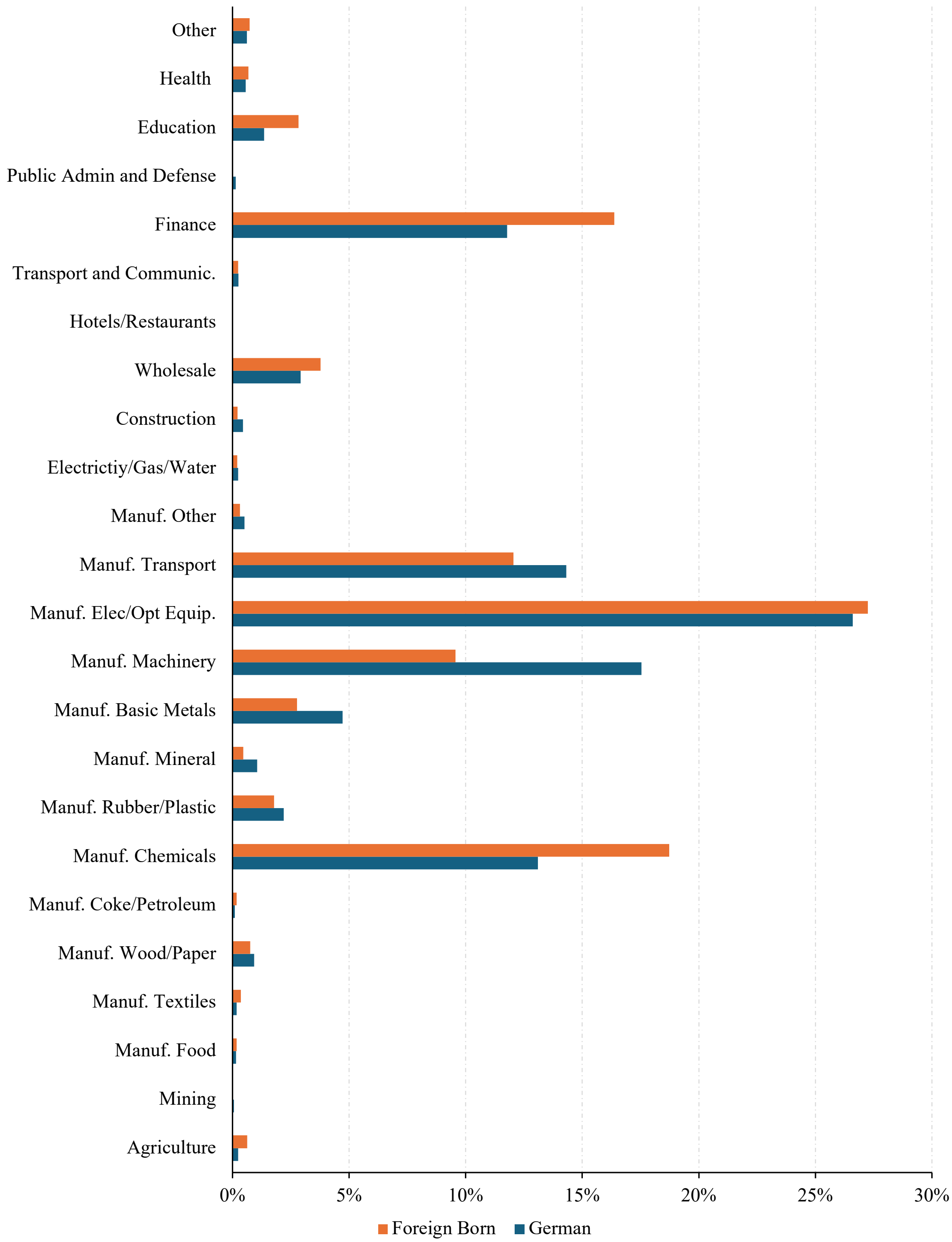
Notes: This figure shows the share of foreign born inventors by nationality/origin for the years 1999 and 2011. Classifications follow Akcigit and Goldschlag (2023).
Source: FDZ IAB

Figure C.8: Technology Classes and Foreign Born Inventor Share



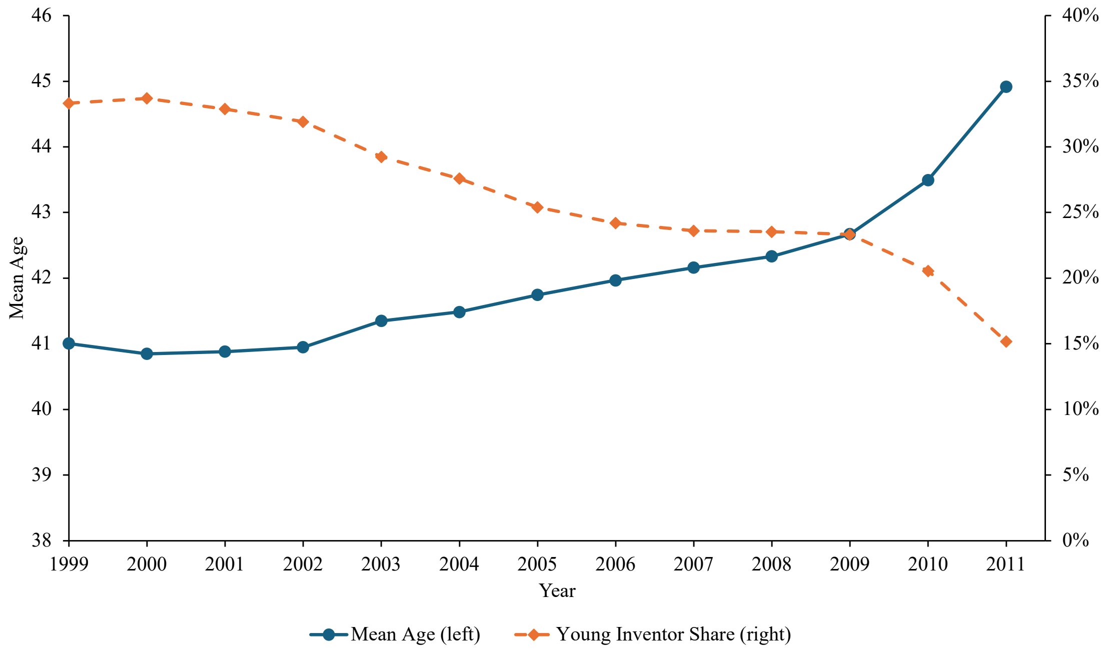
Notes: This figure shows the share of foreign born inventor patents by patent technology class.
Source: FDZ IAB

Figure C.9: Industry Sectors and Foreign Born Inventor Employment



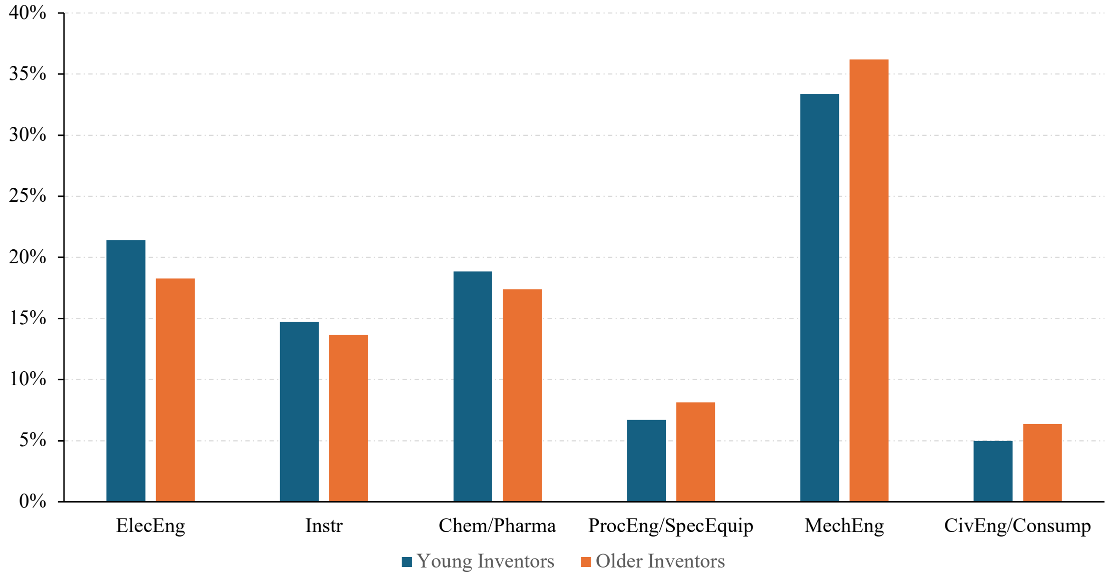
Notes: This figure shows the share of foreign born inventor employment by industry.
Source: FDZ IAB

Figure C.10: Inventor Age



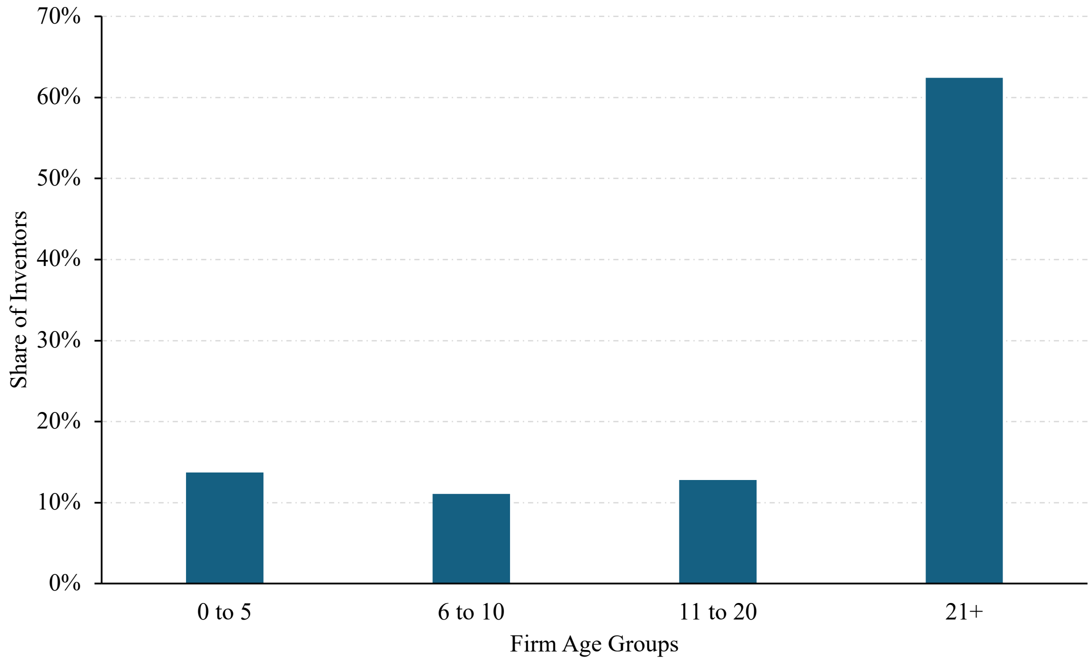
Notes: This figure shows the mean age of all active inventors per year. Young inventors are active inventors that are at most 35 years old.
Source: FDZ IAB

Figure C.11: Technology Classes and Inventor Age



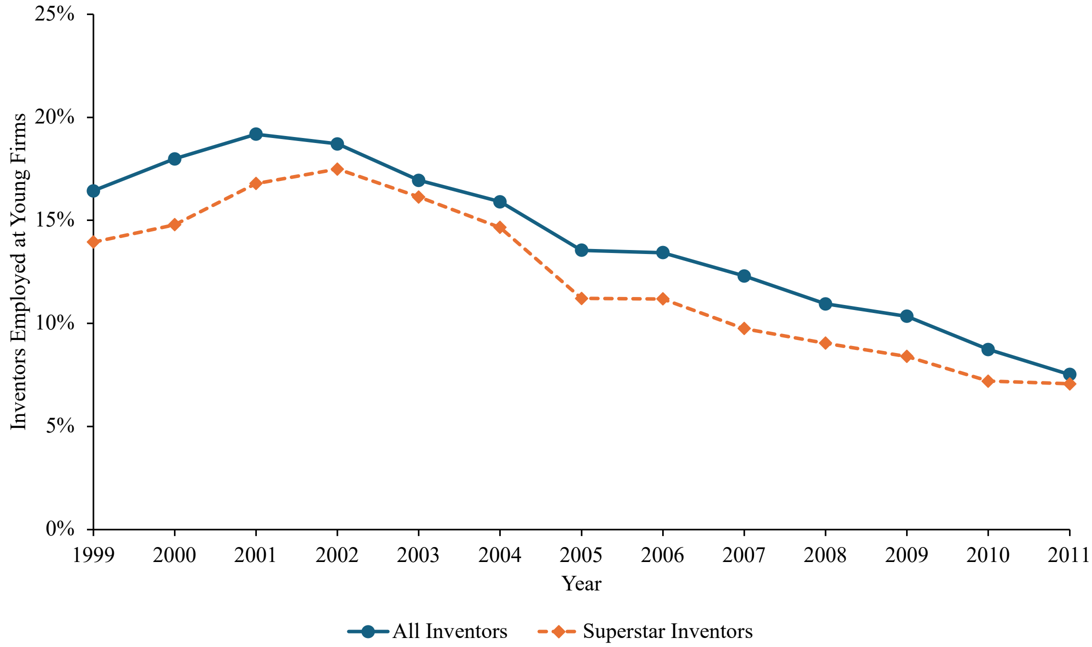
Notes: This figure shows the share of young inventor patents by patent technology class. Young inventors are active inventors that are at most 35 years old.
Source: FDZ IAB

Figure C.12: Inventor Share by Firm Age Groups



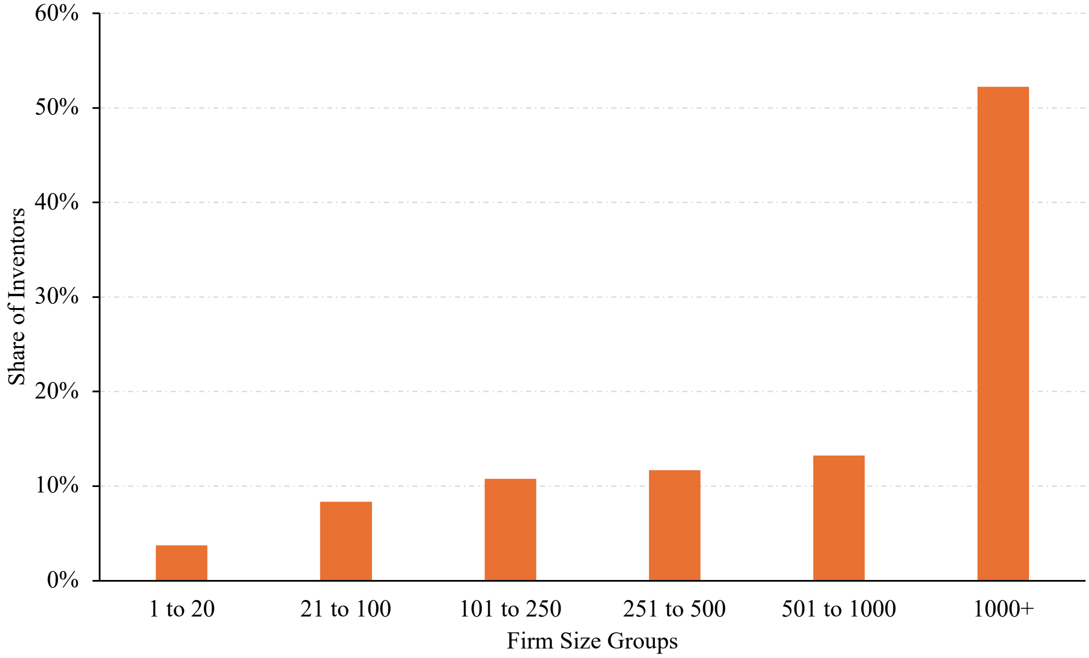
Notes: This figure shows the share of active inventors by firm age groups.
Source: FDZ IAB

Figure C.13: Share of Inventors at Young Firms



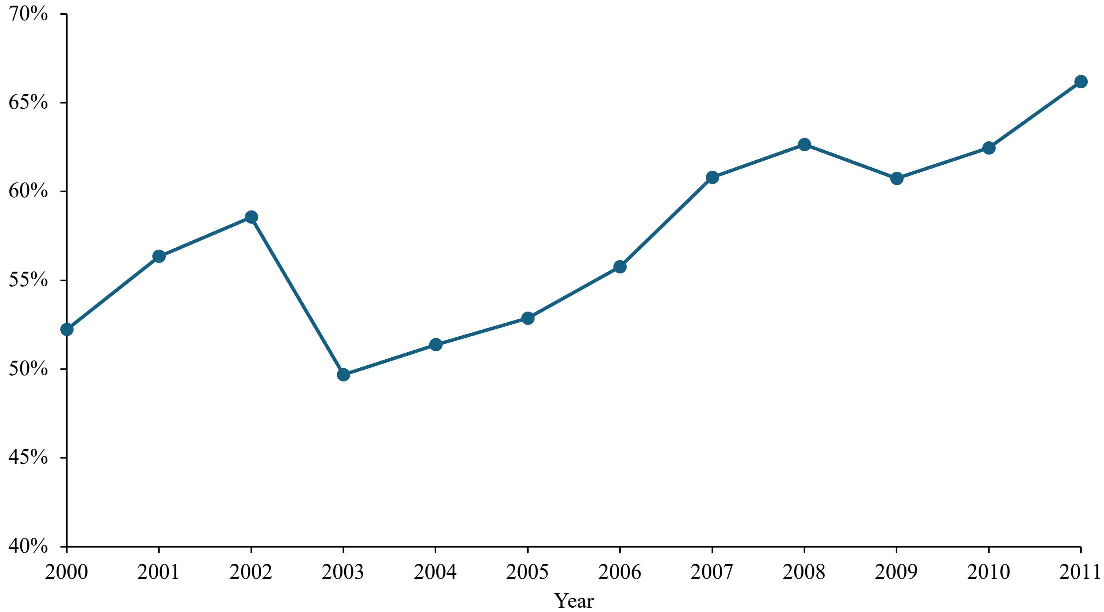
Notes: This figure shows the share of all inventors and superstar inventors employed at young firms (≤ 5 years). Superstar inventors are defined as those with citation counts exceeding the 90th percentile within their age cohort in a given year.
Source: FDZ IAB

Figure C.14: Inventor Share by Firm Size Groups



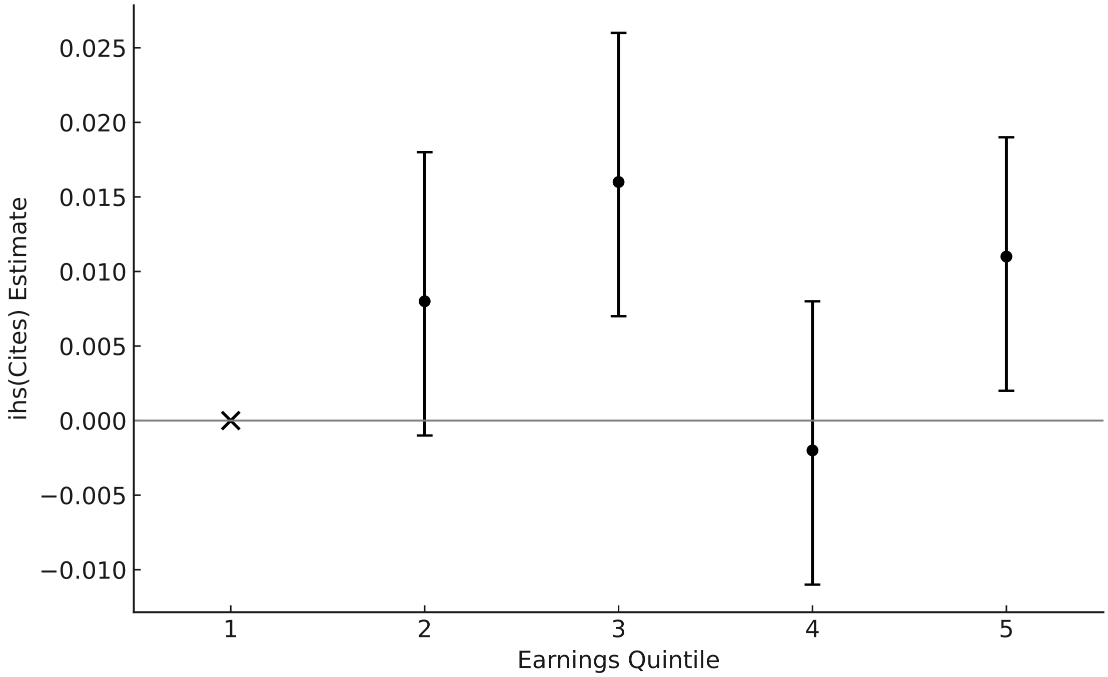
Notes: This figure shows the share of active inventors by firm size groups.
Source: FDZ IAB

Figure C.15: Share of High-Earning Inventors



Notes: This figure shows the share of high-earning inventors. These are inventors with earnings above the social security contribution limit that determines eligibility for the maximum state pension. Typically, 10-13% of the labor force exceed this threshold per year (FDZ IAB Methodenreport).
Source: FDZ IAB

Figure C.16: Earnings and Citations

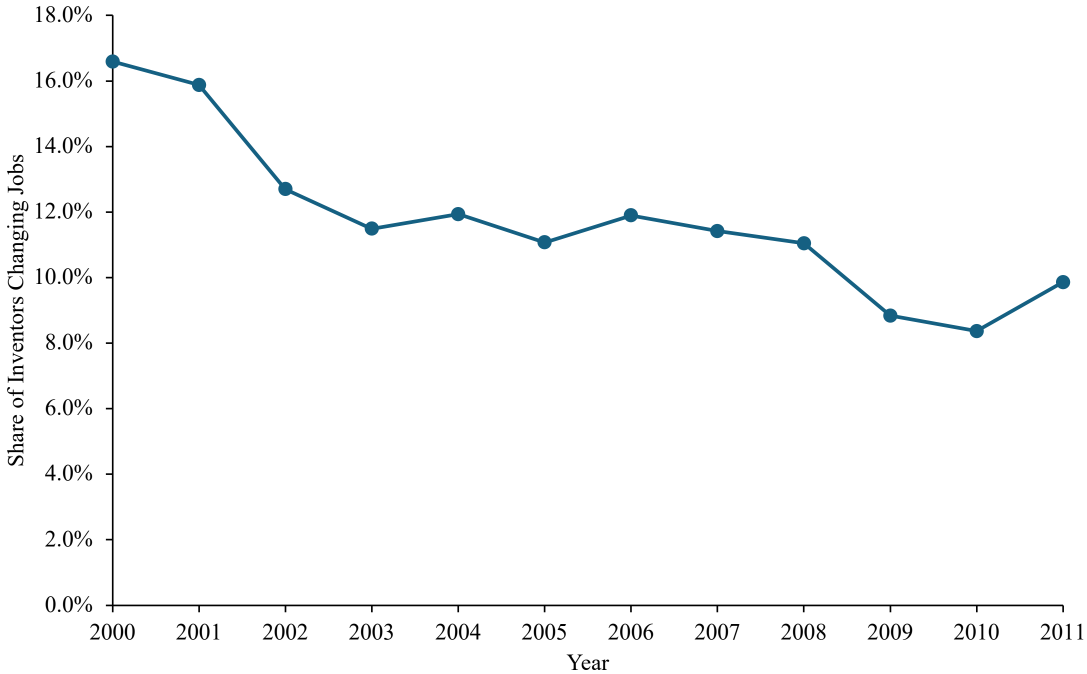


Notes: Following Akcigit and Goldschlag (2025), this figure shows estimates of λ_j, with $j = 1$ being the excluded group, from the following regression:

$$ihs(Cites_{i,t}) = \alpha + \sum_{j=2}^{5} \lambda_j EarnGroup[j]_{i,t} + X + \epsilon_{i,t}$$

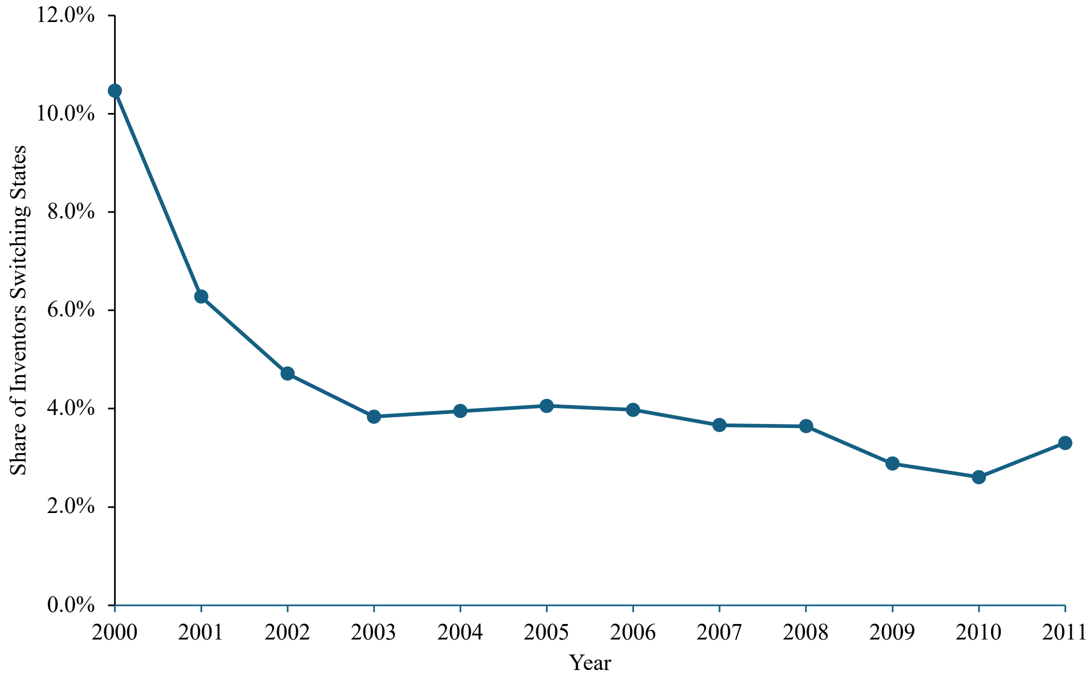
where $ihsCites$ is the ihs() transformed 4-year window cites of patents applied for in t. $EarnGroup$ divides inventors into five earnings categories (*adjusted* earnings "qunitiles"). Group 5 comprises inventors with top-coded wages (above the social security contribution limit). Groups 1-4 represent quartiles of the remaining wage distribution. X contains age and age-squared, year FE, industry FE, firm age group FE, and firm size group FE. Standard errors are clustered at the individual level. Point estimates are represented by circles and 95%-confidence intervals by vertical lines.
Source: FDZ IAB

Figure C.17: Job Mobility



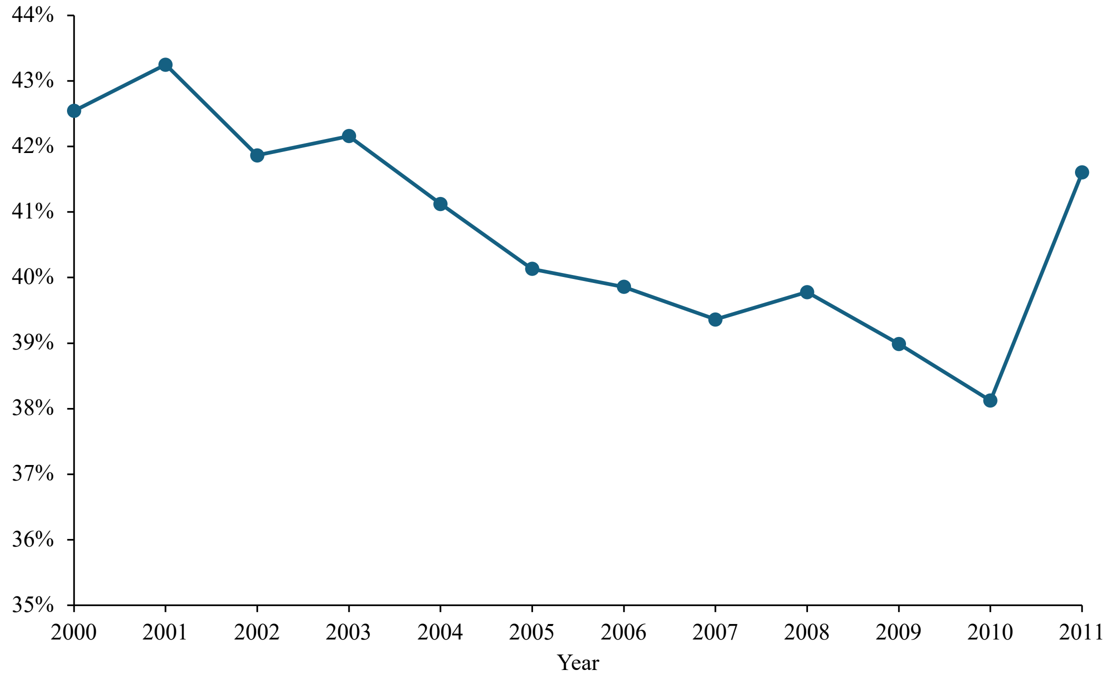
Notes: This figure shows the share of inventors who change jobs, i.e., who move from one firm to another, in a given year.
Source: FDZ IAB

Figure C.18: State Movers



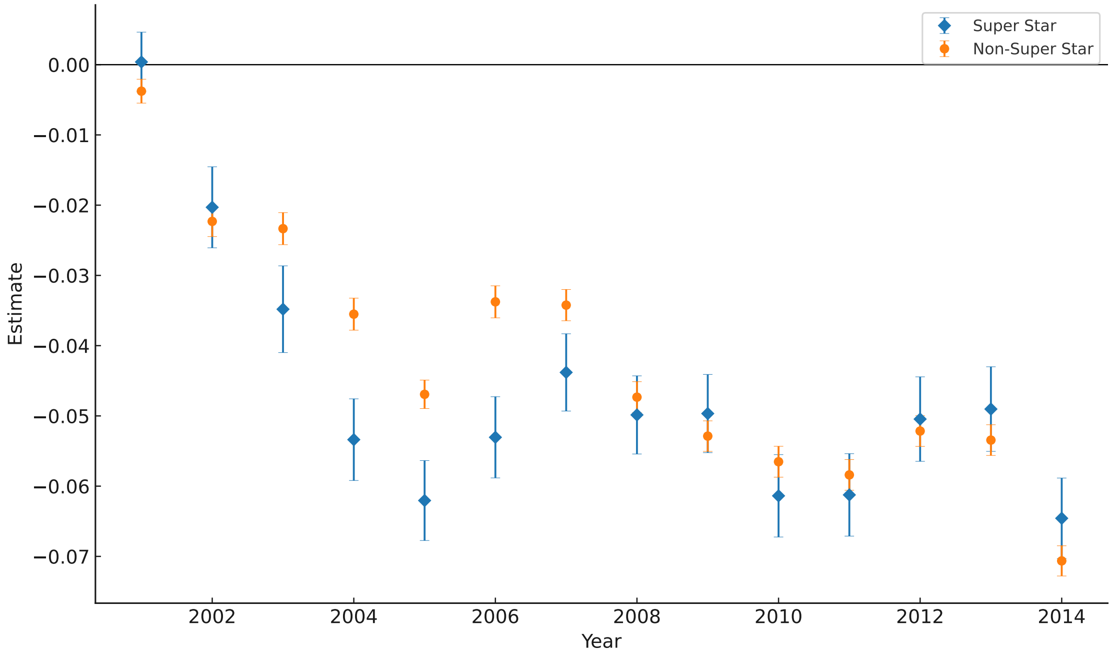
Notes: This figure shows the share of inventors who relocated across state lines between consecutive jobs.
Source: FDZ IAB

Figure C.19: Inventor Share of Top20 Counties



Notes: This figure shows the share of inventors in the twenty counties with the highest number of inventors.
Source: FDZ IAB

Figure C.20: Change in Inventor Entrepreneurship Rate



Notes: Following Akcigit and Goldschlag (2025), this figure shows estimates of β_k from the following regression:

$$Entrep_{i,t} = \alpha + \sum_{k=2001}^{2015} \beta_k D^k + \psi_i + \epsilon_{i,t}$$

where $Entrep_{i,t}$, representing entrepreneurial activity, is an indicator of whether, in year t, an inventor i (1) works at a firm that is three years old or younger and (2) falls in the top quartile of the firm's earnings distribution. D^k are year dummies, and ψ_i are inventor fixed effects. Point estimates are represented by circles and 95%-confidence intervals by vertical lines.

Source: FDZ IAB