Small-Scale Mentoring, Large-Scale Innovation: Evidence from a Superstar Firm

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Abstract

We use the staggered rollout of a mentorship program within a superstar technology firm to test whether small-scale, targeted mentoring can address organizational frictions that influence innovation and productivity. Analyzing novel data capturing the complete innovation pipeline—from initial idea disclosures through patent applications—we study seven mentoring cohorts with 633 mentees and track outcomes for over 20,000 employees. First, we find a positive and significant relationship between participating in the mentorship program and innovation outcomes in the short- and long-term, with underrepresented innovators realizing the largest gains. Second, we document significant indirect spillovers through team formation, with non-mentored peers experiencing productivity gains through collaboration. Survey evidence points to three mechanisms: knowledge transfer about patent processes, expanded professional networks, and increased confidence in identifying patentable ideas. The mentorship program is also associated with broader organizational improvements: engineers in the firm perceive the culture to be better, report higher job satisfaction, and are less likely to leave than engineers at peer firms. Accounting for these spillovers implies that small-scale mentoring programs generate a meaningful return on investment and are a cost-effective way to foster innovation and inclusivity.

JEL: J16, J24, M14, M54, O31, O32, O34

Keywords: Intellectual Property, Innovation, Patents, Diversity, Labor and Finance, Mentorship, Human Capital Investment, Intangible Assets, Corporate Culture, Employee Turnover

We thank Ben Pyle (discussant) and participants at the Conference on Empirical Legal Studies for helpful discussions and comments. We thank the Ciocca Center for Innovation and Entrepreneurship at Santa Clara University for its support. This research was approved by the Institutional Review Board (IRB) at the University of California Berkeley.

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1 Introduction

Firms face significant frictions in how effectively they can convert human capital and creativity into technological advances. These frictions are particularly acute in "superstar" technology firms, which now account for an overwhelming share of private sector R&D investment and patent production (Grullon et al., 2019; Autor et al., 2020). Although the scale of these firms enables massive investments in intangible assets, their size simultaneously creates organizational challenges that can impede knowledge transfer and inclusive innovation.[1] These organizational challenges are particularly salient for innovation, where information asymmetries and coordination costs can impede knowledge flows (Seru, 2014; Bernstein, 2015) and the quantity and direction of innovative activity (Lerner and Wulf, 2007; Koning et al., 2021).

These organizational frictions manifest in persistent gaps between workforce composition and innovative output. For instance, while Black workers comprise 9% of STEM professionals, they represent only 1.2% of patent inventors; similarly, women constitute 35% of STEM workers but only 13% of inventors (NCSES, 2023; Akcigit and Goldschlag, 2022). Such disparities represent both allocative inefficiency in innovation processes and a substantial loss of innovation potential (Heath et al., 2023; Hochberg et al., 2023; Cook et al., 2025). Recent work highlights how institutional barriers and informal social constraints can systematically exclude certain groups from participating in innovation, even when they possess relevant technical expertise (Nishesh et al., 2024; Chien and Grennan, 2024). Although a broader perspective and diverse teams are associated with greater innovation value in theory and practice (Hong and Page, 2004; Bernstein et al., 2022; Li and Wang, 2023), achieving meaningful inclusion remains a challenge in superstar technology firms where the workplace culture has attracted congressional scrutiny and litigation.[2]

[1] A salient example is Meta's 2023 "Year of Efficiency" initiative, where CEO Mark Zuckerberg explicitly acknowledged the need to streamline hierarchies and reduce bureaucratic impediments to innovation and information flow in a company that had grown to over 80,000 employees.

[2] For example, in 2021–2022, the House Financial Services Committee held multiple hearings on discriminatory practices in large technology firms, and several class action lawsuits alleged systematic exclusion of

While a growing literature examines innovation barriers associated with scale (Akcigit and Kerr, 2018), we know relatively little about how firms can overcome these obstacles through targeted internal programs. This gap is particularly notable given that internal development programs represent a substantial investment for firms, and it aligns with the understanding that a large portion of human capital accumulation occurs inside firms through skills development (Becker, 1965; Acemoglu and Pischke, 1998). While previous research indicates that mentoring individuals with STEM backgrounds improves academic success (Azoulay et al., 2010), our knowledge from the field has been limited to call centers and small enterprises (Sandvik et al., Forthcoming; Otis, 2024). Can mentorship programs succeed in superstar firms, or is their organizational structure too logistically complex and the size of the workforce too prohibitively expensive? What is the typical return on investment (ROI) in such programs, and how should leaders think about the trade-off between scaling such programs and returns?

To address these questions, we examine the potential outcomes associated with a targeted mentoring program at a superstar technology firm. The program was focused on increasing exposure to inventing and on converting engineering and related staff with limited experience with the patenting process ("innovators") into individuals that submit inventive ideas for patenting ("inventors"). Our analysis considers both the direct influence of the program on mentored innovators and the indirect influence (i.e., spillovers) onto their non-mentored peers. Our analysis of both the direct and indirect channels allows us to provide novel evidence on the total returns to human capital investments in innovation-intensive firms, and it helps resolve measurement challenges that have historically complicated the evaluation of corporate training programs (Black and Lynch, 1996). This is no small feat given that training and development expenses at U.S. firms exceeded $100 billion in 2023 (Statista, 2024).

Specifically, we analyze the staggered roll-out of a mentorship program involving 633

underrepresented groups from technical and leadership roles.

innovators participating in seven separate mentoring cohorts selected from a set of applicant innovators. The program was open to all of the firm's employees, but those from underrepresented groups (e.g., women and racial minorities) were especially encouraged to apply. Applicants not selected initially, due to caps on the size of each cohort, were contacted about subsequent cohorts and encouraged to re-apply. The program's curriculum paired experienced inventors with proven track records of patenting success (i.e., mentors) with small groups of mentees. The mentors guided their mentees through the inventive idea development and patenting process. The modal group had 5 mentees and 1–2 mentors who met regularly over a calendar quarter.

To estimate the relation between mentorship and innovation outcomes, we employ multiple econometric techniques that exploit the staggered implementation of the mentorship program across the cohorts. Our empirical approach builds on recent methodological advances in the evaluation of staggered treatment designs (Goodman-Bacon, 2021), while addressing specific challenges in measuring innovation outcomes (Lerner and Seru, 2022). Our primary specifications include a two-way fixed effects (TWFE) estimator and a stacked difference-in-differences estimator. The latter addresses potential biases arising from staggered treatment timing and evolving treatment effects (Bellstam et al., 2021; Roth et al., 2023).[3] We construct our stacked difference-in-differences estimator using two control groups: never-mentored innovators and not-yet-mentored innovators (i.e., those who were not selected to participate in an earlier cohort but who participated in a later cohort). This approach allows us to exploit variation in the timing of program participation and to control for selection on unobservables. We test the parallel trends assumption underlying our difference-in-differences approach using event study analyses and visualizations (Sun and Shapiro, 2022). These tests suggest no diverging pre-trends in inventive idea submissions between mentored and non-mentored innovators, supporting the validity of our empirical strategy.

Our ability to estimate the relation between mentorship and inventive idea development

[3]Our inferences are also robust to the use of the estimator proposed by Callaway and Sant'Anna (2021).

is made possible by unprecedented access to proprietary data by the focal firm, including the universe of inventive ideas submitted to patent attorneys for potential patent application filing, regardless of whether they were ultimately filed as a patent application, where the popular dataset constructed by Kogan et al. (2017) begins. In total, our dataset contains 66,397 unique idea disclosures filed by 29,211 unique innovators. This data includes demographic details such as gender, age, marital status, race/ethnicity, job role, and country of residence. We use the race/ethnicity data to identify underrepresented racial minority (URM) innovators as those who are neither Asian nor White (as Asian and White individuals are highly represented among the innovators in this firm and its peer firms) (Cook, 2018). Combining this rich panel of innovator-quarter-level disclosure submissions with data on which innovators participated in the different cohorts of the mentorship program allows us to provide novel insights into the effectiveness of targeted mentoring programs in fostering innovation within large organizations. Further, this detailed view of the innovation pipeline allows us to capture more nuanced and indirect outcomes that may be missed when focusing solely on ideas approved for patent applications, addressing a common limitation in the innovation literature (Kogan et al., 2017; Lerner and Seru, 2022; Mkrtchyan et al., 2024).

Our analyses yield four main results. First, mentorship leads to a significant increase in innovation output. Mentored innovators produce 1–1.5 additional invention disclosures annually relative to control groups, an economically meaningful relation given baseline disclosure rates of 0.5 per year. Importantly, these patterns persist: excluding the program quarter and subsequent two quarters, mentored innovators still generate 0.5 additional disclosures annually. These results are robust to alternative control groups, propensity score-matched samples, and alternative regression specifications. Importantly, these productivity gains are not driven by changes in team size and composition, helping to rule out concerns that the firm may be engaging in "diversity-washing."[4] Finally, triple difference-in-differences esti-

[4]In principle, there are strict standards for inventorship, and patent attorneys would put the firm's patent in jeopardy for not accurately naming the inventors. While penalties have declined since the Leahy–Smith America Invents Act signed into law in 2011 for wrongful naming of inventors, there are still deterrents for misleading, and patent attorneys have been sued for adjusting claims to change inventorship.

mates suggest larger benefits for female and URM innovators, though standard errors are large for these subgroup analyses.

Second, survey evidence reveals the specific channels driving these outcomes. Survey data collected as part of a broader multi-firm innovation study identifies three key mechanisms: direct knowledge transfer about patent processes, expanded professional networks among inventors, and increased confidence in identifying patentable innovations.[5] These mechanisms align with existing theoretical work on barriers to innovation (Manso, 2011; Bohren et al., 2019). Finally, survey responses indicate significantly higher comfort levels with the patent process and stronger social connections with inventors post-mentorship, suggesting formal mentorship programs can enhance inclusivity beyond just producing innovative outcomes.

Third, we document significant positive spillovers to non-participants. Our unique dataset allows us to track the formation of new collaborating teams and the submission patterns for both mentored individuals and their non-mentored peers. To test for such spillovers, we restrict our sample to innovators never directly associated with the mentorship program and construct a measure of mentor exposure using the composition of the innovator team. We find evidence of positive spillovers in innovative productivity among non-mentored innovators who collaborate with mentored innovators. These spillovers appear similarly sized across demographic groups, suggesting knowledge flows operate through organizational proximity rather than demographic matching. Finding a positive indirect influence on non-mentored innovators is particularly useful for estimating ROI, because it indicates these programs increase innovativeness within the firm, rather than merely shifting the focus from one group of innovators to another. This points to the scalability of mentorship programs within large organizations.

Additionally, to validate the suggested social connectedness mechanism in the survey, we use our unique dataset to track the composition of new teams of collaborating innovators.

[5]The survey was conducted in June 2023 as part of a larger multi-firm study on innovation (Chien and Grennan, 2024), and the data collected from that process has been shown to be internally reliable and externally valid.

We find that mentored innovators form more diverse inventor teams after participating in the mentorship program, with diversity measured relative to pre-program team composition baselines. This improved team diversity is particularly important given evidence linking diverse teams to higher-value patents. Hence, these findings suggest mentorship can help inventors overcome the "homophily trap" that many teams find themselves subject to (Heath et al., 2023).

Fourth, we find meaningful empirical relationships between mentorship, corporate culture, and employee retention. Using data from Glassdoor—a career intelligence website that provides transparency about jobs, salaries, and company reviews—and Revelio—a labor market intelligence platform that tracks employee movements across firms—we examine changes in workplace perceptions and retention patterns. Engineers at the focal firm, which implemented the mentorship program, are 4% less likely to leave following program implementation compared to peers at similar firms. This retention pattern represents substantial savings given replacement costs for technical talent (Benson et al., 2019). Changes in workplace perceptions appear connected to this outcome: both mentored and non-mentored engineers report improved satisfaction, career opportunities, and cultural values relative to control groups of (i) non-engineering employees at the focal firm and (ii) engineers at peer firms. These cultural patterns are particularly intriguing given prior work showing how persistent devaluation and lack of sponsorship often drive members of underrepresented groups to exit STEM roles (Spoon et al., 2023).

To account for the multiple channels through which mentorship creates positive returns, we update traditional estimates of mentoring's ROI. Using conservative patent valuations based on maintenance and renewal fees (Pakes, 1986; Abrams et al., 2023), we estimate direct benefits from increased innovation output of approximately $4 million, and another $1 million from spillovers to peers. We also document potential indirect benefits through improved retention and corporate culture, though these effects are more challenging to quantify precisely and their magnitude is much larger. The mentoring program had modest implemen-

tation costs—primarily staff time for coordination and mentoring—indicating the program's ROI is high. Given the mentoring program's success, a natural question may be why a high-performing firm did not implement such a program earlier if it was so valuable. Here, at least anecdotally, we were told that bureaucracy and managerial barriers, similar to those Mark Zuckerberg wanted to remove in his "Year of Efficiency," were preventing progress. Thus, while organizational frictions may have historically delayed the adoption of such programs in large firms, our evidence suggests structured mentoring can offer a cost-effective approach to developing innovation capabilities when properly implemented.

In conclusion, our careful analysis of a small-scale mentoring program in a superstar firm provides actionable insights for leaders and policymakers seeking to enhance innovation quantity and quality. The results suggest that targeted mentoring programs are likely to be a cost-effective means of promoting innovation and inclusivity in the technology sector (Chien, 2024). The remainder of the paper is organized as follows: Section 2 describes the related literature and discusses our contributions. Section 3 describes the institutional setting and the design of the mentorship program. Section 4 describes our data and empirical approach. Section 5 presents the main results of the mentoring program's direct and indirect influence. Section 6 discusses the implications of our findings for scalability and provides a cost-benefit analysis of the mentoring program. Section 7 concludes with policy implications and directions for future research.

2 Related Literature and Contributions

For decades, corporate finance research has focused on asset-intensive, vertically integrated firms where executives exert significant control over employees (e.g., Stein (2003)). However, as early as 2000, scholars like Zingales (2000) and Pagano and Volpin (2005) called for a shift towards studying modern firms where human and intangible capital comprise the majority of assets. Now, this shift has been fully realized as these modern firms, with over 80% of their assets being intangible, dominate the economy (Grullon et al., 2019; Autor et al.,

2020; Tambe et al., 2020; De Loecker et al., 2020; Ayyagari et al., 2024; Kwon et al., 2024). Yet our understanding of the unique challenges these firms face due to labor's distinctive characteristics is still developing, and within-firm inequality and the direction of technological progress have emerged at the forefront among research topics in labor and finance (Nishesh, Ouimet, and Simintzi, 2024).

Our study, which explores the human capital development of innovators at a superstar technology firm, aims to fill in details regarding these new research topics and provides important insights for practitioners seeking to rationalize and improve their firm's human capital investment. By analyzing the staggered involvement of innovators in a mentorship program, we provide novel, causal evidence on the direct and indirect influence of such investments on innovation, productivity, and labor dynamics. This research contributes to studies on mentoring (Porter and Serra, 2020; Sandvik et al., Forthcoming) and advances our understanding of financing and investment decisions of the modern firms that represent the knowledge economy (Barkai, 2020; Crouzet and Eberly, 2023).

We build on prior studies that have established the importance of human capital as a driver of firm value and innovation. This research primarily focuses on cross-sectional or within-firm differences over time and identifies how management practices (Bertrand and Schoar, 2003; Bloom and Van Reenen, 2010; Bloom et al., 2013; Lazear et al., 2015; Chang et al., 2015; Benson et al., 2019) or corporate governance practices (Bernstein, 2015; Bena and Li, 2014; Brav et al., 2018) influence innovative output. Our research differs in two important ways. First, we focus on worker rather than firm heterogeneity, which allows us to speak to inequality across workers. Second, this deep dive into a single superstar firm allows us to more comprehensively explore the mechanisms that lead various groups of innovators—who are selected into the same firm, positions, and compensation levels—to achieve different innovative outcomes. Our analysis reveals how organizational frictions within firms can perpetuate innovation gaps, even among equally qualified individuals.

While prior work has documented how socioeconomic background (Bell et al., 2019a;

Duchin et al., 2020), the patent application processes (Pairolero et al., 2022; Aneja et al., 2024), and the commercialization process (Hochberg et al., 2023; Marx et al., 2024; Cook et al., 2025) create barriers for underrepresented inventors, we show that internal firm structures—including informal networks, mentorship access, and cultural norms—can either amplify or mitigate these challenges. By examining the complete innovation pipeline, from initial idea generation through patent application and grant, we identify specific points where organizational practices influence outcomes for underrepresented innovators.

Our analysis of the immediate and long-term relations between human capital development programs and innovative output provides causal evidence for how targeted investments, like mentorship programs, generate measurable outcomes, particularly patents, that drive firm value (Kogan et al., 2017; Farre-Mensa et al., 2019). This evidence helps to clarify the formation process of intangible capital and improve the precision of their valuations (Edmans, 2011; Eisfeldt and Papanikolaou, 2013; Cohen et al., 2013; Adams et al., 2021; Shu et al., 2022). This complements existing research exploring how financial constraints influence innovative outcomes (Brown et al., 2009; Hall and Lerner, 2010; Mann, 2018; Hochberg et al., 2018; Appel et al., 2019; Bernstein et al., 2020; Bena et al., 2022; Bian et al., 2024).

We find that informal systems—social networks, peer effects, and perceptions of corporate culture—are key mechanisms leading to heterogeneous outcomes. These mechanisms relate to emerging research on how non-wage benefits influence effort and turnover, such as the talent of one's coworkers (Sedláček and Sterk, 2017; Barry et al., 2022; Liu et al., 2023; Simintzi et al., 2024). Our findings contribute to understanding how social networks and cultural norms influence inclusive innovation (Li et al., 2020; Graham et al., 2022a,b, 2023; Gorton et al., 2022; Grennan and Li, 2023), with particular attention to how organizational practices can create or remove barriers for underrepresented innovators. By documenting significant indirect spillovers from mentored innovators to non-mentored innovators, we advance our understanding of peer influence (Hochberg et al., 2007; Mas and Moretti, 2009; Lyle, 2009; Leary and Roberts, 2014; Grennan, 2019; Ouimet and Tate, 2020) and how knowledge

diffuses within firms (Fang and Huang, 2017; Sandvik et al., 2020; Hacamo and Kleiner, 2022; Subramani and Saksena, 2023). Importantly, these spillover effects are particularly pronounced for underrepresented innovators, suggesting that targeted mentorship programs can help create more inclusive innovation environments through direct and indirect channels.

In addition, we contribute to a growing body of research that analyzes the innovation process within firms, highlighting how legal and institutional details influence IP protection (Cohen et al., 2019; Mezzanotti, 2021; Gerhardt et al., 2002; Chien and Ouellette, 2023; Goodman, 2023; Chien, 2024). By examining how mentoring influences the progress of innovative ideas submitted by innovators to patent attorneys, we show that the relation between mentoring and productivity is amplified for underrepresented innovators. This provides insights for decision-makers into how their institutions can improve diversity and innovation (Adams and Lowry, 2022), and suggests that cross-function collaborations—for example, between law and engineering—can lead to more inclusive innovation.

3 Firm Setting

The mentorship program that we study occurred within a firm that develops and manufactures high-technology services and products. For example, services include automation and cloud management, data storage and collaboration, networking, and security capabilities, and products include business and personal software, hardware, and telecommunications equipment. The firm employs over 50,000 individuals, many of whom are engineers and software developers. The firm is headquartered and incorporated in the United States. It has over 20 offices in the United States and operates over 100 offices in more than 80 countries outside the United States. The firm offers employees flexible work arrangements, including in-office, work-from-home, and hybrid options.

The firm is publicly traded on Nasdaq, and it is a part of the S&P 500. The firm relies heavily on research and development. As such, investments in human capital are an especially important aspect of the firm's business strategy. To this end, the firm instituted a

mentorship program aimed at helping its employees, specifically its engineers, gain the skills and knowledge necessary to develop and patent their inventions successfully. While the mentorship program was open to all employees, those from underrepresented demographic groups (e.g., women and racial/ethnic minorities) were especially encouraged to participate. The original advertisements for the mentorship program were sent to all employees involved or potentially involved in the innovation process, including those in upper management positions, so they could encourage their direct reports to apply. In this regard, two of the explicit goals of the mentorship program were (1) to "develop talent with a focus on intellectual property and innovation" and (2) to "diversify invention submissions and patentable inventions from all parts of the [firm's] talent pool."

The mentorship program occurred in seven cohorts from Q4 of 2020 to Q3 of 2023. For the initial cohorts, the program lasted approximately two months, and for later cohorts, the program lasted for four months. The content and curriculum remained largely the same across cohorts. As this program was voluntary and not part of regular work assignments, participants were credited with the time required to complete the program, to reduce the chance that they fell behind on their regular tasks and responsibilities. The start dates of each cohort of the program were as follows: 2020-Q4, 2021-Q1, 2021-Q3, 2021-Q4, 2022-Q3, 2023-Q1, and 2023-Q3. Employees who were interested in participating in the program had to submit an application, wherein they had to acknowledge their commitment to engage with the program for several hours per week for the duration of the program. There was limited space within each cohort, with the main constraint on cohort size being the number of employees willing and able to mentor the program participants. Other constraints included a lack of overlap in geographical locations (e.g., if no one was located in the preferred time zones of a particular applicant) and a lack of overlap in common invention interests (e.g., if only one person expressed interest in a particular research topic). The topics of common interest included (but were not limited to) the following: data analytics (e.g., machine learning); A.I. in security; A.I. in customer experience; and automation and cloud

services. The mentors were all experienced inventors with proven track records of invention development and patenting success. They were instructed to teach research principles, ensure a fair division of labor among the team, connect mentees with resources, and coach mentees on writing effective invention disclosures.

At the beginning of the mentorship program, participants within a cohort were placed into small groups of approximately four to five mentees. Each group was randomly assigned one or two mentors. The program's curriculum had mentors guide their group of mentees through the invention development and patenting process. Figure A.1 illustrates the typical timeline by which these events took place, with teams beginning to work on R&D solutions to their chosen problem statement about a month after their first interactions and then with teams starting to draft up their solution disclosures at the two-month mark, with submissions occurring shortly after the three-month mark. Each team's invention submission culminated in a showcase event, during which the mentees presented their work to senior technical talent teams. In most cases, groups were able to successfully generate and submit an invention disclosure within four months of starting the mentorship program. While these invention submissions constitute successful R&D outcomes in and of themselves, the main purpose behind the mentorship program was to cultivate within the participants the skills and knowledge necessary to develop and patent their own inventions outside of the mentorship program setting. In the following sections, we quantify the direct benefits of the mentorship program such as invention submissions, and we estimate other, possibly unintended, benefits of the program, such as spillovers from program participants to those who did not participate in the program.

4 Data Sources, Innovator Characteristics, and Empirical Specifications

4.1 Data Sources

Estimating the relation between mentorship and the quantity and quality of ideas disclosed as invention disclosures and patent applications requires a detailed panel of innovator-level

data. The firm provided us with this data, which covers all invention disclosures submitted by the firm's employees from 1998–2023. In total, the dataset contains 66,397 unique disclosures filed by 29,211 unique innovators, with most disclosures being developed and submitted by a team of innovators.

This data includes demographic details for most of the individuals, including their gender, age, marital status, race/ethnicity, job role, and the country in which they work. We use the race/ethnicity data to identify URM innovators and define them as those who are neither Asian nor White.

We use these data to form an innovator-quarter level dataset, in which we count the number of invention disclosures each innovator submits in a particular calendar quarter. Our data includes details on the status (as of March 2024) of each disclosure. We categorize disclosures based on their status after submission through the internal review process. Status 2 disclosures are those with any of the following statuses: Approved - Patent Application; Prior Art Search Requested; RFC (return for comments). Status 1 disclosures are those with any of the following statuses: Declined; Auto Declined; Defensive Publication Candidate; Approved - Defensive Publication; Approved - Trade Secret; Deleted; Merged.[6] Figure A.2 highlights the patent process as taught to mentees by mentors as part of the mentorship program. The main terminal outcomes that an invention disclosure submission can realize are (i) being declined, (ii) being approved for defensive publication, and (iii) being a filed or an approved patent. Submissions can also be returned for comments (RFC) by the review committee.

The firm also provided us with data on which individuals were part of each cohort of the mentorship program. This dataset includes details on who each person's mentor was, allowing

[6]For our main analyses, we do not group "Approved - Defensive Publication" and "Approved - Trade Secret" in with the other Status 2 disclosures because defensive publications and trade secrets are generally less valuable, relative to patent applications. In addition, we omit disclosures with the statuses of "Submitted" or "Filing Candidate" from the Status 1 group because those disclosures have not yet been reviewed. They do, however, get included in the *Any Status* group. In Appendix B, we discuss the classification and provide evidence from a series of robustness tests that support the conclusion that our inferences are not sensitive to these classification decisions.

us to account for the observable characteristics of the innovator's mentor. This dataset included data on innovator tenure with the firm and within the industry more broadly. For innovators not in the mentorship program, we have to infer their tenure with the firm based on the timing of their first observed disclosure (we set their hire date to be one year before their first disclosure date). We do not impute industry tenure for innovators without this information.

4.2 Innovator Characteristics

In Table 1, we report the demographic characteristics of the participants in each cohort of the mentorship program. We report innovator characteristics separately for each of the seven cohorts. We also report the demographic characteristics of the non-mentored innovators. There were 101 innovators in the first cohort of the mentorship program. The average participant was 33 years old, and they had about 2.5 years of tenure with the firm and 5.5 years of tenure in the industry. Ninety percent of the first cohort participants were female, 71% were Asian, 2% were Black, 15% were Latinx, 13% were White, and 17% were URMs.[7] Eighty-one percent of the first cohort participants were engineers, 4% were "top" engineers (a distinction signifying their rank within the firm), and 3% were data scientists. The rest of the innovators have alternative job titles, such as "management" and "executive." The participants in the second cohort (N = 73) and third cohort (N = 171), are quite similar to cohort-one participants in their demographic characteristics. The participants in cohorts four through seven are fairly similar as well, though the fraction of male participants increases substantially in the later cohorts (37%–48%, up from 10%–22% in the first three cohorts).[8]

To understand how representative the mentorship program participants are of the broader innovator population, we compare the characteristics of the mentored innovators to those of the over 28,500 innovators who were not involved in the program. We find that the non-

[7]Individuals can be members of multiple different racial/ethnic groups, which is why these percentages sum to a number greater than 100%.

[8]The change in cohort composition was in response to requests from human resources to be more gender inclusive.

participants are, on average, older and more tenured than the mentored innovators. They are also more likely to be male and/or White. These differences come as little surprise, given that the firm specifically targeted the mentorship program towards individuals from under-represented demographic groups, and as it is expected that innovators who are relatively new to the firm would be more likely to seek out mentorship resources to help them get up to speed on the firm's invention development and disclosure process. Interestingly, though, we see in the bottom rows of Table 1 that the pre-mentorship quarterly disclosure outcomes of mentored innovators are quite similar to those of non-participants. This indicates that innovators from the two groups exhibited similar levels of productivity at baseline, suggesting that non-mentored innovators can be used as a helpful comparison group as we estimate the relation between mentorship participation and invention disclosure outcomes.

4.3 Empirical Specifications

To estimate the relation between the mentorship program and the participants' invention outcomes, we compare their quarterly disclosure outcomes before and after they begin the program. We use a multi-pronged empirical strategy to mitigate bias in our estimates and ensure the robustness of our statistical inferences. Specifically, we use both a TWFE estimator and a stacked difference-in-differences estimator. Further, we also explore the robustness of our results using different control groups as a basis for comparison, including never-mentored innovators, not-yet-mentored innovators, and propensity-score-matched innovators.

4.3.1 Two-Way Fixed Effects Estimator

To test for changes in disclosure outcomes after mentorship, we use a TWFE estimator, exploiting the staggered timing of the different mentorship cohorts and the presence of never-mentored innovators. To implement this estimator, we regress an innovator's quarterly disclosure outcome values on the binary indicator *Mentored*, which equals one in the quarter that their mentorship program begins and each subsequent quarter, and zero otherwise. We

include innovator and year-quarter fixed effects, allowing us to estimate within-innovator level changes in productivity, using as a benchmark for comparison those not participating in the mentorship program at that time (or ever). We estimate the following model using ordinary least squares:

$$y_{i,t} = \alpha_i + \beta_1 \text{Mentored}_{i,t} + \lambda_i + \gamma_t + \varepsilon_i, \tag{1}$$

where $y_{i,t}$ is the number of disclosure submissions of a particular status that innovator i has in quarter t, λ_i is an innovator fixed effect, γ_t is a year-quarter fixed effect, and ε_i is an idiosyncratic error.

4.3.2 Stacked Difference-in-Differences Estimator

A critique of TWFE estimations with staggered treatment timing is that bias can stem from comparing already treated subjects with just treated subjects (Barrios, 2021). This bias can be especially problematic if treatment effects are dynamic and evolve over time (Baker et al., 2022). To circumvent these concerns, we use a stacked difference-in-differences estimator, which avoids the potential aforementioned bias (Cengiz et al., 2019). While Gormley and Matsa (2011) first applied stacked difference-in-differences estimation in finance research, the formal econometric properties of this approach were developed and established more recently.

To implement the stacked difference-in-differences estimator, we create a new data set that compares the disclosure outcomes of innovators who participate in a particular cohort of the mentorship program to the outcomes of a set of "clean" control innovators. For each cohort of treated innovators, we use two control groups of innovators: (1) those that never participate in the mentorship program (i.e., never-mentored innovators), and (2) those that first participate in the mentorship program in a later cohort (i.e., not-yet-mentored innovators). One benefit of using not-yet-mentored innovators (i.e., eventually treated subjects) as a control group is that it allows us to exploit variation in the timing of mentorship pro-

gram participation, rather than variation in the decision to participate at all (Deshpande and Li, 2019). In addition, not-yet-mentored innovators are likely to be selected on the same unobservable characteristics that lead currently-mentored innovators to participate in the mentorship program. Furthermore, there were instances in which innovators wanted to participate in earlier mentorship cohorts but were unable to do so due to mentorship cohort capacity limits, so for some mentored innovators, the timing of their participation was quasi-random.

We aggregate all cohort-specific data—i.e., the innovator-quarter observations for innovators treated in a specific cohort and the observations for that cohort's control group innovators—then we stack the aggregated data for all seven of the mentorship program cohorts. This allows us to estimate the following event-study regression using ordinary least squares (OLS):

$$\mathrm{y}_{i,t,g} = \beta_1 \mathrm{Mentored}_{i,t,g} + \alpha_{i,g} + \lambda_{t,g} + \varepsilon_{i,t,g}, \tag{2}$$

where, $y_{i,t,g}$ is the disclosure outcome variable of interest for innovator i in quarter t for mentorship cohort g. $\alpha_{i,g}$ is an innovator-by-cohort fixed effect, $\lambda_{t,g}$ is a year-quarter-by-cohort fixed effect, and $\varepsilon_{i,t,g}$ is an idiosyncratic error term. Finally, standard errors are double clustered by innovator-by-cohort and year-quarter-by-cohort to account for repeated innovator observations across the cohorts (i.e., as the same innovator can be used in the control group for multiple cohorts).[9] This estimation approach is analogous to estimating separate difference-in-differences treatment effects for each mentorship cohort group, using never-mentored or not-yet-mentored innovators as controls, and then taking the average of the treatment effects across all seven cohorts.

[9]The inclusion of time-related fixed effects (year-quarter and year-quarter-by-cohort) and the clustering of standard errors by either year-quarter or year-quarter-by-cohort accounts for the serial correlations in disclosures outcomes that occur across time.

4.3.3 Common Trends Tests

Before we tabulate and discuss our main findings, we first analyze the evidence to see if the common trends assumption, which is necessary for identification, appears to hold. While it is not possible to definitively establish if the assumption is met, these tests can provide suggestive evidence in support.

Our difference-in-differences estimations rely on the assumption that the disclosure of inventive ideas by mentored and non-mentored innovators would follow a common parallel trend in the absence of the mentorship program. To evaluate the credibility of this assumption, we compare the pre-mentorship trends in the quarterly disclosure submission outcomes between mentored and not-yet-mentored innovators (i.e., those who are eventually mentored in a later period).

Figure 1 plots quarterly disclosure submission patterns around the mentorship initiation quarter. The points reflect the relative differences in disclosure submissions between mentored innovators and non-mentored innovators in each quarter. Navy dots with light blue rectangles show the point estimates and 95% confidence intervals for quarters preceding mentorship participation, while maroon dots with light red rectangles show the corresponding estimates for subsequent quarters. We generate these estimates using the Callaway and Sant'Anna (2021) methodology, which addresses potential limitations of the TWFE approach by allowing for more flexible dynamics and reducing potential bias in settings with staggered program adoption.

Following the same procedure, Appendix Figures A.3a and A.3b present corresponding analyses for Status 2 and Status 1 disclosures, respectively. Across all specifications, whether examining combined or separate disclosure types, the figures show consistent patterns in submission rates between mentored and non-mentored groups before the implementation of the mentorship program. This is consistent with no diverging pre-trends in disclosure outcomes. We formally test whether the pre-mentorship coefficients differ from zero. The statistical tests fail to reject the null hypothesis that these coefficients equal zero, with p-values rang-

ing from 0.321 to 0.829. These patterns suggest comparable submission trajectories between future mentored innovators and non-mentored innovators in the quarters preceding program participation.

5 Results

This section presents our empirical findings in four parts. First, we examine the direct relation between mentorship participation and subsequent innovation activities, documenting both immediate and long-term patterns in disclosure submissions. We then analyze how these patterns differ systematically across participant characteristics such as gender and ethnicity (Section 5.1). Second, we analyze survey evidence to explore potential mechanisms associated with mentorship and innovative behavior, examining patterns in knowledge sharing, network formation, and confidence building (Section 5.2). Third, we examine potential indirect spillovers along four dimensions: the relation between mentored and non-mentored collaborators' productivity, patterns in team formation and demographic composition, workplace perceptions and corporate culture, and talent retention rates (Section 5.3). Finally, we address possible alternative explanations for the observed relation between mentorship and innovation output (Section 5.4). Throughout our analysis, we acknowledge potential confounding factors and conduct multiple specification tests to examine the robustness of the observed relations.

5.1 The Mentorship Program and Innovator Productivity

We begin discussing our results by focusing on the direct relation between the mentorship program and mentored participants' invention disclosure submissions, our primary observable productivity outcome measure. We estimate the overall and long-term relation and discuss the robustness of the observed relation to alternative sampling restrictions and econometric specifications. We also consider how heterogeneity across participant demographic characteristics correlates with the propensity to submit invention disclosures.

5.1.1 Mentorship Program Participation and Overall Productivity

We report the relation between mentorship and various disclosure submission outcomes in Table 2. We use both not-yet-mentored innovators and never-mentored innovators as the basis for comparison. The dependent variable in Columns (1)–(2) is the number of unique submissions of any status that an innovator submitted in a given quarter. The dependent variables in Columns (3)–(4) and (5)–(6) are the number of unique submissions at Status 2 and Status 1, respectively, that an innovator submitted in a given quarter. In the odd-numbered columns, we use the TWFE, which includes innovator and time fixed effects. We use the stacked difference-in-differences estimator in the even-numbered columns. The sample size increases when using the stacked difference-in-differences estimator because we form control groups by drawing from the sample of control innovators with replacement.

We draw consistent inferences regardless of the empirical specification used: participating in the mentorship program relates positively to disclosure submission outcomes for innovators. The coefficient estimates in Columns (1)–(2) suggest that mentorship is associated with an increase in disclosure submissions of 0.27–0.42 each quarter, which translates to approximately 1–1.5 additional disclosures per innovator each year. This positive association between mentorship and innovator productivity is economically meaningful and statistically significant in each specification, p-values < 0.01. In Columns (3)–(4) and Columns (5)–(6), we find evidence of significant increases in both Status 2 submissions and Status 1 submissions, respectively, p-values < 0.01 in all cases. The coefficient estimates are 3–4 times larger when considering increases in Status 1 submissions, relative to increases in Status 2 submissions, suggesting that the overall productivity gains are largely driven by Status 1 submissions. This pattern aligns with the nature of Status 1 submissions, which represent a lower technical threshold since they are either initial disclosures awaiting full patent attorney review or previously declined submissions undergoing revision for resubmission. The greater prevalence of Status 1 submissions thus likely reflects both their lower completion requirements and the iterative nature of the disclosure refinement process, where innovators

can more readily generate and revise for resubmissions.

These findings echo the patterns observed in Figure 1 and Figures A.3a–A.3b, which show visual evidence of a significant, positive relation between mentorship and innovator productivity in the quarter of the mentorship program and the quarters following the program.

5.1.2 Mentorship Program Participation and Long-Term Productivity

As mentioned, the mentorship program was designed to help innovators gain the skills, confidence, and knowledge necessary to develop and patent their inventions successfully. To this end, the program's curriculum required mentors to guide their mentees through the invention development and patenting process, with disclosure submission being an expected outcome. Given this expectation, it might be unsurprising that participation in the mentorship program caused innovators to realize gains in disclosure in the near-term. What really matters, from both the firm's perspective and that of the innovators, is that participants finished the program with the skills, confidence, and knowledge necessary to successfully develop and patent their inventions independently.

To test whether this benefit of mentorship occurred, we estimate the long-term relation between mentorship and disclosure outcomes. We use the same specifications as we did in the analyses in Table 2, but we remove the observations of mentored workers in the quarter of their mentorship program and the first and second quarters after the program. In doing so, we exclude the disclosure submissions that most likely occurred directly as part of the mentorship program curriculum, allowing us to isolate post-mentorship influence. We tabulate these results in Table 3. The estimated relation between innovator mentoring and aggregate disclosure counts in Columns (1)–(2) continues to be positive and statistically significant at the 1% level. The estimated relation for Status 2 disclosure counts in Columns (3)–(4) and the Status 1 disclosure counts in Columns (5)–(6) are positive and significant at either the 1%, 5%, or 10% level. The economic magnitude is approximately one-third of the corresponding estimates from Table 2, but they still constitute a meaningful amount of

additional innovation in the long-run from participation in the program (approximately 0.5 additional disclosures per innovator each year). Thus, these results suggest that mentored innovators gained the skills and knowledge necessary to independently develop and disclose inventive ideas that would lead to successful patent applications, further highlighting the merits of the mentorship program.

We show in Appendix B that the estimated relation between mentorship and productivity is robust when using alternative control groups (e.g., only not-yet-mentored innovators or only never-mentored innovators), when employing different estimators (e.g., the Callaway and Sant'Anna (2021) estimator or a Poisson estimator), and when using different disclosure status classifications (e.g., considering only submissions that have become approved patents). We also discuss the results of several other tests that bolster our main inferences. For instance, the relation between mentorship and productivity is similar when we estimate effects separately for each cohort, and the relation is robust when we account for the COVID-19 global pandemic by removing all pre-COVID data from the analysis.

5.1.3 Heterogeneity Tests for Mentorship and Innovation Productivity

Next, we consider whether certain mentees, based on their demographic characteristics, are more or less likely to have positive productivity gains from participating in the mentorship program. While the mentorship program was available to all employees, regardless of their gender or race/ethnicity, a major goal from the firm leadership's perspective was to assist employees from demographic groups that have historically been underrepresented in the innovation and patenting process (Bol et al., 2023). As such, it may have been the case that female and URM innovators benefited more from the program if, for instance, they were especially engaged in the curriculum or if they started with lower confidence or less knowledge of the patenting process than their peers.

In Table 4, we report the results from these heterogeneity tests, wherein we continue to employ the TWFE estimator in the odd-numbered columns and the stacked difference-

in-differences estimator in the even-numbered columns. In Panel A, we interact *Mentored* with the binary indicator *Female*, which allows us to estimate whether female mentees realize more or less mentorship program benefits than do male mentees. The coefficients on *Mentored* × *Female* are positive in all six columns, and the estimate in Column (2) is statistically significant (p-value < 0.10). Similarly, in Panel B, where we interact *Mentored* with the binary indicator *URM*, we estimate positive interaction coefficients in all columns, but none of the estimates are significant. In Panel C, we consider gender and URM status in tandem by interacting *Mentored* with the binary indicator *Female/URM*, which equals one if the innovator is either female or a URM, and zero otherwise. Again, we estimate positive coefficients on the interaction term in all six columns, and in this case the estimates in Columns (2) and (4) are statistically significant at the 10% and 5% levels, respectively.[10]

Taken together, the coefficient estimates on the interaction terms in Table 4 provide some evidence (albeit noisy evidence) that female and URM innovators benefited more from the mentorship program than did male and non-URM innovators. Importantly, though, the average treatment effects, which include male and non-URM innovators, continue to be positive and statistically significant, as indicated by the positive and statistically significant coefficients on *Mentored* in all columns and panels of Table 4. This suggests that all participants benefited from mentorship program participation regardless of their demographic characteristics.[11]

Next, we explore whether heterogeneity in mentor quality is meaningfully associated with the relation between mentorship and productivity. To delineate between high and low levels of mentor quality, we estimate mentor fixed effects using the following approach. First,

[10]These tests require data on the gender and/or race/ethnicity of the individual. We do not have data on these characteristics for all individuals, leading to the reductions in observation counts.

[11]In Table A.1, we consider whether more pronounced productivity gains are present based on the country in which the innovator works (Panel A) or based on whether the innovator participated in one of the first three cohorts or the latter cohorts (Panel B). The coefficient estimates on the interaction term in Panel A vary in sign, and they are never statistically significant. The coefficient estimates on the interaction term in Panel B also vary in sign, with only one estimate, in Column (6), being statistically significant at the 10% level. Given the variation in signs across specifications and best practices under multiple hypothesis testing (Harvey et al., 2015), it is best to conclude that we fail to reject the null hypothesis that geographic or timing differences matter.

for the mentees in the sample, we regress their quarterly number of submissions during and after their first quarter in the mentorship program on their demographic characteristics (age, tenure with firm, tenure in industry, gender, underrepresented minority status, and job role), and we include fixed effects from year-quarter, their initial mentorship cohort, and their work country. We save the residuals from this estimation, and then, for a given mentor, we take the average residuals across all the mentees that the mentor supervised. These averages become the mentor-fixed effects. We then split mentors into above versus below the median value of the mentor fixed effects, creating a *High Mentor FE* indicator, and we interact this indicator with the *Mentored* indicator and re-estimate our main regression models.

The results are reported in Table 5. The positive and statistically significant coefficients on *Mentored* in all six columns indicate a positive relation between mentorship and innovation outcomes among those mentees whose mentors had relatively low fixed effects. The positive and significant coefficients on the interaction term indicate that mentees with high-fixed-effect mentors realized an even greater (i.e., more positive) association between their involvement in the mentorship program and their innovative output. These findings have important implications for the firm's scale-ability of the mentorship program. While the highest quality mentors appear to be associated with the greatest program returns, it is evident that even the lower quality mentors can help participants gain the skills and knowledge they need to successfully develop their innovative ideas.[12]

5.2 Exploring the Mechanisms via Survey Response Data

To better understand the mechanisms through which mentorship works and the barriers innovators face when trying to patent their inventive ideas, we surveyed innovators involved in the mentorship program. This survey was part of a larger, multi-firm interview and survey process conducted by Chien and Grennan (2024). The study began with interviews of patent

[12]We also consider heterogeneity based on alternative mentor characteristics in Table A.2. In general, we find little evidence of significant differences, suggesting that mentors were, on average, quite effective regardless of their gender, job status (i.e., "top engineer"), or prior involvement in the mentorship program.

professionals, mostly general counsels or heads of IP. This particular survey was a small, demographically representative sample of engineers and technical staff in positions deemed by the focal firm as capable of contributing to IP development but who had not necessarily already developed a patent. For our purposes, the firm oversampled from engineers who had participated in the program. Overall, the response rate was about 17%, which is comparable to other corporate surveys (Graham et al., 2005). Consistent with the oversampling, Panel A of Table 6 shows that 43% of the survey respondents had participated in the mentorship program.

Panel B of Table 6 begins by summarizing the perceived experiences of those without a mentor. It shows that the engineers who did not participate mostly did not participate because they did not know about the program. However, women are significantly more likely than men to believe that patent mentorship would not benefit them (11% vs. 3%). This works against the sample selection, as women are more likely to participate.[13] We observe statistically indistinguishable rates of inability to participate across demographic groups. This again supports our identification approach that the capacity constraints on the mentorship program introduced quasi-random variation in participation timing.

Panel C of Table 6 summarizes the perceived experiences of those with a mentor. The frequency of meetings with their mentors after the formal mentorship program ends is consistent across demographic groups, with mentees suggesting their mentor provides advice, suggestions, or support around twice a month. When looking at the perceived benefits, mentored innovators reported benefits from the mentorship received. Mentees indicate a desire to work on more inventions, greater confidence in their ability to incorporate their mentor's tips into their work process, a greater likelihood of having the social connections necessary to get their invention ideas accepted by patent professionals, and having more social connections from introductions to innovators that they anticipate working with. Females' perceptions of mentorship are more positive. Females indicate a statistically significant increase in job

[13]These survey findings relate to recent work on the association between self-selection and treatment effects in human capital development programs (Sandvik et al., Forthcoming).

satisfaction of 1.4 units, compared to 1.0 units for their male counterparts. Females also perceive that they have benefited more in terms of learning about patenting, at 1.5 units compared to 0.9 units for their male counterparts. Overall, the data suggests that mentorship, particularly when formalized, could be a key mechanism for fostering greater inclusivity in the invention process among engineers.

5.3 Spillovers from the Mentorship Program

We find convincing evidence that the mentorship program had a direct, positive influence on the near-term and long-term productivity of the program participants. In this subsection, we extend our analyses to investigate whether innovators who did *not participate* in the program realized any potential benefits by interacting with mentored innovators.

Several hypotheses guide these analyses. First, it is possible that program participants shared their newfound skills and knowledge with their non-mentored peers, leading to indirect benefits of the program. Additionally, the program may influence perceptions about the benefits of diverse innovator teams, potentially leading to changes in subsequent team formation and composition. The program's implementation may also affect employees' overall perceptions of the firm, potentially influencing factors such as job satisfaction and organizational commitment. Furthermore, the presence of mentored colleagues may impact the likelihood of non-participants remaining with the firm, either positively through knowledge spillovers or negatively through perceived disparities in career development opportunities. We also consider that non-mentored employees may experience relative declines in productivity or career progression compared to their mentored peers, potentially due to resource allocation or attention disparities. Lastly, the program's selective nature may influence non-participants' motivation and engagement, potentially leading to either increased effort to match perceived advantages or decreased morale due to exclusion.

The mentorship program's influence on non-participating employees likely involves multiple interconnected relationships that could be either complementary or offsetting. To system-

atically examine these relationships, we implement several econometric specifications that measure different types of spillover patterns. This comprehensive empirical approach allows us to document the intended and unintended relations between the mentorship program and the broader organizational environment. Specifically, we examine how the program correlates with non-participants' productivity, collaboration patterns, workplace culture, and talent retention.

5.3.1 Productivity of Non-Participants

To explore potential productivity pattern changes among non-participants following their exposure to mentored colleagues, we restrict our sample to only include innovators who were not directly associated with the mentorship program at any time, either as a mentor or mentee. For this group of non-participating innovators, we analyze their disclosure submission records to determine when they were first part of an invention disclosure team with an individual who had previously been mentored as part of the program. We create an indicator variable, *Post First Exposure*, which equals one in the first quarter a non-participating innovator is observed to collaborate with a mentored innovator and each subsequent quarter, and zero otherwise. We observe 520 non-participating innovators in our sample who collaborated with a mentored innovator. We then regress disclosure submission outcomes on this indicator using both the TWFE estimator and the stacked difference-in-differences estimator. We report the results in Table 7. We estimate positive coefficients in all six columns, though the inferences about mentored collaborator exposure are only statistically significant in Columns (1), (2), and (4). These results provide evidence consistent with the mentorship program having some positive benefits for non-participating innovators' productivity.

In Table A.3, we consider the extent to which there is heterogeneity in the productivity gains based on the non-participating innovator's demographic characteristics. In Panel A, we interact *Post First Exposure* with the binary indicator *Female*, which allows us to estimate whether female non-participating innovators realize different productivity patterns

than male non-participating innovators when they are exposed to mentored collaborators. The coefficients on *Post First Exposure* × *Female* are negative in Columns (1)–(4), and they are statistically significant in Columns (3)–(4), p-value < 0.10, which suggests that the non-mentored female innovators in our setting may benefit less from exposure to mentored collaborators than non-mentored male innovators when it comes to Status 2 submissions. One potential reason for this pattern may be related to information sharing. Survey evidence from engineers suggests that women engineers are less likely to be privy to the information necessary for inventive purposes and are less likely to have a strong network of engineers to work with (Chien and Grennan, 2024). Both of the patterns evident in the survey are consistent with our observation of women seeing fewer indirect productivity gains from having a mentored colleague.

We note, however, that the estimates are positive in Columns (5)–(6), and they are statistically significant in Column (6), p-value < 0.05, which covers Status 1 submissions. Similarly, in Panel B, where we interact *Post First Exposure* with the binary indicator *URM*, we estimate statistically significant interaction terms but with opposite signs depending on whether we consider Status 2 versus Status 1 submissions. We encounter similar inconsistencies in Panel C, where we interact *Post First Exposure* with the binary indicator *Female/URM*, which equals one if the innovator is either female or a URM, and zero otherwise.

It may be the case that women and URMs benefit more than others across some outcomes but not others, or it may be that the data are too noisy to provide consistent inferences. Either way, we are cautious in our interpretation of these results. So, while it does appear that the mentorship program is associated with positive productivity gains for non-participating innovators, it is not clear whether certain types of non-participants benefited more or less than others.

5.3.2 Innovator Team Composition

Next, we explore whether the mentorship program is related to the demographic composition of mentored innovators' subsequent inventive teams. We may expect to see increases in team diversity due to the mentorship program for multiple reasons. First, the mentorship program may have exposed innovators to a more diverse set of possible collaborators with whom they could work. Second, the program may have highlighted the possible benefits of increasing the diversity of their collaborator teams. Third, the protocol materials for the mentorship program specifically emphasized creating space for "diverse perspectives and inclusivity," with an explicit goal being to "diversify invention submissions and subsequent patentable inventions from all parts of the engineering talent pool." Our empirical analysis investigates whether these program features correspond to systematic differences in the demographic composition of mentored innovators' research teams following program completion.

To do this, we employ the same two empirical approaches from our earlier tests: the TWFE estimator and the stacked difference-in-differences estimator. To study disclosure team diversity, we consider three separate outcomes, each at the innovator-quarter level. First, we consider the average level of female representation among an innovator's collaborator teams, based on the disclosure submissions they had in a particular quarter. If, for example, innovator i submitted two disclosures in quarter t, with two different teams, where one team was composed of 50% women and the other team was composed of 25% women, then the value of *Fraction Female* for innovator i in quarter t would be 37.5% = (50% + 25%) / 2. We also consider the average racial/ethnic composition of an innovator's collaborator teams each quarter. To do this, we compute racial/ethnic HHI metrics for each collaborator team, and then we take the average value across all collaborator teams for a given innovator-quarter. Similarly, we compute average gender-by-racial/ethnic HHI metrics for each innovator-quarter.[14]

[14]Smaller HHI values capture larger amounts of diversity (i.e., less concentration in representation among a single group).

These three team diversity outcomes become the dependent variables in Table 8. In Panel A, we use the full sample of innovator-quarter disclosure submission data, whereas in Panel B we remove near-term observations to estimate long-term effects. In Columns (1)–(2), the dependent variable is the average fraction female of the collaborator teams the innovator is a part of in a given quarter, and the dependent variables in Columns (3)–(4) and Columns (5)–(6) are the average racial/ethnic and gender-by-racial/ethnic HHI values, respectively, of the collaborator teams the innovator is a part of in a given quarter. We find evidence that participation in the mentorship program is positively related to the level of female representation and racial/ethnic diversity of an innovator's collaborator teams. The positive coefficients in Columns (1) and (2) of Panel A suggest that the female representation of mentored innovators' collaborator teams increases by 14–18 percentage points after their involvement in the mentorship program. The negative coefficients in Columns (3)–(6) suggest that racial/ethnic diversity increases, as does gender-by-racial/ethnic diversity. The results in Panel B suggest that some of these team diversity findings persist in the long-term, specifically the gender-by-racial/ethnic diversity of the teams.

This is important economically because the indirect influence of the mentorship program on the diversity of research teams post-mentorship could generate substantial material gains for the firm. For instance, Heath et al. (2023) measures the diversity and productivity of over 40,000 teams from open-source software projects, and they find that team diversity is positively associated with the survival, development, and popularity of the projects. They attribute this increase in performance to teams successfully avoiding the "homophily trap," whereby individuals' natural preference to associate with similar peers may cause research teams to miss out on the opportunity to collaborate with talented, albeit less similar, peers.

5.3.3 Perceptions of the Workplace

The survey results, discussed in Section 5.2, provide useful information about participants' perceptions of the mentorship program, but it is hard to determine if there is appreciable

bias given the anonymous, one-shot nature of the survey. Therefore, to explore the workplace implications of the program on a broader sample of employees, we turn to data from crowd-sourced employee reviews on Glassdoor, a career intelligence website that attempts to provide transparency about jobs, salaries, and companies. Specifically, we consider the relation between the mentorship program and employees' perceptions of their workplace environment. While only 633 innovators participated in the mentorship program (a small number of innovators compared to the total number of innovators within the firm), we have already documented multiple examples of spillovers that may have influenced the perceptions of innovators more broadly. Furthermore, an employee's knowledge that the firm is willing to invest in them by offering a mentorship program—even if they don't participate—could also impact their sentiment towards the firm.

We begin with the full sample of over 13 million individual Glassdoor reviews. Glassdoor has a star rating system, and a five-star rating is considered the top rating. We keep the reviews from current employees of the focal firm and its industry peers, restricting them to those with non-missing job titles (N = 294,277) during the period of study. The industry peer group is defined based on market capitalization, R&D expenditures, and industrial classification. To create a relatively balanced panel around the first cohort of the mentorship program, which began in October 2020, we keep reviews from January 2018 to November 2023, the last month in our dataset (N = 242,066).

First, we estimate the firm's overall star rating change on Glassdoor after the implementation of the mentorship program. Restricting to the 12,019 reviews left by employees of the focal firm, we regress the overall firm rating on the indicator *Post*, which equals one in and after October 2020. The results in Column (1) of Table 9 show that the ratings of the focal firm are significantly greater after the start of the mentorship program. However, this result could be due to general positive trends in employee ratings independent of the mentorship program. To account for this and other possible confounding factors (e.g., work-from-home initiatives increasing employee satisfaction), we consider whether the corre-

lation is more pronounced among engineers, who were the main program participants, than among the other firm employees. To do this, we interact the binary indicator *Engineer* with *Post*. The positive and significant coefficient on *Post* × *Engineer* in Column (2) shows that engineers experience a significantly larger increase in ratings than non-engineers. The fact that engineers are more likely to perceive the workplace as better than their non-engineering peers suggests that the increase in ratings is likely due, in part, to the mentorship program, which impacted engineers much more than the firm's other employees. We spoke to those originally administering the patent mentorship program to understand if there were other confounds present in the firm that may lead engineers to have a different perception of the firm for another reason while the mentorship program was first being implemented. The program administrators indicated that there were no other meaningful changes in the workplace environment for engineers at the time.

One potential concern is that the engineers' improved perceptions of the workplace are driven by broader macro-trends in employee satisfaction, possibly trends that impact engineers differently than non-engineers. Therefore, we also consider whether we observe similar patterns in sentiment among the employees of the focal firm's industry peers. In Columns (3) and (4) of Table 9, we replicate the analyses of Columns (1) and (2) using only the overall firm ratings of current employees from the focal firm's industry peers. In Column (3), we find evidence that peer firms also realize an increase in ratings after the beginning of the mentorship program in the focal firm, suggesting that ratings may tend to drift upward over time. However, in Column (4) we find that the coefficient on *Post* × *Engineer* is small (about a tenth the size of the coefficient in Column (2)) and statistically insignificant. The negligible coefficient on the interaction term for industry peers suggests that the improvement in overall Glassdoor ratings among engineers in the focal firm is not likely due to broader macro-trends in employee satisfaction. Specifically, we show that engineers' perceptions of the focal firm improve more than do the perceptions of engineers in peer firms by estimating the triple-interaction model in Column (5), wherein we find a positive and

significant coefficient on *Post* × *Engineer* × *Focal Firm*. We estimate similar effects when we use employees' ratings of the firms' culture and values.[15]

5.3.4 Innovator Departures

To finish, we explore potential associations between the timing of the mentorship program implementation and patterns in innovator turnover, focusing on differences between engineers and non-engineers, and between the focal firm and its peers. Using data from Revelio, a labor market intelligence platform that tracks employee movements across firms, we examine the relation between the mentorship program and the firm's ability to retain its human capital talent. The Revelio data is compiled using LinkedIn profiles and data from other employment-related social media platforms. Using this data, we construct a measure of the number of employees who exit the firm each month within specific categories of individuals based on their occupation, the region in which they work, their gender, their ethnicity, and their level of seniority. The measure we construct, *Outflows*, captures the number of employee departures that the firm experiences in a particular month within a specific occupation-region-gender-ethnicity-seniority group. We also use, as an alternative dependent variable, the inverse hyperbolic sine transformation of *Outflows*, which accounts for skewness in the data while accommodating year-months in which no turnover is observed within a particular group of individuals. We also develop these turnover metrics for the focal firm's peers, as we did in the analysis of engineers' perceptions of their workplace.

We then estimate OLS regressions that mimic those from the previous workplace perception analysis (in Table 9). Specifically, we report the results in Table 10. The estimate displayed in Column (1) shows that the firm experienced fewer departures in the months following the implementation of the mentorship program relative to the months before. In

[15] If we re-estimate these models for the other ratings measures provided in Glassdoor, we find that engineers in the focal firm, relative to non-engineers, provided increased ratings of their career opportunities, their compensation and benefits, and the firm's culture and values. Note, the triple-interaction regression model includes the *Post* × *Focal Firm* and *Focal Firm* × *Engineer* interaction terms, but, for brevity, we do not report these in the table. Similarly, all models include a constant, which we do not report.

Column (2), we find that this reduction in turnover is stronger among the firm's engineers (i.e., they experience even *less* turnover), relative to employees in the other occupational roles. These findings are statistically significant both when using the count *Outflows* measure in Panel A, and when using the transformed measure in Panel B. The transformed measure provides a tractable economic interpretation of the result, with engineers in the focal firm experiencing about 4% ($= e^{-0.040} - 1$) less attrition after the beginning of the mentorship program, relative to non-engineers. This seemingly modest decrease can translate into substantial economic benefits for the firm when considering the costs associated with employee turnover, particularly for highly skilled positions such as engineers, which we explore further in our ROI calculation presented in Section 6.

In Column (3), where we consider the focal firm's peer firms, we find evidence of *increased* turnover after the implementation of the mentorship program in the focal firm. In Column (4), we find mixed evidence as to whether engineers experience a differential average treatment effect, relative to non-engineers, as the coefficient on *Post* × *Engineer* is not statistically significant in Panel A, but it is significant in Panel B. However, Column (5) of both panels provides evidence that the reduced attrition among engineers is greater in the focal firm, relative to its peer firms.[16]

Overall, the empirical analyses of engineers' departures reveal a consistent pattern across multiple specifications, indicating a negative association between the implementation of the mentorship program and engineer departures from the focal firm. While inferring a direct causal relation is challenging, the robustness of this relation across various model specifications and derived from a quasi-experimental design supports a link between the presence of the mentorship program and lower turnover rates among engineers. This departure pattern, which was observed consistently in our analyses, points to a potential retention benefit and a workplace perception benefit coinciding with the mentorship initiative.

[16]We conduct falsification tests for the workplace perceptions and employee departure tests in Appendix B, which bolster the inferences we draw from the results in Table 9 and Table 10. We also discuss the robustness of the results when different iterations of *Post* are used.

5.4 Addressing Possible Alternative Explanations

One alternative explanation for the observed relationship between mentorship and innovator productivity relates to team composition patterns. Specifically, existing innovation teams might have simply incorporated newly mentored individuals into their ongoing projects, rather than mentorship leading to genuinely new innovative activities. Under this scenario, mentored individuals would show higher submission rates following program participation, as they join more project teams. However, these increased individual-level submissions would not translate into additional firm-wide innovation output since the underlying projects would have occurred regardless of the mentorship program. This distinction between individual and organizational outcomes is important for understanding the program's broader impact on innovation productivity.

However, the patterns we observe in disclosure submissions suggest genuine increases in firm-wide innovation productivity, rather than merely reflecting larger team sizes on existing projects. For instance, before the mentorship program, the average innovation team size for accepted patent applications that eventually-mentored innovators were a part of was 4.62. Whereas, during the mentorship program, it was 4.59, and after the program it was 4.31. None of these team size values are significantly different from each other (p-values range from 0.317 to 0.920). This provides strong evidence against the team-size-boosting concern for accepted patent application disclosures.[17]

To further mitigate the concern about inorganic increases in innovation team size, we use data from the U.S. PTO to compare patent grant team sizes in the focal firm to those in peer firms. In doing so, we show that the team size dynamics within the focal firm mirror those of peer firms, suggesting that the mentorship program did not prompt artificially larger patent

[17]It does appear to be the case that individuals who participated in the mentorship program tend to be on larger teams in general (both before, during, and after the program). Among innovator teams with no individuals who participate in the mentorship program (past, present, or future), the team size for accepted patent applications is 3.08. Importantly, though, the team sizes of innovator teams with no mentored individuals has increased since the start of the mentorship program, to 3.44. This suggests that innovation team sizes do increase over time, regardless of mentorship associations.

grant team sizes. These results are reported in Appendix Table A.4, where we use patent data from the U.S. PTO from 2018 to present. The dependent variable in Columns (1)–(3) is the total number of innovators listed on a particular patent, and the dependent variable in Columns (4)–(6) is the inverse hyperbolic sine of the total number of innovators listed on a particular patent. We employ OLS regressions to analyze how these patent team size measures change in relation to the binary indicator *Post*, which equals one from when the first cohort of the mentorship program began in the focal firm and onward (in Panel A).[18] Some models include interactions between *Post* and the binary indicator *Focal Firm*. The positive and significant coefficients on *Post* in Columns (1), (2), (4), and (5) suggest that patent grant team sizes are larger in the post-period than they were in the pre-period for both the focal firm and its peers, and the small, insignificant estimates on *Post* × *Focal Firm* in Columns (3) and (6) suggest that these changes are no different for the focal firm than for peer firms.

As additional evidence of the mentorship program's effectiveness in boosting overall innovative efficiency at the firm, we also estimate the time it takes for mentored innovators to achieve their second disclosure after submitting their first disclosure. Across all innovators who are not part of the mentorship program, and who we observed submitting at least two disclosures, the time-to-second-submission is about 1.25 years (i.e., five quarters). The average value of time-to-second-submission is very similar for individuals who participated in the mentorship program, but who achieved their second submission before the beginning of the program. In contrast, the average value of time-to-second-submission is less than one year (i.e., less than four quarters) among individuals who achieved their second submission after they enrolled in the mentorship program. Furthermore, innovators who had their first submission experience during or after the mentorship program had an average time-to-second-submission of about one quarter. These findings suggest that the mentorship program substantially expedited the idea-development learning curve among mentored

[18] In Panel B (Panel C), *Post* equals one from when the second (third) mentorship program began and onward.

innovators.

Finally, we do not find evidence of meaningful differences in the quality of the disclosure submissions made by teams with mentored innovators, relative to those made by teams with no mentored innovators. Among all disclosure submissions made by teams with no mentored innovators, approximately 65% have the status of "Declined," whereas the declined rate is only 62% for teams with currently or previously mentored innovators. Furthermore, 35% of the submissions from teams with no mentored innovators have the status of "Approved - Defensive Publication/Patent Application/Trade Secret," "RFC," or "Submitted," and 35% of the disclosure submissions from teams with currently or previously mentored innovators do.[19] These patterns in disclosure status indicate that, during and after the mentorship program, mentored innovators were a part of valuable innovation projects.

6 Return on Investment to the Firm

This section develops an analytical framework to evaluate the returns from the mentorship program, building on established methodologies for assessing human capital investments and organizational productivity (Black and Lynch, 1996; Bell et al., 2019b; Lazear et al., 2015). Our approach begins with directly quantifiable outcomes to establish a baseline estimate of the program's value, then extends to consider broader organizational benefits that are more challenging to measure precisely. For each component of our analysis, we explicitly state our underlying assumptions and detail our calculation methodology. While acknowledging the inherent complexity in measuring returns to human capital programs, we construct careful estimates of the program's ROI by systematically examining both direct innovation outcomes and indirect organizational benefits.

Our exercise incorporates the many value creation streams described in our empirical tests. Specifically, we begin by analyzing the direct channel through which patent mentorship creates value for the firm. Specifically, we observed an increase of 1.5 invention disclosures

[19]The other 3% of submissions from teams with currently or previously mentored innovators have a status such as "Deleted," "Merged," or "Filing Candidate."

per year per mentored innovator. While internal invention disclosure rejection rates vary by firm, and external U.S. PTO patent application rejection rates vary by art unit and by gender, a blended estimate based on statistics reported in Aneja et al. (2024) and Chien and Grennan (2024) suggest that the overall conversion rate from invention disclosure submitted internally to a patent attorney to a granted patent is 11%. To value these patents, we use a highly conservative approach based solely on actual costs: patent attorney fees ($25,000) plus mandatory renewal fees ($13,460), totaling $38,460 per patent. This approach likely understates patent value but provides a concrete lower bound. Thus, the direct productivity gains are approximately $4.017 million (633 mentees $\times$ 1.5 submissions $\times$ 0.11 conversion $\times$ $38,460 per patent). Given that team size for accepted patents is 3.08 named inventors, an even more conservative lower bound estimate of the direct benefit is $1.354 million. Conversely, if we were to measure value using stock market indicators, as proposed by Kogan et al. (2017), our estimated impact would dwarf this estimate and be more than ten times larger since any movement in this superstar technology firm's stock price implies a much larger value for its patents.

We observe limited but measurable spillover effects through two channels. First, the 520 non-mentored innovators who directly collaborated with mentored participants showed a 0.5 increase in annual submissions, generating an estimated $1.1 million in additional patent value using the same conservative valuation approach. Second, we document increased team diversity post-program. Based on prior literature linking team diversity to patent value, we conservatively estimate a 5% increase in value for patents from diverse teams, adding approximately $0.256 million.[20]

The program likely generates additional value through improved retention and cultural

[20]To value the benefits of this type of diversity, we are implicitly assuming that engineering talent is not optimally allocated (Hsieh et al., 2019) and that more diverse innovator teams move the allocation of talent closer to optimal. While it is possible that diversity can increase coordination costs and reduce innovative efficiency (Lazear, 1999; Alesina and Ferrara, 2005) rather than improve problem-solving (Hong and Page, 2001), the empirical evidence is consistent with collaboration among diverse teams being important for innovative efficiency and the production of high-valued patents (Jones, 2009; Wuchty et al., 2007; Fleming et al., 2007; Azoulay et al., 2007; Lerner and Wulf, 2007; Hoogendoorn et al., 2013).

benefits, though these effects are harder to quantify precisely. We observe a 4% reduction in engineer turnover following program implementation. While multiple factors may contribute to this improvement, even a fraction of this effect could generate meaningful savings given typical replacement costs in the technology sector. Similarly, survey evidence suggests improved workplace satisfaction and knowledge sharing, which may enhance long-term organizational capital, though we acknowledge the challenges in precisely quantifying these cultural benefits (Cai et al., 2024).

For instance, one could estimate the value implications of the mentorship program's link to more efficient recruiting from improved employee perceptions of the workplace and higher retention rates among engineers. Attracting and hiring new engineers often involves substantial expenditures on job postings, recruiters, and candidate interviews. New engineering hires also require specialized knowledge and project familiarity to reach full productivity, requiring extensive training to get new hires up to speed and beyond their (typically) low initial productivity levels. This period of reduced productivity and the resources allocated to training represent meaningful expenditures for firms. Research indicates that a new engineer can take up to six months to achieve full productivity. This gap between an engineer's departure and a new recruit reaching full productivity along with the recruiting costs are estimated to be as high as 1.5 times an engineer's annual salary (Berk and Kase, 2010).

Thus, if one were to follow an approach similar to the one used by Lazear et al. (2015) to estimate the portion of value of good bosses associated with reduced churn, one could come up with a value. In this case, we could base our cost savings on the reduction in departures (conservatively, we could assume only 1% of the observed 4% reduction in turnover of the firm's 29,212 inventors is truly from mentoring), and we could conservatively estimate a cost of 1 times the average annual salary of engineers, even though evidence suggests it is 1.5 times. Finally, we get our salary estimate from Revelio for the year before the mentorship program, weighted based on the representation of engineers in different geographic regions and seniority levels ($157,297). This is a superstar technology firm with the capacity and

likelihood of paying much higher salaries, but we will go with this more conservative industry average. Overall, this suggests indirect cost savings from retention of $45.9 million (0.01 $\times$ 29,212 inventors $\times$ replacement cost (1 $\times$ $157,297 annual salary)) based on conservative assumptions.

Further, we know that employees are willing to take a salary cut for better non-pecuniary benefits such as exceptional corporate culture and workplace environments. Based on estimates from Makridis (2018) about the willingness to forgo a salary for better culture ($1,159 per year) and an average turnover rate in the IT industry of 19% according to the U.S. Bureau of Labor and Statistics, we estimate an additional cost savings of $19.2 million from lower initial salaries for new recruits (29,212 inventors $\times$ 0.19 turnover rate $\times$ $1,159 lower salary per year $\times$ 3-year mentorship program).

Finally, to align with the recent literature emphasizing the importance of intangible assets, particularly organizational capital and corporate culture, in driving long-term firm value, we explore how the patent mentorship program contributes to the firm's long-term value creation through this lens. For instance, Graham et al. (2022a) find that improving corporate culture as proxied through Glassdoor culture ratings and other measures is associated with an increase in Tobin's Q. Our focal firm has seen improvements in its Tobin's Q ratio since implementing the patent mentorship program. Given the focal firm's mega market capitalization, even attributing a few percentage points (e.g., 3 p.p.) of that increase in Tobin's Q to culture improvements would suggest benefits in excess of $2 billion. Yet this seems small when considering that engineers account for nearly 30% of the focal firm's workforce.

Alternatively, Eisfeldt and Papanikolaou (2013) suggest that organizational capital is embodied via key talent, and they show that firms with more organizational capital have higher and more variable risk premia. In the context of the patent mentorship program, we can view the program as an investment in organizational capital that enhances the firm's innovative capacity and knowledge base. Eisfeldt and Papanikolaou (2013) estimate a scaling parameter

of 0.3 representing the fraction of SG&A expenditures contributing to organizational capital formation. Based on the salaries of the participating mentees and using their scaling value, this suggests that the firm added $89.6 million (633 mentees × $157,297 average salary × 0.3 scaling parameter × 3-year mentorship program) to its organizational capital through its mentorship program.

The firm anticipated that the main cost of the mentorship program was going to be the opportunity cost of mentors' time. The mentors were among the most prolific innovators in the company, and one might have been concerned that the time required of them to mentor others may have reduced their overall level of innovativeness. We do not find evidence in support of this concern. In fact, we find that mentors increased their innovativeness during the program, and then their innovative output returned to their pre-mentorship baseline levels after the program ended. Specifically, we find that before they began their duties as mentors in the program, mentors submitted 2.6 disclosures per quarter, on average.[21] During their involvement in the program, mentors submitted 3.7 disclosures per quarter, on average, which is equivalent to their normal level of productivity plus the additional disclosure that stemmed from the program itself. Then, in the quarters after the program ended, mentors reverted back to their approximate pre-program levels of disclosure production.

In conclusion, the total benefits of the mentorship program include direct benefits from greater innovation productivity among those mentored ($4.017 million) and indirect benefits from greater innovation productivity among those working with the mentored innovators ($1.1 million), more valuable patents attributable to increased innovator team diversity ($0.256 million), and plausibly large labor market gains as well. These calculations, while systematic and grounded in observable data, require several important qualifications. Our patent valuation methodology relies on cost-based measures rather than market prices, which likely understates the true economic value that patents generate for the firm. When measuring collaboration effects, our approach captures only direct working relationships between

[21] In comparison, non-mentor innovators who were also not part of the program submitted fewer than 1 disclosure *per year*, on average.

mentored and non-mentored employees, potentially missing broader ripple effects through extended professional networks. The relationship between the mentorship program and employee retention presents particular analytical challenges, as other organizational changes could have occurred during our study period that contribute to changes in retention patterns. Although our interviews with program administrators indicated no such changes. Finally, while our survey data and Glassdoor analysis reveal positive shifts in workplace culture following the mentorship program implementation, converting these harder-to-quantify improvements in the organizational environment into quantitative economic measures remains methodologically complex.

Nevertheless, even using these conservative assumptions and focusing primarily on direct benefits, the program's returns appear to exceed its modest implementation costs. The program required minimal infrastructure—just two part-time administrators and voluntary participation from mentors whose productivity did not decrease during their mentorship period. While more speculative benefits could increase the ROI substantially, even our most conservative estimates imply that the mentorship program generates meaningful positive returns.

7 Conclusion

This study examines the relationship between targeted mentoring and innovation outcomes within a superstar technology firm. Using proprietary data on the innovation pipeline from initial idea disclosure through patent application and exploiting the staggered implementation of a mentoring program for identification purposes, we document several findings that contribute to our understanding of human capital development and innovation within knowledge-intensive organizations.

First, we find that mentored innovators submit approximately 1.5 additional invention disclosures annually compared to control groups, with gains persisting well beyond the program period end. Second, we document positive spillovers to non-participants collaborating

with mentored innovators, contributing to the literature on peer effects and knowledge diffusion within firms. Third, we survey the innovators and find support for three potential mechanisms leading to short- and long-term innovative gains: knowledge transfer about patent processes, expanded professional networks, and increased confidence in identifying patentable ideas. The results align with theoretical work on barriers to innovation and provide insight into how mentoring might reduce these frictions.

Fourth, we extend our analysis to document broader organizational changes following the mentorship program implementation. Examining Glassdoor reviews and employee retention data reveals meaningful improvements in employee satisfaction ratings and engineering talent retention relative to multiple control groups. These patterns suggest the mentorship program's benefits ripple throughout the organization's culture.

We synthesize our findings on the many direct and indirect benefits by presenting a detailed estimate of the program's ROI. The analysis demonstrates that direct innovation benefits alone exceed implementation costs, even when using conservative patent valuations based on prosecution and maintenance fees rather than market value. Including the program's broader organizational benefits further strengthens the economic case for mentorship. While precisely quantifying cultural improvements and retention benefits presents methodological challenges, our analysis of these indirect gains from implementing a mentorship program suggests substantial additional value creation. Overall, the ROI estimation contributes to our understanding of the cost-effectiveness of mentorship. By documenting a meaningful ROI, it suggests that small-scale mentoring programs can generate both broader financial returns and create social value by helping improve persistent inequalities within the innovation process.

This study faces important limitations. Most critically, our analysis focuses on a single superstar technology firm with substantial resources and a strong existing innovation infrastructure. The magnitude and nature of mentoring benefits may differ significantly in smaller firms, different industries, or organizations with more constrained resources. Ad-

ditionally, while the staggered implementation of the program helps with identification, we cannot completely rule out concurrent changes that might influence our results. Therefore, we acknowledge that the long-term value of a similar program elsewhere likely depends on firm-specific factors that we cannot fully capture in our single-firm setting.

These limitations, however, suggest several promising directions for future research. Studies examining similar programs across multiple firms would help establish the generalizability of our findings and provide more precise estimates of both direct and indirect benefits. Research exploring how program design features influence outcomes would help inform optimal implementation, especially given the potential for selection (Adams and Ragunathan, 2018; Sandvik et al., Forthcoming). Similarly, examining how the corporate governance of such firms influences their ability to implement mentorship programs would be of interest. Finally, since our research shows that mentorship programs can unlock latent innovation potential, especially among underrepresented groups, this raises interesting questions about whether the market efficiently prices this "innovation option value" and whether firms with more inclusive innovation practices command higher valuations.

In conclusion, our findings offer encouraging evidence that well-designed mentorship programs can meaningfully enhance innovation while simultaneously advancing workplace inclusivity. For leaders seeking to develop their innovation capabilities and for policymakers aiming to broaden participation in the innovation economy, structured mentoring appears to be a promising and relatively low-cost tool worth further exploration and experimentation.

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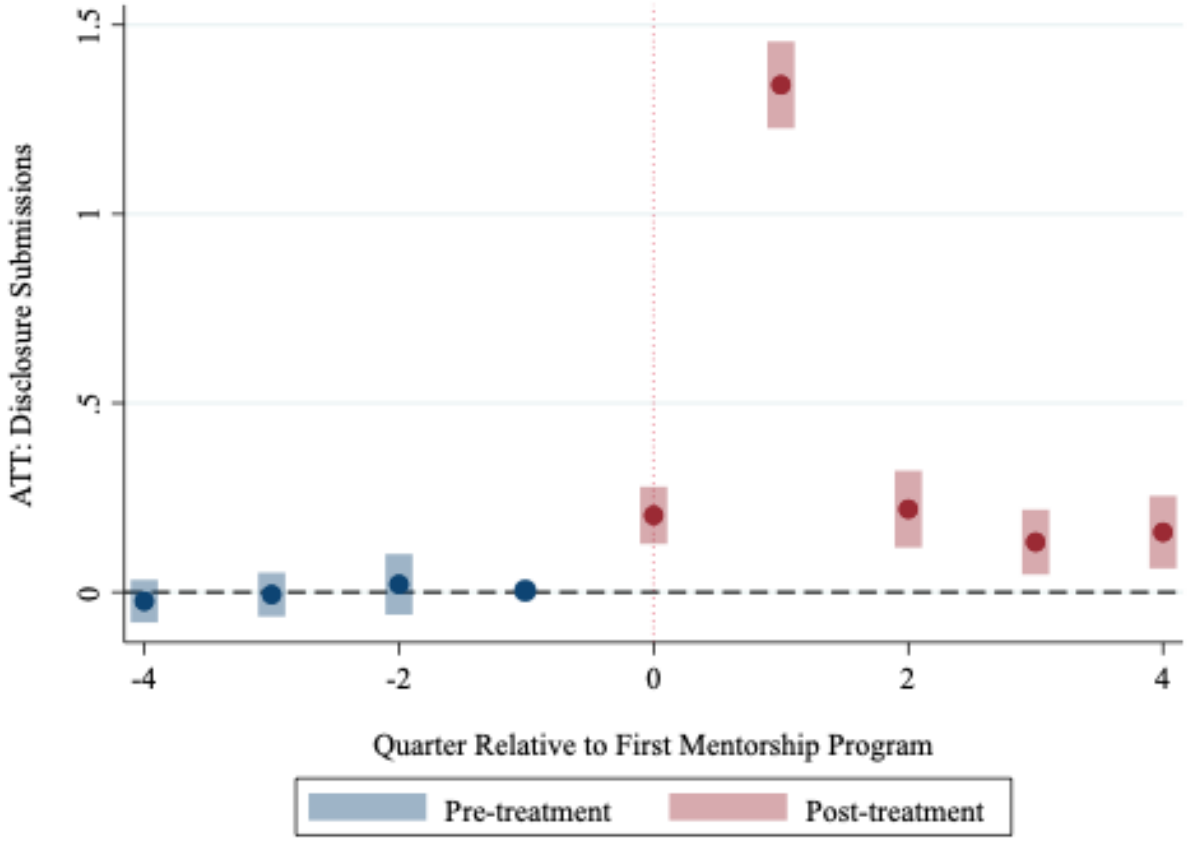
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Figure 1: Test of Common Pre-Trends in Disclosure Submission Activity



Notes. This figure plots the estimates from our parallel trends test. The point estimates capture the quarterly differences in disclosure submission outcomes between mentored and not-yet-mentored innovators. We plot 95% confidence intervals around each estimate, which are generated using the estimator proposed by Callaway and Sant'Anna (2021), with the quarter before the innovator begins the mentorship program as the baseline reference period. We aggregate together Status 2 and Status 1 submissions, and we also include submissions with either a non-disclosed status or with the status of "Submitted" or "Filing Candidate." Status 2 submissions are those with any of the following statuses: Approved - Patent Application; Prior Art Search Requested; RFC. Status 1 submissions are those with any of the following statuses: Declined; Auto Declined; Defensive Publication Candidate; Approved - Defensive Publication; Approved - Trade Secret; Deleted; Merged.

Table 1: Demographics Characteristics of Innovators

	Cohort 1	Cohort 2	Cohort 3	Cohort 4	Cohort 5	Cohort 6	Cohort 7	No Mentor	(1)–(3)	(1)–(7)	t-stat
Age	33.14	35.09	32.83	32.87	30.84	29.45	33.09	41.34	0.161	0.007	0.001
	(8.28)	(7.58)	(6.86)	(8.50)	(8.20)	(6.41)	(7.39)	(8.92)			
Tenure (Firm)	2.50	3.59	2.37	2.96	1.82	0.38	1.58	7.81	0.142	0.001	0.001
	(4.21)	(5.65)	(4.10)	(4.29)	(3.62)	(1.33)	(3.34)	(6.30			
Tenure (Ind.)	5.49	6.97	5.81	5.43	3.73	2.36	5.16		0.235	0.002	
	(5.85)	(6.59)	(5.56)	(6.09)	(5.54)	(3.91)	(5.66)				
Female	0.90	0.78	0.79	0.52	0.55	0.58	0.63	0.12	0.081	0.001	0.001
Asian	0.71	0.64	0.84	0.85	0.62	0.81	0.78	0.64	0.004	0.001	0.001
Black	0.02	0.02	0.00	0.00	0.01	0.00	0.04	0.00	0.181	0.194	0.001
Latinx	0.15	0.08	0.05	0.02	0.12	0.00	0.03	0.02	0.029	0.002	0.001
Multi	0.00	0.00	0.01	0.02	0.01	0.03	0.00	0.00	0.629	0.517	0.133
White	0.13	0.25	0.11	0.12	0.25	0.16	0.17	0.34	0.022	0.038	0.001
URM	0.17	0.10	0.05	0.04	0.14	0.03	0.07	0.03	0.014	0.011	0.001
Executive	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.01	-	-	0.001
Top Engineer	0.04	0.00	0.03	0.11	0.06	0.00	0.02	0.12	0.352	0.048	0.001
Engineer	0.81	0.72	0.73	0.74	0.58	0.47	0.65	0.88	0.429	0.002	0.001
Data Scientist	0.03	0.02	0.02	0.00	0.01	0.00	0.02	0.01	0.831	0.894	0.492
Management	0.00	0.00	0.01	0.00	0.00	0.00	0.00	0.01	0.620	0.814	0.254
Any Status	0.09	0.05	0.11	0.08	0.08	0.07	0.09	0.10	0.336	0.818	0.495
	(0.29)	(0.11)	(0.32)	(0.22)	(0.21)	(0.10)	(0.29)	(0.50)			
Status 2	0.03	0.02	0.01	0.01	0.00	0.01	0.01	0.03	0.360	0.469	0.047
	(0.16)	(0.06)	(0.06)	(0.04)	(0.01)	(0.04)	(0.10)	(0.24)			
Status 1	0.06	0.03	0.09	0.07	0.08	0.05	0.06	0.06	0.099	0.413	0.463
	(0.17)	(0.08)	(0.30)	(0.19)	(0.21)	(0.09)	(0.15)	(0.27)			
Num. innovators	101	73	171	55	88	40	105	28,579			

Notes. This table presents summary statistics of the demographic characteristics of the innovators in the firm. We report characteristics separately for individuals based on the cohort in which they first participated in the mentoring program. The column labeled (1)–(3) reports p-values from F-tests that the average value of the characteristics are equal across cohorts 1, 2, and 3. The column labeled (1)–(7) reports p-values from F-tests that the average value of the characteristics are equal across all seven cohorts. The column labeled t-stat reports p-values from T-tests that the average value of the characteristics are equal between mentored innovators and non-mentored innovators.

Table 2: Mentorship Program Participation and Innovator Productivity

	All Submissions		Status 2 Submissions		Status 1 Submissions	
	(1)	(2)	(3)	(4)	(5)	(6)
Mentored	0.272***	0.416***	0.057***	0.072***	0.179***	0.299***
	(3.827)	(4.121)	(4.771)	(3.945)	(3.227)	(3.975)
Inv. FE	✓		✓		✓	
Time FE	✓		✓		✓	
Cohort × Inv. FE		✓		✓		✓
Cohort × Time FE		✓		✓		✓
Adj. R-Square	0.381	0.446	0.306	0.352	0.306	0.375
Observations	673,039	1,676,863	673,039	1,676,863	673,039	1,676,863

Notes. This table presents an analysis of the relationship between participation in the mentorship program and innovator productivity, as measured by invention disclosure form (IDF) submissions. The data comprises a panel of innovator-quarter submissions. In Columns (1)–(2), the dependent variable is the total number of disclosure submissions of any status that an innovator submitted in a given quarter. The dependent variables in Columns (3)–(4) and (5)–(6) focus on the number of submissions at Status 2 and Status 1, respectively. The variable of interest, *Mentored*, is an indicator equal to one in the quarter in which an innovator is part of the mentorship program and all subsequent quarters, and zero otherwise. We employ two estimation strategies: a two-way fixed effects estimator with innovator and time (calendar quarter) fixed effects in the odd-numbered columns, and we employ a stacked difference-in-differences estimator in even-numbered columns. The control group includes both not-yet-mentored innovators and never-mentored innovators. For the two-way fixed effects model, standard errors are clustered by innovator and quarter. In the stacked difference-in-differences specification, clustering is at the innovator-by-cohort and quarter-by-cohort levels. t-statistics are reported in parentheses. Statistical significance of 10%, 5%, and 1% is denoted by *, **, and ***, respectively.

Table 3: Mentorship Program Participation and Long-term Innovator Productivity

	All Submissions		Status 2 Submissions		Status 1 Submissions	
	(1)	(2)	(3)	(4)	(5)	(6)
Mentored	0.110***	0.143***	0.028***	0.022**	0.072**	0.113***
	(3.020)	(3.006)	(4.003)	(1.970)	(2.214)	(2.780)
Inv. FE	✓		✓		✓	
Time FE	✓		✓		✓	
Cohort × Inv. FE		✓		✓		✓
Cohort × Time FE		✓		✓		✓
Adj. R-Square	0.383	0.447	0.307	0.352	0.309	0.376
Observations	671,140	1,674,964	671,140	1,674,964	671,140	1,674,964

Notes. This table presents an analysis of the relationship between participation in the mentorship program and long-term innovator productivity, as measured by invention disclosure form (IDF) submissions. The data comprises a panel of innovator-quarter submissions, but removes the observations of mentored workers in the quarter of first mentorship and the first and second quarters after first mentorship. In Columns (1)–(2), the dependent variable is the total number of disclosure submissions of any status that an innovator submitted in a given quarter. Columns (3)–(4) and (5)–(6) focus on the number of submissions at Status 2 and Status 1, respectively. The variable of interest, *Mentored*, is an indicator equal to one in the quarter an innovator joins the mentorship program and in all subsequent quarters, and zero otherwise. We employ two estimation strategies: a two-way fixed effects model with innovator and time (calendar quarter) fixed effects in the odd-numbered columns, and a stacked difference-in-differences estimator in the even-numbered columns. The control group includes both not-yet-mentored innovators and never-mentored innovators. For the two-way fixed effects model, standard errors are clustered by innovator and quarter. In the stacked difference-in-differences specification, clustering is at the innovator-by-cohort and quarter-by-cohort levels. t-statistics are reported in parentheses. Statistical significance of 10%, 5%, and 1% is denoted by *, **, and ***, respectively.

Table 4: Heterogeneity in the Association Between Mentorship and Productivity

Panel A: Gender

	All Submissions		Status 2 Submissions		Status 1 Submissions	
	(1)	(2)	(3)	(4)	(5)	(6)
Mentored	0.314***	0.367***	0.058***	0.064***	0.234***	0.263***
	(3.546)	(3.549)	(3.550)	(2.588)	(3.403)	(3.871)
Mentored × Female	0.023	0.116*	0.008	0.028	0.012	0.087
	(0.355)	(1.766)	(0.452)	(1.480)	(0.235)	(1.580)
Inv. FE	✓		✓		✓	
Time FE	✓		✓		✓	
Cohort × Inv. FE		✓		✓		✓
Cohort × Time FE		✓		✓		✓
Adj. R-Square	0.419	0.467	0.358	0.398	0.340	0.380
Observations	175,330	448,186	175,330	448,186	175,330	448,186

Panel B: URM

	All Submissions		Status 2 Submissions		Status 1 Submissions	
	(1)	(2)	(3)	(4)	(5)	(6)
Mentored	0.310***	0.429***	0.058***	0.075***	0.221***	0.307***
	(4.421)	(4.222)	(5.239)	(3.916)	(4.305)	(4.302)
Mentored × URM	0.169	0.154	0.035	0.031	0.161	0.157
	(1.175)	(1.121)	(1.151)	(0.866)	(1.482)	(1.487)
Inv. FE	✓		✓		✓	
Time FE	✓		✓		✓	
Cohort × Inv. FE		✓		✓		✓
Cohort × Time FE		✓		✓		✓
Adj. R-Square	0.401	0.442	0.340	0.370	0.325	0.359
Observations	220,681	562,606	220,681	562,606	220,681	562,606

Panel C: Female or URM

	All Submissions		Status 2 Submissions		Status 1 Submissions	
	(1)	(2)	(3)	(4)	(5)	(6)
Mentored	0.290***	0.390***	0.048***	0.055***	0.219***	0.294***
	(3.527)	(3.822)	(3.727)	(2.728)	(3.249)	(3.914)
Mentored × Female/URM	0.052	0.084*	0.021	0.036**	0.025	0.047
	(1.044)	(1.789)	(1.554)	(2.431)	(0.588)	(1.072)
Inv. FE	✓		✓		✓	
Time FE	✓		✓		✓	
Cohort × Inv. FE		✓		✓		✓
Cohort × Time FE		✓		✓		✓
Adj. R-Square	0.412	0.465	0.355	0.397	0.330	0.377
Observations	228,702	583,243	228,702	583,243	228,702	583,243

Notes. This table presents our analyses of potential heterogeneity in the relation between mentorship program participation and innovator productivity, focusing on gender and racial/ethnic diversity. The data comprises a panel of innovator-quarter IDF (Invention Disclosure Form) submissions. Due to limitations in demographic data availability, the sample size is reduced compared to previous analyses. The key variables of interest are binary indicators: *Female* equals one for female innovators, *URM* equals one for innovators from underrepresented racial/ethnic minority groups, and *Female/URM* equals one if the innovator belongs to either or both categories. These indicators are interacted with the mentorship program participation variable to explore potential differences in the mentorship-productivity association across demographic groups. The empirical specification structure matches that used in Table 2. t-statistics are reported in parentheses. Statistical significance of 10%, 5%, and 1% is denoted by *, **, and ***, respectively.

Table 5: Heterogeneity Based on Mentor Quality

	All Submissions		Status 2 Submissions		Status 1 Submissions	
	(1)	(2)	(3)	(4)	(5)	(6)
Mentored	0.133***	0.275***	0.021***	0.036***	0.087**	0.200***
	(2.572)	(3.299)	(3.106)	(3.069)	(2.094)	(2.998)
Mentored × High Mentor FE	0.244***	0.237***	0.063***	0.059**	0.160***	0.167***
	(4.037)	(4.225)	(3.208)	(2.558)	(3.783)	(3.712)
Inv. FE	✓		✓		✓	
Time FE	✓		✓		✓	
Cohort × Inv. FE		✓		✓		✓
Cohort × Time FE		✓		✓		✓
Adj. R-Square	0.381	0.446	0.306	0.352	0.306	0.375
Observations	673,039	1,676,863	673,039	1,676,863	673,039	1,676,863

Notes. This table presents our analyses of potential heterogeneity in the relation between mentorship program participation and innovator productivity, focusing on mentor quality. The data comprises a panel of innovator-quarter IDF (Invention Disclosure Form) submissions. The key variable of interest is the binary indicator *High Mentor FE*, which equals one for innovators whose mentor has a mentor fixed effect above the median, relative to all mentors in the program, and zero otherwise. This indicator is interacted with the mentorship program participation variable to explore potential differences in the mentorship-productivity association across mentor types. The empirical specification structure matches that used in Table 2. *t*-statistics are reported in parentheses. Statistical significance of 10%, 5%, and 1% is denoted by *, **, and ***, respectively.

Table 6: Survey Evidence on the Mentorship Program

	Full sample		Specific Subgroups					
			RG		URG	Male	Female	URM
Panel A. Mentorship	Obs (1)	Mean (2)	Obs (3)	Mean (4)	Mean (5)	Mean (6)	Mean (7)	Mean (8)
Have you ever participated in our patent mentorship program?	230	43%	101	37%	48%	36%	52%**	38%
Panel B. Perspective of those without a mentor								
What are the reasons you have not sought patent mentorship?								
I wanted to participate but was unable to	125	3%	63	5%	2%	4%	2%	0%
I did not believe patent mentorship would benefit me	125	6%	63	3%	9%	3%	11%**	0%
I did not have time for patent mentorship	125	6%	63	6%	9%	5%	7%	10%
I did not know about it	125	86%	63	87%	84%	90%	82%	90%
I want to avoid activities that highlight my weaknesses to leaders	125	1%	63	2%	0%	1%	0%	0%
My co-workers already help me with IP development, so I don't need a mentor	125	1%	63	2%	0%	0%	2%	0%
I felt that the program would benefit my employer more than it would benefit me	125	1%	63	2%	0%	1%	0%	0%
Panel C. Perspective of those with a mentor								
Since your formal mentorship relationship ended, on average, how many times per month have you met with your mentor outside of your regularly scheduled work meetings?	91	1.7	34	1.8	1.7	1.7	1.7	1.8
To what extent do you disagree or agree with the statements about mentorship? (-2 = Strongly disagree, 2 = Strongly agree)								
I am planning to work on more invention disclosures at work	89	1.2	33	1.1	1.2	1.1	1.3	0.8
I learned a lot about patenting and becoming a better inventor	85	1.2	30	1.1	1.4	0.9	1.5**	0.8
Being mentored increased my satisfaction at work	85	1.2	31	1.1	1.2	1.0	1.4*	0.5**
Mentorship has helped me develop professionally, gain leadership skills, and think more broadly	89	1.2	33	1.2	1.3	1.1	1.3	1.2
I have benefited from my mentoring relationship	86	1.2	31	1.3	1.2	1.1	1.2	0.5*
My mentor introduced me to new inventors that I anticipate connecting with in the future	86	1.1	30	1.0	1.1	1.0	1.2	1.2
I feel confident that I can incorporate my mentor's tips into my work process and develop new inventions	86	0.9	32	1.0	0.9	0.9	0.9	0.5
I feel confident that I have the social connections necessary to get my invention ideas accepted by IP professionals	83	0.9	31	1.0	0.8	0.9	0.9	1.2

Notes. This table provides descriptive statistics of the engineers' perceptions of mentorship, separately by demographic groups. Columns (1)–(2) summarize the frequency and mean for the full sample, while Columns (3)–(8) report statistics for specific subgroups based on gender and ethnicity. Asian and White male engineers define the represented group (RG), while females and underrepresented minorities (URMs) define the underrepresented group (URG). The stars in Columns (5), (7), and (8) denote the significance level from a t-test, indicating the likelihood that the observed differences in subgroup means are not due to chance. ***, ** and * indicate p-values under the assumption of a single test of 1%, 5%, and 10%, respectively.

Table 7: Productivity of Non-Participants After Exposure to Mentored Colleagues

	All Submissions		Status 2 Submissions		Status 1 Submissions	
	(1)	(2)	(3)	(4)	(5)	(6)
Post First Exposure	0.633*	0.685***	0.290	0.259**	0.040	0.170
	(1.651)	(2.612)	(1.617)	(2.173)	(0.373)	(1.482)
Inv. FE	✓		✓		✓	
Time FE	✓		✓		✓	
Cohort × Inv. FE		✓		✓		✓
Cohort × Time FE		✓		✓		✓
Adj. R-Square	0.302	0.317	0.218	0.233	0.247	0.263
Observations	462,427	3,559,385	462,427	3,559,385	462,427	3,559,385

Notes. This table reports our analyses of potential productivity changes among individuals who did not directly participate in the mentorship program but were potentially exposed to it via their mentored colleagues. For these tests, we restrict the sample to innovators who were never formally associated with the mentorship program, either as mentors or mentees. For this group of non-participating innovators, we analyze their disclosure submission records to determine when they were first part of an invention disclosure team with an individual who had previously been mentored as part of the program. We introduce a new variable, *Post First Exposure*, which equals one in the quarter a non-participating innovator first collaborates with a mentored innovator and in all subsequent quarters, and zero otherwise. Our sample includes 520 non-participating innovators who we observe collaborating with mentored innovators. We then regress disclosure submission outcomes on this indicator using both the two-way fixed effects estimator (odd-numbered columns) and the stacked difference-in-differences estimator (even-numbered columns). Standard errors are clustered by innovator and quarter (innovator-by-cohort and quarter-by-cohort) when using the two-way fixed effects (stacked difference-in-differences) estimator, and t-statistics are reported in parentheses. Statistical significance of 10%, 5%, and 1% is denoted by *, **, and ***, respectively.

Table 8: Changes in Innovator Team Composition After Mentorship

Panel A: Full Sample of Data

	Fraction Female		HHI: Race Groups		HHI: Gender-Race Groups	
	(1)	(2)	(3)	(4)	(5)	(6)
Mentored	0.140***	0.179***	-0.115***	-0.108***	-0.164***	-0.172***
	(3.866)	(4.804)	(-3.729)	(-3.449)	(-5.110)	(-3.965)
Inv. FE	✓		✓		✓	
Time FE	✓		✓		✓	
Cohort × Inv. FE		✓		✓		✓
Cohort × Time FE		✓		✓		✓
Adj. R-Square	0.640	0.630	0.442	0.497	0.448	0.514
Observations	18,327	33,515	18,628	33,675	18,826	33,988

Panel B: Long-Term Productivity

	Fraction Female		HHI: Race Groups		HHI: Gender-Race Groups	
	(1)	(2)	(3)	(4)	(5)	(6)
Mentored	0.017	0.043	-0.112**	-0.141	-0.147***	-0.249***
	(0.400)	(0.720)	(-2.601)	(-1.584)	(-2.905)	(-3.098)
Inv. FE	✓		✓		✓	
Time FE	✓		✓		✓	
Cohort × Inv. FE		✓		✓		✓
Cohort × Time FE		✓		✓		✓
Adj. R-Square	0.601	0.605	0.443	0.498	0.443	0.512
Observations	17,799	33,056	18,106	33,215	18,298	33,522

Notes. This table presents an analysis of potential changes in innovator team composition associated with participation in the mentorship program. The data used is a panel of innovator-quarter team diversity metrics. Panel A utilizes the full sample, while Panel B excludes observations of mentored workers in the quarter of first mentorship and the subsequent two quarters. The dependent variable in Columns (1)–(2) is the average fraction female of the innovator teams the innovator is a part of in a given quarter. The dependent variables in Columns (3)–(4) and (5)–(6) are the average race and gender-race HHI values of the innovator teams the innovator is a part of in a given quarter. The key variable of interest, *Mentored*, is an indicator equal to one in the quarter an innovator joins the mentorship program and in all subsequent quarters, and zero otherwise. In both Panels A and B, we employ a two-way fixed effects estimator with innovator and time (calendar quarter) fixed effects in the odd-numbered columns, and we employ a stacked difference-in-differences estimator in even-numbered columns. Standard errors are clustered by innovator and quarter (innovator-by-cohort and quarter-by-cohort) when using the two-way fixed effects (stacked difference-in-differences) estimator, and t-statistics are reported in parentheses. Statistical significance of 10%, 5%, and 1% is denoted by *, **, and ***, respectively.

Table 9: Engineers' Workplace Perceptions After the Mentorship Program

	Focal Firm Only		Peer Firms Only		Both
	(1)	(2)	(3)	(4)	(5)
Post	0.077***	0.024	0.048***	0.046***	0.045***
	(4.087)	(0.991)	(8.211)	(6.872)	(6.848)
Engineer		-0.198***		-0.001	-0.001
		(-5.866)		(-0.099)	(-0.072)
Post × Engineer		0.128***		0.013	0.013
		(3.343)		(0.904)	(0.917)
Post × Engineer × Focal Firm					0.112***
					(2.773)
Country FE	✓	✓	✓	✓	✓
Firm FE			✓	✓	✓
Adj. R-Square	0.008	0.012	0.056	0.056	0.057
Observations	12,019	12,019	230,044	230,044	242,066

Notes. This table presents an analysis of potential changes in engineers' perceptions of their workplace coinciding with the implementation of the patent mentorship program. The data used is individual reviews posted from 2018 to 2024 on Glassdoor, which is a career intelligence website that attempts to provide transparency about jobs, salaries, and companies. We focus on the reviews provided by current employees of the focal firm and its industry peers. The analysis is restricted to reviews with non-missing job title data, which is necessary for identifying engineers. The dependent variable is the overall rating of the firm provided by the reviewer. We employ ordinary least squares regressions to analyze these ratings in relation to the binary indicator *Post*, which equals one from when the mentorship program began and onward. Some models include interactions between *Post* and the binary indicator *Engineer*, while others further incorporate interactions with the binary indicator *Focal Firm*. Columns (1)–(2) use only reviews about the focal firm. Columns (3)–(4) use only reviews about the firm's industry peers. Column (5) uses data from all firms. All models include country fixed effects, and the models in Columns (3)–(5) include firm fixed effects. The sum of observation counts between Columns (1) and (3), 242,063, is smaller than the value in Column (5) due to the omission of singleton observations. t-statistics from robust standard errors are reported in parentheses. Statistical significance of 10%, 5%, and 1% is denoted by *, **, and ***, respectively.

Table 10: Engineers' Departures After the Mentorship Program

Panel A: Outflows

	Focal Firm Only		Peer Firms Only		Both
	(1)	(2)	(3)	(4)	(5)
Post	-0.054***	-0.021***	0.060***	0.060***	0.060***
	(-9.679)	(-8.335)	(15.927)	(19.865)	(19.872)
Engineer		0.945***		0.832***	0.829***
		(33.575)		(67.220)	(67.070)
Post × Engineer		-0.222***		-0.001	-0.001
		(-6.398)		(-0.068)	(-0.068)
Post × Engineer × Focal Firm					-0.220***
					(-5.745)
Region and Gender FEs	✓	✓	✓	✓	✓
Ethnicity and Seniority FEs	✓	✓	✓	✓	✓
Firm FE			✓	✓	✓
Adj. R-Square	0.061	0.076	0.032	0.035	0.036
Observations	706,860	706,860	7,999,936	7,999,936	8,706,796

Panel B: Inverse Hyperbolic Sine of Outflows

	Focal Firm Only		Peer Firms Only		Both
	(1)	(2)	(3)	(4)	(5)
Post	-0.015***	-0.010***	0.007***	0.008***	0.008***
	(-17.264)	(-12.747)	(23.805)	(27.814)	(27.813)
Engineer		0.252***		0.169***	0.169***
		(78.506)		(203.505)	(203.378)
Post × Engineer		-0.040***		-0.006***	-0.006***
		(-9.740)		(-5.696)	(-5.696)
Post × Engineer × Focal Firm					-0.033***
					(-7.856)
Region and Gender FEs	✓	✓	✓	✓	✓
Ethnicity and Seniority FEs	✓	✓	✓	✓	✓
Firm FE			✓	✓	✓
Adj. R-Square	0.206	0.244	0.207	0.223	0.223
Observations	706,860	706,860	7,999,936	7,999,936	8,706,796

Notes. This table presents an analysis of potential changes in employee departure patterns coinciding with the implementation of the patent mentorship program. The data used is employee departure measures from 2018 to 2024 from Revelio, a labor market intelligence firm that tracks employee movements across firms. Panel A analyzes *Outflows*, defined as the number of employees leaving the company within a specific occupation-region-gender-ethnicity-seniority group in a given month. Panel B examines the inverse hyperbolic sine transformation of this scaled external outflows measure. We employ ordinary least squares regressions to analyze these departure measures in relation to the binary indicator *Post*, which equals one from when the mentorship program began and onward. Some models include interactions between *Post* and the binary indicator *Engineer*, while others further incorporate interactions with the binary indicator *Focal Firm*. Columns (1)–(2) use only departure measures from the focal firm. Columns (3)–(4) use only departure measures from the firm's industry peers. Column (5) uses data from all firms. All models include region, gender, ethnicity, and seniority fixed effects, and the models in Columns (3)–(5) include firm fixed effects. t-statistics from robust standard errors are reported in parentheses. Statistical significance of 10%, 5%, and 1% is denoted by *, **, and ***, respectively.

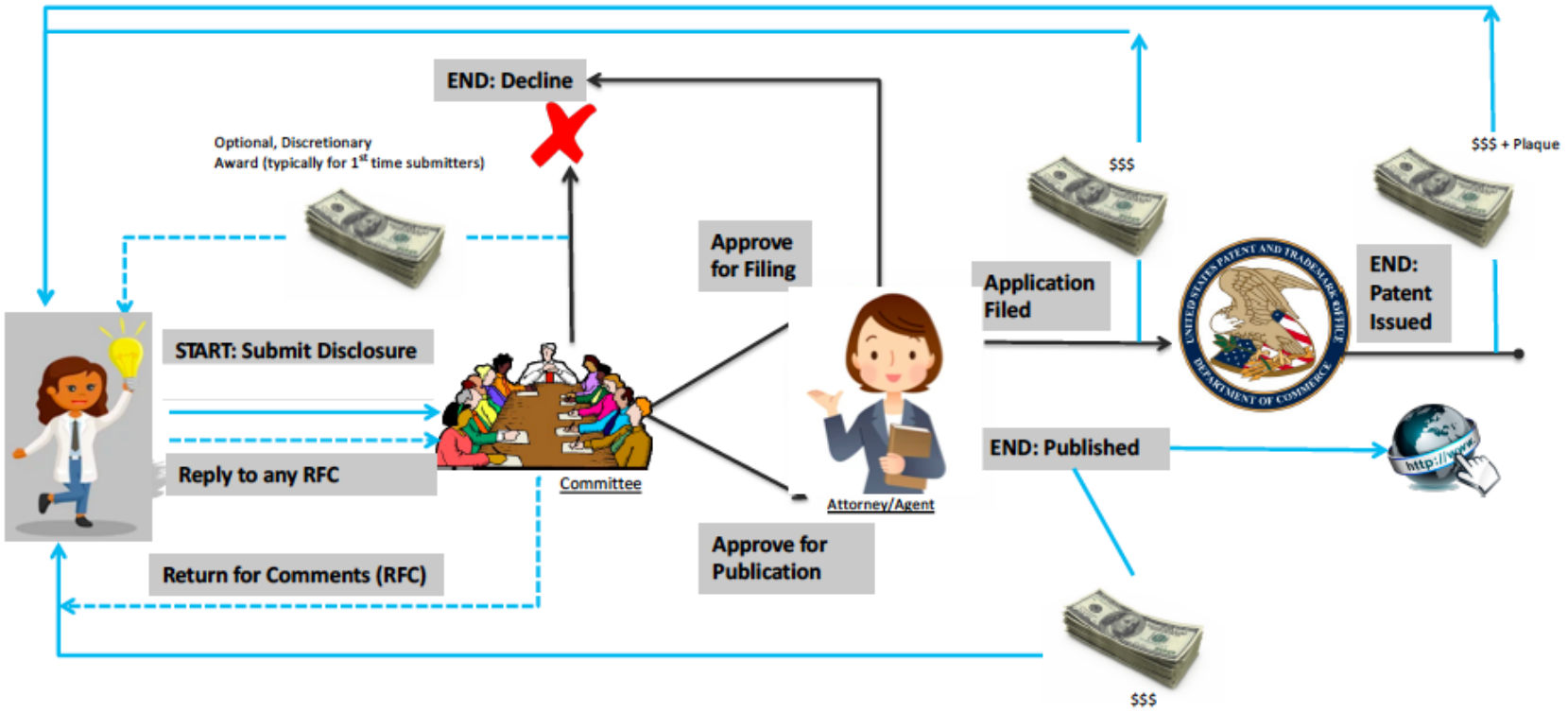
A Appendix Figures and Tables

Figure A.1: Representative Timeline of Events for a Mentorship Cohort



Notes. This figure highlights the typical timeline that mentorship cohorts followed throughout the program. Mentor-mentee teams generally committed to engage in the program curriculum for 3–4 hours a week for about two (four) months in the early (later) cohorts.

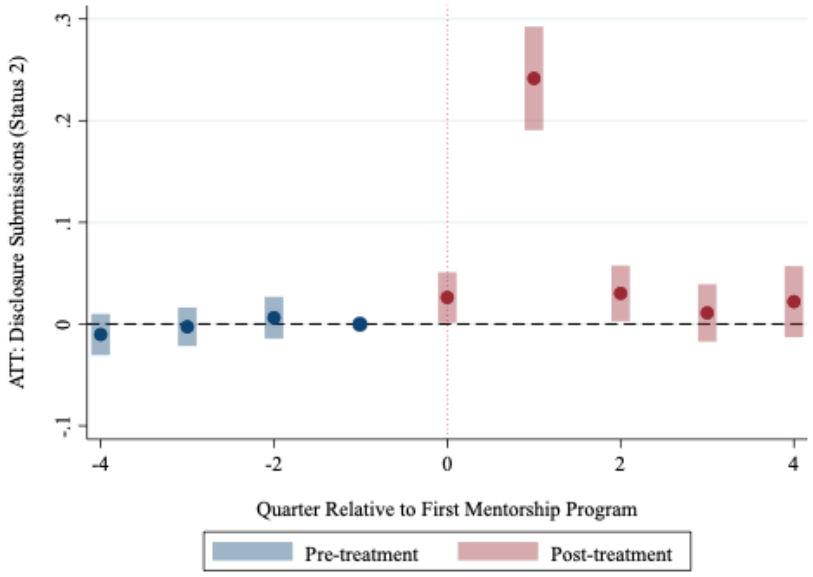
Figure A.2: Patent Process



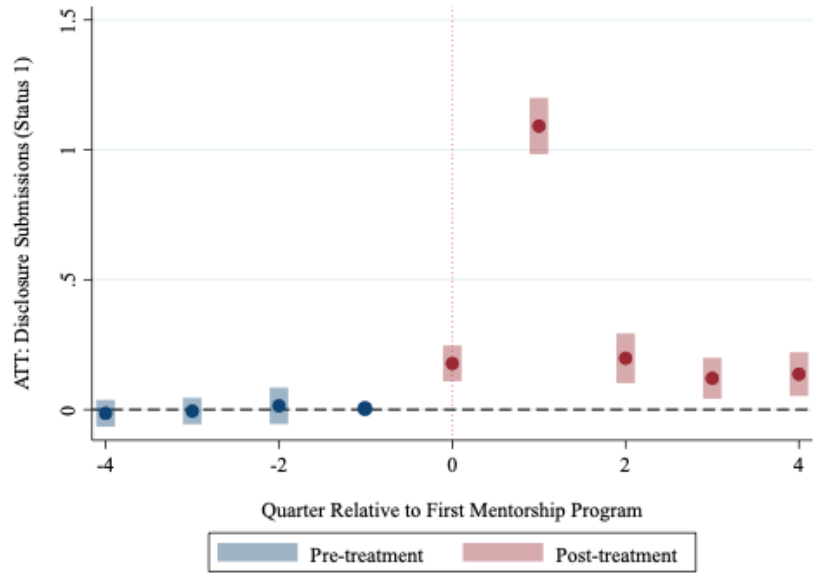
Notes. This figure highlights the patent process as taught to mentees by mentors as part of the mentorship program. The main outcomes that a disclosure submission can realize are (i) being declined, (ii) being approved for defensive publication, and (iii) being a filed or approved patent.

Figure A.3: Test of Common Pre-Trends by Submission Type

(a) Number of Submissions: Status 2



(b) Number of Submissions: Status 1



Notes. These figures plot the estimates from our parallel trends tests. The point estimates capture the quarterly differences in disclosure submission outcomes between mentored and not-yet-mentored innovators. We plot 95% confidence intervals around each estimate, which are generated using the estimator proposed by Callaway and Sant'Anna (2021), with the quarter before the innovator begins the mentorship program as the baseline reference period. Status 2 submissions are those with any of the following statuses: Approved - Patent Application; Prior Art Search Requested; RFC. Status 1 submissions are those with any of the following statuses: Declined; Auto Declined; Defensive Publication Candidate; Approved - Defensive Publication; Approved - Trade Secret; Deleted; Merged.

Table A.1: Innovator Productivity and Heterogeneity by Country and Cohort

Panel A: Work Country

	All Submissions		Status 2 Submissions		Status 1 Submissions	
	(1)	(2)	(3)	(4)	(5)	(6)
Mentored	0.342***	0.434***	0.062***	0.069***	0.248***	0.321***
	(4.609)	(4.590)	(5.248)	(4.534)	(4.057)	(4.312)
Mentored × USA	-0.058	0.020	0.005	0.021	-0.056	0.007
	(-1.110)	(0.279)	(0.312)	(0.974)	(-1.249)	(0.108)
Inv. FE	✓		✓		✓	
Time FE	✓		✓		✓	
Cohort × Inv. FE		✓		✓		✓
Cohort × Time FE		✓		✓		✓
Adj. R-Square	0.388	0.456	0.335	0.391	0.304	0.368
Observations	311,261	790,494	311,261	790,494	311,261	790,494

Panel B: Cohort Timing

	All Submissions		Status 2 Submissions		Status 1 Submissions	
	(1)	(2)	(3)	(4)	(5)	(6)
Mentored	0.327**	0.352**	0.081***	0.085***	0.152**	0.161**
	(2.511)	(2.399)	(3.166)	(3.025)	(2.451)	(2.223)
Mentored × Early	-0.085	0.113	-0.038	-0.024	0.042	0.243*
	(-0.589)	(0.562)	(-1.396)	(-0.669)	(0.521)	(1.770)
Inv. FE	✓		✓		✓	
Time FE	✓		✓		✓	
Cohort × Inv. FE		✓		✓		✓
Cohort × Time FE		✓		✓		✓
Adj. R-Square	0.381	0.446	0.306	0.352	0.306	0.375
Observations	673,039	1,676,863	673,039	1,676,863	673,039	1,676,863

Notes. The data used is a panel of innovator-quarter IDF submissions. The tests in Panel A require data on the work country of the innovator. We do not have data on this characteristic for all individuals, leading to the reductions in observation counts. The variable *USA* is a binary indicator that equals one if the innovator works in the United States. The variable *Early* is a binary indicator that equals one if the innovator was part of the first, second, or third mentorship cohorts. The empirical specification structure matches that used in Table 2. *t*-statistics are reported in parentheses. Statistical significance of 10%, 5%, and 1% is denoted by *, **, and ***, respectively.

Table A.2: Innovator Productivity and Mentor Heterogeneity

Panel A: Mentor Gender

	All Submissions		Status 2 Submissions		Status 1 Submissions	
	(1)	(2)	(3)	(4)	(5)	(6)
Mentored	0.296***	0.427***	0.068***	0.087***	0.192***	0.295***
	(3.984)	(4.114)	(4.726)	(3.839)	(3.428)	(3.992)
Mentored × Female Mentor	-0.069	-0.051	-0.017	-0.031	-0.091**	-0.067
	(-1.323)	(-0.609)	(-1.201)	(-1.382)	(-2.549)	(-0.945)
Inv. FE	✓		✓		✓	
Time FE	✓		✓		✓	
Cohort × Inv. FE		✓		✓		✓
Cohort × Time FE		✓		✓		✓
Adj. R-Square	0.382	0.447	0.306	0.352	0.307	0.375
Observations	671,004	1,674,931	671,004	1,674,931	671,004	1,674,931

Panel B: Mentor Engineer Status

	All Submissions		Status 2 Submissions		Status 1 Submissions	
	(1)	(2)	(3)	(4)	(5)	(6)
Mentored	0.270***	0.336***	0.069***	0.063***	0.137**	0.199**
	(2.890)	(2.901)	(3.565)	(3.223)	(2.113)	(2.455)
Mentored × Top Engineer Mentor	0.017	0.113	-0.006	0.026	0.052	0.116
	(0.266)	(1.321)	(-0.295)	(1.117)	(1.106)	(1.635)
Inv. FE	✓		✓		✓	
Time FE	✓		✓		✓	
Cohort × Inv. FE		✓		✓		✓
Cohort × Time FE		✓		✓		✓
Adj. R-Square	0.382	0.447	0.306	0.352	0.307	0.375
Observations	671,004	1,674,931	671,004	1,674,931	671,004	1,674,931

Panel C: Mentor Formerly in Program

	All Submissions		Status 2 Submissions		Status 1 Submissions	
	(1)	(2)	(3)	(4)	(5)	(6)
Mentored	0.270***	0.414***	0.059***	0.074***	0.181***	0.303***
	(3.895)	(4.180)	(4.808)	(3.951)	(3.228)	(3.974)
Mentored × In Program Mentor	0.044	0.033	-0.026	-0.040	-0.028	-0.050
	(0.352)	(0.214)	(-1.626)	(-1.479)	(-0.433)	(-0.491)
Inv. FE	✓		✓		✓	
Time FE	✓		✓		✓	
Cohort × Inv. FE		✓		✓		✓
Cohort × Time FE		✓		✓		✓
Adj. R-Square	0.381	0.446	0.306	0.352	0.306	0.375
Observations	673,039	1,676,863	673,039	1,676,863	673,039	1,676,863

Notes. The data used is a panel of innovator-quarter IDF submissions. The tests in Panel A require data on the gender of the innovator's mentor. The tests in Panel B require data on the job title of the innovator's mentor. We do not have data on these characteristics for all individuals, leading to the reductions in observation counts. The variable *Female Mentor* is a binary indicator that equals one if the innovator's mentor is female. The variable *Top Engineer Mentor* is a binary indicator that equals one if the innovator's mentor's job title is "Top Engineer." The variable *In Program Mentor* is a binary indicator that equals one if the innovator's mentor was a mentee in a prior cohort of the mentorship program. The empirical specification structure matches that used in Table 2. t-statistics are reported in parentheses. Statistical significance of 10%, 5%, and 1% is denoted by *, **, and ***, respectively.

Table A.3: Heterogeneity in Productivity of Non-Participants After Exposure to Mentored Colleagues

Panel A: Gender

	All Submissions		Status 2 Submissions		Status 1 Submissions	
Post First Exposure	0.883	0.835**	0.399	0.348**	0.081	0.139
	(1.733)	(2.412)	(1.668)	(2.160)	(0.661)	(1.030)
Post First Exposure × Female	-0.588	-0.326	-0.417*	-0.353*	0.133	0.270**
	(-1.299)	(-0.883)	(-1.815)	(-1.824)	(1.314)	(2.061)
Inv. FE	✓		✓		✓	
Time FE	✓		✓		✓	
Cohort × Inv. FE		✓		✓		✓
Cohort × Time FE		✓		✓		✓
Adj. R-Square	0.323	0.331	0.259	0.268	0.255	0.251
Observations	120,811	906,184	120,811	906,184	120,811	906,184

Panel B: URM

	All Submissions		Status 2 Submissions		Status 1 Submissions	
Post First Exposure	0.802	0.840***	0.357	0.316**	0.095	0.212*
	(1.733)	(2.704)	(1.622)	(2.212)	(0.866)	(1.801)
Post First Exposure × URM	-0.483	-0.380	-0.346	-0.291**	0.222**	0.223
	(-1.120)	(-1.210)	(-1.542)	(-2.028)	(2.427)	(1.404)
Inv. FE	✓		✓		✓	
Time FE	✓		✓		✓	
Cohort × Inv. FE		✓		✓		✓
Cohort × Time FE		✓		✓		✓
Adj. R-Square	0.314	0.327	0.256	0.264	0.244	0.257
Observations	152,439	1,149,696	152,439	1,149,696	152,439	1,149,696

Panel C: Female or URM

	All Submissions		Status 2 Submissions		Status 1 Submissions	
Post First Exposure	0.762	0.774**	0.343	0.306**	0.061	0.157
	(1.692)	(2.487)	(1.610)	(2.139)	(0.539)	(1.221)
Post First Exposure × Female/URM	-0.457	-0.289	-0.349*	-0.299*	0.188**	0.253**
	(-1.167)	(-0.954)	(-1.752)	(-1.941)	(2.311)	(2.151)
Inv. FE	✓		✓		✓	
Time FE	✓		✓		✓	
Cohort × Inv. FE		✓		✓		✓
Cohort × Time FE		✓		✓		✓
Adj. R-Square	0.321	0.334	0.259	0.268	0.250	0.262
Observations	157,971	1,191,496	157,971	1,191,496	157,971	1,191,496

Notes. This table reports the results of tests of heterogeneous spillover effects of the mentorship program onto the productivity of non-participants. We restrict the sample to only include innovators who were not directly associated with the mentorship program at any time, either as a mentor or mentee. We create an indicator variable, *Post First Exposure*, which equals one in the first quarter a non-participating innovator is observed to collaborate with a mentored innovator and each subsequent quarter, and zero otherwise. We then regress disclosure submission outcomes on this indicator using both the two-way fixed effects estimator (odd-numbered columns) and the stacked difference-in-differences estimator (even-numbered columns). These tests require data on the gender and/or race/ethnicity of the individual. We do not have data on these characteristics for all individuals, leading to the reductions in observation counts. The variables *Female*, *URM*, and *Female/URM* are binary indicators that equal one if the innovator is female, an underrepresented racial/ethnic minority, or either, respectively. t-statistics are reported in parentheses. Statistical significance of 10%, 5%, and 1% is denoted by *, **, and ***, respectively.

Table A.4: Innovator Team Size Dynamics

Panel A: After First Mentorship Cohort

	Total Innovators			IHS(Total Innovators)		
	Focal	Peer	Both	Focal	Peer	Both
	(1)	(2)	(3)	(4)	(5)	(6)
Post	0.212***	0.187***	0.187***	0.061***	0.046***	0.046***
	(3.962)	(9.382)	(9.381)	(4.315)	(9.509)	(9.509)
Post × Focal Firm			0.025			0.015
			(0.446)			(0.977)
Firm FE		✓	✓		✓	✓
Adj. R-Square	0.004	0.028	0.027	0.004	0.035	0.034
Observations	4,075	60,438	64,513	4,075	60,438	64,513

Panel B: After Second Mentorship Cohort

	Total Innovators			IHS(Total Innovators)		
	Focal	Peer	Both	Focal	Peer	Both
	(1)	(2)	(3)	(4)	(5)	(6)
Post	0.222***	0.197***	0.197***	0.063***	0.049***	0.049***
	(4.067)	(9.439)	(9.439)	(4.355)	(9.612)	(9.612)
Post × Focal Firm			0.025			0.014
			(0.425)			(0.907)
Firm FE		✓	✓		✓	✓
Adj. R-Square	0.004	0.028	0.027	0.004	0.035	0.034
Observations	4,075	60,438	64,513	4,075	60,438	64,513

Panel C: After Third Mentorship Cohort

	Total Innovators			IHS(Total Innovators)		
	Focal	Peer	Both	Focal	Peer	Both
	(1)	(2)	(3)	(4)	(5)	(6)
Post	0.202***	0.213***	0.213***	0.054***	0.052***	0.052***
	(3.422)	(9.293)	(9.292)	(3.455)	(9.367)	(9.367)
Post × Focal Firm			-0.011			0.002
			(-0.173)			(0.093)
Firm FE		✓	✓		✓	✓
Adj. R-Square	0.003	0.028	0.027	0.003	0.035	0.033
Observations	4,075	60,438	64,513	4,075	60,438	64,513

Notes. The data used is patent grant level data from the U.S. PTO from 2018 to present. The dependent variable in Columns (1)–(3) is the total number of innovators listed on a particular patent, and the dependent variable in Columns (4)–(6) is the inverse hyperbolic sine of the total number of innovators listed on a particular patent. We employ ordinary least squares regressions to analyze how these patent team size measures change in relation to the binary indicator *Post*, which equals one from when the first mentorship program began and onward (in Panel A). In Panel B (Panel C), *Post* equals one from when the second (third) mentorship program began and onward. Some models include interactions between *Post* and the binary indicator *Focal Firm*. Columns (1) and (4) use only team size measures from the focal firm. Columns (2) and (5) use only team size measures from the firm's industry peers. Columns (3) and (6) use data from all firms. The models in Columns (2), (3), (5), and (6) include firm fixed effects. *t*-statistics from robust standard errors are reported in parentheses. Statistical significance of 10%, 5%, and 1% is denoted by *, **, and ***, respectively.

B Robustness and Falsification Tests

This appendix demonstrates the robustness of our main findings on the relation between patent mentorship and innovation productivity. First, we validate our results when using alternative control groups, when employing different estimators, and when using different disclosure status classifications. Second, we address scalability by estimating cohort-specific treatment effects, finding statistically indistinguishable coefficients across cohorts. Third, we confirm our results are robust to excluding pre-COVID-19 observations. Finally, we conduct placebo tests for the employee satisfaction and turnover outcomes.

B.1 Alternative Control Groups

In Tables 2 and 3, we presented our main estimates using a combined control group of both not-yet-mentored innovators and never-mentored innovators as the basis for comparison. To assess robustness, we examine alternative control group specifications. Panel A of Table B.1 restricts the sample to program participants only, using not-yet-mentored innovators as the sole control group. The two-way fixed effects estimates in the odd-numbered columns are larger in magnitude relative to those in Table 2, while maintaining statistical significance at the 1% level, suggesting our main specifications provide conservative estimates.

An advantage of using not-yet-mentored innovators as the benchmark stems from their greater comparability with currently-mentored innovators relative to never-mentored innovators. However, the two-way fixed effects estimator may introduce bias through comparisons between already-mentored and currently-mentored innovators. The stacked difference-in-differences estimator addresses this concern by constructing control groups that eliminate such problematic comparisons. The estimates in the even-numbered columns of Panel A are positive and statistically significant at the 1% level, confirming that the positive relation between mentorship and productivity persists when restricting the control group to not-yet-mentored innovators, thereby eliminating bias that might stem from using already-mentored innovators in the control group.

In Panel B of Table B.1, we restrict the control group to never-mentored innovators, and we employ the stacked difference-in-differences estimator. The odd-numbered columns use the full sample of never-mentored innovators, while the even-numbered columns restrict this sample to those most comparable to mentored innovators based on observable characteristics. We identify comparable innovators using the Mahalanobis matching procedure and our rich set of demographic data as observable characteristics (innovator tenure, gender, race/ethnicity, marital status, job role, and work country). We keep only the never-mentored innovators with above-median propensity scores as our final comparison group. The estimates in Panel B are all positive and statistically significant at the 1% level, with magnitudes

comparable to both Panel A's stacked difference-in-differences estimates and those in Table 2. Similarly, the results in Table B.2 demonstrate robust long-term productivity gains across alternative control groups and matching procedures, though statistical precision decreases when restricting the control group to not-yet-mentored innovators, likely due to reduced sample sizes. Collectively, these robustness tests support a positive and significant relation between patent mentorship program participation and innovation productivity as measured by invention disclosure submissions.

B.2 Alternative Estimators

In Table B.3, we show that the estimated relation between mentorship and productivity is robust if we use three alternative estimators: ordinary least squares with no additional fixed effects; the Callaway and Sant'Anna (2021) estimator; and the Poisson pseudo-likelihood estimator, which accounts for the count-like nature of disclosure submissions. In Columns (1), (4), and (7), we estimate ordinary least squares regressions with no other fixed effects. In Columns (2), (5), and (8), we employ the Callaway and Sant'Anna (2021) estimator,[22] and in Columns (3), (6), and (9) we employ the Poisson pseudo-likelihood estimator.

In Panels A and B, we use both not-yet-mentored and never-mentored innovators in the control group. In Panels C and D, we use only not-yet-mentored innovators in the control group. Furthermore, in Panels A and C, we use all available productivity data to account for the overall relation between mentorship and productivity, whereas in Panels B and D, we focus on the long-term relation by removing the observations of mentored workers in the quarter of first mentorship and the first and second quarters after first mentorship.

We find consistent evidence of a positive and statistically significant relation between mentorship and overall submission outcomes, regardless of the choice in estimator or control group. The long-term relation is generally robust across the different combinations of estimator and control group. In particular, the relation is always robust when considering disclosure submissions at any status, and the relations are always statistically significant when using the Poisson pseudo-likelihood estimator.

B.3 Alternative Disclosure Status Classifications

We show in Table B.4 and Table B.5 that our main results are robust to the use of alternative approaches to classifying the various disclosure statuses. In Table B.4, we classify Status

[22] Using this approach, we separately consider the treatment effects across treatment cohorts, where in our case a treatment cohort designation is based on the year-quarter in which an employee first participates in the mentorship program, g. We estimate cohort- and time-specific average treatment effects on the treated employees, $\widehat{ATT}(g,t)$, using two-period/two-group difference-in-differences estimators. Then we compute summary treatment effect estimates by aggregating together the cohort- and time-specific estimates, weighting them by the size of each treatment cohort.

2 disclosures as those with any of the following statuses: Approved - Patent Application; Prior Art Search Requested; RFC; Approved - Defensive Publication; Approved - Trade Secret. Status 1 disclosures are those with no status label or any of the following statuses: Declined; Auto Declined; Defensive Publication Candidate; Deleted; Merged; Filing Candidate; Submitted. The estimates remain very similar to those in Tables 2 and 3, highlighting the robustness of our results when accounting for alternative approaches to categorizing the statuses of the disclosures.

In Table B.5, we show the robustness of our results when using more granular disclosure status classifications. Specifically, we focus on the four most common types of disclosures: "Approved - Patent Application" (31% of the disclosures); "Declined" or "Auto-Declined" (65% of the disclosures); "Defensive Publication Candidate" or "Approved - Defensive Publication" (2% of the disclosures); "Submitted" (1% of the disclosures).[23] We use the full sample of data in Panel A. In Panel B, we consider the long-term productivity effects by removing the observations of mentored workers in the quarter of first mentorship and the first and second quarters after first mentorship. The dependent variable in Columns (1)–(2) is the number of disclosure submissions with the status "Submitted." The dependent variable in Columns (3)–(4) is the number of disclosure submissions with the status "Declined" or "Auto Declined." The dependent variable in Columns (5)–(6) is the number of disclosure submissions with the status "Approved - Patent Application." The dependent variable in Columns (7)–(8) is the number of disclosure submissions with the status "Defensive Publication Candidate" or "Approved - Defensive Publication." We employ the two-way fixed effects estimator in the odd-numbered columns, and we employ the stacked difference-in-differences estimator in even-numbered columns. We find that the relation between mentorship and innovation outcomes is positive and statistically significant across all disclosure statuses, both overall and in the long-term.

B.4 Cohort-by-Cohort Relation Between Mentorship and Productivity

While the firm's quasi-random selection process for program participation limited potential self-selection, we further address self-selection concerns by estimating separate relations between mentorship and productivity for each cohort using never-mentored innovators as controls. Table B.6 presents these estimates, with each column corresponding to a specific cohort. Each specification restricts the sample to never-mentored innovators and innovators first mentored in the designated cohort (first through seventh). We employ the two-way fixed effects estimator throughout, as using only never-mentored innovators as controls eliminates concerns about already-mentored innovator comparisons. Panel A uses the complete sam-

[23]The remaining 1% have statuses such as "RFC" and "Deleted."

ple, while Panel B examines long-term productivity effects by excluding observations from mentored workers during their first mentorship quarter and subsequent two quarters. The dependent variable is the quarterly count of disclosure submissions of any status. Panel A reveals a positive and statistically significant relation between mentorship and productivity for the first five cohorts. While estimates for cohorts six and seven are not significant, their point estimates exceed those of earlier cohorts. Importantly, we cannot reject the null that the seven coefficients are equal when conducting a joint F-test (p-value = 0.952). The reduced precision in cohorts six and seven likely stems from limited post-mentorship data, as our sample ends in November of 2023. Panel B documents a positive and significant relation between mentorship and long-term productivity for three of six cohorts.[24]

These findings suggest that the program is scalable. If productivity gains were limited to the first cohort—potentially comprising particularly motivated participants—this would indicate narrow program benefits. Instead, the positive and significant relation extending through the fifth cohort, despite meaningful demographic differences from early cohorts, suggests broad-based benefits across employee types. These cohort-by-cohort results complement the results in Tables 4 and A.1, and suggest that the program benefits generalize across innovator characteristics and cohorts. This suggests that patent mentoring programs can be scalable, even in large, superstar firms.

B.5 Restricting to Only Post-COVID Data

The first cohort of the mentorship program occurred at the end of 2020, several months after the onset of the COVID-19 global pandemic and the shift to remote and/or hybrid work environments. Because the entirety of the mentorship program occurred in this "post-COVID" setting, we have little reason to worry that our estimates of the relation between the mentorship program and innovator productivity are biased by COVID-related unobservable factors. One COVID-19 related concern that we do account for, however, is that the pre-treatment period used in our analyses includes data from before the onset of COVID-19. In Table B.7, we show the robustness of our results when we restrict to only using the innovator-quarters of productivity data that occurred on or after Q2 of 2020, after the COVID-induced transitions to different work arrangements.[25]

[24] Long-term productivity effects for the seventh cohort cannot be estimated due to sample end-date constraints. As in Panel A, we cannot reject the null that the six long-term coefficients are equal when conducting a joint F-test (p-value = 0.193).

[25] By making this restriction, our sample size decreases from 673,039 to 447,835.

B.6 Alternative Timing of Employee Sentiment and Retention Tests

The results in Table 9 suggested that engineers' perceptions of the focal firm improved after the establishment of the mentorship program, highlighting an additional benefit of the program. In Table B.8, we show that the estimated relation between the mentorship program and engineers' perceptions of the focal firm is robust to alternative definitions of *Post*. In particular, just as we would expect, the positive link remains statistically significant, but attenuated, when *Post* is set to switch from zero to one at a later time (after either the second or third mentorship cohorts begin). This is expected, as the positive perceptions of early cohort participants may be captured in the pre-event period when *Post* switches two one at later times. Again, if the estimated relation is due to forces independent of the mentorship program, we would not be likely to observe this expected pattern in the data.

We also conduct falsification tests using the employee sentiment data to show that the results in Table 9 are not likely driven by some other factors. Specifically, we replace *Post* with the binary indicator *Pseudo-Post*, which equals one in and after October of 2018 (two years before the first cohort of the mentorship program), and we omit data after December of 2019 (to perform the falsification tests solely in the pre-COVID period). We report the results in Table B.9. The null interaction effects on *Pseudo-Post* × *Engineer* in Column (2) and on *Pseudo-Post* × *Engineer* × *Focal Firm* in Column (5) suggest that the corresponding interaction effects in Table 9, which are significant, are not driven by some alternative temporal variation that impacts engineers in the focal firm differently than engineers in peer firms.

Similarly, the results in Table B.10 show that the estimated relation between the mentorship program and engineers' departures from the focal firm is robust to alternative definitions of *Post*. In these tests, we note that the magnitudes of the coefficients are very similar to those in Table 10. So, the relation between the onset of the mentorship program and engineer turnover does not attenuate as immediately as does the relation between the mentorship program and employee sentiment. In addition, the results in Table B.11, which report null interaction effects on *Pseudo-Post* × *Engineer* in Column (2) and on *Pseudo-Post* × *Engineer* × *Focal Firm* in Column (5), suggest that the corresponding interaction effects in Table 10 are not driven by some alternative factors.

Table B.1: Mentor-Productivity Relation: Alternative Control Groups

Panel A: Control Group of Not-Yet-Mentored Innovators Only

	All Submissions		Status 2 Submissions		Status 1 Submissions	
	(1)	(2)	(3)	(4)	(5)	(6)
Mentored	0.518***	0.473***	0.092***	0.083***	0.381***	0.387***
	(4.266)	(4.062)	(4.167)	(3.553)	(3.943)	(4.049)
Inv. FE	✓		✓		✓	
Time FE	✓		✓		✓	
Cohort × Inv. FE		✓		✓		✓
Cohort × Time FE		✓		✓		✓
Adj. R-Square	0.179	0.237	0.076	0.119	0.149	0.182
Observations	11,958	14,745	11,958	14,745	11,958	14,745

Panel B: Control Group of Never-Mentored Innovators Only

	All Submissions		Status 2 Submissions		Status 1 Submissions	
	(1)	(2)	(3)	(4)	(5)	(6)
Mentored	0.416***	0.449***	0.072***	0.071***	0.300***	0.339***
	(4.121)	(4.581)	(3.945)	(4.139)	(3.976)	(4.615)
Cohort × Inv. FE	✓	✓	✓	✓	✓	✓
Cohort × Time FE	✓	✓	✓	✓	✓	✓
PS-Matched Controls		✓		✓		✓
Adj. R-Square	0.447	0.440	0.352	0.394	0.376	0.362
Observations	1,667,577	189,326	1,667,577	189,326	1,667,577	189,326

Notes. The data used is a panel of innovator-quarter IDF submissions. The dependent variable in Columns (1)–(2) is the number of disclosure submissions of any status that an innovator submitted in a given quarter. The dependent variables in Columns (3)–(4) and (5)–(6) are the number of submissions at Status 2 and Status 1, respectively. *Mentored* equals one in the quarter in which an innovator is part of the mentorship program, and each subsequent quarter, and zero otherwise. In Panel A, we employ a two-way fixed effects estimator with innovator and time (calendar quarter) fixed effects in the odd-numbered columns, and we employ a stacked difference-in-differences estimator in even-numbered columns. In Panel A, the control group includes only not-yet-mentored innovators. In Panel B, we use the stacked difference-in-differences estimator in each column, and the control group includes only never-mentored innovators. We use all never-mentored innovators in the odd-numbered columns in Panel B, and we use only those with above median mentorship program participation propensity scores in the even-numbered columns. Standard errors are clustered by innovator and quarter (innovator-by-cohort and quarter-by-cohort) when using the two-way fixed effects (stacked difference-in-differences) estimator, and t-statistics are reported in parentheses. Statistical significance of 10%, 5%, and 1% is denoted by *, **, and ***, respectively.

Table B.2: Long-Term Mentor-Productivity Relation: Alternative Control Groups

Panel A: Control Group of Not-Yet-Mentored Innovators Only

	All Submissions		Status 2 Submissions		Status 1 Submissions	
	(1)	(2)	(3)	(4)	(5)	(6)
Mentored	0.128**	0.140***	0.025**	0.015	0.094*	0.129***
	(2.196)	(2.754)	(2.209)	(1.174)	(1.832)	(2.825)
Inv. FE	✓		✓		✓	
Time FE	✓		✓		✓	
Cohort × Inv. FE		✓		✓		✓
Cohort × Time FE		✓		✓		✓
Adj. R-Square	0.149	0.188	0.074	0.171	0.116	0.163
Observations	10,059	12,846	10,059	12,846	10,059	12,846

Panel B: Control Group of Never-Mentored Innovators Only

	All Submissions		Status 2 Submissions		Status 1 Submissions	
	(1)	(2)	(3)	(4)	(5)	(6)
Mentored	0.143***	0.191***	0.022*	0.021**	0.113***	0.168***
	(3.008)	(4.333)	(1.972)	(2.036)	(2.781)	(4.418)
Cohort × Inv. FE	✓	✓	✓	✓	✓	✓
Cohort × Time FE	✓	✓	✓	✓	✓	✓
PS-Matched Controls		✓		✓		✓
Adj. R-Square	0.448	0.442	0.353	0.395	0.377	0.365
Observations	1,665,678	187,427	1,665,678	187,427	1,665,678	187,427

Notes. The data used is a panel of innovator-quarter IDF submissions, removing the observations of mentored workers in the quarter of first mentorship and the first and second quarters after first mentorship. The dependent variable in Columns (1)–(2) is the number of submissions of any status that an innovator submitted in a given quarter. The dependent variables in Columns (3)–(4) and (5)–(6) are the number of submissions at Status 2 and Status 1, respectively. *Mentored* equals one in the quarter in which an innovator is part of the mentorship program, and each subsequent quarter, and zero otherwise. In Panel A, we employ a two-way fixed effects estimator with innovator and time (calendar quarter) fixed effects in the odd-numbered columns, and we employ a stacked difference-in-differences estimator in even-numbered columns. In Panel A, the control group includes only not-yet-mentored innovators. In Panel B, we use the stacked difference-in-differences estimator in each column, and the control group includes only never-mentored innovators. We use all never-mentored innovators in the odd-numbered columns in Panel B, and we use only those with above median mentorship program participation propensity scores in the even-numbered columns. Standard errors are clustered by innovator and quarter (innovator-by-cohort and quarter-by-cohort) when using the two-way fixed effects (stacked difference-in-differences) estimator, and t-statistics are reported in parentheses. Statistical significance of 10%, 5%, and 1% is denoted by *, **, and ***, respectively.

Table B.3: Mentor-Productivity Relation: Alternative Estimators

Panel A: Full Sample w/ Both Control Groups

	Number: Any Status			Number: Status 2			Number: Status 1		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Mentored	0.218***	0.220***	1.996***	0.028**	0.041***	2.016***	0.163**	0.157***	2.061***
	(3.121)	(9.690)	(7.428)	(2.371)	(4.630)	(6.577)	(2.772)	(8.020)	(7.624)
Estimator	OLS	CSDID	Poisson	OLS	CSDID	Poisson	OLS	CSDID	Poisson
Ad./Ps. R-Sq.	0.001	-	0.400	0.000	-	0.360	0.001	-	0.334
Observations	673,039	672,568	187,441	673,039	672,568	83,189	673,039	672,568	154,496

Panel B: Long-Term Productivity w/ Both Control Groups

	Number: Any Status			Number: Status 2			Number: Status 1		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Mentored	0.061*	0.074***	1.259***	0.002	0.015	1.229***	0.046	0.049**	1.452***
	(1.764)	(3.050)	(5.261)	(0.344)	(1.470)	(4.123)	(1.539)	(2.430)	(5.108)
Estimator	OLS	CSDID	Poisson	OLS	CSDID	Poisson	OLS	CSDID	Poisson
Ad./Ps. R-Sq.	0.000	-	0.401	-0.000	-	0.360	0.000	-	0.335
Observations	671,140	668,415	180,701	671,140	668,415	80,556	671,140	668,415	149,128

Panel C: Full Sample w/ Only Not-Yet-Mentored Control Group

	Number: Any Status			Number: Status 2			Number: Status 1		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Mentored	0.218***	0.270***	2.417***	0.043***	0.050***	2.599***	0.147**	0.223***	2.307***
	(3.113)	(8.710)	(7.916)	(3.624)	(4.360)	(5.280)	(2.465)	(8.150)	(8.045)
Estimator	OLS	CSDID	Poisson	OLS	CSDID	Poisson	OLS	CSDID	Poisson
Ad./Ps. R-Sq.	0.025	-	0.330	0.008	-	0.248	0.016	-	0.303
Observations	11,958	10,043	11,760	11,958	10,043	4,136	11,958	10,043	9,580

Panel D: Long-Term Productivity w/ Only Not-Yet-Mentored Control Group

	Number: Any Status			Number: Status 2			Number: Status 1		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Mentored	0.061*	0.067**	1.125**	0.017**	0.016	1.969***	0.029	0.053*	0.960**
	(1.689)	(1.920)	(2.427)	(2.425)	(1.220)	(2.683)	(0.934)	(1.780)	(1.851)
Estimator	OLS	CSDID	Poisson	OLS	CSDID	Poisson	OLS	CSDID	Poisson
Ad./Ps. R-Sq.	0.003	-	0.252	0.002	-	0.204	0.001	-	0.225
Observations	10,059	8,426	5,020	10,059	8,426	1,627	10,059	8,426	4,212

Notes. The data used is a panel of innovator-quarter IDF submissions. We use the full sample of data in Panels A and C. In Panels B and D, we remove the observations of mentored workers in the quarter of first mentorship and the first and second quarters after first mentorship. The dependent variable in Columns (1)–(3) is the number of disclosure submissions of any status that an innovator submitted in a given quarter. The dependent variables in Columns (4)–(6) and (7)–(9) are the number of submissions at Status 2 and Status 1, respectively. *Mentored* equals one in the quarter in which an innovator is part of the mentorship program, and each subsequent quarter, and zero otherwise. In Columns (1), (4), and (7), we estimate ordinary least squares regressions with no other fixed effects. In Columns (2), (5), and (8), we employ the Callaway and Sant'Anna (2021) estimator, and in Columns (3), (6), and (9) we employ the Poisson pseudo-likelihood estimator with innovator and year-quarter fixed effects. In Panels A and B, we use both not-yet-mentored and never-mentored innovators in the control group. In Panels C and D, we use only not-yet-mentored innovators in the control group. t-statistics (z-statistics) are reported in parentheses. The Callaway and Sant'Anna (2021) estimator does not produce R-squared values. Statistical significance of 10%, 5%, and 1% is denoted by *, **, and ***, respectively.

Table B.4: Mentor-Productivity Relation: Alternative Status 2 vs. Status 1 Classifications

Panel A: Overall Productivity

	All Submissions		Status 2 (Alternative)		Status 1 (Alternative)	
	(1)	(2)	(3)	(4)	(5)	(6)
Mentored	0.272***	0.416***	0.083***	0.112***	0.189***	0.304***
	(3.827)	(4.121)	(4.335)	(3.841)	(3.422)	(4.044)
Inv. FE	✓		✓		✓	
Time FE	✓		✓		✓	
Cohort × Inv. FE		✓		✓		✓
Cohort × Time FE		✓		✓		✓
Adj. R-Square	0.381	0.446	0.325	0.368	0.294	0.367
Observations	673,039	1,676,863	673,039	1,676,863	673,039	1,676,863

Panel B: Long-Term Productivity

	All Submissions		Status 2 (Alternative)		Status 1 (Alternative)	
	(1)	(2)	(3)	(4)	(5)	(6)
Mentored	0.110***	0.143***	0.036***	0.032**	0.074**	0.111***
	(3.020)	(3.006)	(3.933)	(2.360)	(2.523)	(2.954)
Inv. FE	✓		✓		✓	
Time FE	✓		✓		✓	
Cohort × Inv. FE		✓		✓		✓
Cohort × Time FE		✓		✓		✓
Adj. R-Square	0.383	0.447	0.326	0.369	0.296	0.369
Observations	671,140	1,674,964	671,140	1,674,964	671,140	1,674,964

Notes. This table presents an analysis of the relationship between participation in the mentorship program and innovator productivity, as measured by invention disclosure form (IDF) submissions. The data in Panel A comprises a panel of innovator-quarter submissions. The data in Panel B removes the observations of mentored workers in the quarter of first mentorship and the first and second quarters after first mentorship. In Columns (1)–(2), the dependent variable is the total number of disclosure submissions of any status that an innovator submitted in a given quarter. The dependent variables in Columns (3)–(4) and (5)–(6) focus on the number of submissions at Status 2 and Status 1, respectively. Status 2 disclosures are those with any of the following statuses: Approved - Patent Application; Prior Art Search Requested; RFC; Approved - Defensive Publication; Approved - Trade Secret. Status 1 disclosures are those with no status label or any of the following statuses: Declined; Auto Declined; Defensive Publication Candidate; Deleted; Merged; Filing Candidate; Submitted. The variable of interest, *Mentored*, is an indicator equal to one in the quarter in which an innovator is part of the mentorship program and all subsequent quarters, and zero otherwise. We employ two estimation strategies: a two-way fixed effects estimator with innovator and time (calendar quarter) fixed effects in the odd-numbered columns, and we employ a stacked difference-in-differences estimator in even-numbered columns. The control group includes both not-yet-mentored innovators and never-mentored innovators. For the two-way fixed effects model, standard errors are clustered by innovator and quarter. In the stacked difference-in-differences specification, clustering is at the innovator-by-cohort and quarter-by-cohort levels. t-statistics are reported in parentheses. Statistical significance of 10%, 5%, and 1% is denoted by *, **, and ***, respectively.

Table B.5: Mentor-Productivity Relation: Alternative Disclosure Status Classifications

Panel A: Full Sample

	Submission		Declined		Approved Patent		Publication	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mentored	0.237***	0.372***	0.146***	0.245***	0.051***	0.066***	0.027***	0.042***
	(3.660)	(4.109)	(3.188)	(4.103)	(4.480)	(3.855)	(3.124)	(3.352)
Inv. FE	✓		✓		✓		✓	
Time FE	✓		✓		✓		✓	
Cohort × Inv. FE		✓		✓		✓		✓
Cohort × Time FE		✓		✓		✓		✓
Adj. R-Square	0.383	0.442	0.277	0.341	0.306	0.353	0.128	0.139
Observations	673,039	1,676,863	673,039	1,676,863	673,039	1,676,863	673,039	1,676,863

Panel B: Long-Term Productivity

	Submission		Declined		Approved Patent		Publication	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mentored	0.100**	0.134***	0.062**	0.102***	0.023***	0.021*	0.010*	0.012*
	(2.741)	(2.863)	(2.221)	(2.843)	(3.679)	(1.944)	(2.059)	(1.889)
Inv. FE	✓		✓		✓		✓	
Time FE	✓		✓		✓		✓	
Cohort × Inv. FE		✓		✓		✓		✓
Cohort × Time FE		✓		✓		✓		✓
Adj. R-Square	0.385	0.443	0.280	0.342	0.307	0.353	0.131	0.141
Observations	671,140	1,674,964	671,140	1,674,964	671,140	1,674,964	671,140	1,674,964

Notes. The data used is a panel of innovator-quarter IDF submissions. We use the full sample of data in Panel A. In Panel B, we remove the observations of mentored workers in the quarter of first mentorship and the first and second quarters after first mentorship. The dependent variable in Columns (1)–(2) is the number of disclosure submissions with the status "Submitted." The dependent variable in Columns (3)–(4) is the number of disclosure submissions with the status "Declined" or "Auto Declined." The dependent variable in Columns (5)–(6) is the number of disclosure submissions with the status "Approved - Patent Application." The dependent variable in Columns (7)–(8) is the number of disclosure submissions with the status "Defensive Publication Candidate" or "Approved - Defensive Publication." *Mentored* equals one in the quarter in which an innovator is part of the mentorship program, and each subsequent quarter, and zero otherwise. We employ two estimation strategies: a two-way fixed effects estimator with innovator and time (calendar quarter) fixed effects in the odd-numbered columns, and we employ a stacked difference-in-differences estimator in even-numbered columns. The control group includes both not-yet-mentored innovators and never-mentored innovators. For the two-way fixed effects model, standard errors are clustered by innovator and quarter. In the stacked difference-in-differences specification, clustering is at the innovator-by-cohort and quarter-by-cohort levels. t-statistics are reported in parentheses. Statistical significance of 10%, 5%, and 1% is denoted by *, **, and ***, respectively.

Table B.6: Cohort-by-Cohort Relation Between Mentorship and Productivity

Panel A: Full Sample

	Cohort 1	Cohort 2	Cohort 3	Cohort 4	Cohort 5	Cohort 6	Cohort 7
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Mentored	0.311**	0.291**	0.179*	0.208*	0.282*	0.378	0.477
	(2.466)	(2.262)	(1.720)	(1.816)	(1.734)	(1.410)	(1.286)
Inv. FE	✓	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓	✓
Adj. R-Square	0.384	0.384	0.384	0.384	0.384	0.384	0.384
Observations	663,216	662,620	664,530	662,160	662,525	661,696	662,778

Panel B: Long-Term Productivity

	Cohort 1	Cohort 2	Cohort 3	Cohort 4	Cohort 5	Cohort 6	Cohort 7
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Mentored	0.172**	0.181***	0.031	0.059	0.172*	0.155	-
	(2.329)	(3.099)	(0.970)	(1.252)	(2.057)	(1.490)	-
Inv. FE	✓	✓	✓	✓	✓	✓	-
Time FE	✓	✓	✓	✓	✓	✓	-
Adj. R-Square	0.384	0.384	0.384	0.384	0.384	0.385	-
Observations	662,913	662,401	664,017	661,995	662,261	661,576	-

Notes. The data used is a panel of innovator-quarter IDF submissions. We use the full sample of data in Panel A. In Panel B, we remove the observations of mentored workers in the quarter of first mentorship and the first and second quarters after first mentorship. The dependent variable in all columns is the number of disclosure submissions of any status that an innovator submitted in a given quarter. *Mentored* equals one in the quarter in which an innovator is part of the mentorship program, and each subsequent quarter, and zero otherwise. In each column, we restrict the sample to include only never-mentored innovators and those innovators who were first mentored in the designated cohort (the first, second, third, etc.). Due to data limitations, we are not able to estimate long-term productivity effects for the seventh cohort, as our sample ends in the last quarter of 2023. We employ a two-way fixed effects estimator with innovator and time (calendar quarter) fixed effects in all columns. Standard errors are clustered by innovator and quarter. t-statistics are reported in parentheses. Statistical significance of 10%, 5%, and 1% is denoted by *, **, and ***, respectively.

Table B.7: Mentor-Productivity Relation: Restricting to Only Post-COVID Data

Panel A: Full Sample

	All Submissions		Status 2 Submissions		Status 1 Submissions	
	(1)	(2)	(3)	(4)	(5)	(6)
Mentored	0.257***	0.398***	0.052***	0.068***	0.156**	0.280***
	(3.235)	(3.958)	(3.730)	(3.797)	(2.824)	(3.812)
Inv. FE	✓		✓		✓	
Time FE	✓		✓		✓	
Cohort × Inv. FE		✓		✓		✓
Cohort × Time FE		✓		✓		✓
Adj. R-Square	0.429	0.465	0.367	0.386	0.341	0.384
Observations	447,835	1,598,928	447,835	1,598,928	447,835	1,598,928

Panel B: Long-Term Productivity

	All Submissions		Status 2 Submissions		Status 1 Submissions	
	(1)	(2)	(3)	(4)	(5)	(6)
Mentored	0.078**	0.130***	0.017**	0.018*	0.047	0.102***
	(2.160)	(2.894)	(2.769)	(1.704)	(1.455)	(2.668)
Inv. FE	✓		✓		✓	
Time FE	✓		✓		✓	
Cohort × Inv. FE		✓		✓		✓
Cohort × Time FE		✓		✓		✓
Adj. R-Square	0.432	0.466	0.369	0.387	0.346	0.386
Observations	445,936	1,597,029	445,936	1,597,029	445,936	1,597,029

Notes. This table presents an analysis of the relationship between participation in the mentorship program and innovator productivity, as measured by invention disclosure form (IDF) submissions. The data comprises a panel of innovator-quarter submissions. We remove all observations that occur before global economic shutdowns began in March of 2020. We otherwise use the full sample of data in Panel A. In Panel B, we remove the observations of mentored workers in the quarter of first mentorship and the first and second quarters after first mentorship. In Columns (1)–(2), the dependent variable is the total number of disclosure submissions of any status that an innovator submitted in a given quarter. The dependent variables in Columns (3)–(4) and (5)–(6) focus on the number of submissions at Status 2 and Status 1, respectively. The variable of interest, *Mentored*, is an indicator equal to one in the quarter in which an innovator is part of the mentorship program and all subsequent quarters, and zero otherwise. We employ two estimation strategies: a two-way fixed effects estimator with innovator and time (calendar quarter) fixed effects in the odd-numbered columns, and we employ a stacked difference-in-differences estimator in even-numbered columns. The control group includes both not-yet-mentored innovators and never-mentored innovators. For the two-way fixed effects model, standard errors are clustered by innovator and quarter. In the stacked difference-in-differences specification, clustering is at the innovator-by-cohort and quarter-by-cohort levels. t-statistics are reported in parentheses. Statistical significance of 10%, 5%, and 1% is denoted by *, **, and ***, respectively.

Table B.8: Engineers' Workplace Perceptions: Alternative Timing

Panel A: After the Second Mentorship Cohort

	Focal Firm Only		Peer Firms Only		Both
	(1)	(2)	(3)	(4)	(5)
Post	0.101***	0.060***	0.017***	0.012**	0.013**
	(5.732)	(2.584)	(3.195)	(2.079)	(2.092)
Engineer		-0.173***		-0.007	-0.007
		(-5.738)		(-0.644)	(-0.602)
Post × Engineer		0.102***		0.022*	0.022*
		(2.860)		(1.724)	(1.727)
Post × Engineer × Focal Firm					0.074**
					(1.973)
Country FE	✓	✓	✓	✓	✓
Firm FE			✓	✓	✓
Adj. R-Square	0.010	0.014	0.055	0.055	0.056
Observations	12,019	12,019	230,044	230,044	242,066

Panel B: After the Third Mentorship Cohort

	Focal Firm Only		Peer Firms Only		Both
	(1)	(2)	(3)	(4)	(5)
Post	0.094***	0.069***	0.030***	0.030***	0.030***
	(5.726)	(3.162)	(6.618)	(5.804)	(5.853)
Engineer		-0.135***		0.010	0.010
		(-5.593)		(1.110)	(1.180)
Post × Engineer		0.060*		-0.000	-0.000
		(1.843)		(-0.005)	(-0.020)
Post × Engineer × Focal Firm					0.060*
					(1.740)
Country FE	✓	✓	✓	✓	✓
Firm FE			✓	✓	✓
Adj. R-Square	0.010	0.013	0.056	0.056	0.056
Observations	12,019	12,019	230,044	230,044	242,066

Notes. The data used are individual reviews from 2018 to 2024 posted on Glassdoor by current employees of the focal firm and the firm's industry peers. We only use reviews for individuals with non-missing job title data (necessary to identify engineers). The dependent variable is the overall rating of the firm left by the individual. We estimate ordinary least squares regressions of these ratings on the binary indicator *Post*, which equals one in and after January of 2021 (July of 2021) in Panel A (Panel B). In some models, we interact the binary indicator *Engineer* with *Post*, and in others we also interact the binary indicator *Focal Firm* into the model. Columns (1)–(2) use only reviews about the focal firm. Columns (3)–(4) use only reviews about the firm's industry peers. Column (5) uses data from all firms. All models include country fixed effects, and the models in Columns (3)–(5) include firm fixed effects. t-statistics from robust standard errors are reported in parentheses. Statistical significance of 10%, 5%, and 1% is denoted by *, **, and ***, respectively.

Table B.9: Engineers' Workplace Perceptions: Falsification Tests

Panel A: Two-Year Lagged Timing

	Focal Firm Only		Peer Firms Only		Both
	(1)	(2)	(3)	(4)	(5)
Pseudo-Post	0.055	0.028	0.046***	0.041**	0.040**
	(1.242)	(0.491)	(2.987)	(2.434)	(2.352)
Engineer		-0.235***		0.002	0.005
		(-3.180)		(0.062)	(0.153)
Pseudo-Post × Engineer		0.063		0.023	0.022
		(0.713)		(0.609)	(0.581)
Pseudo-Post × Engineer × Focal Firm					0.036
					(0.383)
Country FE	✓	✓	✓	✓	✓
Firm FE			✓	✓	✓
Adj. R-Square	0.012	0.020	0.068	0.068	0.070
Observations	2,261	2,261	24,345	24,345	26,620

Notes. The data used are individual reviews posted on Glassdoor by current employees of the focal firm and the firm's industry peers. We only use reviews for individuals with non-missing job title data (necessary to identify engineers). The dependent variable is the overall rating of the firm left by the individual. We estimate ordinary least squares regressions of these ratings on the binary indicator *Pseudo-Post*, which equals one in and after October of 2018, and we omit data after December of 2019 (to perform the falsification tests solely in the pre-COVID period). In some models, we interact the binary indicator *Engineer* with *Post*, and in others we also interact the binary indicator *Focal Firm* into the model. Columns (1)–(2) use only reviews about the focal firm. Columns (3)–(4) use only reviews about the firm's industry peers. Column (5) uses data from all firms. All models include country fixed effects, and the models in Columns (3)–(5) include firm fixed effects. t-statistics from robust standard errors are reported in parentheses. Statistical significance of 10%, 5%, and 1% is denoted by *, **, and ***, respectively.

Table B.10: Engineers' Departure Decisions: Alternative Timing

Panel A: Outflows (After the Second Mentorship Cohort)

	Focal Firm Only		Peer Firms Only		Both
	(1)	(2)	(3)	(4)	(5)
Post	-0.064***	-0.027***	0.061***	0.059***	0.059***
	(-11.673)	(-10.522)	(16.087)	(19.131)	(19.137)
Engineer		0.954***		0.824***	0.821***
		(34.997)		(70.232)	(70.082)
Post × Engineer		-0.253***		0.014	0.014
		(-7.462)		(0.810)	(0.810)
Post × Engineer × Focal Firm					-0.267***
					(-7.074)
Region and Gender FEs	✓	✓	✓	✓	✓
Ethnicity and Seniority FEs	✓	✓	✓	✓	✓
Firm FE			✓	✓	✓
Adj. R-Square	0.061	0.077	0.032	0.035	0.036
Observations	706,860	706,860	7,999,936	7,999,936	8,706,796

Panel B: Inverse Hyperbolic Sine of Outflows (After the Second Mentorship Cohort)

	Focal Firm Only		Peer Firms Only		Both
	(1)	(2)	(3)	(4)	(5)
Post	-0.018***	-0.012***	0.007***	0.009***	0.009***
	(-20.608)	(-15.649)	(24.464)	(28.628)	(28.625)
Engineer		0.253***		0.169***	0.169***
		(82.073)		(212.368)	(212.247)
Post × Engineer		-0.044***		-0.006***	-0.006***
		(-11.060)		(-6.008)	(-6.008)
Post × Engineer × Focal Firm					-0.038***
					(-9.021)
Region and Gender FEs	✓	✓	✓	✓	✓
Ethnicity and Seniority FEs	✓	✓	✓	✓	✓
Firm FE			✓	✓	✓
Adj. R-Square	0.206	0.245	0.207	0.223	0.223
Observations	706,860	706,860	7,999,936	7,999,936	8,706,796

Notes. This table presents an analysis of potential changes in employee departure patterns coinciding with the implementation of the patent mentorship program. The data used is employee departure measures from 2018 to 2024 from Revelio, a labor market intelligence firm that tracks employee movements across firms. Panel A analyzes *Outflows*, defined as the number of employees leaving the company within a specific occupation-region-gender-ethnicity-seniority group in a given month. Panel B examines the inverse hyperbolic sine transformation of this scaled external outflows measure. We employ ordinary least squares regressions to analyze these departure measures in relation to the binary indicator *Post*, which equals one in and after January of 2021 (when the second mentorship cohort began). Some models include interactions between *Post* and the binary indicator *Engineer*, while others further incorporate interactions with the binary indicator *Focal Firm*. Columns (1)–(2) use only departure measures from the focal firm. Columns (3)–(4) use only departure measures from the firm's industry peers. Column (5) uses data from all firms. All models include region, gender, ethnicity, and seniority fixed effects, and the models in Columns (3)–(5) include firm fixed effects. t-statistics from robust standard errors are reported in parentheses. Statistical significance of 10%, 5%, and 1% is denoted by *, **, and ***, respectively.

Table B.11: Engineers' Departure Decisions: Falsification Tests

Panel A: Outflows (Two-Year Lagged Timing)

	Focal Firm Only		Peer Firms Only		Both
	(1)	(2)	(3)	(4)	(5)
Pseudo-Post	0.010	0.005	0.016***	0.015***	0.015***
	(0.847)	(0.956)	(2.807)	(3.841)	(3.842)
Engineer		0.980***		0.813***	0.812***
		(17.706)		(36.236)	(36.222)
Pseudo-Post × Engineer		0.032		0.003	0.003
		(0.455)		(0.120)	(0.120)
Pseudo-Post × Engineer × Focal Firm					0.029
					(0.378)
Region and Gender FEs	✓	✓	✓	✓	✓
Ethnicity and Seniority FEs	✓	✓	✓	✓	✓
Firm FE			✓	✓	✓
Adj. R-Square	0.064	0.082	0.035	0.040	0.041
Observations	220,320	220,320	2,493,480	2,493,480	2,713,800

Panel B: Inverse Hyperbolic Sine of Outflows (Two-Year Lagged Timing)

	Focal Firm Only		Peer Firms Only		Both
	(1)	(2)	(3)	(4)	(5)
Pseudo-Post	0.003**	0.003*	0.004***	0.004***	0.004***
	(2.052)	(1.878)	(7.379)	(7.483)	(7.480)
Engineer		0.256***		0.167***	0.167***
		(41.909)		(106.357)	(106.456)
Pseudo-Post × Engineer		0.006		0.001	0.001
		(0.714)		(0.282)	(0.282)
Pseudo-Post × Engineer × Focal Firm					0.005
					(0.609)
Region and Gender FEs	✓	✓	✓	✓	✓
Ethnicity and Seniority FEs	✓	✓	✓	✓	✓
Firm FE			✓	✓	✓
Adj. R-Square	0.215	0.259	0.203	0.220	0.221
Observations	220,320	220,320	2,493,480	2,493,480	2,713,800

Notes. This table presents an analysis of potential changes in employee departure patterns coinciding with the implementation of the patent mentorship program. The data used is employee departure measures from Revelio, a labor market intelligence firm that tracks employee movements across firms. Panel A analyzes *Outflows*, defined as the number of employees leaving the company within a specific occupation-region-gender-ethnicity-seniority group in a given month. Panel B examines the inverse hyperbolic sine transformation of this scaled external outflows measure. We employ ordinary least squares regressions to analyze these departure measures in relation to the binary indicator *Pseudo-Post*, which equals one in and after October of 2018, and we omit data after December of 2019 (to perform the falsification tests solely in the pre-COVID period). Some models include interactions between *Post* and the binary indicator *Engineer*, while others further incorporate interactions with the binary indicator *Focal Firm*. Columns (1)–(2) use only departure measures from the focal firm. Columns (3)–(4) use only departure measures from the firm's industry peers. Column (5) uses data from all firms. All models include region, gender, ethnicity, and seniority fixed effects, and the models in Columns (3)–(5) include firm fixed effects. t-statistics from robust standard errors are reported in parentheses. Statistical significance of 10%, 5%, and 1% is denoted by *, **, and ***, respectively.

C Mentorship Survey Questions

1. Have you in your time at [Company] ever acted as a patent mentor to a less experienced employee?

    - Yes
    - No

2. Have you in your time at [Company] ever received patent mentorship from a more experienced employee?

    - Yes
    - No

    If the answer to mentorship is no, ask the following two questions.

3. If the answer to mentorship is no, what are the reasons you have not sought patent mentorship? (check all that apply)

    - I could not identify a potential mentor with overlapping interest
    - I did not believe patent mentorship would benefit me
    - I did not have time for patent mentorship or it was offered at inconvenient times
    - I want to avoid activities that highlight my weakness to leaders
    - I felt that the program would benefit my employer more than it would benefit me
    - I did not think about it
    - My co-workers already help me with IP so I don't need a mentor
    - I want to participate in patent mentorship but was unable to

4. If you were to be paired with a mentor, what type of affinity would you like to share? (Check all that apply)

    - Same gender
    - Same age
    - Same ethnicity
    - Same university
    - Overlapping affinity does not matter

    If the answer to mentorship is yes, ask the following questions.

5. For the next set of questions, think of your most recent patent mentorship relationship where you were the protégé (i.e., the person receiving mentorship).

   Slider with range-2 = Strongly disagree, 0 = Neutral, 2 = Strongly agree

   - I have learned about patenting and become a better inventor as a result of being part of the program
   - Patent mentorship increased my satisfaction at work
   - I am planning to work on more invention disclosures at work
   - I feel confident that I can incorporate my mentor's tips into my work process and develop new inventions
   - I feel confident that I have the social connections necessary to get my invention ideas accepted by IP professionals

   How often would your patent mentor provide you with advice, suggestions, or support?

   - Daily
   - Several times a week
   - Weekly
   - Several times a month
   - Monthly

6. Who first initiated mentor-protégé like contact?

   - Your mentor by offering unsolicited advice, suggestions, or support.
   - You by asking the person for advice, suggestions, or support.
   - Other (please specify)

7. Did you and your mentor have an affinity? (Check all that apply)

   - Yes, we are the same gender
   - Yes, we are the same age
   - Yes, we are the same ethnicity
   - Yes, we went to the same university
   - No, we do not have an overlapping affinity

If [Company] has a formal mentorship program, ask the following question.

8. Since your formal mentorship relationship ended, on average how many times per month have you met with your mentor/mentee outside of your regularly scheduled work meetings, even if just briefly?

    - 0 times
    - 1-2 times
    - 3-4 times
    - 5 or more times