Essays in Finance and Innovation

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Essays in Finance and Innovation

A dissertation presented
by

Yosub Jung

to

The Department of Economics

in partial fulfillment of the requirements
for the degree of
Doctor of Philosophy
in the subject of
Business Economics

Harvard University
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Abstract

My dissertation deals with topics in finance and innovation. In the first chapter, I study how CEOs affect corporate behaviors and R&D workers. In the second chapter, I propose a simple way to address an endogeneity problem in tax multiplier studies. In the third chapter, I study how property rights affect innovation.
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Introduction

My dissertation deals with topics in finance and innovation. In the first chapter, I study how CEOs affect corporate behaviors and R&D workers. I use health-related CEO turnovers (e.g., deaths) for identification. My difference-in-differences design compares firms with CEO turnovers to firms without. I find that inventors leave firms suddenly after CEO turnovers, especially when CEOs without STEM degrees succeed CEOs with STEM degrees (STEM CEOs). Text analysis of 10K filings suggests that STEM CEOs emphasize innovation, which retains inventors. But event studies indicate that having STEM CEOs negatively affects firms’ stock prices. Patent citation analysis suggests why: while STEM CEOs may retain valuable R&D workers, they may overlook less-innovative but valuable R&D projects.

In the second chapter, I propose a simple way to address an endogeneity problem in tax multiplier studies. The endogeneity arises because lawmakers tend to propose and legislate tax cuts in anticipation of a slowing economy, making it difficult to identify the causal impact of tax changes on aggregate output. Although all proposed tax changes are likely to be correlated with the output expectations of lawmakers, only the legislated tax changes directly impact the economy. Hence, proposed tax changes that ultimately fail to become law can serve as a proxy for the unobserved output expectations of lawmakers. Using this proxy method and novel data on unlegislated tax proposals, we obtain a tax multiplier of -1.1 in our baseline specification for the United States from 1975 to 2017. Our approach can have a wide variety of applications to other fiscal multiplier studies.

In the third chapter, I study how property rights affect innovation. In the 1800s, three states in the US passed laws granting married women the economic rights to their patent
inventions. The number of patents granted to women increased by 42 percent upon passage of these laws. Further analysis of the granted patents show that women patented both in household technologies (e.g., kitchenware) and in non-household technologies (e.g., submarine microscopes). These findings suggest that the granting of economic rights to women increases innovation in countries where economic rights are limited.
Chapter 1

Corporate Strategies, Human Capital, and the Management of High-Tech Firms

1.1 Introduction

I study how CEOs affect corporate behaviors and the retention of R&D workers at high-tech firms. Because high-tech firms are among the fastest-growing and largest firms in the world, and they foster innovation that drives economic growth, it is important to understand how CEOs of such firms shape corporate outcomes. Discerning how CEOs retain R&D workers is particularly crucial since such workers are among the most valuable assets at high-tech firms.

Despite the need for such information, we know little about how CEOs affect R&D workers. Existing empirical studies document that CEOs affect such corporate behaviors as investment, M&As, patenting, and R&D spending (e.g., Bertrand and Schoar, 2003; Galasso and Simcoe, 2011; Hambrick and Mason, 1984; Hirshleifer, Low, and Teoh, 2012), but they usually overlook CEO effects on workers. This gap is puzzling because theoretical studies suggest a close link between the two types of CEO effects: when CEOs change corporate
behaviors, this affects workers as well (Rotemberg and Saloner, 2000; Van den Steen, 2005). For example, innovation-oriented CEOs may commit firms to continued innovation, thus assuring R&D workers of future R&D opportunities. If R&D workers derive benefits from doing R&D projects (Stern, 2004), then R&D workers are more likely to stay when innovation-oriented CEOs manage firms. I explore this idea empirically.

Using health-related CEO turnovers (deaths and terminal illnesses) for identification, I analyze how inventors — an important subgroup of R&D workers — respond to changes in CEOs and corporate behaviors. My sample is U.S. public firms that filed patents between 1977 and 2013. I use a difference-in-differences design that compares firms that have health-related CEO turnovers to those that do not. I focus on health-related turnovers because other CEO turnovers are endogenous to firm conditions and produce spurious relationships. For example, bad firm conditions can lead to the firing of CEOs, inventor outflows, and changes in corporate behaviors, resulting in spurious relationships among the three variables. To control for time-varying industry conditions, I use a difference-in-differences design that compares 55 turnover firms (firms with health-related CEO turnovers) and their 1,057 employee-inventors to 55 matched non-turnover firms (firms without health-related CEO turnovers) and their 2,724 employee-inventors.

I find that after CEO turnovers, inventors are 31% more likely to suddenly leave firms. Inventor outflows at turnover and non-turnover firms follow parallel trends in the years before CEO turnovers, suggesting inventors working at non-turnover firms can be used as counterfactuals to inventors at turnover firms. In short, CEO turnovers cause inventor outflows.

My findings suggest that CEOs with STEM (Science, Technology, Engineering and Mathematics) degrees (hereafter, STEM CEOs) retain inventors because the former emphasize innovation over finance. I find that inventors are more likely to leave firms when STEM CEOs are followed by CEOs without STEM degrees (hereafter, non-STEM CEOs). Text analysis of firms’ 10Ks suggests a channel: STEM CEOs emphasize innovation; non-STEM CEOs emphasize finance, and thus potentially drive out inventors. These findings may be
related to inventors’ preference for innovation. For example, my survey results show that inventors prefer doing innovation-oriented jobs (Stern, 2004) and working for STEM CEOs. They seem to prefer STEM CEOs because the latter have a long-term perspective and high tolerance for failure — characteristics that may encourage innovation (Azoulay, Graff Zivin, and Manso, 2011; Lerner and Wulf, 2007; Manso, 2011). Furthermore, I find that inventors who have a stronger preference for innovation are more likely to leave after CEO turnovers. Taken together, my findings suggest that STEM CEOs’ emphasis on innovation helps retain inventors who prefer working at pioneering firms. In that regard, having STEM CEOs may be beneficial to high-tech firms, given inventors’ importance for such firms.

However, my findings also suggest that STEM CEOs may stay too long, causing their management styles to become stale and suboptimal. For example, when non-STEM CEOs succeed STEM CEOs, the stock market reacts positively. Furthermore, in my sample, firms never replace non-STEM CEOs with STEM CEOs, although the reverse is common. These findings suggest a cost to having STEM CEOs. Corporate life-cycle theories (Abernathy and Utterback, 1978; Klepper, 1996) suggest why: mature firms need late-stage innovation, but STEM CEOs may focus on early-stage innovation. Consistent with this hypothesis, under STEM CEOs, firms’ patents cite more academic research articles — a sign of doing early-stage innovation. Taken together, my findings suggest that STEM CEOs may stay too long and delay late-stage corporate development. In this light, having STEM CEOs may be harmful to firms after a period of time.

In sum, my findings suggest that STEM CEOs’ emphasis on innovation is a double-edged sword. It retains valuable R&D workers who want to innovate, but it also causes firms to delay late-stage corporate development. Facing this trade-off, firms may prefer CEOs with different strengths at different times.

My findings have three implications. First, CEOs may affect corporate behaviors, which in turn may affect workers. Existing empirical studies document CEO effects on such corporate behaviors as investment (Benmelech and Frydman, 2015; Bertrand and Schoar, 2003; Graham, Harvey, and Puri, 2013; Hambrick and Mason, 1984; Kaplan, Klebanov, and
Sorensen, 2012; Malmendier and Tate, 2005) and innovation (Acemoglu, Akcigit, and Celik, 2014; Custódio, Ferreira, and Matos, 2017; Galasso and Simcoe, 2011; Hirshleifer, Low, and Teoh, 2012; Islama and Zein, 2018). For example, overconfident CEOs spend more on R&D and produce more patents (Galasso and Simcoe, 2011; Hirshleifer, Low, and Teoh, 2012). But existing empirical studies usually overlook CEOs’ effects on workers.¹ My key contribution is documenting that when CEOs change corporate behaviors, workers at those firms are affected as well. This is in line with theoretical studies on leadership (Bolton, Brunnermeier, and Veldkamp, 2013; Dessein and Santos, 2016; Hermalin, 2012; Rotemberg and Saloner, 2000; Van den Steen, 2005).

Second, CEOs may affect corporate life-cycle development. This idea adds a new wrinkle to studies on how corporate life-cycle affects corporate behaviors (Abernathy and Utterback, 1978; Akcigit and Kerr, 2018; Argente, Lee, and Moreira, 2018; Hoberg and Maksimovic, 2018; Keung, Yang, and Hong, 2014; Klepper, 1996; Rajan, 2012). Corporate life-cycle development may not follow an autonomous and gradual evolution; instead entrenched CEOs may fetter the development of firms’ life-cycle development, which is punctuated by only occasional CEO turnovers. This idea highlights changes in management as drivers of, rather than consequences of, corporate life-cycle development.²

Third, in order to study CEOs of high-tech firms, we need to consider difficult-to-measure variables such as CEOs’ preference for innovation. To that end, I point to recently advanced text analysis techniques as a promising methodology (Antweiler and Frank, 2004; Hansen, McMahon, and Prat, 2014; Hoberg and Phillips, 2010; Tetlock, Saar-Tsechansky, and Macskassy, 2008).

My empirics have three major limitations. First, I have a small sample — 55 CEO turnovers — because I focus on health-related CEO turnovers. This focus addresses some

¹A notable exception is Cronqvist, Heyman, Nilsson, Svaleryd, and Vlachos (2009), who document that entrenched CEOs pay workers higher wages. This study is related to my findings if we view STEM CEOs’ emphasis on innovation as nonpecuniary benefits to inventors.

²Older firms have more professional managers (Keung, Yang, and Hong, 2014). Similarly, at startup firms, venture capital replaces founders with professional managers (Hellman and Puri, 2002).
endogeneity concerns but reduces the study’s sample size and statistical power.

Second, because boards of directors choose new CEOs based on firms’ needs, the types of new CEOs are endogenous. The only complete solution to this problem is randomizing CEOs across firms, which never happens. However, I propose a partial solution — a methodological contribution to CEO studies: in Section 1.4.2, I narrow down a set of possible endogeneity concerns by exploiting the random timing of health-related CEO turnovers. Furthermore, in Section 1.7, I turn this difficulty into an opportunity and study which management styles firms want in CEOs.

Third, because I do not observe inventors’ motives, I cannot completely rule out alternative reasons why inventors leave after CEO turnovers. In Section 1.5.4, I do some indirect tests on two alternative channels: (1) new CEOs lay off unproductive and unprofitable inventors, and (2) inventors leave when new CEOs’ personalities are incompatible with theirs. I find no support for those alternatives — but note that these tests are only indirect and suggestive.

1.2 Information on Inventors

1.2.1 Importance of Inventor Retention

High-tech firms want to retain inventors because inventors are key employees. Furthermore, retaining inventors prevents the leakage of firms’ valuable intellectual property. Because innovation is unpredictable and hard to measure (Holmstrom, 1989), the delivery and transfer of R&D is imperfect. As a result, some of the intellectual property of firms is embodied in their inventors, and departing inventors can take the embodied intellectual property to their new employers (Arrow, 1962; Saxenian, 1996). Many empirical studies confirm this indeed happens (Almeida and Kogut, 1999; Kim and Marschke, 2005; Rosenkopf and Almeida, 2003). Therefore, firms want to retain inventors.

3For example, a suit was brought against Adobe, Apple, eBay, Google, Intel, Intuit, Pixar, and Lucasfilm for allegedly colluding together, from 2005 to 2009, not to hire each other’s R&D workers. These firms settled the case for $415 million in 2015.
1.2.2 Survey of Inventors

I survey inventors in order to understand their preference in jobs and CEOs. Here, I summarize my survey results. See Appendix A for survey questions and methodology.

Figure 1.1A. Survey: Important Job Attributes

These figures show the survey results of 48 inventors who have filed patents in 2013. See Appendix A for survey questions, sample, and distribution method. Each color corresponds to a job or CEO attribute that inventors have ranked. The horizontal axis is the importance value (decreasing in importance from left to right). The vertical axis count the number of corresponding responses.

Figure 1.1B. Survey: Important CEO Attributes

Figure 1.1: Survey Responses

Figure 1.1A shows that inventors rank autonomy to innovate and contribute to society through innovation more important than income. This finding suggests that inventors enjoy working for innovation-oriented firms and may leave firms that switch from innovation to finance, consistent with my findings in Section 1.5.2.
Figure 1.1B shows that inventors prefer CEOs with engineering and science education (Engineer CEOs) to CEOs with business education (Business CEOs), consistent with my findings in Section 1.5.1. Furthermore, inventors value CEOs’ tolerance for failure more than CEOs’ social skills (e.g., understanding, good team worker). This last finding suggests that CEOs’ innovation strategies, not their social skills, are the main channel behind high inventor outflows after CEO turnovers, consistent with my findings in Section 1.5.4.2.

Lastly, I examine how inventors describe their ideal CEOs. Forty-two percent (20/48) explicitly mention CEOs with STEM degrees and R&D experiences, and 29% (14/48) mention long-term perspectives and tolerance for failure. Interestingly, 71% of those responses mentioning long-term perspectives (10/14) also mention CEOs with STEM degrees and R&D experiences. This result is consistent with STEM CEOs promoting early-stage innovation that requires, by definition, long-term perspectives — my finding in Section 1.6.2.

1.3 Data, Sample Construction, and Variables

1.3.1 Data

Patent

The patent data comes from USPTO’s PatentsView database. The PatentsView database is sourced from USPTO-provided text and XML data on published patents with 6.3 million patent application records and 3.5 million inventors for the 1977-2013 period. The dataset contains assignee firm names and locations. I match the patent data with COMPUSTAT and VentureXpert using standardized company and location names. Appendix A explains my matching procedure in detail.

The PatentsView database also includes disambiguated inventor names based on the machine learning algorithm of Li et al. (2014). This allows me to track the career paths of inventors across firms. I say an inventor works at firm $f$ if the inventor’s patent is assigned to firm $f$. For example, Mike Smith files a patent in 2007 with Google, so I say Mike Smith works at Google in year 2007. I define the transition between two firms as occurring when an
inventor files a patent with a new assignee firm. Going back to the example, Mike Smith’s next patent application happens in 2010 with Apple, so I say Mike Smith works at Google until 2009 and starts working at Apple in 2010. As the procedure requires at least two years of patenting to define firm transitions, I remove inventors with only one year of patenting. I can locate all inventors’ employer firms after CEO turnovers as all inventors in my sample have at least two patents after CEO turnovers.

This approach has two limitations. First, I cannot precisely date inventors’ mobility dates. To address this concern, I check the robustness using mid-years between patents as mobility dates. In the example above, this corresponds to Mike Smith moving in year 2008. Second, I miss some mobility events without patenting, so inventors look less mobile than they actually are. Therefore, when interpreting results, I compare changes in inventors’ mobility rates around CEO turnovers to mobility rates before CEO turnovers. If missing mobility events are not correlated with health-related CEO turnovers, this comparison gives the right relative changes.

**CEO Turnovers and CEO Background**

I compile data on health-related CEO turnovers (deaths and terminal illnesses) at U.S. public firms from 1980 to 2010 from two sources. First, I obtain a panel of CEOs of U.S. public firms from 1989 to 2007 from Fee, Hadlock, and Pierce (2013). The authors have compiled the data from Compustat Research Insight CDs. The sample excludes financial firms (SIC codes 6000–6999), utilities (SIC codes 4900–4949), and non-U.S. firms. The sample further excludes firms with less than $10 million in book value of assets (measured in 1990 dollars). Using 10K filings, annual reports, and news reports, the authors have recorded whether CEO turnovers are due to such health-related reasons as deaths and terminal illnesses. Second, I add CEO deaths to the data by searching through obituaries on CEOs at patenting public

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4When an inventor files multiple patents with different assignees in one year or files a patent shared with multiple assignees, the inventor has multiple assignee firms in one year. In that case, I follow the inventor one year before and after and choose the modal assignee firm to be the employer firm. If there are multiple modal firms, I apply the above procedure at a monthly level and choose the assignee firm where the inventor has worked most months during those 3 years.
firms on Factiva and Lexis-Nexis from 1980 to 2010. I hand-collect data on education and career history of CEOs from firms’ proxy statements (DEF14 and DEF14A), BoardEx, college newspapers, FactSet, Lexis-Nexis, and Marquis Who’s Who.

10K Filings and Texts on Finance and Innovation

I obtain the Management’s Discussion and Analysis (MD&A) section of 10K, 10K405, 10KSB, and 10KSB40 filings from the microfiche collection at Harvard Business School’s Baker Library (1977-1993 period) and from the SEC’s EDGAR website (1994-2013 period). MD&A is the section of 10K filings in which management provides an overview of the previous year’s operations and discusses future goals and approaches to new projects. I discard MD&As if they just refer to the Annual Report to Shareholders. I concentrate my analysis on the textual content and remove all tables and exhibits from MD&As.

As explained in detail in Section 3.3.3, I correlate language patterns in the MD&As with those in finance text and those in innovation text. Following Bellstam, Bhagat and Cookson (2017), I use two popular textbooks in finance and innovation: “Corporate Finance: An Introduction” by Welch (2009) and “Managing Innovation: Integrating Technological, Market and Organizational Change” by Tidd, Bessant, and Pavitt (2005). I discard noninformative front and back materials of the textbooks (e.g., publisher information) and keep only the texts in the main bodies (numbered chapters) and the indexes.

To focus on relevant words, I remove all stop words (e.g., “a” and “the”). I also remove the following synonymous words: “firm,” “company,” “organization,” and “institution” because different texts prefer the different, synonymous words. I then convert words into a common linguistic root through stemming so that, for example, “technolog” captures both “technology” and “technological.” Finally, I tabulate the frequency of all single words in the MD&As and textbooks.

Figures 1.2A and 1.2B show the word clouds of the top 50 words in the main bodies.

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5 The full list of stop words and the stemming algorithm can be accessed at http://snowballstem.org/projects.html.
of the finance and innovation textbooks by term frequency-inverse document frequency (TFIDF) values. TFIDF is a statistic that reflects how frequently a word appears in a textbook after adjusting for the fact that some words generally appear more frequently. Figures 1.2C and 1.2D show the word clouds of the top 50 words in the indexes of the textbooks. Clearly, the two textbooks show different word usages. The finance textbook frequently uses finance words like “equiti,” “debt,” “liabil,” and “repurchase” while the innovation textbook frequently uses innovation words like “allianc,” “innov,” “product,” and “technolog.” This variation allows me to compare firms’ language with two different vocabularies.

Figure 1.2: Word Clouds of Textbooks by TF-IDF (Top 50)

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6Formally, TFIDF multiplies Term Frequency (TF) with Inverse Document Frequency (IDF). TF equals the number of times a word appears in a textbook over the total number of words in that textbook. IDF equals the logarithm of the number of the textbooks divided by the number of textbooks where the specific word appears.
Firm Financial Information

I collect firm financial information from COMPUSTAT. All financial variables are inflation-adjusted to 2009 dollars. I winsorize all inflation-adjusted variables at the top and bottom 3% level.

1.3.2 Sample Construction

I construct a sample of inventors at the turnover (firms that have health-related CEO turnovers) and non-turnover firms (firms that look similar but do not have health-related CEO turnovers) by combining the data sources and matching on 4 firm characteristics. Combining the data sources yields 68 health-related CEO turnovers at patenting public firms during the 1980-2010 period. I require the turnover firms to have at least one inventor 3 years before CEO turnovers, resulting in 61 turnovers. I then match each of the turnover firms to a firm that has at least one inventor 3 years before, has similar lagged characteristics, but does not have any health-related CEO turnover 3 years around the time of CEO turnover of the matching turnover firm. The matched non-turnover firms serve as counterfactuals to the turnover firms in the absence of CEO turnovers.

Specifically, I match the turnover firms to the non-turnover firms three years before CEO turnovers. I use genetic matching within calendar year and three-digit SIC industry code strata without replacement. Genetic matching uses a genetic search algorithm to find the optimal balance between matched observations (Diamond and Sekhon, 2013). Intuitively, it maximizes the smallest p-value from the t-test and the Kolmogrov-Smirnov test on the equality of variables between matched observations. By algorithmic design, genetic matching balances matching variables better than previous matching methods such

7Price index data is from the National Income and Product Accounts, Table 1.1.4.

8This restriction may result in overlapping of pre- and post- periods of my difference-in-differences research design. For instance, if a firm does not have a CEO turnover from 2000 to 2006 but has one turnover in 2007, then the firm may be a matched non-turnover firm in 2003 and a turnover firm in 2007. Then the 2004-2006 period becomes both the post-period as the non-turnover firm and the pre-period as the turnover firm. My matched sample does not have such overlap. In other applications, one may require the matched non-turnover firm to have no CEO turnover 6 years around the CEO turnover of the matching turnover firm.
as propensity score matching. I use the following firm characteristics known to affect the mobility of high-tech workers: firm age (years since first appearing in COMPUSTAT), total asset, sales growth, and one-year lagged sales growth (Gomper, Lerner, and Scharfstein, 2005). Finally, I keep 6 years per firm: 3 years around the time of CEO turnover.

**Table 1.1: Balance Table**

<table>
<thead>
<tr>
<th></th>
<th>Turnovers</th>
<th>Non-Turnovers</th>
<th>t-test</th>
<th>Kolmogrov-Smirnov</th>
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</thead>
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<tr>
<td>Firm Age (Years)</td>
<td>18.1</td>
<td>18.2</td>
<td>0.64</td>
<td>1</td>
</tr>
<tr>
<td>Asset (Billion Dollars)</td>
<td>1.99</td>
<td>1.86</td>
<td>0.055</td>
<td>0.59</td>
</tr>
<tr>
<td>Sales Growth</td>
<td>0.13</td>
<td>0.11</td>
<td>0.10</td>
<td>0.082</td>
</tr>
<tr>
<td>Lagged Sales Growth</td>
<td>0.40</td>
<td>0.38</td>
<td>0.11</td>
<td>0.73</td>
</tr>
</tbody>
</table>

Following Diamond and Sekhon (2013), I set the population parameter of evolutionary algorithm to 1000 and bootstrap Kolmogorov-Smirnov test with 1000 samples. P-values of the tests are reported. * p <0.1; ** p < 0.05; *** p < 0.01.

Table 1.1 shows the characteristics of the turnover and non-turnover firms as well as p-values from the t-test and the Komologrov-Smirnov test. The turnover and non-turnover firms are similar in observables as all the p-values are larger than 5%. The total number of CEO turnovers in my matched sample is 55, with 1,057 inventors working at the turnover firms and 2,724 working at the non-turnover firms. Because more inventors work at the non-turnover firms than at the non-turnover firms, I also match on number of inventors and check the robustness (unreported here). Overall, the matched non-turnover firms serve as good counterfactuals to the turnover firms.

Figures 1.3A and 1.3B show the distributions of CEO turnovers across industry (aggregated to two-digit SIC codes) and across years. The six most common industries are high-tech industries: Chemicals (e.g., pharmaceutical firms), Electronics, Measurement Devices (e.g., lab equipment firms), Transportation Equipment (e.g., aircraft firms), Business
Figure 1.3A. CEO Turnovers by Industry

Figure 1.3B. CEO Turnovers by Year

Figure 1.3: CEO Turnover Distribution
Services (e.g., business-to-business software firms), and Computers. CEO turnovers are well spread throughout the period, assuring that few abnormal years do not drive my results.

1.3.3 Firm-Level Variables

Treatment and Post Dummies

I define a treatment indicator variable equal to 1 if health-related CEO turnover happens at the firm and a post indicator variable equal to 1 in the years after CEO turnover. I define the indicator variables at each matched-pair unit. That is, if an inventor appears twice in the data, then the inventor gets two sets of treatment and post dummy variables. For example, if an inventor appears first at a firm with a CEO turnover and later at a different firm without, then the inventor has the treatment variable of 1 in the first CEO turnover unit and that of 0 in the second unit. The inventor also gets two post variables: the first one changing from 0 to 1 in the years after CEO turnover at the inventor’s turnover firm and the second one changing from 0 to 1 in the years after CEO turnover matched to the inventor’s non-turnover firm. Note that the post dummy turns 1 in the post-period for both turnover and non-turnover firms.

CEO Types

Using the career history of new CEOs, I separate 55 CEO turnovers into 12 external and 43 internal hires. A new CEO is an external hire if the CEO’s most recent job is not at the firm. Using the educational background of CEOs, I identify STEM CEOs as CEOs with STEM undergraduate degrees. I exclude business-related STEM degrees such as financial engineering, industrial engineering, and operations research. Out of 55 previous CEOs at the turnover firms, 36 are STEM CEOs, 14 are non-STEM, and 5 are undefined due to insufficient data. Out of 55 new CEOs at the turnover firms, 22 are STEM, 31 are non-STEM, and 2 are undefined. Out of 55 CEOs at the non-turnover firms, 39 are STEM and 16 are non-STEM. Seventy percent of previous STEM CEOs (25/36) are non-founders; 64% of non-STEM CEOs (39/61) hold economics and business undergraduate degrees.
### Table 1.2: CEO Turnover Types

<table>
<thead>
<tr>
<th></th>
<th>Number of Turnovers</th>
</tr>
</thead>
<tbody>
<tr>
<td>External</td>
<td>12</td>
</tr>
<tr>
<td>Internal</td>
<td>43</td>
</tr>
<tr>
<td>STEM→STEM</td>
<td>21</td>
</tr>
<tr>
<td>STEM→nonSTEM</td>
<td>15</td>
</tr>
<tr>
<td>nonSTEM→STEM</td>
<td>0</td>
</tr>
<tr>
<td>nonSTEM→nonSTEM</td>
<td>14</td>
</tr>
</tbody>
</table>

A CEO turnover belongs to *External (Internal)* if the new CEO is externally hired (internally promoted). A CEO turnover belongs to *X → Y* if the previous CEO is of type X and the new CEO is of type Y. For example, a turnover belongs to *STEM → nonSTEM* if the previous CEO is a STEM CEO and the new CEO is a non-STEM CEO. STEM CEOs are CEOs with STEM undergraduate degrees. non-STEM CEOs are CEOs without STEM degrees.
Table 1.2 shows the distribution of turnovers by CEO types. A CEO turnover belongs to $X \rightarrow Y$ if the previous CEO is of type $X$ and the new CEO is of type $Y$. For example, a turnover belongs to $STEM \rightarrow nonSTEM$ if the previous CEO is a STEM CEO and the new CEO is a non-STEM CEO. One fact stands out from the table: in my sample, STEM CEOs never succeed non-STEM CEOs, although the reverse is common. I discuss this finding in light of corporate life-cycle theories in Section 1.6.

**Text-Based Measures of Managerial Focus**

I measure the managerial focus on finance and innovation using text analysis. Specifically, I measure the year-on-year similarities between the MD&A section of 10K filings and the textbooks on finance and innovation, using two different similarity measures: cosine similarity and Jaccard similarity. I compute the two measures in the following ways: let $T_1$ and $T_2$ be the sets of words used in documents 1 and 2, respectively. Define $t_i$ be the $i$th element in $T_1 \cup T_2$. Define the term-frequency vector $TF_1$ that counts the frequency of words for document 1 with its $i$th entry accounting for the frequency of term $t_i$. Define $TF_2$ similarly. The cosine similarity between documents 1 and 2 is:

$$Cosine_{1,2} = \frac{TF_1 \cdot TF_2}{||TF_1|| \times ||TF_2||}$$

where $A \cdot B$ is the dot product of $A$ and $B$, and $||A||$ is the Euclidean norm of $A$. The Jaccard similarity is:

$$Jaccad_{1,2} = \frac{|T_1 \cap T_2|}{|T_1 \cup T_2|}$$

where $|A|$ denotes the number of elements in $A$. Both similarity measures range from 0 (the least similar) to 1 (the most similar). For an illustration on constructing the measures, see Appendix A.

For each MD&A, I compute the cosine and Jaccard similarity measures with respect to the main body of the finance textbook and denote $Cos\ Fin$ and $Jac\ Fin$, respectively. For robustness, I compute the cosine and Jaccard similarity measures with respect to the index
of the finance textbook and denote $Cos\ Fin\ Index$ and $Jac\ Fin\ Index$. I define $Cos\ Innov$, $Jac\ Innov$, $Cos\ Innov\ Index$, and $Jac\ Innov\ Index$ similarly with the innovation textbook.

**Firm’s Propensity to Cite Academic Research**

Following Fleming and Sorenson (2004), I compute the share of citations made to 10,427 journals in the Science Citation Index Expanded List for each patent. My first measure $\%Academic_{ft}$ averages the share of academic citations across firm $f$’s patents filed in year $t$. I construct another measure that adjusts for time and technology class because citation rates vary over time and across technologies. For each application year and technology class stratum, I randomly sample five patents and average their academic shares. A patent’s adjusted share equals its share of academic citations minus the average academic share of the matching five patents. My second measure $Adj.\%Academic_{ft}$ averages the adjusted share of academic citations across firm $f$’s patents filed in year $t$.

**Firm’s Innovation Quality**

I use patent citation-based measures of firms’ innovation quality. Empirical studies show that patent citations measure innovation quality and economic importance (Hall, Jaffe, and Trajtenberg, 2005; Schankerman and Pakes, 1986). I only consider citations that occur during a three-year window following the application year because patents applied at the end of the sample period have less time to receive citations than those applied at the beginning. My first measure, $Total\ Cites_{ft}$, equals the log of 1 plus the total citation count on firm $f$’s patents filed in year $t$. I define another measure to adjust for time and technology class variations. A patent’s adjusted citation count equals the patent’s citation count minus the average citation count of all the patents with the same application year and technology class. My second measure, $Total\ Adj.\ Cites_{ft}$, equals the log of 1 plus the total adjusted citation

---

9 Instead of using all the patents in the same class and year, I choose five matching patents for the adjustment because getting patent-to-academic-journal citations is time-consuming. Unlike patent-to-patent citations, patent-to-academic-journal citations come from an unstructured text file that requires manual de-abbreviation of journal names.
count on firm $f$’s patents filed in year $t$.

1.3.4 Inventor-Level Variables

Inventor’s Technological Specialty

I assign one of six technology classes in which the inventor has filed the most patents by that year as the inventor’s major class. This variable helps control for time-invariant labor market heterogeneity for different technology specialists. The six technology classes come from Hall, Jaffe, and Trajtenberg (2001) and include Chemical, Computer and Communications, Drugs and Medical, Electrical and Electronics, Mechanical, and Others. I use the measure three years before CEO turnovers in order to avoid any effect of CEO turnovers on innovation policies. For example, a computer scientist remains a computer scientist throughout my analysis, even when a new CEO orders the computer scientist to work on electronics patents.

1.3.5 Summary Statistics

Table 1.3 shows the summary statistics. To get a better sense of my sample firms, I compare them to the universe of COMPUSTAT firms after weighting the COMPUSTAT universe to have the same distribution of calendar years and three-digit SIC industry codes as my sample firms. Relative to the weighted COMPUSTAT universe, my median sample firm is older (55th percentile in age), is smaller (41st percentile in asset size), grows slower (42nd percentile in sales growth and 47th percentile in lagged sales growth), spends less on R&D (28th percentile in R&D expenses), and produces more patents (69th percentile in patents filed).

1.4 Research Design

I implement a difference-in-differences design that compares inventors at the turnover firms (firms that have health-related CEO turnovers) to inventors at the matched non-turnover firms (firms that look similar but do not have health-related CEO turnovers). Let me
Table 1.3: Summary Statistics

<table>
<thead>
<tr>
<th></th>
<th>N</th>
<th>Mean</th>
<th>SD</th>
<th>25th</th>
<th>Median</th>
<th>75th</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Inventor Level Variables</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Total Cites</td>
<td>3,781</td>
<td>1.958</td>
<td>1.29</td>
<td>1.10</td>
<td>1.95</td>
<td>2.83</td>
</tr>
<tr>
<td>Adj. Total Cites</td>
<td>3,781</td>
<td>1.27</td>
<td>1.64</td>
<td>0.37</td>
<td>1.37</td>
<td>2.40</td>
</tr>
<tr>
<td>Avg. Cites</td>
<td>3,781</td>
<td>0.46</td>
<td>0.82</td>
<td>0.27</td>
<td>0.43</td>
<td>0.63</td>
</tr>
<tr>
<td>Adj. Avg. Cites</td>
<td>3,781</td>
<td>0.32</td>
<td>1.34</td>
<td>−0.36</td>
<td>0.42</td>
<td>1.13</td>
</tr>
<tr>
<td>%Academic</td>
<td>3,781</td>
<td>−0.004</td>
<td>0.094</td>
<td>0.00</td>
<td>0.00</td>
<td>0.033</td>
</tr>
<tr>
<td>Adj.%Academic</td>
<td>3,781</td>
<td>0.004</td>
<td>0.094</td>
<td>0.034</td>
<td>0.00</td>
<td>0.00</td>
</tr>
<tr>
<td>Any Academic</td>
<td>3,781</td>
<td>0.34</td>
<td>NA</td>
<td>0</td>
<td>0</td>
<td>1</td>
</tr>
<tr>
<td>%FirmCites</td>
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<td>0.083</td>
<td>0.12</td>
<td>0</td>
<td>0.034</td>
<td>0.12</td>
</tr>
<tr>
<td>Adj.%FirmCites</td>
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<td>0.079</td>
<td>0.12</td>
<td>−0.002</td>
<td>0.031</td>
<td>0.12</td>
</tr>
<tr>
<td>YearsWorkingTogether</td>
<td>3,781</td>
<td>4.061</td>
<td>4.17</td>
<td>2</td>
<td>4</td>
<td>11</td>
</tr>
<tr>
<td><strong>Firm Level Variables</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Firm Age (Years)</td>
<td>110</td>
<td>18.14</td>
<td>11.48</td>
<td>9</td>
<td>13</td>
<td>26.25</td>
</tr>
<tr>
<td>Asset (Billion Dollars)</td>
<td>110</td>
<td>1.92</td>
<td>6.25</td>
<td>0.074</td>
<td>0.20</td>
<td>1.43</td>
</tr>
<tr>
<td>Sales Growth</td>
<td>110</td>
<td>0.12</td>
<td>0.37</td>
<td>−0.041</td>
<td>0.034</td>
<td>0.17</td>
</tr>
<tr>
<td>Lagged Sales Growth</td>
<td>110</td>
<td>0.39</td>
<td>1.03</td>
<td>−0.063</td>
<td>0.14</td>
<td>0.40</td>
</tr>
<tr>
<td><strong>Firm-Year Level Variables</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Fin Cos</td>
<td>583</td>
<td>0.33</td>
<td>0.093</td>
<td>0.28</td>
<td>0.34</td>
<td>0.40</td>
</tr>
<tr>
<td>Fin Jac</td>
<td>583</td>
<td>0.086</td>
<td>0.056</td>
<td>0.045</td>
<td>0.074</td>
<td>0.13</td>
</tr>
<tr>
<td>Fin Index Cos</td>
<td>583</td>
<td>0.26</td>
<td>0.074</td>
<td>0.20</td>
<td>0.26</td>
<td>0.30</td>
</tr>
<tr>
<td>Fin Index Jac</td>
<td>583</td>
<td>0.15</td>
<td>0.062</td>
<td>0.12</td>
<td>0.16</td>
<td>0.21</td>
</tr>
<tr>
<td>Innov Cos</td>
<td>583</td>
<td>0.31</td>
<td>0.11</td>
<td>0.26</td>
<td>0.33</td>
<td>0.39</td>
</tr>
<tr>
<td>Innov Jac</td>
<td>583</td>
<td>0.086</td>
<td>0.054</td>
<td>0.047</td>
<td>0.075</td>
<td>0.13</td>
</tr>
<tr>
<td>Innov Index Cos</td>
<td>583</td>
<td>0.17</td>
<td>0.064</td>
<td>0.13</td>
<td>0.18</td>
<td>0.21</td>
</tr>
<tr>
<td>Innova Index Jac</td>
<td>583</td>
<td>0.082</td>
<td>0.025</td>
<td>0.076</td>
<td>0.088</td>
<td>0.10</td>
</tr>
<tr>
<td>log(1+R&amp;D) (Log Million Dollars)</td>
<td>513</td>
<td>2.56</td>
<td>1.73</td>
<td>1.11</td>
<td>2.39</td>
<td>3.75</td>
</tr>
<tr>
<td>R&amp;D/Asset</td>
<td>513</td>
<td>0.051</td>
<td>0.044</td>
<td>0.017</td>
<td>0.035</td>
<td>0.076</td>
</tr>
<tr>
<td>log(1+Patents)</td>
<td>630</td>
<td>3.21</td>
<td>1.76</td>
<td>1.94</td>
<td>2.94</td>
<td>4.00</td>
</tr>
<tr>
<td>TotalCites</td>
<td>630</td>
<td>3.89</td>
<td>1.95</td>
<td>2.49</td>
<td>3.76</td>
<td>5.02</td>
</tr>
<tr>
<td>TotalAdj.Cites</td>
<td>630</td>
<td>3.22</td>
<td>1.90</td>
<td>1.94</td>
<td>3.16</td>
<td>4.20</td>
</tr>
<tr>
<td>%Academic</td>
<td>630</td>
<td>0.061</td>
<td>0.12</td>
<td>0.002</td>
<td>0.021</td>
<td>0.056</td>
</tr>
<tr>
<td>Adj. %Academic</td>
<td>630</td>
<td>0.005</td>
<td>0.076</td>
<td>−0.015</td>
<td>−0.001</td>
<td>0.002</td>
</tr>
</tbody>
</table>
illustrate my research design with the following OLS regression equation:

$$\text{Leave}_{ift} = \{FE\} + \beta_1 \text{Post}_{ft} + \beta_2 \text{Post}_{ft} \times \text{Treated}_f + \epsilon_{ift}$$  \hspace{1cm} (1.1)

$i$ indexes inventor, $f$ indexes firm, and $t$ indexes calendar year. $\text{Leave}_{ift}$ is a dummy variable equal to 1 if inventor $i$ leaves firm $f$ in year $t$. $\{FE\}$ is a set of fixed effects: firm fixed effect, calendar-year fixed effect, inventor’s state of residence fixed effect, and inventor’s technology class fixed effect. $\text{Post}_{ft}$ is an indicator variable equal to 1 in the years after health-related CEO turnover. $\text{Post}_{ft}$ is defined at each matched pair unit; $\text{Post}_{ft}$ equals 1 for both a turnover firm and its matched non-turnover firm in the years after the health-related CEO turnover. $\text{Treated}_f$ is an indicator variable equal to 1 if health-related CEO turnover happens at firm $f$. I drop an inventor from the sample one year after he or she leaves because the inventor is no longer at hazard. To allow for arbitrary dependence of the error terms within firms, I cluster standard errors at the firm level. $\beta_2$ is the coefficient of interest, and the positive value means inventors are more likely to leave after CEO turnovers.

My research design addresses three empirical challenges: endogenous CEO turnovers, time-varying industry conditions, and sample composition changes. First, to avoid a spurious relationship between CEO turnovers and inventor outflows, I need health-related CEO turnovers that are exogenous to firm conditions.\(^{10}\) Otherwise, bad firm conditions can cause both firing of a CEO (Jenter and Lewellen, 2017; Warner, Watts, and Wruck, 1988) and employee exodus (Baghai, Silva, Thell, and Vig, 2017; Benmelech, Bergman, and Seru, 2011), resulting in a spurious relationship between CEO turnovers and inventor outflows. Using health-related CEO turnovers addresses this issue.

Second, to account for time-varying industry conditions, I need a set of counterfactual firms that are similar to the turnover firms but do not have CEO turnovers. Without

\(^{10}\)For three firms that experience multiple CEO turnovers in my sample, I treat these turnover events as if they have happened at different firms and assign different firm fixed effects.

\(^{11}\)Health-related CEO turnovers have been widely used for identification. See, for example, Bennedsen, Perez-Gonzalez, and Wolfenzon (2010); Fee, Hadlock, and Pierce (2013); and Quigley, Crossland, and Campbell (2017).
counterfactual firms, I use an event study design which implicitly compares inventors at a turnover firm before CEO turnovers to those at the same firm after CEO turnovers. Such comparison is inappropriate. For example, for an IT firm with a CEO turnover in 2000, an event study design compares inventors at the firm during the dot-com bubble to other inventors at the same firm after that bubble had burst. But the comparison is invalid because these two groups of inventors face very different labor market conditions. The difference-in-differences design deals with this matter by comparing inventors at turnover firms to inventors at similar non-turnover firms within the same year.

Third, to account for sample composition changes, I control for the passage of time by including $Post_{f t}$. This problem arises because inventors leave my sample when they leave firms. When inventors leave firms, they are no longer at hazard of leaving, so they are dropped from the sample — a standard setup in hazard analysis. But this setup introduces a selection bias: inventors who have already left and inventors who have not left yet have different mobility patterns. This selection bias cannot be controlled for by calendar-year fixed effects because different firms experience CEO turnovers in different calendar years (illustration below).

For illustration, I take two non-turnover firms, create one copy each, and label the copies as turnover firms. Suppose these turnover firms each experience one imaginary CEO turnover, one in 2000 and the other in 2001. Suppose the equation (1) does not include $Post_{f t}$:

$$\text{Leave}_{i f t} = FE_f + FE_t + \beta_1 Post_{f t} \times \text{Treated}_f + \epsilon_{if t}$$

where $FE_f$ and $FE_t$ are firm and calendar-year fixed effects, respectively. Because turnover firms and non-turnover firms are identical firms, they should have the same firm fixed effects. This is because $Post_{f t} \times \text{Treated}_f$ gives turnover firms one more degree of freedom relative to non-turnover firms. For example, if $\beta_1$ is negative, then turnover firms must have more positive firm fixed effects than non-turnover firms to offset the negative value. This further complicates the interpretation of $\beta_1$ and highlights the importance of including $Post_{f t}$.

---

12 If I estimate the above equation, turnover firms and non-turnover firms will have different fixed effects. This is because $Post_{f t} \times \text{Treated}_f$ gives turnover firms one more degree of freedom relative to non-turnover firms. For example, if $\beta_1$ is negative, then turnover firms must have more positive firm fixed effects than non-turnover firms to offset the negative value. This further complicates the interpretation of $\beta_1$ and highlights the importance of including $Post_{f t}$. 

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propensity to leave. Because these fixed effects are only the averages, they cannot perfectly
match inventors’ mobility patterns. Post_{ft} \times Treated_{f} gives turnover firms one more degree
of freedom to match their inventors’ mobility patterns. If inventors who leave earlier are
more mobile than inventors who leave later, then all firms have lower inventors’ propensity
to leave in the post-period than in the pre-period. Post_{ft} \times Treated_{f} captures this effect
only for the turnover firms and assigns a negative value to \beta_{1}. But the turnover and non-
turnover firms are identical copies, and no real CEO turnovers exist! Including Post_{ft} as in
the equation (1) fixes this problem; Post_{ft} captures this effect for both turnover and non-
turnover firms and frees Post_{ft} \times Treated_{f} to capture the CEO turnover effect on inventor
outflows (in this example, 0). Note that Post_{ft} is not collinear with the calendar-year fixed
effects because the year 2000 turns Post_{ft} on 1 for one turnover firm but not on 1 for the
other turnover firm.

The first three columns of Table 1.4 estimate the equation (1). Across all specifications,
the coefficient on Post_{ft} \times Treated_{f} is stable at 0.04 and statistically significant. To translate
the magnitude to a meaningful unit, I compare it to inventor outflows at the turnover firms
before CEO turnovers. During the three years before CEO turnovers, 13% of inventors leave
the turnover firms. So the treatment effect is \frac{0.04}{0.13} = 31\% of the baseline outflow rate. In
other words, inventors are 31\% more likely to leave after CEO turnovers.

This result is robust to estimating by conditional logit, matching on propensity scores,
and matching on number of inventors.

1.4.1 Parallel Trends

The key assumption for my difference-in-differences design is that health-related CEO
turnovers are exogenous conditional on the covariates in the model. This assumption
implies that inventor outflows at the turnover and non-turnover firms would follow parallel
trends if no health-related CEO turnover occurred at the turnover firms.

I validate this assumption by showing that inventor outflows at the turnover and non-
turnover firms follow parallel trends before CEO turnovers. Specifically, I replace Post_{ft}
Table 1.4: Inventor Outflows and CEO Turnovers

<table>
<thead>
<tr>
<th></th>
<th>Leave</th>
<th>Leave</th>
<th>Leave</th>
<th>Leave</th>
<th>Leave</th>
<th>Leave</th>
</tr>
</thead>
<tbody>
<tr>
<td>Post</td>
<td>-0.025**</td>
<td>-0.024**</td>
<td>-0.024**</td>
<td>0.066***</td>
<td>0.053***</td>
<td>0.052***</td>
</tr>
<tr>
<td></td>
<td>(0.010)</td>
<td>(0.011)</td>
<td>(0.011)</td>
<td>(0.023)</td>
<td>(0.008)</td>
<td>(0.008)</td>
</tr>
<tr>
<td>Post X Treated</td>
<td>0.041***</td>
<td>0.042***</td>
<td>0.042***</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.013)</td>
<td>(0.013)</td>
<td>(0.013)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Post X STEM→STEM</td>
<td></td>
<td></td>
<td></td>
<td>-0.036</td>
<td>-0.019</td>
<td>-0.020</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>(0.038)</td>
<td>(0.031)</td>
<td>(0.031)</td>
</tr>
<tr>
<td>Post X STEM→nonSTEM</td>
<td></td>
<td></td>
<td></td>
<td>0.050*</td>
<td>0.051**</td>
<td>0.052**</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>(0.029)</td>
<td>(0.021)</td>
<td>(0.021)</td>
</tr>
<tr>
<td>Post X nonSTEM→nonSTEM</td>
<td></td>
<td></td>
<td></td>
<td>0.021</td>
<td>0.038</td>
<td>0.039</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>(0.030)</td>
<td>(0.025)</td>
<td>(0.026)</td>
</tr>
</tbody>
</table>

Observations: 16,787 16,787 16,787 15,716 15,716 15,716
Adjusted R²: 0.048 0.085 0.085 0.031 0.063 0.064
Firm FE: Y Y Y Y Y Y
Year FE: Y Y Y Y Y Y
Inventor State FE: N Y Y N Y Y
Inventor Tech FE: N N Y N N Y

*p < 0.1; ** p < 0.05; *** p < 0.01.
with a full set of leads and lags around CEO turnovers in the equation (1) and estimate the following OLS regression equation:

\[
\text{Leave}_{i,ft} = \{FE\} + \sum_{k=-2}^{3} \beta_{0k}^k \mathbb{I}(L_{ft} = k) + \sum_{k=-2}^{3} \beta_{1k}^k \mathbb{I}(L_{ft} = k) \times \text{Treated}_f + \epsilon_{ift} \tag{1.2}
\]

where \(\mathbb{I}(L_{ft} = k)\) is a dummy variable equal to 1 if CEO turnover happens \(k\) years after the calendar year \(t\). The parallel trends hold if \(\beta_{1k}^k = 0\) for \(k < 0\). Figure 1.4 plots the estimates of \(\beta_{1k}^k\) with vertical whiskers denoting their 95% confidence intervals. \(\beta_{1k}^k\)'s are statistically insignificant and small in magnitudes for \(k < 0\). The result means inventor outflows at the turnover and non-turnover firms follow the parallel trends before CEO turnovers.

![Figure 1.4: Dynamic Treatment Effects](image)

This result is robust to using the mid-year between patents to define mobility dates.

### 1.4.2 Endogenous Selection of New CEOs

My research design cannot address the endogenous selection of new CEOs. Boards of directors choose new CEOs based on firms’ needs, even when previous CEOs depart exogenously. For example, a board wants to lay off inventors, reduce firms’ innovation activities, and build relationships with banks. Suppose that building banking relationships
affects neither inventors nor innovation activities. Also suppose that the board decides on layoffs and innovation activities while the CEO decides on banking relationships. The board cannot directly build banking relationships, but, after the current CEO dies, it can hire a new CEO who is known to build banking relationships. In this example, the new CEO affects neither inventors nor innovation but merely coincides with the board’s decisions to lay off inventors and to reduce innovation. This problem is prevalent in existing CEO studies, and there is no clean solution besides a random allocation of CEOs across firms.

Two remarks are in order. First, this endogeneity is actually needed in Section 1.6, in which I examine the choice of new CEOs; thus this endogeneity is not a weakness of the entire study. Second, I can exploit the random timing of CEO turnovers and further narrow down a set of possible endogeneity stories — a methodological contribution to CEO studies. Note that not only are health-related CEO turnovers exogenous and random, but the timing of those turnovers are also. Going back to the example above, the board can lay off inventors and reduce innovation activities a couple years before the CEO’s death. Because the exact timing of the death is unanticipated, it must be uncorrelated with the timing of the board’s decisions. But I show that changes in inventor outflows and corporate strategies happen sharply after health-related CEO turnovers. Now the endogeneity stories must be either that the board suddenly revises its plans after the CEO’s death or that the board has new plans but must wait until the new CEO comes in. In these stories, the interpretation is more difficult and nuanced, but some CEO effect is present. Either the dying CEO prevents the board from thinking about or implementing new plans, or the new CEO is hired to do something (banking relationships) that is complementary to the board’s plans (layoffs and reduced innovation). Therefore, examining the lack of changes before CEO turnovers is essential to identifying some CEO effect. This equates to testing parallel pre-trends in a difference-in-differences context.

In sum, my research design addresses three empirical challenges: endogenous CEO turnovers, time-varying industry conditions, and sample composition changes. Like other CEO studies, my study cannot fully address the endogenous selection of new CEOs. But
I highlight that exploiting the random timing of CEO turnovers narrows down a set of possible endogeneity stories.

1.5 STEM CEOs Retain Inventors.

I highlight a benefit of STEM CEOs: inventor retention. Text analysis suggests that STEM CEOs’ preference for innovation retains inventors.

1.5.1 Inventors Leave Particularly When non-STEM CEOs Succeed STEM CEOs.

The last three columns of Table 1.4 estimate the following OLS regression equation:

\[
\text{Leave}_{ift} = \{FE\} + \beta_1 \text{Post}_{ft} + \beta_2 \text{Post}_{ft} \times \text{STEM} \rightarrow \text{STEM}_f \\
+ \beta_3 \text{Post}_{ft} \times \text{STEM} \rightarrow \text{nonSTEM}_f + \beta_4 \text{Post}_{ft} \times \text{nonSTEM} \rightarrow \text{nonSTEM}_f + \epsilon_{ift}
\]  

(1.3)

where \( \text{Leave}_{ift} \) is a dummy variable equal to 1 if inventor \( i \) leaves firm \( f \) in year \( t \). \( X \rightarrow Y_f \) is a dummy variable equal to 1 if CEO turnover happens at firm \( f \), the previous CEO is of type \( X \), and the new CEO is of type \( Y \). For example, \( \text{STEM} \rightarrow \text{nonSTEM}_f \) equals 1 when firm \( f \) replaces a STEM CEO with a non-STEM CEO. \( X \rightarrow Y_f \) equals 0 for the non-turnover firms. I drop an inventor from the sample one year after he or she leaves because the inventor is no longer at hazard. \( \beta_2 \), \( \beta_3 \), and \( \beta_4 \) are coefficients of interest that measure how different types of CEO turnovers affect the probability of an inventor leaving.

Across different specifications, the coefficient on \( \text{STEM} \rightarrow \text{nonSTEM}_f \) has the largest magnitudes and it alone is statistically significant. This result means that inventors leave particularly when non-STEM CEOs succeed STEM CEOs.

One caveat is that the coefficients on \( \text{STEM} \rightarrow \text{STEM}_f, \text{STEM} \rightarrow \text{nonSTEM}_f, \) and \( \text{nonSTEM} \rightarrow \text{nonSTEM}_f \) are statistically indistinguishable due to small sample size and large standard errors. My sample is small by construction: I include only health-related CEO turnovers for causality. In the future, when more health-related CEO turnovers will
have happened, I can re-examine the test with more power. For now, I proceed with the interpretation that inventors leave particularly when non-STEM CEOs succeed STEM CEOs.

The last three columns of Table 1.4 are robust to controlling for firm financial conditions such as EBITDA and sales. Therefore, the results are unlikely to reflect firms in bad firm conditions hiring non-STEM CEOs to prepare for bankruptcy and restructuring and laying off inventors.

1.5.2 The Channel

In this section, I document empirical support for the following channel: non-STEM CEOs emphasize business aspects such as finance while STEM CEOs emphasize innovation, which drives out inventors. The disagreement between Ray Kassar and the founders of Activision exemplifies the channel. Ray Kassar was a non-STEM CEO of Atari, a video game firm. At Atari, he emphasized efficient manufacturing and neglected product development. He once said, “[product developers] are no more important to that game than the guy on the assembly line who puts it together.” Disagreeing with Kassar’s business focus, product developers left Atari to start their own video game firm Activision.

To test this channel, I first examine whether certain CEOs use more finance and innovation words in the MD&A section of firms’ 10Ks. I then examine whether inventors leave in response to the finance and innovation word uses.

After non-STEM CEOs Succeed STEM CEOs, Firms Use More Finance Words and Less Innovation Words.

The first four columns of Table 1.5 estimate the following firm-year level OLS regression:

\[
\begin{align*}
\text{Fin. Word}_{ft} = \{FE\} + \beta_1 \text{Post}_{ft} + \beta_2 \text{Post}_{ft} \times STEM \rightarrow STEM_f \\
+ \beta_3 \text{Post}_{ft} \times STEM \rightarrow nonSTEM_f + \beta_4 \text{Post}_{ft} \times nonSTEM \rightarrow nonSTEM_f + \epsilon_{ft}
\end{align*}
\]

(1.4)
Across different similarity measures, all but one estimates of the coefficient on $STEM \rightarrow nonSTEM_f$ are positive and statistically significant (one at 5% level and two at 10%). The magnitudes are economically large: 0.47, 0.5, 0.65, and 0.27 standard deviation of the similarity measures. The other two CEO turnover types have statistically insignificant and small coefficients. Therefore, firms use more finance words after non-STEM CEOs succeed STEM CEOs.

The last four columns of Table 1.5 repeat the exercise with $Innov\cdot Word_{ft}$ in place of $Fin\cdot Word_{ft}$, where $Innov\cdot Word_{ft}$ measures the similarity between the MD&A section of firms’ 10Ks and the innovation textbook. Across different similarity measures, all but one estimates of the coefficient on $STEM \rightarrow nonSTEM_f$ are negative and statistically significant. The magnitudes are economically large: 0.76, 0.09, 0.9, and 1 standard deviation of the similarity measures. The other two CEO turnover types have statistically insignificant and small coefficients. Therefore, firms use less innovation words after non-STEM CEOs succeed STEM CEOs.

These Word Changes Happen Suddenly after non-STEM CEOs Succeed STEM CEOs.

I show that firms use more finance words and less innovation words suddenly after non-STEM CEOs succeed STEM CEOs. I replace $Post_{ft}$ with a full set of leads and lags around CEO turnovers in the equation (4) and estimate the following OLS regression equation:

$$Cos_{ft} = \{FE\} + \sum_{k=-2}^{3} \beta_{k}^{1} \mathbb{I}(L_{ft} = k) + \sum_{k=-2}^{3} \beta_{k}^{2} \mathbb{I}(L_{ft} = k) \times STEM \rightarrow STEM_{f} + \sum_{k=-2}^{3} \beta_{k}^{3} \mathbb{I}(L_{ft} = k) \times STEM \rightarrow nonSTEM_{f} + \sum_{k=-2}^{3} \beta_{k}^{4} \mathbb{I}(L_{ft} = k) \times nonSTEM \rightarrow nonSTEM_{f} + \epsilon_{ft} \tag{1.5}$$

where $Cos_{ft}$ measures the cosine similarity between firm $f$’s MD&A in year $t$ and the main body of a given textbook. The left panel uses the finance textbook while the right uses the innovation textbook. $\mathbb{I}(L_{ft} = k)$ is a dummy variable equal to one if CEO turnover happens
Table 1.5: Word Uses and CEO Turnover Types

<table>
<thead>
<tr>
<th>Dependent variable: Similarity to Textbooks</th>
<th>Finance Textbook</th>
<th>Innovation Textbook</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Cos Fin</td>
<td>Jac Fin</td>
</tr>
<tr>
<td>Post</td>
<td>-0.005</td>
<td>0.003</td>
</tr>
<tr>
<td></td>
<td>(0.010)</td>
<td>(0.006)</td>
</tr>
<tr>
<td>Post X STEM→STEM</td>
<td>0.004</td>
<td>0.015</td>
</tr>
<tr>
<td></td>
<td>(0.016)</td>
<td>(0.012)</td>
</tr>
<tr>
<td>Post X STEM→nonSTEM</td>
<td>0.044**</td>
<td>0.028*</td>
</tr>
<tr>
<td></td>
<td>(0.020)</td>
<td>(0.015)</td>
</tr>
<tr>
<td>Post X nonSTEM→nonSTEM</td>
<td>0.003</td>
<td>0.015</td>
</tr>
<tr>
<td></td>
<td>(0.021)</td>
<td>(0.014)</td>
</tr>
</tbody>
</table>

Observations: 583 583 583 583 583 583 583 583
Adjusted R²: 0.682 0.678 0.584 0.645 0.555 0.648 0.536 0.587
Firm FE: Y Y Y Y Y Y Y Y
Year FE: Y Y Y Y Y Y Y Y

* p <0.1; ** p < 0.05; *** p < 0.01.
$k$ years after the calendar year $t$.

![Figure 1.5A. STEM to STEM](image)

![Figure 1.5B. STEM to non-STEM](image)

![Figure 1.5C. non-STEM to non-STEM](image)

![Figure 1.5D. STEM to STEM](image)

![Figure 1.5E. STEM to non-STEM](image)

![Figure 1.5F. non-STEM to non-STEM](image)

**Figure 1.5: Word Uses Around CEO Turnovers**

Figures 1.5 plot $\beta_k^2$, $\beta_{k'}^3$, and $\beta_{k'}^4$ with vertical whiskers denoting their 95% confidence intervals. Figures 1.5B and 1.5E show that, before non-STEM CEOs succeed STEM CEOs, firms use a similar amount of finance and innovation words as before. But after non-STEM CEOs succeed STEM CEOs, firms use more finance words and less innovation words than before. All the other CEO turnovers show no changes in word uses throughout the entire period.

The sudden changes in word uses after non-STEM CEOs succeed STEM CEOs suggests some CEO effect. The sudden increase means either boards of directors suddenly decide to switch from finance-oriented strategies to innovation-oriented strategies after STEM CEOs die, or the boards do not want to implement the new strategies until non-STEM CEOs come in. The first explanation suggests that the departure of STEM CEOs triggers the revision in strategies, while the second suggests the arrival of non-STEM CEOs facilitates the adoption
of new strategies. Either way suggests some CEO effect.

**Inventors Leave when Firms Use More Finance Words and Less Innovation Words after CEO Turnovers.**

The first four columns of Table 1.6 estimate the following inventor-year level OLS regression equation:

$$\text{Leave}_{it} = (\text{FE}) + \beta_1 \text{Post}_t + \beta_2 \text{Post}_t \times \text{Treated}_f + \beta_3 \text{Post}_t \times \Delta \text{Fin}.\text{Word}_f$$
$$+ \beta_4 \text{Post}_t \times \text{Treated}_f \times \Delta \text{Fin}.\text{Word}_f + \epsilon_{it}$$

(1.6)

where $\Delta \text{Fin}.\text{Word}_f$ is the three-year post-period average of the similarity measure between firm $f$’s MD&A and the finance textbook minus its three-year pre-period average. The coefficient of interest is $\beta_4$. The positive value indicates inventors are more likely to leave when firms use more finance words after CEO turnovers.

Across different similarity measures, the coefficient on $\text{Post}_t \times \text{Treated}_f \times \Delta \text{Fin}.\text{Word}_f$ is positive and statistically significant (one at 10% and three at 5% levels). The magnitudes are economically large. If firms increase finance words by one standard deviation of $\text{Fin}.\text{Word}_f$ after CEO turnovers, then inventor outflows increase by 10%, 37%, 3%, and 37%, respectively. Therefore, inventors are more likely to leave when firms use more finance words after CEO turnovers.

The last four columns of Table 1.6 repeat the exercise with $\Delta \text{Innov}.\text{Word}_f$ in place of $\Delta \text{Fin}.\text{Word}_f$, where $\Delta \text{Innov}.\text{Word}_f$ is the three-year post-period average of the similarity measure between firm $f$’s MD&A and the innovation textbook minus its three-year pre-period average. Across different similarity measures, the coefficient on $\text{Post}_t \times \text{Treated}_f \times \Delta \text{Innov}.\text{Word}_f$ is negative and statistically significant (two at 10% and two at 5% levels). The magnitudes are economically large. If firms decrease innovation words by one standard deviation of $\text{Innov}.\text{Word}_f$ after CEO turnovers, then inventor outflows increase by 25%, 33%,
Table 1.6: Inventor Outflows and Changes in Word Uses

<table>
<thead>
<tr>
<th></th>
<th>Finance Textbook</th>
<th></th>
<th>Innovation Textbook</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Cos Fin</td>
<td>Jac Fin</td>
<td>Cos Fin Index</td>
<td>Jac Fin Index</td>
</tr>
<tr>
<td>Post</td>
<td>-0.038***</td>
<td>-0.024**</td>
<td>-0.033***</td>
<td>-0.024**</td>
</tr>
<tr>
<td></td>
<td>(0.012)</td>
<td>(0.011)</td>
<td>(0.011)</td>
<td>(0.012)</td>
</tr>
<tr>
<td>Post X Treated</td>
<td>0.036***</td>
<td>0.022*</td>
<td>0.034***</td>
<td>0.028**</td>
</tr>
<tr>
<td></td>
<td>(0.013)</td>
<td>(0.012)</td>
<td>(0.012)</td>
<td>(0.011)</td>
</tr>
<tr>
<td>Post X ΔWord</td>
<td>-0.424**</td>
<td>-0.098</td>
<td>-0.416**</td>
<td>-0.096</td>
</tr>
<tr>
<td></td>
<td>(0.185)</td>
<td>(0.205)</td>
<td>(0.166)</td>
<td>(0.276)</td>
</tr>
<tr>
<td>Post X Treated X ΔWord</td>
<td>0.147*</td>
<td>0.853**</td>
<td>0.061**</td>
<td>0.787**</td>
</tr>
<tr>
<td></td>
<td>(0.081)</td>
<td>(0.330)</td>
<td>(0.027)</td>
<td>(0.360)</td>
</tr>
<tr>
<td>Observations</td>
<td>14,985</td>
<td>14,985</td>
<td>14,985</td>
<td>14,985</td>
</tr>
<tr>
<td>Adjusted R²</td>
<td>0.086</td>
<td>0.086</td>
<td>0.086</td>
<td>0.086</td>
</tr>
<tr>
<td>Firm FE</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>Year FE</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>Inventor State FE</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>Inventor Tech FE</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
</tr>
</tbody>
</table>

* p < 0.1; ** p < 0.05; *** p < 0.01.
50%, 29%, and 31%, respectively. Therefore, inventors are more likely to leave when firms use less innovation words after CEO turnovers.

Tables 1.5 and 1.6 are robust to controlling for firm financial conditions such as EBITDA and sales. Therefore, the results are unlikely to reflect firms in bad conditions hiring non-STEM CEOs and using more words like “bankruptcy” and “restructuring.”

Taken together these findings suggest that non-STEM CEOs emphasize finance while STEM CEOs emphasize innovation, which drives out inventors.

1.5.3 Inventors with a Preference for Early-Stage Innovation Are More Likely to Leave after CEO Turnovers.

I have another empirical support for inventors leaving because firms switch from innovation-oriented strategies to finance-oriented strategies after CEO turnovers. I find that inventors who may have a preference for innovation are more likely to leave after CEO turnovers. Existing survey studies find that high-quality engineers and scientists who do academic-related innovation have a strong preference for early-stage innovation (Sauermann and Cohen, 2010; Sauermann and Roach, 2014). My survey results confirm this to hold with inventors too. I conjecture that inventors who are inclined to work at VC-backed startups also have a strong preference for early-stage innovation. This conjecture follows from casual observations that many VC-backed startups focus on early-stage innovation that creates new products, while public firms do late-stage innovation that updates their existing products and technologies. So VC-backed startups naturally attract inventors who want do early-stage innovation.

These inventors with a preference for early-stage innovation must experience more

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13I compute these figures as $\frac{\text{Coefficient} \times \text{Std. Dev.}}{0.13}$, where 0.13 is the pre-period inventor outflow rate at the turnover firms.

14Avg. Adj. Cites$_{it}$ is positively correlated with the survey response on the importance of basic research at 0.11, uncorrelated with the importance of applied research at 0.01, and uncorrelated with the importance of commercializing research at -0.004. Adj.%Academic$_{it}$ is positively correlated with the importance of basic research at 0.34, negatively correlated with the importance of applied research at -0.25, and negatively correlated with the importance of commercializing research at -0.21.
negative shocks when CEOs steer corporate focus from innovation to commercialization and finance. Therefore, if inventors left because of the corporate strategy change, then I would expect that these inventors with a strong preference for early-stage innovation are more likely to leave after CEO turnovers. Consistent with this hypothesis, I find that CEO turnovers strongly affect high-quality inventors who do academic-related innovation and that disproportionally many inventors leave to VC-backed startup firms after CEO turnovers.\footnote{The last finding agrees with numerous case studies that show disagreement on corporate strategies motivating R&D workers to leave to startups (see the references in Klepper and Thompson, 2010).}

### 1.5.4 Alternative Channels behind Inventor Outflows

**Unlikely: New CEOs Firing Unprofitable and Unproductive Inventors.**

High inventor outflows after CEO turnovers may reflect new CEOs laying off unproductive and unprofitable inventors. This hypothesis is motivated by Weisbach (1995) who finds that new CEOs divest unprofitable physical assets.

I find three findings inconsistent with this alternative hypothesis. First, I find that high-quality inventors are more likely to leave after CEO turnovers than low-quality inventors. Second, after CEO turnovers, both inventors with valuable technological specialties and those with less valuable specialties leave at a similar rate. This finding speaks against new CEOs laying off unprofitable specialist inventors. Third, inventors with weak outside options are more likely to leave after CEO turnovers than those with strong options. But theories suggest that these inventors get lower wages relative to their productivity and yield a larger share of their production values to firms (Becker, 1962; Lazear, 2009). So if high inventor outflows reflected new CEOs laying off inventors to increase firms’ profit margins, then I would see inventors with strong options, rather than those with weak options, leave after CEO turnovers — opposite to my finding. Taken together, new CEOs laying off unproductive and unprofitable inventors is unlikely to explain high inventor outflows after CEO turnovers.
Unlikely: Inventors Leaving Because New CEOs Have Incompatible Personalities.

High inventor outflows after CEO turnovers may reflect inventors leaving because new CEOs have incompatible personalities. This hypothesis is motivated by the famous conflict between William Shockley and the Fairchild founders. Shockley was a distinguished scientist but an unpleasant manager at Shockley Semiconductor. He once asked all of his employees to go through a lie detector test. Annoyed engineers left Shockley to start their own semiconductor firm Fairchild.

My findings speak against this hypothesis. In Figure 1.1B, my survey shows that inventors value CEOs’ tolerance for failures more than CEOs’ social skills (e.g., understanding, motivating). This finding suggests that CEOs’ innovation strategies, not their social skills, are the main channel behind high inventor outflows after CEO turnovers. To further assess this alternative channel, I further test three implications from the channel and find no support. Therefore, new CEOs’ incompatible personalities are unlikely to explain high inventor outflows after CEO turnovers.

1.6 STEM CEOs Delay the Corporate Life-Cycle.

I highlight a cost of STEM CEOs: delayed corporate life-cycle transition from the early-stage innovation phase to the late-stage innovation phase. Both the stock market and boards of directors seem to price this cost in when valuing high-tech companies and choosing next CEOs.

1.6.1 The Positive Stock Market Reaction to non-STEM CEOs Succeeding STEM CEOs.

The stock market reacts positively to the news of non-STEM CEOs succeeding STEM CEOs. In the unreported figure (see the full draft), I plot daily abnormal returns around the announcement date of health-related CEO turnovers by CEO turnover types. The announcement date \((t = 0)\) is the earliest date the CEO turnover is reported by the firm.
(through a press release or 8K filing) or by any other available news source. The left panel plots the average market-adjusted abnormal returns around the turnover announcement date, while the right plots the average factor-model-adjusted abnormal returns. The details for the adjustment are in the figure’s footnote. The figures show positive and statistically significant abnormal returns to the news of non-STEM CEOs succeeding STEM CEOs. The magnitudes are economically large at 3.9 and 3.5 percentage points, respectively.

The positive market reaction is puzzling, given that valuable inventors leave when non-STEM CEOs succeed STEM CEOs. To understand this puzzle, I first turn to the words of Steve Jobs, a former CEO of Apple, who said a high-tech firm needs different types of managers based on its development stage:

> The company does a great job [and grows]... and then the quality of the product becomes less important. [B]ecause they’re the ones who can move the needle on revenues[,]... the salespeople end up running the company... When the sales guys run the company, the product guys don’t matter so much, and a lot of them just turn off.

He knew a non-STEM CEO would “turn off” Apple’s R&D workers. But with Apple’s $300 billion market capitalization, “the needle on revenues” might have been too large. In 2011, the dying Jobs picked his successor: Tim Cook, a non-STEM CEO.¹⁶

An idea emerges: STEM CEOs overlook something that becomes increasingly important for mature firms, so they replace STEM CEOs with non-STEM CEOs. I test the idea in light of the corporate life-cycle theories.

1.6.2 STEM CEOs May Delay the Corporate Life-Cycle.

Corporate life-cycle theories state that mature firms switch from doing early-stage innovation to doing late-stage innovation. Two influential studies theorize reasons behind the transition. Abernathy and Utterback (1978) argues that firms learn what types of products their customers want over time, so the return to early-stage innovation falls (e.g., inventing

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¹⁶Tim Cook studied industrial engineering in college. I count industrial engineering as a non-STEM degree to capture the idea that STEM CEOs are those who have degrees for making new technologies and products.
cheaper materials for components). Klepper (1996) argues that firms grow over time, and late-stage innovation becomes cheaper as firms get larger.

Although mature firms need late-stage innovation, STEM CEOs, due to their preference for innovation, may stay in the early-stage innovation phase too long. Therefore, when STEM CEOs die, mature firms may hire non-STEM CEOs in order to switch from early-stage innovation to late-stage innovation.17

To test this hypothesis, I examine whether firms switch from early-stage innovation to late-stage innovation when non-STEM CEOs succeed STEM CEOs. I use firms’ propensity to cite academic research as a measure of firms’ early-stage innovation. The idea is that academic research is very early-stage innovation, so firms using academic research do more early-stage innovation than firms not using it.

The first two columns in Table 1.7 estimate the following firm-year level OLS regression:

$$\%\text{Academic}_{ft} = \{FE\} + \beta_1 Post_{ft} + \beta_2 Post_{ft} \times STEM \rightarrow STEM_f$$

$$+ \beta_3 Post_{ft} \times STEM \rightarrow nonSTEM_f + \beta_4 Post_{ft} \times nonSTEM \rightarrow nonSTEM_f + \epsilon_{ft}$$

(1.7)

where $\%\text{Academic}_{ft}$ measures firm $f$’s propensity to cite academic research papers in their patents filed in year $t$. The coefficient of interest is $\beta_3$ that measure how firms’ share of early-innovation changes when non-STEM CEOs succeed STEM CEOs. The hypothesis above predicts $\beta_3 < 0$.

Consistent with the hypothesis, the first two columns find statistically significant, negative, and economically meaningful coefficients on $STEM \rightarrow nonSTEM_f$. The magnitudes are 37% and 63% of the standard deviations of academic cite measures, respectively. This finding stands out; the last five columns use more traditional innovation measures (R&D expenses, patent quantities, and patent qualities) and find statistically insignificant and

17Management and strategy scholars have noted that different types of CEOs may be needed for different circumstances. See, for example, Carroll, 1984; Datta and Rajagopalan, 1998; Haveman, 1993; Henderson, Miller, and Hambrick, 2006; Shen and Cannella, 2002; Tushman and Rosenkopf, 1996.
### Table 1.7: Innovation and CEO Turnovers

<table>
<thead>
<tr>
<th>Post</th>
<th>%Academic Adj.</th>
<th>%Academic log(1+R&amp;D)</th>
<th>R&amp;D/Asset log(1+Patents)</th>
<th>TotalCites</th>
<th>TotalAdj.Cites</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>0.001</td>
<td>-0.001</td>
<td>-0.022</td>
<td>-0.001</td>
<td>-0.014</td>
</tr>
<tr>
<td></td>
<td>(0.004)</td>
<td>(0.005)</td>
<td>(0.076)</td>
<td>(0.003)</td>
<td>(0.036)</td>
</tr>
<tr>
<td>Post X STEM→STEM</td>
<td>-0.008</td>
<td>-0.005</td>
<td>0.072</td>
<td>0.004</td>
<td>0.085</td>
</tr>
<tr>
<td></td>
<td>(0.013)</td>
<td>(0.015)</td>
<td>(0.123)</td>
<td>(0.005)</td>
<td>(0.101)</td>
</tr>
<tr>
<td>Post X STEM→nonSTEM</td>
<td>-0.044**</td>
<td>-0.048**</td>
<td>0.041</td>
<td>-0.001</td>
<td>-0.066</td>
</tr>
<tr>
<td></td>
<td>(0.018)</td>
<td>(0.021)</td>
<td>(0.141)</td>
<td>(0.006)</td>
<td>(0.094)</td>
</tr>
<tr>
<td>Post X nonSTEM→nonSTEM</td>
<td>0.002</td>
<td>0.006</td>
<td>-0.44*</td>
<td>-0.018</td>
<td>-0.031</td>
</tr>
<tr>
<td></td>
<td>(0.003)</td>
<td>(0.005)</td>
<td>(0.248)</td>
<td>(0.014)</td>
<td>(0.106)</td>
</tr>
</tbody>
</table>

| Observations | 513 | 513 | 630 | 630 | 630 | 630 | 630 |
| Adjusted R² | 0.970 | 0.886 | 0.962 | 0.890 | 0.986 | 0.983 | 0.987 |
| Firm FE | Y | Y | Y | Y | Y | Y | Y |
| Year FE | Y | Y | Y | Y | Y | Y | Y |

* p < 0.1; ** p < 0.05; *** p < 0.01.
economically small (less than 0.05 standard deviations of outcome measures) coefficients on $STEM \rightarrow nonSTEM_f$. This contrast is consistent with recent research showing that not all innovations are invented equal, and they behave differently (Akcigit and Kerr, 2018; Arora, Belenzon, and Patacconi, 2018). In sum, when non-STEM CEOs succeed STEM CEOs, firms move from doing early-stage innovation to doing late-stage innovation, without changing R&D expenses and innovation quantities and qualities.

1.6.3 STEM CEOs for Young Firms, Non-STEM CEOs for Mature Firms.

STEM CEOs may stay in the early-innovation phase too long. Corporate life-cycle theories suggest that such behavior negatively affects firms mature. Therefore, mature firms may replace STEM CEOs with non-STEM CEOs. If the corporate life-cycle usually flows from the early-stage innovation phase to the late-innovation phase, I must see more firms replacing STEM CEOs with non-STEM CEOs than firms replacing non-STEM CEOs with STEM CEOs. Consistent with this idea, Table 1.2 shows that no firms in my sample replace non-STEM CEOs with STEM CEOs, although the reverse transition is common.

1.7 Conclusion

My findings suggest that STEM CEOs’ emphasis on innovation is a double-edged sword. It retains valuable R&D workers but also causes firms to delay late-stage corporate development. Mature firms may find delayed corporate development too costly and thus prefer non-STEM CEOs.

I suggest four fruitful extensions to my study. First, collecting data on health-related CEO turnovers in other countries will increase the study’s sample size and statistical power. Second, further narrowing down the endogeneity concerns arising from the selection of new CEOs — as I have done in Section 1.4.2 — will give more credibility to empirical CEO studies. Third, conducting a larger-scale survey of inventors, using questions with a greater level of
detail, will help us better understand what inventors want in firms and CEOs.\textsuperscript{18} Because inventors and other R&D workers create the innovation that drives economic growth, it is important to understand how to better manage and incentivize them.

Lastly, studying the endogenous selection of CEOs will help us understand which management styles corporate boards want. For example, in Section 1.6.3, I do some analysis along this line and show that, in my sample, firms never replace non-STEM CEOs with STEM CEOs, although the reverse transition is common. This finding suggests that mature firms may prefer non-STEM CEOs. But it leads to a new puzzle: why have mature firms’ corporate boards not fired STEM CEOs but, instead, have waited till the CEOs’ deaths? Perhaps board members with STEM degrees bond with STEM CEOs, as inventors do with STEM CEOs do. Thus, board members with STEM degrees may be more likely to hire and less likely to fire STEM CEOs. This homophily between board members and CEOs may lead to a corporate governance issue. In future studies, I plan to shed light on this idea by analyzing CEOhirings and firings at high-tech firms.

\textsuperscript{18}Sauermann and Cohen (2010) and Sauermann and Roach (2014) do this for a different subgroup of R&D workers — Ph.D. scientists.
Chapter 2

The Impact of Tax Changes on the Macroeconomy: A New Approach Using Failed Tax Changes

2.1 Introduction

To what extent tax changes impact the aggregate economy is a central question in macroeconomics. Despite this, estimating the causal impact of tax changes on the aggregate output remains a challenge due to endogeneity. Since lawmakers tend to propose and legislate a tax cut in anticipation of a slower output growth, tax changes are positively correlated with the output growth expectations of lawmakers which are unobserved by the econometrician. When unaddressed, this leads to an upward bias in the estimated impact of tax changes on the economy.

In this paper, we propose addressing the endogeneity in tax multiplier studies using time series of unlegislated tax changes tax changes considered by the Congress but ultimately fail to become law as a proxy for the unobserved output expectations of lawmakers. Our approach is motivated by the finding that legislative bills aimed at stabilizing output are often delayed or fail entirely due to political reasons (e.g., Chappell and Keech (1986),
Alesina and Drazen (1991), Alesina and Rosenthal (1994), Poterba (1994), Fatás and Mihov (2003)). If a substantial fraction of stabilizing tax proposals fail to pass for political reasons, then even the time series of unlegislated tax changes is likely to have a positive correlation with the output expectations that affect legislated taxes. Moreover, unlike legislated taxes, unlegislated taxes by definition cannot affect output directly. Hence, in a tax multiplier study that regresses future output growth on legislated tax changes, including the unlegislated tax change variable helps absorb the effect of the unobserved output expectations without affecting the causal relationship between legislated tax changes and output.

To illustrate our approach, we collect data on both legislated and unlegislated tax revenue changes in the United States from 1975 to 2017. Among the 268 tax bills with revenue estimates from the Joint Committee on Taxation, 74 bills (28%) eventually get legislated and 194 bills (72%) fail to become law. From these we obtain our quarterly measures of legislated and unlegislated tax revenue changes.

Consistent with our assumption that unlegislated tax changes reflect the output expectations of lawmakers, they predict future GDP growth with a positive coefficient. When regressing real GDP growth on contemporaneous and lagged unlegislated tax changes over 12 quarters to mimic the conventional time-series tax multiplier regression, we find that a 1% increase in the unlegislated tax change as a fraction of GDP is associated with around 1.5% increase in the GDP growth over the next 12 quarters. Since unlegislated tax changes do not affect GDP directly, this large “unlegislated tax multiplier” of around 1.5 reflects that more tax cuts (increases) are proposed in anticipation of a slower (faster) output growth. Furthermore, we find that unlegislated tax changes contain information about future output growth orthogonal to other potential predictors of output growth. Unlegislated tax changes positively predict future GDP growth after controlling for lagged GDP growth and various survey forecasts.

Moving onto our main empirical approach, we illustrate how the unlegislated tax variable helps correct the tax multiplier. We find that a naive regression which does not address endogeneity implies a positive tax multiplier of around 0.1. This small but positive
value suggests that the endogeneity of lawmakers legislating more tax cuts in anticipation of a slowing economy overwhelms the potential direct effect of tax cuts stimulating the economy. On the other hand, once we control for unlegislated tax changes as a proxy for the anticipated output growth, the legislated tax multiplier falls to around $-1.1$, which we argue is more likely to capture the causal effect of legislated tax changes on output. We also find this tax revenue estimate to be reasonably robust. Regardless of alternative specifications and data constructions, we obtain a tax multiplier of around $-0.8$ to $-1.6$.

To summarize, our contribution is to propose a simple proxy approach to dealing with the endogeneity issue in fiscal multiplier studies and to illustrate the approach in the context of tax multipliers. Other types of fiscal policy also have historical data on both the legislated and unlegislated changes, so one can apply the proxy variable method to obtain the correct fiscal multiplier in other settings.

Our paper belongs to the large literature proposing alternative ways to obtain the correct fiscal multiplier.\footnote{The literature is too large to list here in a satisfactory manner. Ramey (2011a) is a recent survey paper on the topic.} Although various approaches have been proposed, we do not view this literature as crowded given the importance of estimating the correct fiscal multiplier and the wide range of the estimates found in the literature.

The structural VAR approach identifies the tax multiplier by imposing additional structures on the evolution of the economy.\footnote{Examples are Perotti (1999), Fatas and Mihov (2001), Blanchard and Perotti (2002), and Mountford and Uhlig (2002) among others.} For example, Blanchard and Perotti (2002) use elasticities inferred from institutional information about tax and transfer systems and assume that discretionary fiscal policy takes longer than one quarter to respond to news about the economy. Mountford and Uhlig (2002) imposes restrictions on the sign of impulse responses. However, the structural VAR approach can be sensitive to the structural assumptions (Caldara and Kamps, 2012) and to assumptions about the implementation lag in the policy variable (Martens and Ravn (2010) and Favero and Giavazzi (2012)). The simple fiscal VAR has also been extended to incorporate key country characteristics that
fiscal shocks depend on, such as the level of development, exchange rate regime, openness to trade, and public indebtedness (Ilzetzki, Mendoza, and Végh (2010)) and debt dynamics analysis (Ilzetzki, 2011).

The narrative approach identifies the principal motivation for policy actions from presidential speeches and Congressional reports to distinguish between “exogenous” and “endogenous” actions. Using this approach, Romer and Romer (2010) and Cloyne (2013) obtain a large GDP tax multiplier of around $-2.5$ to $-3$ in the U.S. and the U.K., respectively, whereas Ramey (2009) and Perotti (2012) obtain much smaller multipliers. The narrative approach is a departure from the earlier studies which focused on correcting for the relationship between output and revenues and the behavior of government spending to obtain an unbiased estimate of the tax multiplier (Romer and Romer, 2010). However, the narrative approach tends to be time-consuming and subjective.

Others combine the VAR and narrative approaches or suggest an entirely new approach. Martens and Ravn (2014) use narrative measures as proxies for structural shocks to total tax revenues in an SVAR. Ramey and Shapiro (1998) and Ramey (2011b) use defensive spendings due to war events to gauge the government spending multiplier. Barro and Redlick (2011) use marginal tax rates series to estimate a tax multiplier but instrument the variation using the Romer-Romer tax dataset and find a negative multiplier of $-1.1$. However, they find that the “tax revenue” multiplier is negligible due to the substitution effect. Some others use the cross-sectional variation in fiscal shocks to identify their effect on macroeconomic variables (e.g., Johnson, Parker, and Souleles (2006), Chodorow-Reich, Feiveson, Liscow, and Woolston (2012), Parker Souleles, Johnson, and McClelland (2013), and Chodorow-Reich (2018) among others).

Some papers focus on reconciling the differences in the SVAR and narrative measures with the premise that the difference arises from either the identification assumptions of the SVAR or from the assumed reduced-form transmission mechanisms. Charhour, Schmitt-Grohe, and Uribe (2012) however reject this hypothesis and suggest instead that the observed differences are due to either both models failing to identify the same tax shocks or due to
small-sample uncertainty. Favero and Giavazzi (2012) aim to reconcile the difference between Romer and Romer (2010) and Blanchard and Perotti (1991) by including narrative shocks in a VAR model. They create an encompassing model where the Romer-Romer taxes appear as a limited information approach since while it directly identifies tax shocks, it omits other sources of information that is included in the VAR approach. Perotti (2011) counters this by claiming that Favero and Giavazzi is biased towards zero since the discretionary component of tax will have different effects compared to the automatic response of tax revenues to macroeconomic variables. Leeper, Walker, and Yang (2008) on the other hand argue that even the most creative identification schemes in a fiscal VAR cannot extract economically meaningful shocks to taxes because of the existence of the non-invertible moving average component in the equilibrium time series that results in biased tax multipliers. Furthermore, even narrative approaches that aim to identify fiscal foresight ex-ante will only be successful depending on the degree to which forecasted revenue changes reflect exogenous changes in taxes and the relative volatility of the random components of tax decisions.

Our approach is appealing in multiple ways. Unlike the structural VAR approach, we do not rely heavily on the structural assumption on the evolution of the economy. The assumption we do impose is that all tax proposals legislated or unlegislated carry some information about the lawmakers’ expectations of future economic activities. We test the validity of this assumption. Unlike the narrative approach, our method has less room for subjectivity and can be implemented quickly. The weakness of our approach is the assumption that the unlegislated actions are determined by similar variables that determine the legislated actions. However, one can address this issue by presenting evidence consistent with the assumption as we do based on the GDP predictability evidence.

2.2 The framework

We use a simple econometric model to describe why a naive regression of the output growth on the legislated tax changes is biased and how using unlegislated tax changes solves this issue.
We begin by highlighting how the endogeneity of legislated tax changes leads to a bias in the tax multiplier estimation. Suppose that the data-generating process for output growth at time $t+1$ is

$$\Delta Y_{t+1} = \beta \Delta T_t + g_t + \epsilon^Y_{t+1}, \quad (2.1)$$

where $\Delta T_t$ measures the change in legislated tax revenue, $g_t$ is the deviation in the economic agent’s expectation of the output growth from the stationary level of growth, and $\epsilon^Y_{t+1}$ measures other shocks to the economy that are independent of everything else. Importantly, the legislated tax revenue change at time $t$ follows the data generating process,

$$\Delta T_t = f(g_t) + \epsilon^T_t, \quad (2.2)$$

where $\epsilon^T_t$ is a measurement error that is independent of everything else. If lawmakers legislate tax cuts when anticipating a recession, then $\frac{df}{dg} > 0$. For simplicity, we suppose $f(g) = \gamma_1 g_t$, where $\gamma_1 > 0$.

The problem is that the econometrician does not observe $g_t$. Hence, a naive tax multiplier regression estimates the following model:

$$\Delta Y_{t+1} = b \Delta T_t + \epsilon^Y_{t+1} \quad (2.3)$$

This leads to a bias $b > \beta$ because $\text{Cov}(\Delta T_t, g_t) > 0$. Intuitively, if lawmakers anticipate a recession and legislate tax cuts, then a naive econometrician observes a low output growth after tax cuts and erroneously conclude that tax cuts reduce the future economic growth.

Our approach is to use additional information contained in changes in unlegislated tax revenues. Because unlegislated tax revenue changes do not become law, they do not directly enter into the data generating process for the output growth. Instead, they load on $g_t$. Specifically, we assume that the unlegislated tax revenue change at time $t$ follows the following data generating process:

$$\Delta U_t = h(g_t) + \epsilon^U_t \quad (2.4)$$
where $e_t^U$ is a measurement error that is independent of everything else. If lawmakers propose tax cuts when anticipating a recession, then $\frac{dh}{dg} > 0$. For simplicity, we assume linearity $h(g) = \gamma_2 g_t$.

We model $f(g_t)$ and $h(g_t)$ separately because a legislated tax bill may have more components than an unlegislated tax bill. For example, lawmakers may add “pork barrel” components components that help their constituents for political reasons into a tax bill as the bill goes through the legislation process (e.g., passing the House, resolving the difference between the House and the Senate). In this case, since a legislated tax bill goes through more steps in the legislation process than an unlegislated tax bill, we would expect $f(g) > h(g)$ for the same $g$.

Solving for $g_t$, we have

$$g_t = \frac{\Delta U_t - e_t^U}{\gamma_2} \quad (2.5)$$

Plugging $g_t$ into the output growth data generating process, we have

$$\Delta Y_{t+1} = \beta \Delta T_t + \frac{\Delta U_t - e_t^U}{\gamma_2} + e_t^Y$$

$$= \beta \Delta T_t + \frac{\Delta U_t}{\gamma_2} + \left( e_t^Y - \frac{e_t^U}{\gamma_2} \right) \quad (2.6)$$

Because $e_t^Y$ and $e_t^U$ are independent of everything else, we can correctly estimate the tax multiplier $\beta$ now.

### 2.3 Data

Legislated and unlegislated tax revenue changes. We collect data on revenue estimates for tax proposals in the U.S. over 1975–2017. We begin with the universe of revenue estimates available on the Joint Committee on Taxation (JCT) website since the JCT provides revenue estimates for all tax proposals (bills) considered by the Congress since July 1974. To obtain revenue estimates for tax proposals, we apply two criteria. First, we require that the title of the revenue estimate document contains the bill identifier information (e.g., House bill “H.R. 4”). This discards revenue estimates that are not specific to any specific tax bill (e.g.,
overview of tax expenditures in a given year). Second, we require that the document
contains a table with revenue estimates to minimize errors in the digitization process. This
leaves us with 514 JCT revenue estimates on 369 distinct tax bills. Some tax bills have
multiple JCT estimates since the Congress may revise the proposal as the bill progresses
through the legislative rounds.

Using the latest tax revenue estimates for all 369 distinct bills may result in double
counting since the Congress often introduces different bills based on similar ideas. For
example, Democrats and Republicans may propose two different versions of tax changes
based on the same idea of offsetting an anticipated recession. To address this issue, we
define a “peer group” for tax bills. First, we associate each tax bill with all “associated” JCT
documents mentioning the bill at least once in the document. Then, we define a peer group
as all tax bills with at least one common associated JCT document. In each peer group,
we pick one bill that has the higher number of associated JCT documents and call it the
dominant tax bill. If there is a tie in the number of associated JCT documents within a peer
group, we break the tie randomly.

From this, we identify 309 dominant bills. Excluding 41 bills with zero revenue estimates
and taking the latest revenue estimate, we obtain 268 proposed tax changes. Figure 2.1
summarizes these 268 proposed tax changes by the last congressional action on the bill.
We find that the number of bills that do not pass either of the chamber of the Congress,
those that pass at least one chamber but fails to pass the other chamber, and those that
successfully become law make up 31%, 41%, and 28% of all proposed tax changes.

Matching tax revenue estimates with legislative records on the U.S. Congress website,
we obtain the dates when the bill was last considered in the Congress. For legislated bills,
this is the day when the bill was legislated, and for unlegislated tax bills, this is the date
when the bill was last discussed in the Congress. However, we need to find the actual and
supposed implementation date of the tax change for the legislated and unlegislated bills.

3If there are multiple JCT revenue estimate documents for the latest date associated with the bill, we assume
that they are estimates for different provisions of the bill and take a sum over those estimates. We show in our
robustness section that taking an average leads to similar results.
Figure 2.1: Distribution of Proposed Tax Bills by Last Legislative Action

The figure reports the number of tax bills in our legislated and unlegislated tax revenue change data by three mutually exclusive categories: passed 0 chamber before failing; passed at least 1 chamber before failing; and became law.

Mertens and Ravn (2008) report that the median lag between the legislation date and the implementation date is 6 quarters. Assuming a similar lag, we add 4 and 6 quarters to the last record date to obtain the implementation quarter for legislated and unlegislated tax changes, respectively. That is, we assume a “legislation lag” (the time it would have taken for an unlegislated tax to pass) of 2 quarters and an “implementation lag” (time it would have taken for a legislated tax to be implemented) of 4 quarters. However, we consider alternative lags and find that our numbers do not change significantly. Following Romer and Romer (2010), we focus on the effect of the initial change in the tax policy. We do this by constructing our series based on JCT’s estimate of tax revenue change to the first year of implementation, assuming that the tax revenue changes in the following years merely reflect the continuation of the same policy change.4

Aggregating all tax changes in a given quarter and dividing the resulting sum by nominal GDP, we obtain the time series of legislated and unlegislated tax changes. These are plotted

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4We put this tax revenue change in the first year to the supposed quarter of implementation rather than spreading it out across different quarters. Our robustness check shows that spreading the change over four quarters generates similar results.
in Figure 2.2. Both legislated and unlegislated tax change proposals tend to be sparse and smaller in magnitude in the early half of the sample, whereas they are more frequent and negative in the second half of the sample. Although this is consistent with having more tax cut proposals around economic downturns, it could also lead the two tax change series to act as a \((-1\) times\) dummy variable on the second half of the sample.\(^5\) This could result in a bias if GDP growth rate has slowed down over time or was low following the financial crisis of 2008-2009. We address this concern in two ways. First, we include a dummy variable for the second half of the sample of 1997-2017, thereby using only the variation within each half of the sample. Second, we also repeat our regressions using the pre-crisis sample of 1975-2007 and report it as one of our robustness specifications.

**Macroeconomic variables** The following data are from the National Income and Product Accounts: Nominal GDP from Table 1.1.5, Real GDP from Table 1.1.3 (Index : 2012=100), Price Indices for GDP from Table 1.1.4 and Government spending from Table 3.1. Government Current Receipts and Expenditures. All of these are provided in billions of dollars.

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\(^5\)One reason for the sparse tax changes in the 1980s is the rule we apply to obtain JCT tax revenue estimates. Some of the large tax proposals in the 1980s did not have accompanying JCT tax revenue estimates in a table format, making it difficult to digitize the information. We are currently addressing this problem by manually going through JCT tax revenue documents at the time again.
and are seasonally adjusted at annual rates. The data on the three-year bond rate are from the Board of Governors of the Federal Reserve System, series H15/H15/RIFLGFCY03_N.M. All the above data were last revised on October 26, 2018.

**GDP forecast data** In the next section, we evaluate the ability of unlegislated tax changes to predict output growth beyond survey forecasts. The forecast variables we look at are from the Survey of Professional Forecasters (SPF), the Livingstone Survey (Livingstone), the Survey of Consumers (SC) and Fed Staff’s Greenbooks (Greenbooks). Apart from the Survey of Consumers which uses the level value of real GDP, all the other datasets provide the growth rates of the real GDP forecasts. Data is available from 1975 to 2018 for all except the Survey of Consumers and the Fed Staff’s Greenbooks. While the Survey of Consumers misses data from 1975 and is only available from 1978, the Fed Staff’s Greenbooks provide data only until 2012. Another key distinction is that while most of the forecast variables are available at a quarterly rate, the Livingstone forecasts are available only at a semi-annual rate.  

2.4 Unlegislated tax changes as a proxy for unobserved growth expectations

In this section, we provide evidence consistent with our conjecture that unlegislated tax changes are positively correlated with the unobserved output expectations. If unlegislated tax changes load positively on output expectations that are on average correct but cannot affect future output directly, we would expect the multiplier on unlegislated tax changes to be positive.

To check this, we regress real GDP growth on the contemporaneous and lagged unlegislated tax changes (up to 12 quarters) to infer the coefficients on those tax changes over

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6The forecast data from the Survey of Consumers is available at a monthly rate and was transformed into a quarterly forecast.
1975-2017:

\[ \Delta Y_t = \alpha + \sum_{i=0}^{12} \beta_i \Delta U_{t-i} + \epsilon_t \]  

(2.7)

where \( \Delta Y_t \) denotes real GDP growth in quarter \( t \) and \( \Delta U_{t-i} \) denotes the unlegislated tax change in quarter \( t - i \). In other words, the cumulative GDP response \( \sum_{i=0}^{12} \beta_i \) represents the “unlegislated tax multiplier” coming purely from unlegislated tax changes being positively correlated with the lawmakers’ expectations of future GDP growth and not from any causal effect on GDP.

![Figure 2.3: The Unlegislated Tax Multiplier](image)

Gray lines denote the one standard deviation confidence band. They are computed by taking 10,000 draws of the coefficient vector from a multivariate normal distribution with mean and variance-covariance matrix equal to the point estimates and variance-covariance matrix of the regression coefficients.

Figure 2.3 shows that the unlegislated tax multiplier is positive. That is, output growth tends to be faster (slower) following unlegislated proposals to increase (cut) taxes. The magnitude is large. A one-percentage increase in the unlegislated tax change proposals as a fraction of GDP is associated with around 1.5 percentage point (pp) increase in the GDP growth rate over the next 12 quarters following the supposed implementation. This suggests that unlegislated tax increases (cuts) tend to be proposed in anticipation of higher (lower) output growth rate, consistent with the premise of our proxy approach.

Table 2.1 shows that the finding is robust to controlling for other potential predictors.
Table 2.1: Tax Multiplier on Legislated Tax Changes

<table>
<thead>
<tr>
<th>Legislation plus implementation lag (quarters)</th>
<th>Baseline</th>
<th>Dummy</th>
<th>Lagged GDP</th>
<th>SPF</th>
<th>SC</th>
<th>Greenbooks</th>
<th>Livingstone</th>
</tr>
</thead>
<tbody>
<tr>
<td>2</td>
<td>1.34*</td>
<td>0.84*</td>
<td>1.13*</td>
<td>1.08**</td>
<td>0.38</td>
<td>1.69*</td>
<td>1.13*</td>
</tr>
<tr>
<td></td>
<td>(0.69)</td>
<td>(0.78)</td>
<td>(0.69)</td>
<td>(0.51)</td>
<td>(0.66)</td>
<td>(0.88)</td>
<td>(0.71)</td>
</tr>
<tr>
<td>4</td>
<td>1.41**</td>
<td>0.49</td>
<td>0.71*</td>
<td>1.22**</td>
<td>0.71*</td>
<td>1.50*</td>
<td>1.54**</td>
</tr>
<tr>
<td></td>
<td>(0.70)</td>
<td>(0.76)</td>
<td>(0.69)</td>
<td>(0.51)</td>
<td>(0.63)</td>
<td>(1.03)</td>
<td>(0.70)</td>
</tr>
<tr>
<td>6</td>
<td>1.57**</td>
<td>0.65</td>
<td>0.81*</td>
<td>1.35***</td>
<td>1.05*</td>
<td>1.98*</td>
<td>0.95*</td>
</tr>
<tr>
<td></td>
<td>(0.69)</td>
<td>(0.77)</td>
<td>(0.69)</td>
<td>(0.51)</td>
<td>(0.63)</td>
<td>(1.12)</td>
<td>(0.72)</td>
</tr>
<tr>
<td>8</td>
<td>1.57**</td>
<td>1.26*</td>
<td>1.46**</td>
<td>1.42***</td>
<td>1.15*</td>
<td>1.83*</td>
<td>0.92</td>
</tr>
<tr>
<td></td>
<td>(0.71)</td>
<td>(0.76)</td>
<td>(0.70)</td>
<td>(0.52)</td>
<td>(0.64)</td>
<td>(1.32)</td>
<td>(1.20)</td>
</tr>
</tbody>
</table>

The standard errors are reported in parenthesis. * - significant at 32%; ** - significant at 5%; *** - significant at 1%.
of output growth and to alternative assumptions about the number of quarters it would have taken for the unlegislated tax proposals to be implemented. This suggests that the output expectations of lawmakers contained in unlegislated tax change proposals are not fully captured by other time-series variables. It is also reassuring that the results are not sensitive to the assumption about the number of quarters between the last congressional action on unlegislated tax bills and the supposed date of implementation.

2.5 The legislated tax multiplier

Using unlegislated tax changes as a proxy for the growth expectations of lawmakers, we show how the proxy variable addresses the endogeneity problem in tax multiplier studies. In all analyses, the baseline specification is 4 quarters’ implementation lag (the lag between legislation and implementation) and 2 quarters’ legislation lag (the time it would have taken for an unlegislated tax bill to pass).

2.5.1 The naive multiplier

We begin by estimating the naive multiplier. This is useful in highlighting the presence of endogeneity in a tax multiplier regression and serves as a benchmark for the next subsection, when we address the endogeneity problem with a proxy variable. To do this, we regress real GDP growth on the contemporaneous and lagged legislated tax changes (up to 12 quarters) as well as lagged GDP growth to infer the coefficients on the tax changes:

\[ \Delta Y_t = \alpha + \sum_{i=0}^{12} \beta_i \Delta T_{t-i} + \sum_{j=1}^{12} \eta_j \Delta Y_{t-j} + \epsilon_t \]  

(2.8)

where \( \Delta T_{t-i} \) denotes the unlegislated tax change in quarter \( t - i \).

Figure 2.4 shows that the naive tax multiplier is slightly positive (0.15) rather than negative for the baseline sample of 1975-2017, contrary to the notion that a tax increase (cut) has a contractionary (expansionary) effect on GDP.\(^7\) This points to the endogeneity

\(^7\)In the specification without lagged GDP, the “tax multiplier” would simply be the sum of the betas \( \sum_{i=1}^{12} \beta_i \).
Figure 2.4: The Naive Tax Multiplier on Legislated Tax Changes: Estimated Change in GDP Associated with an Legislated Tax Increase of 1 Percent of GDP

Figure 2.4A. Baseline specification

Figure 2.4B. With a dummy variable for the 1997-2017 sample
problem that motivates our study. Even if legislated tax changes have a negative causal effect on future output, they are likely to be positively correlated with the component of future output observed by lawmakers, leading to an upward bias. The conclusion is similar when we include a dummy variable for the second half of the sample (1997-2017) to use only the variations within each half of the sample, in which case we get a small negative naive multiplier of $-0.58$.

### 2.5.2 The proxy variable approach

We offer a simple remedy to the endogeneity problem illustrated above. Since unlegislated tax changes are also likely to be positively correlated with the output expectations of lawmakers, one can include unlegislated tax changes as a proxy variable for those unobserved expectations:

$$
\Delta Y_t = \alpha + \sum_{i=0}^{12} \beta_i \Delta T_{t-i} + \sum_{j=0}^{12} \gamma_j \Delta U_{t-j} + \sum_{k=1}^{12} \eta_k \Delta Y_{t-k} + \epsilon_t
$$

(2.9)

As explained in Section 2, this corrects for the bias arising from the omitted variable if unlegislated tax changes have a nonzero loading on the unobserved growth expectations, even if the loading is different from that of legislated tax changes.

Figure 2.5 shows that the tax multiplier on legislated tax changes is now $-1.10$ as opposed to $0.15$ in the naive approach, more in line with the notion that the causal effect of tax changes on GDP is negative. This suggests that the positive correlation between legislated tax changes and output expectations are now absorbed by unlegislated tax changes, leaving the coefficients on legislated tax changes to only reflect the causal effect on future output. In terms of the magnitude, cutting tax revenues by 1% in the legislated tax change as a fraction of GDP is associated with around 1.1pp increase in the GDP growth rate over the next 12 quarters. The conclusion is again similar in the specification with a dummy variable for the second half of the sample. In this case, the implied tax multiplier is $-1.67$ instead of $-0.58$ from the naive approach.

For the specification with lagged GDP, the tax multiplier is the dynamic tax multiplier that accounts for the feedback effect between $\Delta T$ and $\Delta Y$ as in Romer and Romer (2010).
Figure 2.5A. Baseline specification

Figure 2.5B. With a dummy variable for the 1997-2017 sample

Figure 2.5: Predicting GDP Growth Using Unlegislated Tax Changes

Gray lines denote the one standard deviation confidence band. They are computed by taking 10,000 draws of the coefficient vector from a multivariate normal distribution with mean and variance-covariance matrix equal to the point estimates and variance-covariance matrix of the regression coefficients.
Table 2.2: Tax Multiplier on Legislated Tax Changes

<table>
<thead>
<tr>
<th>Implementation lag (quarters):</th>
<th>2</th>
<th>4</th>
<th>6</th>
</tr>
</thead>
<tbody>
<tr>
<td>Legislation lag (quarters):</td>
<td>0</td>
<td>2</td>
<td>4</td>
</tr>
<tr>
<td>Naive tax multiplier</td>
<td>——</td>
<td>——</td>
<td>0.05</td>
</tr>
<tr>
<td></td>
<td>(0.72)</td>
<td>(0.70)</td>
<td>(0.68)</td>
</tr>
<tr>
<td>Tax multiplier (corrected</td>
<td>-1.02</td>
<td>-1.00*</td>
<td>-0.87</td>
</tr>
<tr>
<td>using the proxy approach</td>
<td>(1.07)</td>
<td>(0.99)</td>
<td>(0.97)</td>
</tr>
</tbody>
</table>

The standard errors are reported in parenthesis. * - significant at 32%; ** - significant at 5%; *** - significant at 1%.
Table 2.2 summarizes our result by comparing regression models (8) and (9) under different assumptions about the number of lags between legislation and implementation (baseline: 4 quarters) as well as the number of lags between last congressional action on failed bills and supposed legislation (baseline: 2 quarters). Across all specifications, the proxy variable approach imply a tax multiplier of around (negative) 0.7 to -1.7, whereas the naive approach leads to a tax multiplier of around (negative) 0.6 to 0.2.

2.5.3 Robustness

We study how the resulting tax multiplier estimate changes with a battery of robustness checks. The results are summarized as Table 2.3.

First, tax increases may be legislated to offset more government spending. If government spending positively affects the future GDP growth, then not controlling for government spending may result in a bias. To address this concern, we add the change in total government expenditures divided by nominal GDP as a control. The results are almost identical to the baseline specification.

How does dropping lagged GDP affect our result? Although we believe it prudent to include lagged GDP as additional potential determinants of future GDP, it is useful to know how the result changes. In this case, the tax multiplier goes from 0.74 to -0.71 when we switch from the naive approach to our proxy variable approach.

We also repeat our analysis using the pre-crisis sample of 1975-2007 instead of the full sample period of 1975-2017. In this case, we obtain a similar conclusion. Not including the proxy variable implies a naive tax multiplier of -0.06, whereas controlling for the proxy implies a multiplier of -0.82.

Next, we consider an alternative definition of our proxy variable. It is possible that tax bills that are not consistent with the expectation of future economic activity fail earlier in the legislative process, whereas tax bills more consistent with the expectation lasts longer in the Congress. In this case, proposed tax changes that pass at least one chamber of the Congress but ultimately fail (“barely failed” tax bills) may be a superior proxy for the expectation.
<table>
<thead>
<tr>
<th></th>
<th>Naive Tax Multiplier</th>
<th>Corrected Tax Multiplier (Proxy Approach)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Baseline specification</strong></td>
<td>0.15</td>
<td>-1.10*</td>
</tr>
<tr>
<td></td>
<td>(0.70)</td>
<td>(0.97)</td>
</tr>
<tr>
<td><strong>Post 1997 dummy</strong></td>
<td>-0.58</td>
<td>-1.67*</td>
</tr>
<tr>
<td></td>
<td>(0.88)</td>
<td>(1.10)</td>
</tr>
<tr>
<td><strong>Government spending</strong></td>
<td>0.14</td>
<td>-1.06*</td>
</tr>
<tr>
<td></td>
<td>(0.72)</td>
<td>(0.97)</td>
</tr>
<tr>
<td><strong>Without lagged GDP</strong></td>
<td>0.74*</td>
<td>-0.71</td>
</tr>
<tr>
<td></td>
<td>(0.69)</td>
<td>(1.04)</td>
</tr>
<tr>
<td><strong>Pre-crisis sample</strong></td>
<td>-0.06</td>
<td>-0.82</td>
</tr>
<tr>
<td></td>
<td>(1.14)</td>
<td>(1.00)</td>
</tr>
<tr>
<td><strong>Using &quot;barely failed&quot; bills</strong></td>
<td>0.11</td>
<td>-0.83</td>
</tr>
<tr>
<td></td>
<td>(0.69)</td>
<td>(1.02)</td>
</tr>
<tr>
<td><strong>Averaging JCT estimates</strong></td>
<td>0.23</td>
<td>-1.18*</td>
</tr>
<tr>
<td></td>
<td>(0.74)</td>
<td>(1.14)</td>
</tr>
<tr>
<td><strong>ΔT spread over 1 year</strong></td>
<td>-0.12</td>
<td>-1.31*</td>
</tr>
<tr>
<td></td>
<td>(0.74)</td>
<td>(1.12)</td>
</tr>
<tr>
<td><strong>Using net present value</strong></td>
<td>-0.06</td>
<td>-1.38*</td>
</tr>
<tr>
<td></td>
<td>(0.73)</td>
<td>(1.00)</td>
</tr>
</tbody>
</table>

The standard errors are reported in parenthesis. * - significant at 32%; ** - significant at 5%; *** - significant at 1%.
of future economic activity. However, using these subset of 110 tax bills to construct our unlegislated tax change variable leads to a tax multiplier of $-0.83$.

As mentioned in Section 2.3, the dominant bill may have multiple JCT revenue estimates on the last congressional action date. In this case, we took a sum over all revenue estimates. Under the alternative approach of taking an average, the tax multiplier estimate is $-1.18$.

Our baseline approach assumes that the tax revenue change happens instantly in the implementation quarter. Alternatively, we could split the tax revenue evenly across the four quarters starting with the implementation quarter and normalize the resulting tax revenue by the GDP in the corresponding quarter. The tax multiplier in this case becomes more negative at $-1.31$.

People may follow the permanent income hypothesis and respond to not just the immediate change in tax but also respond to news about the future changes. To address this concern, we add the net present value of tax changes, similarly to Romer and Romer (2010). In this case, the tax multiplier estimate become more negative at $-1.38$.

## 2.6 Conclusion

In this paper, we propose correcting for the bias in tax multiplier studies using an unlegislated tax change series as an additional control. Using this approach, we obtain a tax multiplier of around $-0.7$ to $-1.7$ in the recent U.S. sample of 1975-2017.

We believe our approach can have fruitful applications. Since it uses the readily available information about failed bills, one can apply our method to other fiscal multiplier estimations. It would also be possible to collect state-level legislation information to study local fiscal multipliers. These extensions are left to future studies.
Chapter 3

How Much Do Property Rights Matter for the Rate of Innovation?

3.1 Introduction

The underrepresentation of women and other minority groups in the sciences and innovation is well-documented in both official government statistics and in the economics literature. In 2016, women made up only 28% of the total science and engineering workforce (NSF2018), and the representation of women among patent holders was even lower, at 12% (USPTO2019). In many developing countries, the underrepresentation is starker.¹ Recent work by Hsieh (2013) and Celik (2018) has argued that underrepresentation and misallocation of talent may be sufficiently consequential to substantially hinder economic growth.

In this paper, we show that the lack of property rights to protect economic incentives may have been a reason that women in the United States only rarely participated in innovation-related activities during the nineteenth century. Until the mid-1800s, women in the United States were considered men’s property and were controlled by either their father, if single, or husband, if married. This concept, known as “coverture,” meant that women did not have the rights to own their own property or keep any wages they earned. Starting in the

¹See https://www.wipo.int/wipo_magazine/en/2018/02/article_0008.html
mid-1800s, states began granting women property rights and rights to their earnings.

Our paper analyzes the effects of these property rights laws on women’s patenting. Patents are designed to incentivize innovation by rewarding inventors with temporary exclusive rights to monetize their inventions. However, without sufficiently strong property or earnings rights, the incentive to patent and monetize an invention may not be meaningful. By passing laws that granted married women control over any profits generated by their patents, states increased the benefit to married women of owning a patent. All else equal, the number of patents held by women would be expected to increase.

We use US data on patents granted between 1836 and 1920 combined with a difference-in-differences empirical strategy to compare the change in women’s patenting between states that granted and did not grant property and earnings rights to married women. Our estimates show that, on average, patenting by women increased by around 7% in states that passed property rights and earnings rights laws for married women. Controlling for population and for the existence of co-ed colleges for women does not reduce the magnitude of this estimate. Quantitatively, the average number of women’s patents prior to passage of the property and earnings rights laws was very low or zero in most states. Hence, we interpret the estimates with the caveat that any effect on women’s patenting perhaps had limited impact overall due to the low rate of women’s patenting both before and after the laws were implemented.

Nevertheless, our findings do provide evidence of a positive effect of stronger property rights laws on the number of patents held by women, which suggests that institutions that uphold property rights, along with granting autonomy over earnings or profits, are important. Although our study uses historical data, the status of women in the United States during the 19th century might be similar to the status of other marginalized groups in the present day. Many developing countries discourage women from participating in economic activities much like in the nineteenth century United States. The World Bank estimates that at least 40% of economies have at least one constraint on women’s property rights, such as the inability of women to own titled property or unequal inheritance rights between sons.
and daughters (WBG, 2018). Allowing people to own property and manage their earnings may incentivize them to innovate when they otherwise would not have because their returns to innovating were not protected.

Our paper relates to a literature on how government policies, laws, and institutions affect innovation. Acemoglu (2001) and Glaeser (2004) show that countries with inclusive and non-extractive government policies have more innovation and faster economic growth. Recent studies have shown that not just property laws, but also labor laws (Acharya, 2013) and bankruptcy codes (Acharya, 2009) affect innovation. Our study is most closely related to Khan (1996), who also studies the effects of granting property rights to married women on the number of patents granted to women during the 19th century. Khan (1996) does a decade-by-decade comparison of women’s per capita patenting for the years 1870-1895 and documents that states that had yet to pass laws had lower rates of patenting by women and that patenting by women increased after passage of property rights laws. We expand upon (Khan, 1996) by using a comprehensive dataset of all patents granted between 1836 and 1916 and use the distribution of female and male names during this time period to infer the inventor’s gender. We are able to analyze a broader period and perform a yearly analysis with our data, which allows us to study how the law impacted most states and assess any trends in the rate of patenting over time for both male and female inventors.

Our work also builds upon Geddes (2000) and Geddes (2002), who also analyze the passage of state laws expanding women’s rights. Their work aims to explain why states were motivated to pass the laws and they present a model in which, as markets grow, greater gains from human capital investment makes coverture more costly. They show that states with greater per capita wealth, a greater fraction of city dwellers, and a greater fraction of literate women were more likely to enact such laws. Most relevant to our study are the years of state law passage presented in Geddes (2000), which we use to determine when married women could control both their earnings and their separate property.

Lastly, our work is also related to the work by Bell (2018), who conjecture that there are “lost Einsteins,” or potential innovators who do not innovate due to a variety of barriers.
Their paper analyzes the lack of exposure to innovation during childhood and finds that children who grew up in areas with a high innovation rate are more likely to innovate. We study the lack of property rights and show that not having property rights may have suppressed innovation in the United States. States that granted property and earnings rights to married women had more patents granted to women relative to states that did not.

3.2 Coverture and married women’s property rights

In the 1800s, the practice of coverture meant that married women were under their husband’s authority. During the mid-1800s, states began passing laws that granted married women rights to own property. The first laws that introduced the concept of separate estates for married women were enacted to protect a wife’s property from her husband’s creditors if her husband could not repay his debts. Later laws that granted married women property rights allowed them to own and control property separately from their husbands. States also passed laws allowing married women to keep their labor earnings instead of requiring them to relinquish their earnings to their husband. In addition, some states also passed “sole trader” laws that permitted married women to operate their own businesses and execute contracts.

Patents are a form of property that grant individuals (or firms) the exclusive monopoly rights to an invention. The decision of whether to obtain a patent depends on how great the benefits of exclusive rights are relative to the costs of patenting. An inventor can generate revenue from a patent by commercializing and selling a product using the patent, or by selling the patent to someone who will do so.

Under coverture, married women who obtained a patent could not keep any revenue that the patent generated and would have to relinquish control to her husband. Our study focuses on two types of laws that economists and historians have studied in the context of women’s commercial activity: property rights laws and earnings laws. Property rights laws would have allowed married women to retain ownership of their patents and prevented husbands from selling the patent without their wives’ consent. In addition, having control
over their earnings would have meant that married women could keep any profits generated by the patent.

Laws granting married women the rights to own property and manage her earnings would have, in theory, given wives greater bargaining power over the distribution of household resources. Before the laws, coverture allowed husbands to be the sole decision maker with regard to household resource allocation, because they controlled all of the household’s property and earnings. Therefore, under coverture a woman may not have received all of the return to owning a patent if her husband managed the patent’s profits in a manner that was suboptimal for her. Once she could control and own her own property, she could realize all of the returns to innovating, and thus she might be more likely to apply for a patent. However, it is possible that coverture or the property and earnings laws were irrelevant for the household, and that household decisions were the same regardless of whether the wife or husband owned property or earned income. If so, then these laws would not have had an effect on the incentives for married women to innovate. By examining whether the property and earnings laws affected women’s patenting behavior, our paper is also testing whether external rules regarding household resource distribution matter for the household.

Table 3.1 documents the years in which states passed laws granting married women 1) rights to own separate property and 2) rights to their earnings. The years are compiled from three sources: Geddes (2000), our primary source, as well as Khan (1996) and Hoff (1991). There are several possible reasons why the years do not align across the three sources. First, the years were primarily obtained by examining records of state session laws and noting the earliest year in which married women’s property or earnings rights appeared. It could be that an earlier year was missed by some sources. Second, the laws were written differently for each state, and whether a law granted property and earnings rights may be subject to interpretation. For example, the earliest forms of property rights acts may have allowed married women to hold separate property in order to protect the property from her husband’s creditors, but they were not intended to promote her independence. Finally,
because the data was hand-curated, human error could also have contributed to misaligned years across sources.

The states highlighted in green in Table 3.1 are states in which the sources are consistent within two years of each other. In other words, the latest year for a state in Khan (1996) is within two years of the year in Geddes (2000), and similarly for the latest year in Hoff (1991). Only 19 out of 48 states have years consistent across all three sources. We use the years in Geddes (2000) for our analysis, but we also include results using only states in which years are consistent. For purposes of analyzing time trends

The extent to which the property and earnings laws impacted economic outcomes for women has been studied by historians and economists, with mixed results. Historians have argued that the laws did little to change women’s economic status, due both to social norms that assumed husbands controlled wives’ earnings and also to courts prioritizing marital unity over upholding women’s property rights (Basch, 1979). Women’s labor force participation seems to have increased, but only modestly, upon passage of the property rights laws (Roberts, 2006). Granting married women certain property rights does seem to have affected the types of property married women held; Combs (2005) studies the passage of a similar law in Britain in 1870 to show that married women shifted their wealth holding toward personal property once they were granted control over personal property.

If households pool resources and maximize joint utility, then who controls the household budget is irrelevant, and granting the wife control over any earnings would not cause any changes. In the economics literature, this unitary model of the household has generally been rejected in favor of a model in which the distribution of resources within a household affects household decisions. For example, Thomas (1990) studied families in Brazil and showed that, when women control a family’s unearned income, family health and in particular child survival probabilities improved. Similarly, Duflo (2003) showed that pensions received by women had a positive effect on the weight and height of girls in the household, with no similar effect found for pensions received by men.

Given that it is not just pooled income that determines household decisions, but also
Table 3.1: Years of State Laws

<table>
<thead>
<tr>
<th>State</th>
<th>Geddes &amp; Lueck Property &amp; Earnings</th>
<th>Khan Property</th>
<th>Earnings</th>
<th>Hoff Property</th>
<th>Earnings</th>
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the distribution of resources within a household that matters, granting married women
rights to own separate property and control their earnings would have shifted household
resources to women. The laws should have increased the appeal of owning a patent for
married women, because they could now choose how to spend the associated income. We
would expect that, all else equal, more patents were held by women following passage of
married women’s property and earnings rights laws.

Patenting among women was overall not common during the 19th century, but several
women patented meaningful inventions during this time. Prolific female inventors came
from diverse education and family backgrounds, and they patented in diverse classes.
Lucena M. Morden, the most prolific female inventor in the years 1836 to 1916, was born in
1866 in Nebraska and attended Teachers’ College in Emporia, Kansas (Guest, 2014). She
filed her first patent in 1898, for a “Separate-Leaf Book.” She was granted at least 30 patents
related to notebooks and paper binding. Morden not only patented her inventions but she
also established a plant in Waterbury, Connecticut, to manufacture her patented products.
Morden worked as a stenographer after college, which may have inspired her patents.

Helen Augusta Blanchard received 28 patents related to sewing and sewing machines,
yet she did not receive any technical training. She was born in Portland, Maine to a wealthy
family in 1840, but her family had to sell their property after her father’s business suffered
losses. After his death, Blanchard moved to Boston, where she began patenting inventions
related to sewing, such as the zig-zag stitch sewing machine. She later moved to Philadelphia
and established companies to manufacture her inventions (NIHF, 2006).

Harriet Ruth Tracy also obtained many patents, but there is no record of her attending
college. Tracy was born in South Carolina but moved to New York, where she spent most of
her life. She held at least 27 patents, which were related to cribs, sewing machines, and also
elevator safety. Her sewing machine, which was presented in the 1893 Chicago Columbian
Exposition, was praised by the Chicago Mail, which said “All ladies who see the machine
are delighted with it and her other meritorious inventions are proud that a woman has
accomplished what man failed to do…” (Hughes, 2017).
Margaret Eloise Knight, who was considered a “woman Edison,” received only a basic education but was a productive patentee. Her first invention, which she created at the age of 12, was a loom safety device that was developed after she witnessed an accident while working at a cotton mill. Knight moved to Springfield, Massachusetts at the age of 29 and began working at the Columbia Paper Bag Company. She soon invented a machine that assembled a flat-bottomed brown paper bag, but the design was stolen by a man named Charles Annan, who became aware of the machine while the prototype was being created. Knight filed a lawsuit against Annan and was granted her patent in 1871 (Smith, 2018).

3.3 Data

3.3.1 US Patent & Trademark Office historical patents database

Our primary dataset is based on historical patent data from the US Patent & Trademark Office. We obtain our data from Petralia (2016), who constructed a historical database of patents granted from 1836 to 1975 by digitizing records of patent documents made available by Reed Tech and Google. The digitized patent data contains patent numbers, grant dates, states, and inventor names. Once we identify which inventor names are likely to be associated with a female inventor, we can track the number of patents invented by female inventors at a given state in a given year.

The earliest patent in the data was granted in 1836, and although the data extends to 1975, we restrict our analysis to the years 1836-1916 for two reasons. First, the US became involved in World War I in 1917, and the number of patents that were granted in the following years dropped significantly, as shown in Figure 3.1. Second, by 1920 most states, with the exception of Arizona, Florida, Louisiana, and New Mexico, had adopted laws that granted earnings and property rights to married women.

We exclude Arizona, Florida, Louisiana, and New Mexico from our analysis due to their particularly late or undefined adoption of the laws. The four states passed the property and earnings acts in 1973, 1973, 1980, and an undefined year, respectively. During this time
period, married women’s status was very different from that in the 19th century, and the coverture was no longer practiced. Geddes (2002) also exclude the four states from their paper, as by the time the laws were passed in the four states, women had voting rights and broader access to education, unlike in the 19th century when most other states passed their laws. We also exclude Washington D.C. from our analysis.

3.3.2 Gender classification

We identify which patents were invented by women by using R’s ‘gender’ package to predict the gender of inventors’ names. The package assigns the probability that a name is female based on the gender distribution of people with that name in historical datasets. The package allows the time period and the preferred historical dataset to be specified. We use the gender distribution of first names in the US Census, provided by IPUMS, in the years between 1789 and 1900, to be consistent with the years available in our patent data.

We cross-check our gender classification algorithm with Our algorithm is consistent with the ledger and correctly assigns a high probability of a female inventor to the patents listed in the document. However, it also captures many more female names than are included in the publication. Another advantage to using the gender-prediction algorithm is that it
allows us to analyze all the patents in the USPTO data, and not just the ones invented in the
time period 1790-1895.

We check the performance of the gender classification package by comparing the pack-
age’s calculated probability of a female name for known women inventors in 1790-1895.
We obtain the list of women inventors and their patents from the USPTO, which in 1888
published “Women inventors to whom patents have been granted by the United States
government, 1790 to July 1, 1888,” a historical ledger of women inventors that was later
extended to 1895 with appendices published later (USPTO, 1888). The gender prediction
algorithm calculates over a 95% probability that the inventor’s name is female for most
of these patents. Table 3.2 shows the distribution of the probabilities. About 75% of all
names identified as female by the USPTO have a 95% or greater probability of being female
according to our algorithm. Our algorithm would correctly identify these names as female.
About 16% of female names are not assigned a probability due to a failure to match names,
which could occur due to an unusual name or a misspelling. Around 9% of names are
not assigned a high enough probability according to our algorithm, and we would not
classify these names as female. However, overall the gender assignment algorithm correctly
identifies the majority of names in the USPTO’s list as female names.

Table 3.2: Distribution of Prob(Female Inventor) 1790-1895

<table>
<thead>
<tr>
<th>Probability Female Name</th>
<th>Count</th>
<th>Percentage</th>
</tr>
</thead>
<tbody>
<tr>
<td>0.0-0.9</td>
<td>265</td>
<td>8.6%</td>
</tr>
<tr>
<td>0.9-0.95</td>
<td>23</td>
<td>0.8%</td>
</tr>
<tr>
<td>0.95-0.99</td>
<td>194</td>
<td>6.3%</td>
</tr>
<tr>
<td>0.99-1.00</td>
<td>2,098</td>
<td>68.5%</td>
</tr>
<tr>
<td>Missing</td>
<td>484</td>
<td>15.8%</td>
</tr>
<tr>
<td>Total</td>
<td>3,064</td>
<td>100.0%</td>
</tr>
</tbody>
</table>

This table shows the distribution of the probabilities that the inventor of a patent listed in
the USPTO’s list of women’s patents is a female.

After confirming that the algorithm does capture the names we know to be female, we
then check whether the algorithm assigns a high female probability to other names not
Table 3.3: Distribution of Prob(Female Inventor) 1830-1910

<table>
<thead>
<tr>
<th>Probability Female Name</th>
<th>Count</th>
<th>Percentage</th>
</tr>
</thead>
<tbody>
<tr>
<td>0.0-0.05</td>
<td>854,760</td>
<td>86.4%</td>
</tr>
<tr>
<td>0.05-0.95</td>
<td>48,378</td>
<td>4.9%</td>
</tr>
<tr>
<td>0.95-1.00</td>
<td>12,665</td>
<td>1.3%</td>
</tr>
<tr>
<td>Missing</td>
<td>73,297</td>
<td>7.4%</td>
</tr>
<tr>
<td>Total</td>
<td>989,100</td>
<td>100.0%</td>
</tr>
</tbody>
</table>

This table shows the distribution of the probabilities that the inventor of a patent listed in the USPTO’s list of women’s patents is a female.

Listed in the USPTO’s list of women inventors. There are 1,314 patents with over a 95% probability of a female inventor in the time period 1790-1895 and that were not listed in the USPTO’s document, which lists 3,064 patents by US women during that time. A manual check confirms that the names are female and that the USPTO failed to include them in their publication. Names such as Mary, Elizabeth, and Barbara have a 0.9976 chance of being a female yet in many cases these inventors’ patents are not listed in the USPTO’s directory of female-invented patents.

Possible explanations for why the USPTO’s list did not include all women’s patents include human error and male bias (Hintz, 2017). The list was compiled upon multiple petitions by feminist reformer Charlotte Smith to the USPTO for the publication of an official document containing the names of women inventors. After multiple rejections by the USPTO on the grounds of cost, Smith obtained $300 from Congress to fund the publication. In 1888, four male patent clerks at the USPTO spent ten days combing through over 500,000 existing patents to identify women’s names. It is plausible and highly likely that not all women’s names were identified. Stanley (1987) analyzed all patents granted in 1876 and compared the women’s patents she identified with those identified by the USPTO. She shows that almost one-fifth of the patents she identified were not included in the USPTO’s publication. Furthermore, some of the women’s patents that are not included were related to military or industrial technologies, while the patents that were included tended to be
for domestic or household purposes. Stanley (1987) suggests that male bias of the patent clerks who were in charge of identifying women’s patents could have led them to attribute industrial patents to men. This bias could have contributed to the incompleteness of the USPTO’s list.

Based on the algorithm’s validity in correctly predicting known women inventors and its ability to better identify female inventors than the USPTO, we opt to use the package’s classification of male and female inventors instead of the USPTO’s published list. We categorize an inventor as female if the name is assigned a 0.95 probability or greater of being a female name.

The laws that we study applied to married women, but we do not have data on marital status, so we analyze patenting by both married and unmarried women. Khan (1996) obtained information from city directories for about 900 female patentees and found that very few were single (about 14%), about 30% were married or previously married, 26% were widows, and the remainder had unknown marital status. We do not obtain similar information but find Khan (1996)’s work to be evidence that married women were indeed patenting during the time period of our study. If the laws did increase married women’s patenting, then the patenting rate by all women should also have increased, assuming that unmarried women did not experience a decrease in patenting rates. Including unmarried women thus attenuates our estimates, and we believe that our estimated effects might be understated relative to what we would estimate using data on married women only. Of the women whose marital status was identified by Khan (1996), about \( \frac{14}{14+30+26} = 20\% \) were unmarried, which suggests that our results could be underestimated by 20% as well.

### 3.3.3 Descriptive statistics

Figure 3.2 shows the total number of patents granted to female inventors in each state from 1836 to 1916, sorted from most to fewest. Figure 3.2a shows the raw number of patents and Figure 3.2b shows the number of patents per capita. States with larger industrial cities and that passed the property and earnings laws earlier, such as New York, Illinois, and
Pennsylvania, have the most total patents by female inventors during our time period. On a per capita basis, however, other states such as Colorado and Washington rank high. Overall the female patenting rate was low; there was less than one patent granted to a female inventor per 1,000 people living in a state during the 80 year span between 1836 and 1916.

Figure 3.2A. Number of Patents Granted to Women From 1836 to 1916

Figure 3.2B. Patents Granted to Women Per Capita From 1836 to 1916

Figure 3.2: Distribution of Female Patents by State

Figure 3.3 shows the distribution of total number of female inventors’ patents granted to each state in each year between 1836 and 1916. For many states in many years, the number of patents awarded to female inventors was zero, resulting in a distribution highly skewed right. Taking a log transformation does not solve the skewness, although it does reduce
the dispersion. Based on the highly skewed, often zero-outcome nature of the data in our sample, our preferred specification in our regressions is a Poisson regression.

Table 3.4 shows the patent classes with the most inventions by female inventors between 1836 and 1916. Panel (a) shows the classes with the highest number of patents by female and male inventors and Panel (b) shows the classes with the highest percentage of patents by female inventors. Classes are based on the United States Patent Classification system (USPC). As shown in Panel (a), women tended to patent primarily in classes related to apparel, such as foundation garments, buckles and clasps, and toilet (i.e., toiletries and cosmetics),

Figure 3.3: Distribution of Patents Awarded to Female Inventors
as well as in classes related to the household, such as stoves and furnaces and general cleaning instruments. However, many men also patented in similar classes such as buckles and clasps, stoves and furnaces, and general cleaning instruments. When considering the highest percentage of patents with a female inventor, as done in Panel (b), women still had the most representation in foundation garments (36%), toilet (14%), and apparel (11%), but also in wetting agents (11%) and education (8%).

3.4 Empirical results

3.4.1 Main regression and event study

We estimate the following regression:

$$WomensPatents_{it} = \beta \times LawsPassed_{it} + \delta_i + \eta_t + \epsilon_{it}$$

This specification relates the number of women’s patents in state $i$ in year $t$ on an indicator for whether a state had both property rights and earnings rights for married women in that year. State fixed effects $\gamma_i$ absorb differences across states that are constant during our sample’s time period, and year fixed effects $\delta_t$ account for yearly differences that are common across states. Standard errors are clustered at the state level. We use a Poisson specification to model the count of women’s patents.

We also estimate an event study specification around the year by which a state had the two types of married women’s property acts in place. The event study allows us to see how women’s patenting changed over time before and after the laws were enacted. We use the following event study specification:

$$Patents_{it} = \sum_{k=-20}^{20} \lambda_k 1 (years \ relative \ to \ enactment = k)_{it} + \delta_i + \eta_t + \epsilon_{it}$$

where $i$ refers to a state and $t$ refers to a year. We estimate this event study specification only on the states for which the years in which the laws became effective are consistent across Geddes (2000), Khan (1996), and Hoff (1991). Figure 3.4 plots the coefficients $\lambda_k$ from this
**Table 3.4: Patent Classes with the Most Female Inventors**

**Table 3.4A. Classes with the Most Patents by All Inventors**

<table>
<thead>
<tr>
<th>Class</th>
<th>Female Inventors</th>
<th>Number of Patents</th>
<th>Male Inventors</th>
<th>Number of Patents</th>
</tr>
</thead>
<tbody>
<tr>
<td>Apparel</td>
<td>1,090</td>
<td>Buckles, buttons, clasps, etc.</td>
<td>19,640</td>
<td>742 Machine element or mechanism</td>
</tr>
<tr>
<td>Buckles, buttons, clasps, etc.</td>
<td>742</td>
<td>Machine element or mechanism</td>
<td>18,618</td>
<td>402 Earth working</td>
</tr>
<tr>
<td>Stoves and furnaces</td>
<td>402</td>
<td>Earth working</td>
<td>13,662</td>
<td>338 Fluid handling</td>
</tr>
<tr>
<td>Beds</td>
<td>338</td>
<td>Fluid handling</td>
<td>13,646</td>
<td>297 Stoves and furnaces</td>
</tr>
<tr>
<td>Brushing, scrubbing, and general cleaning</td>
<td>297</td>
<td>Stoves and furnaces</td>
<td>13,618</td>
<td>285 Static structures (e.g., buildings)</td>
</tr>
<tr>
<td>Foundation garments</td>
<td>285</td>
<td>Static structures (e.g., buildings)</td>
<td>10,876</td>
<td>258 Brushing, scrubbing, and general cleaning</td>
</tr>
<tr>
<td>Baths, closets, sinks, and spitoons</td>
<td>258</td>
<td>Brushing, scrubbing, and general cleaning</td>
<td>10,593</td>
<td>250 Miscellaneous hardware (e.g., bushing, carpet fastener, caster, etc.)</td>
</tr>
<tr>
<td>Geometrical instruments</td>
<td>250</td>
<td>Miscellaneous hardware (e.g., bushing, carpet fastener, caster, etc.)</td>
<td>9,844</td>
<td>207 Cutting</td>
</tr>
<tr>
<td>Foods and beverages: apparatus</td>
<td>207</td>
<td>Cutting</td>
<td>9,427</td>
<td>206 Harvesters</td>
</tr>
<tr>
<td>Toilet</td>
<td>206</td>
<td>Harvesters</td>
<td>9,387</td>
<td></td>
</tr>
</tbody>
</table>

**Table 3.4B. Classes with the Highest Percentage of Inventions by Female Inventors**

<table>
<thead>
<tr>
<th>Class</th>
<th>Percent Female</th>
</tr>
</thead>
<tbody>
<tr>
<td>Foundation garments</td>
<td>36%</td>
</tr>
<tr>
<td>Toilet</td>
<td>14%</td>
</tr>
<tr>
<td>Apparel</td>
<td>11%</td>
</tr>
<tr>
<td>Colloid systems and wetting agents; subcombinations thereof; processes of</td>
<td>11%</td>
</tr>
<tr>
<td>Education and demonstration</td>
<td>8%</td>
</tr>
<tr>
<td>Apparel apparatus</td>
<td>8%</td>
</tr>
<tr>
<td>Drug, bio-affecting and body treating compositions</td>
<td>7%</td>
</tr>
<tr>
<td>Textiles: ironing or smoothing</td>
<td>7%</td>
</tr>
<tr>
<td>Trunks and hand-carried luggage</td>
<td>7%</td>
</tr>
<tr>
<td>Cleaning and liquid contact with solids</td>
<td>6%</td>
</tr>
</tbody>
</table>

This table shows the patent classes with the most female inventors for patents granted between 1836 and 1916. Panel (a) shows the classes with the highest number of patents by female inventors and Panel (b) shows the classes with the greatest percentage of patents by female inventors.
regression. Figure 3.4a shows the results for number of patents granted to female inventors and Figure 3.4b shows the results for number of patents granted to male inventors. There is a slight upward pre-trend in the rate of patenting by male inventors prior to the laws, and a plateau thereafter. However, for patents granted to female inventors there is no discernable upward trend prior to the laws becoming effective, and there is an increase (albeit not very statistically significant) after the laws are passed. Relative to the male inventors, female inventors were granted more patents once married women received property and earnings rights.

Figure 3.4A. Number of Patents Granted to Female Inventors

Figure 3.4B. Number of Patents Granted to Male Inventors

Figure 3.4: Event Study around Passage of Property Rights Laws
Table 3.5: Married Women’s Property Rights and Patents

<table>
<thead>
<tr>
<th></th>
<th>All Years 1836-1978</th>
<th>30 Years</th>
<th>20 Years</th>
<th>10 Years</th>
</tr>
</thead>
<tbody>
<tr>
<td>Laws Passed</td>
<td>1.3674* (0.2380)</td>
<td>1.0978</td>
<td>1.0712</td>
<td>1.0721</td>
</tr>
<tr>
<td>State FE</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>Year FE</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>N</td>
<td>3564</td>
<td>2466</td>
<td>1705</td>
<td>886</td>
</tr>
<tr>
<td>Pre-Period Mean</td>
<td>.459</td>
<td>.616</td>
<td>.764</td>
<td>1.1</td>
</tr>
</tbody>
</table>

* p <0.1; ** p < 0.05; *** p < 0.01.

Table 3.5 shows the regression results. We present results using all years as well as results using data in 30, 20, and 10 year windows before and after law enactment. The estimates shown are exponentiated coefficients, or incidence-rate ratios. They can be interpreted as the factor by which patenting by female inventors increased. In other words, a coefficient of 1.37, as shown in the first column, suggests that the laws increased female patents by 37%. Overall, the laws seemed to have had a positive effect, although when using fewer years of data the statistical significance decreases.

Although we estimate a positive effect of the laws on patenting by women, we note that there was very little patenting by women before the laws were implemented. On average, there were only a handful of patents in each state in each year. For example, the regressions on the 20-year periods before and after the laws show a 7% increase in the number of patents by women, but relative to a pre-period mean of 0.8. This means that, on average, a state passing the property and earnings rights laws saw an additional $0.8 \times 7\% \times 20 \approx 1$ patent by a female inventor in the 20 years following the laws.

3.4.2 Subset of states with consistent enactment years

We also show results from limiting the analysis to states in which the event year (i.e., the year by which both property rights and rights to earnings were granted to married women)
Table 3.6: Married Women’s Property Rights and Patents (Subsample of States with Consistent Enactment Years)

<table>
<thead>
<tr>
<th></th>
<th>All Years 1836-1978</th>
<th>30 Years</th>
<th>20 Years</th>
<th>10 Years</th>
</tr>
</thead>
<tbody>
<tr>
<td>Laws Passed</td>
<td>1.8624***</td>
<td>1.2887***</td>
<td>1.1204</td>
<td>0.9290</td>
</tr>
<tr>
<td></td>
<td>(0.3570)</td>
<td>(0.1225)</td>
<td>(0.0946)</td>
<td>(0.3963)</td>
</tr>
<tr>
<td>State FE</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>Year FE</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
<td>Y</td>
</tr>
<tr>
<td>N</td>
<td>1372</td>
<td>975</td>
<td>677</td>
<td>348</td>
</tr>
<tr>
<td>Pre-Period Mean</td>
<td>.242</td>
<td>.339</td>
<td>.47</td>
<td>.857</td>
</tr>
</tbody>
</table>

* p < 0.1; ** p < 0.05; *** p < 0.01.

listed in Khan (1996) and in Hoff (1991) is within two years of the event year listed in Geddes (2000). We determine the subset of states with “consistent” years as follows. First, for the years listed in Khan (1996) and Hoff (1991), we calculate for each state the later of the property rights year and the earnings rights year. This later year is the year by which both rights were granted to married women, which is the equivalent of what is listed in Geddes (2000). We then calculate the difference between Khan (1996) and Geddes (2000) and the difference between Hoff (1991) and Geddes (2000). If both of the differences are two years or smaller, we consider the state to have consistent years across sources.

We re-estimate the regressions shown in Table 3.5 for the subsample of states with consistent years. For each state, we only include the years before the earliest year across the three sources and the years after the latest year across the three sources. We remove the years in between to alleviate ambiguity over when the rights were or were not in place. The results of these regressions are shown in Table 3.6. The results are similar to those in Table 3.5.
3.5 Contemporary international evidence

To further support our claim that excluding women may hinder a country’s innovation and growth prospects, we show the positive correlation between women’s economic participation in a country and the country’s innovation. Figure 5a plots the positive relationship between women’s labor force participation and a country’s Global Innovation Index in 2018. The Global Innovation Index (GII) is meant to measure a country’s innovative activity and the extent to which the country enables innovation. More innovative countries, as measured by the GII, have more women participating in the economy. Even after controlling for women’s education level and their share of engineering and science degrees, which may be correlated with both a country’s innovativeness and women’s labor force participation, we show that the positive relationship survives as seen in Figures 5b and 5c.

While we do not interpret these figures causally, there seems to be growing evidence that women’s economic participation is critical for growth, which has led to an increase in efforts to encourage women’s labor force participation and innovation. UN Women created the Global Innovation Coalition for Change (GICC) in 2017, with the goal of identifying and addressing barriers to innovations by women, and in particular innovations by women for other women (UNWomen, 2019). Prior research has also shown that innovation can in turn improve women’s outcomes through health-related advances, products that reduce housework loads, and increased employment opportunities (Follett, 2018).

Relatedly, recent literature has hypothesized, and also found evidence, that including women and other minority groups in leadership positions leads to improved performance. For example, Hong (2004) theorize a model in which a group of randomly selected agents from a diverse population outperforms a group of the highest ability agents due to the benefits of functional diversity (i.e., diversity in how people solve problems). Empirically, Woolley (2010) show that groups with a higher proportion of females outperform other groups, even when taking into account the average intelligence or maximum individual intelligence in a group. Hunt (2015) present additional evidence supporting the theory that diversity can improve performance by showing that companies in the top quartile for gender
Figure 3.5: Female Share of Employment vs. Global Innovation Index

The Global Innovation Index (GII) is a score of countries’ innovativeness. A higher score suggests a more innovative country. Female Labor Force Share comes from the World Bank and is in percentage (i.e., 10 denotes 10 percent of the labor force is female).
or racial diversity generate returns above their national industry medians. Collectively, these studies support the burgeoning interest in increasing women’s participation in both the labor force and in the arena of innovation.

3.6 Conclusion

Until the mid-nineteenth century, married women in the United States did not have the rights to own their own property or keep any wages they earned. Our analysis suggests that this lack of economic rights may have inhibited the rate of patenting by women. Although historical, our research may still be relevant today. Many developing countries marginalize not only women but other subpopulations from participating in economic activities, much like the nineteenth century United States. Our findings suggest that countries that oppress select groups of people may also oppress their economic growth.

Women were more likely to patent in certain classes such as apparel, toiletries, and education, as shown in Table 3.4. Although we did not show this formally, the fact that men and women tended to patent more in different areas suggests that women brought new innovations that may not have happened otherwise if only men innovated. It is also possible that property and earnings rights increased the buying power of married women, which could have affected the demand for certain types of goods and also influenced the types of innovations that occurred.

Coverture is no longer in practice, and property rights and earnings rights are now universal in the US. However, women are still underrepresented among patent-holders. In 2016, only 12% of all patent inventors were women, and women are less likely to patent than men even among science and engineering degree holders. Despite the extension of property rights contributing to more patenting by women, other barriers still remain that lead to relatively low women’s patenting rates. Our paper investigates a barrier that, although not in place in the US, does still exist in other countries. However, what contributes to persistent underrepresentation of women among patent holders in the US remains an open question.
References


USPTO, 1888. Women inventors to whom patents have been granted by the United States govern ment, 1790 to July 1, 1888 [and Appendix no. 1-2, July 1, 1888 to March 1, 1895], Washington Government Printing Oce.


Appendix A

Appendix to Chapter 1

A.1 Survey of Inventors

A.1.1 Survey Questions

My survey has a total of four questions. In the first question, I ask “When thinking about an ideal job, how important is each of the following factors to you?” Respondents rate the importance of three attributes on a 5-point scale (1 = not at all important, 5 = extremely important). The three attributes are “Income (e.g. salary and bonus),” “Contributing to the society through my R&D projects,” and “Freedom to pursue interesting R&D projects.” This question mirrors existing survey studies (Sauermann and Cohen, 2010; Sauermann and Roach, 2014) and elicits inventors’ preference for innovation.

In the second question, I ask “When thinking about the future, how important would you find the following kinds of work?” Respondents rate the importance of three attributes on a 5-point scale (1 = not at all important, 5 = extremely important). The three attributes are “Basic research that contributes to fundamental insights or theories,” “Applied research that creates knowledge to solve practical problems,” and “Commercializing research results into products and services.” This question is to test whether certain inventors (e.g., those who cite academic research) have a stronger preference for innovation.

In the third question, I ask “When thinking about an ideal CEO, how important is each
of the following factors to you?” Respondents rate the importance of four attributes on a 5-point scale (1 = not at all important, 5 = extremely important). The four attributes are “CEOs have formal education in relevant science or engineering studies,” “CEOs have formal education in business and commercialization,” and “CEOs understand how to motivate and work with R&D teams,” “CEOs believe in my R&D projects and will tolerate a few negative preliminary results.” This question is to directly assess inventors’ preference for CEOs.

In the last questions, I ask “Please describe your ideal CEO.” Inventors are free to describe their ideal CEOs in 30 words.

A.1.2 Survey Sample and Distribution

Ideally, I survey inventors in my sample. But my sample time period goes back in decades, and I cannot locate many of their contact information on the Internet. Therefore, I survey more recent inventors. I start with a random sample of 1,500 inventors who have filed patents in 2013. After manual searching, I find 1,039 inventors’ LinkedIn profiles. I friend-request them all, wait for 30 calendar days, and distribute my survey. 245 accept my friend requests. With LinkedIn’s premium account (Recruiter Lite), I find that 105 view my profile but do not accept friend requests. I send my survey by free messages to those 245 friends and by premium inMails to those 105 non-friends. My survey lasts for 45 calendar days after the distribution. Out of 350, 48 (13.7%) have completed my survey. Two survey participants are given $100 Amazon.com Giftcards.

A.1.3 Potential Survey Biases

Nonresponse Bias

We may be concerned about a potential nonresponse bias: survey respondents and nonrespondents may have different opinions, and this difference can overturn my survey results. This issue deals with missing data, so there is no complete solution. But I have two sets of tests to suggest the bias is unlikely. The first set of tests comes from comparing inventor characteristics used in this paper between respondents and non-respondents. I find no
meaningful statistical differences between the two groups: two smallest p-values from the t-tests are 0.089 and 0.28, and two smallest p-values from the Komologrov-Smirnov tests are 0.11 and 0.32. The second set of tests come from comparing answers to my survey questions between early respondents and late respondents. The idea behind this comparison is to think of nonrespondents as extremely late respondents (i.e., they will almost surely complete my survey given infinite time) who may be more similar to late respondents than to early respondents. To implement this idea, I separate respondents to two equally sized groups based on their survey completion dates. I find no meaningful statistical differences between the two groups: two smallest p-value from the t-tests are 0.09 and 0.14, and two smallest p-values from the Komologrov-Smirnov tests is 0.056 and 0.68.

Social Desirability Bias

We may be concerned about a potential social desirability bias: survey respondents may want to present themselves in a positive or socially acceptable way. In the context of our survey, an important concern is that this bias may lead respondents to answer they value innovation-related job attributes and respect other engineers and scientists (STEM CEOs) more than they actually do. To help elicit truthful responses, I have administered the survey in an anonymous manner and informed the participants that neither their identity nor their individual responses will be revealed.

A.2 Matching Patent Data with COMPUSTAT and VentureXpert

A.2.1 Name Standardization

I standardize the firm names in the USPTO patent data, COMPUSTAT, and VentureXpert with the name standardization routines developed by the NBER Patent Data Project. The routines standardize common firm prefixes and suffixes (e.g., Inc.) as well as identifying a firm’s stem name excluding the prefixes and suffixes.
A.2.2 The Matching Procedure

I use the following matching procedures:

1. Each standardized firm name in COMPUSTAT and VentureXpert is matched to standardized names in the patent data. Keep only the exact matches.

2. For the remaining unmatched names in COMPUSTAT and VentureXpert, each stem name associated with a firm is matched with stem names in the patent data. If an exact match is found and geographical states match as well, then keep the matches.

3. For the remaining unmatched names in COMPUSTAT and VentureXpert, each stem name associated with a company name is matched with up to 10 close stem names in the patent data using Levenshtein distance less than 0.05. Keep the matches if geographical states match as well.

4. Hand-check all the matches from the step 3.

A.3 Illustration of Text Similarity Measures

I illustrate the text similarity measures. Consider these three short texts:\(^1\)

\[ T_1 : \text{The company focuses on technology and new products.} \]
\[ T_2 : \text{The company focuses on innovation and new products.} \]
\[ T_3 : \text{The company focuses on finance and financial engineering.} \]

Because \( T_1 \) and \( T_2 \) are both about new products while \( T_3 \) is about finance, I expect the similarity measures to be higher between \( T_1 \) and \( T_2 \) than between \( T_1 \) and \( T_3 \). The union of \( T_1 \) and \( T_2 \) is:

\[ T_1 \cup T_2 = \{ \text{the, company, focuses, on, technology, and, new, products, innovation} \} \]

\(^1\)In this example, I do not stem any word.
The term frequency vectors are:

$$TF_1 = [1, 1, 1, 1, 1, 1, 1, 0]$$

$$TF_2 = [1, 1, 1, 1, 0, 1, 1, 1]$$

The cosine similarity is:

$$Cosine_{1,2} = \frac{TF_1 \cdot TF_2}{||TF_1|| \times ||TF_2||} = \frac{1 + 1 + 1 + 0 + 1 + 1 + 0}{\sqrt{8} \times \sqrt{8}} = 0.875$$

The intersection of $T_1$ and $T_2$ is:

$$T_1 \cap T_2 = \{\text{the, company, focuses, on, and, new, products}\}$$

The Jaccard similarity is

$$Jaccard_{1,2} = \frac{|T_1 \cap T_2|}{|T_1 \cup T_2|} = \frac{7}{9} = 0.778$$

The union of $T_1$ and $T_3$ is:

$$T_1 \cup T_3 = \{\text{the, company, focuses, on, technology, and, new, products, finance, financial, engineering}\}$$

The term frequency vectors are:

$$TF_1 = [1, 1, 1, 1, 1, 1, 1, 0, 0, 0]$$

$$TF_3 = [1, 1, 1, 1, 0, 1, 0, 1, 1, 1]$$

The cosine similarity is:

$$Cosine_{1,3} = \frac{1 + 1 + 1 + 0 + 1 + 0 + 0 + 0 + 0 + 0}{\sqrt{8} \times \sqrt{8}} = 0.625$$
The intersection of $T_1$ and $T_3$ is:

\[ T_1 \cap T_3 = \{\text{the, company, focuses, on, and}\} \]

The Jaccard similarity is

\[ Jaccard_{1,3} = \frac{5}{11} = 0.455 \]

The similarity measures between $T_1$ and $T_2$ are higher than those between $T_1$ and $T_3$, as expected.