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By

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Abstract

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This dissertation comprises three studies that examine competition, public policy, and innovation. The first study investigates how product market competition affects the intensity and breadth of innovation activities of firms, using the formation and breakup of price fixing cartels to proxy for competition or lack thereof. The second and third studies investigate how public policy on restrictive covenants, namely non-compete agreement, affects business dynamism/concentration and strategic knowledge management of firms, respectively.

For the first study, I assembled a unique dataset comprising 461 prosecuted cartel cases in the U.S. from 1975-2016, where I match 1,818 collusive firms to firm-level data on patenting, R&D investment, and other measures of innovation. I then use a difference-in-difference methodology, matching colluding firms to various counterfactual firms. Empirical results show a negative causal relationship between competition and innovation in the cartel context. When collusion suppressed market competition, colluding firms increased R&D investment by 12%, patenting by 51%, and top-quality patents by 20%. Furthermore, firms also broadened their areas of innovation when competition was suppressed by collusion, with the number of patented technology fields increasing by 33%. The increased and broadened innovation activities reverted back, close to previous levels, when competition was restored by collusion breakup. Further tests suggest that financial constraint ("ability to innovate") and the industry's growth rate ("incentive to innovate") are important economic mechanisms behind the trade-off between price competition in the product market and innovation growth.

I then turn to labor market competition and study the effects of legal enforcement of non-competition agreements on business activities, including entrepreneurship and innovation. The second study, co-authored with Lee Fleming, isolates the impact of non-compete enforcement on regional business dynamism and concentration by focusing on Florida's 1996 legislative change that eased restrictions on the enforcement of non-competes. We

first establish the contrast between legal regimes and note that wage trends did not change when comparing wages before and after the passage of the legislation. Difference-in-differences models show that following the change, establishments of large firms were more likely to enter Florida; these firms also created a greater proportion of jobs and increased their share of employment in the state. Entrepreneurs or establishments of small firms, in contrast, were less likely to enter Florida following the law change; they also created a smaller proportion of new jobs and decreased their share of employment. Consistent with these location and job creation dynamics, a variety of business concentration measures increased significantly following the law change in Florida. Nationwide cross-sections demonstrate consistent correlations between state-level non-compete enforcement and business dynamism/concentration dynamics illustrated in Florida.

Expanding the questions on the impact of labor market competition and worker mobility, the third study examines how firms strategically manage innovation processes and outcomes against mobility of workers (this work is co-authored with Wyatt Lee). In 1998, a California Court of Appeal ruled that non-compete agreements ("non-competes") signed by an employer and an employee outside of California are not enforceable in California. This court decision created a loophole for employees of non-California firms, as these firms could no longer enforce non-competes by which their employees were previously bound, and the employees could now move freely to California firms. We use a difference-in-difference methodology comparing firms in states that have been enforcing non-competes with firms in states that have not been enforcing non-competes. We find that this California-driven loophole significantly affected knowledge management and innovation processes of non-California firms that are affected. These firms decreased R&D investments (an important input for innovation) because such R&D activities became more costly and risky. On the other hand, these firms increased patent filings (without compromising their quality) despite the decrease in innovation input. In other words, firms increased their propensity to patent, suggesting that firms rely more on strategic patenting than on secrecy when facing higher mobility of and competition for workers.

To My Family

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1 How Does Competition Affect Innovation? Evidence from U.S. Antitrust Cases

"The incentive to invent is less under monopolistic than under competitive conditions" (Arrow, 1962, p. 619)

"A monopoly position is in general no cushion to sleep on. As it can be gained, so it can be retained only by alertness and energy" (Schumpeter, 1942, p. 102)

1.1 Introduction

Innovation is considered an engine of economic growth and welfare (Schumpeter, 1934). Innovation brings new technologies and products to markets, benefiting consumers, producers, and society at large. It is therefore important to promote innovative activities of firms. R&D and the innovation processes, however, require risky and uncertain investment, the returns on which take several years, if not decades, to reap. Furthermore, the social return on investment in R&D and innovation is much higher than its private value (Griliches, 1992; Bloom *et al.*, 2013) because firms may fail to internalize the broader impact of their innovation activities under the presence of technology spillovers (or positive externalities). These two features of innovation lead to underinvestment in R&D and under provision of innovation. It is therefore necessary to understand firms' incentives and ability to innovate in order to promote their innovation activities.

Another source of social benefit is healthy competition, which keeps prices low and production efficient. However, there has been a long-standing debate on the role of competition in innovation. One approach argues that competition promotes innovation activities of firms (e.g., Arrow, 1962). On the other hand, motivated by the insights of Schumpeter (1942), another body of work argues that a certain amount of market power can promote innovation – more than would be achieved in a competitive market – by providing firms with the financial resources and predictability required for innovative activities. The so-called "competition-innovation debate" confirms that competition and innovation have a strong relationship, but no consensus exists on its direction. Given this theoretical ambiguity, it is particularly important to empirically study which of the two opposing effects dominates and what the mechanisms are. These empirical findings also contribute to the extant theoretical debates.

This paper examines the effects of market competition on a firm's ability and incentives to innovate. Put differently, how do firms change their intensity and breadth of innovation in response to market competitiveness? The critical obstacle to empirical studies in this field is that competition and innovation are endogenously determined – changes in competition may be correlated with unobservable factors that also affect innovation. In addition, firms that are successful in innovation gain market power, implying a reserve causality. These are the reasons why there have been a limited number of systematic, large-sample studies demonstrating a causal relationship between competition and innovation (Cohen and Levin, 1989; Sidak and Teece, 2009, p. 588).

I address these challenges by exploiting variations coming from price-fixing cartels. The formation and breakup of price-fixing cartels provide an ideal, novel setting to proxy for competition, or lack thereof. The *formation* of collusion suppresses market competition because the primary purpose of a cartel is to eliminate competition and to raise prices. The *breakup* of collusion, in turn, terminates the conspiracy to suppress competition and therefore increases market competitiveness; this is indeed the key mission of the DOJ's antitrust enforcement (https://www.justice.gov/atr/mission). I collected and digitized data on all known cartel cases in the U.S. from 1975-2016 rather than focusing on a single collusion case. As a result of this effort, a total of 461 (non-financial) cartel cases – along with 1,818 firms and 1,623 managers – are identified.

In addition, a review of the literature reveals that existing theories – and also empirics – of innovation assumed that innovative activities fall along a unidirectional continuum. An important question that has received relatively little attention is how competition changes the qualitative characteristics of innovation. Taking a step beyond the intensity of innovation, I explore the *types* and *breadth* of innovation, or how firms change their area(s) of innovation in response to market competition. Since the nature of innovation is a mixture or recombination of existing technologies, it is important that firms explore new technologies and use several ingredients in their innovation processes. The different types of – or broader – innovation also build a firm's absorptive capacity to identify, assimilate, and apply such knowledge ingredients (Cohen and Levinthal, 1990). Competitive pressure should change whether firms conduct basic versus applied research and whether they explore new fields that are not directly related to their current area of innovation. Making this distinction between the intensity and breadth of innovation could lead to a better understanding of the "creative destruction" processes (Schumpeter, 1942) and may reconcile the competing views on the relationship between competition and innovation.

Using a difference-in-difference methodology and matching colluding firms to carefully defined counterfactual firms, I find a negative causal relationship between cartel-induced competition and innovation. When a cartel suppressed market competition, colluding firms increased R&D investment by 10-16% and patenting by 41-62%. I also find evidence that the breadth and types of innovation change in parallel. With decreased competition, firms broadened their areas of innovation by 18-38%. The increased and broadened innovation activities reverted back to their previous levels when competition increased due to a cartel's breakup. Further tests suggest that financial constraint ("ability to innovate") and the industry's growth rate ("incentive to innovate") are important economic mechanisms behind the trade-off between price competition and innovation growth. The industry-wide aggregate effects (for both colluding and non-colluding firms) show a similar pattern to that of colluding firms alone, though smaller in magnitude, suggesting that these colluding firms drove the overall industry-wide outcomes.

These findings shed new light on the nature of the relationship between market competition and innovation. I find an interesting strategic shift that firms move toward innovation competition when price competition weakens. With a careful interpretation and application of the results, this finding has particularly important implications for competition policy, namely that promoting price competition may not be a one-size-fits-all solution, and its anti-innovation effects merit further consideration. The relationship between collusion-driven competition and innovation is also relevant for the growing literature on how market competition is associated with international trade and with mergers and acquisitions (M&As), and how each affects firm innovation (e.g., Autor *et al.*, 2013, 2017; Miller and Weinberg, 2017).

1.2 Market Competition and Innovation

1.2.1 Intensity of Innovation

A longstanding debate exists on which market structure provides the incentives and ability for businesses to innovate ("the competition–innovation debate"). Arrow (1962) argues that monopolistic firms do not have an incentive to invest in innovation activities because these firms already enjoy high excess profits (mark-ups), so the marginal benefit of engaging in risky and uncertain R&D projects is low. Firms in a highly competitive market, on the other hand, should pursue innovation to survive and to achieve competitive advantage and thereby outperform their competitors. The U.S. DOJ and European Commission take this standpoint that "one of the best ways to support innovation is by promoting competition" (European Commission, 2016).

A model by Lefouili (2015) shows that the intensity of (regulator-induced) yardstick competition increases the incentives to invest in (cost-reducing) innovations. Several empirical studies support this view. Correa and Ornaghi (2014) find a positive relationship between innovation and foreign competition, measured by patents, labor productivity, and total factor productivity of publicly traded manufacturing firms in the U.S. A reduction in tariffs – which promotes international competition – contributed to productivity growth in the manufacturing sector of Brazil (Schor, 2004) and for trading firms in China (Yu, 2015), respectively. Another interesting setting for studying the effects of competition on innovation is a patent pool, where two or more patent owners agree to pool a set of their patents and license them as a package (Lerner and Tirole, 2004). A patent pool can reduce technological competition among pool members. Lampe and Moser (2010) find that patent pools in the 19th-century sewing machine industry decreased patenting intensity of pool members. Interestingly, another measure of productivity – sewing machine speeds – barely changed during the pool period and then increased after the pool was dissolved. Lampe and Moser (2016) again find that patent pools decreased patenting intensity and citations across twenty industries. An important mechanism behind this relationship is that patent pools weakened competition in R&D, which in turn decreased innovation output. In the context of the global optical disc industry, Joshi and Nerkar (2011) find that patent pools – interpreted as a unique form of R&D consortia – decreased both the quantity and quality of patents of the pool member firms.

Schumpeter (1942), on the other hand, argues that market power can promote innovation. R&D and innovation activities require a large amount of fixed investment and a long-term, risk-taking orientation. This can only be achieved when firms have an ability and incentives to innovate. Fierce competition in the market restricts a firm's *ability* to innovate, because lower prices and profit suggest firms have fewer financial resources that can be allocated to innovation processes. Reduced competition, on the other hand, suggests that firms set prices higher than marginal cost and enjoy higher profits, providing financial resources for innovation (Schumpeter, 1942; Cohen and Levin, 1989). Several empirical studies support this view. Macher *et al.* (2015) studied how cement manufacturers adopt new cost-saving technology at different levels of market competition. Although these manufacturers understand the effectiveness of new technology in reducing costs, their adoption pattern differed depending on market competitiveness. Adoption was indeed higher under low levels of market competition. Gong and Xu (2017) studied how Chinese import competition changed R&D reallocation of publicly traded manufacturing firms in the U.S. and find that (1) competition decreased R&D expenditures and (2) R&D investment was reallocated toward more profitable firms within each sector. This suggests that competition hampers a firm's ability to engage in R&D and innovation activities by reducing its profits (or slack resources).

Reduced competition could also provide *incentives* for innovation in three ways. First, reduced competition increases a firm's probability of survival and makes the behavior of competitors more visible and predictable, which enables firms to more confidently invest in long-term R&D projects. Since R&D projects and innovation processes take several years, if not decades, it is important that firms anticipate that they can survive and reap the gains from innovation ("Schumpeterian rents"). Second, firms expect higher returns from innovation (or appropriability) when there are fewer firms competing with each other. This provides additional incentives for innovation (Cohen and Levin, 1989; Schumpeter, 1934). Put differently, no market power lasts forever. With this dynamic view on market competition, even monopolists have an incentive to invest in R&D in the current period to sustain their competitive advantage and profits in future periods. Several empirical studies support this view. Im *et al.* (2015) find in the U.S. manufacturing sector that a firm's incentive to innovate increases in response to tariff cuts when market competition is mild (and the incentive decreases when firms face fierce market competition). Hashmi (2013) finds a negative relationship between market competition and citation-weighted patenting of publicly traded manufacturing firms in the U.S. Autor *et al.* (2017) also find that competitive pressure by Chinese imports decreased R&D expenditure and patenting of U.S. manufacturing firms. The evaluation of R&D by financial markets is also consistent with these findings; investors expect R&D to offer them greater returns when firms face lower competition (Greenhalgh and Rogers, 2006). Third, reduced competition could prevent duplicate R&D investment, reducing a preemption risk and waste of resources on developing the same technology. A concern that competing firms will preemptively patent or commercialize new technology impedes firms' investment in new R&D projects. Reduced competition significantly decreases such concern or risk because it is easier to monitor or communicate with other firms in the market. This effect is magnified when it comes to cartels in which competing firms coordinate and monitor each other's production levels and pricing.[1]

Studies that embrace the competing views bring in a non-monotone relationship between market competition and innovation (Loury, 1979). Using privatization of public firms and other industry-wide changes in the regulatory regime, Aghion *et al.* (2005) find an inverted U-shaped relationship between competition and patenting behavior of U.K. firms in the U.S. In line with this finding are a formal model developed by Boone (2001) and empirical studies on R&D intensity (Levin *et al.*, 1985) and on the market value of innovation (Im *et al.*, 2015) in the U.S. manufacturing sector.[2]

1.2.2 Types and Breadth of Innovation

Many theories and empirical approaches regard innovative activities as falling along a one-dimensional continuum. An important aspect that has not been considered enough, however, is the breadth of innovation. Since innovation is a recombination of existing technologies in a novel fashion (Grant, 1996; Henderson and Clark, 1990; Kogut and Zander, 1992; Nelson and Winter, 1982; Schumpeter, 1934), it is important that firms engage in different types of innovation and broaden their area of innovation as an input for further innovation. Broader exploration of technologies could lead to an unprecedented recombination of existing knowledge and breakthrough innovation.

However, it is even more difficult to broaden the scope of technological innovation than to simply increase the intensity. Conducting R&D on a new technological field is more difficult and riskier than conducting R&D on an existing field. Firms do not possess as much absorptive capacity for new areas, and the project may develop slowly under a learning curve (Cohen and Levinthal, 1990). This makes the innovation activities on new areas more costly, risky, and time-consuming. In this sense, all the requirements for and difficulties in innovating discussed earlier in this section apply more aggressively for broadening the scope of innovation.

Consider the two types of investments: incremental (exploitative) investment and radical (explorative) investment. Up to a certain profit level, firms may keep investing in incremental innovation that cut production costs or add marginal features to their technology or product; this is more relevant to a survival strategy to keep minimal competitiveness in the current market. Explorative investment, on the other hand, can be pursued only after securing a position in the market. When profit exceeds a certain threshold, the residual (extra profit) can be used for searching for new innovation that had not yet been pursued; this is to perform better in the *future* market. When firms enjoy higher profit and face less uncertainty thanks to reduced market competition, they have “slack” time, financial, and cognitive resources that can be put on longer-term and riskier projects. In this sense, reduced competition can provide firms with incentives and the ability to broaden their technological area and conduct more aggressive and ambitious research.

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[1] See, for example, Igami and Sugaya (2017) on how colluding firms communicate with and monitor each other. It also has implications for the types and breadth of innovation, which will be discussed in Section 1.5.2.

[2] Relatedly, Williamson (1965) finds an optimal concentration ratio of 30 from the linear model. The number goes down to 5 when the log-linear model is used.

In addition, reduced competition can promote R&D coordination between firms, either explicit or implicit. Collusion, for example, facilitate communication and increase visibility among competing firms. As colluding firms discuss price level and internalize each other's objectives, they learn about one another's R&D activities, which prevents duplicate investment on the same technology by multiple firms. In other words, reduced competition de-homogenizes and diversifies R&D projects of firms, leading to an expansion of technological fields.

1.3 Data and Measures

Data for this research come from several different sources. First, detailed data on collusion and its operations come from the DOJ and Trade Regulation Reporter. Second, PatentsView and Compustat provide data on patenting and R&D expenditure of firms, respectively. In addition, the U.S. Census Research Data Center (RDC) provides restricted-use microdata on a wide range of business activities for the entire set of non-farm business units in the U.S., including their innovation activities, although the results are not reported in this study.

1.3.1 Collusion Data

The Antitrust Division of the DOJ typically releases three different types of documents for each collusion case in their Antitrust Case Filings repository: information (indictment), plea agreement, and final judgment. These documents contain detailed information on who the colluding firms are, when the collusion started and ended, and how exactly the collusion was operated. It also clearly defines the relevant market by four-digit SIC (for older cases) or six-digit NAICS (for recent cases). Since the documents come at the defendant firm and/or individual level (not necessarily at the collusion-level) in most cases, I grouped indicted individuals and firms at the collusion level. This process is straightforward for most cases because co-conspirators in the same collusion case are usually mentioned in the indictments. Information on collusion period and relevant market are used to further check the quality of collusion grouping.

Another source of data for collusion is the Commercial Clearing House (CCH) which has been providing legal information in trade regulation since 1914. Its Trade Regulation Reporter section (recently renamed to Antitrust Cases) provides summaries on the aforementioned original documents of the DOJ. The Trade Regulation Reporter covers more cases than the DOJ's online repository and keeps track of recent developments. Where there are corresponding documents available in the DOJ, however, its original documents contain more detailed information than the summaries in the Trade Regulation Reporter.

I digitized and merged all documents from the DOJ and the Trade Regulation Reporter that are relevant to collusion (i.e., the violation of Section 1 of the Sherman Antitrust Act). For early documents that report relevant markets using SIC codes, I looked at the SIC-NAICS crosswalk and additionally consulted detailed descriptions of each industry classification to convert the SIC code to the NAICS code. As a result of this effort, I identified 461 collusion cases of 1,818 firms in the U.S. from 1975-2016.[3] Descriptive statistics on cartels are presented in Table 1.1.

The most important information on collusion is the names of (co-)conspirators and the year of collusion formation and breakup. The DOJ investigates collusion and estimates the date of collusion formation and breakup. We have good reason to believe that their estimation is fairly accurate, because in most cases, indictees and the DOJ agree on "plea bargaining" – meaning that indictees pledge to fully cooperate with the investigation and provide all the evidence in return for reduced punishment. Yet it should still be noted that colluding firms have strong incentive to *understate* the true collusion period (unless the DOJ has accurate evidence), and the DOJ should have strong, real evidence to claim a longer collusion period. This makes the DOJ's *estimation* on collusion duration to be a lower bound for the actual duration; in other words, the *real* collusion start date may be earlier than the estimated date appearing in the indictment. This motivates my event-study approach and the difference-in-differences estimation, where I can flexibly and explicitly check if colluded firms adjusted their behavior even before the *estimated* year of collusion formation. The accuracy of breakup date is less of a concern, because many collusion cases are broken down by the antitrust intervention of the DOJ (Levenstein and Suslow, 2011), and therefore the DOJ has more information about and more accurate data on the *real* breakup date.

1.3.2 Patent Data

The primary source of patent data is PatentsView which offers information on every aspect of patents. Supported by the Office of Chief Economist in the US Patent & Trademark Office (USPTO), the PatentsView database has information on inventors, assignee firms, their locations, and other details available in the original patent document and covers all granted patents in 1976-2017. It provides a unique identifier for assignee firms and inventors based on a name disambiguation algorithm. As a complementary source, I also make use of other patent data such as the USPTO Patent Application Information Retrieval system (PAIR), NBER Patent Data, Fung Institute's Patent Data, and Google Patents.

One concern is that information on location is sometimes not accurate or consistent. For some cases, assignee firms or inventors from the same location have different location information. For other cases, there are inconsistencies in the level of municipality in that city names appear in the state or province field or vice versa. There also are typos in the names of the location, and it is difficult to hand-correct 6,647,699 patents in the PatentsView sample (as of May 28, 2018). To deal with this problem, I use geographic coordinates – latitude and longitude – that are available for more than 99.9% of patents in the PatentsView data. I use Google Maps Geocoding API ("reverse geocoding") to convert the geographic coordinates to the names of country, state/province, and city. This process ensures accurate and consistent geographic information for all assignee firms and inventors. For instance, even if geographic coordinates slightly differ for the same location, the

[3] I excluded collusion cases in the financial sector (e.g., real estate, interest rate, foreign currency exchange). Years of breakup are used throughout this study, if not otherwise stated.

resulting names of city, state/province, and country from reverse geocoding should be the same.[4]

Another concern is that patent data has no information on industry at the patent or assignee firm level, which is important when defining markets and assigning control groups. To deal with this problem, I converted the patent-level technology field (Cooperative Patent Classification; CPC) to the North American Industry Classification System (NAICS) and then aggregate it at the firm-level. A recent project of the USPTO and the Commerce Data Service uses Natural Language Processing (NLP) to provide the Cosine Similarity table (many-to-many crosswalk) between all 6-digit NAICS codes and the 4-character CPC subclasses.[5] Using this many-to-many bridge, I first construct a one-to-one bridge between NAICS and CPC. In other words, for each CPC, I assigned the most similar NAICS code to it at the patent level. I then constructed the assignee firm-level NAICS industry codes as follows. For each patent and its CPC subclass, I constructed a vector of the CPC's Cosine Similarity score for each NAICS code. I then sum this vector of similarity scores for all patents at the assignee firm-NAICS level. The resulting similarity score represents each assignee firms' engagement in each 6-digit NAICS industry, and I assigned the top scored NAICS industry to each firm as the main industry. I also conducted a variant of this approach, by either normalizing its similarity score at the patent level (i.e., percentage score) or by calculating the score for each year (rather than pooling the years).

The next step was to match the names of firms in the collusion data and the patent data. I used two different name matching schemes to match the names of colluded firms to patent assignee firms. First, I came up with case-insensitive regular expression for the name of all colluded firms. For example, `^sam.*sung.* elec` captures "Samsung Electronics," "Sam-sung Elec," or "Sam sung Electronics, Ltd."[6] I then manually checked the quality of match by comparing firm names and their addresses. Second, I applied string distance algorithms (q-gram or cosine distance) and picked the top 20 unique candidates. I then manually checked the quality of the match, based on their names and addresses. This process additionally matches firm names that are not captured in the first process. The combined set of the two approaches constituted the treatment group (colluding firms) in the patent data. Of 1,668 colluded firms, 554 firms (33%) filed at least one patent. Firm-level descriptive statistics on patents are presented in Table 1.2(a).

As a result of the above processes, I constructed a firm-year panel dataset, using the universe of granted patents for 1976-2017. For each assignee firm, I identified the year of their first and last patent application. For any firm-year observation where I did not observe a patent, I assigned zero if the year occurred between the firm's first and last year of patenting. This leads to a balanced panel within the lifetime of firms. I defined a firm's primary and secondary technology fields based on the frequency of patenting in each technology class (CPC). I used patent count and citation-weighted count to derive measures

[4] One caveat is that Google Maps Geocoding API does not provide the location name on disputed territories. There are few such cases, and I manually checked and cleaned PatentsView's location for these exceptions.
[5] Detailed explanation and crosswalk files are available at https://commercedataservice.github.io/cpc-naics.
[6] `^sam.*sung.* elec` captures all firm names that (1) starts with "sam", (2) followed by "sung" no matter what characters are inbetween, and (3) followed by space and "elec" no matter what characters are inbetween. I used an option that ignores uppercase, lowercase, or mixed case.

for innovation intensity. Patent technology classes and technology class-weighted patents are used to measure the breadth of innovation.

1.3.3 R&D Data: Compustat

It is important to study the input for innovation, because innovation output may be a noisy measure of fundamental innovation activities of firms. For example, firms change their propensity to patent over time. I used two different data on R&D investment of firms. First, Standard & Poor's Compustat North America provides accounting, financial, and market information on firms in North America. I used Research and Development Expense (XRD), defined as "all costs incurred during the year that relate to the development of new products or services," to measure the innovation input of firms. The same name matching processes are used for firms in the Compustat as for firms in the patent data. An important thing to note is that the Compustat sample is different from my patent sample in that Compustat consists only of publicly traded companies in North America, which makes the data biased toward large firms. However, R&D investment is disproportionately performed by large firms, so it is likely that my methods could capture a majority of R&D performers from the Compustat. Descriptive statistics for Compustat are presented in Table 1.2(b).

1.4 Research Design and Empirical Strategy

> *People of the same trade seldom meet together, even for merriment and diversion, but the conversation ends in a conspiracy against the public, or in some contrivance to raise prices (Adam Smith, The Wealth of Nations, 1776)*

1.4.1 Collusion, Antitrust Enforcement, and Competition

Collusion and Antitrust Enforcement

Collusion or a cartel is an agreement between competitors to restrict competition, deter entry of new firms, and inflate prices. The Antitrust Division of the U.S. DOJ categorizes collusion as (horizontal) price fixing, bid rigging, and market allocation. In many cases, multiple schemes are used at the same time. Although variations exist in the types of conduct and their market consequences, the utmost goal of collusion is to restrict competition in the market. Standard economic theory predicts that, by suppressing competition, collusion increases prices, transfers consumer surplus to producers, and reduces social welfare (via incurring deadweight loss). The DOJ estimates that collusion can raise prices by more than 10% and that "American consumers and tax payers pour billions of dollars each year into the pockets of cartel members" (US DOJ: Klein, 2006, p.1). A survey of literature concludes that price overcharge by collusion ranges from 18% to 37% (Connor and Lande, 2006). This is why the DOJ views collusion as a supreme evil of antitrust.

As such, government and competition authorities designed a strict set of rules that govern collusion. In the U.S., since the enactment of the Sherman Antitrust Act (26 Stat.

209, 15 U.S.C. §1) in 1890, collusion has been *per se illegal* and felony punishable. The latest revision of Section 1 of the Sherman Antitrust Act (as amended June 22, 2004) states the following:

> ***15 U.S. Code § 1 - Trusts, etc., in restraint of trade illegal; penalty***
> *Every contract, combination in the form of trust or otherwise, or conspiracy, in restraint of trade or commerce among the several States, or with foreign nations, is declared to be illegal. Every person who shall make any contract or engage in any combination or conspiracy hereby declared to be illegal shall be deemed guilty of a felony, and, on conviction thereof, shall be punished by fine not exceeding $100,000,000 if a corporation, or, if any other person, $1,000,000, or by imprisonment not exceeding 10 years, or by both said punishments, in the discretion of the court.*

The antitrust laws are considered as the "most effective brake against the cartelization of industry" (Arnold, 1965; Pate, 2003). In the U.S., the Antitrust Division of the DOJ and the Bureau of Competition of the Federal Trade Commission (FTC) jointly deal with collusion. Criminal enforcement, in particular, is primarily conducted by the DOJ, under the mission to promote economic competition. Figure 1.1 shows the number of collusion cases discovered (brown bars) along with the number of firms and individuals indicted (blue solid and dashed lines, respectively). Interestingly, the number of collusion breakups has seemed to decrease over time, while the strength of punishment – total criminal fines and prison sentences – has increased.

Collusion has been widely studied, yet a majority of studies focus on cartel birth, survival, and death. For instance, Levenstein and Suslow (2016) study the correlation between the business cycle (firm-level interest rate) and collusion breakup. Others examine the incentives for collusion formation or determinants of collusion duration/breakup (e.g., Miller, 2009). However, there has been little empirical work on the economic outcomes of collusion formation or breakup. As one of the few exceptions, Sproul (1993) studied 16 cases of collusion and found mixed directions of price change. Other studies focus on a single collusion case – for example, dairy products, a railroad, or vitamins. Important economic consequences – for example, entry, growth, exit, price, quantity, productivity, and innovation of firms – of collusion/competition still remain underexplored, especially in the empirical context.

Measuring Competition by Collusion

A major difficulty in empirical studies on competition is that competition is difficult to measure. Although "we have spent too much time calculating too many kinds of concentration ratios" (Joskow, 1975, p. 278), C3 (sum of market share of the three largest firms), C5 (sum of market share of the five largest firms), or the Herfindahl-Hirschman index (HHI) often fail to capture the level of market competition. Another challenge is that competition is endogenous; in many cases, changes in competition may be correlated with observable and unobservable factors that also affect the outcome of interest.

This paper exploits collusion cases to capture the changes in competition and to mitigate concerns over endogeneity.[7] Formation and breakup of collusion change the level of competition in the market (in opposite directions) and provide unique opportunities to estimate how market competition affects key economic outcomes. The formation, by definition, significantly suppresses market competition and inflates prices. breakup of collusion in turn abruptly increases (recovers) the level of competition. For some cases that are discovered and dissolved by the DOJ's intervention, investigations on collusion are kept confidential to collect enough evidence before an indictment; otherwise, colluding firms can hide traces of the crime and coordinate their responses before the investigation. For example, the "DOJ may investigate cartel conduct without notice by issuing search warrants to search companies or conducting dawn raids" (DOJ). This ensures an exogeneity of collusion breakup, compared to privatization of public firms, tariff changes, or other regulatory reform, which require public announcements and discussions in advance (e.g., a public hearing). Levenstein and Suslow (2011, p. 466) estimate that "about 80 percent of the cartels in the sample ended with antitrust intervention" and that "the determinants of cartel breakup are legal, not economic, factors." For other cases that ended before the DOJ's investigation, the breakup may still not be expected by colluding firms and other competitors in the market. This is because collusion is per se illegal and felony punishable, and thus it is expected that colluding firms keep it confidential. Another important reason to treat the breakup of collusion as an exogenous shock is the "leniency program" in the U.S.[8] This program grants immunity only to the first whistle-blower that informs the DOJ of the existence of collusion and provides enough evidence to prosecute. If any collusion participants (either a firm or an individual in the firm) expect a breakup of collusion, it is their dominant strategy to report it to the DOJ before any of their co-conspirators do and thus be exempt from criminal punishments.[9]

While the breakup event is more exogenous and causes an abrupt change in competition, the formation provides an additional opportunity to study our question when carefully considered in conjunction with the breakup event. As long as the sources of endogeneity are different, our analysis on both events – and opposite findings for the two – is doubly assuring and mitigates concerns that the findings may come from some endogenous factors other than the collusion-induced change in market competition.

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[7] Only a few studies have used collusion cases to measure the market competition. Symeonidis (2008) used the introduction of cartel law (i.e., antitrust law) in the U.K. in the late 1950s and found its positive impact on labor productivity but no effect on wages. Symeonidis compared *previously* cartelized industries to non-cartelized industries, abstracting away from each cartel case and actual existence of cartel. Levenstein *et al.* (2015) used the collapse of seven international cartels and found no significant effect of competition (due to cartel breakup) on spatial patterns of trade. In this study, I study how competition induced by collusion affects innovation. This study is distinct from existing ones in the following ways. First, I collect *all* known collusion cases and colluded firms in the U.S. and study their average effects (while carefully considering heterogeneous effects). Second, I exploit both formation and breakup events to doubly assure that the findings indeed come from competition effects. Third, my focus is not limited to prices which have been a main focus of the cartel study and highlight a wide range of innovation outcomes.

[8] The DOJ has been implementing the leniency program since 1978, but it has not been effective until a major revision was made in 1993 (for corporate leniency) and 1994 (for individual leniency).

[9] See Levenstein and Suslow (2006; 2011; 2016), Igami and Sugaya (2017) for more detailed discussion on the determinants of collusion duration or breakup.

1.4.2 Difference-in-Differences Estimation

This study examines the role of market competition on innovation for colluding firms alone and also the colluded market as a whole. This motivates the difference-in-differences estimation. At the firm level, I compare colluding firms (treatment group) to firms in the adjacent/similar market – but not in the same market – as a counterfactual (control group). At the industry level, I compare each colluded industry – including both colluding and non-colluding firms – to its adjacent/similar industry to test the industry-wide effects of collusion.

Firm-level Analysis

The treatment group is colluding firms that are identified by name/address matching with firms in the collusion data. The control group is defined as firms that share the same 4-digit NAICS code but not the 6-digit NAICS code.13 For example, if a colluding firm belongs to the NAICS 325411, firms that belong to the NAICS 325412, 325413, and 325414 constitute the control group.

I take two different approaches for the difference-in-differences estimation. First, for each firm in the treatment group, I pair many control firms based on the 4-digit NAICS code. This results in a very large set of paired treatment-control firm data. Second, I use the panel data at the firm-year level as it is, and assign a treatment indicator. I also create a variable for relative time (to either collusion formation or breakup) for treated firms only. I then compared treated and control firms that share 4-digit NAICS, regression with 4-digit NAICS×year fixed effects (along with firm fixed effects), as desired. The latter is my preferred approach for simplicity and the ease of calculating and interpreting standard errors. As a robustness check, I also conducted all analyses with the first approach, which produced very similar results.

The primary research output comes from regression estimates that explain how measures on innovation respond to changes in competition, using linear regression techniques. I estimate various forms of the difference-in-differences model in Equation (1.1):

$$y_{it} = \beta_1 \cdot [Treat_i \cdot Post_{it}] + \beta_2 \cdot Post_{it} + \rho_i + \gamma_{jt} + \epsilon_{it} \tag{1.1}$$

where the outcome of interest y_{it} for firm i in year t with the inverse hyperbolic sine transformation (IHS), $\sinh^{-1}(\cdot)$, is regressed on an interaction term between $Treat_i$ (an indicator variable for collusion participation for establishment i) and $Post_{it}$ (an indicator variable that is meant to capture the post-event – either collusion formation or breakup – periods at the establishment, industry, and year levels).[10] The regression model also

[10] A great advantage of the inverse hyperbolic sine transformation is that it is defined at zero. The transformation is defined as $y^{IHS} = \log\left(y + \sqrt{y^2+1}\right)$. Since inverse sine is approximately equal to $\log 2y = \log y + \log 2$ (except for very small values of y), it has the same interpretation as a standard logarithmic dependent variable (Burbidge, Magee, and Robb, 1988; MacKinnon and Magee, 1990; Pence, 2006). If any, the transformed variables "place less weight on impacts in the upper quantiles of the conditional distribution of outcomes (Kline *et al.*, 2017, pp. 20, 65)". For all specifications, I did robustness checks with natural

includes firm fixed effects ρ_i (note that $Treat_i$ is absorbed by the firm fixed effect) and industry group (4-digit NAICS)×year fixed effects γ_{jt} to control for both time invariant characteristics of a firm that may determine the outcome of interest as well as any industry- and time-varying components of economic activity that may influence entrepreneurial and innovation activities. Note that the 4-digit NAICS code (*j*) is used in the industry group×year fixed effects to compare treated and control firms within the same broadly defined sector. I excluded firms in the control group that share the same 6-digit NAICS code with the colluded firms to avoid spillover effects of collusion in the same narrowly defined market. The coefficient of interest in this model is β_1, which tells us the relationship between collusion-induced competition and innovation.

I also estimate a number of variants of this regression that include more flexible econometric specifications. Formal event study regression techniques are expressed in Equation (1.2) and Equation (1.3):

$$\begin{aligned} y_{it} = {} & \beta_1 \cdot [Treat_i \cdot Pre_{[-4:-2]}] + \beta_2 \cdot [Treat_i \cdot Post_{[1:2]}] + \\ & \beta_3 \cdot [Treat_i \cdot Post_{[3:4+]}] + X_{it} + \rho_i + \gamma_{jt} + \epsilon_{it} \end{aligned} \tag{1.2}$$

$$y_{it} = \beta_1 \cdot [Treat_i \cdot \textstyle\sum(t-\tau)] + \beta_2 \cdot \textstyle\sum(t-\tau) + \rho_i + \gamma_{jt} + \epsilon_{it} \tag{1.3}$$

where $Pre_{[-1]}$ is an indicator variable that takes the value of 1 for a year before the event of interest and serves as a baseline. $Post[1:2]$ is an indicator variable that takes the value of 1 for the first two years of collusion and 0 otherwise, while $Year_{[3:4+]}$ is an indicator variable that takes the value of 1 for the following four years of collusion (from third to sixth year of collusion) and 0 otherwise. X_{it} includes all lower-order terms, and τ is the year of event (i.e., either cartel formation or breakup). With this flexible event study approach, I can explicitly test the parallel trend assumption for the pre-event period and how the effects vary for the post-event period.

In addition, to get a more complete picture and compare the size of change from both events, I run a regression that incorporates both the formation and breakup of collusion in a single framework, as in Equation (1.4):

$$\begin{aligned} y_{it} = {} & \beta_1 \cdot [Treat_i \cdot Pre_{[-6:-4]}] + \beta_2 \cdot [Treat_i \cdot Collude_{[1:3]}] + \\ & \beta_3 \cdot [Treat_i \cdot Collude_{[4+]}] + \beta_4 \cdot [Treat_i \cdot Post_{[1:3]}] + \\ & \beta_5 \cdot [Treat_i \cdot Post_{[4:6]}] + \beta_6 \cdot [Treat_i \cdot Post_{[7:9]}] + \\ & X_{it} + \rho_i + \gamma_{jt} + \epsilon_{it} \end{aligned} \tag{1.4}$$

where $Pre_{[a:b]}$ means a to b years prior to the formation of collusion. $Collude_{[c:d]}$ represents early collusion periods: c to d years after the formation of collusion. $Post_{[e:f]}$ means e to f years after the breakup of collusion. To account for varied collusion periods, $Collude_{[4+]}$ represents the fourth year of collusion and onward up to a year before the collusion breakup. $Pre_{[-3:-1]}$ serves as a baseline for this regression.

For firms in the Compustat data, information on SIC code is complete, but NAICS codes are available for recent years only. I therefore use SIC to define the relevant market

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logarithm (by adding arbitrarily small number ϵ to deal with zero values) and found very similar results.

and the control group. The treatment group is colluding firms, and the control group is a set of firms that share 3-digit SIC codes but not the 4-digit SIC.[11] This approach (i.e., using SIC codes to define the relevant market) has been adopted in many studies (e.g., Kogan *et al.*, 2017). Estimation strategy is the same as in the patent analysis. Unlike patent data, there are many missing observations on R&D expenditure (XRD) in the Compustat data. Prior studies regarded missing observations as no R&D expenditure (i.e., assigned 0 to missing values). However, there are many missing values even if a firm (1) reports positive employment and revenue in the focal year and (2) reports positive R&D expenditure the year before and after the focal year. In this case, it does not make sense to assign zero R&D expenditure to the missing observation. I excluded missing values from the data. As a robustness check, I (1) use mean imputation or (2) take firms that have complete observation within the period I analyze (e.g., five years before and after the event), following the methods of Kogan *et al.* (2017).

Industry-level Analysis

My analyses so far have focused on colluded firms. It is as important and interesting to assess the industry-wide effects and test whether the aggregate level of innovation increased as the level of market competition changes. Theories on collusion and competition suggest that there exists a spillover effects – often called the "umbrella effect" – of collusion on non-colluding firms in the same market. The industry-wide aggregate effects measure the social welfare effect of collusion and have direct policy implications.

I aggregate the number of patent applications and other outcomes at the industry-year level (*jt*) and run the regression, as in Equation (1.5), Equation (1.6), and Equation (1.7):

$$y_{jt} = \beta_1 \cdot \left[Treat_j \cdot Post_{jt}\right] + \beta_2 \cdot Post_{jt} + \rho_j + \gamma_{j't} + \epsilon_{jt} \tag{1.5}$$

$$\begin{aligned} y_{jt} = \beta_1 \cdot \left[Treat_j \cdot Pre_{[-4:-2]}\right] + \beta_2 \cdot \left[Treat_j \cdot Post_{[1:2]}\right] + \\ \beta_3 \cdot \left[Treat_j \cdot Post_{[3:4+]}\right] + X_{jt} + \rho_j + \gamma_{j't} + \epsilon_{jt} \end{aligned} \tag{1.6}$$

$$y_{jt} = \beta_1 \cdot \left[Treat_j \cdot \sum(t-\tau)\right] + \beta_2 \cdot \sum(t-\tau) + \rho_j + \gamma_{j't} + \epsilon_{jt} \tag{1.7}$$

In addition, to get a whole picture and compare the size of change from both events at the industry aggregated level, I run a regression that incorporates both the formation and breakup of collusion, as in Equation (1.8):

$$\begin{aligned} y_{jt} = \beta_1 \cdot \left[Treat_j \cdot Pre_{[-6:-4]}\right] + \beta_2 \cdot \left[Treat_j \cdot Collude_{[1:3]}\right] + \\ \beta_3 \cdot \left[Treat_j \cdot Collude_{[4+]}\right] + \beta_4 \cdot \left[Treat_j \cdot Post_{[1:3]}\right] + \\ \beta_5 \cdot \left[Treat_j \cdot Post_{[4:6]}\right] + \beta_6 \cdot \left[Treat_j \cdot Post_{[7:9]}\right] + \\ X_{jt} + \rho_j + \gamma_{j't} + \epsilon_{jt} \end{aligned} \tag{1.8}$$

where X_{jt} includes the intercept and all lower-order terms.

[11] Some SIC has unique 3-digit code, which make it not possible to construct the control group based on 3-digit SIC. In this case, I use neighboring industry based on 3-digit SIC as a control group. For example, 2810 has no sub-classification within the 281- family, so I use firms in the 280- and 282- families as the control group.

The Stable Unit Treatment Value Assumption, Validity of Control Group, and Measurement Error

An important assumption behind causal inference is the Stable Unit Treatment Value Assumption (SUTVA). In my setting, this assumption may be violated if a formation or breakup of collusion affected firms in the control group. This is why I excluded firms in the control group that share the 6-digit NAICS code with the colluding firms. While it is not possible to completely rule out the possibility of a SUTVA violation, to the extent that firms in the control group are affected by collusion, I am *underestimating* the size of the effects. For example, if non-colluded firms in the adjacent market are affected by the focal collusion, and this increase the price of their products, this spillover effect of collusion works *against* my findings (non-colluding firms are affected in the same way as the colluding firms).

It is possible that the Antitrust Division of the DOJ did not indict some firms participating in collusion because they did not know they colluded, could not collect enough evidence to indict, or granted amnesty to some colluded firms (as per the Leniency Program). Since my control group consists of firms in the adjacent yet not in the same market, I do not expect that these omitted firms affect the validity of the control group. Even if they are mistakenly included in the control group, it will work *against* my findings, leading to underestimation, not overestimation, of the effects.

In addition, the event-study DiD estimation (in Equation (1.2) and Equation (1.3)) and synthetic control approach (in Section 1.6.4) complement each other. In the event-study approach, I can explicitly test for parallel trends by investigating yearly estimates for pre-periods. In the synthetic control approach, I rather mechanically impose the parallel trend, leaving the post-period *ex ante* unknown. While neither of these approaches is perfect, I can assure the validity of the control groups by finding consistent results from the two approaches.

1.5 Results

1.5.1 Intensity of Innovation

Patents

Table 1.3, columns (1)-(4), shows the effects of competition on four measures of innovation intensity: patent count, citation-weighted patents, and count of top 10% and 25% cited patents, respectively. The results are based on Equation (1.1) where $Post_{it}$ is an indicator variable for the year of event (either collusion formation or breakup) and the following three years. Pre-event years before the event ($t \in [-4, -2]$) serve as a baseline.[12] After the formation of collusion, colluding firms increased patenting by 51%. Colluding firms on average filed 33.9 patents per year immediately before the formation of collusion, so the 51% increase in patenting is equivalent to 17 more patents per year for each colluding firm.

[12] $t = -1$ is excluded to account for a potential misestimation on the year of collusion formation or breakup.

After the breakup of collusion, on the other hand, colluding firms decrease patenting by 4%, though this result is imprecisely estimated. The precise and small point estimation for $Treat_i \times Post_{it}$ is in fact reasonable and expected outcome because, in the real-world setting, it is not possible to stop all ongoing R&D projects suddenly and instantaneously. Furthermore, it takes several years from the onset of an R&D project to file a patent. In other words, even after collusion breakup, firms keep filing patent applications based on R&D activities undertaken before the breakup. I check longer-term effects later in this section in Figure 1.3 and confirm that patent applications gradually revert back to the pre-collusion level in the longer term.

Table 1.4 shows a more flexible approach based on Equation (1.2). $Pre_{[-4:-2]}$ is an indicator variable that takes the value of 1 for four to two years prior to collusion formation or breakup and 0 otherwise. $Post_{[1:2]}$ is an indicator variable for the first and second years of the event (either collusion formation or breakup). $Post_{[3:4+]}$ indicates three to six years after the event (when collusion formation is an event of interest, I excluded the post-collusion period for short-lived collusion). $Pre_{[-1]}$ serves as a baseline. After the formation of collusion, colluding firms increase patenting by 41% in the short term ($Treat \times Post_{[1:2]}$, column 1) and by 62% in the long term ($Treat \times Post_{[3:4+]}$). After the breakup of collusion, however, colluding firms decrease patenting by 11% in the long term ($Treat \times Post_{[3:4+]}$). Again, the imprecise and small point estimation for $Treat \times Post_{[3:4+]}$ is not inconsistent with my predictions. Figure 1.2 visualizes the results on patent count. Horizontal lines and boxes around them represent the point estimates and 95% confidence intervals based on pooled difference-in-differences estimation (grouped by three years around the event of interest), as in Equation (1.2).

Furthermore, I report estimates from the event study approach with distributed leads and lags based on Equation (1.3). In Figure 1.2, each of the points and vertical bars represents yearly event-time estimates and 95% confidence intervals of the event study approach, with relative year −1 as baseline. This approach makes it possible to identify the trends of outcomes for post-event periods (how the effects change over time) and to explicitly test the parallel trend assumption for pre-event periods. Standard errors are clustered at the industry group level (4-digit NAICS). Figure 1.2(a) shows that colluding firms gradually increase their patenting activities after they begin to suppress competition by forming a cartel. Figure 1.2(b), on the other hand, shows the opposite: that colluding firms decrease their patenting activities after the breakup of collusion recovers market competition.

There is a significant amount of variation in the quality of patents, and therefore, a mere count of patent applications may not capture their quality or impact. To better measure the fundamental innovation activities of firms, I look at quality-adjusted patents. First, studies find that citation-weighted patents are more highly correlated with patent quality or market value than patent counts (Lampe and Moser, 2016; Hall *et al.*, 2005; Trajtenberg, 1990). The results on citation-weighted patents are similar to those on patent counts, as shown in Table 1.3 (column 2). Second, I further examine the counts of high-quality patents – patents that are cited by future patents more than 10 times (75th percentile) and 25 times (90th percentile), respectively. As shown in columns 3 and 4 in Table 1.3 and Table 1.4, the results are consistent with what I find from patent counts and citation-weighted patents, though smaller in magnitude. Firms indeed increased innovation activities that produce impactful and high-quality patents when collusion suppressed market competition.

The Pairwise Synthetic Control Method (discussed in Section 1.6.4) provides a more comparable control group in that each control unit is synthesized to mimic the trend of an outcome variable of firms in the treatment group for the pre-event period only. Figure 1.11 visualizes pairwise regression results with a sample of colluding firms and their Synthetic Control. The results are qualitatively and quantitatively very similar to the main results in Table 1.3 and Figure 1.2.

The above approaches consider the formation and breakup of collusion as if they are separate events. These two events, however, are closely connected aspects of collusion, and therefore it is useful to analyze them in a single framework. A difficulty arises because each instance of collusion has a different duration and the relative time to cartel formation and breakup varies across cases. To circumvent this problem, I merge the relative years into seven time groups and estimate average effects within these time groups. I then let one of these time groups represent all the later periods of collusion. The regression results on innovation intensity are shown in Table 1.5 (columns 1-5). $Pre_{[a:b]}$ means a to b years prior to the formation of collusion. $Collude_{[c:d]}$ represents early collusion periods: c to d years after the formation of collusion. $Post_{[e:f]}$ means e to f years after the breakup of collusion. To account for varied collusion periods, $Collude_{[4+]}$ represents the fourth year of collusion and thereafter up to a year before the collusion breakup. $Pre_{[-3:-1]}$ serves as a baseline. Figure 1.3 graphically shows the results. This analysis on the life cycle of collusion is consistent with my previous findings. Furthermore, two key results – the opposite responses to the formation and breakup of collusion, and the finding that innovation intensity increases only during the collusion period and then gradually reverts back toward the pre-collusion level after collusion breakup – assure that I have indeed captured the effects of collusion-induced changes in competition, and not those of some unobservable factors unrelated to collusion/competition and unknown to researchers.

R&D Investment

Patents are an intermediate or final output of innovation activities. It is possible that firms merely change their "propensity" to patent in response to changing competition in the market. The observed change, for example, may be due to changes in the need for strategic patenting (e.g., Hall and Ziedonis, 2001; Lerner, 1995; Kang and Lee, 2019) rather than reflecting fundamental innovation activities. It is therefore important to check how firms change an input for innovation. I examine R&D investment of firms using two different data sources: the U.S. Census and Compustat. Here I focus on analyses based on Compustat North America which consists of publicly traded firms in the U.S. and Canada.

Results on R&D investment show that colluding firms indeed increase their innovation input, as shown in Table 1.3 (column 5) and Figure 1.4. Colluding firms in the Compustat sample increase their R&D expenditure by about 12% during collusion and decrease it by 18% after collusion breakup.

An analysis of the effects of collusion on R&D investment throughout the life cycle of collusion is shown in Table 1.5 (column 5) and Figure 1.5, which show that colluding firms increase their investment in R&D activities by 17%-33% during the collusion period, compared to pre-collusion period.

1.5.2 Breadth of Innovation

As a next step, I examine how competition influences the breadth of innovation. To see if firms broaden their scope of innovation activities, I measure the breadth of innovation by counting (1) the number of unique technology fields (defined by the 4-digit Cooperative Patent Classification; CPC) at the firm-year level[13], and (2) technology class-weighted patents. Table 1.6, Table 1.7, and Figure 1.6 show how a firm's patenting breadth changes as market competition changes. The breadth of technological innovation increased by 33% when market competition was suppressed by collusion (Table 1.6(a), column 1). After the breakup of collusion, on the other hand, the breadth of patenting dropped by 6% (Table 1.6(b), column 1). The results, taken together, suggest that competition changes the breadth of innovation as well as its intensity. A single-framework by the life cycle of collusion (i.e., including both cartel formation and breakup events) is shown in Table 1.5 and Figure 1.7. An alternative measure, the technology class-weighted patents, also confirms the findings on the breadth of innovation (column 2 in Table 1.6 and Table 1.7).

To see where the effects come from, I further test how patenting changes for a firm's primary technological area – measured by patent counts in each firm's most frequently patented technology classes – and for its peripheral technological area – measured by patent counts not in each firm's three most frequently patented technology classes. The former concerns continuing innovation, whereas the latter captures innovation activities in the fields that are new to the firm. Firms indeed increase innovation in both extensive (new areas) and intensive (existing areas) margins. Table 1.6 and Table 1.7 (columns 3-4) show that the intensity of innovation increased for both primary and peripheral technology areas of firms. Increased innovation activities during collusion do not exclusively come from new searches for unexplored technologies.

This line of the results is to some extent consistent with recent empirical findings in different contexts. Krieger *et al.* (2018) study the pharmaceutical industry and find that R&D on "novel" drugs (as opposed to "me-too" drugs) is riskier and that more profits promote R&D on novel drug candidates. The key mechanism here is that financial frictions hinder the ability and incentives to invest in novel, riskier drugs. Turner *et al.* (2010) find that, in a less competitive market, software firms in the U.S. became more responsive to generational product innovations (GPIs) by external actors (and less responsive to their own historical patterns of innovation). In other words, firms explore unprecedented innovations that are new to the organization as the competition level decreases. Findings on patent pools are in line with these results in that firms in the pool (i.e., reduced technological competition) increases innovation in an alternative technology (Lampe and

[13] The results remain unchanged if I divide the number of unique technology fields by the maximum possible number of CPC. For example, I take it into account that (1) a firm can explore at most five CPC subsections if it filed only five patents and (2) a firm can explore only 626 CPC subsections (the total number of CPC subsections) even if it filed more than 626 patents.

Moser, 2013) despite the decrease in innovation in the focal technology (Lampe and Moser, 2010). The focus of Macher *et al.* (2015) discussed in Section 1.2 was on an adoption of cost-saving technology for the current line of products. This "inability to invest in new technology" should be much higher for new areas of innovation that are not directly linked to the current products or technologies. This is especially the case when we consider the finding that firms that produce a substitute technology are substantially more likely to fail (Lampe and Moser, 2013).

Individual- or team-level studies also support this view. Bracha and Fershtman (2013) find from a lab experiment that competition induces agents to work harder but not necessarily smarter. Subjects are likely to choose simple tasks ("labor effort") in a head-to-head tournament competition, whereas they are more likely to choose more complicated tasks ("cognitive effort") in a pay-for-performance setting without competition. Gross (2018) finds from a logo competition platform that heavy competition decreased the originality and unprecedentedness of ideas; competition impeded individual artists' exploration on a wide range of possibilities and ideas.[14]

1.5.3 Industry-level Analysis

The behavior of colluding firms – e.g., suppressing competition in the market and raising prices – may not only change their own behavior but also affect the relevant market as a whole. The spillover effect of collusion is sometimes called the Umbrella Effect, where non-colluding firms to some extent benefit from the existence of collusion in the market. It is therefore important to study what the overall industry-wide effects are.

Table 1.7 shows our main outcomes based on industry-level aggregated data for both colluding and non-colluding firms based on Equation (1.8). Patenting increased by as much as 17% when competition was suppressed by collusion. A similar pattern is observed for the breadth of innovation. The industry-level outcomes closely follow the pattern of the colluding firms' outcome. The pro-innovation effect of reduced competition and then anti-innovation effect of increased competition still hold at the broader industry level, though these effects are generally smaller in magnitude and imprecisely estimated, compared to the outcome for colluding firms.

1.6 Additional Analyses

1.6.1 Economic Mechanism: Financial Constraints

One of the main arguments of the Schumpeterian view is that reduced competition provides firms with more financial resources that can be invested in innovation activities. A testable implication of this argument is that firms experiencing high revenue growth during collusion should invest more in R&D activities compared to those experiencing low

[14] It is important to note that intensifying competition (from no competition) also induced artists to produce original, untested ideas. This finding is in line with an inverted U-shaped relationship between competition and creativity.

revenue growth. I test this hypothesis by calculating each firm's revenue growth during collusion compared to pre-collusion periods and dividing them into quartile groups based on their revenue growth. I then run separate regressions by group on R&D expenditure, based on Equation (1.1). Figure 1.8 graphically shows the results. The increase in R&D expenditure during collusion has a positive monotone relationship with revenue growth during collusion. This analysis confirms that a growth in revenue (or an ease of financial constraints) is one important economic mechanism behind the negative causal relationship between competition and innovation intensity identified in this study (Section 1.5.1).

1.6.2 Economic Mechanism: Industry Growth Rate

Industries differ in many characteristics. A response to market competition should therefore differ across industries. One very important characteristic that distinguishes industries in terms of innovation is maturity. If an industry is fast-growing and attracts new innovators, we should observe a greater effect on innovation when collusion changes the ability and incentives to innovate. Fast-growing industry provides firms with additional incentive to innovate because the pie gets bigger. On the other hand, if an industry is mature or stagnant, a reduction in competition may not be able to spur innovation. I test whether the effects differ across industries by their growth rate. First, I calculate the average growth rate of innovation activities (measured by successful patent applications) in each industry for five years prior to the formation of collusion and divide them into quartile groups based on this measure of average growth rate. Second, I run four separate regressions by the quartile group on three measures of innovation activities, as in Equation (1.1).

Figure 1.9 shows the results for all patents (red bars), high-quality patents (i.e., those that are cited more than 25 times; brown bars), and the number of unique technology fields patented (blue bars). The measures of innovation intensity and breadth are higher for industries that exhibit above-median growth rates (i.e., 3rd and 4th quartiles) in terms of patenting activities.

It is important to note that innovation growth rates are measured at the industry group (4-digit NAICS) level, whereas our regression approach compares firms in treatment and control groups within such industry groups. This mitigates the concern that my estimates are driven solely by the pre-existing growth pattern of each industry group.

This finding has a very important implication for policy in that the enforcement of competition policy should differ depending on the growth rate of innovation in different markets; this is specially the case given limited amount of resources of the antitrust authority (see Section 1.7 for a more detailed discussion).

1.6.3 Leniency Program and the Temporal Heterogeneity

An important source of heterogeneity is temporal change in competition and innovation. Enforcement of antitrust laws by the DOJ may change depending on budget allocation to and within the Antitrust Division of the DOJ and other political and economic factors. For example, the DOJ introduced a new corporate and individual leniency program in 1993 and 1994, respectively, to better enforce antitrust laws (see Section 1.4.1). This policy not only affects the initial formation of collusion (deterrence) but also the probability of detection (discovery) (Miller, 2009). Advances in communication technologies and transportation may also have affected how colluding firms discuss prices levels and share information. Furthermore, patterns of technological innovation have also changed. We are witnessing rapid growth in the Artificial Intelligence and Machine Learning fields, and the role of competition in these fields may be different from the role of competition in the emerging fields in the 1970s and '80s. It is therefore important to check whether our main results change over time. As such, I ran regressions based on Equation (1.1) separately for periods before and after the introduction of the Corporate Leniency Program in 1993.

Figure 1.10 graphically presents the results. I did not find a noticeable difference between the two time periods, suggesting that the effect of competition on innovation patterns of firms remains relatively constant over time despite new competition policies and advancements in technologies

1.6.4 Pairwise Synthetic Control Method

Although we can explicitly test the parallel trend assumption with an event-study approach, one concern is that firms in the control group may be less comparable to the colluding firms (the treatment group). For example, the size of firms in the control group, on average, is smaller than those in the treatment group. To deal with this issue of potential imbalance between the two groups, I use the synthetic control method (Abadie *et al.*, 2010). This method provides a powerful tool when there is a single treatment unit and many control units. In this study, I apply this method for each colluding firm to synthesize its counterfactual and then repeat this work for all colluded firms, which results in many treatment-control pairs. Control groups are matched and synthesized based on their patent count for the pre-period ($t \in [-5, -2]$).[15] In this way, I estimate a pair-wise difference-in-differences model, which generally is not possible with the single-treatment-unit synthetic control approach. The results, show in Figure 1.11, are very similar to those of the main analysis (Figure 1.2).

1.6.5 Placebo Tests

To control for the possibility that my main findings resulted from a mechanical, spurious pattern generated in the data construction and empirical analysis stages, I ran a set of placebo tests by randomly assigning treatment status. For each colluded firm, five firms in the same 6-digit NAICS industry were randomly selected (from a pool of both colluded and non-colluded firms) and assigned to the placebo treatment group. This random assignment experiment was repeated 1,000 times. Figure 1.12 graphically presents the results for citation-weighted patents. Figure 1.12(a) and Figure 1.12(b) correspond to Figure 1.2(a) and Figure 1.2(b), respectively. Figure 1.12(c) corresponds to Figure 1.3.

[15] There are cases where collusion lasted less than five years. When we match on five years of pre-breakup period, we capture the formation of collusion within this pre-period, which hampers the validity of the Synthetic Control. In such cases, I excluded pre-formation period and matched on post-formation and pre-breakup period.

Gray lines represent 1,000 placebo simulations. I confirm from this experimentation that my findings for colluded firms are clearly distinct from placebo tests and do not come from spurious, arbitrary components.

1.7 Strategy and Policy Implications

Innovation activities of firms fundamentally determine their long-term competitive advantage and performance. The firm-level findings on the intensity and the breadth of innovation, altogether, provide insights for a firm's innovation strategies. Interestingly and importantly, firms shift toward innovation competition and broaden their search for new technological opportunities when price competition weakens. In other words, reduced competition implies an important change in the rules of the game and is not a cushion to sleep on (Schumpeter, 1942). It is important for firms to understand that they should compete on different margins and to come up with appropriate (counter-)strategies. The strategic implications of the findings of this study, of course, differ across firms and sectors. As explored in Section 1.6.1 and Section 1.6.2, the negative relationship between competition and innovation is magnified for firms that reap more profits and industries that grow fast. The heterogeneous effects – e.g., by nascent vs. mature industries – should also be considered.

Perhaps more important are implications for public policy and law enforcement. The ultimate goal of the DOJ has been to promote competition on prices. While the DOJ, in its merger analysis, acknowledges the importance of promoting innovation (US DOJ: Alford, 2018), it maintains the position that "cartels inflate prices, restrict supply, inhibit efficiency, and reduce innovation" (US DOJ: Pate, 2003). The European Commission (EC) has a similar attitude. In their innovation theory of harm (ITOH), the EC and its economists view that mergers reduce innovation, not to mention colluding behavior of firms, and conclude that competition is the mother of invention (European Commission, 2016).

Put differently, the antitrust authorities have been assuming that firms compete on prices holding products and innovations constant. In other words, collusion only affects the distribution of products, given a fixed product design or production process. This assumption does not consider the possibility that price in turn affects the quantity, quality, and types of goods (possibly through innovation). This paper suggests that it is important to have a comprehensive view that competition in the product market not only affect the market price of products but also changes the fundamental characteristics (innovativeness) of products that firms design and produce.

The findings of this study suggest – in the context of price-fixing collusion – that competition may hamper a firm's ability to and incentives for innovation.[16] In other words, it is possible that the pro-innovation effect of market power is higher than its anti-price effect (or price distortion), providing net positive social value. For instance, the development of a vaccine for Zika virus may have more social value than selling aspirin at a lower price. In price terms, new inventions reduce the price of previously unavailable

[16] This finding is in line with the model of Loury (1979, p. 408) where "more competition reduces individual firm investment incentives in equilibrium."

products from infinity to a certain finite level. Furthermore, studies have consistently found that social return to investment in R&D is higher than the private return: "the gross social returns to R&D are at least twice as high as the private returns (Bloom *et al.*, 2013)." Thus, it is important to promote market structures that provide firms with incentives and the ability to innovate (Gilbert, 2006a,b) (to the extent that the social benefit of innovation outweighs social loss of price distortion). In this sense, policy makers and regulators who promote competition should also carefully consider its (potentially negative) impact on innovation. It should also be noted that competition changes the breadth and the direction of innovation and that firms are more likely to pursue novel and riskier innovation activities when facing less competitive pressure in the market.

Yet research shows that existing firms tend to continue with established areas of technology, whereas new startups and entrepreneurs tend to bring breakthrough innovation to the market. This dynamic argues for strong antitrust enforcement because market power and collusion may deter the entry of new entrepreneurs and startup companies. In this sense, both the extensive and intensive margins should be carefully considered in the design and enforcement of antitrust policy.

The findings of this study also raise an important question regarding what competition really means. My understanding is that competition is mostly context-specific, making it almost impossible to define competition in general terms. The mission of the Antitrust Division of the DOJ is to promote economic competition, but what competition means differs when they assess, say, mergers and acquisition versus price-fixing collusion. Furthermore, it is as important to clearly define what the policy goal is. While the aim of the DOJ has been on promoting competition on prices, they are many other important economic outcomes such as the intensity and/or breadth of innovation. This is especially important because the target is moving: firms shift their domain of competition to innovation when they stop competing on prices. Therefore, the first step in antitrust authorities achieving their goal may be to precisely define what competition means in different contexts and then determining how to achieve the social optimum by balancing the consequences of this more precise definition of competition on price and innovation.

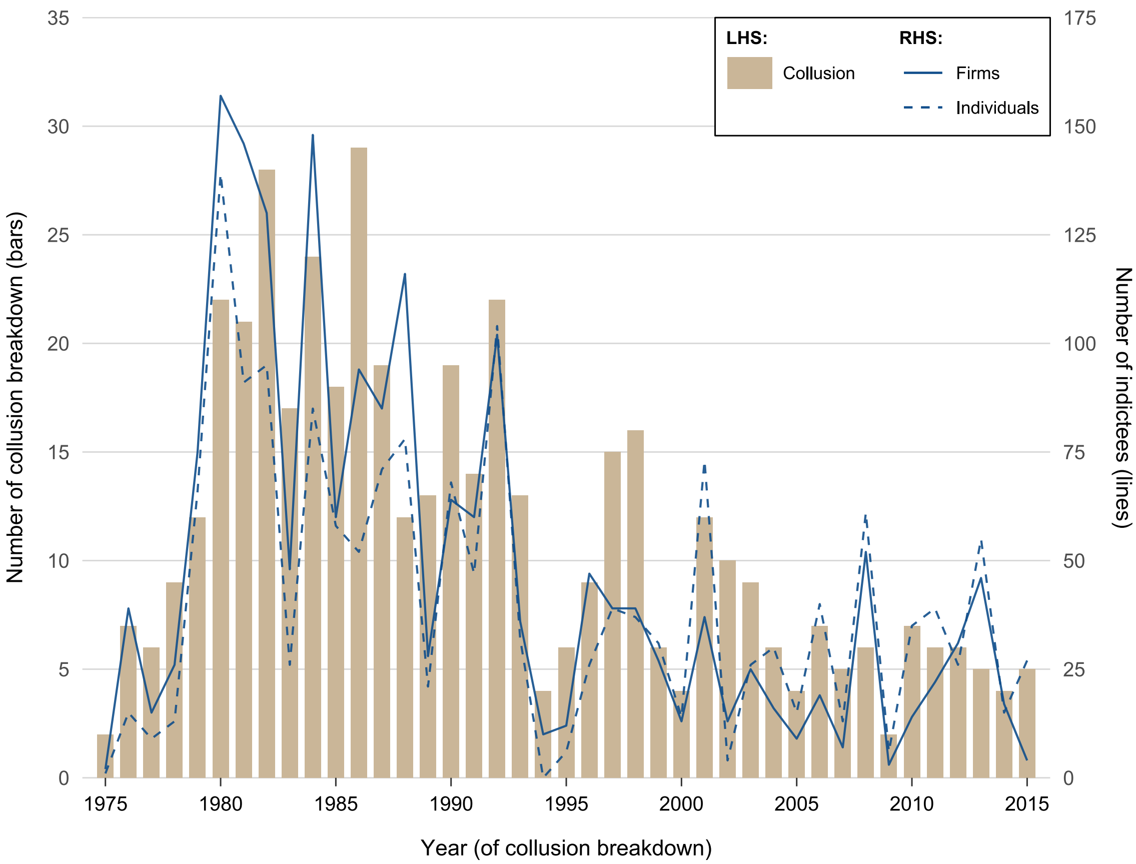
1.8 Concluding Remarks

In this study, I examine how market competition affects the intensity and breadth of innovation of firms, exploiting all 461 cases of collusion in the U.S. from 1975-2016. I find a negative causal relationship between competition and innovation. When collusion suppressed market competition, colluding firms increased R&D investment by 12% and patenting by 51% (or 17 more patent applications per year for each colluding firm). The number of high quality patents (i.e., patents with more than 25 non-self forward citations; 90th percentile) increased by 20%. Furthermore, I find evidence that firms broadened their areas of innovation at this time; the number of patented technology fields increased by 33% under collusion. The increased and broadened innovation activities reverted back, close to the previous level, when competition was restored by collusion breakup. Further tests suggest that financial constraint ("ability to innovate") and the industry's growth rate ("incentive to innovate") are important economic mechanisms behind the trade-off between price competition and innovation growth. The industry-wide aggregate analysis across all firms shows a similar pattern to that of colluding firms, though smaller in magnitude, indicating that colluding firms indeed drove the overall industry-wide outcomes.

It should be noted, however, that the focus of this study is on price-fixing collusion, and the findings herein may not be generalizable to other contexts. Implications for competition that are induced by foreign trade (import penetration), subsidies, mergers, patent pools, or privatization of public firms may differ across contexts. For example, although Autor *et al.* (2017) find a similar outcome – specifically that the U.S. manufacturers decrease their patenting activities when facing higher competition from Chinese import penetration – the competitive pressure from low-end, cheaper products is by no means comparable to the formation and breakup of collusion among (oligopolistic) industry leaders. The generalizability of the findings of this study require further studies and careful interpretation.

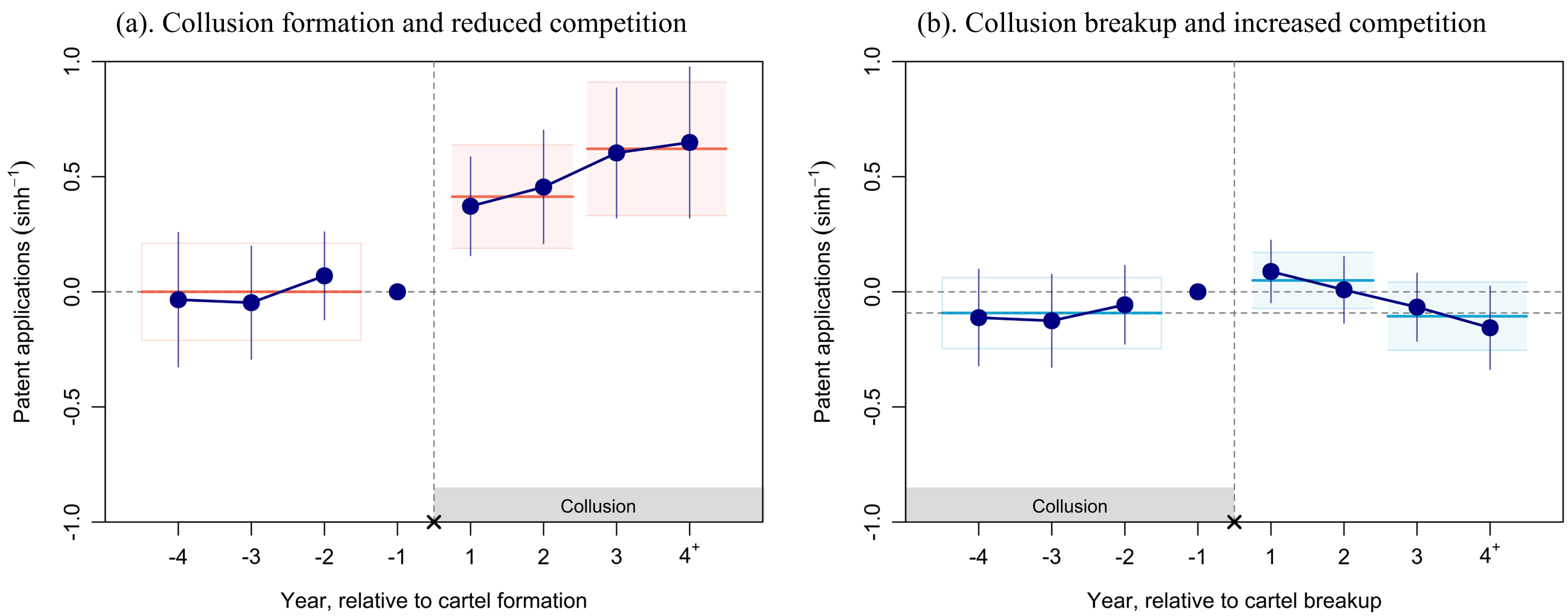
This study contributes to the literature in the following ways. First, the results broaden our understanding of the effects of competition beyond the price level. I consider another important outcome, innovation, and thereby move beyond the assumption that competition changes only the distribution (prices) of products. I indeed find that market competition further changes not only the development of products but their innovativeness as well. The findings highlight the trade-off between price competition and innovation growth. These findings lead to a more comprehensive understanding of how market competition changes firm behaviors, especially innovative activities, which are becoming more and more important in the current knowledge-based economy. Second, taking a step beyond the intensity of innovation, I shed light on the breadth and direction of innovation. This distinction enables us to investigate the relationship between competition and innovation at a deeper level – that firms not only change the intensity of innovation but also alter the breadth of their technological search and innovation. Third, I collected data on all known collusion cases and used the formation and breakup of collusion as plausibly exogenous sources of variation in the competition level. This novel approach enables researchers to measure competition and test its effects on important economic outcomes. In addition, a cartel is a highly strategic (yet illegal) agreement not to compete on prices between firms in the same market, which itself is a very interesting and important research area. Thus, new collusion data and the variation that results from collusion events provide new avenues for studying important questions in the fields of management, economics, political science, and public policy.

Figure 1.1: Collusion: 1975-2015



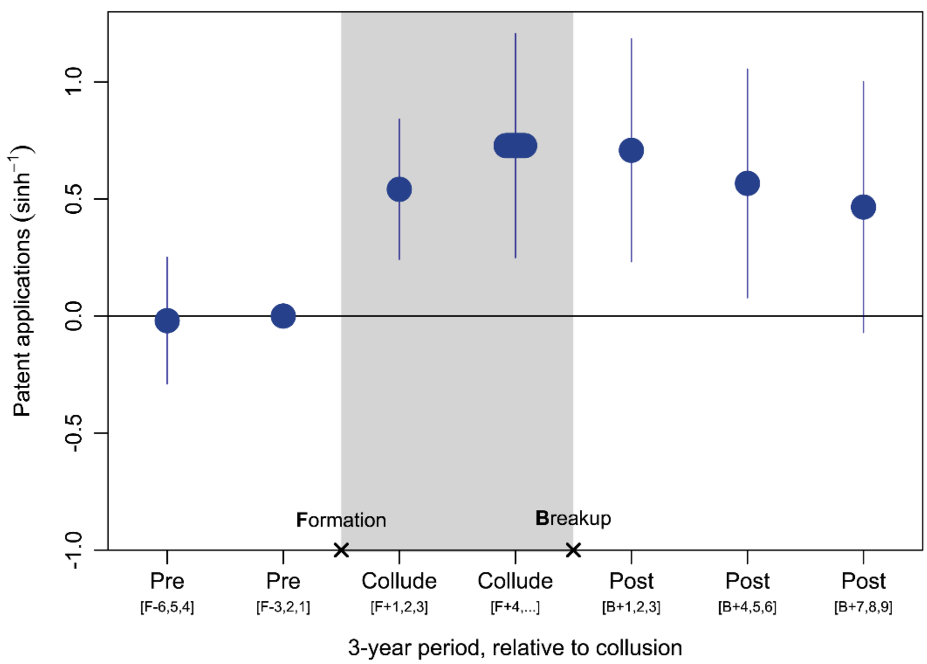
This figure shows the trend in collusion breakup and antitrust enforcement in the U.S. for 1975-2015. Brown bars show the number of collusion breakup cases in each year. The blue solid line shows the number of firms indicted for collusion in each year, whereas the blue dashed line shows the number of managers accused of participating in collusion. Collusion cases in the finance sectors (e.g., real estate brokerage, mortgage rate, interest rate) are excluded. Note that the number of collusion breakup cases is right-censored. In other words, there may be more cases of collusion breakup in 2015 that have not yet been indicted due to an on-going closed investigation. *Data*: my own data collection from the U.S. Department of Justice (DOJ) and the Trade Regulation Reporter by the Commercial Clearing House (CCH).

Figure 1.2: Effects of Collusion and Competition on Innovation: Patenting



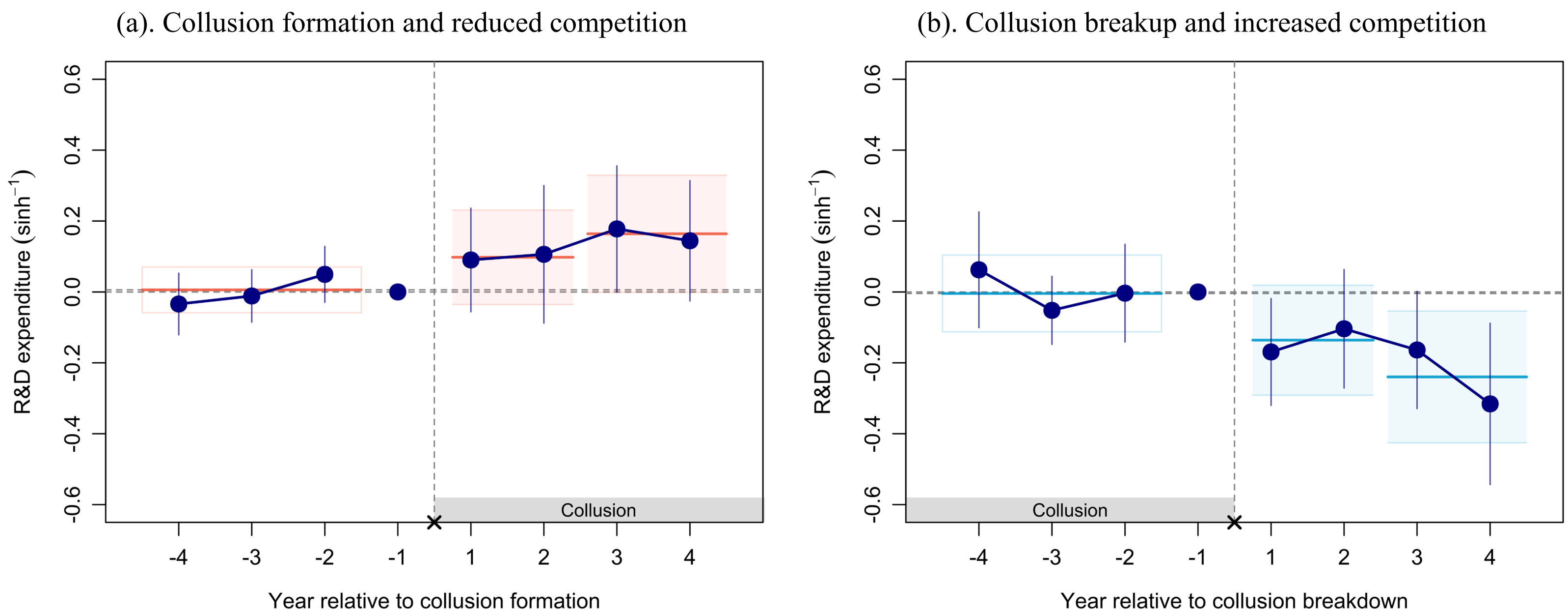
Plotted are the event-time coefficient estimates (dots) from a version of Equation (1.3), where the dependent variable consists of patent applications (that are eventually granted) with the inverse hyperbolic sine transformation in an assignee firm×year. The vertical lines represent 95% confidence intervals. Colored horizontal lines and boxes around them represent the pooled difference-in-differences estimates and 95% confidence intervals from a version of Equation (1.2), grouped by two or three years around the event of interest). The regression model controls for assignee firm fixed effects and sector×year fixed effects. A sector is defined by four-digit North American Industry Classification System. The year of collusion formation (in Panel A) or breakup (in Panel B) corresponds to year 0 in the graphs and is omitted to account for potential mis-estimation of the true year of collusion formation or breakup. Year_{-1} is used as a baseline. The superscript + on year term means that it includes two additional years for its estimation (i.e., the estimate for year 4^{+} represents the pooled estimates for years 4, 5, and 6). Standard errors are clustered at the sector level. *Data*: PatentsView.

Figure 1.3: The Life Cycle of Collusion and Innovation: Patenting



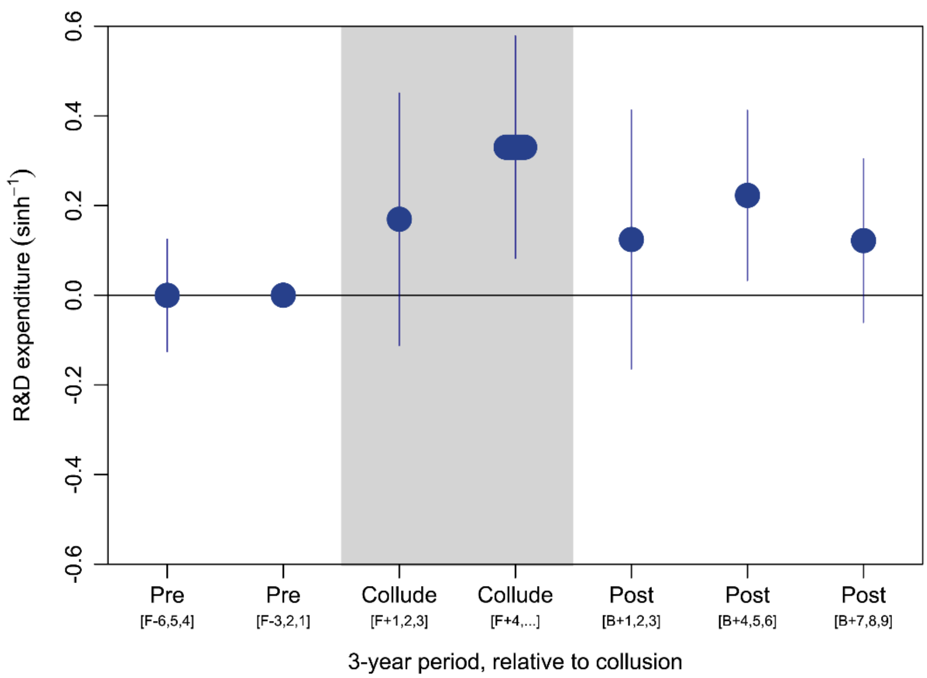
Plotted are the event-time coefficient estimates from a version of Equation (1.4), where the dependent variable consists of patent applications (that are eventually granted) with the inverse hyperbolic sine transformation in an assignee firm×year. The vertical lines represent 95% confidence intervals. This figure incorporates both the formation and breakup of collusion (Figure 1.2) to get a complete picture and compare the size of effects in a single framework. Years are grouped into seven time periods, each representing the three-year period around the events of interest. $Pre_{[F-6,5,4]}$ means 4 to 6 years prior to the formation of collusion. $Collude_{[F+1,2,3]}$ represents early collusion periods: 1 to 3 years after the formation of collusion. $Post_{[B+1,2,3]}$ means 1 to 3 years after the breakup of collusion. To account for varied collusion periods, $Collude_{[F+4,\ldots]}$ represents the fourth year of collusion and thereafter up to a year before the collusion breakup. $Pre_{[F-3,2,1]}$ serves as a baseline. The regression model controls for the assignee firm fixed effects and sector×year fixed effects. A sector is defined by the four-digit North American Industry Classification System (NAICS). Standard errors are clustered at the sector level. *Data*: PatentsView.

Figure 1.4: Effects of Collusion and Competition on Innovation: R&D Expenditure



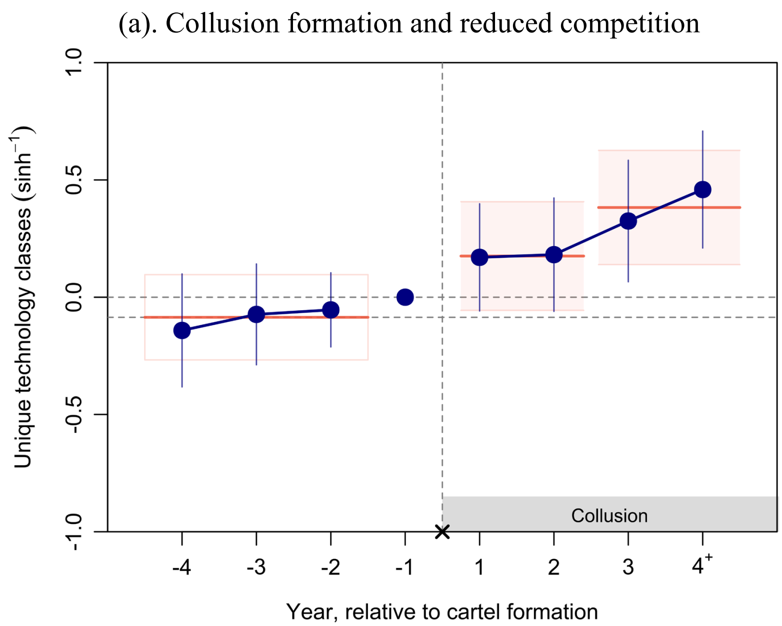
Plotted are the event-time coefficient estimates (dots) from a version of Equation (1.3), where the dependent variable consists of R&D expenditures (in million U.S. dollars) with the inverse hyperbolic sine transformation in a firm×year. The vertical lines represent 95% confidence intervals. Colored horizontal lines and boxes around them represent the pooled difference-in-differences estimates and 95% confidence intervals from a version of Equation (1.2), grouped by two or three years around the event of interest). The regression model controls for firm fixed effects and sector×year fixed effects. A sector is defined by four-digit North American Industry Classification System. The year of collusion formation (in Panel A) or breakup (in Panel B) corresponds to year 0 in the graphs and is omitted to account for potential mis-estimation of the true year of collusion formation or breakup. Year_{-1} is used as a baseline. The superscript + on year term means that it includes two additional years for its estimation (i.e., the estimate for year 4^{+} represents the pooled estimates for years 4, 5, and 6). Standard errors are clustered at the sector level. *Data*: Compustat.

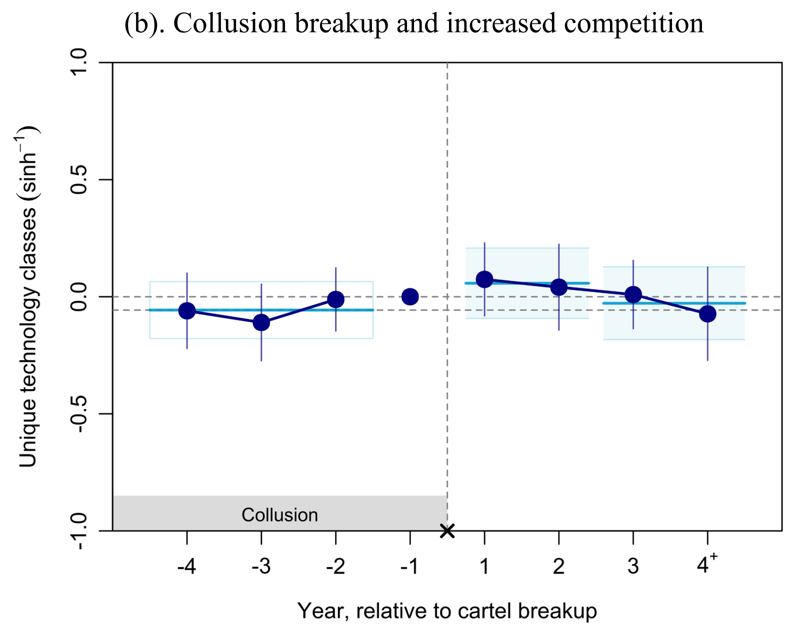
Figure 1.5: The Life Cycle of Collusion and Innovation: R&D Expenditure



Plotted are the event-time coefficient estimates from a version of Equation (1.4), where the dependent variable consists of R&D expenditure (in million U.S. dollars) with the inverse hyperbolic sine transformation in a firm×year. The vertical lines represent 95% confidence intervals. This figure incorporates both the formation and breakup of collusion (Figure 1.4) to get a complete picture and compare the size of effects in a single framework. Years are grouped into seven time periods, each representing the three-year period around the events of interest. $Pre_{[F-6,5,4]}$ means 4 to 6 years prior to the formation of collusion. $Collude_{[F+1,2,3]}$ represents early collusion periods: 1 to 3 years after the formation of collusion. $Post_{[B+1,2,3]}$ means 1 to 3 years after the breakup of collusion. To account for varied collusion periods, $Collude_{[F+4,...]}$ represents the fourth year of collusion and thereafter up to a year before the collusion breakup. $Pre_{[F-3,2,1]}$ serves as a baseline. The regression model controls for the firm fixed effects and sector×year fixed effects. A sector is defined by the four-digit North American Industry Classification System (NAICS). Standard errors are clustered at the sector level. *Data*: PatentsView.

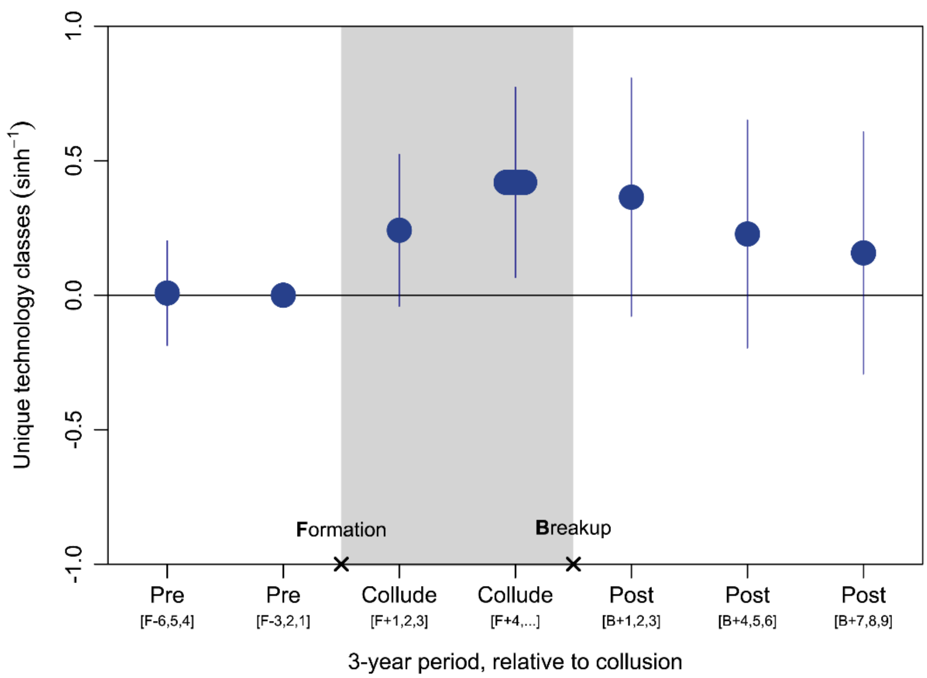
Figure 1.6: Effects of Collusion and Competition on Innovation: Breadth of Patenting





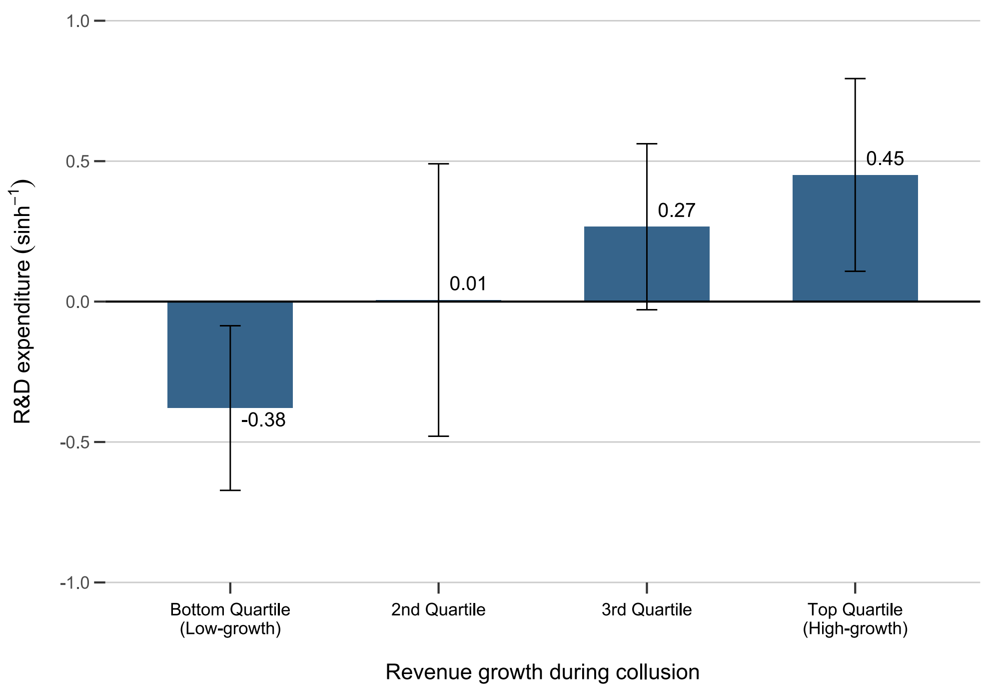
Plotted are the event-time coefficient estimates (dots) from a version of Equation (1.3), where the dependent variable consists of total number of unique patent classes (3-digit CPC) with the inverse hyperbolic sine transformation in an assignee firm×year. The vertical lines represent 95% confidence intervals. Colored horizontal lines and boxes around them represent the pooled difference-in-differences estimates and 95% confidence intervals from a version of Equation (1.2), grouped by two or three years around the event of interest). The regression model controls for assignee firm fixed effects and sector×year fixed effects. A sector is defined by four-digit North American Industry Classification System. The year of collusion formation (in Panel A) or breakup (in Panel B) corresponds to year 0 in the graphs and is omitted to account for potential mis-estimation of the true year of collusion formation or breakup. Year_{-1} is used as a baseline. The superscript + on year term means that it includes two additional years for its estimation (i.e., the estimate for year 4^{+} represents the pooled estimates for years 4, 5, and 6). Standard errors are clustered at the sector level. *Data*: PatentsView.

Figure 1.7: The Life Cycle of Collusion and Innovation: Technology Classes



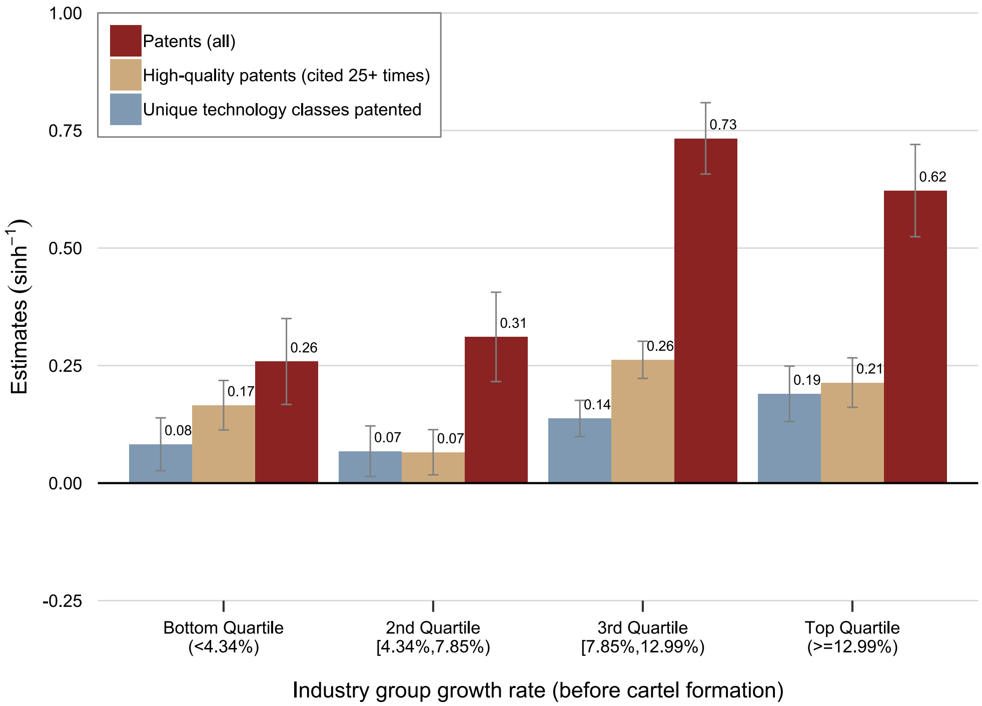
Plotted are the event-time coefficient estimates from a version of Equation (1.4), where the dependent variable consists of total number of unique patent classes (4-digit CPC) with the inverse hyperbolic sine transformation in an assignee firm×year. The vertical lines represent 95% confidence intervals. This figure incorporates both the formation and breakup of collusion (Figure 1.2) to get a complete picture and compare the size of effects in a single framework. Years are grouped into seven time periods, each representing the three-year period around the events of interest. $Pre_{[F-6,5,4]}$ means 4 to 6 years prior to the formation of collusion. $Collude_{[F+1,2,3]}$ represents early collusion periods: 1 to 3 years after the formation of collusion. $Post_{[B+1,2,3]}$ means 1 to 3 years after the breakup of collusion. To account for varied collusion periods, $Collude_{[F+4,\ldots]}$ represents the fourth year of collusion and thereafter up to a year before the collusion breakup. $Pre_{[F-3,2,1]}$ serves as a baseline. The regression model controls for the assignee firm fixed effects and sector×year fixed effects. A sector is defined by the four-digit North American Industry Classification System (NAICS). Standard errors are clustered at the sector level. *Data*: PatentsView.

Figure 1.8: Economic Mechanism: R&D Expenditure by Financial Constraints



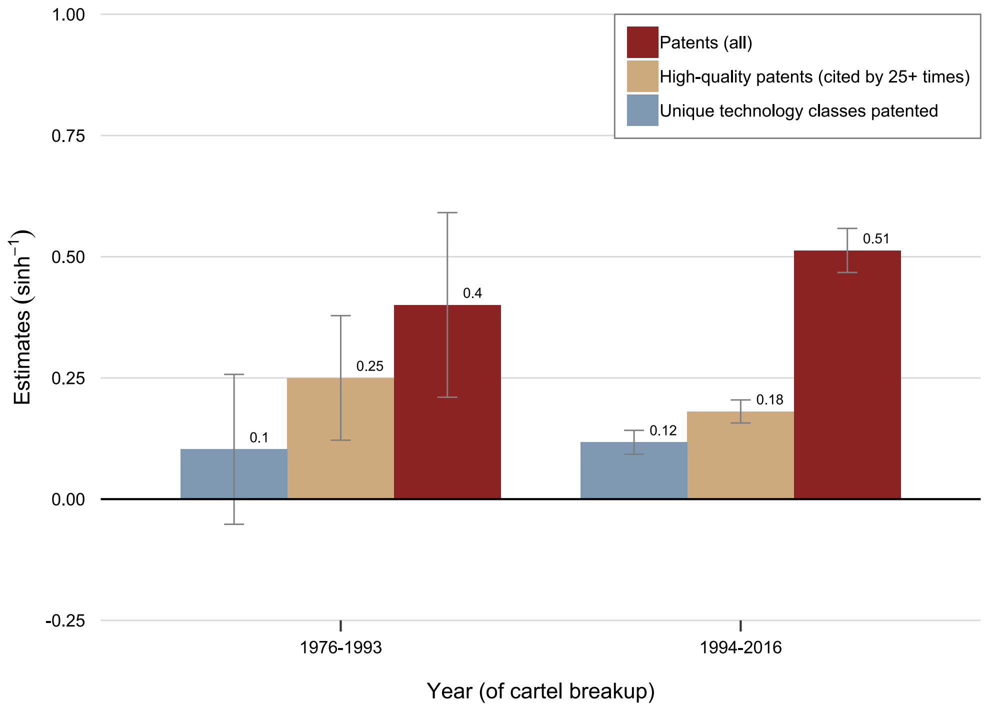
Plotted are the difference-in-differences coefficient estimates from four separate regressions based on Equation (1.1). Firms in the treatment group are sub-grouped by their revenue growth from pre-collusion ($t \in [-5, -1]$) to collusion periods ($t \in [1,5]$). Cutoffs for quartiles are 26.88% (lower quartile), 38.51% (median), and 68.55% (upper quartile). The dependent variable consists of log R&D expenditure in a firm×year. Numbers above or below the bar show regression estimates, whereas vertical bars represent 95% confidence intervals. The regression model controls for firm fixed effect and major group (two-digit SIC)×year fixed effect. *Data*: Compustat.

Figure 1.9: Economic Mechanism: Innovation Growth Rate by Industry Group



Plotted are the difference-in-differences coefficient estimates from 12 separate regressions – three outcomes of interest for four quartile groups – based on Equation (1.1). Innovation growth rates are measured at the industry group level (i.e., 4-digit NAICS), and each colluding firms (along with their counterfactual firms) are divided into four quartile groups based on this rate. The formation of collusion is used as an event of interest. The dependent variable consists of patent applications (red colored bars), patents with more than 25 non-self forward citations (brown bars), and the number of unique technology classes patented, all transformed by the inverse hyperbolic sine function, in an assignee firm×year. Numbers above the bar show regression estimates, whereas vertical bars represent 95% confidence intervals. The regression model controls for assignee firm fixed effect and industry group (4-digit NAICS)×year fixed effect. *Data*: PatentsView.

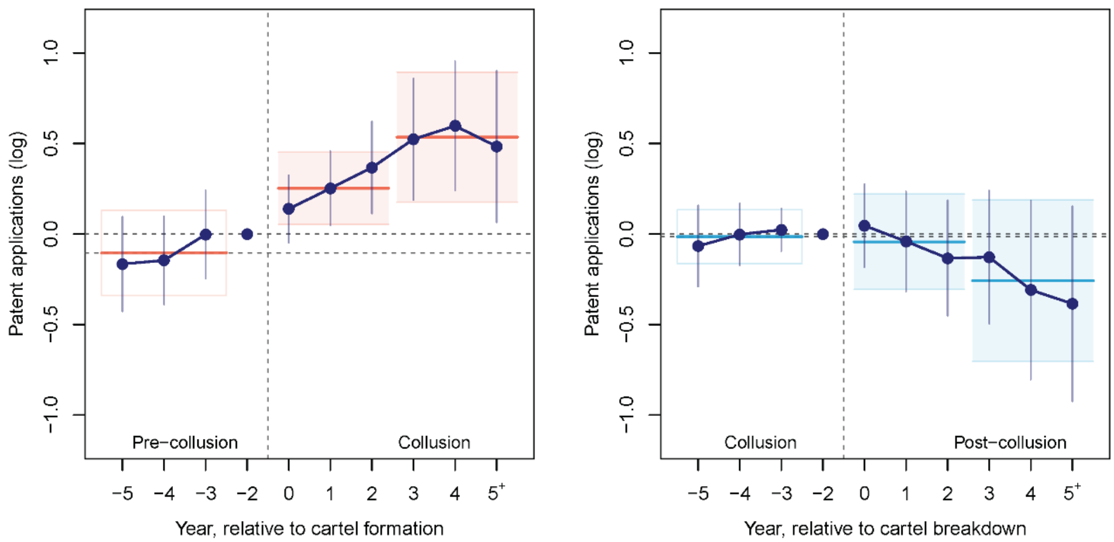
Figure 1.10: Effects of Collusion and Competition on Innovation: Temporal Heterogeneity



Plotted are the difference-in-differences coefficient estimates from 6 separate regressions (three outcomes for two time periods) based on Equation (1.1) with the formation of collusion as an event of interest. The dependent variable consists of patent applications (red colored bars), patents with more than 25 non-self forward citations (brown bars), and the number of unique technology fields patented, all transformed by the inverse hyperbolic sine function, in an assignee firm×year. Numbers above the bar show regression estimates, whereas vertical bars represent 95% confidence intervals. The regression model controls for assignee firm fixed effect and industry group (4-digit NAICS)×year fixed effect. *Data*: PatentsView.

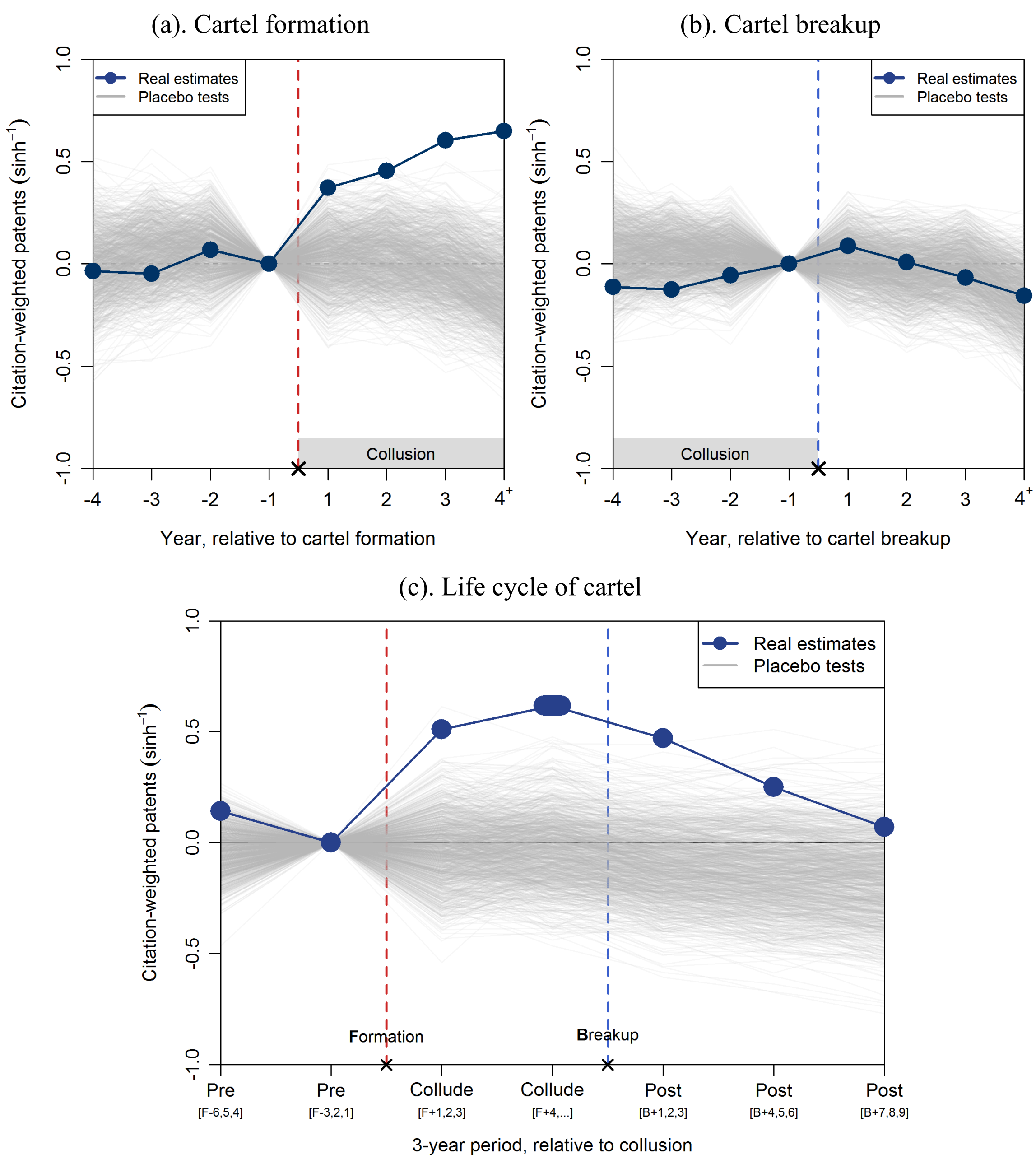
Figure 1.11: Effects of Collusion and Competition on Innovation: Pairwise Regression with Synthetic Control Method

(a). Reduced competition by collusion formation (b). Increased competition by collusion breakup



Plotted are the event-time coefficient estimates (dots) from a version of Equation (1.3), where the dependent variable consists of log patent applications in an assignee firm×year. The vertical lines represent 95% confidence intervals. Colored horizontal lines and boxes around them represent the pooled difference-in-differences estimates and 95% confidence intervals from a version of Equation (1.6), grouped by two or three years around the event of interest). Sample consists of the pairs of colluding firms (treatment group) and their corresponding synthetic control (control group). In other words, for each colluded firm, I match a single synthetic control which is a weighted average of all other firms in the control pool. Some pairs (<10) are omitted due to the computational failure to synthesize the adequate synthetic control. The regression model controls for the pair fixed effects and the year fixed effects. A sector is defined by the four-digit North American Industry Classification System. The year of collusion formation (in Panel A) or breakup (in Panel B) corresponds to year 0 in the graphs and is omitted to account for potential mis-estimation of the true year of collusion formation or breakup. Year -1 is used as a baseline. The superscript + on year term means that it includes two additional years for its estimation (i.e., the estimate for year 5^+ represents the pooled estimates for years 5, 6, and 7). Standard errors are clustered at the sector level. Standard errors may be overestimated in this specification. *Data*: PatentsView.

Figure 1.12: The Life Cycle of Collusion and Citation-Weighted Patents: A Placebo Test



Plotted are the event-time coefficient estimates from a version of Equation (1.3) (panels a and b) and Equation (1.4) (panel c). The dependent variable consists of citation-weighted patents with the inverse hyperbolic sine transformation in an assignee firm×year. Blue lines with white points represent the real treatment group (colluded firms), whereas gray lines show the results for placebo tests. In the placebo tests, treatment indicator is randomly reassigned to five firms from the pool of both colluded and non-colluded firms that belong to the same 6-digit NAICS industry. This random assignment simulation is repeated for 1,000 times. *Data*: PatentsView

Table 1.1: Descriptive Statistics: Collusion Data

	Mean	Std. Dev.	Min	Median	Max
A. Collusion level (N=461)					
Duration (year)	6.275	5.265	1.000	5.000	36.000
Number of firms indicted	4.335	5.713	1.000	3.000	47.000
Number of managers indicted	5.294	6.501	1.000	3.000	44.000
Total criminal fine for firms ($Mil)	25.200	156.520	0.000	0.300	1,902.630
Total criminal fine for managers ($Mil)	0.224	12.765	0.000	0.000	31.3232
B. Firm level (N=1,818)					
Criminal fine ($Mil)	8.361	38.772	0.000	0.200	
Sum of all criminal fine ($Mil)	10676.570				
C. Individual level (N=1,623)					
Criminal fine ($Mil)	0.133	1.167	0.000	0.025	29.603
Sum of all criminal fine ($Mil)	98.881				
Prison sentence (days)	360.8	441.133	1.000	182.000	5,110.000
Sum of all prison sentence (days)	203,878				

Note. This table shows the descriptive statistics for all non-financial collusion cases in the U.S. for 1975-2015 at the collusion, firm, and individual manager level, respectively. Collusion cases in the finance sectors (e.g., real estate brokerage, mortgage rate, interest rate) are excluded. *Data*: author's own data collection from the U.S. Department of Justice (DOJ) and the Trade Regulation Reporter by the Commercial Clearing House (CCH).

Table 1.2: Descriptive Statistics: Patent and Compustat Data

(a). Patent data (assignee firm level)

	Obs	Mean	Std. Dev.	Min	Median	Max
Year	1,850,632	1998	10.77	1970	2000	2016
Patent	1,850,632	3.06	36.75	0.00	1.00	8,916.00
Citation-weighted patents	1,840,762	37.89	493.99	1.00	1.00	152,653.00
Patents in main class	1,840,762	1.36	11.86	0.00	1.00	4,289.00
Patents in peripheral class	1,840,762	2.18	25.72	0.00	1.00	4,658.00
Technology classes	1,016,768	2.87	10.62	1.00	1.00	547.00
Backward citations	1,016,768	14.57	31.53	0.00	7.67	5,834.50
Forward citations	1,016,768	13.76	34.05	0.00	5.00	2,753.00

(b). Compustat data (firm level)

	Obs	Mean	Std. Dev.	Min	Median	Max
Year	400,931	1995	12.47	1970	1996	2016
Employment	311,636	7.25	32.52	0.00	0.61	2,545.21
Capital expenditure	326,126	128.05	892.51	0.00	2.78	65,028.00
R&D expenditure	162,633	61.35	419.54	-0.65	1.33	16,085.00

Note. The two tables report descriptive statistics for patent and Compustat data, respectively. Panel (a) shows the pooled (cross-sectional) descriptive statistics for the patent data (1976-2016) at the assignee firm level. Assignee firms are identified by name disambiguated *assignee_id* provided by PatentsView. *Source*: PatentsView (May 28, 2018 version). Panel (b) shows the pooled (cross-sectional) descriptive statistics for the Compustat data (1970-2016) at the firm level. Firms are identified by Compustat ID (*GVKEY*). Descriptive statistics are calculated for all firms that operated at least two years in the sample period (1975-2016). I set negative *XRD* and *CAPX* values to zero because R&D and capital expenditures should not be negative. *Data*: Compustat.

Table 1.3: Effects of Collusion and Competition on the Intensity of Innovation

(a). Cartel formation and the suppression of competition

	Dependent variables (log):				
	Patents	Cite-weighted Patents	Patents ($Cite \geq 10$)	Patents ($Cite \geq 25$)	R&D Expenditure
	(1)	(2)	(3)	(4)	(5)
Treat × Post	0.507***	0.547***	0.305***	0.201***	0.117
	(0.115)	(0.163)	(0.077)	(0.059)	(0.079)
Observations	444,172	444,172	444,172	444,172	135,199
R^2	0.566	0.490	0.483	0.454	0.922
Adjusted R^2	0.452	0.355	0.347	0.310	0.910

(b). Cartel breakup and the recovery of competition

	Dependent variables (log):				
	Patents	Cite-weighted Patents	Patents ($Cite \geq 10$)	Patents ($Cite \geq 25$)	R&D Expenditure
	(1)	(2)	(3)	(4)	(5)
Treat × Post	0.043	−0.059	−0.118	−0.110	−0.183*
	(0.069)	(0.125)	(0.116)	(0.077)	(0.094)
Observations	444,248	444,248	444,248	444,248	135,258
R^2	0.570	0.490	0.481	0.452	0.922
Adjusted R^2	0.456	0.355	0.344	0.307	0.911

Note. These tables report regression coefficients from 10 separate regressions based on Equation (1.1). The top table uses cartel formation as an event, whereas the bottom table uses cartel breakup as an event. The dependent variable consists of patent applications (column 1), citation-weighted patents (columns 2), patent counts with more than 10 and 25 non-self forward citations (columns 3 and 4), and R&D expenditure (column 5), all transformed by the inverse hyperbolic sine function, in a firm×year. *Treat* is an indicator variable that takes the value of 1 for colluding firms and 0 otherwise. *Post* is an indicator variable that takes the value of 1 for the post-event (either collusion formation or breakup) period and 0 otherwise. A sector is defined by the four-digit North American Industry Classification System. All of the regressions control for firm fixed effects and sector×year fixed effects. Standard errors are in parentheses and are clustered by sector. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively. *Data*: PatentsView and Compustat.

Table 1.4: Effects of Collusion and Competition on the Intensity of Innovation: Flexible Approach

(a). Cartel formation and the suppression of competition

	Dependent variables (log):				
	Patents	Cite-weighted Patents	Patents ($Cite \geq 10$)	Patents ($Cite \geq 25$)	R&D Expenditure
	(1)	(2)	(3)	(4)	(5)
Treat × $\text{Pre}_{[-4:-2]}$	0.0002	0.159	0.039	0.058	0.006
	(0.108)	(0.214)	(0.081)	(0.063)	(0.033)
Treat × $\text{Post}_{[1:2]}$	0.413^{***}	0.563^{***}	0.305^{***}	0.226^{***}	0.098
	(0.115)	(0.218)	(0.088)	(0.071)	(0.068)
Treat × $\text{Post}_{[3:4+]}$	0.621^{***}	0.772^{***}	0.365^{***}	0.260^{***}	0.164^{*}
	(0.148)	(0.232)	(0.097)	(0.080)	(0.084)
Observations	444,172	444,172	444,172	444,172	135,199
R^2	0.566	0.490	0.483	0.454	0.922
Adjusted R^2	0.452	0.355	0.347	0.310	0.910

(b). Cartel breakup and the recovery of competition

	Dependent variables (log):				
	Patents	Cite-weighted Patents	Patents ($Cite \geq 10$)	Patents ($Cite \geq 25$)	R&D Expenditure
	(1)	(2)	(3)	(4)	(5)
Treat × $\text{Pre}_{[-4:-2]}$	−0.092	−0.016	0.085	0.062	−0.004
	(0.079)	(0.154)	(0.101)	(0.061)	(0.055)
Treat × $\text{Post}_{[1:2]}$	0.050	0.054	0.011	−0.015	-0.136^{*}
	(0.062)	(0.118)	(0.065)	(0.059)	(0.079)
Treat × $\text{Post}_{[3:4+]}$	−0.106	−0.238	-0.167^{**}	-0.146^{**}	-0.240^{**}
	(0.076)	(0.147)	(0.083)	(0.071)	(0.095)
Observations	444,172	444,172	444,172	444,172	135,199
R^2	0.566	0.490	0.483	0.454	0.922
Adjusted R^2	0.452	0.355	0.347	0.310	0.910

Note. These tables report regression coefficients from 10 separate regressions based on Equation (1.2). The top table uses cartel formation as an event, whereas the bottom table uses cartel breakup as an event. The dependent variable consists of patent applications (column 1), citation-weighted patents (columns 2), patent counts with more than 10 and 25 non-self forward citations (columns 3 and 4), and R&D expenditure (column 5), all transformed by the inverse hyperbolic sine function, in a firm×year. *Treat* is an indicator variable that takes the value of 1 for colluding firms and 0 otherwise. $Pre_{[-4:-2]}$ is an indicator variable that takes the value of 1 for -4 to -2 years prior to collusion formation or breakup and 0 otherwise. $Post_{[1:2]}$ is an indicator variable that takes the value of 1 for the first two years of collusion or its breakup and 0 otherwise. $Post_{[3:4+]}$ is an indicator variable that takes the value of 1 for the third year of collusion formation/breakup and thereafter and 0 otherwise. $Pre_{[0]}$ is omitted, and $Pre_{[-1]}$ serves as a baseline. A sector is defined by the four-digit North American Industry Classification System. All of the regressions implicitly or explicitly control for firm fixed effects and sector×year fixed effects. Standard errors are in parentheses and are clustered by industry group (4-digit NAICS). $^{***},^{**},^{*}$ denotes statistical significance at the 1%, 5%, and 10% levels, respectively. *Data*:

Table 1.5: The Life Cycle of Collusion and the Intensity and Breadth of Innovation

	Dependent variables (log):						
	Patents	Cite-weighted Patents	Patents ($Cite \geq 10$)	Patents ($Cite \geq 25$)	R&D Expenditure	Technology Classes	Tech-weighted Patents
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treat × $\text{Pre}_{[-6:-4]}$	−0.020	0.137	0.037	0.020	−0.0003	0.008	−0.002
	(0.138)	(0.237)	(0.091)	(0.062)	(0.064)	(0.099)	(0.160)
Treat × $\text{Collude}_{[1:3]}$	0.541^{***}	0.523^{**}	0.164	0.137	0.169	0.242^{*}	0.418^{**}
	(0.153)	(0.237)	(0.135)	(0.102)	(0.144)	(0.144)	(0.206)
Treat × $\text{Collude}_{[4+]}$	0.728^{***}	0.630^{**}	0.112	0.077	0.330^{***}	0.419^{**}	0.722^{**}
	(0.244)	(0.282)	(0.132)	(0.105)	(0.127)	(0.180)	(0.282)
Treat × $\text{Post}_{[1:3]}$	0.708^{***}	0.485^{*}	−0.044	−0.046	0.124	0.365	0.656^{**}
	(0.243)	(0.267)	(0.220)	(0.163)	(0.147)	(0.226)	(0.325)
Treat × $\text{Post}_{[4:6]}$	0.567^{**}	0.270	−0.228	−0.171	0.223^{**}	0.228	0.517
	(0.249)	(0.295)	(0.223)	(0.169)	(0.097)	(0.216)	(0.329)
Treat × $\text{Post}_{[7:9]}$	0.466^{*}	0.090	−0.440*	-0.320^{*}	0.122	0.157	0.407
	(0.273)	(0.325)	(0.234)	(0.175)	(0.093)	(0.229)	(0.340)
Observations	475,300	475,300	475,300	475,300	135,543	246,540	246,540
R^2	0.578	0.500	0.4891	0.463	0.922	0.660	0.664
Adjusted R^2	0.459	0.359	0.347	0.311	0.911	0.413	0.420

Note. This table reports regression coefficients from seven separate regressions based on Equation (1.4) where the dependent variable consists of patent applications (column 1), citation-weighted patents (column 2), patent counts with more than 10 and 25 non-self forward citations (columns 3 and 4), R&D expenditure (column 5), total number of unique technology classes patented (column 6), technology class-weighted patents (column 7), all transformed by the inverse hyperbolic sine function, in a firm×year. Treat is an indicator variable that takes the value of 1 for colluding firms and 0 otherwise. Years are grouped into seven time periods, each representing the three-year period around the events of interest into one time group. $Pre_{[a:b]}$ means a to b years prior to the formation of collusion. $Collude_{[c:d]}$ represents early collusion periods: c to d years after the formation of collusion. $Post_{[e:f]}$ means e to f years after the breakup of collusion. To account for varied collusion periods, $Collude_{[4+]}$ represents the fourth year of collusion and thereafter up to a year before the collusion breakup. $Pre_{[-3:-1]}$ serves as a baseline. The regression model controls for the assignee firm fixed effects and sector×year fixed effects. A sector is defined by the four-digit North American Industry Classification System (NAICS). Standard errors are in parentheses and are clustered by sector. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively. *Data*: PatentsView and Compustat.

Table 1.6: Effects of Collusion and Competition on the Breadth of Innovation

(a). Cartel formation and the suppression of competition

	Dependent variables (log):			
	# Tech Classes	Tech-weighted Patents	Patents in Primary Tech Area	Patents in Peripheral Tech Area
	(1)	(2)	(3)	(4)
Treat × Post	0.328^{***}	0.509^{***}	0.419^{***}	0.353^{***}
	(0.109)	(0.156)	(0.093)	(0.103)
Observations	229,672	229,672	444,172	444,172
R^2	0.648	0.652	0.509	0.540
Adjusted R^2	0.414	0.421	0.379	0.419

(b). Cartel breakup and the recovery of competition

	Dependent variables (log):			
	# Tech Classes	Tech-weighted Patents	Patents in Primary Tech Area	Patents in Peripheral Tech Area
	(1)	(2)	(3)	(4)
Treat × Post	0.058	0.098	0.058	0.030
	(0.083)	(0.099)	(0.064)	(0.060)
Observations	229,793	229,793	444,248	444,248
R^2	0.652	0.656	0.512	0.544
Adjusted R^2	0.420	0.427	0.383	0.424

Note. These tables report regression coefficients from 10 separate regressions based on Equation (1.1). The top table uses cartel formation as an event, whereas the bottom table uses cartel breakup as an event. The dependent variable consists of (1) total number of unique technology classes patented (column 1), (2) technology class-weighted patents (column 2), (3) patent applications in the primary technological area of an assignee firm (column 3), and (4) patent applications in the peripheral technological field of an assignee firm (column 4), all transformed by the inverse hyperbolic sine function, in a firm×year. *Treat* is an indicator variable that takes the value of 1 for colluding firms and 0 otherwise. *Post* is an indicator variable that takes the value of 1 for the post-event (either collusion formation or breakup) period and 0 otherwise. A sector is defined by the four-digit North American Industry Classification System. All of the regressions control for firm fixed effects and sector×year fixed effects. Standard errors are in parentheses and are clustered by sector. ***, **, * denotes statistical significance at the 1%, 5%, and 10% levels, respectively. *Data*: PatentsView and Compustat.

Table 1.7: The Life Cycle of Collusion and Innovation: Industry-wide Aggregate Effects

	Dependent variables (log):						
	Patents	Cite-weighted Patents	Patents ($Cite \geq 10$)	Patents ($Cite \geq 25$)	R&D Expenditure	Technology Classes	Tech-weighted Patents
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treat × $\text{Pre}_{[-6:-4]}$	0.003	0.068	0.002	−0.001	−0.059	0.015	0.003
	(0.069)	(0.109)	(0.069)	(0.063)	(0.138)	(0.047)	(0.064)
Treat × $\text{Collude}_{[1:3]}$	0.134*	0.077	0.090	0.085	−0.053	0.034	0.126*
	(0.079)	(0.121)	(0.066)	(0.058)	(0.116)	(0.048)	(0.068)
Treat × $\text{Collude}_{[4+]}$	0.174*	0.063	0.076	0.076	0.002	0.031	0.162*
	(0.103)	(0.133)	(0.094)	(0.084)	(0.160)	(0.055)	(0.093)
Treat × $\text{Post}_{[1:3]}$	0.168	−0.069	−0.0004	0.039	−0.041	0.054	0.173*
	(0.111)	(0.145)	(0.113)	(0.101)	(0.277)	(0.067)	(0.105)
Treat × $\text{Post}_{[4:6]}$	0.119	−0.130	−0.021	0.011	−0.033	0.0001	0.095
	(0.118)	(0.167)	(0.112)	(0.100)	(0.239)	(0.069)	(0.102)
Treat × $\text{Post}_{[7:9]}$	0.212	−0.020	−0.015	0.054	−0.004	0.031	0.165
	(0.147)	(0.189)	(0.141)	(0.121)	(0.251)	(0.081)	(0.130)
Observations	21,220	21,220	21,220	21,220	23,497	18,560	18,560
R^2	0.955	0.919	0.958	0.961	0.853	0.962	0.962
Adjusted R^2	0.912	0.839	0.917	0.922	0.816	0.920	0.919

Note. This table reports regression coefficients from seven separate regressions based on Equation (1.8) where the dependent variable consists of patent applications (column 1), citation-weighted patents (column 2), patent counts with more than 10 and 25 non-self forward citations (columns 3 and 4), R&D expenditure (column 5), total number of unique technology classes patented (column 6), technology class-weighted patents (column 7), all transformed by the inverse hyperbolic sine function, in an industry×year. Treat is an indicator variable that takes the value of 1 for colluding firms and 0 otherwise. Years are grouped into seven time periods, each representing the three-year period around the events of interest into one time group. $Pre_{[a:b]}$ means a to b years prior to the formation of collusion. $Collude_{[c:d]}$ represents early collusion periods: c to d years after the formation of collusion. $Post_{[e:f]}$ means e to f years after the breakup of collusion. To account for varied collusion periods, $Collude_{[4+]}$ represents the fourth year of collusion and thereafter up to a year before the collusion breakup. $Pre_{[-3:-1]}$ serves as a baseline. The regression model controls for the industry fixed effects and sector×year fixed effects. A sector is defined by the four-digit North American Industry Classification System (NAICS), and an industry is defined by the six-digit NAICS. Standard errors are in parentheses and are clustered by sector. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively. *Data*: PatentsView and Compustat.

Table 1.8: The Life Cycle of Collusion and the Intensity and Breadth of Innovation

	Dependent variables (log):						
	Patents	Patents: CW	Patents ($Cite \geq 10$)	Patents ($Cite \geq 25$)	R&D Investment	Tech Classes	Patents: TW
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treat × $\text{Pre}_{[-6:-4]}$	−0.020	0.137	0.037	0.020	−0.0003	0.008	−0.002
	(0.138)	(0.237)	(0.091)	(0.062)	(0.064)	(0.099)	(0.160)
Treat × $\text{Collude}_{[1:3]}$	0.541^{***}	0.523^{**}	0.164	0.137	0.169	0.242^{*}	0.418^{**}
	(0.153)	(0.237)	(0.135)	(0.102)	(0.144)	(0.144)	(0.206)
Treat × $\text{Collude}_{[4+]}$	0.728^{***}	0.630^{**}	0.112	0.077	0.330^{***}	0.419^{**}	0.722^{**}
	(0.244)	(0.282)	(0.132)	(0.105)	(0.127)	(0.180)	(0.282)
Treat × $\text{Post}_{[1:3]}$	0.708^{***}	0.485^{*}	−0.044	−0.046	0.124	0.365	0.656^{**}
	(0.243)	(0.267)	(0.220)	(0.163)	(0.147)	(0.226)	(0.325)
Treat × $\text{Post}_{[4:6]}$	0.567^{**}	0.270	−0.228	−0.171	0.223^{**}	0.228	0.517
	(0.249)	(0.295)	(0.223)	(0.169)	(0.097)	(0.216)	(0.329)
Treat × $\text{Post}_{[7:9]}$	0.466^{*}	0.090	−0.440*	-0.320^{*}	0.122	0.157	0.407
	(0.273)	(0.325)	(0.234)	(0.175)	(0.093)	(0.229)	(0.340)
Observations	475,300	475,300	475,300	475,300	135,543	246,540	246,540
R^2	0.578	0.500	0.4891	0.463	0.922	0.660	0.664
Adjusted R^2	0.459	0.359	0.347	0.311	0.911	0.413	0.420

Note. This table reports regression coefficients from seven separate regressions based on Equation (1.4) where the dependent variable consists of patent applications (column 1), citation-weighted patents (column 2), patent counts with more than 10 and 25 non-self forward citations (columns 3 and 4), R&D expenditure (column 5), total number of unique technology classes patented (column 6), technology class-weighted patents (column 7), all transformed by the inverse hyperbolic sine function, in a firm×year. Treat is an indicator variable that takes the value of 1 for colluding firms and 0 otherwise. Years are grouped into seven time periods, each representing the three-year period around the events of interest into one time group. $Pre_{[a:b]}$ means *a* to *b* years prior to the formation of collusion. $Collude_{[c:d]}$ represents early collusion periods: *c* to *d* years after the formation of collusion. $Post_{[e:f]}$ means *e* to *f* years after the breakup of collusion. To account for varied collusion periods, $Collude_{[4+]}$ represents the fourth year of collusion and thereafter up to a year before the collusion breakup. $Pre_{[-3:-1]}$ serves as a baseline. The regression model controls for the assignee firm fixed effects and sector×year fixed effects. A sector is defined by the four-digit North American Industry Classification System (NAICS). Standard errors are in parentheses and are clustered by sector. ***,**,* denotes statistical significance at the 1%, 5%, and 10% levels, respectively. *Data*: PatentsView and Compustat.

2 Non-competes, Business Dynamism, and Concentration: Evidence from a Florida Case Study

2.1 Introduction

Much recent research has documented a trend of increasing industry concentration, possibly due to scale and network effects (Shambaugh *et al.*, 2018), deregulation (De Loecker, Eeckhout, and Unger, 2018), or efficiencies of scale, mergers and acquisitions, innovation, or regulatory barriers (Council of Economic Advisors, 2016). Other work has documented a broad decline in business dynamism across many sectors in the U.S, including a flat trend in firm exit and declining trends in firm entry and job reallocation (Hathaway and Litan, 2014) and a decrease in entrepreneurship (Haltiwanger, Jarmin, and Miranda, 2011; Kauffman, 2016). Hathaway and Litan (2014) comment that, "Whatever the reason, older and larger businesses are doing relatively better to younger and smaller ones." A policy brief from the White House (2016) documents a decline in competition, new firm formation, and business dynamism - and associates these trends with state level non-compete laws that typically decrease workers' mobility. Scatter plots at the state level, illustrated in Figure 2.1, illustrate positive relationships between non-competes and the share of large firms, job creation by large firms, and regional business concentration. Such plots, however, are static and bivariate, surely mask omitted variable bias, and like other work that has only documented the trends, "…remain[ed] silent on the causes." (De Loecker, Eeckhout, and Unger, 2018; p. 32)

To investigate one dynamic that could give rise to increased business concentration, we identify a clear change in one state's non-compete laws, a subsequent change in establishment entry and employment by firm size, and a consistent effect on business concentration. We begin by documenting recent changes in non-compete laws across all U.S. states and establish that Florida's 1996 non-compete law provides an unambiguous step change in the strength of enforcement. Other states have also changed their non-compete laws, though not as cleanly for the purposes of isolating the impact of non-competes on business concentration. For example, Michigan's 1985 change – the Michigan Anti-trust Reform Act – was explicitly intended to increase competitiveness; the legislators and analysts had no intent to change non-compete law (Marx, Strumsky, and Fleming, 2009). Adding to the attractiveness of the research site, there appears to have been little change in institutional and electoral influences, and Florida's wage trends remained stable over the time period of study. Florida's experience appears internally consistent and provides a plausible pathway from non-compete enforcement to business concentration. We discuss and illustrate possible mechanisms, but hesitate to claim wide applicability, due to the difficulty of generalizing across the many idiosyncrasies that accompany each state's change in non-compete laws, and the many potential influences on business concentration.

Florida's sharp legislative change in non-compete enforcement enables illustration of how stronger non-compete laws might alter business dynamism and the regional size distribution of firms. The law change appears to have favored and attracted establishments of larger firms, and such firms created more new jobs. Stronger enforcement did not increase the establishment of start-ups, the arrival of small firms to the state, and job creation by such firms. Consistent with these trends, we find a significant increase in business concentration measures following Florida's strengthening of non-competes. These results are robust to analyzing adjacent counties on Florida's borders, synthetic matching, industry matching, and placebo tests, and are consistent with a nationwide cross section of states' noncompete enforcement and shares of establishment entry, employment growth, and business concentration.

2.2 Employee Non-Competes

> *If you are a chief executive of a large company, you very likely have a non-compete clause in your contract, preventing you from jumping ship to a competitor until some period has elapsed. Likewise if you are a top engineer or product designer, holding your company's most valuable intellectual property between your ears. And you also probably have a non-compete agreement if you assemble sandwiches at Jimmy John's sub sandwich chain for a living (New York Times, Oct 14, 2014).*

Covenants not to compete ("non-competes") are agreements in which an employee agrees not to work for the current employer's direct competitors in a specified area for a certain amount of time. They are becoming increasingly prevalent in many industries besides high technology (Starr, 2015); 351 of 500 U.S. firms (70.2%) reported that they had non-compete agreements with their top executives from 1992 to 2004 (Garmaise, 2009).[17] Amazon requires their warehouse employees, including part-time laborers, to sign non-competes, under which they will not work at "any company where they directly or indirectly support any good or service that competes with those they helped support at Amazon (The Verge, 2015)".[18] Physicians, dentists, accountants, and even lawyers can be subject to non-competes (Tanick and Troubaugh, 2012).

Non-competes have developed in part because employers typically prefer labor contracts with mechanisms that aid in the retention of desirable employees. Such contracts respond to concerns about employee separation and are often intended to mitigate the market failure of under-investment in employee training and research activities (Samila and Sorenson, 2011). With non-competes in place, employers can invest in their employees and provide confidential yet necessary information with less fear of information leakage or potential competition. Employees, likewise, can credibly pledge or commit that they will not use the training and information they receive from the current employer for the benefit

[17] Garmaise (2009) selected a random sample of 500 firms from the Execcomp database (1992-2004). This is only a lower bound because firms are not required to disclose this information.
[18] Amazon removed non-competes after intense media coverage and controversy in 2015 (Business Insider, 2015).

of its competitors. High technology firms often invest heavily in research and development, and their technical professionals learn a great deal in performing that work. If an employee moves to another organization, the intellectual assets that he or she developed may leak to competitors, posing a significant threat to the former employer (Conti, 2014; Ganco, Ziedonis, and Agarwal, 2015).

Addressing the under-investment in training and research problem may have other effects, however, on firms, industries, and the regions they both operate in (Gilson, 1999). The enforcement of non-competes creates complications and, in practice, the optimal degree and nuance of their application remains unclear. It is difficult to monitor observance of the agreement and contract on every possible contingency. Non-competes can distort the labor market and create inefficiency, as prior employees cannot utilize their expertise and experience in the same field for a certain amount of time. Employers can potentially increase their leverage over employees because employees have fewer outside options and less bargaining power under a non-compete. Employees often do not understand the legal nuances of labor law and their chances of prevailing, should they face prosecution by their former employer. This confusion can create a chilling effect on worker mobility, as employees are reluctant to incur potentially debilitating personal expenses for an uncertain legal outcome (Marx, 2011). By restricting mobility, non-competes can make it more difficult for firms to hire the talent they need, slow the optimal matching of human capital and opportunities (Jackson, 2013), and potentially retard the diffusion of knowledge and expertise (Fallick, Fleischman, and Rebitzer, 2006; Belenzon and Schankerman, 2013).

Empirical work has established a variety of relationships with non-compete enforcement. Stuart and Sorenson (2003) established that greater entrepreneurship followed IPOs in regions that lacked enforcement. Using multiple times-series and cross-sectional variations of enforceability across U.S. states, Garmaise (2009) found that stronger enforcement promoted executive stability and reduced executive compensation. The Michigan Antitrust Reform Act (MARA) in 1985 has been used with difference-in-differences models to demonstrate decreased intra-state mobility of inventors (Marx, Strumsky, and Fleming, 2009), career detours (Marx, 2011), and inter-state brain drain of inventors (Marx, Singh, and Fleming, 2015). Using panel regressions and an instrument based on university endowment returns, Samila and Sorenson (2011) found that the number of patents, number of start-ups, and rate of employment are more responsive to the supply of venture capital in states that restrict the enforceability of non-competes. Conti (2014) illustrated an increase in breakthrough and failed innovations in states that enforced non-competes, arguably due to greater risk-taking by firms that were less afraid of losing their technical personnel. Starr, Balasubramanian, and Sakakibara (2017) used matched employer-employee data and found that non-compete enforceability is negatively correlated with formation of small (0-19 employees) within-industry spinouts, but positively correlated with the survival of such new spinouts. Balasubramanian, *et al.* (2017) found that non-compete enforceability correlates with longer job spells in technology industries, without an increase in wages.

None of the work to date has considered how non-competes might have different impacts on firms of different sizes and in particular, their location decisions and rates of job creation, and ultimately, on their concentration. Figure 2.1 introduced above suggests that stronger enforcement might lead to larger firms, greater employment by larger firms, and higher business concentration. Before discussing potential mechanisms, we will first establish why Florida's 1996 law change best enables one investigation of the dynamics that might underlie these relationships.

2.2.1 Strengthened Enforcement of Non-competes in 1996 Florida

An examination of the 1996 amendment to the statutes, along with legal professionals' accounts, illustrates how the amendment strengthened the enforceability of non-competes in Florida. As this is the first study exploiting Florida's legislative change in studying non-competes and its downstream effects, we discuss changes in Florida's non-competes enforcement in detail. In what follows, we highlight the most important changes that made the post-1996 legal regime (§542.335) more lenient to employers seeking non-compete enforcement than the 1990 to 1996 legal regime (§542.33B).

Protection of Business Interests

The 1996 change, §542.335(1)(b), lists five legitimate business interests that can be protected: (1) trade secrets; (2) confidential business or professional information (not otherwise a trade secret); (3) substantial relationships with prospective or existing customers or clients; (4) customer goodwill associated with a certain practice, geographic location or marketing area; and (5) specialized training. This provision provides employers with a broad range of protections for legitimate business interests (Cornell, 2013). For example, a legitimate business interest exists when the employee has access to confidential and proprietary business information. It does not have to be a trade secret; it is sufficient that the information is confidential (Adler, undated). The statute also provides that relationships with specific prospective or existing customers comprise legitimate business interests. More importantly, the statutes explicitly clarify that the list is nonexclusive ("includes, but not limited to"); other *unspecified* interests may also merit protection. In this sense, this list provides an open-ended enumeration of what the employers can do (but not what they cannot do) regarding the enforcement of non-competes.

Blue Pencil: The Modification of Over-broad Covenants

The former rule (§542.33B) allowed courts the flexibility to either modify the restrictions or to choose not to enforce the covenants at all ("blue pencil" refers to the court's ability to essentially rewrite or nullify the contract). In contrast, the 1996 amendment (§542.335) only allows courts to modify overly broad (geographic or time) restrictions. After the amendment, a court could only modify the excessive restraints rather than declaring the non-compete non-enforceable.[19] "If a contractually specified restraint is over-broad, overlong, or otherwise not reasonably necessary to protect the legitimate business interest or interests, a court shall modify the restraint and grant only the relief reasonably necessary to protect such interest or interests" (§542.335(1)(c)). This change made it much easier for

[19] Prior to 1990, under §542.33A, Florida courts were required to modify over-broad covenants. The 1990 amendment removed this requirement, but did not prohibit modification.

employers to write highly restrictive covenants without fearing that they would be overturned (Garmaise, 2009).

Burden of Proof

Unlike the earlier §542.33, the amended statutes specify a burden of proof. An employer initially bears the burden of proof that the non-compete meets the "legitimate business interests" restriction. Once this burden is met, however, the burden of proof shifts to a former (separated) employee ("the person opposing enforcement"). This significantly enhances employers' power to enforce non-competes. "If a person seeking enforcement of the restrictive covenant establishes prima facie that the restraint is reasonably necessary, the person opposing enforcement has the burden of establishing that the contractually specified restraint is over-broad, overlong, or otherwise not reasonably necessary to protect the established legitimate business interest or interests" (Fla. Stat. §542.335(1)(c)). The employer advantage is even greater if we consider injunctions and presumption of irreparable injury detailed in the following paragraph.

Injunctions and the Presumption of Irreparable Injury

There was no judicial presumption of irreparable injury in the pre-amendment statute, §542.33B. Under the new statute §542.335, by contrast, once a former employer shows the intentional breach of a non-compete, irreparable harm is presumed:

> *A court shall enforce a restrictive covenant by any appropriate and elective remedy, including, but not limited to, temporary and permanent injunctions. The violation of an enforceable restrictive covenant creates a presumption of irreparable injury to the person seeking enforcement of a restrictive covenant (Fla. Stat. §542.335(1)(j)).*

This provision significantly reduces the burden placed on an employer to show it suffered an irreparable injury, making it easier for employers to receive injunctions (Cornell, 2013; p. 28). Considering the burden of proof, Grant and Steele (1996) concluded that: "once the proponent of the restriction establishes one or more legitimate business interests justifying the restriction, irreparable injury must be presumed and the burden shifts to the defendant to establish the absence of such injury."

Furthermore, a Florida court may issue an injunction that prohibits competition not only by the former employee, but also by his/her new employer. A court may also award damages for a violation of a covenant, including lost profits and damages derived from unfair employee competition (Adler, undated; p. 23). Given this language and the changes it described, employees probably perceived the new statute in 1996 as more intimidating.

Limitations on Public Policy Defense

The older statute (§542.33B) allowed the courts to consider public policy and welfare in their rulings:

> *However, the court shall not enter an injunction contrary to public health, safety, or welfare or in any case where the injunction enforces an unreasonable covenant*

not to compete or where there is no showing of irreparable injury. (§542.33B).

The amended statute §542.335 reflected a shift toward employers' stance, stipulating that a court could not refuse enforcement of an otherwise enforceable restrictive covenant on the grounds that it violated public policy, with few exceptions. §542.335(1)(i) sharply limited the use of the "contrary to public policy" defense against the enforcement of a restrictive covenant:

> *No court may refuse enforcement of an otherwise enforceable restrictive covenant on the ground that the contract violates public policy unless such public policy is articulated specifically by the court and the court finds that the specified public policy requirements substantially outweigh the need to protect the legitimate business interest or interests established by the person seeking enforcement of the restraint (§542.335(1)(i)).*

No Consideration of Individual Economic Hardship

The 1996 statue did not allow the court to consider an employee's individual hardship in determining the enforceability of non-competes. This represents a dramatic change in favor of employers from §542.33B, which had attempted to balance the interests of the employer and former employee (Malsberger, 2004; Garmaise, 2009).

> *In determining the enforceability of a restrictive covenant, a court: 1. Shall not consider any individualized economic or other hardship that might be caused to the person against whom enforcement is sought (§542.335(1)(g(2))).*

An Interpretation Favoring Business Protection

Under the new law, courts were statutorily required to construe covenants "in favor of providing reasonable protection to all legitimate business interests established by the person seeking enforcement" (§542.335). The new law stipulated that a Florida court could not construe the covenant narrowly against the drafter or against enforcement:

> *A court shall construe a restrictive covenant in favor of providing reasonable protection to all legitimate business interests established by the person seeking enforcement. A court shall not employ any rule of contract construction that requires the court to construe a restrictive covenant narrowly, against the restraint, or against the drafter of the contract (§542.335(1)(h))*

Enforcement Despite the Discontinuation of Business

The fact that the employer no longer ran a business did not void the non-compete; rather, the employee had to prove that the discontinuation of the former employer's business had nothing to do with his or her work for the competitor. The burden of proof remained difficult and with the employee.

> *May consider as a defense the fact that the person seeking enforcement no longer continues in business in the area or line of business that is the subject of the action to enforce the restrictive covenant only if such discontinuance of business is not the result of a violation of the restriction (§542.335(1)(g)*

Award of Attorney's Fees

The 1996 statute also allowed for the awarding of attorney's fees and costs to the prevailing party. This is a strong provision; as shown in the statute, this rule applied even in the absence of a contractual provision. Contractual provisions waiving or limiting such attorneys' fees were also unenforceable. The awarding of attorney fees placed an asymmetric burden on employers and employees. Employees were at risk of paying more than their annual salaries, whereas for employers, the cost represented only a marginal portion of their budget or business profits.

> *In the absence of a contractual provision authorizing an award of attorney's fees and costs to the prevailing party, a court may award attorney's fees and costs to the prevailing party in any action seeking enforcement of, or challenging the enforceability of, a restrictive covenant. A court shall not enforce any contractual provision limiting the court's authority under this section (§542.335(1)(k)).*

2.2.2 Use of the 1996 Florida Change in Non-competes as an Instrument

In order to investigate the impact of non-competes, we consider a 1996 change in non-compete law in Florida. This change offers a close to ideal research site, in contrast to law changes in other states. Florida provides an ideal site because (1) the legislation was focused purely on restrictive covenants, notably non-competes and (2) it was clearly intended to strengthen non-compete enforcement in the state. Furthermore, it does not appear that wage trends changed in Florida around 1996 (please see robustness check in Section 2.7.3). Considering the presence of non-compete law in Florida for the preceding four decades, employers and employees were probably familiar with and accustomed to non-competes.

At least three important features of the 1996 amendment support its use as a quasi-natural experiment. First, the amendment explicitly stated and thereby clarified which rule governed a contract and stipulated a clear break on July 1, 1996. Second, an examination of the 1996 amendment to the statutes, along with legal professionals' accounts, illustrates how the amendment significantly strengthened the employer's position in terms of the enforceability of non-compete covenant. The number of words almost tripled, from 455 words in §542.33A to 1,211 words in §542.33B, in the direction of strengthening employers' enforcement of non-competes. The new law was construed in favor of business protection, and courts could no longer refuse non-compete enforcement on the grounds of employee economic hardship or public policy concerns. Third, the 1996 amendment marked a sharp contrast to the preceding 1990 amendment. The post 1990-amendment statute made it more difficult to enforce non-compete covenants; in contrast, the post-1996-amendment statute, §542.335, made it easier to enforce non-compete covenants for employers. A legal professional commented that the 1996 amendment "has once again swung the pendulum representing the enforceability of non-competition agreements more in favor of employers (Findlaw, 2008)." Table 2.1 highlights and summarizes the most important changes that made the post-1996 legal regime (§542.335) more lenient to employers seeking non-compete enforcement than the previous legal regime (§542.33B).

2.3 The Differential Effects of Non-Competes by Firm Size

Despite a growing literature on non-competes, little work to date has investigated how non-competes might impact firm location and employment, which might in turn influence business concentration, if there were different effects on small vs. large firms. We consider the differential effects of non-competes by firm size on regional location choice (at birth or in movement of extant establishments), job creation, and business concentration. We discuss how the law change in Florida might cause a 1) shift in the distribution of firm sizes, 2) shift in the sources of new job creation, and 3) change in regional business concentration. We discuss mechanisms, but present no formal theory, and explore the answer empirically.

2.3.1 Non-competes and Location Choice, for Startups

The recruitment of high quality and experienced employees constitutes one of the greatest challenges in the founding and scaling of a new business (Stuart and Sorenson, 2003). Entrepreneurial companies in particular need to hire already capable and experienced workers because 1) they do not have the resources or time to invest in employee training, and 2) compared to large incumbents, they are less likely to have a systematic training process for novice workers. Startups therefore might prefer locations with weak non-compete laws, as they would ideally like to hire experienced employees (who will be more experienced if they were recently working in a similar job or for a competitor). Hiring unemployed workers remains unattractive because they are generally less experienced than active employees; furthermore, an unemployed yet experienced worker might still be bound by a non-compete and therefore off limits to competitors because non-competes typically hold even when an employee is laid off or fired. Since startups by construction cover narrower businesses and geographic boundaries, a departing employee will have a wide range of employment opportunities that do not include competitors. This wide range will make it less likely that an employee is leaving for an obvious competitor, because the non-compete will not cover this situation. Add to this the greater likelihood that a startup will lack the resources or strategic motivation to pursue legal action against former employees, and a startup might place lower value on location in a region with strong non-compete enforcement.

Startups may also have reasons to prefer locations with strong non-compete laws. Founders and their immediate teams probably share more complete access to all information within the organization, due to the small size of the firm, shared responsibilities, and weak and yet to be formalized information-sharing protocols. Given that startups often have no reputation and few complementary assets, their ideas and intellectual property are often their only advantages, and they may be attracted to legal regimes where they can more easily keep an employee from departing, particularly to a better-resourced competitor. Foreseeing growth, startups might also prefer locations with strong non-compete laws, as such laws would help keep their current employees as they seek new employees. Empirically, if startups find strong non-competes attractive, we would expect to find an increase in the number of small firms, following a shift to the stronger non-compete enforcement (and the opposite if startups find non-competes unattractive).

2.3.2 Non-competes and Location Choice, for Existing Firms

Existing firms, especially if they are not attempting to hire more than a small proportion of their extant workforce, are more likely to prefer regions with stronger non-compete enforcement, and hence more likely to move there or establish additional franchises. When large firms do need to hire, and in contrast to the challenges faced by smaller firms, non-competes might also magnify the typically superior financial and legal resources of large firms. Such firms are more able to buy out non-compete provisions from new employees' former employers. Potential legal costs also favor large firms, which generally have more experience, financial resources, and economies of scale when utilizing legal services, such as contracting advisory or litigation.

Similar to startups and small firms, the strategic importance of retaining existing employees is also likely to be very important for larger firms. Large firms typically have systematic processes in place to train their workers (which is costly) and have granted them access to strategic assets and information. If these workers move to (emerging) competitors, large incumbents could lose their investment in their trained workforce; furthermore, mobile employees might also unwillingly transfer important strategic assets of former employers, either implicitly or explicitly, to the competing firms. Therefore, firms that are not growing rapidly may feel that they gain more than they lose from immobilized employees and thus may place a higher value on location in a region with strong non-compete enforcement.

Regions with strong non-compete enforcement may also attract larger firms because they can temporarily allocate newly hired (or explicitly poached) employees to business units or subsidiaries that do not directly compete with their former employer. Such firms can then reallocate employees to the most relevant units after their non-compete term expires. In other words, large firms are more likely diversified and thus run businesses in multiple fields; these diversified business units serve as a "holding tank" (Marx and Fleming, 2012) for new employees who might be bound by non-competes. Small firms, in contrast, are more likely to focus on a specific area and lack diversified business units that could serve as legitimate holding tanks.

Analogous to "voting with feet (Tiebout, 1956)," firms (re-)locate in municipalities that offer their preferred business environment, essentially shopping for advantageous policies. For the reasons described above, large firms are more likely to be prefer strong non-compete regions and hence may open new establishments in Florida or move extant establishments to Florida, following the amendment. The advantages to entrepreneurial firms, on the other hand, are mixed (and it is very possible that there is no monotonic relationship between firm size and preference for non-compete regions – we leave it as an empirical question). Firms surely vary in their preference to contract on and enforce non-competes. Yet large [small] firms that were at the margin – i.e., that previously saw the benefit of non-compete enforceability and cost of (re)locating to Florida as a break-even opportunity – will prefer Florida more [less] after the 1996 law change, because it will have increased [reduced] the benefits and thus made it more lucrative [unprofitable] to [re]locate in Florida. Empirically, if existing (and typically larger) firms find strong non-competes relatively more attractive, we would expect to find an increase in the number of large firms, following a shift to the stronger non-compete enforcement.

2.3.3 Non-competes and Job Creation, for Small Firms

The enforceability of non-competes may also differentially affect the creation of new jobs and employment, depending on a company's size. All other things being equal (for example, assuming that all firms want to hire and grow), if it becomes harder [easier] for larger [smaller] firms to hire new workers, we would expect to observe a shift in the distribution of sources of new jobs, following the 1996 law change. We will focus on how non-competes could make it more or less attractive or difficult for different types of firms – small or large – to hire.

Regional mobility (of workers) decreases with stronger enforcement (Marx, Strumsky, and Fleming, 2009; Balasubramanian *et al.*, 2017) and this decrease may put startups and small firms at a greater disadvantage in hiring employees and creating new jobs. If workers expect to be bound by a non-compete, they may avoid opportunities at smaller and entrepreneurial firms. When workers are unable to hop between jobs and find a better match by trial and error, they are more likely to choose a large employer that typically offers better benefits packages, job stability, internal job hopping, and other non-pecuniary incentives. This is more so when non-competes remain in force after an employee is laid off; workers who sign non-competes bear additional risks should the business go awry, because they remain bound by commitment (and small businesses and particularly startups are more likely to go awry).

Further adding to small firms' challenge in creating jobs, they are typically less able to offer appealing and competitive incentives to prospective employees. Small firms are generally riskier, pay less, and are focused on less diverse businesses (thus affording fewer internal career transfers). Furthermore, they offer less protection from potential non-compete prosecution by larger firms with intimidating legal resources. This is in contrast to a location without non-competes, where (marginal) job seekers may be more likely to choose small firms that are riskier, because they can leave the small firm and get another job more easily.

This argument, however, can also be turned on its head. Under a strong non-compete enforceability, potential employees may prefer startups and small firms, if they anticipate that those firms will lack the resources or will to prosecute a non-compete. Furthermore, and consistent with the argument above, a narrow startup probably has fewer market and geographical competitors, thus making it less likely that a new employer would compete with the prior employer.

2.3.4 Non-competes and Job Creation, for Large Firms

Larger firms should be less challenged in hiring and creating jobs in strong non-compete locations, due in part to the opposite arguments just made for startups (difficulty in attracting risk-averse talent, inability to offer competitive compensation, weaker legal resources in non-compete litigation). Existing firms will find hiring (and training) new employees more attractive, because non-competes make it more likely they will retain their employee and recoup their investment.

Firms that benefit from non-competes will also accrue additional resources that in turn enable future growth in their work force. The greater enforceability of non-competes reduces an employee's outside alternatives, e.g., under standard non-competes, workers cannot be hired by a new employer that operates in the same field as their former employer. This significantly decreases the possibility that a worker is pursued by other employers and thus weakens the worker's negotiating power against his or her current employer. To the extent that the best alternative for an employee becomes unavailable due to non-competes, the current employer can appropriate this increased gap between the expected value of the current job versus alternatives (Garmaise, 2009). This mechanism provides additional advantage and resources to a *current* employer that can be invested in the expansion of the firm's work force; furthermore, firms with a larger stock of workers will benefit more from it.

2.3.5 Regional Business Concentration

A demographic shift towards small or large firms and a proportional change in job creation and employment by either group implies a restructuring of the local economy and change in business concentration, through entrepreneurship, firm relocation, and endogenous growth. We will not repeat the mechanisms detailed above, and instead focus on the impact of those mechanisms on the distribution of firm sizes and regional business concentration.[20]

With regards location of entrepreneurship, if startups are more attracted to a location due to a strengthening in non-compete enforcement, the density of small firms will increase (and decrease if they are not). With regards relocation decisions, if larger (and assumedly incumbent) firms are attracted, they will move into the region or open more establishments, and increase the density of large firms there (at least on the margin). With regards endogenous growth and observed job creation, any differential impact will be observable in the sources of job creation; if startups and small firms are advantaged, they will exhibit an increase in job creation following the law change, and if large firms are advantaged, they will exhibit an increase. The mechanisms need not be monotonic or

[20] The literature provides varying definitions of "market concentration" or "industry concentration". In some cases, researchers use market concentration to refer product sales concentration, and define industry concentration by firm within SIC or NAICS categories. To avoid confusion, we use the term "(regional) business concentration" that consists of the following three measures. We measure "establishment (or business-unit) concentration" when looking at the share of establishments by large firms, "employment concentration" when looking at the share of employment by large firms, and Pseudo HHI (as defined in Section 5.3.).

asymmetric; if the market is restructured in a way that attracts large firms and crowds out small firms, and large firm employment growth is favored over small firm growth, this should be observable as an overall increase in business concentration.

2.4 Empirical Design

2.4.1 Data and Sample

We use the Business Dynamic Statistics (BDS) provided by the U.S. Census Bureau for our main analysis. This data covers almost the universe of establishments and firms in the U.S. and their characteristics. It provides MSA-Firm Size-Year level data on establishment (including count, entry, and exit), job creation, and employment; for each MSA-year, variables on establishments and their employment are provided for twelve firm size categories.

One limitation is that the data are not available at the MSA-Industry-Firm Size-Year level; in other words, we are not able to run industry-specific analysis. To overcome this restriction, in Section 2.7.1, we use industry information from a separate data source, the Quarterly Census of Employment and Wages (QCEW). This data is constructed from the unemployment insurance (UI) accounting system for each state in the U.S. and provided by the Bureau of Labor Statistics (BLS). We match treated and controlled MSAs based on their industry composition. We calculated the distance in industry composition as the squared sum of differences in employment share by 5-digit NAICS industries. For each treated MSA, we selected and matched five control MSAs that have the most similar industry composition.

Table 2.2 provides descriptive statistics and a correlation table. There is little evidence of high correlations across variables in our models.

2.4.2 Difference-in-Differences Model

To empirically test our hypothesized relationships, we run a difference-in-differences (DiD) estimation. The basic idea is that, as we do not observe MSAs in Florida in the absence of the 1996 amendment, we use non-Florida MSAs (which did not undergo any changes in the rules governing non-competes) as counterfactuals. An important identifying assumption is:

$$\mathrm{E}\left[Y_{i,post}^{FL}(0) - Y_{i,pre}^{FL}(0)\right] \approx \mathrm{E}\left[Y_{i,post}^{NonFL}(0) - Y_{i,pre}^{NonFL}(0)\right]$$

where the 0 in parentheses indicates a lack of treatment (i.e., no amendment). While there is no data on the left-hand side, we can observe the right-hand side of the equation and use it as a counterfactual Florida. In other words, we assume that MSAs in our treatment state (Florida) and control states (non-Florida) exhibit the same trends in outcome variables, in the absence of treatment. To better facilitate this "parallel trend," we exclude MSAs in Alaska, California, Hawaii, Texas, and Puerto Rico from the control group. It is widely accepted that Alaska, California, Hawaii, and Puerto Rico are quite different from other states in terms of economic and geographic characteristics. California and Texas experienced changes in non-compete enforcement in 1998 and 1994, respectively (results remain robust with the inclusion of MSAs in these states). To further minimize the possibility of unobservable variables, Section 6 provides two robustness checks focusing exclusively on treated (Florida) and control (non-Florida) MSAs (1) that have the same industry composition and (2) that are located near the Florida borderline.

In our difference-in-differences regressions, we consider an indicator variable that adopts a value of unity for years following 1996 (*Post*). We interact this with an indicator variable that equals 1 for the MSAs in Florida (*FL*). To test the heterogeneous effects by firm size, we split the sample into two groups: one for firms with no more than 50 employees ("Small") and another for firms with more than 1,000 employees ("Large"). We then run separate log-linear regressions in Equation (2.1) for the split samples for 1993-1999 (± three years from the year of the amendment)[21]:

$$\log Y_{it} = \mu + \alpha_i + \delta_t + \tau \cdot Post_t \cdot FL_i + X_{it}' \cdot \beta + \epsilon_{it} \qquad (2.1)$$

where Y_{it} is an outcome of interest, μ constant, α_i MSA fixed effect, δ_t year fixed effect, and X_{it}' matrix of covariates. Note that FL_i and $Post_t$ variables are absorbed by the MSA and year fixed effects. The treatment is the 1996 amendment to the Florida statutes – i.e., stronger enforcement of non-competes –, and the parameter of interest is τ.

A difference-in-differences estimation in Equation (2.1) forces estimates to be the same within pre- or post-treatment years. We run a more flexible econometric model with distributed leads and lags ("event study regression techniques") as in Equation (2.2). We interact the treatment indicator (FL_i) with year indicators ($\mathbf{1}\{Year = t\}$), rather than uniformly assigning zero and unity for all pre- and post-treatment years. We leave the treatment year, 1996, as a baseline reference.

$$\log Y_{it} = \mu + \alpha_i + \delta_t + \textstyle\sum_{t \neq 1995} \tau \cdot FL_i \cdot 1\{Year = t\} + X_{it}' \cdot \beta + \epsilon_{it} \qquad (2.2)$$

An alternative, more comprehensive approach, is to compare the effects by firm size in the same model. The BDS data provides twelve firm size categories: 1 for 1-4 employees, 2 for 5-9 employees, 3 for 10-19 employees, 4 for 20-49 employees, 5 for 50-99 employees, 6 for 100-249 employees, 7 for 250-499 employees, 8 for 500-999 employees, 9 for 1,000-2,499 employees, 10 for 2,500-4,999 employees, 11 for 5,000-9,999 employees, and 12 for 10,000 or more employees. We created four dummy variables for firm size by collapsing three categories into one: *Size S* (1-19), *Size M* (20-249), *Size L* (250-2,500), and *Size XL* (2,500+). We then run the difference-in-differences estimation in Equation (2.3) for the period ranging from 1993 to 1999 with full sample. *SizeS* is used as a baseline:

$$\begin{aligned}\log Y_{ist} = {} & \pi^M \cdot Post_t \cdot FL_i \cdot SizeM_s + \pi^L \cdot Post_t \cdot FL_i \cdot SizeL_s + \\ & \pi^{XL} \cdot Post_t \cdot FL_i \cdot SizeXL_s + \mu + \alpha_i + \delta_t + X_{it}' \cdot \beta + \epsilon_{ist} \end{aligned} \qquad (2.3)$$

where X_{it}' includes all relevant two-way interactions ($FL_i \cdot Post_t$, $FL_i \cdot SizeM_s$, $FL_i \cdot SizeL_s$, $FL_i \cdot SizeXL_s$, $Post_t \cdot SizeM_s$, $Post_t \cdot SizeL_s$, and $Post_t \cdot SizeXL_s$) and firm size dummies

[21] A variation of the window i.e., ± two, three, or five years does not qualitatively change our result.

(*SizeM*, *SizeL*, and *SizeXL*). Note that FL_i and $Post_t$ variables are absorbed by the MSA and year fixed effects. The parameters of interest are π^M, π^L, and π^{XL} .

One concern is that our data are yearly. Since the new law applied to the contracts written on and after July 1, 1996, the inclusion of 1996 as a post-treatment year might bias the estimates. In addition, since the amendment was introduced by the Florida legislature, it is possible that employers and employees expected the change ex ante and adjusted their behavior before the effective date, July 1, 1996 (Barnett and Sichelman, 2016). We therefore exclude 6 months before and after the effective date, July 1, and run the regressions in Equations (2.1)-(2.3) for 1993-1999, leaving out the year of amendment, 1996.[22]

2.5 Results

2.5.1 Business Size and Location Preferences

Figure 2.2 illustrates the density change of firm size in Florida between 1995 and 1997. The solid line represents the density in 1995, while the dashed line represents the density in 1997 (left-hand side y-axis). Bars behind the density lines show changes in density between 1995 and 1997 (right-hand side y-axis). In Panel (a) of Figure 2.2, the entry of establishments (business units or branches) of small firms (including small single-unit firms) significantly decreased in 1997, whereas that of large firms shows an increase. As might be expected due to the large number of establishments that do not move, the density lines are less discernable for the total number of establishments in Panel (b). Changes in density shown in bars, however, are consistent with the entry comparison. The decrease in establishments comes from small firms, and large firms increase the number of establishments in Florida, after the amendment.

Figure 2.3 splits firms *within* Florida by their size. The solid line and left-hand side y-axis represent “Small” Florida firms that have less than 50 workers (first four categories of firm size in the BDS data). The dashed line and right-hand side y-axis represent “Large” Florida firms with more than 1,000 workers (last four categories of firm size in the BDS data). The idea of this approach is to find a divergent movement for Small vs. Large firms, after the 1996 amendment. The two subgroups may differ in several characteristics, and there could be an idiosyncratic factor that specifically affects small firms. To check this and facilitate the comparison, we adjusted and aligned pre-treatment years (1991-1995) by rescaling the y-axis ranges. We generally find a parallel trend between the Small and Large firms for pre-amendment years, 1991-1995. We show in Panel (a) that the number of establishments of large firms increased to a greater extent than that of small firms, following the amendment in 1996 in Florida.

Figure 2.4 turns to inter-state analysis, comparing Florida and a counterfactual *synthetic* Florida. We use the Synthetic Control Method to construct a control unit that approximates the characteristics of the treated unit, Florida. This procedure compares a

[22] The results are robust to the inclusion of 1996 as treatment year.

single treated unit to a weighted average of all the other control units (Abadie and Gardeazabal, 2003; Abadie, Diamond, and Hainmueller, 2010). For the synthetic Florida (control), the weight of each state is chosen based only on the *pre*-treatment period (1991-1995) trends for all the U.S. states except for Alaska, California, Florida, Texas, and Puerto Rico. More specifically, we calculated the weights based on our outcome of interest during 1991-1995 after normalizing values relative to the 1991 value. An important advantage of normalizing the values is that we account for the time-invariant difference between Florida and other states, as in the formal difference-in-differences model. In other words, we take it into account that MSAs have different (absolute) numbers of establishments and employment and rely on (relative) changes over time. In this way, we could construct a parallel trend for Florida and its synthetic control for pre-treatment periods in all four graphs in Figure 2.4.

Since we study differential effects by firm size, we split the sample and plot the result by Small (where there are less than 50 employees) vs. Large firms (where there are more than 1,000 employees). In Figure 2.4, the red solid line represents Florida, while the brown dashed line represents the counterfactual synthetic Florida. We find in Panel (a) that the number of establishments of Small firms in Florida becomes significantly lower than that in synthetic/counterfactual Florida, beginning from 1996. In contrast, the number of establishments of Large firms shows the opposite trend: it becomes higher than counterfactual Florida. It is reassuring that we find the opposite response by Small vs. Large firms.

To test if the Synthetic Control Method captures real and not spurious effects from our treatment, we perform a set of "placebo tests" to our control states. We perform the Synthetic Control analyses *as if* our control states had received the treatment (the 1996 law change), even if they were not. We then compare the distribution of the estimator for Florida and all the other control states, under the null hypothesis that the law change had no effect. If we observe similar trends for Florida and other control states that received placebo treatments, we cannot reject the null hypothesis of no effect. To reject the null hypothesis, we need that Florida exhibit a distinct change after 1996. The results are presented in Figure 2.5. This Figure illustrates that the trend of Florida (black bold line) is exceptional, compared to the distribution of other control states with false treatment assignments (grey lines).

Table 2.3 provides the result from formal difference-in-differences models. Equation (2.1) estimates a split sample model. As hypothesized, for the establishment entry in Column (1), we consistently find opposite signs for $FL \times Post$ between the Small (<50 employees) and Large (>1,000 employees) split-samples. Entry by Small firms decreased by 5.6 percent, whereas large firm entry increased by 8.5 percent. The number of establishments in Column (2) shows a similar pattern though the estimate from Small sample is not precisely estimated.

Table 2.4 shows the results from alternative models (with full sample) where we interact indicators for four firm size categories with $FL \times Post$. For the establishment entry in Column (1), we consistently find that the estimates are positive and large for large firms. Entry of establishments of firms with 20-249 workers is 3.7% larger than that of firms with 1-19 workers. Entry of establishments of firms with 250-2,500 and more than 2,500 workers is 15.3% and 12.4% larger than that of firms with 1-19 workers, respectively. Column (2) illustrates consistent results for the total number of establishments. The number of establishments of firms with 250-2,500 and more than 2,500 workers increased by 4% and 11% compared to that of firms with 1-19 workers.

This approach estimates the effects for larger firms *relative* to the smallest firm size category, $Size\ (1-19)$. To estimate the effects more generally, we estimate separately for each firm size category in Equation (2.1). The results for the number of establishments by firm size are summarized in Panel (a) of Figure 2.6, where each dot represents an estimate for $FL \times Post$ from four separate regressions for each firm size category: $Size\ S\ (1-19)$, $Size\ M(20-249)$, $Size\ L\ (250-2{,}500)$, and $Size\ XL\ (2{,}500+)$. We find that the effects primarily come from responses by large firms, as their results are larger and more precisely estimated. Large firms prefer to locate in regions that enforce non-competes.

Entrepreneurship may also weaken with stronger enforcement of non-competes. Existing studies generally view firms with less than 19 employees as more likely to be entrepreneurial (e.g., Starr, 2015). In the results not presented in this paper, a 5.6% decrease in the entry of establishments by Small firms (with 1-50 workers) suggests such a chilling effect on entrepreneurship. The entry of firms with 1 to 4 employees decreased by 7.5 percent ($-0.0913 + 0.0167 \times 1$) compared to MSAs in control states, while that of firms with 5 to 9 employees decreased by 5.8%, and that of firms with 10 to 19 employees decreased by 4.1%, relative to control state MSAs. Overall, the change in non-compete law appears to have made Florida a more attractive location for large firms and a less attractive location for small firms.

2.5.2 Business Size, Job Creation, and Employment

Panel (c) of Figure 2.2 illustrates job creation by size of firm in Florida between 1995 and 1997. While job creation by the smallest (<50 employees) and largest (> 5,000) categories clearly decreased and increased, respectively, the results in the middle of the distribution are mixed. Employment in Panel (d) of Figure 2.2 shows a similar pattern. Figure 2.3 splits the data between Small and Large firms *within* Florida. In Panel (b), employment in Small firms (dashed line) decreased, as opposed to that in Large firms (real line), following the 1996 amendment. Finally, an inter-state comparison with the Synthetic Control in Panel (b) of Figure 2.4 shows consistent results. We find decreased employment by Small firms in the left-hand side, relative to a weighted average of other control states, beginning from the amendment year, 1996. In contrast, increased employment by large firms is found in the right-hand side figure. Note that both figures in Panel (b) show a fairly good parallel trend for pre-amendment years, 1991-1995.

Table 2.3 estimates split sample models and illustrates that small firms decreased their job creation by 1.8%, whereas large firms did the opposite (increased by 7.6%), though the estimate for the small firm sample is imprecisely estimated. The alternative specification with four categories for firm size in Table 2.4 finds similar and consistent results. Firms that have more than 250 workers increased their job creation and employment by 8-24% and 13-16%, respectively, compared to firms that have 1-19 workers. With regards to job creation and employment by entrepreneurial firms, job creation and employment by entrepreneurs with 1 to 4 employees decreased by 1.25 percent and 3.7%, respectively, in the specification with linear *Size* variable interaction (from the results not presented in this paper).

We then estimate the effects separately for the four firm size categories. The results are summarized in Panel (b) of Figure 2.6. Each dot represents an estimate for $Post \times FL$, and we again find that the effects primarily come from hiring expansions by large firms (rather than shrinking employments by small firms).

The change in non-compete law appears to have altered job creation and employment by small and large firms. Even though the total number of jobs in Florida increased after the amendment was instituted, these jobs predominantly came from large firms; small firms created relatively fewer jobs.

2.5.3 Regional Business Concentration

The first two results imply an increase in business concentration for two reasons. First, large firms appear to prefer a region that enforces non-competes when they launch or relocate establishments; small firms appear to be crowded out. Second, large firms appear to be adding jobs and growing at a faster rate than small firms.

Although we do not have firm-level data that covers both small and large firms (note that Compustat only includes large, publicly traded firms), we can estimate changes in business concentration using the following three measures: 1) share of establishments that belong to large firms ("establishment concentration"), 2) share of workers that belong to large firms ("employment concentration"), and 3) Pseudo Herfindahl-Herschman Index (HHI). Note that this Pseudo-HHI measure also uses the share of employees. It is calculated based on the weighted average of the share of employees in each firm size category in each MSA:

$$PseudoHHI_{it} = \sum_{s=1}^{12} \left[\frac{Min_s + Max_s}{2} \times \left(\frac{Number\ of\ Employees_{ist}}{\sum_j Number\ of\ Employees_{ist}} \right)^2 \right]$$

where $\frac{Min_s+Max_s}{2}$ is the representative firm size in each firm size category s ("weight"), and $\frac{Number\ of\ Employees_{ijt}}{\sum_j Number\ of\ Employees_{ijt}}$ is the share of employees in size category s in MSA i in year t ("share"). We then calculate a sum over all twelve categories. This measure mimics the way we calculate the original firm-share based HHI and captures the degree of business concentration at the MSA-year level.

Figure 2.7 shows the results from the Synthetic Control Method. In both Panel (a) and Panel (b), we consistently find that business concentration increases after the year of law change, 1996. We then run the differences-in-difference regression in Equation (2.4) with the three different measures of business concentration:

$$\log y_{it} = \mu + \alpha_i + \delta_t + \tau \cdot Post_t \cdot FL_i + \epsilon_{it} \tag{2.4}$$

In our result in Column (1) in Table 2.5, we find that the establishment-based share of Large firms that have more than 1,000 employees increased by about 2.4%. Column (2) shows the employment-based share of large firms that have more than 1,000 employees.

Consistent with our prediction, the results show an increase by 4.7%. Column (3), Table 2.5, again illustrates that business concentration measured by the Pseudo-HHI increases after stronger non-compete enforcement, by 17.4%.

Since a difference-in-differences model imposes a uniform effect for pre- and post-treatment years, we run a more flexible model with event study techniques. We interact the treatment indicator with year indicators (instead of the $Post_t$ indicator). The results are illustrated in Figure 2.8, where the solid line shows the estimates by year and vertical lines represent a 95% confidence interval. In Panel (a), the establishment-based share of Large firms increased after the amendment in 1996. In Panel (c), the employment-based share of Large firms increased after the amendment in 1996.

It is worrisome that there appears to be a pre-trend in Figure 2.8, especially an increase from 1993 to 1994. To further check if our findings result from pre-existing trends, we interact yearly outcomes for pre-amendment years with a full set of year dummies. This absorbs all the pre-1996 differences in employment share of large firms in our analyses, and some of the post-1996 variation, but makes our post-1996 comparisons close to *ceteris paribus* (Cantoni, Dittmar, and Yuchtman, 2018). The results are shown graphically in Panel (d). By design, there are no pre-1996 differences in trends between treatment and control groups. We again confirm from this very stringent specification that following the 1996 amendment that large firms expanded their employment and increased their share of employment in Florida. The same technique is applied to the establishment-based share of large firms, presented in Panel (b). In summary, the change in non-compete law appears to have preceded increased business concentration, through different firm (re)location choices by size of firm and relatively faster employment growth by larger firms.

2.6 Potential Threats to Identification

Since we investigate a single event that happened at the state-level to identify the effects, the results are vulnerable to other simultaneous and confounding events, particularly if there was a change that operated in the same direction as the non-compete amendment (i.e., benefitting large firms and harming small firms or start-ups). While it is not possible to consider every event that happened in 1996, we discuss two specific threats to identification: Enterprise Florida, Inc. and electoral changes. Figure 2.11 illustrate how wage trends appeared unchanged before and after 1996, which eases concerns that the law change impacted the economy through wage changes.

2.6.1 Enterprise Florida, Inc.

Enterprise Florida, Inc. (EFI) is a "public-private partnership between Florida's business and government leaders," aiming to, "expand and diversify the state's economy through job creation". When describing their history, EFI states, "In 1996, under Governor Lawton Chiles, Florida became the first state in the country to place principal responsibility for economic development, international trade, research and business image marketing in the hands of a public-private partnership." If EFI began a program in 1996 that (1) could affect

Florida businesses and (2) disproportionately favored large established firms, there would be potential confounds. However, we do not find any evidence that EFI actively initiated any programs around 1996 or that its policies favored large firms, at the expense of small firms.

First, according to the EFI's history statement, it was not until 2011 that the EFI created a, "seamless economic development team," and began publishing annual reports and assessments. Archival research did not find any evidence of its activities in the 1990s. Furthermore, the EFI states that it focused on reforming the state's industry structure from tourism and agriculture to a more sophisticated mix. Figure 2.9 reveals, however, no noticeable change in Florida's industry composition for 1991-2001. Second, even if the EFI had actively operated beginning from 1996, its website stated that EFI "…supports small and minority businesses through its capital programs," and other entrepreneurial goals.

2.6.2 Electoral Changes

If electoral outcomes changed sharply around 1996 in preference to pro-big business candidates, the findings might result from policies that favored large firms. We do not, however, see a discontinuous change in Florida party politics at this time. First, incumbent Republican U.S. Senator Connie Mack III won re-election to a second term in 1994. Second, in 1992, President Bill Clinton (Democratic) won over Senator Bob Dole (Republican) by a margin of 5.7%. This represented an improvement over his narrow loss of the state in 1992.[23] Lastly, in 1996, in the 23 districts in Florida, 20 incumbents were re-elected. The remaining three incumbents retired, and candidates from the same party kept the districts. In summary, it does not appear that electoral outcomes would disproportionately favor large firms against small firms or start-ups in Florida around 1996.

2.7 Robustness Checks

2.7.1 Matching MSAs on Industry Composition

Although enforcement of non-competes typically applies equally to all industries, adoption and implementation (by employers and employees) could still differ. Starr, Prescott, and Bishara (2016) in fact find in their 2014 survey that the use of non-compete varies across states and industries, for example, they find few incidences of non-competes in agriculture and hunting (9%), compared to information (32%), mining and extraction (31%), and professional and scientific (31%) industries. Here we test if our results remain robust to industry heterogeneity across MSAs.

We are not able to control directly for industry composition because the BDS data lack information by industry. As an alternative, we look at the Quarterly Census of Employment and Wages (QCEW) data that provides information on county, MSA, and

[23] Note that it is generally believed that pro-big business policies are most likely to be adopted by Republicans.

state-level industry composition. Figure 2.9 shows Florida's industry composition. The idea is that, using industry information in the QCEW, we can control for conflating effects of industry composition by matching control MSAs that share the same industry composition as Florida MSAs. We then bring this treatment-control MSA pairs to the BDS data and re-run the regressions.

Five-digit North American Industry Classification System (NAICS) code (11111-99999) and its employment in each MSAs are used to calculate the Euclidean distance between industry compositions of any two MSAs:

$$Industry\ Distance_{A,B} = \sum_{NAICS\ (5-digit)} (Emp_{A,NAICS} - Emp_{B,NAICS})^2$$

where $Emp_{A,NAICS}$ and $Emp_{B,NAICS}$ are the number of employment in industry NAICS in MSA A and B, respectively. For each Florida MSA, we identify ten non-Florida MSAs that have the most similar industry structure as the focal Florida MSA. We then run the same difference-in-differences estimation using this paired MSA data. Results provided in Table 2.6 and Table 2.7 (odd-numbered columns) and Table 2.8 are not qualitatively different from our main findings, making it less likely that the results are driven by a discrepancy in industry composition between the treated and control MSAs.

2.7.2 State-Bordering MSAs

One concern is that the treatment group (MSAs in Florida) and control group (MSAs in states other than Florida) may differ in terms of unobservable characteristics. To mitigate this concern, we compare the MSAs near the Florida state border. In this case, the treatment group is the MSAs in Florida within *n* miles of the border, while the control group is the MSAs in Alabama and Georgia within *n* miles of the Florida border. It is expected that the MSAs near the Florida borderline would share many unobservable characteristics, strengthening the validity of the control group and the parallel trend assumption.

MSAs in Alabama, Florida, and Georgia near the border of Florida are identified in Figure 2.10. There are four MSAs in Florida, two in Alabama, and one in Georgia. Thanks to geographic proximity and an arbitrary straight border, these MSAs should share many unobservable or intangible characteristics such as commutable area, culture, weather, etc. The results of the formal regression, Equation (2.1) and Equation (2.2), are presented in Table 2.6 and Table 2.7 (even-numbered columns). The results are not qualitatively different from those in Table 2.3 and Table 2.4 (and matching results in odd-numbered columns in Table 2.6 and Table 2.7), though with a much smaller number of observations, the estimates become less precise.

The magnitudes are generally larger in the model only with border MSAs. One potential explanation for this result (which can only be tested with establishment-level panel data) is a substitution effect arising in the borderline sample. Given the geographic proximity and cultural similarity between the treated and the control in the borderline, the closer a firm is to Florida, the more likely that this firm moves to Florida, *in response to* the 1996 Florida amendment. For example, it is much more likely that potential new entrants choose between Tallahassee MSA (Florida) vs. Valdosta MSA (Georgia) than

Tallahassee MSA (Florida) vs. San Francisco MSA (California). The borderline sample captures this substitution effect to a greater extent than the full sample. A move between state-bordering MSAs will more likely to lead to a *double-counting* of the effect when a large firm moves into Florida and a small firm leaves, because a move of single firm from control MSA to treatment MSA is counted twice when we calculate the difference in the number of firms between the two groups.

This argument implies that our control MSAs in Alabama and Georgia borders are also affected by the 1996 Florida amendment. This magnified border effect provides additional evidence that the 1996 Florida amendment drives the observed changes. We find greater effects even if the two MSAs share most of business environments *other than* legal institutions that govern non-compete enforcement, strengthening the probability that the changes in the enforceability may be the only reason for increased relocation of businesses after the 1996 Florida amendment. This magnified result for state-bordering MSAs increases our confidence that firms move in response to changes in non-compete enforceability.

2.7.3 Potential Conflation with Changes in Wages

The models estimated above rest on the assumption that non-competes impact small and large firms differently and that this difference cannot be adjusted, most obviously, in wages. "Consideration" – i.e., a benefit an employee receives in response to non-competes – provides an obvious threat to this assumption. For example, if employees fully understand the consequences of non-compete enforcement *and* have strong bargaining power (e.g., they are irreplaceable) or an attractive alternative job option, they can negotiate a wage increase to compensate for their reduced mobility. In this situation, small and large firms may behave similarly, because any benefits and losses from non-compete enforceability would be efficiently reflected in wages (or other forms of employee benefits). In other words, firms would pay for the reduced mobility of their workers, and therefore the benefits and costs that arise from non-compete enforcement would offset each other. For this situation to hold, the employee needs to 1) be fully aware of the consequences of non-competes and 2) have the bargaining power to receive a higher wage. This may be rare; a survey by Starr, Bishara, and Prescott (2016) reports that only 10 percent of workers subject to non-competes try to bargain over their non-compete.

Recent empirical investigation of wage consideration by Balasubramanian *et al.* (2017) finds that non-compete enforceability is *not* positively associated with wage levels for technology workers; the relationship in fact was found to be negative. The U.S. Department of the Treasury (2016; p. 19) also suggests that "a standard deviation in non-compete enforcement reduces wages by about 1.4%". Balasubramanian *et al.* (2017) interpret their results as a wage suppressing effect due to a reduction in bargaining power. Even though a more careful study is required to tease out the exact mechanisms for the lack of increased or reduced wages, their findings demonstrate potential frictions in the labor market (i.e., at least one of the conditions of awareness and bargaining power is not met),

such that the employees' reduced mobility is not offset by wages.[24]
To address the concern that changes in wages might conflate the impact of non-competes upon firm sizes, we investigated wage trends in Florida and real and synthetic control states, using data from the Quarterly Census of Employment and Wages (QCEW). Figure 2.11(a) compares wage trends in Florida and other comparison states for 1991-2001 and Figure 2.11(b) shows wage trends for Florida and its synthetic control. Both graphs indicate very similar wage trends between Florida and control states around 1996. Differences-in-differences estimations also show that we cannot reject the null hypothesis that Florida's wage change is not different from the control states (estimate: 0.0051, p-value: 0.195). It appears that wage levels in Florida remained relatively unaffected by the 1996 amendment.

Furthermore, we examine the possibility that different sized firms adjust their wages differently when they ask their employees to sign a non-compete. On the one hand, it could be the case that employees have more bargaining power against small firms than against large firms, and therefore the wage impact of the law change acts primarily through small firms. In this case, we would expect that the wage paid by small firms would increase disproportionately relative to that of large firms. On the other hand, if large firms extensively use the covenants, the wages of workers in large firms would increase disproportionately as their workers gained compensation. We would then expect to see a larger wage increase for large firms.

We examine wage changes by establishment size over time in Figure 2.12.[25] Panels (a) and (b) in Figure 2.12 show that the share of wage by establishment size does not change meaningfully around 1996 and that wage growth rates are not systematically different for establishments of different size. These analyses provide evidence that the allowed imposition of non-competes is not fully reflected in worker wages.

2.8 Discussion

This study shares limitations with existing studies on non-competes in that the variation in the legal regime we exploit occurs at the state level (unfortunately, most policy or legislative changes on non-competes occur, at a minimum, at the state level), and researchers do not observe individual labor contracts (i.e., whether each employee signed non-competes or not). The stark change in non-compete enforcement makes Florida a good research site, however, and our additional analyses on the industry-matched MSAs and Florida borderline should lessen these concerns. While we investigated other states' changes in non-compete laws, none offered the sharp and focused change of Florida's 1996 statute, and most experienced only a weak change in enforcement or were vulnerable to

[24] One caveat for generalizing their finding is their use of cross-sectional variations in the non-compete enforceability to compare high vs. low tech and high vs. low wage workers. In other words, their results are correlations between non-compete enforceability and wages of high-tech [high-wage] workers, relative to non-high-tech [non-high-wage] workers. The current study does not exclusively focus on high-tech or high-wage workers.

[25] Unfortunately, the BDS data does not provide wage information. We get wage data from the Quarterly Census of Employment and Wages where total quarterly wages and the number of establishments are provided by nine "establishment" size categories.

other confounding factors.

While the results presented here focus only on Florida following a strengthening of non-compete enforcement, they imply that states that enforce non-competes could experience a decrease in small firm entry and employment, an increase in large firm entry and growth, and eventually an increase in their business concentration. As illustrated before in Figure 2.1, we found consistent results from state-level correlations between non-compete enforceability and the Florida outcomes. The left panels in Figure 2.1 show that states which strongly enforce non-competes tend to have a smaller proportion of small firm establishments and employment. The right panels in Figure 2.1 reflect this result for larger firms; stronger non-compete enforceability and the proportion of large firm establishments and employment are positively correlated. The sharp contrast between small vs. large firms' cross-sectional correlations are consistent with the illustrated mechanisms in Florida. Panel (c) in Figure 2.1 then illustrates a positive relationship between a state's strength of enforcement and its business concentration as measured by pseudo HHI. These relationships hold consistently for two indices of enforceability (Garmaise, 2009 and Starr, 2016) and without the outliers of California and North Dakota.

Analogous to the brain drain of talented individuals by non-competes (Marx, Singh, and Fleming, 2015), these results could be labeled as a small – and probably entrepreneurial – firm drain (though Florida obviously benefited from the location choices and increased employment of large firms). If both human and organizational capital leaves states that enforce non-competes for states that do not, it is not surprising that California and other non-enforcing states have become hotbeds of entrepreneurship (Guzman and Stern, 2015). For example, Facebook moved when still small from an enforcing state (Massachusetts) to a non-enforcing state (California). Is such movement an anomaly or characteristic of more promising small firms? Compounding this effect, Marx and Fleming (2012) illustrated that the proportion of elite inventors – as measured by career prior art citations and number of co-authors – have become increasingly likely to emigrate to states that do not enforce non-competes. Fallick, Fleischman, and Rebitzer (2006) also suggest that weaker enforcement of non-competes is positively correlated with "the reallocation of talent and resources towards firms with superior innovations." Weighed against this concern is that large firms tend to do better than smaller ones (Hathaway and Litan, 2014) and our finding demonstrated here that more jobs were created in Florida immediately following the strengthening of non-compete enforcement.

Also beyond the scope of this paper, another empirical question would be whether jobs at start-ups and large conglomerates play different roles in firms and the economy, and how. Asymmetries in firm positioning and employment growth (i.e., the dominance of large firms and the jobs they offer) could have important implications for welfare for consumers and producers. For instance, if new jobs at start-ups create unique value for firms and the economy that cannot be provided by already mature firms (for example, if startups are more likely to incorporate productivity enhancing innovations), state governments may want to attract entrepreneurs and the jobs they create.

2.9 Concluding Remarks

Non-compete covenants provide useful and important tools with which both employers and employees can commit themselves and prevent potential market failures. Non-competes decrease the unfair competition caused by separating employees. However, they may also hamper employers' competition for employees and employees' freedom to choose their jobs. It is thus important to understand these trade-offs and their consequences.

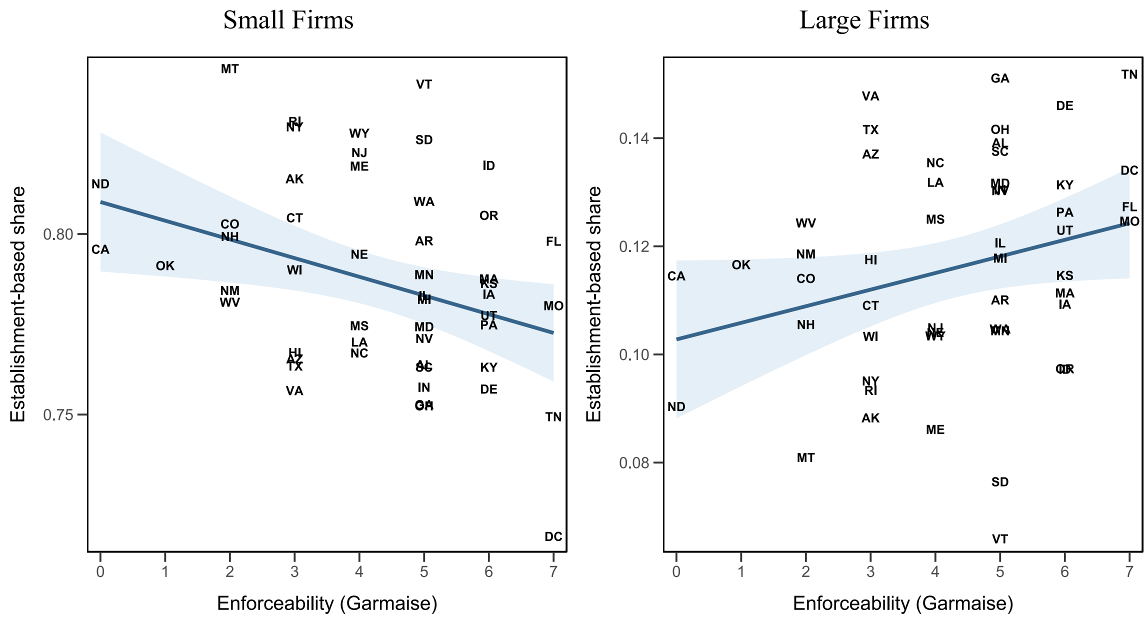
We examined how the stronger enforcement of non-competes influenced business dynamism in one local economy, exploiting the 1996 amendment to Florida statutes on non-competes. The results contribute to the literature by exploring the heterogeneous effects of non-competes by firm size on firm location choice and employment growth, and business concentration. The enforcement of non-competes not only affects inter-state competition for attracting businesses, but also the in-state distribution of businesses and jobs. Small and large firms responded to non-compete enforceability in opposing ways: large firms appeared more likely to locate (either launch or move) their establishments in Florida and small firms appeared less likely. Regardless of whether they were new or existing firms, small firms appeared reluctant or less able to create new jobs. In contrast, large firms boosted their rate of new job creation following the law change. Likewise, the level of employment decreased for small firms and increases for large firms. Consistent with these results, we observed an increase in the business concentration in Florida, following strengthened non-compete enforcement. It does not appear that non-competes influenced wages in Florida, or that business friendly policies or legislators caused the effect. Furthermore, across all U.S. states, we observe a negative cross-sectional correlation between non-compete enforcement and small firms' establishment and employment. In consistent contrast, a positive relationship exists between non-compete enforcement and large firms' establishment and employment. Business concentrations also exhibit positive relationships with non-compete enforcement across all U.S. states.

These differential effects on firm (re-)location and employment by firm size have important yet overlooked managerial and policy implications. Firm strategies on R&D and innovation differ by their size (e.g., Cohen and Klepper, 1996a,b), and thus it is important for managers to understand how small and large firms (re)locate and grow differently in response to non-compete enforcement. Managers need to be aware that non-compete enforcement not only affects the mobility of its own workers but also changes competition and the broader market environment through the redistribution of firm size and increased concentration. Stronger enforcement may have lowered the "birth" rate and/or move-in of establishments of small firms and simultaneously attracted large firms. This, for instance, could affect a firm's search for alliance partners or acquisition targets, competitive strategy, and ultimately performance.

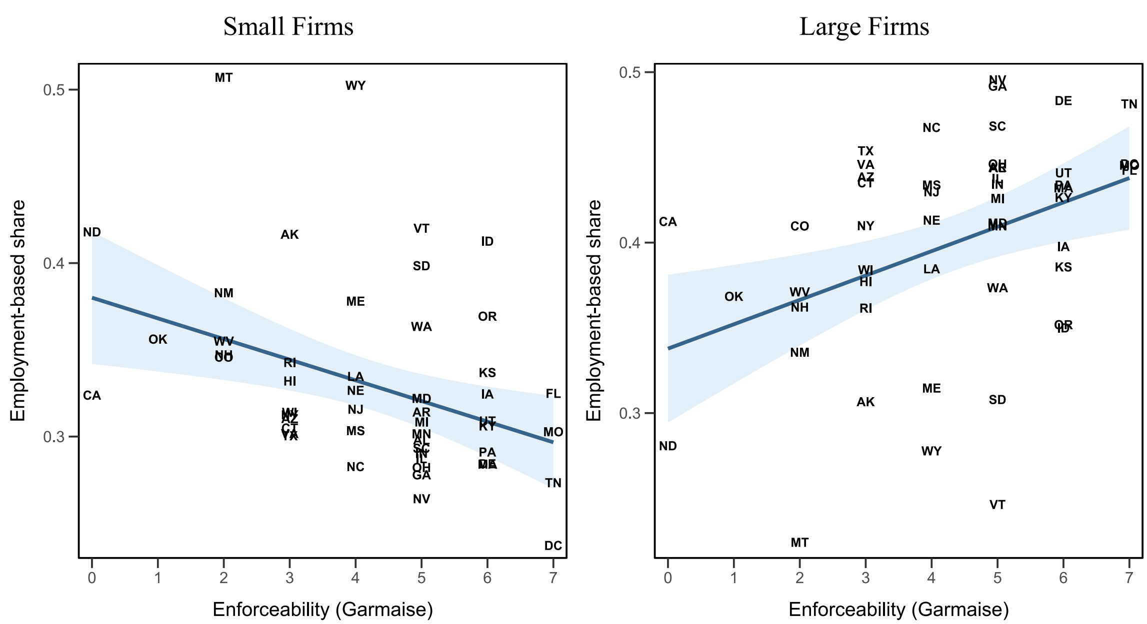
Furthermore, to the extent that small and large firms provide different values and jobs to local economies (e.g., incremental vs. break-through innovations, the quantity and quality and types of jobs, application of productivity enhancing innovations), the effects of non-competes on a local economy could be varied and large. Geographic agglomeration and clustering of different sizes of firms also have important implications for entrepreneurship, innovation, intellectual property protection, and regional economic growth (The White House, 2016). In this sense, policies and legal constraints on non-competes should not be considered in isolation. Non-competes are not mere contractual provisions agreed upon by employees and employers; they have further implications for consumer, social welfare, inter-state competition in attracting businesses, intra-state competition for labor forces, and business dynamism. Policy makers and legislators should take these broader impacts into account.

Figure 2.1: U.S. State Non-competes Enforceability and Regional Business Concentration

(a). Share of establishments by firm size



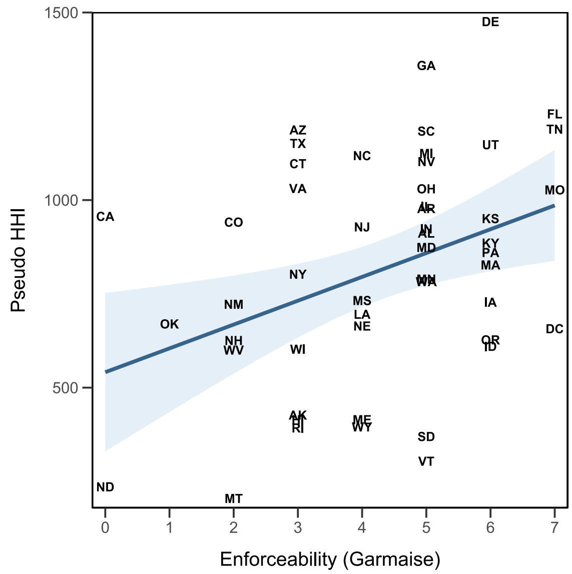
(b). Share of employment by firm size



(continued on the next page)

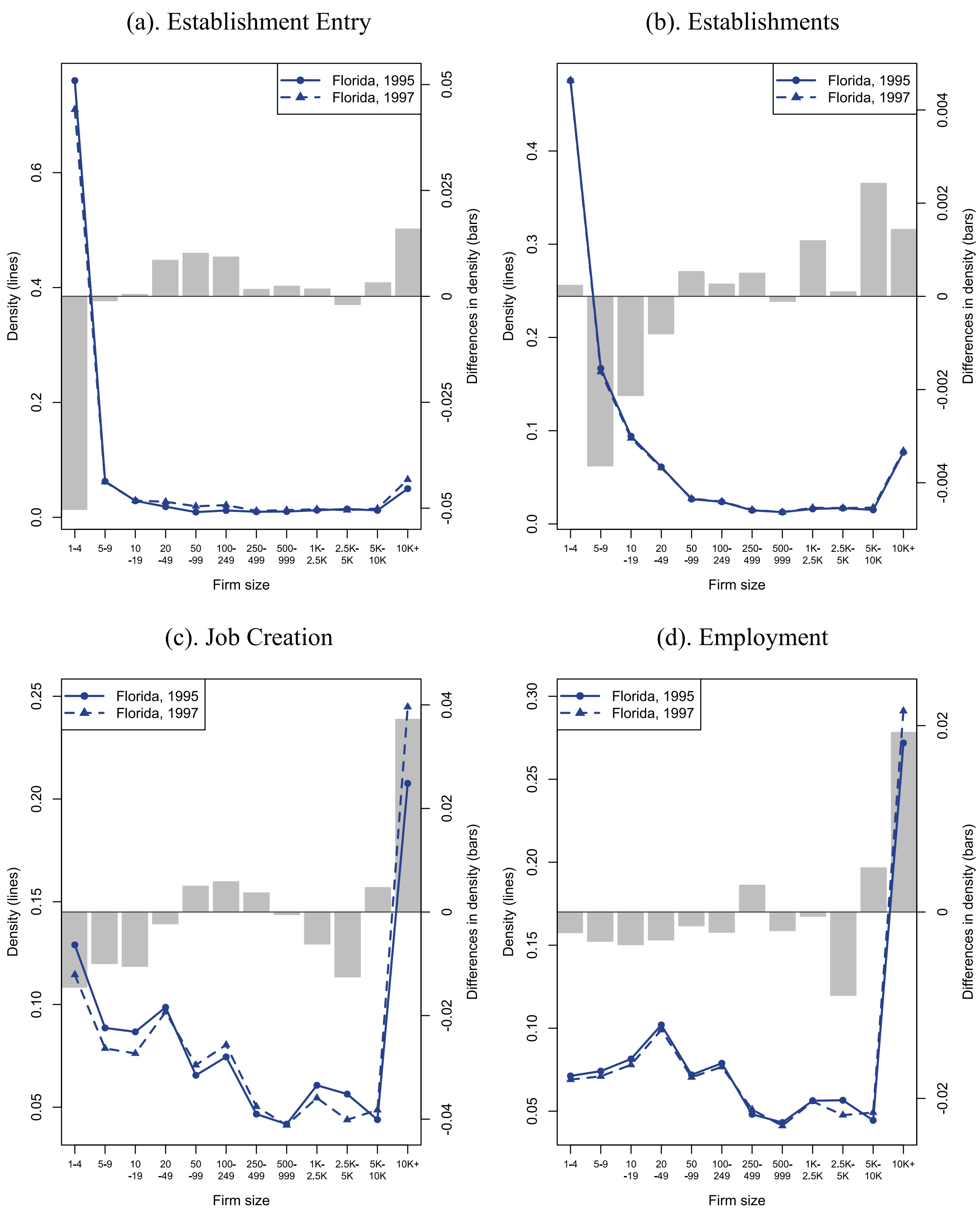
(Figure 2.1 continued)

(c). Pseudo HHI



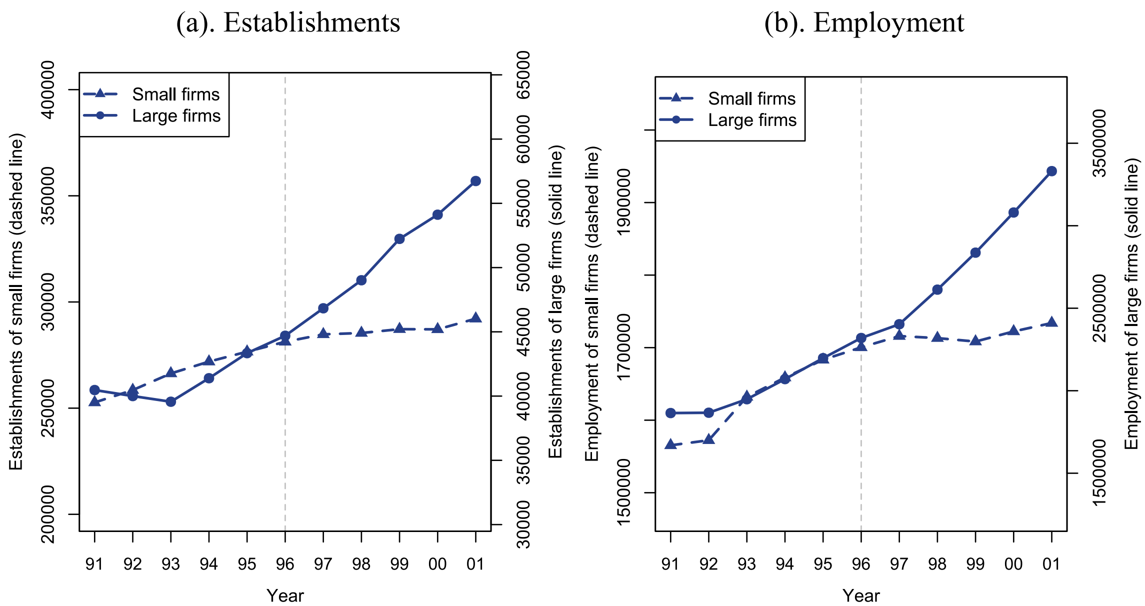
All panels: Blue solid line represents a fitted (bivariate) regression line with full sample: regressed each outcomes on non-compete enforceability, including an intercept. Results for regressions: (a). left panel: coefficient -0.0052, standard error 0.0021, p-value 0.0162; right panel: coefficient 0.0031, standard error 0.0016, p-value 0.0570; (b). left panel: coefficient -0.0120, standard error 0.0041, p-value 0.0057; right panel: coefficient 0.0143, standard error 0.0047, p-value 0.0036; (c). coefficient 63.51, standard error 22.79, p-value 0.0076. Small firms: <50 employees. Large firms: >1,000 employees. *Data*: Business Dynamics Statistics (BDS), 1996.

Figure 2.2: Density of Establishments and Employment of Florida Firms by Size: 1995 vs. 1997



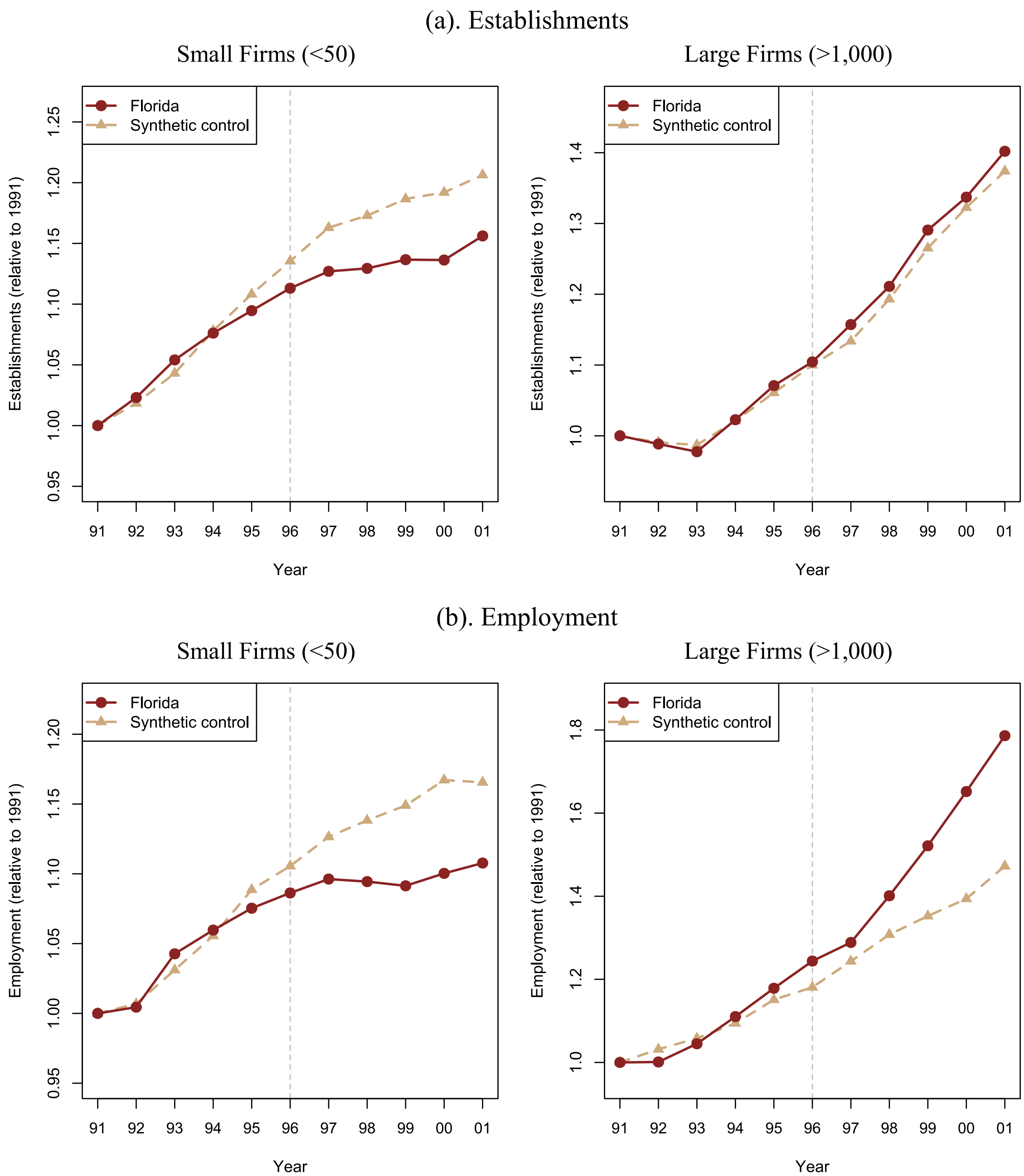
Lines represent firm size distribution (density), while bars represent the difference in density between 1995 and 1997. *Data*: Business Dynamics Statistics (BDS), 1995 and 1997.

Figure 2.3: Trends in Establishments and Employment of Florida Firms by Size: Split Sample



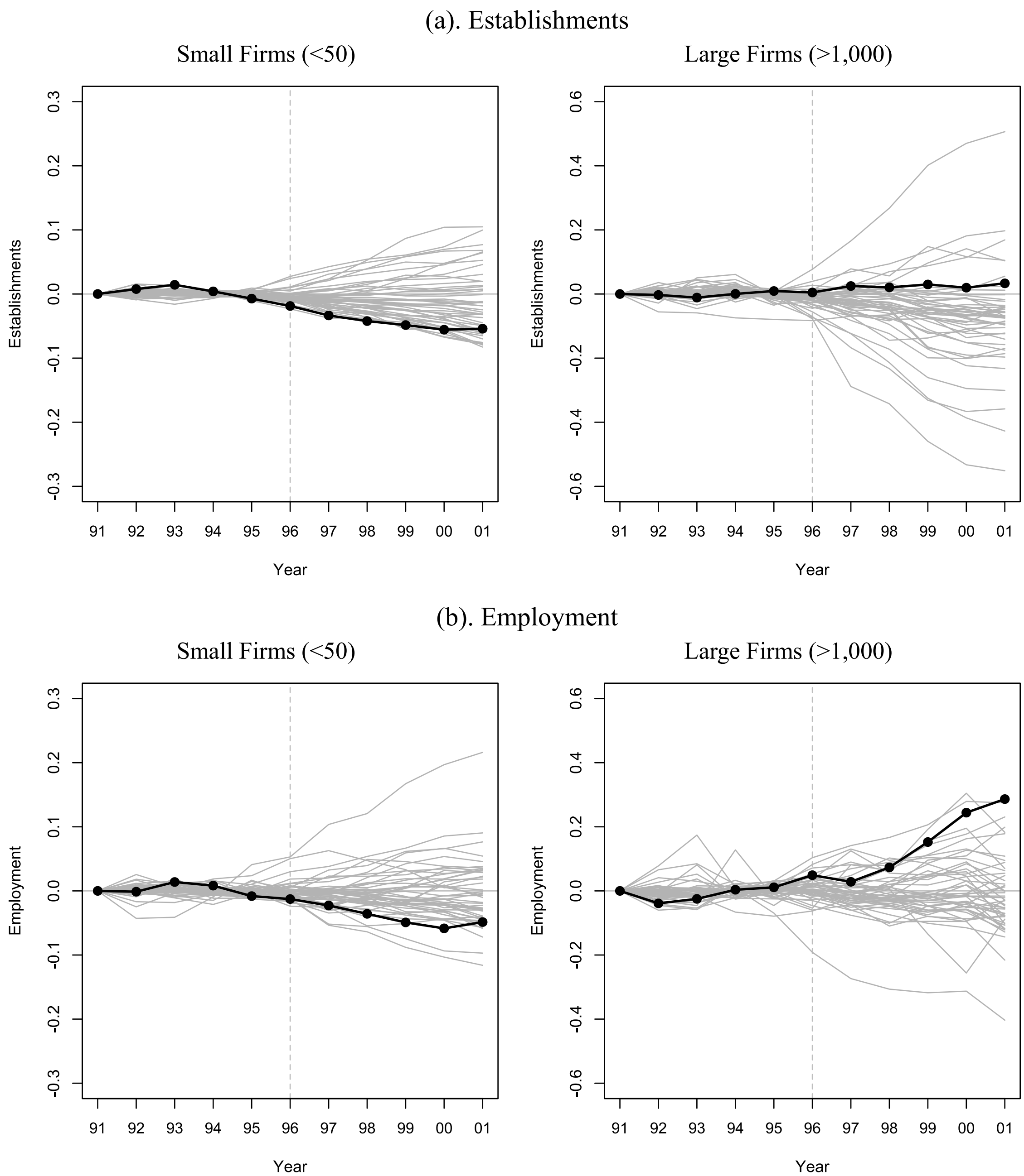
Data: Business Dynamics Statistics (BDS), 1991-2001.

Figure 2.4: Establishments and Employment by Firm Size: Florida vs. Synthetic Control



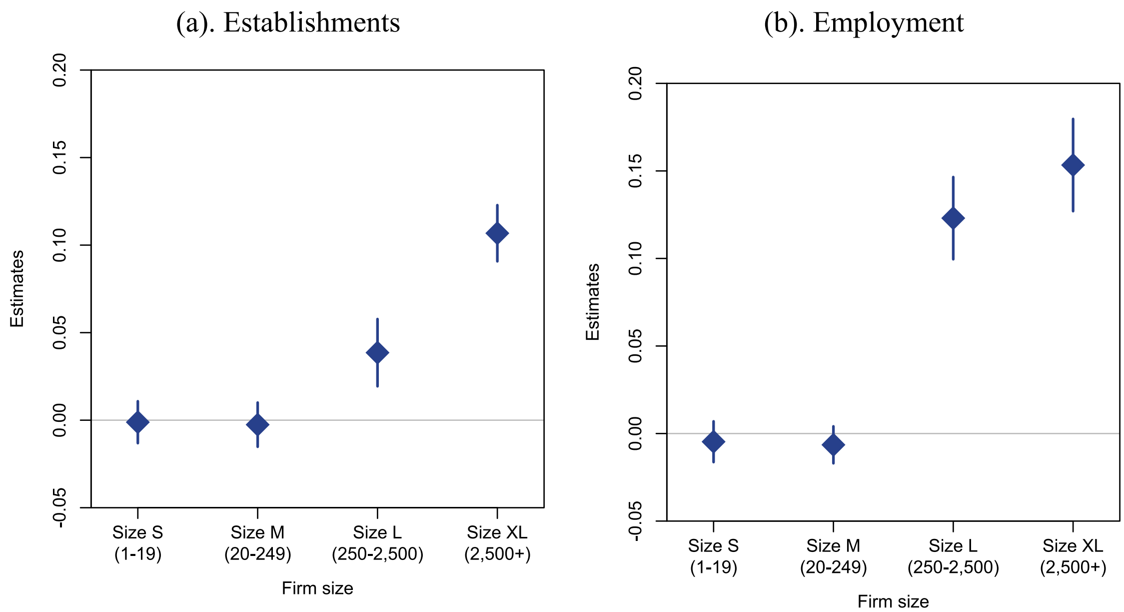
Note: The outcome variables for Florida are normalized relative to their 1991 value. *Data*: Business Dynamics Statistics (BDS), 1991-2001.

Figure 2.5: Establishments and Employment by Firm Size: Placebo Tests for Synthetic Controls



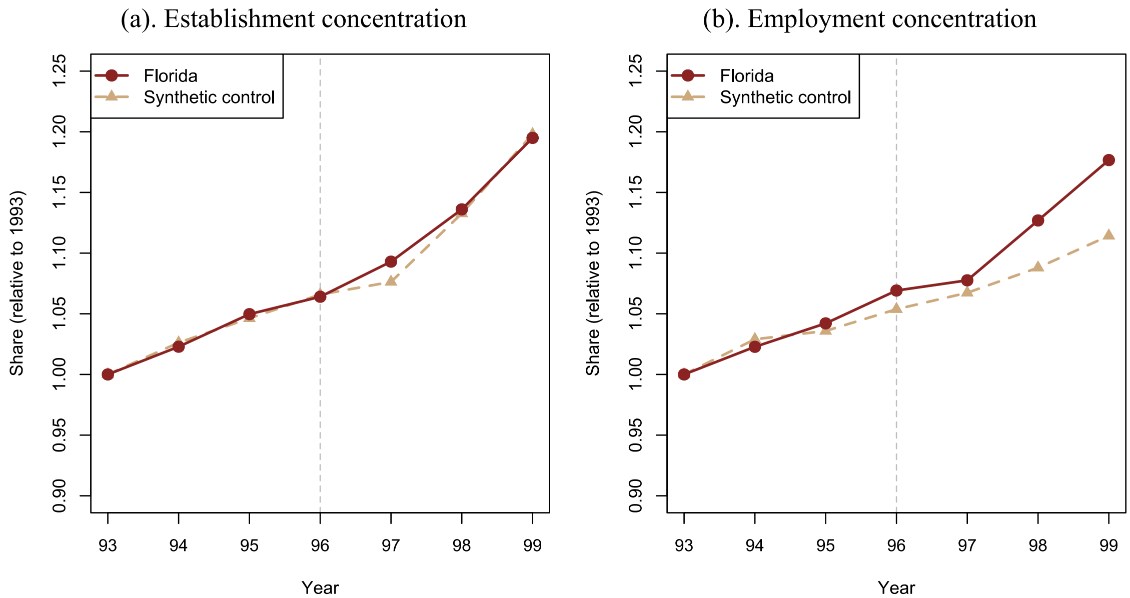
These figures show the *gaps* in the outcome variable for the treated state and the synthetic control. The black line represents our test for Florida (the actual treated state). We additionally perform *placebo tests*, pretending that the states in our control group were treated. Each of these placebo tests are presented in the gray lines. The outcome variables for Florida are normalized relative to their 1991 value. *Data*: Business Dynamics Statistics (BDS), 1991-2001.

Figure 2.6: Establishments and Employment of Florida Firms by Size: Split-Sample Regressions



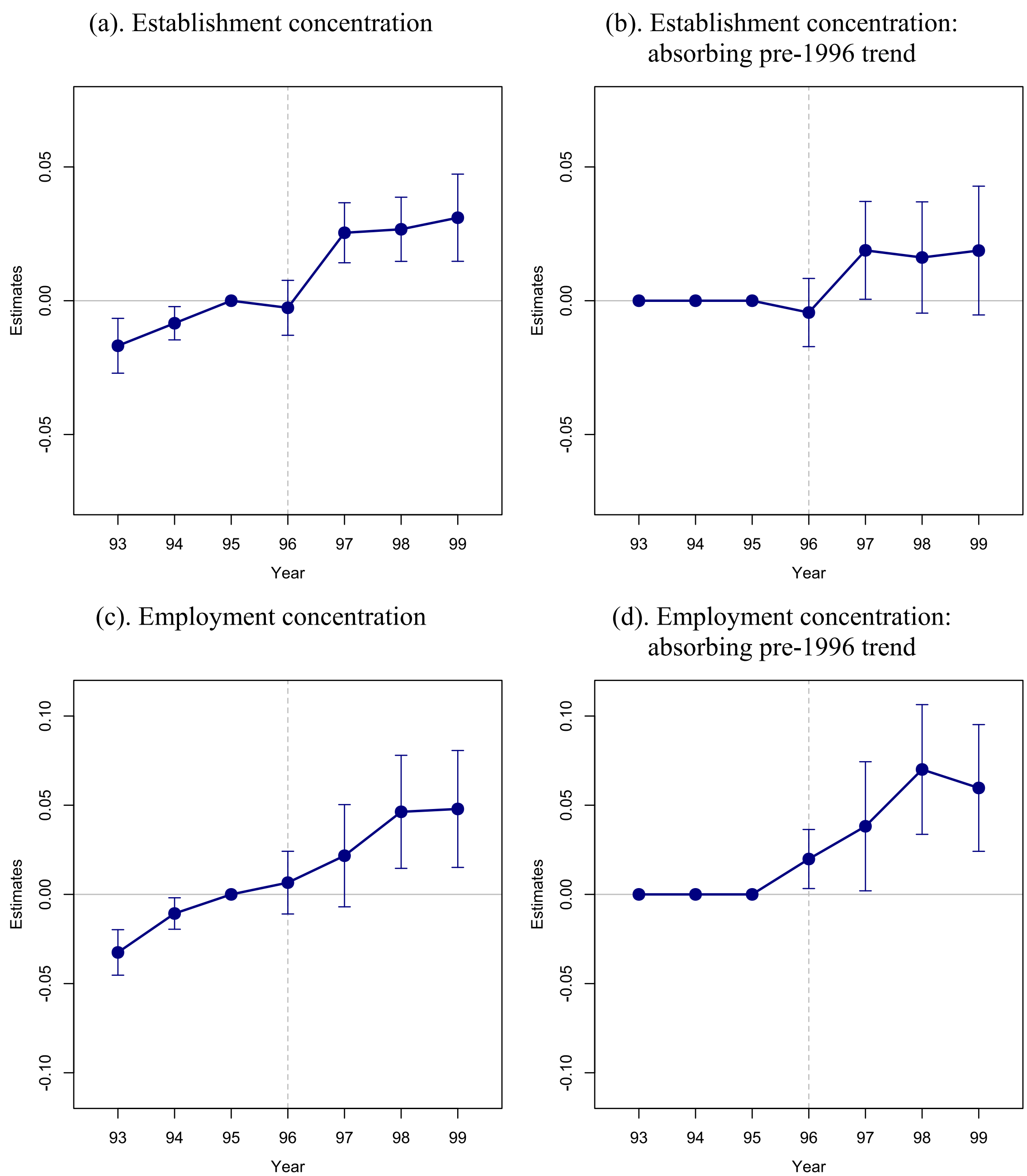
Note: Each point stands for an estimate (*Post* × *FL*) from separate regressions by firm size category. Red real lines stand for 95% confidence internal based on standard errors clustered at the state level. *Data*: Business Dynamics Statistics (BDS), 1993-1999.

Figure 2.7: Business Concentration: Florida vs. Synthetic Control



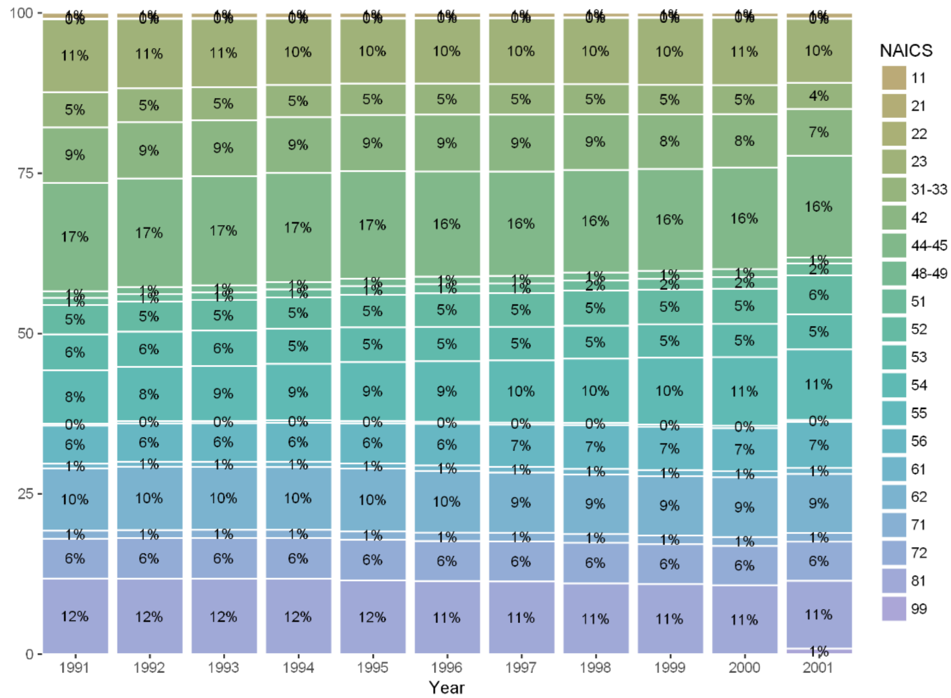
Note: The outcome variables are normalized relative to their 1991 value. We measure "establishment (or business-unit) concentration" as the share of establishments by Large firms (that have more than 1,000 employees) and "employment concentration" as the share of employment by Large firms. *Data*: Business Dynamics Statistics (BDS), 1993-1999.

Figure 2.8: Regional Business Concentration in Florida: Even Study Approach



The solid lines show the estimates by year and vertical lines represent a 95% confidence interval. *Panels (b) and (d):* we interact yearly outcomes for pre-amendment years with a full set of year dummies. This absorbs all the pre-1996 differences in concentration in our analyses, and some of the post-1996 variation, but makes our post-1996 comparisons close to *ceteris paribus.* By design, there are no pre-1996 differences in trends between treatment and control groups. *Data*: Business Dynamics Statistics (BDS), 1993-1999.

Figure 2.9: Industry Composition in Florida, 1991-2001

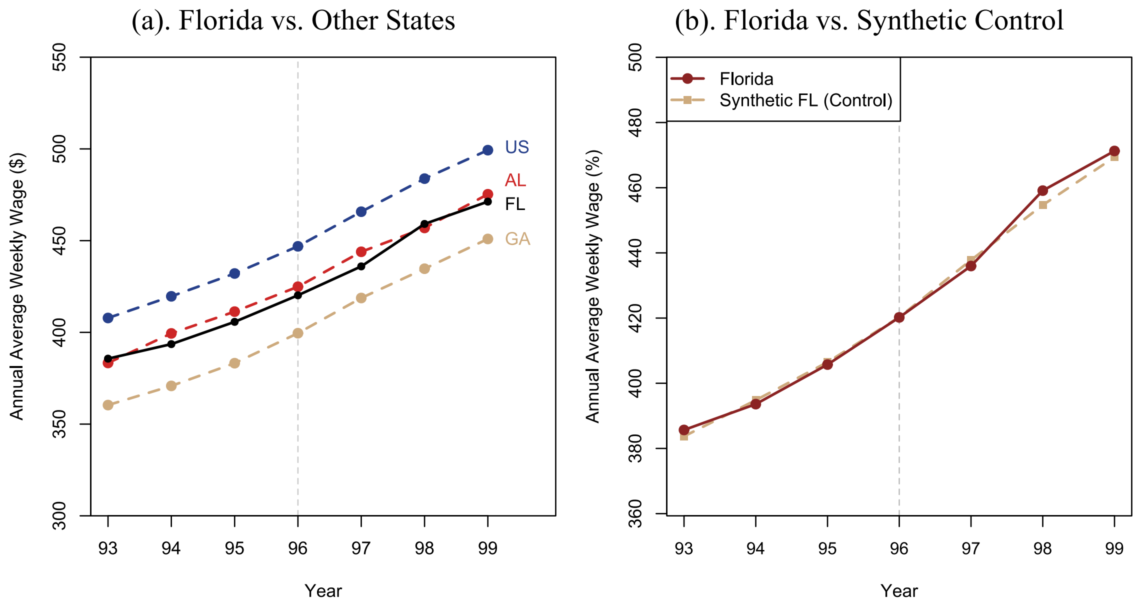


Share of industries calculated based on the number of establishments in each industry. *Data*: Quarterly Census of Employment and Wages (QCEW), Bureau of Labor Statistics (BLS).

Figure 2.10: MSAs Near the Florida Border

Border MSAs included: Pensacola-Ferry Pass-Brent, FL (C3786: Escambia County, Santa Rosa County), Fort Walton Beach-Crestview-Destin, FL (C2302: Okaloosa County), Tallahassee, FL (C4522: Leon County, Gadsden County, Wakulla County, Jefferson County), Jacksonville, FL (C2726: Duval County, Clay County, St. Johns County, Nassau County, Baker County), Mobile, AL (C3366: Mobile County), Dothan, AL (C2002: Geneva County, Henry County, Houston County), Valdosta, GA (C4666: Brooks County, Echols County, Lanier County, Lowndes County). *Note*: more than half of the counties in these borderline areas do not belong to any MSAs.

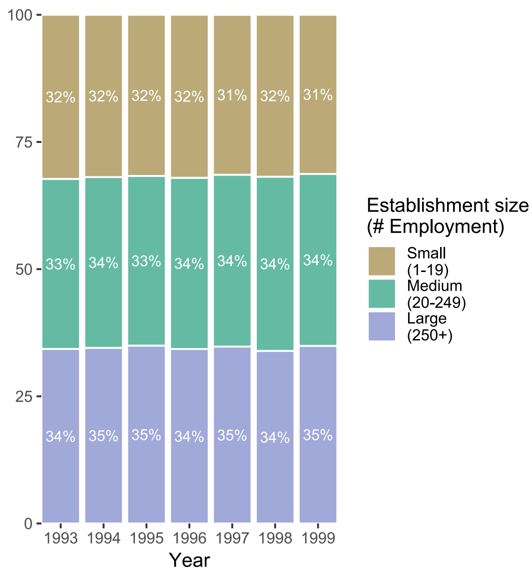
Figure 2.11: Wage Trends in Florida and Other States



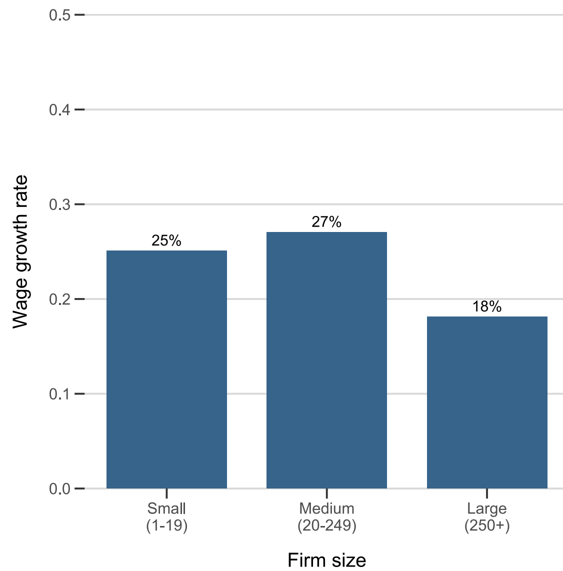
Panel (a): Black solid line represents Florida. Blue, red, and brown dashed lines represent the rest of U.S. states (excluding California, Hawaii, Louisiana, and Texas), Alabama, and Georgia, respectively. Differences-in-differences estimations also show that we cannot reject the null hypothesis that Florida's wage change is not different from the control states (estimate: 0.0048, robust standard error clustered at the state level: 0.0040, p-value: 0.226). *Panel (b)*: Red solid line represents Florida. Brown dashed line represents the counterfactual Florida from the Synthetic Control Method. The outcome variable, average weekly age (annual), is normalized relative to its 1991 value. *Data*: Quarterly Census of Employment and Wages (QCEW), Bureau of Labor Statistics (BLS). 1993-1999.

Figure 2.12: Wage in Florida by Establishment Size

(a). Share of Wage by Establishment Size



(b). Wage Growth Rate by Establishment Size



Panel (a): Graph shows the share of wage by establishment size in Florida over time. *Panel (b)*: Bars show the growth rate of wage by establishment size. Average wages in 1997-2000 is subtracted and divided by average wages in 1992-1995 by each establishment size. *Data*: Quarterly Census of Employment and Wages (QCEW), Bureau of Labor Statistics (BLS). 1993-1999.

Table 2.1: The 1996 Amendment to the Florida Statutes and Non-competes Enforceability

	§542.33B (1990 – Jun 1996)	§542.335 (July 1996 – Present)	Note
Protection of business interests	Not specified	Lists five *non-exclusive* legitimate business interests that can be protected	Provides an open-ended enumeration of what the employers can do (but not what they cannot do)
The modification of over-broad covenants ("Blue pencil")	Courts have option either to modify or not to enforce	Courts can only modify the excessive restraints rather than declaring it non-enforceable	Made it easier for employers to write highly restrictive covenants (without fearing it being overturned)
Burden of proof	Not specified	Once an employer proves that the non-competes meet the "legitimate business interests" restriction, the burden of proof shifts to employee	§542.335(1)(c): "the person opposing enforcement has the burden of establishing that the restraint is over-broad, overlong, or otherwise not reasonably necessary …"
Injunctions and the presumption of irreparable injury	Not specified	Once an employer shows the intentional breach of non-competes, irreparable harm is presumed. Courts may issue an injunction that prohibits competition not only by the former employee, but also by his/her new employer	Made it easier for employers to receive injunctions. Courts may also award damages for a violation of non-competes, including lost profits and damages
Limitations on public policy defense	Allows the courts to consider public policy and welfare (when entering injunction)	Courts could not refuse enforcement on the grounds that it violated public policy, with few exceptions	Sharply limited the use of the "contrary to public policy" defense against the enforcement of non-competes
Consideration of individual economic hardship	Not specified	Not allowed to consider an employee's individual hardship	
An interpretation favoring business protection	Not specified	Required to construe covenants "in favor of providing reasonable protection to all legitimate business interests established by the person seeking enforcement"	Not allowed to construe the covenant narrowly against the drafter or against enforcement
Enforcement despite the discontinuation of business	Not specified	An employee has to prove that the discontinuation had nothing to do with his or her work for the competitor	
Award of attorney's fees	Not specified	Allowed for the awarding of attorney's fees and costs to the prevailing party	Imposed asymmetric burden to an employee

Table 2.2: Descriptive Statistics and Correlations

Variables	Mean	Std. dev	Min	Max	1	2	3	4	5	6	7	8	9	10
MSA-FSIZE-YEAR Level														
1 Establishment Entry	123.7	867.7	0.0	43,299.0	1.00	0.95	0.65	0.25	0.42	0.25	0.02	0.01	-0.16	0.01
2 Establishments (Total)	1,031.8	4,910.7	2.0	230,333.0	0.95	1.00	0.75	0.41	0.56	0.39	0.01	0.01	-0.19	0.01
3 Job Creation by Incoming Firms	1,108.4	3,494.4	0.0	99,261.0	0.65	0.75	1.00	0.85	0.94	0.86	0.03	0.03	-0.04	0.03
4 Job Creation by Continuing Firms	1,956.2	5,707.1	0.0	193,425.0	0.25	0.41	0.85	1.00	0.98	0.97	0.01	0.02	0.04	0.02
5 Job Creation (Total)	3,064.6	8,874.3	0.0	291,020.0	0.42	0.56	0.94	0.98	1.00	0.97	0.02	0.03	0.01	0.03
6 Employment	18,484.3	54,145.2	7.0	1,673,631.0	0.25	0.39	0.86	0.97	0.97	1.00	0.00	0.02	0.09	0.02
7 Florida (Indicator)	0.1	0.2	0.0	1.0	0.02	0.01	0.03	0.01	0.02	0.00	1.00	0.00	0.00	0.00
8 Post 1996 (Indicator)	0.5	0.5	0.0	1.0	0.01	0.01	0.03	0.02	0.03	0.02	0.00	1.00	0.00	0.93
9 Firm Size (Categorical)	6.5	3.5	1.0	12.0	-0.16	-0.19	-0.04	0.04	0.01	0.09	0.00	0.00	1.00	0.00
10 Year	1996	2.2	1993	1999	0.01	0.01	0.03	0.02	0.03	0.02	0.00	0.93	0.00	1.00
MSA-YEAR Level														
1 Small Firms' (<50 employees) Share of Establishments (%)	11.0	0.6	8.8	14.2	1.00	-0.29	0.66	-0.12	-0.05					
2 Large Firms' (>1,000) employees) Share of Establishments (%)	1.8	0.4	0.8	3.1	-0.29	1.00	-0.46	0.70	0.51					
3 Small Firms' (<50 employees) Share of Employment (%)	4.6	0.9	2.7	8.9	0.66	-0.46	1.00	-0.65	-0.43					
4 Large Firms' (>1,000 employees) Share of Employment	5.9	1.2	2.0	10.3	-0.12	0.70	-0.65	1.00	0.78					
5 Pseudo HHI	19.6	12.0	2.1	98.2	-0.05	0.51	-0.43	0.78	1.00					

Table 2.3: Effects of Non-competes on Establishments and Employment of Florida Firms by Size: Split Samples

	Dependent variables:			
	Establishment Entry	Establishment	Job Creation	Employment
	(1)	(2)	(3)	(4)
	A. Split Sample: Small Firms (#Employees<50)			
Post×FL	-0.0562^{***}	-0.0033	-0.0183	-0.0048
	(0.0101)	(0.0062)	(0.0074)	(0.0060)
MSA F.E.	Y	Y	Y	Y
Year F.E.	Y	Y	Y	Y
Observations	7,488	7,488	7,488	7,488
	B. Split Sample: Large Firms (#Employees>1,000)			
Post×FL	0.0849^{***}	0.0981^{***}	0.0760^{***}	0.1468^{***}
	(0.0154)	(0.0073)	(0.0187)	(0.0121)
MSA F.E.	Y	Y	Y	Y
Year F.E.	Y	Y	Y	Y
Observations	7,488	7,488	7,488	7,488

*p<0.1; **<0.05; ***p<0.01. Small (Panel A) and Large (Panel B) firm split samples. Robust standard error, clustered at the state level. *Data*: Business Dynamics Statistics (BDS), 1993-1999.

Table 2.4: Effects of Non-competes on Establishments and Employment of Florida Firms by Size: Interaction

	Dependent variables:			
	Establishment Entry (1)	Establishment (2)	Job Creation (3)	Employment (4)
Post×FL	–0.0541***	–0.0011	–0.0273***	–0.0047
	(0.0105)	(0.0060)	(0.0073)	(0.0058)
Post×FL×Size M (20-249)	0.0372***	–0.0014	0.0241***	–0.0018
	(0.0131)	(0.0039)	(0.0084)	(0.0039)
Post×FL×Size L (250-2,500)	0.1526***	0.0397***	0.2357***	0.1277***
	(0.0140)	(0.0086)	(0.0210)	(0.0095)
Post×FL×Size XL (2,500+)	0.1236***	0.1079***	0.0832***	0.1580***
	(0.0181)	(0.0066)	(0.0200)	(0.0123)
MSA F.E.	Y	Y	Y	Y
Year F.E.	Y	Y	Y	Y
Observations	22,464	22,464	22,464	22,464

*p<0.1; **<0.05; ***p<0.01. Full sample. Robust standard error, clustered at the state level. *Data*: Business Dynamics Statistics (BDS), 1993-1999.

Table 2.5: Effects of Non-competes on Regional Business Concentration

	Dependent variables: Business Concentration		
	Establishment concentration (1)	Employment concentration (2)	Pseudo HHI (3)
Post×FL	0.0244^{***}	0.0467^{***}	0.1739^{***}
	(0.0051)	(0.0055)	(0.0273)
MSA F.E.	Y	Y	Y
Year F.E.	Y	Y	Y
Observations	1,872	1,872	1,872

*p<0.1; **<0.05; ***p<0.01. Log-Linear regression with full sample. We measure "establishment (or business-unit) concentration" as the share of establishments by Large firms and "employment concentration" as the share of employment by Large firms. Large firms are defined as firms that have more than 1,000 employees. Robust standard error, clustered at the state level. *Data*: Business Dynamics Statistics (BDS), 1993-1999.

Table 2.6: Effects of Non-competes on Establishments and Employment of Florida Firms by Size: Split Samples (Border & Matching)

	Dependent variables:							
	Establishment Entry		*Establishment*		*Job Creation*		*Employment*	
	Matching (1)	Border (2)	Matching (3)	Border (4)	Matching (5)	Border (6)	Matching (7)	Border (8)
	A. Split Sample: Small Firms (#Employees<50)							
Post×FL	–0.0506***	–0.0022	–0.0161	–0.0058	–0.0315***	–0.0341	–0.0180*	–0.0287**
	(0.0178)	(0.0169)	(0.0106)	(0.0139)	(0.0114)	(0.0519)	(0.0109)	(0.0159)
MSA F.E.	Y	Y	Y	Y	Y	Y	Y	Y
Year F.E.	Y	Y	Y	Y	Y	Y	Y	Y
Observations	1,248	168	1,248	168	1,248	168	1,248	168
	B. Split Sample: Large Firms (#Employees>1,000)							
Post×FL	0.1368***	0.2439***	0.1168***	0.1622***	0.0847**	0.2658	0.169***	0.0969***
	(0.0328)	(0.0781)	(0.0188)	(0.0263)	(0.0376)	(0.2007)	(0.0400)	(0.0445)
MSA F.E.	Y	Y	Y	Y	Y	Y	Y	Y
Year F.E.	Y	Y	Y	Y	Y	Y	Y	Y
Observations	1,248	168	1,248	168	1,248	168	1,248	168

*p<0.1; **<0.05; ***p<0.01. Only borderline MSAs are included in columns (2), (4), (6), and (8). Robust standard error, clustered at the state level. *Data*: Business Dynamics Statistics (BDS), 1993-1999. Small (Panel A) and Large (Panel B) firm split samples.

Table 2.7: Effects of Non-competes on Establishments and Employment of Florida Firms by Size: Interaction (Border & Matching)

	Dependent variables:							
	Establishment Entry		*Establishment*		*Job Creation*		*Employment*	
	Matching (1)	Border (2)	Matching (3)	Border (4)	Matching (5)	Border (6)	Matching (7)	Border (8)
Post×FL	–0.0396	–0.0292	–0.0131	–0.0082	–0.0328	–0.0437***	–0.0166	–0.0209***
	(0.0171))	(0.0340)	(0.0103)	(0.0202)	(0.0119)	(0.0202))	(0.0097)	(0.0035)
Post×FL× Size (20-249)	0.0561	0.0611***	0.0040	0.0181	0.0345***	0.0188**	–0.0016	–0.0434***
	(0.0369)	(0.0095)	(0.0103)	(0.0555)	(0.0139)	(0.0099)	(0.0104)	(0.0161)
Post×FL× Size (250-2,500)	0.1521***	0.0672	0.0325***	0.1450***	0.1638***	–0.0926	0.1181***	–0.1268
	(0.0383)	(0.0417)	(0.0137)	(0.0296)	(0.0413)	(0.3020)	(0.0295)	(0.0890)
Post×FL× Size (2,500+)	0.1669***	0.3211**	0.1355***	0.1169***	0.1091***	0.4576***	0.1911***	0.2645***
	(0.0278)	(0.1835)	(0.0136)	(0.0079)	(0.0349)	(0.0638)	(0.0334)	(0.0691)
MSA F.E.	Y	Y	Y	Y	Y	Y	Y	Y
Year F.E.	Y	Y	Y	Y	Y	Y	Y	Y
Observations	3,744	504	3,744	504	3,744	504	3,744	504

*p<0.1; **<0.05; ***p<0.01. Only borderline MSAs are included in columns (2), (4), (6), and (8). Robust standard error, clustered at the state level. *Data*: Business Dynamics Statistics (BDS), 1993-1999.

Table 2.8: Effects of Non-competes on Regional Business Concentration (Matching)

	Dependent variables: Business Concentration		
	Establishment Concentration (1)	Employment Concentration (2)	Pseudo-HHI (3)
Post×FL	0.0361***	0.0530***	0.0467
	(0.0072)	(0.0145)	(0.0337)
MSA F.E.	Y	Y	Y
Year F.E.	Y	Y	Y
Observations	276	276	276

*p<0.1; **<0.05; ***p<0.01. Log-Linear regression with full sample. We measure "establishment (or business-unit) concentration" as the share of establishments by Large firms and "employment concentration" as the share of employment by Large firms. Large firms are defined as firms that have more than 1,000 employees. Robust standard error, clustered at the state level. *Data*: Business Dynamics Statistics (BDS), 1993-1999.

3 Protecting Invention or Inventor? Strategic Knowledge Management against Worker Mobility

"Mobility of personnel among firms provides a way of spreading information (Arrow, 1962; p. 615)"

3.1 Introduction

The inter-firm mobility of employees has received widespread attention, especially in the context of knowledge-based industries (e.g., Agarwal *et al.,* 2004; Arrow, 1972; Saxenian, 1996). The research stream on "learning-by-hiring," in particular, shows that firms can leverage employee hiring decisions as an opportunity to absorb external knowledge (e.g., Palomeras and Melero, 2010; Rosenkopf and Almeida, 2003; Singh and Agrawal, 2011; Song, Almeida, and Wu, 2003; Stolpe, 2002).

For employers who *lose* an employee, however, the mobility of employees poses a threat, as knowledge that resides in the firm can leak to competitors (e.g., Agarwal, Ganco, and Ziedonis, 2009; Campbell, Ganco, *et al.,* 2012; Carnahan, Agarwal, and Campbell, 2012; Gambardella, Ganco, and Honoré, 2014; Ganco, Ziedonis, and Agarwal, 2015). Retaining employees is critical for firms to protect their business secrets, existing customer base, and valuable knowledge from flowing to competitors (Agarwal, Campbell, Franco, and Ganco, 2016; Wezel, Cattani, and Pennings, 2006). Employee mobility to competitors is a double loss for a prior employer, as it not only damages a firm's own competitive advantage but also transfers its competitiveness to them (Agarwal, Ganco, and Ziedonis, 2009; Campbell, Ganco, Franco, and Agarwal, 2012; Cohen and Levinthal, 1900; Somaya, Williamson, and Lorinkova, 2008).

A dilemma for innovating firms in knowledge-based industries is that they must protect as well as create knowledge. Although the field of strategy has long been interested in how employee participation and turnover impact firms' strategic decisions (e.g., Cohen, March, and, Olsen 1972; Kogut and Zander, 1992), few studies have explored how to manage innovation processes and protect the knowledge produced.

We examine how firms strategically manage their knowledge and innovation processes when their workers' mobility increases. Drawing from literature on innovation and knowledge management and taking insights from previous research which shows that firms make innovation decisions in relative and competitive contexts rather than as individual agents (e.g., Grossman and Shapiro, 1985; Shapiro, 1985), we make two predictions on a firm's strategic knowledge management when facing higher worker mobility. First, we posit that firms will have less incentive to invest in R&D. This is because the output of R&D investments can, without intending to, leak to their competitors when key personnel with relevant knowledge leave to join them (Anton and Yao, 2004; Arora, 1997; Audretsch and Feldman, 1996; Jaffe, Trajtenberg, and Henderson, 1993; Wezel, Cattani, and Pennings, 2006; Zander and Kogut, 1995). Second, firms will rely more on patenting – a formal protection of their intellectual property – than on secrecy to protect knowledge produced. Firms make a choice between patenting and secrecy, depending on the mobility of their workers (Cohen, Nelson, and Walsh, 2000; Hall *et al.,* 2014). Although secrecy has the advantage of not revealing a firm's inventions publicly, this strategy becomes less effective as worker mobility increases (Friedman, Landes, and Posner, 1991; Lemley, 2008; Png, 2017).

Our empirical strategy leverages an exogenous source of variation that changed the employee mobility faced by U.S. firms. The Application Group, Inc. v. Hunter Group, Inc., 61 Cal. App. 4th 881 (1998) – henceforth Application vs. Hunter (1998) – provides us with a nearly ideal setting to study our research question. In the U.S., many firms prevent their employees from joining competitors by having them sign non-compete agreements (Prescott, Bishara, and Starr, 2016). Non-compete agreements are contracts in which an employee agrees not to work in direct competition with the current employer for a certain amount of time in a specified area of expertise (Garmaise, 2009; Marx and Fleming, 2012). In Application vs. Hunter (1998), the California Court of Appeal refused to enforce *out-of-state* non-compete agreements written between a non-California employer and a non-California employee. After 1998, employees who were bound by non-competes in states other than California could now move freely to employers that reside in California without being bound by their non-competes. This court decision caused California to emerge as a "loophole" in non-compete enforcement. Yet, the Application vs. Hunter (1998) only affected firms that had been previously enforcing non-competes and left firms that had not been enforcing non-competes unaffected by this loophole. In our difference-in-differences model, we study its impact on firms *outside* California, comparing firms in states that enforce non-competes ("treatment group") against firms in states that do not enforce non-competes ("control group"), for the pre-1998 period versus the post-1998 period.

We find that, post-1998, firms in states that enforce non-competes decreased their innovation input, R&D investment. Yet they filed more patents, suggesting that firms patent more intensely, not as a result of their fundamental R&D activities, but as a means of protecting their knowledge that could leak to competitors through separating employees. In other words, firms rely more on a formal patent system ("invention protection") than on secrecy by retaining workers ("inventor protection") when facing higher employee mobility and knowledge leakage.

Our findings, taken together, contribute to a broad stream of literature. First, this is one of the first studies to investigate how firms' innovation strategies are determined by the threat of employee separation. Understanding how firms protect their knowledge from employee separation is of great importance, as employee mobility to competitors not only hurts a firm's innovation capabilities but also benefits those of its competitors. Second, we show how a policy change in one state has far-reaching consequences outside the focal state. Third, relatedly, our identification strategy exploits the legal change in California but examines firms in other states that are exogenously affected by the change. Prior studies regarding non-compete agreements have focused on several policy changes – notably, the

Michigan Antitrust Reform Act of 1984 or the amendment to the Florida Statutes of 1996 – and its consequences *within* the focal states (e.g., Kang and Fleming, 2019; Marx, Strumsky, and Fleming, 2009). This study, in contrast, examines the *externality effects* of the change in local legal enforcement and how such change reshapes strategic knowledge management of firms in the rest of the states across the nation. That way, we have much larger set of treatment (and control) groups, with treatment being most likely to exogenously affect the treatment group only.

3.2 Application vs. Hunter (1998) and Out-of-State Non-competes

We exploit California's law on its enforcement of *out-of-state* non-compete agreement. To understand its implications for non-compete enforcement in states other than California, we briefly document the history of non-compete enforcement in California.

California has a strong public policy against the enforcement of restrictive covenants in the employment, including even a voluntarily entered non-competes. The most relevant statute is California Business & Professional Code Section 16600 ("Section 16600") which states that:

> *Except as provided in this chapter, every contract by which anyone is restrained from engaging in a lawful profession, trade, or business of any kind is to that extent void.*

Since the enactment of Section 16600 in 1872, California has consistently rejected to enforce *in-state* non-compete agreements (non-competes that are agreed between a California employer and employee). *Out-of-state* non-competes (signed by an employer and employee *outside* California), on the other hand, had been construed to be enforceable under California law.

Application vs. Hunter (1998) was the first legal decision that established that out-of-state non-competes are not enforceable in California (and this is the case even if a "choice-of-law" provision was present in their agreement).[26] In this case, the issue arose when a California-based company, Application Group, Inc. ("AGI") hired a consultant in computerized human resources management system from a competing firm in Maryland, Hunter Group Inc ("Hunter") in 1991. (Kahn, 1999). Maryland law, unlike California law, to some extent allows employers to contract on non-competes. When hiring, Hunter had signed a non-compete agreement with Pike, which prohibited the consultant from working for a competing firm until one year after the termination of her employment. The contract also included a "choice-of-law" provision, which allowed Hunter to contend that the court must decide this issue under Maryland law.

After Pike resigned Hunter to join AGI, both companies took instant actions separately. First, in 1992, Hunter sued Pike and AGI in the Maryland Circuit Court for a

[26] The California Labor Code Section 925 went into effect in January 2017. This policy specifically restricts an employer's ability to enter agreements that include out-of-state choice of law provision with employees who primarily resides and works in California.

breach of contract and unlawful interference. A few month later, AGI filed a complaint to California courts for a declaratory judgement arguing that Section 16600 rather than Maryland law should be applied for this case. The Maryland Circuit Court, however, favored AGI in their decision, denoting that Hunter did not provide enough evidence to claim damages. This decision allowed California courts to proceed with their requests with AGI's declaratory relief which was pending on Maryland Court's decision. The California trial courts decided, in January 1995, that California law should indeed apply to the hiring of Pike, revising its initial decision.

In February 1998, the California Court of Appeal further confirmed the trial court's decision, following a response of Hunter's timely appeal to the trial court's decision. The California Court of Appeal stated that enforcing non-competes in California would violate California's public policy, even if the contract was signed between a Maryland firm and a Maryland resident and specifically stated that it should be construed under Maryland law.[27]

The Application vs. Hunter (1998) provided an "emergency exit" for workers under the shackle of non-competes. After the court decision, employees bound by non-competes could move freely to firms in California, as the state no longer enforced out-of-state non-competes that are signed outside of California. From an employer's perspective, non-California firms faced an unexpected "loophole" in the enforceability of their non-competes, as the decision made all existing non-competes suddenly void in California.

3.3 Worker Mobility and Strategic Knowledge Management

3.3.1 R&D Investment

We examine how firms manage the process of knowledge creation and management in response to the mobility of their knowledge workers. We first establish that firms will reduce (or at least have no reason to increase) their R&D investments when worker mobility increases. First, the benefit side of R&D investment decreases. Knowledge is embedded in human capital, and separating employees take both tacit and explicit knowledge with them when leaving their former employers (Argote and Ingram, 2000; Møen, 2005). The misappropriation risk that resides in an R&D project increases as the workers become more mobile (Anton and Yao, 2004). Employee mobility can lead to knowledge leakage if separated employees with such knowledge join competitors who can then use knowledge from the employee's previous employer to develop similar projects or engage in reverse engineering (Agarwal *et al.,* 2009; Ganco, 2013). Separating employees

[27] There were several other decisions prior to the final decision by the California Court of Appeal. In trial court, Judge Norman issued a statement of decision, denying AGI's claims for declaratory relief (January 30, 1995). In April 5, 1995, however, Judge Norman issued a revised statement of decision (in response to AGI's objections to the proposed statement of decision) which, for the most part, adopted the rationale of Judge Cahill's prior ruling that California law applies to AGI's hiring of Hunter employees. On June 15, 1995, judgment was entered based on Judge Norman's revised statement of decision and Judge Cahill's orders. The trial court decision in 1995 can be thought of as an early, preliminary treatment. We will consider this when discussing the event-study approach in the Result, Section 3.5.

can also use knowledge from previous employers to create their own entrepreneurial spin-offs (Agarwal *et al.,* 2004; Klepper and Sleeper, 2005; Phillips, 2002; Starr, Balasubramanian, and Sakakibara, 2015). In either case, employee separation significantly decreases the expected benefits of an R&D project, compared to a situation where firms can retain the employee.

Second, the risk associated with highly uncertain R&D investment becomes higher as worker mobility increases. It is highly likely that a research project is delayed or even suspended until a replacement can be found when an R&D person separates from the employer. Because highly skilled workers and employees are difficult to substitute – and this becomes even more difficult when we consider firm-specific knowledge required in in-house R&D projects – the separation of a knowledge worker can cause significant delays in the entire R&D processes (Campbell, Coff, and Kryscynski, 2012; Wang, He, and Mahoney, 2009). Therefore, with higher mobility of workers, many R&D projects are delayed, if not abandoned, and some R&D projects that could have been initiated do not get approved.

Third, the cost side of R&D investment increases. During the R&D processes, workers get access to propriety assets of firms, and they also gain knowledge and experience. As such, those researchers or inventors participating in the R&D projects gains more bargaining power, which increases the cost of retaining such workers (Emerson, 1962; Pfeffer and Salancik, 2003). This situation is exacerbated when workers become more mobile and have a larger set of outside options. The bargaining power held by an employee is contingent upon the level of friction in the labor market (Starr, Frake, and Agarwal, 2017), and as workers have more job opportunities, it becomes more costly to retain the workers. On the other hand, if workers are not mobile (e.g., strongly bound by non-compete agreements), firms can leverage this friction and easily retain the worker without much cost, even if the worker has valuable knowledge and assets.

The costs and benefits of R&D projects, along with the prospect of smoothly completing them, significantly depends on the mobility of workers, and higher mobility disincentivizes a firm's R&D investment along all three dimensions.

> *Prediction 1: Firms will decrease their R&D investments when facing a higher risk of employee separation.*

3.3.2 Patenting

Firms generally protect their knowledge in two ways: patenting and secrecy (Hall *et al.,* 2014). Firms can either disclose their invention and register patents to obtain a formal protection by patent law ("invention protection") or retain their employees who possess proprietary knowledge ("inventor protection").

Studies have demonstrated that firms use the two appropriability mechanisms as substitutes for the other, depending on industry and technology factors (Anton and Yao, 2004; Arundel, 2001; Png, 2017). There are tradeoffs to each type of protection mechanisms. Most important, invention protection by patents provides effective protection by law for explicit and codifiable knowledge. Under the U.S. patent law, patent assignees are generally granted 20 years of protection for their invention. Patenting, however, entails certain costs and risks. Technology disclosure is among the most crucial risk, and this risk increases if competitors are able to invent around the knowledge to produce similar products (Bessen and Maskin, 2009; Galasso and Schankerman, 2014; Green and Scotchmer, 1995). Registration and maintenance fees and legal uncertainty are additional costs and risks for firms to consider when patenting their knowledge (Kitch 1977; Williams 2013). In addition, tacit knowledge, such as the know-how of inventors, is often difficult to patent by its nature (Cohen *et al.,* 2000; Hegde, 2014).

Secrecy, in contrast, is less time-consuming and less costly to implement (Friedman *et al.* 1991). Protection by secrecy has an advantage in terms of these costs and risks. Non-competes facilitate this approach, as these agreements physically restrict employees from moving to competing firms. Yet, secrecy becomes less effective when firms cannot retain their workers within firm boundaries. Non-competes, for instance, may not be enforceable in all cases, depending on state laws or other circumstances, including a worker's conduct or contract clauses. Firms decide on their protection mechanisms by considering these trade-offs and the expected net benefit of each approach (Anton and Yao, 2004; Png, 2017).

Firms should increase their use of patenting, rather than secrecy, as worker mobility increases due to, for example, a loophole in their non-compete agreements. First, knowledge held by employees becomes more vulnerable to loss under a higher risk of employee mobility. As discussed above, secrecy is most effective when employers can bind employees within their firm boundaries. A loophole in non-competes (or more generally, higher mobility of workers), however, increases the probability of employee turnover, as it unfetters workers who previously had been restricted in their ability to join the current employer's competitors.[28]

In addition, higher mobility of workers forces employers to file a patent under their own name before an employee does it without them. Firms have great incentives to preemptively file a patent (i.e., before former [current] workers who left [will leave] the firm file a patent by themselves or with a new employer) to transfer any innovation outputs that are embedded in human capital to a more formal repository, a patent, owned by the firm. Preemptive patenting is crucial for firms to prevent misappropriation problems and potential patent infringement litigations.

Furthermore, workers can demand higher wages (or other types of compensations) as their bargaining power increases due to the higher mobility. Workers who possess valuable knowledge now have more outside options (e.g., they can now move to firms in California that compete with their current employer), which significantly raises the cost of inventor retention or secrecy. This cost comes in addition to the increased risk of employee separation and subsequent knowledge leakage discussed above. The cost of using patents for knowledge protection, on the other hand, is unaffected, regardless of worker mobility. We therefore predict that firms will rely more on patenting ("invention protection") than on secrecy ("inventor protection") when worker mobility increases – for example, when a loophole in non-compete enforcement emerges.

[28] Even if workers do not intend to move to direct competitors (e.g., career detour), uncertainties in legal blame and potential legal costs deter the turnover of workers. In other words, non-competes tend to decrease the separation of workers even in cases where the non-compete clause does not directly apply.

Prediction 2: Firms will increase their patenting activity when facing a higher risk of employee separation.

3.4 Empirical Strategy

3.4.1 Setting: the Validity of Using the Application vs. Hunter (1998)

We exploit the Application vs. Hunter (1998) and study its externality effects on firms *outside* California. This is an ideal research setting for our research questions, as it provides plausibly exogenous change in non-compete enforceability. First, Application vs. Hunter (1998) was a court decision (as opposed to a policy or legal change) on which an individual or a firm could exert little influence. Second, before the decision, businesspeople and lawyers believed that California did not have the right to refuse to enforce non-competes written between an out-of-state employer and employee and with a choice-of-law provision. Third, we focus on states other than California to further ensure the exogeneity of the decision and the validity of the treatment and control group. California court decisions may be correlated with other business or legal environments in California. It could also be the case that California firms exerted effort (e.g., lobbying) to influence the decision. We circumvent this endogeneity problem by examining firms that do business outside California.

It should be noted that Application vs. Hunter (1998) may not bind all future cases on out-of-state non-competes in California. Different cases are considered differently in courts, and economic agents cannot predict that workers who move to California firms always win the litigation on out-of-state non-compete enforcement. What is important in this case, however, is the employer's *belief* about or *perception* of the enforceability of their non-compete agreement. Before the Application vs. Hunter (1998), non-California employers and their legal counsels believed that their non-compete agreements would be judged in their own court or at least respected in California court. After the decision, employers and lawyers realized that this is most likely not the case and they significantly updated their belief about the probability of worker separation.

3.4.2 Method

We estimate the difference-in-differences model, exploiting the Application vs. Hunter (1998) decision. This unexpected and unprecedented court decision significantly increased the mobility of non-California employees who were bound by non-competes to firms in California. Our focus is *not* on California per se but on all the other states in the U.S. We compare firms in non-compete-enforcing states (treatment group) with those in non-enforcing states (control group) for pre-1998 decision and post-1998 decision periods. The idea is that the Application vs. Hunter (1998) only affected our treatment group by introducing a loophole in their non-compete enforcement: Workers in these states who are bound by non-competes could move freely to California. On the other hand, the decision did not affect our control group because these firms were not enforcing non-competes even before the decision. This method, along with firm and year fixed effects, helps us account for unobservable differences between the two groups. If firms in the control group did, to some extent, enforce non-competes, this would bias our estimates against our findings.

Ordinary Least Squares (OLS)

Our first specification compares firms in high enforcing states (treatment group) to firms in low enforcing states (control group). We use Garmaise (2009) index on non-competes enforceability, which is based on 12 questions regarding the *legal* enforceability of non-competes. For example, if an employer in Maryland can say "Yes" to 6 questions (out of 12) about the legal enforcement of non-competes, then the enforceability of Maryland is 6. Figure 3.1 illustrate the variation in non-competes enforceability across U.S. states. We estimate the following difference-in-differences model with an indicator for being in the high-enforcing states:

$$y_{ist} = Post_t \cdot \mathbf{1}\{Enforce_s > 4\} + \alpha + \delta_i + \gamma_t + \epsilon_{ist} \tag{3.1}$$

where y_{ist} is the natural log transformation of our outcomes of interest. $Post_t$ is an indicator that takes value of unity after 1998, and $1\{Enforce_s > 4\}$ is an indicator that takes value of unity if a state's enforceability score is higher than 4. The remaining terms α, δ_i, and γ_t are intercept, firm fixed effect, and year fixed effect, respectively.

An alternative specification is to use the *raw score* of the Garmaise index ($Enforce_s$) and interact it with $Post_t$, as in Equation (3.2):

$$y_{ist} = Post_t \cdot Enforce_s + \alpha + \delta_i + \gamma_t + \epsilon_{ist} \tag{3.2}$$

We also conduct a more flexible econometric analysis ("event study approach") by replacing $Post_t$ with year indicators (distributed leads and lags) and leaving an indicator for 1997 out as a baseline. This allows the estimates to vary across years. Equation (3.3) estimates the effects *separately* for treatment and control group, enabling us to compare the estimates for the two groups. Equation (3.4) integrates these separate estimations into the difference-in-differences framework. We can explicitly test not only the *parallel trend assumption* for pre-treatment years (1994-1997) but also the patterns of the effects (e.g., one-time adjustment or gradual increase) for post-treatment years (1999-2002).

$$y_{ist} = \sum_{\substack{k=1994,\\ k\neq 1997}}^{2002} \beta_t \cdot 1\{t = k\} + \alpha + \delta_i \tag{3.3}$$

$$y_{ist} = \sum_{\substack{k=1994,\\ k\neq 1997}}^{2002} \mathbf{1}\{Enforce_s > 4\} \cdot 1\{t = k\} + \mathbf{1}\{Enforce_s > 4\} + \alpha + \delta_i + \gamma_t \tag{3.4}$$

We additionally conduct an analysis that allows pre-1998 patenting intensity to affect post-1998 patenting intensity, including interaction terms between each firm's patent applications (in logs) in each pre-1998 year and a full set of year dummies. This absorbs all of the pre-1998 differences in patent applications in our analyses, and some of the post-1998 variation, but makes our post-1998 comparisons close to *ceteris paribus* (Cantoni, Dittmar, and Yuchtman 2018; Kang, 2019). By design, there are no pre-1998 differences in trends between treatment and control groups.

Poisson Quasi-Maximum Likelihood Estimation for Count Variables

Another standard way of estimating a count variable is the Poisson regression model. Compared to the log-linear OLS model, the Poisson model has an advantage in dealing with zero counts. Compared to alternative count models, including the negative binomial model, the Poisson is more robust to distributional misspecification even if the data generating process is mis-specified, as long as the conditional mean is correctly specified (Cameron and Trivedi, 2013; Wooldridge, 2002). The Poisson model, however, relies on an assumption that conditional mean and variance are the same. In many cases, including our data, the variance is larger than the mean. The Poisson Quasi-Maximum Likelihood Estimator (QMLE) relaxes this assumption and estimate the over-dispersion parameter (ϕ) from the data.

The estimation coefficients of the Poisson QMLE are the same as the Poisson model, but the former accounts for the over-dispersion parameter when estimating the standard error, which leads to *larger* standard errors (i.e., standard Poisson model underestimates standard errors in the presence of overdispersion). In addition, standard errors need to be adjusted for clusters in which errors are correlated; otherwise, standard errors tend to overstate estimator precision, leading to absurdly small standard errors (Cameron and Miller, 2015). We conduct the same analyses as we do with the OLS model for patent and present different types of standard errors for comparison.

3.4.3 Data and Sample

Our main data for patent comes from PatentsView which is supported by the Office of Chief Economist in the US Patent & Trademark Office (USPTO). We use the November 27, 2018 update of the PatentsView, accessed on March 1, 2019. We also combine other patent data from NBER, UC Berkeley Fung Institute, and USPTO Public Patent Application Information Retrieval (PAIR) databases to cross-check the quality and collect additional information on patents. These databases provide detailed information on patent application/grant dates, contents, technology class, assignees, inventors, their location, and citations. PatentsView, in particular, provides unique identifiers for assignees and inventors, after disambiguating assignee firm and inventor names. We identify all U.S.-based assignee firms from patents *applied* between 1994 and 2002, four years prior and subsequent to our treatment year (1998).[29] We confine our interest to patents applied by U.S. corporations and do not include assignees that are individuals or government institutions, because these types of assignees are only marginally or indirectly affected by the Application vs. Hunter (1998). We exclude assignees in Florida, Louisiana, and Texas, as these states experienced significant changes in the enforceability of non-competes during our sample period (Garmaise, 2009; Kang and Fleming, 2019). Alaska and Hawaii are omitted to account for geographical barriers that restrict employee mobility. Lastly, we restrict our sample to assignee firms that had at least five unique inventors during 1993-1997. This minimal

[29] We are interested in a firm's strategic response to worker mobility and subsequent risk of knowledge leakage and do not want to count patent review and revision period. As such, we use the date of patent *application* rather than the date of patent grant.

restriction allows us to focus on firms that have at least some inventors to retain. Our final sample consists of 70,155 firms (patent assignees) with 542,728 patent applications.[30] We construct our measure of patenting by 1) counting the number of patents applied by a firm in a given year and 2) weighting the patent counts by future (forward) citations received. In addition, we collect information on R&D expenditure for publicly traded firms in the U.S. from Standard and Poor's Compustat. Table 3.1 provides descriptive statistics for our key variables.

3.5 Results

3.5.1 R&D Investment

We find that firms tend to decrease their R&D investment following the Application vs. Hunter (1998). Regression results in Table 3.2, columns (1) and (2), shows the effects on R&D investment. Column (1), using the enforceability indicator (0 or 1) specified in Equation (3.1), indicates that treated firms decrease their R&D investment by 11.3% although we cannot reject the null hypothesis that the estimated coefficient is equal to zero. Column (2) shows the results based on the raw enforceability score (0 to 7) as in Equation (3.2). Treated firms decrease their R&D investment by about 3.8% as the enforceability index increases by 1 point.

Event-study framework allows for more flexible and detailed estimation. Figure 3.3(a) illustrates the estimation results based on Equation (3.3). We run two separate regressions for the treatment group and the control group, respectively, using one year prior to the decision, 1997, as the baseline year. It allows us to compare the patenting intensity of firms in the treatment group to that of firms in the control group. The y-axis indicates the percentage change in R&D investment compared to that in 1997, and the x-axis indicates year. Red solid line represents the results for the treatment group (states that are above the median in the enforceability score), while yellow dashed line shows the results from for the control group (states that are below the median in the enforceability score). We find a parallel trend until 1998, justifying the difference-in-differences approach and validating our choice of treatment and control groups.

We then integrate the two separate regressions into a single event study framework as in Equation (3.4). Figure 3.3(b) confirms our finding that parallel trend persists until 1998 and that the treatment group increases its R&D investment right after the decision by about 10.4%, compared to the control group. Figure 3.3(c) further deals with pre-trends. We allow pre-1998 patenting intensity to affect post-1998 patenting intensity, including interaction terms between each firm's R&D investment (in logs) in each pre-1998 years and a full set of year dummies. This absorbs *all* of the pre-1998 differences in patent applications in our analyses, and *some* of the post-1998 variation, but makes our post-1998 comparisons close to *ceteris paribus*. By design, there are no pre-1998 differences in trends

[30] Our sample consists of patent applications that are eventually approved and registered. This can be thought of as imposing a minimum bar for the quality of patent applications we analyze.

between treatment and control groups. We again confirm from this very strict specification that the 1998 decision made firms in the treatment group decrease their R&D investment by at least 4% after the decision.

We obtain the consistent results for R&D investment per worker: R&D expenditure (in millions of dollar) divided by the number of workers (in thousands). Taken altogether, we conclude that firms decrease their R&D investment when facing higher mobility of workers.

3.5.2 Patenting Activities

Table 3.3 reports results of differences-in-differences estimation for patenting activities. Column (1) shows the effects on patent applications using enforceability indicator (0 or 1), as specified in Equation (3.1). Firms in the high enforcing states (states that are above the median in the enforceability score) increased their patenting by about 6.6%, compared to the low enforcing states (states that are below the median in the enforceability score), consistent with our Prediction 2. Column (2) shows the results based on the raw enforceability score (0 to 7) as in Equation (3.2). When the enforceability score increases by one point, firms increase their patenting by about 1.8%, after the Application vs. Hunter (1998).

Event-study framework allows for more flexible and detailed estimation. Figure 3.2(a) illustrates the estimation results based on Equation (3.3). We run two separate regressions for the treatment group and the control group, respectively, using one year prior to the decision, 1997, as the baseline year. It allows us to compare the patenting intensity of firms in the treatment group to that of firms in the control group. The y-axis indicates the percentage change in the patent applications (that are eventually issued) compared to the patent applications in 1997, and the x-axis indicates year. Red solid line represents the results for the treatment group (states that are above the median in the enforceability score), while yellow dashed line shows the results from for the control group (states that are below the median in the enforceability score). We find a parallel trend until 1998, justifying the difference-in-differences approach and validating our choice of treatment and control groups. Right after the Application vs. Hunter (1998), firms in the treatment group increase their patent application, compared to those in the control group.

We then integrate the two separate regressions into a single event study framework as in Equation (3.4). Figure 3.2(b) confirms our finding that parallel trend persists until 1998 and that the treatment group increases its patent application right after the decision by about 5.6%, compared to the control group. Figure 3.2(c) further deals with pre-trends, allowing pre-1998 patenting intensity to affect post-1998 patenting intensity, including interaction terms between each firm's patent applications (in logs) in each pre-1998 years and a full set of year dummies. We again confirm from this very strict specification that the 1998 decision made firms in the treatment group increase their patenting intensity by about 7.1% after the decision.

The log transformation of count outcomes is convenient and easy to implement, but does not deal with zeros very well. To check the robustness of our results, we also run the Poisson QMLE. The results are shown in Figure 3.6(a). We present different standard errors for comparison, including non-parametric *clustered* bootstrap standard errors based on 10,000 repetitions. Standard errors based on Poisson and Quasi-Poisson are clearly underestimated (these do not account for correlation within clusters), whereas bootstrapping provides fairly conservative standard errors. It is assuring that we find statistically significant increase in patenting intensity for years after the Application vs. Hunter (1998) across all types of standard errors. In sum, the two approaches – log-linear OLS estimation and the Poisson QMLE – produce similar results, confirming that our findings do not come from methodology choices.

There is a significant amount of variation in the quality of patents. Mere count of patent applications may not capture their quality or impact. Therefore, I adjust the quality of patents by using the citation-weighted patent measures. Studies find that citation-weighted patents are more highly correlated with patent quality or market value than patent counts (Lampe and Moser, 2016; Hall *et al.*, 2005; Trajtenberg, 1990). The results on citation-weighted patents are similar to those on patent counts, as shown in Figure 3.5, Figure 3.6(b), Table 3.3 (columns 3-4), and Table 3.4 (column 4).

The two findings that firms increase their patenting activities with no meaningful changes in R&D investment suggest that the increased patenting intensity does not come from fundamental R&D activities. It rather confirms our prediction that firms strategically patent more to protect their (existing) inventions from their own employees who could now move to competitors. In other words, firms rely more on patenting (or invention protection) to protect their knowledge and innovation output than on secrecy (or inventor protection), facing higher risk of employee separation.

3.6 Discussion and Conclusion

This paper examines firm strategies on knowledge management and innovation against worker mobility that arises from a loophole in non-competes enforcement. We study firm-side strategic responses to worker mobility (or non-competes enforceability), particularly focusing on knowledge protection and innovation mechanisms that are becoming increasingly important yet have little been studied. We take advantage of a milestone decision by California – Application vs. Hunter (1998) – on its non-enforcement of out-of-state non-competes. We find substantial effects on firms *outside* California that had been enforcing non-competes. First, firms increase patenting activities without increasing R&D expenditure. In other words, firms patent strategically to protect their *invention*, moving away from *inventor* protection or secrecy.

There are several points that merit further discussion. First, our findings suggest that firms do patent strategically, given their choice between patenting (invention protection) and secrecy (inventor protection). This "strategic patenting" implies that patent counts, which have widely been used as a proxy for innovation outputs, require more careful investigation, because increased patenting may not necessarily reflect fundamental innovation activities of firms. Furthermore, increased patenting suggests that more information on inventions become publicly available (though protected). This may affect follow-on innovations: for example, firms could avoid duplicate investments on the same technology by looking at each other's patent documents. It would be important and interesting to study the effects of information disclosure through patenting (as opposed to secrecy) on follow-on innovations and economic welfare. In addition, although we examined a firm's choice of "inventor protection" versus "invention protection," the two options may not be *completely* substitutable for each other.[31] If this is the case, a move from one equilibrium to another may incur welfare losses.

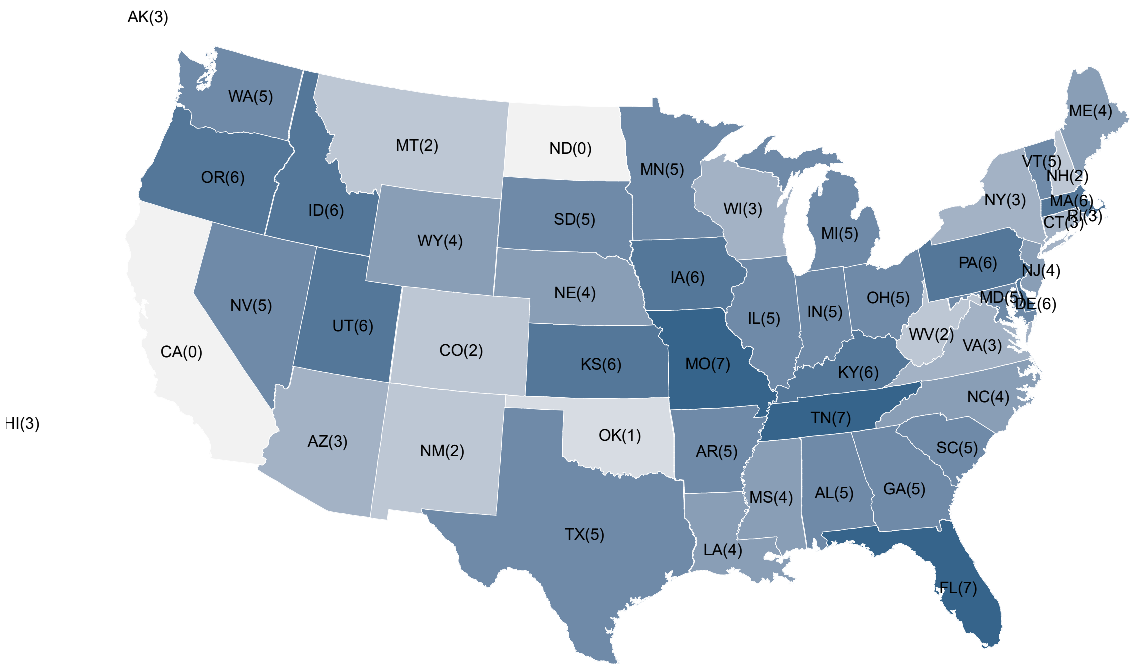
Second, this study focused on *externality* effects of an important legal change. We compare firms *outside* California: non-California firms in states that enforce non-competes (treatment group) and non-California firms in states that hardly enforce non-competes (control group). This approach has both theoretical and methodological contributions. We find that a legal decision in a single state could affect all firms nationwide, highlighting the roles of third parties on firm strategies. Business environments that shape firm strategies are a complex combination of not only local but also broader (even global) policy and legal institutions. Businesspeople and policymakers should carefully consider how policy or legal institution affect regions outside their intended area as well as their own state. Methodologically, this approach mitigates an endogeneity concern, because California's court decision is much less correlated with non-California firms (if any) than with California firms.

Our results are particularly important for small businesses and ventures. These firms face greater misappropriation risk because they need to deal with larger, established competitors that have more resources (Katila, Rosenberger, and Eisenhardt, 2008). They are at a disadvantageous position when holding existing workers or poaching workers form others (Kang and Fleming, 2019). Therefore, employee mobility and knowledge protection should have differential effects on firm birth, growth, and death by firm age/size, possibly through innovation incentives and capabilities. It calls further research on how non-competes and knowledge appropriability affect new businesses (particularly entrepreneurs) and established incumbents differently.

Worker mobility, knowledge protection, and innovation strategies are increasingly receiving attention from business practitioners as well as academia. Policy makers also strive to promote both fair competition towards workers and innovation input/output. For example, many U.S. states and the federal government consider policy changes to promote innovation through fair competition towards workers. We hope that this study contributes to our understanding of a complex relationship between worker mobility, knowledge protection, and innovation strategies. We also look forward that upcoming policy and legislative changes will provide ample research opportunities in this area.

[31] Patents only protect the claims that are explicitly explained in the patent document. In some cases, the patent office does not protect certain inventions and may modify or remove patent claims submitted by applicants. In other cases, it is simply not possible to express an invention in written words. The patent system does not provide perfect protection due to an imperfect monitoring of others' use of the invention; it is sometimes difficult to detect misuse of patented inventions by other entities. Enforcing the exclusive rights that patents are intended to provide is not easy either; there are certain costs (both time and financial resources) and uncertainties over court decisions associated with patent enforcement.

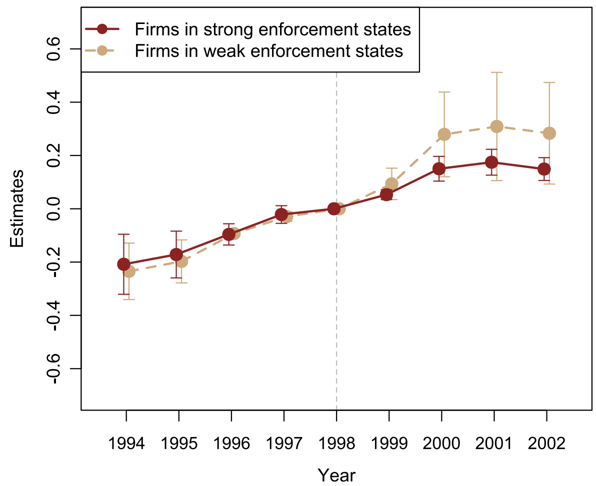
Figure 3.1: Non-competes Enforceability Index



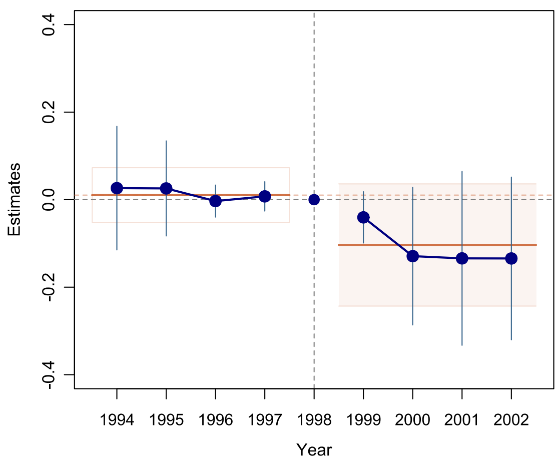
Legal enforceability index (0-7) as of 1994. *Data*: Graphic was prepared by the authors with data from Marlsberger (2002) and Garmaise (2009).

Figure 3.2: Effects of Worker Mobility on R&D Expenditure

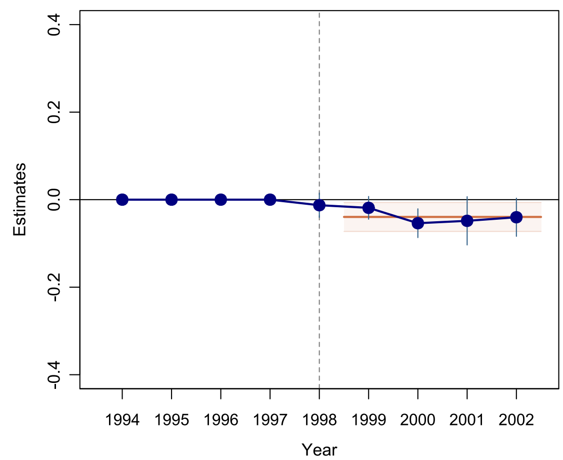
(a) Event study: treatment vs. control group



(b). Flexible difference-in-differences



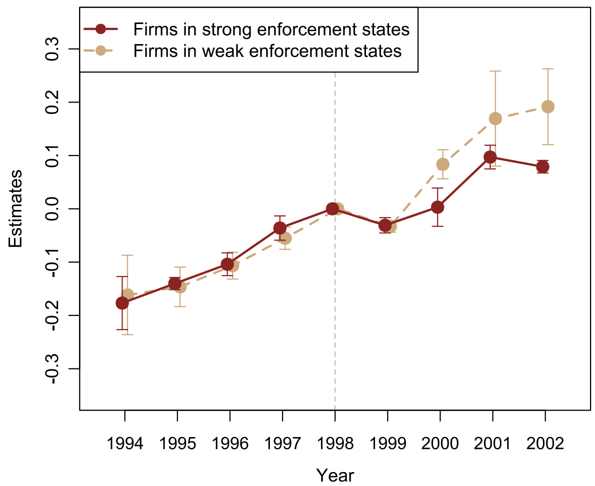
(c). Absorbing pre-treatment year outcomes



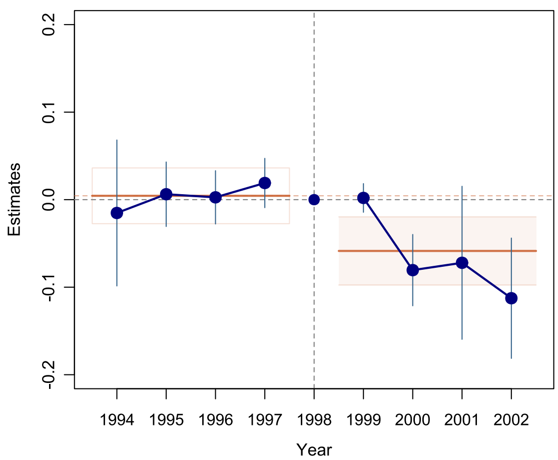
Panel (a): each series is from a separate difference-in-difference regression, as in Equation (3.3). Red solid line represents the estimates for the treatment group (states that are above the median in the enforceability score). Yellow dashed line represents the estimates for the control group (states that are below the median in the enforceability score). *Panel (b)*: This graph shows estimates in Table 3.4, Column (1), which tabulates the results from Equation (3.4). Vertical lines indicate 95% confidence interval. *Panel (c)*: Based on Equation (3.4), we allow pre-1998 patent flows to affect post-1998 patenting outcomes, including interactions between each firm's patent applications (in logs) in each pre-1998 years and a full set of year dummies. This absorbs all of the pre-1998 differences in R&D expenditure in our analyses, and some of the post-1998 variation, but makes our post-1998 comparisons close to ceteris paribus. By design, there are no pre-1998 differences in trends between treatment and control groups. *Data*: Compustat.

Figure 3.3: Effects of Worker Mobility on R&D Expenditure per Worker

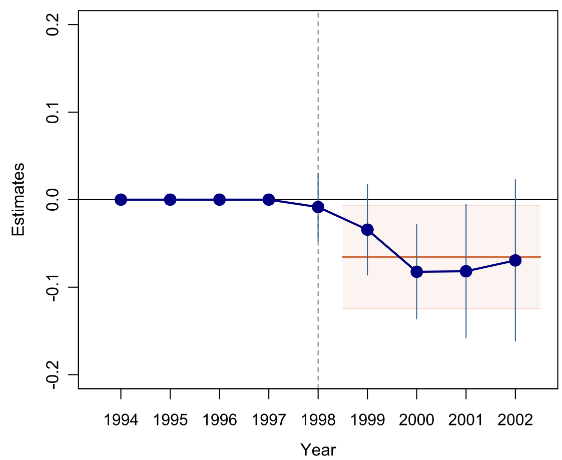
(a) Event study: treatment vs. control group



(b). Flexible difference-in-differences

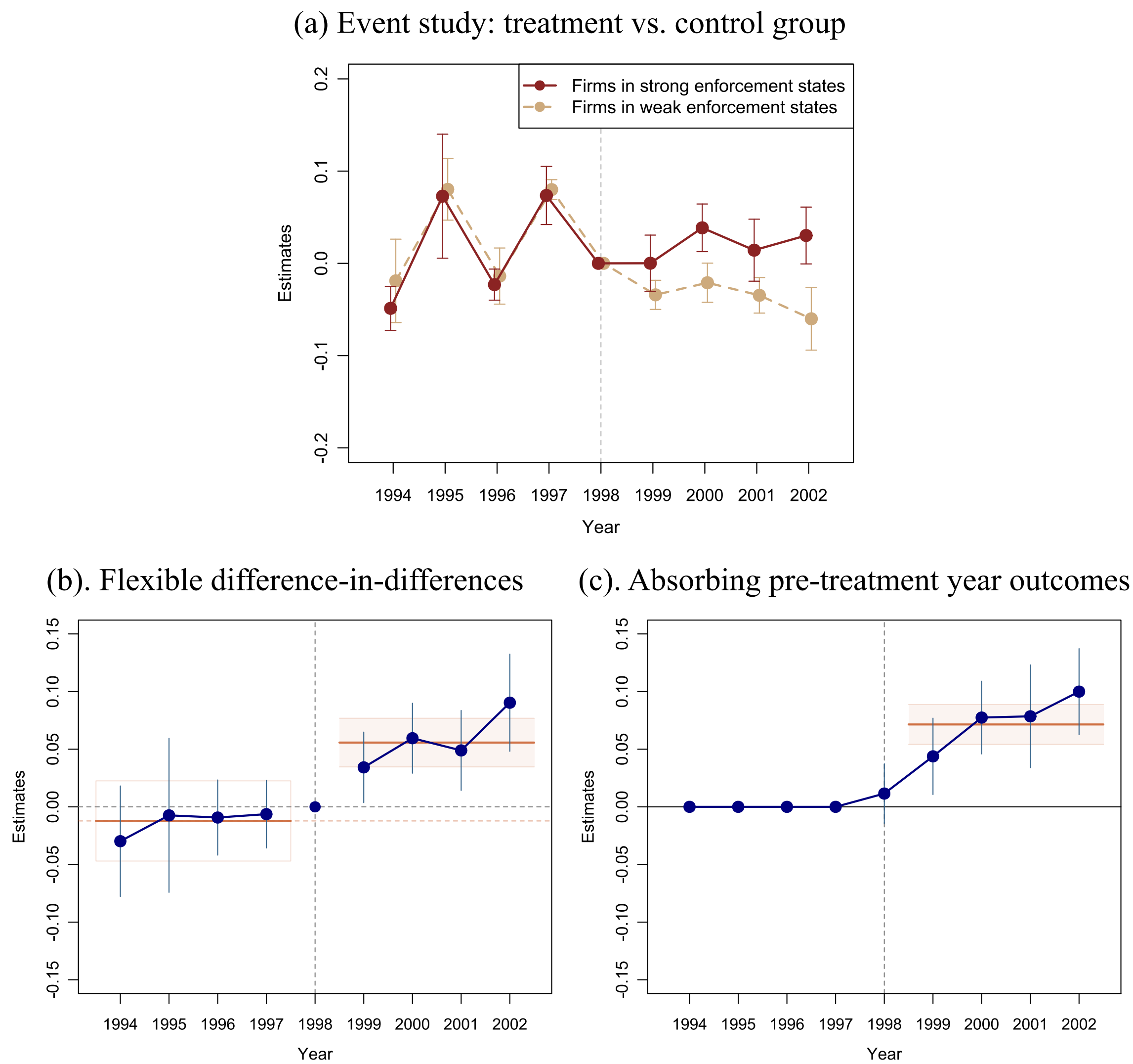


(c). Absorbing pre-treatment year outcomes



Panel (a): each series is from a separate difference-in-difference regression, as in Equation (3.3). Red solid line represents the estimates for the treatment group (states that are above the median in the enforceability score). Yellow dashed line represents the estimates for the control group (states that are below the median in the enforceability score). *Panel (b)*: This graph shows estimates in Table 3.4, Column (2), which tabulates the results from Equation (3.4). Blue dashed lines indicate 95% confidence interval. *Panel (c)*: Based on Equation (3.4), we allow pre-1998 patent flows to affect post-1998 patenting outcomes, including interactions between each firm's patent applications (in logs) in each pre-1998 years and a full set of year dummies. This absorbs all of the pre-1998 differences in R&D expenditure per worker in our analyses, and some of the post-1998 variation, but makes our post-1998 comparisons close to ceteris paribus. By design, there are no pre-1998 differences in trends between treatment and control groups. *Data*: Compustat.

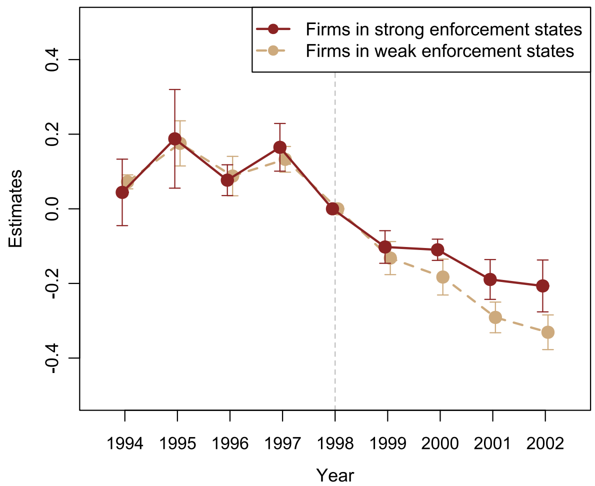
Figure 3.4: Effects of Worker Mobility on Patent Applications



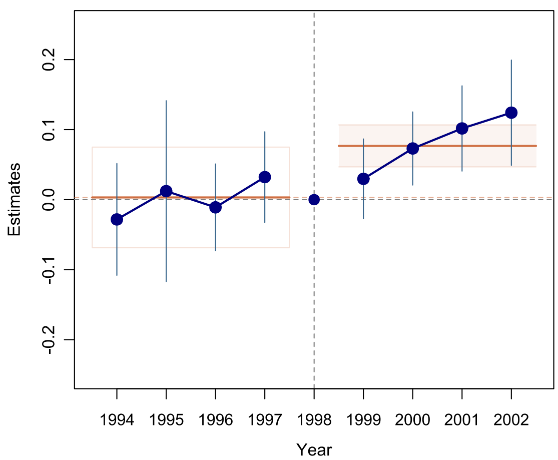
Panel (a): each series is from a separate difference-in-difference regression, as in Equation (3.3). Red real line represents the estimates for the treatment group (states that are above the median in the enforceability score). Yellow dashed line represents the estimates for the control group (states that are below the median in the enforceability score). *Panel (b)*: This graph shows estimates in Table 3.4, Column (3), which tabulates the results from Equation (3.4). Vertical lines indicate 95% confidence interval. *Panel (c)*: Based on Equation (3.4), we allow pre-1998 patent flows to affect post-1998 patenting outcomes, including interactions between each firm's patent applications (in logs) in each pre-1998 years and a full set of year dummies. This absorbs all of the pre-1998 differences in patent applications in our analyses, and some of the post-1998 variation, but makes our post-1998 comparisons close to ceteris paribus. By design, there are no pre-1998 differences in trends between treatment and control groups. *Data*: PatentsView.

Figure 3.5: Effects of Worker Mobility on Citation-weighted Patents

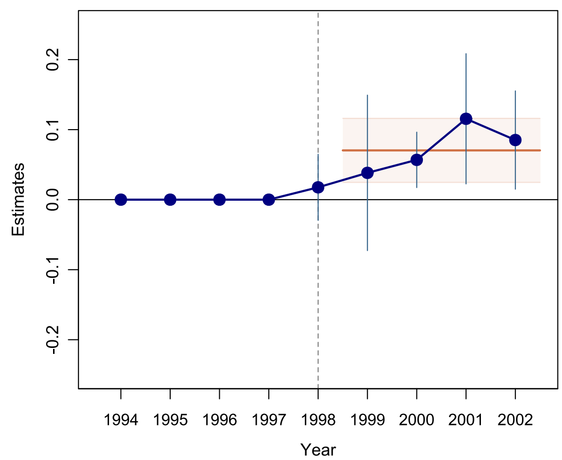
(a) Event study: treatment vs. control group



(b). Flexible difference-in-differences



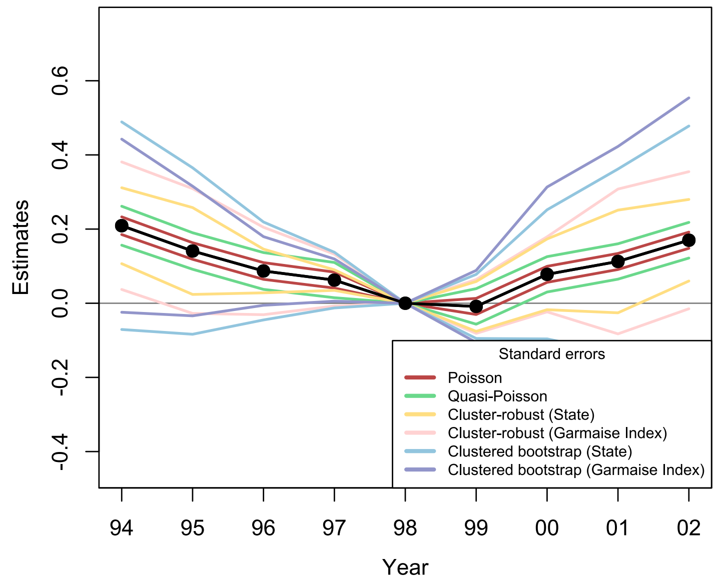
(c). Absorbing pre-treatment year outcomes



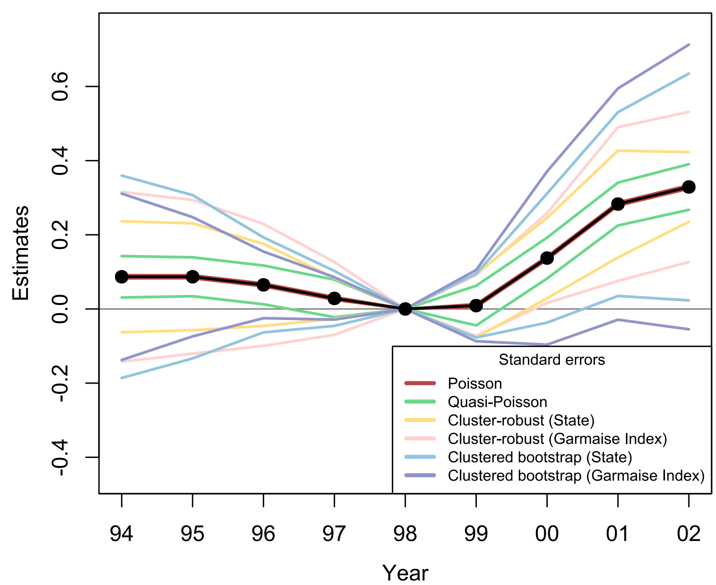
Panel (a): each series is from a separate difference-in-difference regression, as in Equation (3.3). Red real line represents the estimates for the treatment group (states that are above the median in the enforceability score). Yellow dashed line represents the estimates for the control group (states that are below the median in the enforceability score). *Panel (b)*: This graph shows estimates in Table 3.4, Column (3), which tabulates the results from Equation (3.4). Vertical lines indicate 95% confidence interval. *Panel (c)*: Based on Equation (3.4), we allow pre-1998 patent flows to affect post-1998 patenting outcomes, including interactions between each firm's patent applications (in logs) in each pre-1998 years and a full set of year dummies. This absorbs all of the pre-1998 differences in citation-weighted patents in our analyses, and some of the post-1998 variation, but makes our post-1998 comparisons close to ceteris paribus. By design, there are no pre-1998 differences in trends between treatment and control groups. *Data*: PatentsView.

Figure 3.6: Effects of Worker Mobility on Patenting Activities: Poisson Quasi-MLE

(a). Patent applications



(b). Citation-weighted patents



The two figures show the difference-in-differences estimates from the Poisson quasi-maximum likelihood estimation, with six different types of standard errors. The dispersion parameter for the quasi-Poisson family is 4.86 (patent applications) and 182.01 (citation-weighted patents), respectively, suggesting a significant overdispersion in our sample. We provide six different standard errors for comparison. Data: PatentsView.

Table 3.1: Summary Statistics

	N	Mean	Median	S.D.	Min	Max	NA's
Year	–	1998	1998	2.58	1994	2002	–
State enforceability (score)	51	4.31	5	1.74	0	7	–
State enforceability (indicator)	51	0.55	1	0.50	0	1	–
Patent variables							
Patent applications	27,765	19.91	4.00	104.14	0.00	4,436.00	8,865
Patent applications†	27,765	1.96	1.61	1.15	0.00	8.40	8,865
Citation-weighted patents	27,765	111.80	20.00	735.52	0.00	33,508.00	8,865
Citation-weighted patents†	27,765	3.19	3.05	1.49	0.00	10.42	8,865
R&D variables							
R&D expenditure ($million)	38,775	33.52	0.00	246.28	0.00	8,900.00	–
R&D expenditure ($million)†	38,775	1.46	1.06	1.55	0.00	9.09	–
R&D expenditure ($million) per thousand workers	26,285	63.46	14.83	1423.45	0.01	129,741.00	–
R&D expenditure ($million) per thousand workers†	26,285	2.75	2.76	1.41	0.01	11.77	–

†: descriptive statistics based on log-transformation.

Table 3.2: Effects of Worker Mobility on R&D Investment

	Dependent variables:			
	R&D Expenditure (log)		*R&D Expenditure per Worker (log)*	
	Indicator (0,1)	Score (0-7)	Indicator (0,1)	Score (0-7)
	(1)	(2)	(3)	(4)
Post×Enforce	–0.1130	–0.0379***	–0.0634***	–0.0162***
	(0.0856)	(0.0121)	(0.0226)	(0.0027)
Firm F.E.	Y	Y	Y	Y
Year F.E.	Y	Y	Y	Y
Observations	38,775	38,775	26,285	26,285

*p<0.1; **<0.05; ***p<0.01. Regression results from Equation (3.1) ("indicator") and Equation (3.2) ("score"). R&D sample includes firms that have at least one non-zero R&D observation on pre- and post-treatment period, respectively, in Compustat. The variables *Enforce* and *Post* are absorbed in the firm and year fixed effects. Standard errors clustered at the Garmaise enforceability index. *Data*: Compustat.

Table 3.3: Effects of Worker Mobility on Patenting

	Dependent variables:			
	Patent applications (log)		Citation-weighted patents (log)	
	Indicator (0,1)	Score (0-7)	Indicator (0,1)	Score (0-7)
	(3)	(3)	(3)	(4)
Post×Enforce	0.0657***	0.0179***	0.0749**	0.0211**
	(0.0191)	(0.0104)	(0.0251)	(0.0109)
Firm F.E.	Y	Y	Y	Y
Year F.E.	Y	Y	Y	Y
Observations	18,900	18,900	18,900	18,900

*p<0.1; **<0.05; ***p<0.01. Regression results from Equation (3.1) ("indicator") and Equation (3.2) ("score"). Patent sample includes assignee firms that have at least five inventors in 1997. The variables *Enforce* and *Post* are absorbed in the firm and year fixed effects. Standard errors clustered at the Garmaise enforceability index. *Data*: PatentsView.

Table 3.4: Estimates from Flexible Event Study Specification on Main Outcomes

	Dependent variables (logged):			
	R&D		*Patents*	
	R&D investment	R&D investment per worker	Patent applications	Citation-weighted patents
	(1)	(2)	(3)	(4)
Enforce×1994	0.026	–0.015	–0.030	–0.028
	(0.072)	(0.043)	(0.024)	(0.041)
Enforce×1995	0.026	0.006	–0.007	0.012
	(0.055)	(0.019)	(0.034)	(0.066)
Enforce×1996	–0.003	0.003	–0.009	–0.011
	(0.019)	(0.016)	(0.017)	(0.032)
Enforce×1997	0.008	0.019	–0.006	0.032
	(0.017)	(0.014)	(0.015)	(0.033)
Enforce×1999	–0.040	0.002	–0.034**	0.030
	(0.030)	(0.008)	(0.016)	(0.029)
Enforce×2000	–0.129	–0.081***	0.060***	0.073***
	(0.080)	(0.021)	(0.015)	(0.027)
Enforce×2001	–0.134	–0.072	0.049***	0.102***
	(0.101)	(0.045)	(0.018)	(0.031)
Enforce×2002	–0.134	–0.113***	0.090***	0.124***
	(0.0095)	(0.035)	(0.022)	(0.038)
Year F.E.	Y	Y	Y	Y
Firm F.E.	Y	Y	Y	Y
Observations	38,775	26,285	18,900	18,900

*p<0.1; **<0.05; ***p<0.01. Regression results from Equation (3.4) with the same sample as Table 3.2 and Table 3.3. Standard errors clustered at the Garmaise enforceability index. *Data*: Compustat and PatentsView.

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