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Noncompete agreements and inventor mobility – a multi-faceted approach with evidence from California

Master thesis

Abstract

This study researches the relationship between noncompete agreement legislation and inventor mobility by exploiting exogenous additions to labour legislation on a state and national level in the United States. California is the geographic region of interest, as the state is known for having the strictest legislation opposing noncompete agreements (NCAs) in the United States. The dataset of 168,276 individual inventors was divided into several subsamples filtering inventors on different gradients of mobility. I use a difference-in-difference method with year and state fixed effects that exploits the additions to Californian and national labour legislation as the main tool to test whether increased tolerance of noncompete agreements decreases inventor mobility. I find that in certain subsamples inventors affected by the policy changes were significantly less mobile between states, not between firms. The results contribute to the existing literature on noncompete agreements and mobility by providing case study evidence which exploits further changes in Californian labour policy.

Keywords: Noncompete agreements, inventor mobility, California, patents, difference-in-difference

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1 Introduction

1.1 Research question

On July 9th 2021 the President of the United States Joe Biden issued an executive order regarding a lesser known but increasingly important topic in the American economy; it had nothing to do with health care, taxes, corporate bailouts, infrastructure projects or anything similar. On that day, Joe Biden issued an executive order encouraging the Federal Trade Commission to "*curtail the unfair use of noncompete clauses and other clauses or agreements that may unfairly limit worker mobility*."

The issue of noncompete agreements (NCAs) is extremely relevant in the modern American economy. According to research by Colvin and Shierholz (2019), between 36 million and 60 million private-sector workers are subject to noncompete agreements. The White House found that in 2021, approximately 20% of workers must sign an NCA on their first day on the job. This becomes especially problematic when low-wage workers are forced to sign NCAs, as they often are not in the position to turn down a job if they must sign an NCA or negotiate the terms of the agreement. Luckily, according to the American Bar Association (2022), bans on NCAs for low-wage workers are more frequent than bans on NCAs in general. As of June 2022, ten states prohibit NCAs for low-wage workers, while only three states prohibit noncompete agreements in general: Oklahoma, North Dakota and most notably, California.

The state of California has a long history of prohibiting all agreements that may infringe on employee freedom in any way. As will become clear in further chapters, Californian jurisprudence forms a strong precedent to rule against noncompete agreements in all cases whatsoever. California has always been an oddball in that sense, but the state has benefited severely. Research has shown that the increased employee mobility caused by the lack of noncompete agreements is one of the catalysts for the success of Silicon Valley (Gilson, 1999; Saxenian, 1996). The anti-NCA stance of California is documented in Business and Professions Code paragraph 16600, which instates a prohibition against any "*contract by which anyone is restrained from engaging in a lawful profession, trade, or business of any kind*." This provision was formally documented in 1941, before that judges relied on rulings of other Californian judges as precedent.

However, California abruptly switched courses in 2007. Exemptions to California Business and Professions Code paragraph 16600 were unthinkably before 2007, until noncompete agreements in the sale or dissolution of corporations (16601), partnerships (16602), and limited liability corporations (16602.5) were suddenly tolerated. These provisions were added in 2007 and apply to business owners and partners, not employees. From January 1st 2007, business owners could sign a carefully designed noncompete agreements with each other, which would uphold in court in case of a dispute. For example, if two bakers opened a bakery together in California in 2008, signed an agreement that in case the business failed they would not open up new bakeries within 20 miles from each other and one of the partners violated the agreement, then the noncompete agreement would uphold in court due to section 16602 of the California Business and Professions Code.

Two other policy changes will be researched in this study as well. In contrast to state legislation in California, the impact of *The Defend Trade Secrets Act*, a federal U.S. law allowing noncompete agreements in case of trade secrets, will be analysed on Californian inventors. Lastly, the installation of section 925(e) in the Californian Labour Code, allowing the use of choice-of-law provisions in selective cases, will be used to test if choice-of-law provisions impact inventor mobility.

In light of the introduction above, this thesis researches the following question:

Do changes in Californian state and national legislation that increase tolerance of noncompete agreements decrease labour mobility amongst Californian inventors?

Economic research has shown that the use of noncompete agreements reduces mobility among inventors, meaning high-skilled workers with registered patents to their name (Johnson, Lavetti and Lipsitz, 2019; Lipsitz and Starr, 2019 & Gurun, Stoffman, and Yonker, 2019). I am interested in whether relatively small changes in state and national law regarding increased tolerance of NCAs can have large, possibly negative, effects on inventor mobility.

To uncover the effects of the policy changes listed above and inventor mobility, I follow Marx, Strumsky and Fleming's (2009) paper on the Michigan experiment. In this paper, the researchers also exploit an exogenous shift in legislation, the 1985 ban of noncompete agreements in Michigan. They find that inventor mobility increased significantly in Michigan compared to other nonenforcing states. I will research if this relationship holds in the opposite direction, namely whether subtle changes of law increase tolerance towards NCAs decrease inventor mobility.

I will investigate this using a cross-sectional dataset of 168,276 inventors in the United States with at least two patents between 2000 and 2021. I divide this main sample into eight other subsamples to thoroughly test the impact on inventor mobility. A difference-in-difference with state and year fixed effects estimation technique, exploiting the instalment of legislation on the Californian and national level quasi-naturally creating a treatment and control group, will measure what would have happened in California in absence of the intervention compared to the rest of the U.S. In essence, this thesis addresses the question of whether increased legislative tolerance of noncompete agreements decreases inventor mobility or if these changes in the law are too subtle to actually impact an inventor's decision to switch states or firms.

This topic is highly relevant in economic literature. McAdams (2020) concludes in his paper that exploiting further policy changes would be useful to sharpen the empirical aspect of noncompete literature. An example he gives is changes at the state law level, which is exactly what this thesis looks at. Also, as the history of NCAs shows, which will be touched upon in the following chapter, the sentiment toward restrictive covenants shifted over the centuries along with other major socio-economic changes such as the Industrial Revolution and the rise of the Information Age. Therefore, NCA legislation is very non-predictable, making it thus the more interesting to study. On top of the unpredictability, changes in NCA legislation have serious consequences for employees, employers, innovation, labour mobility and economic growth, ensuring the topic's economic significance and importance for policymakers. As Marx & Fleming (2012) state: "policy makers' decisions whether or not to enforce noncompetes should be driven by the extent to which they want to optimize for the preservation of established firms versus the flexibility of individual employees."

1.2 Preview of the results

Before discussing a preview of the most important results, it is important to note that difference-in-difference estimates for a certain subsample are only unbiased when pre-treatment trends are equal between both groups. Therefore, a significant coefficient does not automatically indicate an unbiased and significant effect on inventor mobility. If pre-treatment trends are equal and a coefficient is statistically significant, then conclusions can be drawn from regressions.

With that being said, a difference-in-difference regression with 49 states as a control group to California reveals that in a sample with inventors that have proven to be mobile between firms, the 2007 addition to the California Business and Professions Code leads to a significant reduction in between-state mobility. This result is especially interesting in light of an upward trend in inventor mobility after 2007. This upward trend does not hold for inventors with patents in California after 2007. A similar regression with 9 states that are comparable to California regarding anti-NCA legislation reveals similar results with a sample of inventors with at least two patents.

I also find that the enactment of the Defend Trade Secrets Act in May 2016 led to a discontinuity in the number of states inventors from all states patented in. A national upward inventor mobility trend suddenly switched downwards in 2017. This trend is the opposite for Californian inventors; in 2017 they were more mobile between states than in earlier years. Lastly, I that inventor mobility decreased in California due to the installation of section 925(e) in the Californian Labour Code, allowing choice-of-law provisions under strict circumstances, measured in number of states an inventor patents in. This finding indicates that choice-of-law provisions are a tool to circumvent anti-NCA legislation.

This study is structured as follows. First, a theoretical framework is constructed which discusses the most relevant economic findings regarding noncompete agreements, inventor mobility and choice-of-law provisions. This will be supported by jurisprudence regarding the most important rulings on noncompete agreements. The three hypotheses in this study are incorporated in this section as well. After that the dataset, the sample and the main variables will be discussed, followed by an explanation of the econometric methodology used to study the relationship between noncompete agreement legislation and inventor mobility. Next, the main results will be interpreted, followed by conclusions for the three hypotheses. This study will be concluded with a discussion of the results, containing limitations, contributions to economic research and suggestions for future research.

2 Theoretical framework

2.1 Noncompetes in the U.S. and California timeline and background

The legal historian Harlan Blake (1960) observed in his article on the history of post-employment noncompete agreements that the history of these contracts reflects "*the evolution of industrial technology and business methods, as well as the ebb and flow of such social values as freedom of contract, personal economic freedom, and business ethics.*" This quote portrays the variability of noncompete laws through the development of modern society. The consequence of this ebb and flow is the differentiating effect legislation on this topic has on social architecture, specifically the relationship between employer and employee.

The earliest examples of noncompete agreements date back to 16th century England. In this period an apprentice would serve a master craftsman for a period of 7 years, after which the apprentice was free to join the guild and practice the craft however he or she willed. Interference with apprentices' right to enter the guild was deemed a restraint of trade and seen as a serious violation of public policy. If a master withheld his apprentice's entry into the guild by lengthening the apprenticeship period or asked the apprentice not to practice in town, common law held that their agreement was void as a restraint of trade without further consideration (Blake, 1960).

English courts, the precursor of American courts, viewed any such an agreement as a trade restriction, thus rendered void. However, a court ruling in 1711 marked a switch in public policy, a footnote that is relevant until this day. In the case *Mitchel v. Reynolds* (1711), Reynolds rented his bakery to Mitchel for a period of five years, along with an agreement that Reynolds would not compete with Mitchel in the baking industry in those five years in the same town. It turned out that Reynolds in fact did open another bakery in the same town and the court ruled that the noncompete covenant was valid, thus essentially restricting trade for Reynolds.

The court concluded that: "*the refusal to enforce a reasonable restraint accompanying the transfer of a business would result in unnecessary hardship on the buyer of the business*." This decision marked the first exemption in NCA policy, allowing some form of noncompete agreement for business owners, *not employees*. The strictness against NCAs originated from two cases in the 15th and two cases in the 17th century. However, these cases all involved apprentices and unethical behaviour of their masters, indicating that restraints incident to the transfer of business interests have always been held valid if reasonably tailored to the scope of the transaction; only employee NCAs were originally held invalid due to the customary rules of apprenticeship. Still, *Mitchel v. Reynolds* uses these four cases as a precedent and applies them to business owners.

This ruling establishes the concept of *reasonable restraints of trade*, which is still relevant in modern court rulings. It is based on three pillars: duration, territory, and scope of activity prohibited (Packer & Cleary, 2006). The principle of reasonable restraints of trade, later called the *rule of reason test* or *reasonableness test*, was used later in *Addyston Pipe & Steel Co. v. United States* in 1899 by William Howard Taft, who later became president of the United States.

After the American independence in 1776, U.S. courts initially followed the strict anti-noncompete laws established by the English, with the exemption of instances where reasonable restraints of trade for business owners was applicable, such as in *Mitchel v. Reynolds*. A notable example of such an exemption occurred in 1874 during the Industrial Revolution. At the time U.S. courts began to rule that reasonably tailored NCAs could still be enforced. In *Oregon Steam Navigation Company v. Winsor* (1874) , the California Steam Navigation Company sold one of their steamships to the Oregon Steam Navigation Company, provided the latter company would agree that the steamship would not be used in California waters for a period of ten years. Three years after the initial sale, the ship was sold to Winsor, provided that Winsor would not navigate California and Oregon waters. Winsor proceeded to sail the ship in California waters, opiniated that the agreement not to navigate California waters did not apply to them. The Supreme Court of Washington ruled that the contract is *divisible*, meaning that some parts can be void while others are not. In the end, the judge ruled that an agreement that operates merely in partial restraint of trade is legal, provided it is not unreasonable and there is a consideration to support it. This marks an important moment, as partial and reasonable restraints on trade were not permitted in the United States until now.

From that point on, NCAs became more common. The focus of courts shifted to balancing freedom of the market against freedom of contract when an employee entered into a contract limiting his economic mobility. Even restrictive covenants for employees became enforceable in the 20th century if the agreement was deemed reasonable by the rule of reason test. This shift was due to the Industrial Revolution and the subsequent shift in employer-employee relationships. In line with Blake's theory of ebb and flow (1960), advancing ideologies such as economic liberalism, industrial and technological changes, improved geographical mobility and increasing specialization of factory work all resulted in the 1853 ruling that reversed the traditional rule that all restraints of trade are invalid. This shifted, pro-NCA view, prevailed in England until 1913 and is in part responsible for the fact that during the latter part of the nineteenth century in England virtually all such covenants, in employment contracts or elsewhere, were upheld. Similar trends occurred in the United States. NCAs were increasingly being used due to the increased use of confidential information and trade secrets. (Lee, 2019).

Developments in the United States from 2000 - present

The ebb and flow theory is still valid. In the information age it has become common to switch employers frequently. In addition, trade secrets have begun to play a large role in generating competitive advantages for firms. NCAs are a tool to prevent these trade secrets from leaking to competitors. Moreover, firms have begun to use employees with valuable knowledge at far more positions than in the past. Another trend was uncovered by Stone and Arthurs (2002). They argue that the psychological contract that insured long-lasting job security has been replaced by a new one in which employers provide employees with opportunities for training, networking, and building general human capital instead of job security. This creates *employability* security, rather than *employment* security. As a result, moving between jobs has become normal and NCAs limit labour mobility, thus generating backlash in certain states. As U.S. states set their own labour regulations, a central issue that still remains is how states individually formulate the reasonableness test, if they permit the use of noncompete agreements at all and how they balance the upsides and downsides of NCAs.

Nowadays, 47 states in the U.S. permit the use of noncompetes, albeit to a greater or lesser extent, as deemed to be necessary to protect companies from certain types of unfair competition such as uncovering trade secrets, as mentioned earlier. North Dakota (since 1865), Oklahoma (since 1890) and especially California (since 1872) have banned employee noncompetes in any form whatsoever. Montana legislators have spoken out against NCAs, although a ban has not been instated, and Michigan had a ban in place between 1905 and 1985. Colorado, Georgia, Delaware, Montana, and Nevada now claim that noncompete agreements are not enforceable, except with respect to employees in managerial or key positions. Nebraska, Virginia, Arkansas and Wisconsin claim that a noncompete should be voided if it contains unreasonable restrictions on length and geographic range that violate the law. Oregon, Hawaii, Utah, and New Mexico moderately limit how long noncompetes can enforced, and/or the geographic range of the limitation (Stanberry, 2022).

In the period between 2007-2013, several states began revaluating their noncompete laws, thus triggering other states to do the same. Georgia added a requirement of advance notice to the employee that a noncompete will be required, a minimum compensation threshold and the ability to use noncompetes for employees below the wage threshold by paying them a specified amount during the noncompete period. Massachusetts followed suit in 2009 with efforts to add similar amendments to their own state legislation. Up until 2021, 24 states have made changes to their noncompete laws, although it has never come to a ban. However, Washington D.C is preparing for a vote on a ban on NCAs in 2022. In 2021, 66 noncompete bills were pending in 25 different states. This indicates a nationwide shift of sentiment towards labour mobility-restricting legislation. (Beck, 2021)

NCAs have also been revaluated on a national scale. A sandwich shop chain, Jimmy John's, sparked controversy in 2014 when it was uncovered that their sandwich makers were required to sign NCAs. This example of employee abuse, combined with the increasing body of research on NCAs by economists such as Matt Marx, Evan Starr and J.J. Prescott and legislative efforts of states such as Georgia and Massachusetts, prompted further legislative efforts in 2015.

According to the American Bar Association (2022), bans on NCAs for low-wage workers are more frequent than bans on NCAs in general. As of June 2022, ten states prohibit NCAs for low-wage workers. Unfortunately, minimum wage regulation is subject to interstate differences, just as NCA regulation since 1811, meaning that the definition of low-wage workers differs per state. This makes it increasingly difficult to battle NCAs for low wage workers on a national scale.

Several federal bills such as the Freedom for Workers to Seek Opportunity Act were filed, but none were eventually signed into law. The current summum of federal legislation is an executive order issued by president Joe Biden on July 9, 2021, encouraging the Federal Trade Commission to "*to curtail the unfair use of non-compete clauses and other clauses or agreements that may unfairly limit worker mobility*" (The White House, 2016; U.S. Department of Treasury, 2016).

To conclude, noncompete agreements were initially strictly prohibited under all circumstances. After that, more and more exemptions were made until NCAs were widely used and permitted during the Industrial Revolution up until the Information Age, in which trade secrets were deemed valuable and worth protecting. The current sentiment is shifting towards anti-NCA regulation, as they have negative economic effects, which will be discussed further on.

California

In the intricate web of noncompete legislation, California deserves individual acknowledgement. While noncompete agreements have been a subject of debate in the United States since the 1800s, California has traditionally been the state with the most strict anti-NCA regulation in place. California is comparable to 17th century England in this aspect. Open competition and employee mobility for employees is the top priority. This brings along several issues, on which will be touched upon later in this chapter.

Under common law, as is still true in many states today, contractual restraints on the practice of a profession, business, or trade, were considered valid, as long as they were reasonably imposed. This was true even in California. However, in 1872 California rejected the reasonableness test. Today in California all covenants not to compete are void. However, this rule is subject to several exceptions which will be discussed further on. To be precise, Business and Professions Code paragraph 16600 instates a prohibition against any "*contract by which anyone is restrained from engaging in a lawful profession, trade, or business of any kind*." This provision has been in place since 1941.

An excellent example of how courts adhere to this strict piece of legislation is *Edwards v. Arthur Andersen LLP* (2008). In this case Edwards signed an NCA when he started a job at Arthur Andersen in 1997. After the firm was indicted, Edwards received an offer from HSBC, a New York-based bank. In order to start his new job, he must first sign a Termination of Noncompete Agreement (TONC), which is essentially a new NCA applicable to the person at his new job regarding trade secrets and placed Edwards personally in the crosshairs of potential future indictment. He refused to sign the TONC in light of being restrained to function in a new position, resulting in Edwards losing his new position at HSBC and being fired from Andersen. Edwards called upon section 16600, claiming that his employee mobility was restrained.

Andersen asserts the term "restrain" under section 16600 should be interpreted to mean simply to "prohibit," so that only contracts that totally prohibit an employee from engaging in his or her profession, trade or business are illegal. It would then follow that a mere limitation on an employee's ability to practice his or her profession would be allowed under section 16600, when tested by the reasonableness test. The court eventually ruled in an appeal that the initial NCA was invalid under section 16600. Also, requiring Edwards to sign the TONC as a condition to be released from an agreement that was invalid in the first place was in violation of public policy, because the NCA restrained his ability to practice his profession.. Therefore, this ruling displays that California courts can adhere to section 16600 in a very strict manner if they see fit.

The main precedent used in the Edwards case that eventually led to Edwards winning the case was *Metro Traffic Control, Inc. v. Shadow Traffic Network* (1994). Metro Traffic Control wanted to restrain Shadow Traffic Network from soliciting Metro's employees to violate the noncompete and trade secret clauses of their employment contracts. The court quickly affirmed that the clauses are unenforceable, even if there is no evidence that Metro possesses protectable trade secrets. The main conclusion from the Metro case that was used in the eventual ruling in the Edwards case is: "that every citizen shall retain the right to pursue any lawful employment and enterprise of their choice".

Section 16600 does not always have to be interpreted as strictly as in the appeal of the Edwards case. A year later, in *Comedy Club, Inc. v. Improv West Associates* (2009), California courts applied the *Ninth Circuit's narrow-restraint exception,* which is essentially a reasonableness test. This exception flowed from *Campbell v. Trustees of Leland Stanford Jr. Univ.* (1987), in which NCAs where one is restrained from pursuing only a small or limited part of the business, trade or profession are accepted. The Comedy Club case is the opposite of *Edwards v. Anderson*, because of the lenient interpretation of section 16600. The courts followed a previous decision*, Dayton Time Lock Service, Inc. v. The Silent Watchman* (1975), which rejected the absolute illegality of a noncompete agreement. Instead, the court employed a reasonableness test type of analysis to uphold the covenant under investigation. A reasonableness test was also applied in *South Bay Radiology Medical Associates v. Asher* (1990). Comedy Club entered a trademark agreement with Improv West Associates to open comedy clubs in the lower 48 states. The agreement included a provision prohibiting Comedy Club from opening any non-Improv comedy clubs in the lower 48 states for 19 years. Instead of throwing out the noncompete agreement immediately, the court ruled that by applying the reasonableness test the NCA could hold, but with weaker requirements. This less strict stream of rulings on NCAs prohibits only broad agreements that prevent a person from engaging entirely in his chosen business, trade or profession. Agreements that only regulate some aspects of post-employment conduct, are not within the scope of section 16600. Essentially, these are examples of relaxation of California's strict anti-NCA policy.

Nowadays, courts vary in how strict they interpret section 16600. On the one hand, all forms of profession-restraining covenants can be deemed void, as done in the Edwards case, but on the other hand, narrow-restraint exceptions can be made, as done in the Comedy Club case. Also, sections 16601, 16602 and 16602.5 of the California Business and Professions Code created certain exceptions to section 16600 in 2007, namely noncompetition agreements in the sale or dissolution of corporations (16601), partnerships (16602) and limited liability corporations (16602.5). These provisions were added in 2007 and apply to business owners and partners, not employees. A third exception can be made when the NCA pertains to trade secrets. As *Muggill v. Reuben H. Donnelley Corporation* (1965) concludes, noncompete agreements are void in California, unless they are necessary to protect the employer's trade secrets. It is up to the employer to prove that the employee disposes of those trade secrets and that the firm will be severely damaged if the trade secrets become public. This has been formalized on a national level in *The Defend Trade Secrets Act* (DTSA) of May 2016. Also, in this 1965 case a narrowly tailored agreement was ruled to be an improper restraint under section 16600, because it required a former employee to forfeit his pension rights upon commencing work for a competitor. This ruling remains relevant to this day, which indicates how long the anti-NCA sentiment has prevailed in California. However, this sentiment may not be viable in modern California due to the (often negative) economic effects of noncompetes. In the following section the most relevant findings in the field of economic research on NCAs will be touched upon.

2.2 Economic research on noncompete agreements

2.2.1 Introduction

The dynamics between noncompete agreements and economic variables such as innovation, labour mobility and employment have yet to be perfectly balanced. Areas with strict anti-noncompete laws, such as California, show evidence of more patent registrations and newly founded firms (Samila & Sorensen, 2011). On the flip side, research has shown that in areas where NCAs are permitted there are more investments in research and development (R&D) (Conti, 2014). NCAs potentially solve an investment hold-up problem, allowing firms to make mutually beneficial investments in workers. On the other hand, the agreements potentially erode workers' future bargaining position by limiting labour mobility. NCA legislation is an effective tool for a government to use, as the economic benefits can be large compared to the effort of passing a bill. This section will discuss the most important findings in economic literature regarding the effects of noncompete agreements and lay the groundwork for the hypotheses of this study.

To simplify the matter of NCA enforcement, the level of NCA regulation will be measured by the index proposed by Garmaise (2011): the *noncompetition enforceability index* (*NEI*). The higher the NEI-score, the easier for employers and courts to enforce NCAs within a state. For example, California would have a low score due to strict anti-NCA regulation. On the other hand, having no NCA regulation would lead to a high score. As a rule of thumb: a high score means that NCAs are permitted and a low score means that NCAs are not permitted.

Variations in the NEI-score can have large consequences. Research by Gilson (1999) and Saxenian (1996) has shown that different NEI-scores impact regional economic success. Gilson (1999) examines the relationship between the development of high-tech industrial districts and legal infrastructure by comparing the NCA regimes of California's Silicon Valley and Massachusetts's Route 128. In terms of legal regime, the main difference between these high-tech development areas is the enforceability of NCAs. He finds that laws that impact employee mobility influence the dynamics of high-tech industrial districts by either encouraging rapid employee movement between employers and start-ups, as in Silicon Valley, or discouraging such movement, as in Route 128. Because California does not enforce NCAs, firms in Silicon Valley gain from knowledge spillovers between firms. These knowledge spillovers have allowed Silicon Valley firms to thrive while Route 128 firms have deteriorated. He concludes that Business and Professions Code section 16600 has been an initial condition of the strategic dynamic set in place by the growth of Silicon Valley.

Still, Silicon Valley's success cannot be completely attributed to NCA legislation, even though the findings suggest that it does play a significant role. Although it may not be all-encompassing, this paper does lead to an important notion: knowledge is transferred between firms by the movement of employees between employers and start-ups (Saxenian, 1996). This relationship especially holds for knowledge-intensive industries. A culture of high labour mobility and a bias against vertical firm integration benefits innovation more than a culture of career-long employment.

2.2.2 Noncompete agreements and mobility of human capital

The relationship between noncompete agreements and mobility has received attention in previous years. Most recently, The Biden Administration issued an Executive Order in July 2021, urging the Federal Trade Commission to consider banning most noncompetes; especially those acting as a restriction on job mobility and wage growth for low-wage workers. According to the Federal Reserve Bank of Minneapolis (2021), NCAs are pervasive throughout the labour market and their use restricts workers' access to job opportunities, which can have severe economic consequences.

Marx (2011) establishes the notion that firms have the power to shape career paths and labour markets by using NCAs. He uses data that shows that nearly half of technical professionals active in the United States are asked to sign an NCA. The findings indicate that these workers subject to NCAs are more likely to leave their technological field if they choose to leave their firm in order to prevent lawsuits. Firms do offer something for restricting employee mobility: higher wages, but only for top executives. Kini, Williams and Yin (2018) find that covenants that restrict mobility lead to more favorable compensation structures for CEOs. This is the opposite for the average American worker. Evidence found by Colvin and Shierholz (2019) indicates that NCAs reduce pay for lower-wage workers because they act to deter workers from leaving their job, especially when changing jobs is how many workers get a raise. Lipsitz and Starr (2019) estimate that Oregon's ban on non-competes in 2008 led to a 2.2 to 3.1% increase in average wages for low-wage (hourly) workers compared to several control groups. Stanberry (2022) suggests that if the federal ban President Biden is speaking of actually is instated, this would have a positive effect on wages for low-skilled workers, for example in cases such as the Jimmy John's case mentioned earlier.

Noncompete agreements are a tool firms use to retain valuable knowledge and human capital, thus solving a hold-up problem. Common examples of investments likely to be subject to hold-up in this context include non-tangible assets such as training, information such as trade secrets and client lists. Because of NCAs, firms will now do these mutually beneficial investments that they otherwise may not decide to do. If workers leave the firm, the advantages of the investment for the firm will disappear along with them (Rubin & Shedd, 1981). Starr (2019) estimates that moving a state from non-enforcement to average enforcement would increase the incidence of worker training by 18%. Johnson and Lipsitz (2017) find that NCA use is associated with a 14% higher likelihood of firms providing on-the-job training among hair stylists. Increased investments in human capital is a positive effect of NCAs for firms and employees. However, Gilson (1999) coins the following question: if NCAs are so necessary to protect intellectual property after investing in human capital, why did Novartis AD invest 250 million in the development of intellectual knowledge in California, a state that is known for its lack of NCA enforcement?

Still, this advantage disappears when taking labour mobility into account. According to Garmaise (2011), NCAs have potentially offsetting effects on investments in employee training. Reducing hold-up tends to increase the incentive for firm-sponsored training. However, limiting an employee's outside options of employment will tend to decrease their incentive to invest in personal skills. Thus, the net impact on human capital accumulation is theoretically ambiguous. Also, NCAs can harm firms as well. The caused reduction in worker mobility will tend to increase recruitment costs for all firms as the pool of potential applicants will shrink. Findings suggest that switching jobs account for a third of early career wage growth, so restricting mobility will lead to a decrease in worker morale or a sharp spike in wages to prevent this. As mentioned earlier, an increase in wages does not occur for the majority of (low-wage) American workers, so a decrease in worker morale is inevitable (Topel & Ward, 1992).

Job mobility is essential for a well-functioning market economy and for individual workers to boost their wages. Labour market fluidity ensures that employees can move freely from low to high productive firms, thereby increasing aggregate productivity, income and wellbeing prospects (Decker et al., 2017; Foster et al., 2016). Job-to-job flows tend to be the main mechanism for workers to make career progress and to increase their earnings (Hyatt et al., 2018; Haltiwanger et al., 2016). Likewise, high mobility ensures that firms experiencing a positive productivity shock can get the workers they need to expand. The literature further reveals that hiring a key inventor from another firm can lead to knowledge transfer (Song et al. 2003). In particular, the transfer of tacit knowledge, which is otherwise immobile, is facilitated by inventor mobility (Dosi, 1988).

Research has shown that NCAs reduce mobility. Johnson, Lavetti and Lipsitz (2019) find that moving from a policy of NCA unenforceability to the highest enforceability observed across U.S. states is predicted to reduce the monthly probability of workers changing employers by 26.1%. Similarly, for low-wage workers, Lipsitz and Starr (2019) show that Oregon’s ban on enforcing NCAs led to an increase in job mobility across firms of 12.2 to 18.3%. Inventors in Michigan were 8.1% less likely to switch jobs after Michigan strengthened its enforcement of NCAs in the mid-1980s, with even lower switching rates among those with firm-specific and technological expertise (Marx, Strumsky, and

Fleming, 2009). Hawaii's ban on NCAs for technology workers led to an 11% increase in mobility, relative to comparable workers in other states, in the years after the ban (Balasubramanian et al., 2018). Top executives were substantially (47%) less likely to change jobs within industries as noncompetes became more strictly enforced and their tenure increased by 16% (Garmaise, 2011). Financial advisers are substantially more likely to switch firms when NCAs are not enforced against them (Gurun, Stoffman, and Yonker, 2019). Marx, Singh and Fleming (2015) find that noncompete agreements are responsible for a brain drain of knowledge workers out of states that enforce such contracts to states where they are not enforceable.

Noncompete agreements also lead to longer tenures at companies, evidently a by-product of reduced labour mobility. Workers in enforcing states have had 8% fewer jobs than similar workers in non-enforcing states (Balasubramanian et al. 2018). Workers in states with stricter NCA-enforcability tend to have a longer tenure and the effect of NCA-incidence is even higher in states with stronger enforceability. Starr, Frake, and Agarwal (2019) find that a 10 percentage point increase in the incidence of NCA use is associated with an 0.8 year increase in tenure in enforcing compared to non-enforcing states. The finding that mobile inventors are more productive than non-movers provides evidence that NCAs deteriorate economic activity as well (Hoisl, 2007).

In conclusion, noncompete agreements are predicted to increase worker tenure and decrease job switching (McAdams, 2019). The Michigan experiment by Marx, Strumsky and Fleming (2009) proves this by exploiting a natural experiment in Michigan, where NCAs were banned in 1985. Exploiting this policy change, Marx and co-authors found that labour mobility increased. This thesis researches if this relationship holds in the opposite direction and if more subtle changes in NCA legislation also impact mobility by exploiting the passage of legislation in 2007 in California that creates exemptions to anti-NCA legislation. I therefore hypothesize the following:

Hypothesis 1: *Compared to other non-enforcing states, inventor mobility will decrease in California due to the passing of exemptions on Business and Professions Code 16600 in 2007 regarding business owners.*

2.2.3 Trade Secrets

A similar loosening of the anti-NCA legislative framework in California occurred in 2017. The *Inevitable Disclosure Doctrine (IDD)* was upheld by the Third Circuit court in California, marking the beginning of an era where this doctrine could be applied. The case in question was *Fre-sco Systems USA, Inc. v. Hawkins* (2017). Kevin Hawkins worked for Fres-co Systems, quit and started at Transcontinental Ultra

Flex, Inc. Fres-co Systems was afraid Hawkins would expose trade secrets and even though there was no evidence Hawkins did such a thing, the court ruled that substantial overlap between Hawkins's work for Fres-co and his intended work for Transcontinental – same role, same industry, and same geographic region – would ensure the use of his confidential knowledge to Fres-co's detriment. The result was that Hawkins was prohibited from soliciting twelve clients whom he had serviced while at Fres-co.

The doctrine refers to a legal doctrine where an employer can claim misappropriation of trade secrets to seek an injunction against an employee, without the need for proof. It typically applies when a former employee, with knowledge of the former employer's confidential or trade secret information, accepts a similar role with a competitor. The original firm can prevent an ex-employee from sharing the trade secret with the new employer, thus limiting labour mobility. It also is important that the application of the inevitable disclosure doctrine is not dependent on proof that trade secrets were misappropriated. As explained by Flowers (2018), California always decided against IDD, until 2017.

This shift in the tendency to uphold IDD is due to the federal law DTSA (Defend Trade Secrets Act) instated in 2016 on May 11th. This act allows an owner of a trade secret to sue in federal court when its trade secrets have been misappropriated. While states are not completely bound by this piece of federal legislation, it does impact the rulings state courts make.

Economic research has shown that supporting IDD leads to lower labour mobility. Png and Samila (2015) found that state court rulings supporting IDD are associated with less job switching. Mobility is especially lower in states with weaker enforcement of noncompete covenants such as California. Patel and Devaraj (2022) find evidence that IDD rejection by state courts could therefore be a potential catalyst to entrepreneurial activity. Related to concerns about the flow of employees, Gu, Huang, Mao, and Tian (2020) found that venture capitalists are more likely to invest in IDD adoption states in start-ups with higher human capital, especially when VC investment has higher uncertainty in returns and when monitoring costs are high. Firms also increase their trade secret protections after IDD adoption (Liu & Ni, 2021). Seo and Somaya (2019) and Wang (2019) show that employers actually use IDD injunctions when states support IDD to restrict employee mobility.

Gilson (1999) is opinionated that IDD will undermine the advantages of California's anti-NCA legislation regarding labour mobility and innovation. More specifically, the inevitable disclosure doctrine threatens just the type of knowledge spillovers that has been so critical to the development of an area such as Silicon Valley. Gilson: "*it is because of the very character of tacit knowledge that an employee cannot avoid its use*". Undermining Business and Professions Code 16600 eventually means that NCAs will be more enforceable. Therefore, following the economic findings, the use of the inevitable disclosure doctrine and the creation of DTSA in 2016, I hypothesize the following:

Hypothesis 2: *The Defend Trade Secrets Act of 2016 will lead to lower labour mobility among inventors in California.*

2.2.4 Further implications of noncompetes on innovation

Labour mobility leads to the transfer of knowledge, which is essentially an engine for innovation. Braunerhjelm, Ding and Thulin (2018) find support for the theory of *learning-by-hiring*. This indicates that firms gain valuable knowledge by hiring workers from innovative firms. Braunerhjelm and co-authors find that mobility of knowledge-intensive employees has a positive and significant impact on firm innovation output. Mobility of high-skilled workers is the foundation for this innovation-increasing phenomenon, which is significantly influenced by noncompete agreements. Therefore, shedding light upon the relationship between NCAs, mobility and innovation is appropriate.

This relationship between noncompete agreements and labour mobility has implications for innovative activity. While it is a fact that stricter enforcement of NCAs leads to lower labour mobility, economic research has shown that noncompete agreements have an ambiguous effect on innovation. Cooper (2001) finds that NCAs act as a double-edged sword. While helping firms protect research investments and trade secrets, they also prevent the exchange of workers, leading to lower employee mobility. Research has shown that increased labour mobility has a positive effect on innovation, especially in knowledge-intensive industries.(Kaiser, Kongsted & Rønde, 2015; Cruz-Castro & Menéndez, 2005; Braunerhjelm, Ding & Thulin, 2015, Castillo et al., 2020). On top of that, technology-intensive human capital increases the probability of switching jobs (Glaser & Smite, 2011). These findings are backed by Yin et al. (2017), who find evidence that firms innovate less in states with strict NCA enforcement (high NEI-score), unlike California for example. They accredit this to lack a of labour mobility.

Inventors moving from one firm to another allows for the transfer of tacit knowledge to new geographical areas. A high-skilled employee moves from one firm to another, thus bringing along his human capital to benefit the hiring firm. Arrow (1962) observed that the "*mobility of personnel among firms provides a way of spreading information*". Knowledge spills over to other employees at the firm and even to other firms in geographical proximity. (Audretsch, 1995; Acs, Braunerhjelm, Audretsch & Carlsson, 2010). The relative importance of human capital compared to organizational capital is highlighted by Bhaskarabhatla et al. (2021), who find that human capital is 5–10 times more important than firm capabilities for explaining innovation output.

While NCAs block employee mobility and knowledge spillovers via the transfer of human capital, at the same time they allow firms to invest more in their employees knowing that they will remain at the firm, thus ensuring a return on human capital investment. Conti (2014) finds evidence that in areas with high NEI-scores there are more investments in research and development. Carlino (2021) finds that a doubling of enforcement led to an increase of about 8 percent in the start-up job creation rate in Michigan.

Stam (2019) balances the double-edged sword of NCAs and their effect on innovation. He states that economic research has arrived at a consensus that NCAs significantly decrease labour mobility. This serves the interest of incumbent firms, but constrains the founding and growth of new innovative firms. These new innovative firms develop and diffuse new knowledge that challenge the position of incumbents. Therefore, NCAs benefit large firms, they can now freely invest in human capital without the risk of employees relocating. To conclude, the effect of noncompetes on innovation is ambiguous, while noncompetes surely lead to lesser labour mobility. It is a fact that lower labour mobility can turn out to have a positive or negative effect on innovation, depending on the circumstances. The latter outreaches the scope of this study, which focuses on the effect noncompete agreements have on the mobility of high-skilled employees in California.

An interesting finding to put the discussion of NCAs in perspective is Prescott, Bishara and Starr (2016). In their paper, they performed a survey of more than 11500 labour force participants to find out how common the use of NCAs in the United States is. A relevant finding in the light of this thesis is that the incidence of noncompete agreements in a state (after controlling for potentially confounding factors) does not influence the level of enforcement of such agreements in that state. In other words, an employee in California (where noncompetes are prohibited) appears to be just as likely to work under an NCA as an employee in Florida (where noncompetes are much more likely to be enforced). Apparently, employers use NCAs for reasons unrelated to their legal effect. Still, as Gilson (1999) puts it: “my point is that California's legal infrastructure made it extremely unlikely that post-employment covenants not to compete would be enforced”. It remains puzzling to researchers why noncompete incidence is only weakly correlated with state enforceability.

2.3 Choice-of-law provisions

2.3.1 Introduction to choice-of-law provisions

An important aspect of noncompete litigation is the so-called *choice-of-law (COL) provision* or *governing law provision*. This is an addition to a contract that allows the parties involved to agree that a particular U.S. state's law will apply to the agreement, even if the parties involved are active in other states (Stim, 2022). In the case of conflict of law, private actors are granted the power to choose the law that will govern their contracts under the doctrine of *party autonomy* (Coyle, 2020). When applying this type of provision, courts must assess if there is a reasonable connection between the chosen state and the transaction described in the contract. The state that has the closest connection to the case is determined by the judge and that state's law is then applied to the case. A choice-of-law provision differs from jurisdiction shopping in the sense that a jurisdiction refers to where a dispute will be resolved, while a choice-of-law provision decides which state's law is going to be used to solve the dispute. An example of such a provision in a contract is:

Governing law: *This Agreement shall be governed by the laws of the state of California*.

For employers located in states that are more hostile to enforcement of restrictive covenants, such as California, these provisions may allow the parties to choose the laws of a state that would be more likely to enforce the restrictive covenants in order to protect customer information, goodwill, trade secrets and confidential information, to name a few. Still, courts always reserve the right to refuse the use of a choice-of-law provision if the judge does not deem the relationship between the state and the relevant parties reasonable (Pappas, 2020).

A recent study by Nyarko (2018) found that 75 percent of material contracts executed by public companies contain a choice-of-law clause. Several examples of cases regarding choice-of-law provisions in noncompete agreements have reached U.S. courts, each highlighting important aspects of this type of provision. Firstly, in *NuVasive v, Day* (2020) a Massachusetts resident named Timothy Day was prevented from working for a competitor of his former employer NuVasive, a company in Delaware with its headquarters in California. During his employment, Day signed an agreement with an NCA and a choice-of-law provision specifying that Delaware law was to be applied to the agreement. When leaving to work for a competitor, NuVasive sought an injunction, because Delaware law was enforceable and permits the enforcement of NCAs. Day argued that the Delaware choice-of-law provision could not be used and argued that anti-NCA Massachusetts law should apply. The court stated that choice-of-law provisions in employment contracts are enforceable in Massachusetts, unless the parties have no substantial relationship with the chosen state, in this case, Delaware, or unless the application of the law of the chosen state would be contrary to a fundamental policy of Massachusetts. The court ruled that both exceptions did not apply, so the choice-of-law provision was enforced.

The opposite happened in *Cabela's v. Highby* (2020). Two senior managers left Cabela's, an outdoor sports retailer, to set up their own business in the same sector. Cabela's was incorporated in Delaware with headquarters in Nebraska. The NCAs of the managers contained a choice-of-law provision that made Delaware law applicable, but the court ruled that Nebraska law should be applied, thus rejecting the choice-of-law provision in the employment agreement. The reason for the rejection is that the application of Delaware law would be contrary to Nebraska's fundamental public policy of not enforcing NCAs. Also, Nebraska has a "*materially greater interest*" in the case than Delaware, as the new store was located in Nebraska and set up by Nebraska residents. The important takeaway from this case is that the application of Delaware law would be *contrary to Nebraska's fundamental public policy against enforcing restraints of trade*. This means that the Delaware choice-of-law provision was unenforceable, even though Nebraska has no statutory prohibition against enforcing choice-of-law provisions in restricting covenants.

Another example that ties these two ends of the spectrum together is *Medtronic Inc v. Walland* (2020). In this case a New York court applied California law through a choice-of-law analysis and refused to enforce the noncompete provision in Walland's employment agreement. In this case the CEO of Medtronic, Joseph Walland, left the company to work for a competitor. As CEO of Medtronic Walland lived in New York, where NCAs may be enforced, and eventually moved to California to work for the competitor, where NCAs are prohibited. All this time Walland had an employment agreement with Medtronic that included a noncompete clause. The dispute was whether New York or California law should apply to this case, which would have consequences for the enforceability of the previously signed NCA. The court determined that the New York law violates California public policy, therefore, California law applies and the NCA was barred. This once again displays the power of choice-of-law and its alignment with fundamental policies.

This example shows that courts will consider factors such as the current locations of the employee and employer, the places where the agreement was negotiated and executed and whether there is a conflict between states' laws where one state has a materially strong public policy regarding noncompetes.

A Pennsylvania case underscores the impact a choice-of-law provision can make and shows that fundamental public policy does not always trump a choice-of-law provision. In *Synthes v. Peter Harrison* (2013), the employer sought an injunction to enforce an NCA against a former employee who had worked in California. The contract provided that the employment relationship and noncompetition clause would "be governed by Pennsylvania law applicable to contracts entered into and performed in Pennsylvania", indicating a choice-of-law provision. The Pennsylvania court held that California law applied because the contract was not performed in Pennsylvania, but the appellate court reversed the decision, concluding that Pennsylvania law applied. The reversal was significant, for had California law applied the noncompete would likely have been held invalid (Tuschman, 2014).

Other examples of the tension between choice-of-law provisions and fundamental state policies are *Dexcom, Inc. v. Medtronic, Inc.* (2021), *Change Capital Partners Fund I, LLC v. Volt Elec. Sys., LLC* (2018), *DePuy Synthes Sales, Inc. v. Howmedica Osteonics Corp.* (2022) and *Diversant, LLC v. Artech Info. Sys., LLC* (2018). In *Arkley v. Aon Risk Services Companies, Inc.* (2012) an Illinois company required employees to sign an NCA with an Illinois choice-of-law provision, regardless of their state of residence. As could be excepted of California courts in the meantime, the choice-of-law provision did not hold for Arkley, a California resident, because California once again had a *materially greater interest* in enforcing its own laws to protect California residents. These cases all show that if a choice-of-law provision clashes with public policy, that public policy will prevail. Also, the state that has the materially greater interest in the case also weighs in on the final judgement if the NCA will hold or not in the instances that the choice-of-law provision could technically hold according to public policy.

2.3.2 Choice-of-law provisions in noncompete agreements in California

At the time of writing his article, Gilson (1999) pointed out that section 16600 always overshadowed the application of choice-of-law provisions in California, thus upholding California's reputation for employee-friendliness. He noted that even if a contract containing a postemployment covenant not to compete designates the law of another state which is reasonably connected to the contract and the parties involved, California courts will nonetheless apply section 16600 to invalidate the contract. (Gilson, 1999). *Application Group Inc v. Hunter Group Inc*. (1998) is a prime example. In this case an NCA between a Maryland-based employer and a California-based employee was deemed unenforceable, even though the contract specifically added a choice-of-law provision to apply Maryland law to the case, which permitted NCAs (Lester & Ryan, 2009).

However, legislation is always liable to change. Specifically, a court decision in 2015 led to the creation of a new section in the California Labour Code in order to prevent misunderstandings regarding choice-of-law and NCAs in the future. In *Ascension Insurance Holdings, LLC v. Underwood* (2015), Underwood, a California resident, signed an NCA with Ascension, a Delaware-based firm that was active in California, that included a choice-of-law provision to apply Delaware law to the case. The judge assessed which state had a materially greater interest in the case and the outcome was California. Therefore, the disallowing of NCAs trumps the freedom of contract in Delaware, making the NCA void.

This case resonated further. Due to the possible application of an exception in the anti-NCA statutes of California, in the case of the sale of goodwill of a business (section 16601 of the California Business and Professions Code), the cry for an exception in choice-of-law provisions increased. As a result, section 925 was added to the Labour Code in 2016 in an attempt to provide clarity in the balance between section 16600 of the Business and Professions Code and the legality of choice-of-law provisions. It reads the following:

(a) *An employer shall not require an employee who primarily resides and works in California, as a condition of employment, to agree to a provision that would do either of the following:*

(1) *Require the employee to adjudicate outside of California a claim arising in California.*

(2) *Deprive the employee of the substantive protection of California law with respect to a controversy arising in California.*

(b) *Any provision of a contract that violates subdivision (a) is voidable by the employee, and if a provision is rendered void at the request of the employee, the matter shall be adjudicated in California and California law shall govern the dispute.*

Section 925 bars employers from applying choice-of-law provisions from another state to employment contracts for California residents, with one important exception in section 925(e). This section reads:

(e) This section shall not apply to a contract with an employee who is in fact individually represented by legal counsel in negotiating the terms of an agreement to designate either the venue or forum in which a controversy arising from the employment contract may be adjudicated or the choice of law to be applied.

This exception permits California employees to *voluntarily* agree to apply another state's law to his or her contract when represented by counsel. Section 925(e) weakens California's public policy against NCAs and gives the employee the freedom to determine whether an NCA should be void or not (Brooks, 2021; California Labour Code, 2022).

An important aspect of the exception in section 925(e) is that the employee must be represented by counsel. *Lyon v. Neustar Inc.* (2019) proved this. The main issue in this case was whether section 925 allowed an employer to apply a Virginia choice-of-law provision in an employment agreement to enforce an NCA against a California-based employee. The employee turned out not to be represented by counsel, which led to the court ruling that section 925 could not be applied. Later the court noted that if the employee was represented by lawyers, they would have used section 925 to enforce the choice-of-law provision and thus the NCA. Similar cases that try to trigger section 925(e), applicable to cases after January 1st 2017, are *Miller-Garcia v. Avani Media LLC* (2020) and *Midwest Motor Supply*

Co. v. Superior Court (2020). These rulings all boil down to the voluntary aspect of the choice-of-law provision and in general rule to uphold an NCA, if it satisfies all the requirements.

Synthes v. Knapp (2017) is an example of a firm successfully enforcing an NCA via a choice-of-law provision in California, thus proving that the inception of section 925(e) has an impact. In this case the California resident Knapp sought to work at a competitor of his previous employer Synthes, located in Pennsylvania. Knapp went to the California courts to abolish the NCA with a Pennsylvania choice-of-law provision, but the California courts denied his injunction and applied Pennsylvania law to his case. Knapp voluntarily signed the NCA, was represented by legal counsel and the courts determined that Pennsylvania had a materially greater interest in the case. Dasdoff and Yu (2017) even mention that this decision is the result of section 925.

As a rule of thumb, cases on the choice-of-law provision in noncompete agreements often boil down to a conflict between fundamental public policy to uphold freedom of contract in one state and fundamental public policy in another state of not enforcing NCAs. The solution most often lies in determining which state's interest outweighs the other and which state is more involved in the transaction, thus having a "*materially greater interest*". This is up to a judge to decide (Pappas, 2020).

In conclusion, from January 1st 2017 onwards, a California resident can voluntarily enter into a noncompete agreement by adding a choice-of-law provision in his or her employment agreement that applies a different state's law to their employment agreement, if the employee is represented by counsel This gives the employee the power to subject himself to an NCA, thus exploiting the positive economic effects NCAs can have if used correctly. This will be touched upon in the following section.

2.3.3 Economic research on choice-of-law provisions in noncompete agreements

There is relatively little economic research on the effect of choice-of-law provisions in NCAs in relation to labour mobility or other economic variables. Still, legal research is extensive and mostly covers the incidence of choice-of-law provisions and how it differs per state.

Sanga (2014) finds, for example, that states most commonly used in choice-of-law are Delaware, New York and Nevada. She finds this by introducing the term *relative use of law*, which portrays the likelihood that a contract chooses a certain state's law normalized by the likelihood that a company that reports a contract is headquartered in that same state. Sanga (2014) builds upon Eisenberg and Miller (2008), who analyse choice-of-law provisions in 2,882 contracts from 8-K filings in 2002, published by the Securities and Exchange Commission. Their main finding is that the most common jurisdictional choice is New York at 46 percent while the second-most, Delaware, trails behind at 15 percent.

Mahnhold (2010) researches choice-of-law provisions in NCAs in a German setting. The paper assesses whether choice-of-law provisions provide the parties with greater leeway for structuring NCAs. He concludes that COL provisions cannot be used to structure NCAs in such a way that they circumvent relevant legislation. Germany resembles California in its anti-NCA legislative attitude and has a similar law similar to section 16600, which cannot be circumvented by COL provisions according to this paper.

Until 2017, all attempts to circumvent California labour regulation by using COL provisions have proven to be futile. Wu (2003) discusses whether California courts should uphold COL provisions in NCAs or not. She finds that California courts have reached conflicting decisions on the topic, because they still use a case-by-case approach. Therefore, she suggests that California courts should instead apply a bright line rule that upholds choice of law provisions for employees who have not worked in California. Since her article in 2003, COL provisions are also upheld by Californian courts when the events giving rise to the claim occurred outside California. Hoffheimer (2015) mentions two examples where the *territorial theory* was used by the Supreme Court of California in 2010 and 2011. These rulings provided a precedent for cases in the future regarding California law in other states, but there was still no clarity on how courts should treat contracts with choice-of-law provisions within the state, until 2017 when section 925 came into law. This piece of legislation permits the use of choice-of-law provisions in noncompete agreements in certain cases. This evidently couples choice-of-law provisions to the economic consequences of NCAs.

Consider the following example. Noncompete legislation in a certain state can influence a firm's location decision significantly. A firm that generates a competitive advantage with trade secrets and tacit knowledge is inclined to avoid California, where NCAs cannot be used to protect sources of competitive advantage.

However, if choice-of-law provisions can be used, which is the case in California since January 1st 2017, do NCAs still influence the location decision and labour mobility? As many examples have shown before, California law trumps other jurisdictions often when it comes to noncompete agreements. However, if reasonable choice-of-law agreements are permitted as of 2017, the following could occur: a firm with a significant amount of trade secrets avoids California before 2017, but after 2017 the firm can incorporate choice-of-law provisions in noncompete agreements to another state to which it has a strong and reasonable connection. These NCAs will hold under section 925(e). California's anti-NCA regulation is circumvented and therefore does not influence the location decision of the firm anymore.

Additionally, the mobility of the employees of the firm is now limited due to their NCA's with COL provisions.

Another example: a California resident works in New York for a New York-based firm. The employee signs a choice-of-law agreement that chooses New York as governing law. If the employee leaves the firm to work for a competitor, the old firm will call upon the noncompete agreement. As a California resident, the employee will call upon section 16600, banning all restrictive covenants. Before 2017, judges would have ruled that section 16600 prevails and that the NCA is void. But section 925(e) permits COL provisions in NCAs if the connection to the other state is reasonable. California courts will now rule that the NCA holds. Therefore, correctly designed choice-of-law provisions can undermine anti-NCA legislation and reverse or moderate the positive effects it has on inventor mobility. I therefore hypothesize the following:

Hypothesis 3: *The introduction of Section 925(e) in California labour law in 2017 regarding the tolerance of choice-of-law provisions under strict circumstances leads to a further reduction of inventor mobility.*

2.4 Hypotheses

In conclusion, the theoretical framework deduced three hypotheses from the existing literature. The hypotheses are listed again below:

Hypothesis 1: *Compared to other non-enforcing states, inventor mobility will decrease in California due to the passing of exemptions on Business and Professions Code 16600 in 2007 regarding business owners.*

Hypothesis 2: *The Defend Trade Secrets Act of 2016 will lead to lower labour mobility among inventors in California.*

Hypothesis 3: *The introduction of Section 925(e) in California labour law in 2017 regarding the tolerance of choice-of-law provisions under strict circumstances leads to a further reduction of inventor mobility.*

3 Data

3.1 Data sources

A common measure in the literature to measure inventor mobility is the use of patent data (Marx, Strumsky & Fleming, 2009; Kim & Marschke, 2005; Agarwal, Ganco & Ziedonis, 2009). I obtained patent data from PatentsView, a data analysis platform that filters the large databank of the United States Patent and Trademark Office (USPTO). The dataset contains information on inventors, their patents and their assignees, for example. The state and year in which the patent was granted are included as well.

Distilling a suitable dataset from this wide databank can be difficult. First of all, a matching problem exists with respect to name and address information derived from the patent document, as explained by Hall (2004) in the Patent Name-Matching Project. Papers often solve this first problem by performing an extensive survey among inventors, such as Starr, Prescott and Bishara (2021) and Hoisl (2007). In line with earlier economic research on inventor mobility, I use patent data to proxy mobility. Patenting at a firm or in a state at t=0 and patenting at another firm or in another state at t=1 indicates a move.

I use patent data for several reasons. Firstly, patents are public documents so they are easily accessible. Secondly, the USPTO requires an extensive amount of information to be added to a patent application, such as location, year, patent type and assignee. Lastly, patents are a proxy for innovation, thus broadening the scope of this study from only mobility to questions about transfer of knowledge and innovation, thus rightfully gaining its place in the literature (Acs, Anselin, & Varga, 2002; Crosby, 2000).

However, there are several drawbacks to using patents as a measure of mobility. First, not all firms decide to patent an invention. They may encourage an inventor to resort to secrecy rather than legal protection of generated tacit knowledge. Second, patenting behaviour may differ per industry, thus creating a bias towards industries that do make extensive use of patenting, such as the electronics and pharmaceutical industries (Melero & Palomeras, 2015). Also, patents take years to be processed, which is problematic when researching the effects of 2007 and 2017 policy changes. Marx, Strumsky and Fleming (2009) also explain why using patents to detect inventor movement is inexact for three reasons. First, patents may fail to detect movements that occurred between an inventor's patents. An inventor could have patented in a certain city in 2003 and in another city in 2015, but also could have lived in a third city between those years. Secondly, it is also not possible to know precisely when the move occurred within the time interval between the two application dates (Song et al. 2003) or whether the employee-employer separation was voluntary or involuntary. I solve the last concern

Marx, Strumsky and Fleming have, namely that patents are not indexed by inventor, by constructing a longitudinal dataset categorized by inventor instead of patent.

3.2.1 Sample

To create the dataset, I submitted a query at PatentsView to retrieve all inventors with granted patents in the United States between 2000 and 2021. I restructured the data into a longitudinal inventor dataset, instead of per patent. Now an inventor was listed once instead of listed for every patent he or she held, which enabled analysis focused on inventors rather than patents.

I also restructured the dataset in such a way that there were count variables for number of patents, states and assignees. In addition, I added a series of binary variables to indicate in which year and in which state an inventor had a patent. The final dataset has 50 indicators for all the states in the U.S. and 22 indicators for the years 2000-2021. If an inventor has 9 patents in 3 different states in 9 different years, the patent state dummies would have 9 ones and 41 zeros and the patent year dummies would have 3 ones and 19 zeros for that specific inventor. This enables me to analyse the career paths of inventors. The dataset does not indicate how many patents there are per year for each inventor, only if an inventor patented in that year or not. Luckily, that is irrelevant as year is only used as a measure of whether an inventor has been influenced by a policy change or not. Before dropping duplicate inventor observations, I created a dummy variable whether an inventor had a patent after 2007 or 2017. This allows for analysis of the policy changes in these years on inventor mobility.

To create an inventor mobility variable, I only kept inventors whose careers can be tracked with patent data. This implies dropping inventors who appear only once in the dataset. A similar approach was done by Melero & Palomeras (2015). The next step is to create a subsample of states with anti-NCA policy. While only California, Oklahoma and North Dakota have banned noncompete agreements, several other states have policies in place that restrict the use of NCAs. These states are Alaska, Nevada, Washington, Montana, Minnesota, West Virginia, and Connecticut. This is similar to the approach used by Marx, Strumsky and Fleming (2009). Trends in inventor movement in these states serve as a viable counterfactual to the trend that would have occurred in California in absence of the policy changes. Therefore, the first hypothesis will be tested with 49 states as a control group and a subsample with 9 states with anti-NCA policy as a control group.

A potential bias in the final sample may arise due to the patent application truncation problem, as explained by Piwonski (2021) and Dass et al. (2017). This is a common problem among research that uses patent data. A phenomenon in the world of patenting is that there are several years between the patent application and the actual granting of the patent. The sample only contains data on granted patents. Therefore, in the final years of the sample it could be possible that inventors have moved to a different state, applied for a patent and are awaiting the granting of that patent. The dataset is not able to observe a move for this inventor that in reality has already made a move. Therefore, subsequent results may have a downward bias, due to the fact that the number of moves that actually occurred is artificially lower in the sample.

The final sample spans the years 2000-2021, because I am disentangling the effects of policy changes in 2007 and 2017 on inventor mobility. This means that moves by inventors before 2007 and 2017 must be added to the sample to analyse these changes in Californian and national law.

The ideal data source for choice-of-law provisions is the information from 8-K and 10-K contract filings of the firms where the researched inventors are employed so a dummy indicator can be manually added to indicate whether an inventor was subject to a noncompete agreement with a choice-of-law provision. After contacting the SEC, I discovered that this information is subject to strict privacy regulations in the U.S. As an alternative, I created a subsample with just two states, California, the state of the policy change, and Delaware and New York, the two states most often chosen in choice-of-law clauses in employment contracts. These states allow choice-of-law provisions, have favourable employment regulations for firms and allow the use of noncompete agreements (Sanga, 2014; Eisenberg and Miller, 2008).

The original sample contains 348,579 inventors who patented between 2000 and 2022. Inventors with only 1 patent and inventors with more than 47 patents were dropped from the sample. Inventors with more than 47 patents are in the 99th percentile of the distribution of number of patents, so these observations were dropped to prevent bias formed by outliers. The final sample consists of 168,276 inventors with at least 2 patents. The sample with inventors with at least 2 patents and at least 2 states contains 13,323 inventors and the sample with inventors with at least 2 patents and with at least 2 assignees contains 43,253 inventors. This is the first group of samples.

The second group of samples, the subsample for hypothesis 1 with 10 nonenforcing states, consists of 59,269 inventors. The sample with inventors with at least 2 patents and with at least 2 states contains 889 inventors and the sample with inventors with at least 2 patents and with at least 2 assignees contains 11,384 inventors.

The third group of samples, the subsample for hypothesis 3 with California, New York and Delaware, consists of 49,377 inventors. The sample with inventors with at least 2 patents and with at least 2 states contains 400 inventors and the sample with inventors with at least 2 patents and with at least 2 assignees contains 10,050 inventors.

In conclusion, I created 9 subsamples to serve as different control groups, categorized by number of states included in the sample, amount of states an inventor has patented in and amount of assignees an inventor has. The idea behind the subsamples is that inventors with only at least 2 patents prove that their innovative activity is not coincidental, ensuring that they have the *potential* to move to a different firm or state. An inventor with at least 2 patents in at least 2 different states has *proven* to be mobile between states and an inventor with at least 2 patents with at least 2 different assignees has *proven* to be mobile between firms.

3.2.2 Summary list of subsamples

Table 1 – list of subsamples

Subsample 1.1	50 states, inventors with more than 2 patents
Subsample 1.2	50 states, inventors with more than 2 patents in more than 2 states
Subsample 1.3	50 states, inventors with more than 2 patents with more than 2 assignees
Subsample 2.1	10 states, inventors with more than 2 patents
Subsample 2.2	10 states, inventors with more than 2 patents in more than 2 states
Subsample 2.3	10 states, inventors with more than 2 patents with more than 2 assignees
Subsample 3.1	3 states, inventors with more than 2 patents
Subsample 3.2	3 states, inventors with more than 2 patents in more than 2 states
Subsample 3.3	3 states, inventors with more than 2 patents with more than 2 assignees

3.3 Variables

3.3.1 Dependent variable

This thesis uses two dependent variables that both proxy a different aspect of inventor mobility. The changes in these two variables that can be explained by changes in Californian and national legislation will be uncovered in the following chapters.

States This variable measures the number of different states an inventor has patented in, in the period 2000-2021. Therefore, *states* is the first of two measures for inventor mobility. It is a count variable that denotes the number of different states an inventor has patented in. It only takes on non-negative integer values in discrete steps. Therefore, if an inventor has 20 patents to his or her name this variable still takes value 1 if all the 20 patents were granted in the same state. A similar approach to measure inventor mobility is used by Melero & Palomeras (2015), Cappelli et al. (2019), Jin and Zhu (2021) and van der Wouden and Rigby (2021). This variable is the proxy for *geographical* inventor mobility. An inventor can switch firms several times within the same state, thus being a mobile inventor, but still only have a count of 1 for the *states* variable.

Assignees The second dependent variable that denotes inventor mobility is *assignees.* This is a count variable that counts the number of assignees to the patents of a given inventor, thus counts the number of firms an inventor works at. I identify inventors as having changed jobs when successive patents have different assignees. This variable measures the second aspect of inventor mobility, namely *firm* mobility. A similar approach is used by Trajtenberg (2005), Marx, Strumsky and Fleming (2009) and Hoisl (2007).

A problem arises with using assignees of a patent as a proxy for the number of firms an inventor has worked at. If an inventor works at firms A, B and C in that order and only patents at firms A and C, the *assignees* variable will count 2 firms instead of 3. This may lead to underestimation of the actual effect of the policy changes on inventor mobility.

3.3.2 Explanatory variables

California This study researches whether changes in California's noncompete agreement policy that create exemptions that allow NCAs in certain cases impact inventor mobility. Hypothesized is that mobility reduces due to the allowance of inventor-restricting NCAs. To test that, I created a dummy indicator that takes on value 1 if an inventor has patented in California and 0 otherwise.

Post_2007 To answer hypothesis 1 I additionally created a dummy variable that takes on value 1 if an inventor patented after 2007 and 0 if before 2007. The paragraphs in the California Business and Professions Code 16601, 16602 and 16602.5 that serve as exemptions to paragraph 16600 became effective on January 1st 2007.

CA_post_2007 This main explanatory variable is created by interacting *California* and *post_2007.* If an inventor patented in California after 2007, he or she will be governed by these policy changes and thus be affected by the treatment. In a difference-in-difference context, which will be touched upon in the following chapter, this variable is the effect of the treatment. This variable takes value 1 if an inventor has patented in California after 2007 and zero otherwise.

Post_2017 To answer hypothesis 3 regarding the allowance of choice-of-law provisions under strict circumstances, I created a dummy variable that takes on value 1 if an inventor patented after 2017 and 0 if before 2017. This variable is also used to assess hypothesis 2. The Defend Trade Secrets Act came into law in May 2016. Therefore, the effect of the law could be lagged by a year.

CA_post_2017 This main explanatory variable is created by interacting *California* and *Post_2017.* If an inventor patented in California after 2017, he or she will be governed by these policy changes and thus be affected by the treatment. This variable takes value 1 if an inventor has patented in California after 2017 and zero otherwise.

Patents This variable is a count variable that denotes the number of patents an inventor has, meaning the number of granted patents. If this variable takes value 1 that inventor will be dropped from the dataset as that indicates that the inventor has only 1 patent. Having 1 patent means that no mobility can be measured with this dataset. As mentioned earlier, if an inventor applied for a patent in the final years of the dataset and it is not granted yet, this patent is not included in the dataset. This can potentially cause a downward bias in measuring the number of patents an inventor has.

PatentState_XX This variable is a series of 50 variables for each state. The variables are separated by their two-letter code which is substituted in the *XX.* For example, *PatentState_CA* denotes California. Furthermore, this variable is a dummy variable that indicates if an inventor has patented in that specific state or not. This allows for the development of subsamples and to look closer at the relationships in between-state mobility in the U.S.

PatentYear_XXXX This variable is similar to *PatentState_XX*, except it denotes the years in which an inventor has patented instead of the state. For example, *PatentYear_2012* takes on value 1 if an inventor has patented in this year. I stress that this is not a count variable. If an inventor has 3 patents in 2012, the value of *PatentYear_2012* will still be 1. This study looks at mobility, not at innovation, or else this would be an issue.

3.3.3 Control variables

Year_fixed_effects I follow the existing literature and include year fixed effects in the regression. This series of dummy variables is meant to control for time-invariant changes in the macroeconomic environment (Firebaugh, Warner & Massoglia, 2013). Fixed effects control for time-invariant unobserved differences between years. Other research that uses year fixed effects include Mummolo and Peterson (2018) and Jakiela (2021).

State_fixed_effects A likely issue in the natural experiment design is that the effect of the policy change on mobility is affected differently between states. To control for time-invariant unobserved causes of moving between states I add state fixed effects to the models. Similar approaches have been done by Aghion (2008) and Strumpf and Phillippe (1999). Year and state fixed effects combined serve as a two-way fixed effects control.

Data for other control variables was not available at the inventor level or could not be matched to individual inventors. However, the robustness of difference-in-difference, which will be touched upon in the next chapter, ensures that other factors that might account for inventor moves, time-variant or time-invariant unobserved differences between groups, are kept constant. Therefore, adding both a year and a state component (California and post-2007) isolates the effect of the treatment.

3.3.4 Descriptive statistics

The dataset measuring inventor mobility is divided into three subsamples. Below the summary statistics for the three subsamples are provided. The most relevant aspects of the tables for the subsample with all 50 states are that states are relatively evenly divided except for California, which is overrepresented in the sample. Secondly, the years are relatively evenly divided as well, with a slight skewness towards more recent years. This is also visible in tables 2.3, 2.4 and 2.5. The percentage of observations later than the given year decreases when comparing 2007, 2016 and 2017. Evenly divided observations among years assures that there is no bias to earlier or more recent years. In the sample with 168,276 observations, the sample mean of states is 1.088 states, the sample mean of assignees is 1.331 firms and the sample mean of number of patents is 5.3 patents per inventor.

Subsample 1 – All states

Table 2.1 - Tabulation of state

	Freq.	Percent	Cum.
AK	56	0.03	0.03
AL	972	0.58	0.61
AR	534	0.32	0.93
AZ	2354	1.40	2.33
CA	43094	25.61	27.94
CO	3358	2.00	29.93
CT	1222	0.73	30.66
DC	388	0.23	30.89
DE	928	0.55	31.44
FL	5685	3.38	34.82
GA	3196	1.90	36.72
HI	191	0.11	36.83
IA	1791	1.06	37.90
ID	1978	1.18	39.07
IL	5419	3.22	42.29
IN	3714	2.21	44.50
KS	1199	0.71	45.21
KY	1530	0.91	46.12
LA	615	0.37	46.49
MA	5227	3.11	49.59
MD	1988	1.18	50.77
ME	121	0.07	50.85
MI	5488	3.26	54.11

MN	3742	2.22	56.33
MO	2196	1.30	57.64
MS	142	0.08	57.72
MT	319	0.19	57.91
NC	4176	2.48	60.39
ND	186	0.11	60.50
NE	779	0.46	60.96
NH	549	0.33	61.29
NJ	2189	1.30	62.59
NM	1727	1.03	63.62
NV	1264	0.75	64.37
NY	8486	5.04	69.41
OH	5091	3.03	72.44
OK	1062	0.63	73.07
OR	2370	1.41	74.48
PA	3905	2.32	76.80
PR	28	0.02	76.81
RI	271	0.16	76.97
SC	923	0.55	77.52
SD	279	0.17	77.69
TN	2147	1.28	78.96
TX	15454	9.18	88.15
UT	2031	1.21	89.36
VA	2292	1.36	90.72
VI	1	0.00	90.72
VT	155	0.09	90.81
WA	11462	6.81	97.62
WI	3627	2.16	99.78
WV	165	0.10	99.88
WY	210	0.12	100.00
Total	168276	100.00	

Table 2.2 - Tabulation of year

	Freq.	Percent	Cum.
2000	4734	2.81	2.81
2001	5024	2.99	5.80
2002	4810	2.86	8.66
2003	4964	2.95	11.61
2004	4683	2.78	14.39
2005	4042	2.40	16.79
2006	4914	2.92	19.71
2007	4440	2.64	22.35
2008	4387	2.61	24.96
2009	4664	2.77	27.73
2010	6227	3.70	31.43
2011	6332	3.76	35.19
2012	7366	4.38	39.57
2013	8614	5.12	44.69
2014	9929	5.90	50.59
2015	8468	5.03	55.62
2016	9113	5.42	61.04
2017	10575	6.28	67.32
2018	11299	6.71	74.04
2019	14357	8.53	82.57
2020	15046	8.94	91.51

2021	14288	8.49	100.00
Total	168276	100.00	

Table 2.3 - Tabulation of post_2007

	Freq.	Percent	Cum.
0	18502	11.00	11.00
1	149774	89.00	100.00
Total	168276	100.00	

Table 2.4 - Tabulation of post_2016

	Freq.	Percent	Cum.
0	63918	37.98	37.98
1	104358	62.02	100.00
Total	168276	100.00	

Table 2.5 - Tabulation of post_2017

	Freq.	Percent	Cum.
0	72380	43.01	43.01
1	95896	56.99	100.00
Total	168276	100.00	

Table 2.6 - Descriptive Statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
states	168276	1.088	.32	1	8
assignees	168276	1.331	.663	1	38
patents	168276	5.3	5.869	2	47

Subsample 2 – subsample of 10 nonenforcing states

In subsample 2 with 10 states instead of 40, inventors with patents in California take up a much larger percentage of the observations when compared to the first sample, 68.78% compared to 25.61. This could lead to a potential bias. Furthermore, the years are relatively evenly divided as well, with a slight skewness towards more recent years. This is also visible in tables 3.3, 3.4 and 3.5. The percentage of observations later than the given year decreases when comparing 2007, 2016 and 2017. Evenly divided observations among years assures that there is no bias to earlier or more recent years. In the sample with 59,269 observations, the sample mean of states is 1.015 states, the sample mean of assignees is 1.237 firms and the sample mean of number of patents is 5.455 patents per inventor.

Table 3.1 - Tabulation of state

	Freq.	Percent	Cum.
AK	52	0.09	0.09
CA	40763	68.78	68.86

CT	1114	1.88	70.74
MN	3542	5.98	76.72
MT	297	0.50	77.22
ND	177	0.30	77.52
NV	1195	2.02	79.54
OK	978	1.65	81.19
WA	10999	18.56	99.74
WV	152	0.26	100.00
Total	59269	100.00	

Table 3.2 - Tabulation of year

	Freq.	Percent	Cum.
2000	1158	1.95	1.95
2001	1264	2.13	4.09
2002	1257	2.12	6.21
2003	1341	2.26	8.47
2004	1257	2.12	10.59
2005	1197	2.02	12.61
2006	1453	2.45	15.06
2007	1290	2.18	17.24
2008	1401	2.36	19.60
2009	1505	2.54	22.14
2010	2053	3.46	25.61
2011	2126	3.59	29.19
2012	2505	4.23	33.42
2013	2999	5.06	38.48
2014	3608	6.09	44.57
2015	3236	5.46	50.03
2016	3486	5.88	55.91
2017	4083	6.89	62.80
2018	4438	7.49	70.28
2019	5668	9.56	79.85
2020	6108	10.31	90.15
2021	5836	9.85	100.00
Total	59269	100.00	

Table 3.3 - Tabulation of California

	Freq.	Percent	Cum.
0	18506	31.22	31.22
1	40763	68.78	100.00
Total	59269	100.00	

Table 3.4 - Tabulation of post_2007

	Freq.	Percent	Cum.
0	5088	8.58	8.58
1	54181	91.42	100.00
Total	59269	100.00	

Table 3.5 - Tabulation of post_2016

	Freq.	Percent	Cum.
0	19846	33.48	33.48
1	39423	66.52	100.00

Total	59269	100.00	

Table 3.6 - Tabulation of post_2017

	Freq.	Percent	Cum.
0	22941	38.71	38.71
1	36328	61.29	100.00
Total	59269	100.00	

Table 3.7 - Descriptive Statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
states	59269	1.015	.123	1	3
assignees	59269	1.237	.55	1	9
patents	59269	5.455	6.033	2	47

Subsample 3 – choice-of-law provisions in California, Delaware and New York

Tables 4.1 until 4.7 display the summary statistics of the third sample containing California, Delaware and New York. Years are relatively evenly distributed, although states are skewed heavily towards California. Table 4.1 shows that 82.15 percent of the inventors in the sample have patented in California. The sample contains 49,377 observations with a sample mean of states of 1.008 states, a sample mean of assignees of 1.253 firms and a sample mean of number of patents of 5.475 patents per inventor.

Table 4.1 - Tabulation of state

	Freq.	Percent	Cum.
CA	40563	82.15	82.15
DE	881	1.78	83.93
NY	7933	16.07	100.00
Total	49377	100.00	

Table 4.2 - Tabulation of year

	Freq.	Percent	Cum.
2000	1057	2.14	2.14
2001	1140	2.31	4.45
2002	1123	2.27	6.72
2003	1217	2.46	9.19
2004	1179	2.39	11.58
2005	1044	2.11	13.69
2006	1270	2.57	16.26
2007	1087	2.20	18.46
2008	1134	2.30	20.76
2009	1155	2.34	23.10
2010	1605	3.25	26.35
2011	1674	3.39	29.74
2012	2060	4.17	33.91
2013	2470	5.00	38.91
2014	3027	6.13	45.05

2015	2650	5.37	50.41
2016	2874	5.82	56.23
2017	3546	7.18	63.41
2018	3561	7.21	70.63
2019	4648	9.41	80.04
2020	4977	10.08	90.12
2021	4879	9.88	100.00
Total	49377	100.00	

Table 4.3 - Tabulation of California

	Freq.	Percent	Cum.
0	8814	17.85	17.85
1	40563	82.15	100.00
Total	49377	100.00	

Table 4.4 - Tabulation of post_2007

	Freq.	Percent	Cum.
0	4760	9.64	9.64
1	44617	90.36	100.00
Total	49377	100.00	

Table 4.5 - Tabulation of post_2016

	Freq.	Percent	Cum.
0	17014	34.46	34.46
1	32363	65.54	100.00
Total	49377	100.00	

Table 4.6 - Tabulation of post_2017

	Freq.	Percent	Cum.
0	19600	39.69	39.69
1	29777	60.31	100.00
Total	49377	100.00	

Table 4.7 - Descriptive Statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
states	49377	1.008	.09	1	2
assignees	49377	1.253	.569	1	10
patents	49377	5.475	6.106	2	47

Pairwise correlations

Table 5 – Pairwise correlations between the most important variables of interest

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)
(1) patents	1.000						
(2) states	0.086*	1.000					
	(0.000)						
(3) assignees	0.277*	0.500*	1.000				
	(0.000)	(0.000)					
(4) California	0.032*	-0.033*	-0.011*	1.000			
	(0.000)	(0.000)	(0.000)				
(5) post_2007	0.113*	0.075*	0.072*	0.055*	1.000		
	(0.000)	(0.000)	(0.000)	(0.000)			
(6) post_2016	0.136*	0.104*	0.089*	0.084*	0.449*	1.000	
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)		
(7) post_2017	0.138*	0.102*	0.091*	0.079*	0.405*	0.901*	1.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	

**** p<0.01, ** p<0.05, * p<0.1*

As table 5 shows, a couple of slightly significant correlations appear in the data (subsample 1 with all 50 states). Most interestingly all variables of interest are positively correlated with the number of patents an inventor has. The magnitude of the positive correlation increases in later years, indicating that patenting activity increases over years. Patenting in California is positively correlated with number of patents, but negatively correlated with number of states and number of assignees an inventor has. Furthermore, later years are positively correlated with assignees and states.

4 Methodology

4.1 Econometric tools

4.1.1 Difference-in-difference

The main econometric approach I use to assess the impact of increased tolerance of noncompete agreements in California on inventor mobility is difference-in-difference (DiD). This approach is preferred in quasi-natural experimental settings, for example when estimating the effects of certain policy interventions and policy changes that do not affect all observations at the same time and in the same way. This research design disentangles the causal effect of a policy change by taking the difference between two groups. What DiD essentially does in this context is take the average change in inventor mobility in the control group in both periods and subtract that from the average change in inventor mobility in the treatment group in both time periods. The difference between the differences is the causal effect of the intervention or treatment. The abrupt timing of the implementation of a certain policy change in only one state thus permits a difference-in-difference identification strategy that compares inventor mobility activity before and after each regulation change relative to a control group of states not undergoing a regulation change (Hombert & Matray, 2017; Lechner, 2011).

The DiD approach is particularly well-suited to estimate the causal effect of sharp changes in policies or practices, such as the addition of section 16601 on January 1st 2007 to the California Business and Professions Code, the installation of the Defend Trade Secrets Act on May 11th 2016 and the adding of section 925(e) to the Californian Labour Code on January 1st 2017. This approach has provided policy-makers with vital information even in the absence of controlled or natural experiments. It has, therefore, been used extensively to study the impacts of various policy changes. Prime examples include reforms of compulsory schooling and tracking (Meghir & Palme, 2005; Pekkala, Kerr, Pekkarinen & Uusitalo, 2013; Meghir, Palme & Simeonova, 2018), education priority zones for disadvantaged schools (Bénabou, Kramarz & Prost, 2009), subsidized child care (Havnes & Mogstad, 2011), paid parental leave (Danzer & Lavy, 2018) and the effect of immigration on the employment of natives by Card (1990). The DiD approach in the latter paper follows the estimation:

$$Y_i = \beta_t + \gamma_c + \delta M_i + \varepsilon_i \tag{1}$$

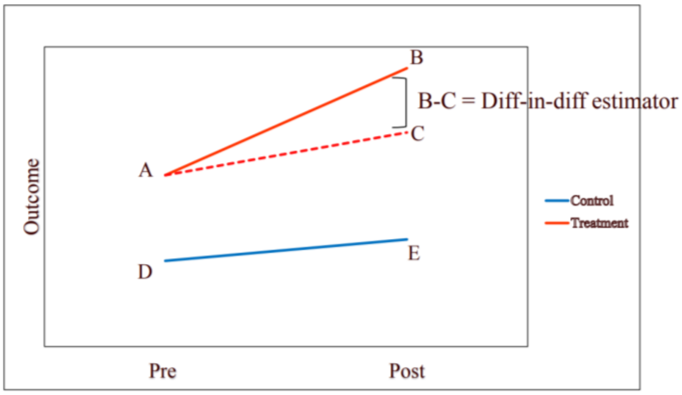
With M_i in equation 1 as an interaction term equal to the product of a dummy indicating observations after 1980 and a dummy indicating residence in Miami, thus a combination of the location indicator γ_c and the time indicator β_t. These are the two main components of a DiD estimation, a location dummy variable and a time dummy variable. The δ, the interaction effect between the two, is what this study is trying to uncover. Extrapolated to the case with Californian noncompete regulation, this coefficient is:

$$\{E[Y_i|c = California, t = post2007] - E[Y_i|c = California, t = pre2007]\} -$$
$$\{E[Y_i|c = control\ group, t = post2007] - E[Y_i|c = control\ group, t = pre2007]\}$$

Therefore, a similar approach is used in this thesis as in Card (1990), with two dummy variables for location and time interacted creating the main explanatory variable, the DiD estimator. The interaction effect serves as an indicator of which inventors are subject to the policy change, thus affected by the 'treatment', and which inventors serve as the control group. An inventor is affected by the policy changes, thus receives treatment, if the inventor patented in California after 2007 for hypothesis 1 and after 2017 for hypotheses 2 and 3.

In summary, the idea behind the DiD identification strategy is simple. The two groups might be observationally different. That is, the group-specific means might differ in the absence of treatment. However, as long as this difference is constant over time (in the absence of treatment), it can be differenced out by deducting group-specific means of the outcome of interest. The remaining difference between these group-specific differences must then reflect the causal effect of interest. Therefore, DiD controls for unobserved heterogeneity between groups by differencing these differences out (Schwerdt and Woessmann, 2020). This is best shown in a figure:

Figure 1 – Graphical representation of DiD. Source: Yoon (2019)



A difference-in-difference strategy has several advantages. First, it tackles the endogeneity issue. Endogeneity arises when an unobserved part of the error term, meaning variables or differences between groups and individuals that cannot be observed, explains a part of the causal effect between X and Y. Endogeneity can be caused by reverse causality, omitted variable bias and measurement errors, for example. By subtracting the mean differences between two periods of the treatment and control group, unobserved heterogeneity between groups is not an issue anymore (Riumallo-Herl, 2022).

A second advantage is the quasi-experimental design of DiD. Randomly assigning treatment on a large scale is virtually impossible to accomplish. In a DiD setting, the random assignment is done by the policy intervention of interest. The policy change is therefore a tool to research the relationship between two variables, in this case the level of noncompete agreement tolerance and inventor mobility. The treatment and control groups are created via a random policy design. This solves selection bias, where units of observation are placed into treatment and control groups based on pre-existing differences instead of randomly (Duflo, Glennerster & Kremer, 2007).

Third, DiD is simple to implement and the interpretation is intuitive. Any deviation in the trend of the treatment group compared to the trend of the control group is the causal impact of the intervention. Treatment and time indicators are dummy variables and the DiD-estimator is simply the interaction effect between the two, as the formulas for the hypotheses will show in the following section. An OLS linear regression is then estimated, generating a coefficient for the DiD estimator which can then be interpreted as the causal effect of the treatment (Yoon, 2019; Berger & Roman, 2020).

Important to note when using DiD is having the right control group. To test this, I will use a total of 9 subsamples in this study, categorized by states included and the degree of inventor mobility. Using multiple samples allows me to curate a compatible control group for California.

Still, causal inference in the context of NCAs is very difficult. For example, do engineers emigrate to California because of noncompete enforcement in their home state? Or are their choices driven by the availability of jobs or maybe even the weather? Without a natural experiment, it becomes very difficult to isolate the causal effect. A quasi-natural experiment is the closest an experimental design can come in absence of an actual natural experiment.

DiD is a good tool to solve these issues, but two assumptions must hold in order for the estimation to be accurate.

1) *Parallel Trends Assumption (PTA)* This assumption ensures that the trend of the control group is equal to the trend of the treatment group in absence of treatment. Pre-treatment trends must be similar for the treatment and control groups. This can be visualized in a graph by comparing two lines: inventor mobility per year for both groups. The underlying assumption is that no other factors that impact the treatment and control groups change differentially over time. This assumption minimally holds in my dataset, as the figures in Appendix A show. Luckily, two-way fixed effects solves this issue partially, which will be touched upon later in this chapter.

To test the Parallel Trends Assumption I created 18 graphs with two lines, one representing the control group and the other the treatment group. For each of the 9 samples I first collapsed the dataset to state means, restored the dataset and then collapsed to assignee means. The 9 samples can be divided into three groups; sample with all 50 states, with 10 nonenforcing states and 3 states to assess choice-of-law provisions. For each sample I created a subsample with inventors with at least 2 patents, with inventors with at least 2 patents in at least 2 states and with inventors with at least 2 patents with at least 2 assignees. This leads to 18 graphs. All 18 are provided in Appendix A, two examples are provided in figure 2.

Figure 2 – Mean value of states (left) and mean value of assignees (right) per inventor plotted against year for samples 3.2 (left) and 1.2 (right).

The graphs in figure 2 provide evidence that for certain subsamples PTA holds. The subsamples for which it does not hold are 1.1, 1.3 (for assignees), 2.1 (for assignees), 2.2 (for states), 2.3, 3.1 (for assignees) and 3.3 for assignees.

2) *Stable Unit Treatment Value Assumption (SUTVA)* The second assumption for accurate DiD estimation is that the treatment effect is only due to the treatment and not due to interactions between members of the population. It is therefore essential that individuals cannot transfer between the treatment and control groups. This assumption cannot be visually tested, but it can be reasoned why this would hold or not. For the inventor mobility dataset this is rather straightforward. An inventor belongs to the treatment group if he or she has patented in California after 2007. If the inventor has done that, the inventor is virtually ‘tagged’, meaning that the inventor can move wherever he or she wants, but will not escape the fact that the inventor has a patent in California after 2007. This study is interested in the difference in movement behaviour of inventors with more than 2 patents compared to the movement behaviour of inventors with more than 2 patents that have patented in California after 2007. If an inventor has not patented in California after 2007, the inventor is placed in

the control group. It is therefore not possible to switch between treatment and control groups, indicating that SUTVA holds.

4.1.2 Negative binomial regression

The next approach used in this thesis is a negative binomial regression. This model is appropriate due to the count data nature of the dependent variables; number of states and number of assignees. A count data model models the probability that a value takes 1,2,3, etc. as a function of explanatory variables (Bago d'Uva, 2022). Wooldridge (2012) found that OLS regressions work best when a dependent variable is continuous and normally distributed, but due to the discrete nature of counts they are not normally distributed, but follow a Poisson or negative binomial distribution. Therefore, these models will be used to calculate the probability that an inventor patents in multiple states and the probability that these patents have multiple assignees as a function of changes in NCA regulation in California. The model tests if the probability of having higher mobility depends on the DiD estimators (interaction effect between patenting in California after a given year). Results on the negative binomial regressions will serve as robustness checks for the difference-in-difference estimations.

The starting point for count data models is a Poisson regression model. However, this model is likely to be misleading unless restrictive assumptions are met because individual counts are usually more variable than is implied by the model. To be precise, Poisson models are accurate under the strict assumption of equidispersion, meaning that the sample mean is equal to the sample variance; $E(Y) = VAR(Y)$. The dataset is characterized by overdispersion when the variance is larger than the mean (Gardner, Mulvey & Shaw, 1995). Table 6 displays the mean and variance for the count variables states, assignees, patents and logarithmic transformations for the main sample with all 50 states.

Table 6 – Mean and variance of states, assignees and patents

Variable	Obs	Mean	Variance
states	168276	1.088	0.103
assignees	168276	1.331	0.440
Patents	168276	5.3	34.447
Log_states	168276	0.058	0.041
Log_assignees	168276	0.205	0.134

The table shows that *patents* displays signs of overdispersion. Therefore a Poisson model is not appropriate. In a negative binomial regression model, a random term reflecting unexplained between-subject differences is included in the regression model and relaxes the assumption of equidispersion. The equidispersion assumption can be relaxed due to the addition of a heterogeneity parameter in a negative binomial regression to account for the overdispersion (Hilbe, 2011; Liu et al., 2005).

Therefore, a negative binomial model will be used when modelling number of patents. This is not a mobility variable, but is still added to provide a complete overview of inventor activity. The variances for *states* and *assignees* are smaller than the mean, thus displaying signs of underdispersion. This indicates that the observed data is less variable than the model allows. A negative binomial regression with its relaxed equidispersion assumption is more applicable than a Poisson model in the case of underdispersion as well. Therefore, negative binomial regression models will be used when modelling *states* and *assignees* as well (Long & Freese, 2006).

In conclusion, the count data models predict the expected number of moves based on the mobility behaviour of inventors.

4.1.3 Regression Discontinuity Design (RDD)

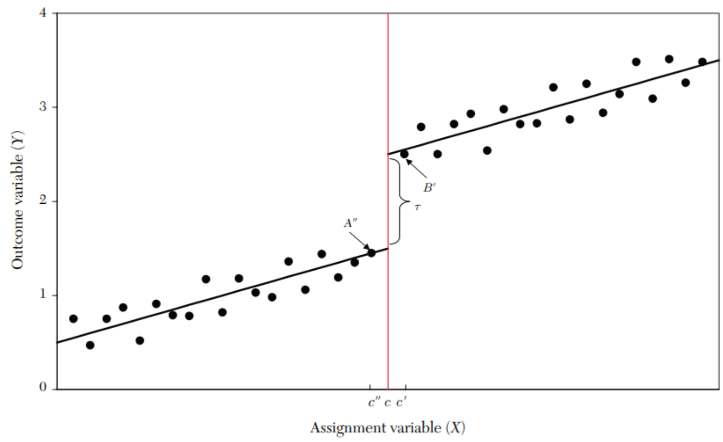
Lastly, an RDD approach will be used to analyse the impact of the Defend Trade Secrets Act of 2016 in hypothesis 2. I use an RDD approach rather than a DiD approach, because the DTSA is a national law and changes in California legislation are at the state level. Therefore, there is no viable control group to difference out the causal effect of the policy change. However, RDD is a helpful alternative. In short, Regression Discontinuity Design is a quasi-experimental evaluation option, just like DiD, that measures the impact of an intervention or treatment, by applying a treatment assignment mechanism based on a continuous eligibility index which is a variable with a continuous distribution. In the context of the national installation of DTSA, RDD can be used to measure the difference in mobility of inventors clustered around the defined cut-off point. This necessitates the determination of a 'bandwidth' around the cut-off point within which individual units are shown to be statistically comparable. If the assumptions hold, the difference in outcomes between those above and below the cut-off can be attributed to the program.

The cut-off used is 2017. As DTSA came into law on May 11th 2016, effects will not be visible at the beginning of 2016. As the dataset does not contain month-to-month data, I am obligated to use 2017 as a cut-off. This is not a problem, as a lag in the effect of DTSA on inventor mobility is feasible. It takes a while for judges to incorporate newly curated noncompete legislation in their rulings. I am aware that RDD designs usually exploit stricter cut-offs, but setting a cut-off in 2017 is the closest I can get to estimating the causal effect of DTSA on inventor mobility with this dataset.

I will use a fuzzy RDD approach. This approach is used in instances where treatment is assigned, but not all individuals are actually influenced by the treatment. The assignment rule defines treatment status probabilistically, not perfectly. Thus, the design is fuzzy. The fuzziness with DTSA may result from many factors. Examples are imperfect compliance by firms, imperfect and differential implementation by judges across states, the imperfect recognition of cases where trade secret law is applicable and that states are not completely bound by this piece of federal legislation. Therefore, a fuzzy RDD approach is more applicable than a sharp RDD. The two groups that will be compared are inventors with patents in California and inventors without patents in California.

A drawback of RDD is that the results are limited externally valid, due to the fact that results are only generalizable around the cut-off. (Hahn, Todd & van der Klauw, 2001). On the other side, the main advantage is that RDD comes very close to a randomized trial close to the cut-off due to local randomization, meaning that individuals have no control if they receive the treatment or not (Lee and Lemieux, 2010). RDD is best illustrated by a figure.

Figure 3 – Example of an RDD setting. Source: Lee and Lemieux, 2010



The running or assignment variable for hypothesis 2 is year. This calls upon the question whether time series analysis is more appropriate. I follow Ito (2015), who uses a time threshold in his study that determines eligibility for an electricity rebate program. Customers who initiated service after the threshold date were ineligible, while those before were eligible. This is similar to the group that is not influenced by DTSA before 2017 and is influenced after 2017 (Hausman & Rapson, 2017).

4.2 Models

4.2.1 Hypothesis 1

First, I hypothesize that inventor mobility will decrease in California due to the passing of exemptions on Business and Professions Code 16600 in 2007 regarding business owners. I will answer this hypothesis by using a DiD with two dependent variables as a proxy for inventor mobility, *states* and *assignees*. I will run the models for 2 samples. The first set of samples contains 50 states and the second set 10 nonenforcing states. Both samples have 3 subsamples, one with the only condition that inventors have at least 2 patents, the second with two conditions that inventors have at least 2 patents in at least 2 states and the third with two conditions that inventors have at least 2 patents with at least 2 assignees.

Additionally, I use three count data models, models 4, 5 and 6 for the 2 samples.

$$States = \beta_0 + \beta_t * post\_2007 + \gamma_c * California + \delta * CA\_post\_2007 + \mu_n * D_{state} + \theta_n * D_{year} + \alpha_i + u_{it} \quad (2)$$

$$Assignees = \beta_0 + \beta_t * post\_2007 + \gamma_c * California + \delta * CA\_post\_2007 + \mu_n * D_{state} + \theta_n * D_{year} + \alpha_i + u_{it} \quad (3)$$

Equations 2 and 3 represent difference-in-difference estimation with β_t as a time indicator, γ_c as a location indicator, δ as the interaction term between the time and location indicator, μ_n as state fixed effects denoted as a dummy per state and θ_n as year fixed effects denoted as a dummy per year. Furthermore, α_i denotes the fixed part of the error term, the unobserved unit heterogeneity and u_{it} denotes the time-varying part of the error term.

Equations 4, 5 and 6 describe the negative binomial models with *states, assignees* and *patents* as dependent variables and *post_2007, California* and *CA_post_2007* as independent variables. The other two dependent variables are added as independent variables in the equations where they do not serve as the dependent variable. State and year fixed effects are also included. These models will be used three times for the sample containing all 50 states and three times in the sample containing 10 nonenforcing states. Once with a subsample with the only condition that inventors have at least 2 patents, the second time with a subsample with two conditions that inventors have at least 2 patents in at least 2 states and the third time with a subsample with two conditions that inventors have at least 2 patents with at least 2 assignees. Once again, α_i denotes the fixed part of the error term, the unobserved unit heterogeneity and u_{it} denotes the time-varying part of the error term.

$$States = \beta_0 + \beta_1 * post_{2007} + \beta_2 * California + \beta_3 * CA_{post_{2007}} + \beta_4 * assignees + \beta_5 * patents + \mu_n * D_{state} + \theta_n * D_{year} + \alpha_i + u_{it} \quad (4)$$

$$Assignees = \beta_0 + \beta_1 * post_{2007} + \beta_2 * California + \beta_3 * CA_{post_{2007}} + \beta_4 * states + \beta_5 * patents + \mu_n * D_{state} + \theta_n * D_{year} + \alpha_i + u_{it} \quad (5)$$

$$Patents = \beta_0 + \beta_1 * post_{2007} + \beta_2 * California + \beta_3 * CA_{post_{2007}} + \beta_4 * states + \beta_5 * assignees + \mu_n * D_{state} + \theta_n * D_{year} + \alpha_i + u_{it} \quad (6)$$

4.2.2 Hypothesis 2

Hypothesis 2 hypothesizes that The Defend Trade Secrets Act of May 11th 2016 will lead to lower labour mobility among inventors in California. To answer this a Regression Discontinuity Design (RDD) with year as the running or assignment variable and number of states and number of assignees as outcomes variables. The RDD will therefore be used twice for the two measures of inventor mobility in sample 1.1. The models are constructed in equations 7 and 8.

$$states = \beta_0 + \tau * year + \beta 1 * California + +\mu_n * D_{state} + \theta_n \; * D_{year} + \; \alpha_i + u_{it} \quad (7)$$

$$assignees = \beta_0 + \tau * year + \beta 1 * California + +\mu_n * D_{state} + \theta_n \; * D_{year} + \; \alpha_i + u_{it} \quad (8)$$

Where $\mu_n * D_{state}$ denotes state fixed effects, $\theta_n \; * D_{year}$ year fixed effects and τ the running variable year. These regressions compare the impact of nationwide regulation on inventor mobility for Californian and non-Californian inventors. Furthermore, α_i denotes the fixed part of the error term, the unobserved unit heterogeneity, and u_{it} denotes the time-varying part of the error term.

4.2.3 Hypothesis 3

Hypothesis 3 hypothesizes that the introduction of Section 925(e) in California labour law in 2017 regarding the tolerance of choice-of-law provisions under strict circumstances enhances the negative effect between NCAs and labour mobility. A similar combination of DiD and negative binomial regressions will be used, so the models are the same as hypothesis 1, except for the fact that the regressions are run in a sample with only California, Delaware and New York. I will run regressions 2-6 each three times, once in a subsample with the only condition that inventors have at least 2 patents, the second time in a subsample with two conditions that inventors have at least 2 patents in at least 2 states and the third time in a subsample with two conditions that inventors have at least 2 patents with at least 2 assignees. The *post_2007* variable will be changed to *post_2017.* Equations 2 and 3 extrapolated to hypothesis 3 give equations 9 and 10.

$$States = \; \beta_0 + \beta_t * post\_2017 + \; \gamma_c * California + \; \delta * CA\_post\_2007 + \mu_n * D_{state} + \theta_n \; * D_{year} + \alpha_i + u_{it} \quad (9)$$

$$Assignees = \; \beta_0 + \beta_t * post\_2017 + \; \gamma_c * California + \; \delta * CA\_post\_2007 + \mu_n * D_{state} + \theta_n \; * D_{year} + \alpha_i + u_{it} \quad (10)$$

4.3 Two-way fixed effects

The difference-in-difference method attempts to control for unobserved variables that bias estimates of causal effects. However, pre-existing differences in the treatment and control group can be an issue. To account for that and the fact that the pre-treatment trends between the treatment and control group are not very similar, I added state and year fixed effects to the regression. According to Jakiela (2021), using two-way fixed effects to control for location-specific and period-specific shocks is common in DiD estimations that evaluate the impact of a program. The main advantage of adding unit fixed effects and time fixed effects to a regression is that one can control for time-invariant differences between units and time periods when control data is not obtainable (Woolridge, 2021).

When estimating an average treatment effect across locations and time periods, observations with below mean treatment intensity receive negative weight and may be thought of as part of the control group while in reality they are part of the treatment group. However, in the case of two-way fixed effects, it is outcomes with below mean levels of *residualized* treatment intensity – after controlling for state and year fixed effects – that receive negative weight.

The problem of negative weights can be solved with a sufficiently large never-treated group combined with enough pre-treatment data. This will guarantee that negative weights do not occur in the treatment group. However, in data sets with a limited number of pre-treatment periods, or with periods in which all or most units are treated, two-way fixed effects estimation will often put negative weight on the treatment effects in later periods for early-adopter units, thus leading to incorrect estimates.

Luckily, the problem of negative weights is not completely applicable in this thesis, so two-way fixed effects can be used as a control mechanism. When treatment effects are homogeneous, that is to say do not differ severely (as is the case here), the two-way fixed effects model is correctly specified. An OLS regression then correctly adjusts for the fact that the estimated fixed effects associated with high-treatment units and high-treatment periods are capturing some of the true treatment effect. When treatment effects are heterogeneous, then severe problems can occur (Goodman-Bacon, 2018). Imai and Kim (2020) also suggest that the ability of a two-way fixed effects model to simultaneously adjust for location- and time-associated unobserved variables critically relies upon the assumption of linear additive effects.

For example, Baker et al. (2022) show that estimates of the impact of banking deregulation on inequality in the United States are biased because the impacts of deregulation appear to grow larger over time. Hence, it is important to test whether difference-in-differences estimates derived from two-way fixed effects estimation are influenced by the inclusion of later years receiving negative weight in the calculation of the average treatment effect and whether the assumption of treatment effect homogeneity is plausible.

In conclusion, two-way fixed effects will be added to the regressions described above via the adding of state and year dummy variables.

5 Results

This chapter contains the results of the main models used to study the relationship between changes in noncompete agreement regulation and inventor mobility. However, not all models can be equally interpreted. An important assumption for difference-in-difference is the Parallel Trends assumption, explained in the previous chapter. In short, it requires that in the absence of treatment, the difference between the treatment and control groups is constant over time. If PTA is violated, the internal validity of the model is at risk and will generate biased estimations of the causal effect. There is no statistical test for this assumption, but visual inspection is useful. In Appendix A PTA is visually assessed with graphs plotting *states* and *assignees* between 2000-2021. Table 7 provides a summary of whether PTA holds or not for both dependent variables for all 9 subsamples (summarized in table 1).

Table 7 – List of subsamples and if PTA is violated

	Description	***States* PTA violated?**	***Assignees* PTA violated?**
Subsample 1.1	50 states, inventors with more than 2 patents	Yes	Yes
Subsample 1.2	50 states, inventors with more than 2 patents in more than 2 states	No	No
Subsample 1.3	50 states, inventors with more than 2 patents with more than 2 assignees	No	Yes
Subsample 2.1	10 states, inventors with more than 2 patents	No	Yes
Subsample 2.2	10 states, inventors with more than 2 patents in more than 2 states	No	Yes
Subsample 2.3	10 states, inventors with more than 2 patents with more than 2 assignees	Yes	Yes
Subsample 3.1	3 states, inventors with more than 2 patents	Yes	Yes
Subsample 3.2	3 states, inventors with more than 2 patents in more than 2 states	Yes	No

Subsample 3.3	3 states, inventors with more than 2 patents with more than 2 assignees	No	Yes

In conclusion, the models where the Parallel Trends assumption is not violated generate unbiased estimates of the causal effect of policy changes on inventor mobility. These models are 3, 4 and 5 from table 7, 7 and 9 from table 9 and 20 and 21 from table 12. The DiD estimators in these unbiased models are marked bold for further reference. These results will be interpreted below.

5.1 Hypothesis 1

5.1.1 Difference-in-difference with all 50 states

As explained in the previous chapter, I analyse the effect of the strictness of noncompete agreement legislation on inventor mobility by exploiting an exogenous change of the Californian Business and Professions Code that increases the tolerance of NCAs. In line with existing literature, I have hypothesized that increased tolerance to NCAs in California will lead to a reduction in inventor mobility, measured by number of states an inventor has patented in and number of assignees those corresponding patents have. I do so by using a difference-in-difference approach with multiple subsamples. The results are displayed below.

Table 8 – Results of the difference-in-difference regression with fixed effects with states and assignees as dependent variables for samples 1.1, 1.2 and 1.3

	(1) **states**	(2) **assignees**	(3) **states**	(4) **assignees**	(5) **states**	(6) **assignees**
California	-0.0452	0.0189	0.0917^{***}	0.214	-0.142	0.176^{*}
	(0.0475)	(0.0748)	(0.0116)	(0.149)	(0.120)	(0.0818)
post_2007	0.181^{***}	0.500^{***}	0.106^{***}	0.358^{***}	0.292^{***}	0.278^{***}
	(0.00421)	(0.00853)	(0.0115)	(0.0336)	(0.00980)	(0.0123)
CA_post_2007	-0.0215^{***}	-0.0193^{*}	**0.0137**	**0.0675**	**-0.0435^{**}**	0.0112
	(0.00305)	(0.00956)	(0.0124)	(0.0903)	(0.0141)	(0.0188)
Constant	1.093^{***}	1.254^{***}	1.905^{***}	1.931^{***}	1.263^{***}	1.961^{***}
	(0.0476)	(0.0748)	(0.0182)	(0.129)	(0.120)	(0.0818)
Observations	168276	168276	13323	13323	43253	43253

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 8 displays the results of the DiD regressions testing the effect of the changes made to the Californian Business and Professions Code that indicate the tolerance of NCAs under strict circumstances. The year and state dummies covering fixed effects have been omitted for sake of brevity. Models 1 and 2 contain all inventors with 2 or more patents in 50 states, models 3 and 4 contain all inventors with 2 or more patents in 2 or more states in 50 states and models 5 and 6 contain all inventors with 2 or more patents with 2 or more assignees in all 50 states.

The pre-treatment trends of sample 1 are not similar. Therefore, models 1 and 2 generate biased estimates of the causal effect of the policy change. However, the results do indicate that in a group of inventors that have proven to be mobile between firms, the change in California legislation has a negative effect on inventor mobility, keeping all else constant. This effect is significant at the 1% level. If an inventor has patented in California after 2007, that inventor patents on average in 0.0435 fewer states compared to inventors who have not patented in California after 2007 according to model 5, keeping all else constant. No significant effect on the number of assignees is found.

This finding is especially interesting in light of the significant and positive coefficient of *post_2007* for all six models. This indicates that for the entire sample of inventors, patenting after 2007 has a positive effect on the number of states s inventor patents in and on the number of assignees an inventor has. However, this positive effect is completely taken away if these inventors patent in California after 2007. This provides evidence that the increased tolerance of NCAs in California after 2007 decreases inventor mobility.

When doing the same analysis on a subsample with inventors that have already proven to be mobile between states, the significance of the DiD-estimator disappears. This means that patenting in California after 2007 does not have a significant effect on inventor mobility for a group of inventors that already have proven to be mobile between states, keeping all else constant.

5.1.2 Difference-in-difference with 10 nonenforcing states

An important part of DiD estimation is the curation of a viable control group. Table 8 uses all 49 states (besides California) as a control group. These states differ significantly in terms of tolerance of noncompete agreements. To create a more viable control group, I follow Marx, Strumsky and Fleming (2009) and use nine states with a similar anti-NCA stance to California as a control group. The trend in inventor mobility in these states should be an appropriate estimate of the counterfactual trend in California, that is to say, what would have happened with Californian inventors if NCA regulation had not changed. The results of the DiD model with Oklahoma, North Dakota, Alaska, Nevada, Washington, Montana, Minnesota, West Virginia, and Connecticut as a control group as displayed in table 9.

Table 9 – Results of the difference-in-difference regression with fixed effects with states and assignees as dependent variable for samples 2.1, 2.2 and 2.3

	(7) states	(8) assignees	(9) states	(10) assignees	(11) states	(12) assignees
California	-0.0524	-0.0227	0.0232	-0.188	-0.194	0.102
	(0.0371)	(0.0759)	(0.0161)	(0.447)	(0.130)	(0.106)
post_2007	0.0389***	0.390***	0.00908	0.108	0.0993***	0.241***
	(0.00413)	(0.0175)	(0.0122)	(0.183)	(0.0169)	(0.0327)
CA_post_2007	**-0.0144***	0.00947	**-0.0163**	0.190	-0.0701***	-0.00540
	(0.00271)	(0.0139)	(0.0137)	(0.382)	(0.0165)	(0.0321)
Constant	1.059***	1.301***	1.974***	2.181***	1.200***	2.069***
	(0.0372)	(0.0772)	(0.0171)	(0.361)	(0.130)	(0.109)
Observations	59269	59269	889	889	11384	11384

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Again the year and state dummies covering fixed effects have been omitted in table 9 for sake of brevity. Models 7 and 8 contain all inventors with 2 or more patents in 10 states, models 9 and 10 contain all inventors with 2 or more patents in 2 or more states in 10 states and models 11 and 12 contain all inventors with 2 or more patents with 2 or more assignees in all 10 states.

The results are slightly different than in table 8. In the group of inventors with multiple patents, the change in California legislation has a negative effect on inventor mobility, keeping all else constant, according to model 7. Models 7 and 9 generate unbiased results, because they do not violate PTA. If an inventor has patented in California after 2007, that inventor patents on average in 0.0144 less states compared to inventors who have not patented in California after 2007, keeping all else constant. This effect is highly significant at the 0.1% level The fact that using this sample also generates significant results solidifies the notion that the change of law had a negative effect on inventor mobility.

These findings are also interesting in the light of the significant and positive coefficient of *post_2007* in model 7. This indicates that for the sample with 10 states, patenting after 2007 has a positive effect on the number of states an inventor patents in. However, this positive effect is completely taken away if these inventors patent in California after 2007. This provides evidence that the increased tolerance of NCAs in California after 2007 decreases inventor mobility in terms of states, not in terms of number of assignees.

When doing the same analysis on a smaller subsample with inventors that have already proven to be mobile between states, the significance of the DiD-estimator once again disappears. This means that patenting in California after 2007 does not have a significant effect on inventor mobility, keeping all else constant.

Lastly, the DiD analysis with a smaller subsample with inventors that have already proven to be mobile between firms, PTA is violated. This makes interpretation of the results for models 11 and 12 unreliable.

In conclusion, I find partial support for hypothesis 1 when mobility is measured by the number of states an inventor patents in, not the number of firms. Two findings from tables 8 and 9 generate this support:

1) In table 8, the DiD estimation using the sample with all 50 states and inventors that have proven to be mobile between firms reveals a significant and negative relationship between the additions to the Californian Business and Professions Code and inventor mobility measured in terms of states an inventor patents in (model 5).
2) In table 9, the DiD estimation using the sample with 10 nonenforcing states and inventors with at least two patents also reveals a significant and negative relationship between the additions to the Californian Business and Professions Code and inventor mobility measured in terms of states an inventor patents in (model 7).

5.2 Hypothesis 2

Hypothesis 2 tests if The Defend Trade Secrets Act of May 11th 2016 will lead to lower labour mobility among inventors in California. To test this, I use a fuzzy regression discontinuity design. Figure 4 below displays the mean number of states an inventor patents in per year and figure 5 displays the mean number of assignees an inventor has. These plots are based on sample 1.1 containing all 50 states with inventors with at least two patents. The discontinuity is tested in 2017, as shown by the vertical line in both plots. The graphs show that the mean number of states per year shows a discontinuity in 2017, while the mean number of assignees does not in 2017.

Figure 4 – Graphical analysis of regression discontinuity for number of states

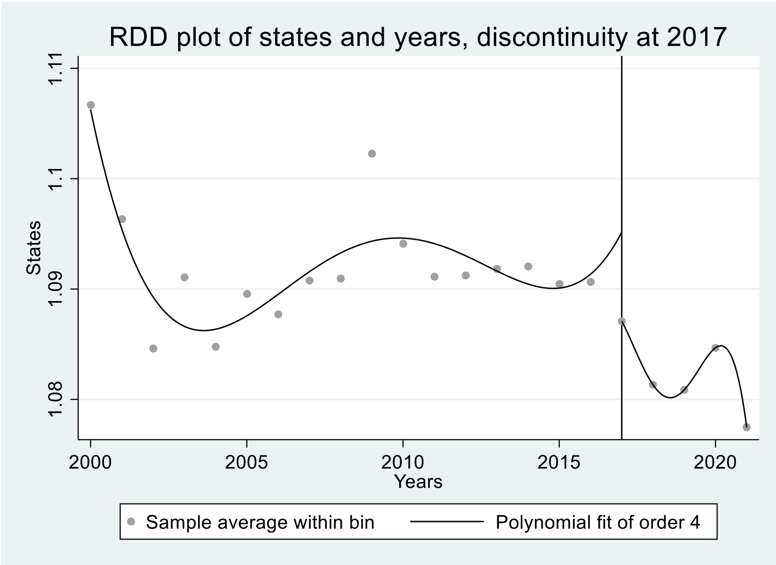


Figure 5 – Graphical analysis of regression discontinuity for number of assignees

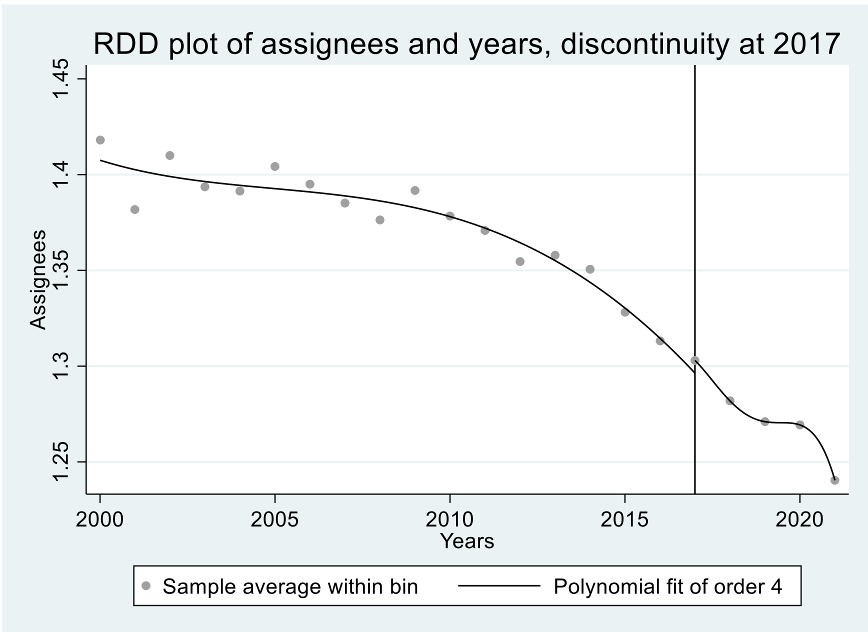


Table 10 – Regressions testing the significance of discontinuity for Californian inventors

	(2015) states	(2019) states	(2015) assignees	(2019) assignees
California	-0.0293***	-0.0177**	-0.0137	-0.0158
	(0.00793)	(0.00565)	(0.0186)	(0.0109)
Constant	1.099***	1.086***	1.332***	1.276***
	(0.00423)	(0.00305)	(0.00992)	(0.00588)
Observations	8468	14357	8468	14357

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 10 tests the significance of the discontinuity for Californian inventors. A bandwidth of 2 years is taken on both sides of the treatment year, meaning that models 1 and 3 are based on 2015, while models 2 and 4 are based on 2019. The results are negative and significant for *states* and negative and insignificant for *assignees*. This is in line with the graphical evidence shown in the graphs above.

For the number of states, there is a causal treatment effect between the cut-off years, 2015-2019. The results for *states* suggest that DTSA increased the number of states a Californian inventor patented in by 0.0116 states on average at the discontinuity. Still, the coefficient has a negative sign, but the relevant finding is that the magnitude of the effect is less negative in 2019 compared to 2015. This indicates that while there are signs of a negative discontinuity for the whole sample, the effect is positive for Californian inventors at the discontinuity. California has always been an oddball regarding state legislative actions and this finding enforces this notion; the trend for inventors from all 49 states between 2015 and 2019 regarding number of states in which they patented decreased (figure 4), while Californian inventors started patenting in more states in the same period.

5.3 Hypothesis 3

5.3.1 Difference-in-difference with 3 states

Lastly, I also use a difference-in-difference estimation technique to test if the allowance of choice-of-law provisions in California on January 1st 2017 has a moderating effect on the negative effect of more noncompete agreements on inventor mobility. I use sample 3 which contains inventors that have a patent in California, New York and Delaware. The literature has shown that New York and Delaware are the states where choice-of-law provisions are most common (Sanga, 2014; Eisenberg and Miller, 2008). Therefore, California converges toward these states by allowing choice-of-law provisions under strict conditions, making New York and Delaware a viable control group. The results are displayed in table 11.

Table 11 – Results of the difference-in-difference regression with fixed effects with states and assignees as dependent variable for samples 3.1, 3.2 and 3.3

	(17) **states**	(18) **assignees**	(19) **states**	(20) **assignees**	(21) **states**	(22) **assignees**
California	-0.00776***	0.0331***	0	-0.0429	-0.0495***	0.00740
	(0.00170)	(0.00895)	(.)	(0.122)	(0.00883)	(0.0190)
post_2017	0.0326***	0.420***	0	0.0107	0.0734***	0.243***
	(0.00388)	(0.0156)	(.)	(0.0969)	(0.0138)	(0.0290)
CA_post_2017	-0.0210***	-0.0681***	0	**0.212**	**-0.0613*** **	-0.0246
	(0.00327)	(0.0136)	(.)	(0.163)	(0.0135)	(0.0295)

Constant	1.016***	1.292***	2	2.282***	1.077***	2.202***
	(0.00381)	(0.0200)	(.)	(0.188)	(0.0150)	(0.0388)
Observations	49377	49377	400	400	10050	10500

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Again the year and state dummies covering fixed effects have been omitted in table 11 for sake of brevity. Models 17 and 18 contain all inventors with 2 or more patents in 3 states, models 19 and 20 contain all inventors with 2 or more patents in 2 or more states in 3 states and models 21 and 22 contain all inventors with 2 or more patents with 2 or more assignees in all 3 states.

As the table shows, the DiD estimators with *states* and *assignees* as dependent variables are significant and negative for the sample with all inventors with more than two patents. However, the estimates for this sample with *states* and *assignees* are both biased, as the Parallel Trends assumption does not hold for models 17 and 18. If PTA was not violated, model 17 would indicate that the change of law regarding the tolerance of choice-of-law provisions in 2017 leads to a 0.0210 decrease in number of states a Californian inventor patents in, *ceteris paribus*.

The subsample with inventors with at least two patents in at least two states does not yield any significant results. The subsample with inventors with at least two patents with at least two assignees generates a significant and negative DiD estimator, which is in line with the findings of model 17. According to sample 3.3, the change of law regarding the tolerance of choice-of-law provisions in 2017 leads to a 0.0613 decrease in number of states a Californian inventor patents in, *ceteris paribus*. Therefore, I find one reliable source of evidence that inventor mobility decreased in California due to the installation of section 925(e) in the Californian Labour Code.

Therefore, allowing choice-of-law provisions leads to a decrease in inventor mobility of Californian inventors after 2017. I hypothesized that the tolerance of COL provisions under strict circumstances leads to a further reduction of Californian inventor mobility. This is because COL provisions indirectly create increased tolerance for NCAs by establishing a means to circumvent anti-NCA legislation in states such as California. The one negative, significant and unbiased coefficient provides partial evidence that employers use COL provisions to circumvent anti-NCA legislation in California as mobility further decreases after 2017, according to this dataset. However, I am aware of the limitations of this approach, which will be discussed further on.

6 Conclusion and Discussion

6.1 Conclusions

This study has approached the relationship between noncompete agreement legislation and inventor mobility from different angles. First, using two proxies for mobility, between states and between firms, allows for a broader scope. Second, the use of subsamples tells an interesting story as well. The difference between the main three samples is the number of states, thus crafting a specific control group enabling research in a different part of the NCA legislation spectrum. Thirdly, the subsamples form the final facade. The discrepancy is between inventors who have the potential to be mobile (more than 2 patents), inventors who have proven to be mobile between states and inventors who have proven to be mobile between firms. Different angles provide different insights into how inventor mobility is caused by slight but meaningful changes in noncompete legislation. For example, I find that noncompete legislation only influences the number of states an inventor patents in, not the amount of firms an inventor patents at.

This study analyses the beforementioned relationship by exploiting exogenous additions to state and national legislation. These exogenous additions are the addition of sections 16601, 16602 and 16602.5 to the Californian Business and Professions Code on January 1st 2007 which allow the use of noncompete agreements in case of the sale or dissolution of corporations (16601), partnerships (16602), and limited liability corporations (16602.5). The exogenous addition is the creation of the Defend Trade Secrets Act on May 11th 2016, a federal law that allows an owner of a trade secret to sue in federal court when its trade secrets have been misappropriated. The third exogenous addition is section 925(e) of the Californian Labour Code on January 1st 2017, which permits California employees to *voluntarily* agree to apply another state's law to his or her contract when counsel represents them, thus weakening California's anti-NCA policy. The effects of these additions to Californian and national legislation on inventor mobility are the main focus of this study.

In line with existing literature, I have hypothesized that increased tolerance of NCAs in California will lead to a reduction of inventor mobility, measured by the number of states an inventor has patented in and the number of assignees those corresponding patents have. I do so by using fixed effects difference-in-differences with multiple subsamples, a regression discontinuity design and negative binomial regressions as robustness checks. These models will answer the question of what would have happened with Californian inventors if noncompete regulation had not changed.

6.1.1 Conclusion to hypothesis 1

Compared to other nonenforcing states, inventor mobility will decrease in California due to the passing of exemptions on Business and Professions Code 16600 in 2007 regarding business owners.

This study uses a difference-in-difference approach with year and state fixed effects and two different control groups to answer hypothesis 1. The results of this estimation reveal that in certain configurations of the group of inventors a significant and negative relationship exists between the additions to the Californian Business and Professions Code and inventor mobility measured in terms of states an inventor patents in. Therefore, I find partial evidence for hypothesis 1. If the relationship would hold for all configurations of the group of inventors and for the number of firms an inventor patents at, this would count as full support of the hypothesis.

This conclusion is especially interesting in light of the positive and significant coefficients for *post_2007*, which indicate that for the entire sample of inventors, patenting after 2007 has a positive effect on the number of states an inventor patents in and on the number of assignees an inventor has. However, this positive trend over time in employee mobility does not apply to inventors that patent in California. The elimination of this positive trend is due to the increased tolerance and use of noncompete agreements in California after 2007.

6.1.2 Conclusion to hypothesis 2

The Defend Trade Secrets Act of 2016 will lead to lower labour mobility among inventors in California.

The second hypothesis of this study tests if changes in noncompete regulation on a national scale impact inventor mobility in California as well. The Defend Trade Secrets Act was chosen, as it formalized the use of NCAs when trade secrets were involved. More specifically, this act allows an owner of a trade secret to sue in federal court when its trade secrets have been misappropriated. While states are not completely bound by this piece of federal legislation, it does impact the rulings state courts make. As courts are the ones to uphold or deny a noncompete agreement, this change of law is relevant.

To test if DTSA reduced labour mobility a regression discontinuity design was used to find evidence for disruptions in the trend of number of states an inventor patents in and number of firms an inventor patents at in 2017.

I find evidence of a significant disruption in 2017 for the number of states an inventor patents in. More specifically, for the number of states there is a causal treatment effect between the cut-off years, 2015-2019. A two-year bandwidth was chosen. The results suggest that DTSA increased the number of states a Californian inventor patented in by 0.0116 states on average at the discontinuity in 2017. This indicates that while there are signs of a negative discontinuity for the whole sample, the effect is positive for Californian inventors at the discontinuity. California has always been an oddball regarding state legislative actions and this finding enforces this notion; the trend for inventors from all 49 states between 2015 and 2019 regarding number of states in which they patent decreased (figure 4), while Californian inventors started patenting in relatively more states in the same period.

Therefore, I reject hypothesis 2. To be precise, I find the opposite of what I excepted. There was a discontinuity in 2017, indicating that DTSA played a role. However, the effect on the number of states an inventor patents in was negative for the control group, while positive for Californian inventors. This may indicate a lack of enforcement of DTSA in Californian courts or relatively less use of trade secrets by Californian businesses compared to companies in other states.

6.1.3 Conclusion to hypothesis 3

The introduction of Section 925(e) in California labour law in 2017 regarding the tolerance of choice-of-law provisions under strict circumstances leads to a further reduction of inventor mobility.

I approach the third hypothesis with a difference-in-difference design with state and year fixed effects. I curated a control group consisting of New York and Delaware, that have a history of allowing choice-of-law provisions. I compare these two states with California, which started tolerating choice-of-law provisions under strict circumstances due to section 925(e). Theoretically, inventor mobility should decrease even more in California to meet the New York and Delaware trends after 2017.

I find evidence in one subsample that inventor mobility decreased in California due to the installation of section 925(e) in the Californian Labour Code measured in *states*, not in *assignees*. This piece of evidence supports the notion that choice-of-law provisions are a tool to circumvent anti-NCA legislation. Sample 3.3 generates a negative and significant coefficient for the DiD estimator for *states* as the dependent variable, while not violating the Parallel Trends assumption. In conclusion, I find partial evidence supporting hypothesis 3.

6.1.4 Conclusion to the research question

Do changes in Californian state and national legislation that increase tolerance of noncompete agreements decrease labour mobility amongst Californian inventors?

I exploited three changes in California state and national law to research three hypotheses. These three hypotheses combined form an answer to the research question. The first hypothesis found a negative, significant and unbiased effect on inventor mobility measured by the number of states an inventor patents in for two of the twelve difference-in-difference models, not the number of assignees an inventor has. The second hypothesis found a significant discontinuity in 2017 in terms of the number of states an inventor patents in, not in terms of the number of assignees an inventor has. Lastly, the third hypothesis also found a negative, significant and unbiased effect on inventor mobility measured by the number of states an inventor patents in for one of the six difference-in-difference models, not the number of assignees an inventor has. This is a similar finding to the first hypothesis.

Based on these results I conclude that changes in Californian state and national legislation that increase tolerance of noncompete agreements decrease labour mobility amongst Californian inventors when inventor mobility is measured in *states*, not *assignees*.

6.2 Discussion

6.2.1 Limitations

The conclusions must be interpreted with caution due to several limitations. First of all, the lack of control variables on the individual inventor level possibly led to overestimation of the causal effects due to omitted variable bias. This opens the possibility of alternative explanations for inventor mobility than changes in noncompete legislation. While I do use two-way fixed effects to control for unobserved time-invariant unit heterogeneity, there are most likely other factors that impact inventor mobility that are not added to the models. Control variables on the inventor level such as income, preferences for living in a certain state and other underlying characteristics that justify a move between states or firms were unavailable. Also, using state level control variables that influence the decision to move to a new state were difficult to add to the dataset due to matching issues.

These issues are why previous research in this field has used surveys instead of patent data to measure inventor mobility. Another disadvantage of using patent data instead of a survey is mentioned by Marx, Strumsky and Fleming (2009), namely that the dataset cannot determine whether job changes are voluntary or involuntary. A move to another state or firm because an inventor got fired is different than voluntarily switching states or firms due to inconvenient policy changes. However, creating a nationwide survey for high-skilled employees at American firms, such as done by Prescott, Bishara and Starr (2016), simply exceeds the scope of this study.

Another limitation is possible misinterpretation of the results of hypothesis two. The running variable in regression discontinuity is mostly a continuous variable such as grades, weight and distance, not a time variable as used in this study. Ito (2015) uses a similar approach. As mentioned earlier, in his paper customers who initiated service after the threshold date were ineligible, while those before were eligible. In this study, inventors all became eligible in 2017, thus relying on comparing a control group from before 2017 and a treatment group from after 2017. An alternative approach could be time series analysis. However, the dataset of individual inventors is not suitable for this kind of analysis, as time series requires recorded data points at consistent intervals over a set period of time. The dataset used in this study is not designed in such a way, nor would such an approach be relevant for measuring the number of moves an inventor makes. Therefore, the case of the Defend Trade Secrets Act is not completely suitable for RDD and time series analysis. I created a combination of the two, just like Ito (2015), to find a way to estimate the causal effect of instating the DTSA on inventor mobility in California. However, the limitation of lack of control variables returns here. While the graphs in figures 4 and 5 are interesting, the statistical tests determining the significance of the discontinuity are likely to be biased due to the influence of unobserved differences between individuals, states and years.

Furthermore, I ran into data collection issues. I did not succeed in obtaining data on choice-of-law provisions in noncompete agreements or choice-of-law provisions in employment contacts in general. I contacted the Structured Data department at the Securities and Exchange Commission, which advised me to reach out to the Division of Corporation Finance's Office of Chief Counsel. Unfortunately, choice-of-law provisions data is not structured or systematically collected to allow analysis. Still, COL provisions is a topic of growing importance within the discussion of noncompete agreements. I sought a way to research this topic with the dataset I created. Ideally, I would have added a dummy variable for each individual inventor indicating if that inventor has a choice-of-law provision in their noncompete agreement or not. As this was not possible, I created a difference-in-difference estimation to compare California with New York and Delaware. I obtain reliable results, but a model with a choice-of-law provision dummy would estimate the causal effect on inventor mobility more accurately.

Lastly, external validity may be an issue. This study focuses on inventor mobility in California, making it a case study for the state of California. The state is over-represented in the data, with 25,61% of all observations pertaining to inventors with a patent in California. Also, the question arises whether the subtle changes of law can impact mobility in other states. The impact on mobility this study found for California was significant but not overwhelming. As California is the state with the most extreme stance on noncompete agreements, how would similar changes in the law allowing increased tolerance of NCAs impact, for example, mobility in Pennsylvania? What would happen if a state that allows NCAs starts promoting the use of them? Would mobility decrease even further or is the relationship only significant when a state has anti-NCA legislation in the first place? In addition, California is an oddball in the United States in terms of culture, weather, innovation and countless other measures. These factors could attract inventors just as much as anti-NCA legislation.

6.2.2 Contributions to existing research on noncompete agreements and mobility

This study makes three important contributions to research regarding noncompete agreements and inventor mobility, despite its limitations. Besides being relevant for economic research, this study also makes contributions that are relevant from a policymaker's perspective. I provide evidence that the tools that policymakers have, namely to add legislation, are effective in terms of affecting inventor mobility.

First of all, this study adds to the Michigan experiment by Marx, Strumsky and Fleming (2009). In their paper, they research a policy change that abolishes noncompete agreements completely and find that this has a positive effect on inventor mobility. This study researches if subtle additions to legislation increasing tolerance towards noncompete agreements evidently have a negative effect on inventor mobility. I find that this is indeed the case in certain subsamples, thus strengthening the findings of Marx, Strumsky and Fleming (2009). The relationship they found holds in the opposite direction for more subtle changes of law as well.

Secondly, I find that noncompete agreement legislation impacts the number of *states* on inventor patents in, while it does not impact the number of *firms* an inventor patents at. This is not surprising. It is likely that firm-specific characteristics have a larger role in explaining why an inventor moves to a different firm than state legislation. This study researches state and national legislation, which logically would have a greater impact on the decision to move to another state than the decision to move to another firm.

Lastly, different types of inventors yield different results. This study creates three groups: inventors that have the potential to move, inventors that have proven to be mobile between states and inventors that have proven to be mobile between firms. The subsample analysis uncovers these differences. Significant results were found for inventors proven to be mobile between firms and inventors that have the potential to be mobile, but not for inventors that have previously been mobile between states. This may indicate that there is a limit to how many states an inventor wants to move to. The results also uncover that inventors that apparently are open to changing firms, are also more liable to change states due to changes in noncompete agreement legislation, which is an important contribution to this field.

6.2.3 Suggestions for future research

Several questions remain unanswered in the study which would be interesting to analyse in future research, some already mentioned in the limitations section. For example, the question remains how firms will protect trade secrets and other confidential information. As the American Bar Association

(2022) puts it: "*How, then, are these employers able to protect their goodwill, safeguard their trade secrets, and prevent unfair competition while still providing the employee the required advance notice*". Exploiting policy changes regarding trade secrets at the state level could be an interesting follow-up study.

This study has focused on the impact of noncompete agreements in California in the period from 2000-2021. NCAs are an effective tool to protect competitive advantages, it is not the only tool. Future research can focus on the effect of other noncompete covenants, such as non-disclosure agreements. For low-wage workers, a *springing noncompete provision*, an NCA that becomes active in case a certain event occurs, can be added to safeguard business information available to all employees within a firm, including low-wage workers. In my opinion, a springing noncompete provision is the optimal way to balance the advantages and disadvantages of NCAs for employees and employers.

A broader area for follow-up research could analyse start-up rates in light of the 2007 changes as they apply to business owners. Allowing restrictive covenants for business owners could have a positive effect on start-up rates in a certain state. Individuals could be more willing to start a business with an enforceable NCA, knowing that if their business partner leaves the firm, that the existing business can still thrive without increased competition. On the other hand, the signing of an NCA with a business partner limits the area in which a partner can start a new business. This could subsequently decrease the number of new firms founded in a certain area. This is especially interesting in California in light of Gilson (1999), who found evidence that California's anti-NCA stance has significantly contributed to the emergence of Silicon Valley.

Lastly, a time-series approach to uncover the causal effects of noncompete agreement legislation can be used. I suggest using a survey project to collect data. Even though it is costly and time-consuming, the added value of having data on individual-level control variables is significant and would solve one of the main limitations of this study. Time series analysis would be especially interesting for policymakers due to its forecasting capabilities, which can be used to predict the impact of changes to noncompete legislation.

Further research is still needed to answer other questions that remain unanswered in this thesis. For example, can states mitigate the negative effect of increased use of NCAs by promoting start-ups? Do inventors fundamentally differ from each other in terms of how willing they are to move? Can higher salaries offset the negative effects employees experience due to NCAs? Also, what role do courts precisely play and does their influence differ per judge? Is there a time lag until policy changes regarding noncompete agreements actually influence inventor mobility? These questions can be the foundation of future research in economics and law.

7 Robustness checks

7.1 Negative binomial regression approach

A different way to approach the relationship between noncompete agreements and inventor mobility is to use negative binomial regressions rather than difference-in-difference. DiD is only unbiased when the main assumptions, Parallel Trends and Stable Unit Treatment Value, are upheld. In reality, this proves to be difficult for the majority of the models estimated in this thesis (see table 7). Therefore, this section contains the same analysis, the effect of changes in noncompete agreement legislation on inventor mobility, but with a negative binomial regression approach rather than difference-in-difference. The models are described in chapter 4, Methodology with equations 4, 5 and 6. The count data models predict the expected number of moves based on mobility behaviour of inventors and the average marginal effects. The results of all the negative binomial regressions are displayed below. The discussion of the results will focus on the interaction terms and compare them with the DiD estimators computed in the Results section.

7.1.1 Hypothesis 1

Negative binomial regression with 50 states

Table 12.1 is the negative binomial equivalent to table 8. The interpretation of these models is different than DiD due to the logistic nature of the model, rather than linear. The interpretation of the negative binomial regression coefficient is as follows: for a one unit change in the predictor variable, the difference in the logs of expected counts of the response variable is expected to change by the respective regression coefficient, keeping all else constant. This is difficult to interpret and does not give a sense of magnitude, only of significance and sign. Therefore, I also compute the average marginal effects to calculate probabilities. Marginal effects can be interpreted as follows: an inventor that patented in California after 2007 is predicted to patent in 0.3 more states on average compared to the control group, keeping all else constant, for example.

Table 12.1 – Results of the negative binomial regressions with states, assignees and patents as dependent variables for sample 1.1

	(1) states	(2) assignees	(3) patents
main			
post_2007	0.0966***	0.175***	0.636***
	(0.0116)	(0.0104)	(0.00966)
CA_post_2007	-0.0152	-0.00678	0.0412*
	(0.0195)	(0.0179)	(0.0164)
assignees	0.138***		0.357***
	(0.00196)		(0.00319)
patents	-0.00179***	0.0152***	
	(0.000412)	(0.000310)	

states		0.460***	-0.170***
		(0.00435)	(0.00652)
Constant	-0.0852	-0.306**	0.563***
	(0.126)	(0.116)	(0.112)
/ lnalpha	-52.03	-26.28	-0.903***
	(.)	(.)	(0.00462)
Observations	168276	168276	168276

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 12.2 – Results of the corresponding average marginal effects for sample 1.1

	(1.1: states)	1.1: assignees	1.1: patents
post_2007	0.105***	0.227***	3.208***
CA_post_2007	-0.016	0.009	0.208*
assignees	0.149***	0.020***	1.798**
patents	-0.002***		
states		0.597***	-0.856***

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

In table 12.1 the fixed effects estimators have been omitted to ensure brevity. Model 1 shows a positive and significant effect at the 0.1% level for *post_2007*. Therefore, patenting after 2007 leads to an increase in the number of states an inventor patents in. The same goes for the number of assignees. Also, having more patents indicates patenting in fewer states. Models 2 and 3 provide similar significant findings. Most interestingly, the DiD estimator is insignificant in all three models, indicating that sample 1.1 of all 50 states does not show evidence of an effect of the 2007 additions to the Californian Business and Professions Code.

The average marginal effects are computed in table 12.2. The DiD estimator is insignificant for *states* and *assignees*, but significant at the 5% level for *patents* as the dependent variable. This indicates that an inventor that patented in California after 2007 is predicted to have 0.208 more patents on average compared to the control group, keeping all else constant. Comparing these results to the DiD estimation for the same sample, it is not surprising that the average marginal effects are insignificant. Even though models 1 and 2 in table 8 generate negative and significant DiD estimators, table 7 reveals that PTA is violated for both models. This indicates biased estimates. The marginal effects conclude that an inventor that patented in California after 2007 is not predicted to have patented in more or less states at more or less firms due to the policy change. Also interesting to note is that post_2007 is positive and significant in all three models, indicating that patenting activity is higher after 2007 than before.

Table 13.1 - Results of the negative binomial regressions with states, assignees and patents as dependent variables for sample 1.2

	(1) states	(2) assignees	(3) patents
main			
post_2007	0.00977	0.0740	0.460^{***}
	(0.0429)	(0.0419)	(0.0503)
CA_post_2007	-0.00114	0.0118	0.0263
	(0.108)	(0.105)	(0.125)
assignees	0.114^{***}		0.360^{***}
	(0.00683)		(0.00906)
patents	-0.00105	0.0124^{***}	
	(0.000943)	(0.000748)	
states		0.313^{***}	-0.0889^{***}
		(0.0103)	(0.0191)
Constant	0.428	0.0875	-0.0603
	(0.255)	(0.248)	(0.305)
/			
lnalpha	-36.98	-28.28	-0.927^{***}
	(.)	(.)	(0.0159)
Observations	13323	13323	13323

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 13.2 – Results of the corresponding average marginal effects for sample 1.2

	(1.2: states)	(1.2: assignees)	1.2: patents
post_2007	0.021	0.172	2.897***
CA_post_2007	-0.002	0.027	0.165
assignees	0.240 ***		2.266***
patents	-0.002	0.029***	
states		0.727***	-0.560***

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The results for the negative binomial regression for sample 1.2 are similar to the results for sample 1.1. The regression reveals no significant coefficients for the interaction term between *post_2007* and *California*, indicating that inventors that have patented in California after 2007 have not patented in more or less states at more or less firms when compared to the control group. The average marginal effects in table 13.2 are all insignificant for the interaction term.

Table 14.1 - Results of the negative binomial regressions with states, assignees and patents as dependent variables for sample 1.3

	(1) states	(2) assignees	(3) patents
main			
post_2007	0.220^{***}	0.0389^{*}	0.558^{***}

	(0.0216)	(0.0160)	(0.0194)
CA_post_2007	-0.0260 (0.0440)	0.00620 (0.0316)	-0.00119 (0.0382)
assignees	0.105^{***} (0.00353)		0.384^{***} (0.00590)
patents	-0.00465^{***} (0.000592)	0.0109^{***} (0.000387)	
states		0.133^{***} (0.00536)	-0.189^{***} (0.00707)
Constant	0.0137 (0.202)	0.511^{**} (0.168)	0.245 (0.206)
/ lnalpha	-27.24 (.)	-36.36 (.)	-0.770^{***} (0.00829)
Observations	43253	43253	43253

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 14.2 – Results of the corresponding average marginal effects for sample 1.3

	(1.3: states)	(1.3: assignees)	(1.3: patents)
post_2007	0.291***	0.088*	3.883***
CA_post_2007	-0.034	0.014	-0.008
assignees	0.139***		2.674***
patents	-0.006***	0.025***	
states		0.302***	-1.314***

The results for the negative binomial regression for sample 1.3 are once again similar to the results for samples 1.1 and 1.2. The regression reveals no significant coefficients for the interaction term between *post_2007* and *California*, indicating that inventors that have patented in California after 2007 have not patented in more or less states at more or less firms when compared to the control group. The average marginal effects in table 14.2 are all insignificant for the interaction term.

The negative binomial regression models using all 50 states reveal that the change in Californian Business and Professions Code in 2007 did not affect the predicted amount of states and firms an inventor had.

Negative binomial regression with 10 states

Table 15.1 – Results of the negative binomial regressions with states, assignees and patents as dependent variables for sample 2.1

	(1) states	(2) assignees	(3) patents
main post_2007	0.0203 (0.0311)	0.195^{***} (0.0280)	0.688^{***} (0.0260)
CA_post_2007	-0.0145 (0.0316)	0.0153 (0.0293)	-0.00522 (0.0272)

assignees	0.0499***		0.342***
	(0.00747)		(0.00583)
patents	-0.000509	0.0141***	
	(0.000704)	(0.000537)	
states		0.570***	-0.132***
		(0.0225)	(0.0264)
Constant	-0.00812	-0.372**	0.467***
	(0.139)	(0.129)	(0.124)
/ lnalpha	-52.23	-26.42	-0.823***
	(.)	(.)	(0.00754)
Observations	59269	59269	59269

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 15.2 – Results of the corresponding average marginal effects for sample 2.1

	(1.3: states)	(1.3: assignees)	(1.3: patents)
post_2007	0.021	0.239***	3.609***
CA_post_2007	-0.015	0.019	- 0.027
assignees	0.051 ***		1.791***
patents	-0.000	0.017***	
states		0.698***	-0.694***

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 16.1 – Results of the negative binomial regressions with states, assignees and patents as dependent variables for sample 2.2

	(1) states	(2) assignees	(3) patents
main			
post_2007	0.00366	0.0254	0.177
	(0.240)	(0.229)	(0.277)
CA_post_2007	-0.00946	0.0643	0.448
	(0.332)	(0.320)	(0.395)
assignees	0.00528		0.371***
	(0.0360)		(0.0360)
patents	0.000190	0.0127***	
	(0.00357)	(0.00286)	
states		0.244	0.414
		(0.239)	(0.305)
Constant	0.669	0.318	-1.016
	(0.469)	(0.643)	(0.807)
/ lnalpha	-19.39	-33.50	-0.867***
	(.)	(.)	(0.0598)
Observations	889	889	889

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 16.2 – Results of the corresponding average marginal effects for sample 2.2

	(1.3: states)	(1.3: assignees)	(1.3: patents)

post_2007	0.007	0.057	1.179
CA_post_2007	-0.019	0.143	2.988
assignees	0.011		2.475***
patents	0.000	0.028***	
states		0.544	2.763

Table 17.1 – Results of the negative binomial regressions with states, assignees and patents as dependent variables for sample 2.3

	(1) states	(2) assignees	(3) patents
main			
post_2007	0.0908	0.0675	0.604***
	(0.0718)	(0.0506)	(0.0627)
CA_post_2007	-0.0617	-0.000451	-0.0603
	(0.0797)	(0.0561)	(0.0697)
assignees	0.0186		0.363***
	(0.0159)		(0.0127)
patents	-0.00117	0.00868***	
	(0.00121)	(0.000743)	
states		0.0412	-0.143***
		(0.0238)	(0.0288)
Constant	0.132	0.681***	0.181
	(0.259)	(0.200)	(0.248)
/			
lnalpha	-26.86	-36.61	-0.700***
	(.)	(.)	(0.0157)
Observations	11384	11384	11384

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 17.2 – Results of the corresponding average marginal effects for sample 2.3

	(1.3: states)	(1.3: assignees)	(1.3: patents)
post_2007	0.098	0.150	4.544***
CA_post_2007	-0.066	-0.001	-0.454
assignees	0.019		2.727***
patents	-0.001	0.019***	
states		0.092	-1.073***

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Tables 15.1 until 17.2 all have insignificant coefficients for the interaction term between *California* and *post_2007*. This indicates that in a sample with 10 anti-NCA states, patenting in California after 2007 does not affect inventor mobility between states or between firms. Interesting to note is that if an inventor has more patents, that inventor has been active at more firms and the other way around. This effect is highly significant throughout the second set of subsamples.

Conclusion

The negative binomial regression models reveal that the change in Californian Business and Professions Code in 2007 did not affect the predicted amount of states and firms an inventor had. These results are different for models 5 and 7 in tables 8 and 9, which have negative and significant DiD estimators while not violating PTA, but similar for all other models. Therefore, the difference-in-difference technique generated some support for hypothesis 1, while the negative binomial regression approach found no evidence for hypothesis 1.

However, both approaches measure something different. Difference-in-difference measures if a certain policy change had an effect, while negative binomial regression estimates the predicted amount of a variable according to other explanatory variables. The insignificant interaction terms in all the negative binomial regressions for the models with *states* and *assignees* as dependent variables simply mean that having patented in California after 2007 or after 2017 does not have predictive power on the average amount of states an inventor patents in or the number of firms an inventor works at. Therefore, the insignificant results of the robustness checks weaken the notion that changes in noncompete legislation at the state level impact inventor mobility, but they do not fully undermine the significant results found in the main models of this study.

Finally, this robustness check generated an extra interesting result. Table 12.2 indicates that an inventor that patented in California after 2007 is predicted to have 0.208 more patents on average compared to the control group, keeping all else constant. Therefore, this study on noncompete agreements and mobility coincidentally found evidence that Californian inventors after 2007 innovate more, measured in number of patents, than inventors that did not patent in California after 2007.

7.1.2 Hypothesis 2

Hypothesis 2 looks into the effect of the Defend Trade Secrets Act on labour mobility among inventors in California. A regression discontinuity design was used to graphically illustrate that the hypothesis finds support for the number of states an inventor patents in, but finds no support for the number of assignees an inventor has. This is statistically backed up by a linear regression between *states* and *California* in 2015 and 2019 and a linear regression between *assignees* and *California* in 2015 and 2019. The results are displayed in table 10 and are in line with the graphs in figures 4 and 5. As *states* and *assignees* are count variables, I also test if the graphs are statistically supported when using a negative binomial regression. The results are displayed in table 18.

Table 18 – Negative binomial regressions testing the significance of discontinuity for Californian inventors

	(2015) states	(2019) states	(2015) assignees	(2019) assignees
California	-0.0270	-0.0165	-0.0104	-0.0124
	(0.0232)	(0.0177)	(0.0210)	(0.0163)
Constant	0.0942***	0.0825***	0.287***	0.243***
	(0.0122)	(0.00952)	(0.0111)	(0.00878)
Observations	8468	14357	8468	14357

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 18 shows that all coefficients of *California* lose significance in a negative binomial regression. This result indicates that patenting in California in 2015 and 2019 does not affect the predicted values for *states* and *assignees* compared to the control group, keeping all else constant. Therefore, hypothesis two is not supported when using a negative binomial regression approach.

7.1.3 Hypothesis 3

A similar approach to test the robustness of the results is used for hypothesis 3.

Negative binomial regression with 3 states

Table 19.1 – Results of the negative binomial regressions with states, assignees and patents as dependent variables for sample 3.1

	(1) states	(2) assignees	(3) patents
main			
post_2017	0.0217	0.219***	0.543***
	(0.0243)	(0.0213)	(0.0188)
CA_post_2017	-0.0184	-0.0466*	0.195***
	(0.0236)	(0.0212)	(0.0187)
assignees	0.0267***		0.311***
	(0.00811)		(0.00601)
patents	-0.000263	0.0137***	
	(0.000784)	(0.000591)	
states		0.566***	-0.126**
		(0.0332)	(0.0385)
Constant	-0.0262	-0.351***	1.134***
	(0.0328)	(0.0432)	(0.0457)
/			
lnalpha	-52.23	-26.40	-0.882***
	(.)	(.)	(0.00843)
Observations	49377	49377	49377

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 19.2 – Results of the corresponding average marginal effects for sample 3.1

	(1.3: states)	(1.3: assignees)	(1.3: patents)
post_2017	0.022	0.272***	2.805***
CA_post_2017	-0.019	-0.058*	1.010***

assignees	0.027**		1.606**
patents	-0.000	0.017***	
states		0.702***	-0.654***

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

In tables 19.1 and 19.2, the coefficient for the interaction term between *California* and *post_2017* are significant at the 5% and 0.1% level, respectively. The coefficient in model 1.3: assignees indicates that the predicted number of assignees an inventor has is 0.058 lower on average if an inventor patented in California after 2017, keeping all else constant. This means that the installation of section 925(e) in 2017 led to a reduction in predicted average intra-firm inventor mobility, providing evidence for hypothesis 3. This effect is significant at the 5% level. Also, table 19.2 provides evidence that patenting in California after 2017 leads to an on average rise of 1.01 in the predicted number of patents, keeping all else constant. This effect is highly significant at the 0.1% level.

Table 20.1 – Results of the negative binomial regressions with states, assignees and patents as dependent variables for sample 3.2

	(1) states	(2) assignees	(3) patents
main			
post_2017	6.22e-15	-0.0221	0.172
	(0.131)	(0.122)	(0.140)
CA_post_2017	6.40e-15	0.0567	0.0889
	(0.178)	(0.166)	(0.192)
assignees	2.72e-15		0.469***
	(0.0527)		(0.0503)
patents	3.05e-16	0.0169***	
	(0.00524)	(0.00378)	
states		0	0
		(.)	(.)
Constant	0.693**	0.676**	0.654*
	(0.257)	(0.220)	(0.277)
/			
lnalpha	-19.58	-63.71	-0.942***
	(.)	(.)	(0.0905)
Observations	400	400	400

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 20.2 – Results of the corresponding average marginal effects for sample 3.2

	(1.3: states)	(1.3: assignees)	(1.3: patents)
post_2017	0.000	-0.051	1.162
CA_post_2017	0.000	0.132	0.602
assignees	0.000		3.175***
patents	0.000	0.032***	
states		0.000	0.000

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Secondly, the negative binomial regression model is applied to sample 3.2, with California, Delaware and New York and inventors with at least 2 patents in 2 states. Descriptive statistics show that it is uncommon in this sample of three states for inventors to have patented in at least two states. Therefore, the coefficients for the interaction term are insignificant, meaning that in this sample patenting in California after 2017 has no predictive power on the average expected number of states an inventor has patented in or number of firms an inventor has worked at.

Table 21.1 – Results of the negative binomial regressions with states, assignees and patents as dependent variables for sample 3.3

	(1) states	(2) assignees	(3) patents
main			
post_2017	0.0653	0.0720^{*}	0.304^{***}
	(0.0469)	(0.0327)	(0.0385)
CA_post_2017	-0.0537	-0.0200	0.218^{***}
	(0.0497)	(0.0346)	(0.0409)
assignees	0.0146		0.344^{***}
	(0.0168)		(0.0133)
patents	-0.000614	0.00865^{***}	
	(0.00131)	(0.000792)	
states		0.0606	-0.135^{**}
		(0.0341)	(0.0416)
Constant	-0.00288	0.690^{***}	0.952^{***}
	(0.0685)	(0.0531)	(0.0704)
/			
lnalpha	-26.83	-31.58	-0.721^{***}
	(.)	(.)	(0.0168)
Observations	10050	10050	10050

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 21.2 – Results of the corresponding average marginal effects for sample 3.3

	(1.3: states)	(1.3: assignees)	(1.3: patents)
post_2017	0.068	0.161*	2.294***
CA_post_2017	-0.056	-0.045	1.646***
assignees	0.015		2.600***
patents	-0.000	0.019***	
states		0.135	-1.021**

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Lastly, the negative binomial regression model is applied to sample 3.3, with California, Delaware and New York and inventors with at least 2 patents and at least 2 assignees. The interaction terms are insignificant for the first two models, but highly significant when patents is the dependent variable. An inventor that patented in California after 2017 is predicted to have 1.646 more patents on average compared to the control group, keeping all else constant. This effect is significant at the 0.1% level.

Conclusion

Sample 3 generated significant results for model 17 with subsample 3.1 and model 21 with subsample 3.3, both with *states* as the dependent variable. These negative DiD estimators led for example to the conclusion that the change of law regarding the tolerance of choice-of-law provisions in 2017 leads to a 0.0613 decrease in number of states a Californian inventor patents in, keeping all else constant.

In the context of negative binomial regression, significant results for interaction terms of *states* and *assignees* are only found in sample 3.1 for assignees. That means that the results from the difference-in-difference estimation are not supported when examining a different regression specification. In conclusion, the signs and significance levels of the interaction terms differ from the results in table 11. As mentioned in the conclusion for the robustness check of hypothesis 1, this does not mean that the results are invalid. Difference-in-difference measures if a certain policy change had effect, while negative binomial regression estimates the predicted amount of a variable according to other explanatory variables. Therefore, the insignificant results of the robustness checks for hypothesis three weaken the notion that changes in choice-of-law legislation on state level impact inventor mobility, but they do not fully undermine the significant results found in the main models of this study.

7.2 Additional robustness check hypothesis 2

In regression discontinuity design results are only generalizable around the cut-off, thus limiting external validity. To additionally test the robustness of the main results, I narrowed the bandwidth to one year instead of two. Instead of regressing *states* and *California* in 2015 and 2019 and regressing assignees and California in 2015 and 2019, thus creating a two year bandwidth around 2017, the table below shows the results of the same regressions, except now in 2016 and 2018, creating a one year bandwidth.

Table 22 – Regressions testing the significance of discontinuity for Californian inventors with a one year bandwidth

	(2016) states	(2018) states	(2016) assignees	(2018) assignees
California	-0.00929	-0.0217***	0.00752	-0.0155
	(0.00748)	(0.00640)	(0.0150)	(0.0124)
Constant	1.093***	1.088***	1.311***	1.286***
	(0.00404)	(0.00347)	(0.00809)	(0.00674)
Observations	9133	11299	9113	11299

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

As table 22 shows, the results partially lose significance when narrowing the bandwidth. The models for *assignees* are still insignificant, just like table 10, but the left boundary for the *states* model, now adjusted to 2016, is insignificant. This means that in 2016 patenting in California did not have a significant impact on the number of states an inventor patented in, keeping all else constant. The right boundary, now adjusted to 2018, is still significant at the 0.1% level. This indicates that mobility decreases in 2018 for Californian inventors in terms of number of states an inventor patents in. Therefore, hypothesis two still finds some support when using RDD, as the graphs in figures 4 and 5 are still partially statically supported.

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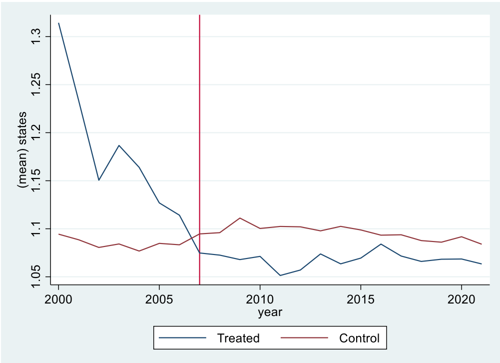
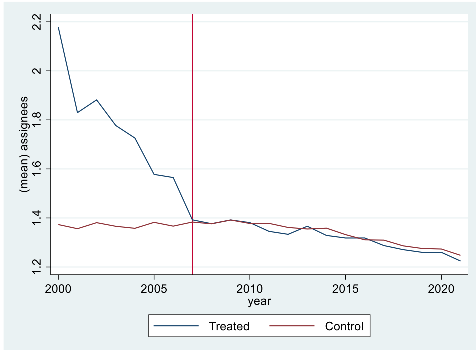
10 Appendix

10.1 Appendix A

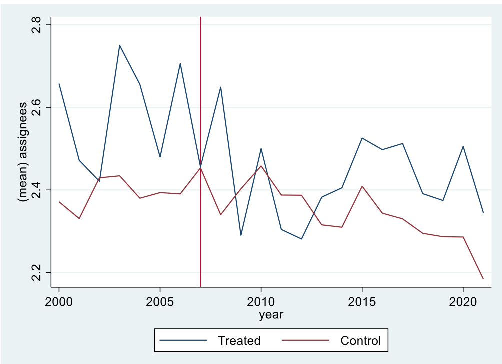
The following graphs provide visual evidence of whether or not the Parallel Trends assumption (PTA) is violated or not for each of the 9 subsamples with states and assignees as dependent variables. Table 7 is based on these graphs.

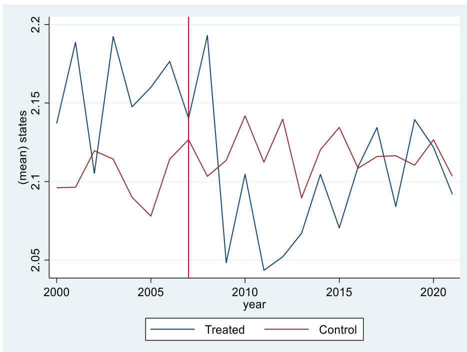
Sample 1.1 – Inventors with at least 2 patents for all 50 states



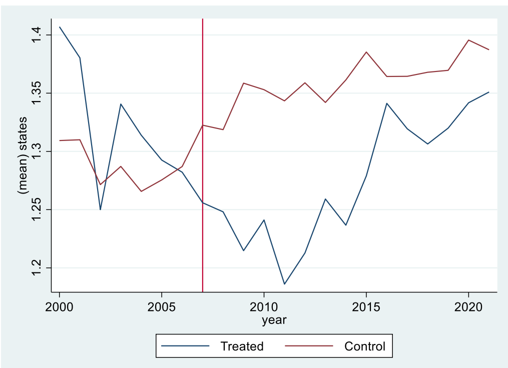


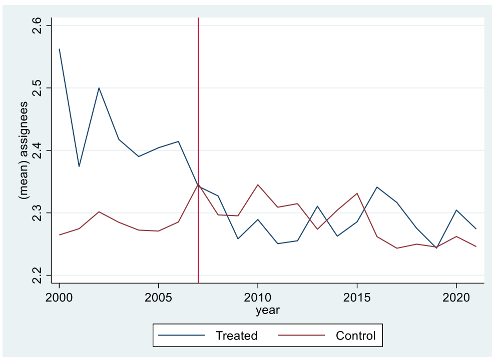
Sample 1.2 – Inventors with at least 2 patents in at least 2 states for all 50 states



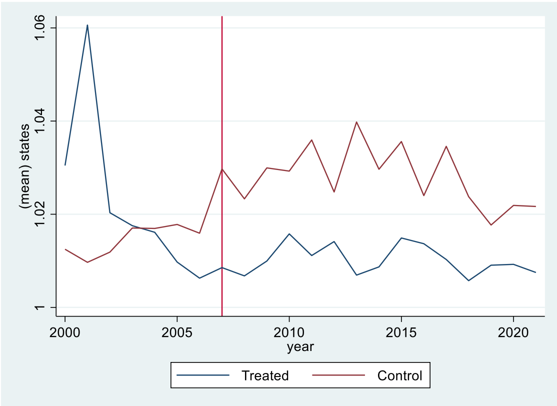


Sample 1.3 – Inventors with at least 2 patents with at least 2 assignees for all 50 states

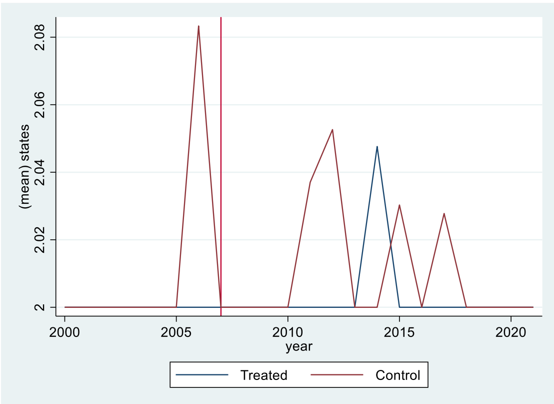


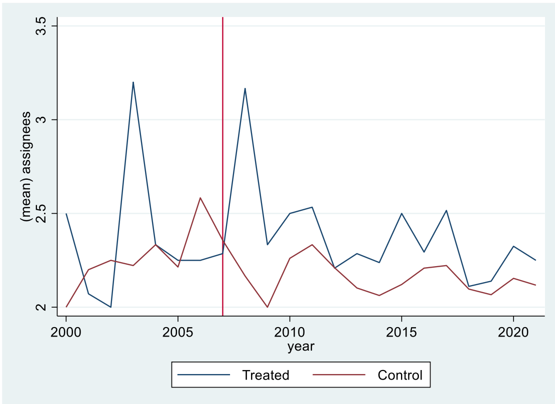


Sample 2.1 – Inventors with at least 2 patents for 10 nonenforcing states

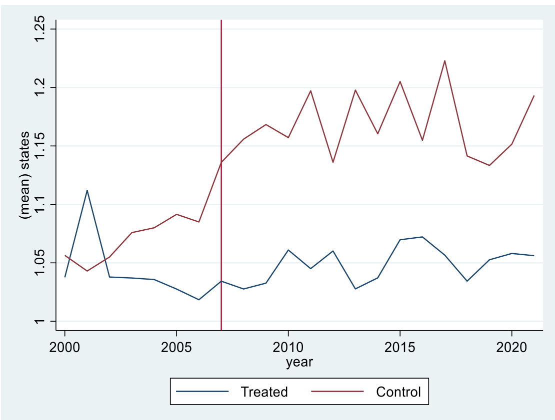


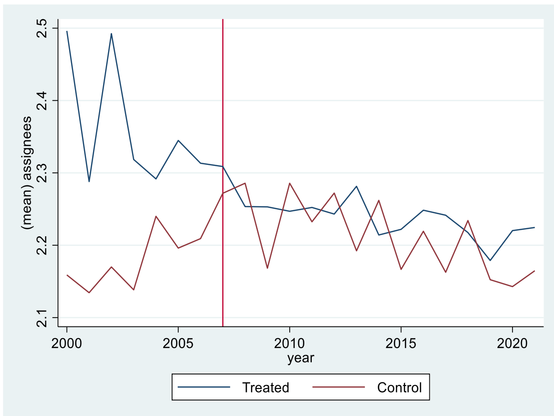
Sample 2.2 – Inventors with at least 2 patents in at least 2 states for 10 nonenforcing states



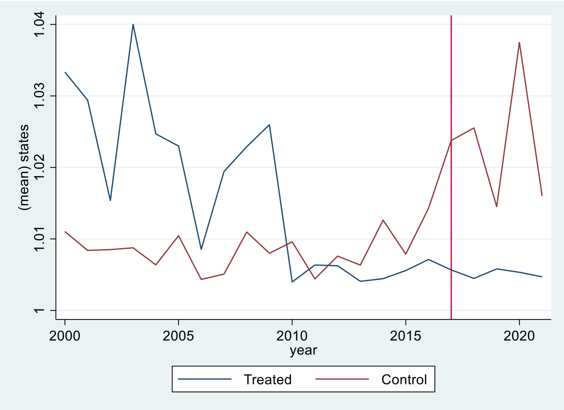


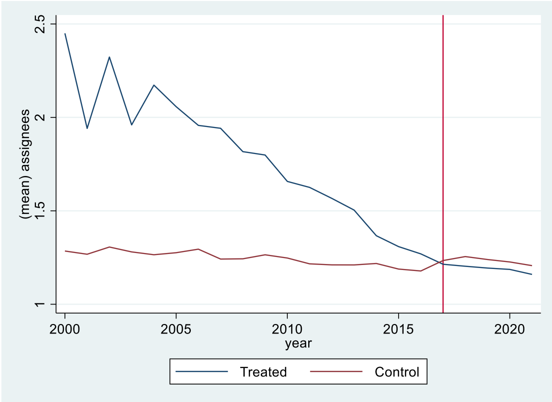
Sample 2.2 – Inventors with at least 2 patents with at least 2 assignees for 10 nonenforcing states



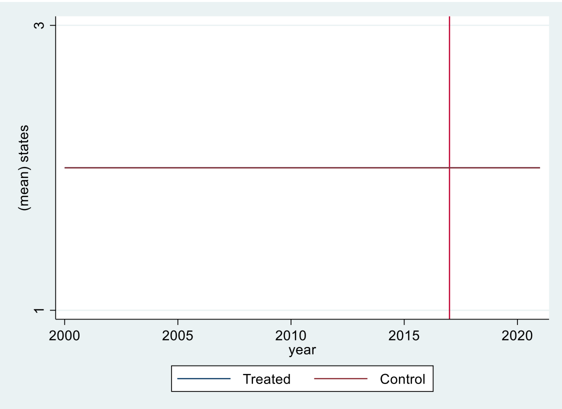


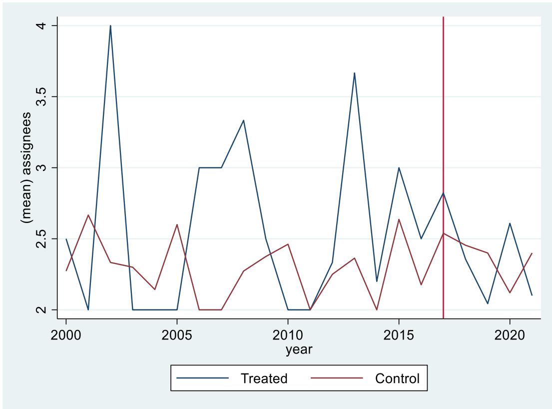
Sample 3.1 – Inventors with at least 2 patents for California, New York and Delaware





Sample 3.2 – Inventors with at least 2 patents in at least 2 states for California, New York and Delaware





Sample 3.3 – Inventors with at least 2 patents with at least 2 assignees for California, New York and Delaware