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Entropy Balancing Autor's (2003) Data on the US Labor Markets

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Abstract

This thesis studies the impact of employment protection legislature on employment outsourcing via temporary help services. Given the recent trend of companies shifting away from traditional employment contracts to short term contracts one should study whether this is caused by more stringent introductions to employment protection legislation. As the usage of temporary hiring services over the course of the study has continued to increase David Autor attempts to disentangle implied contract's shock from other causes.

We evaluate David Autor's empirical study on exceptions to the employment at-will doctrine and how given exceptions affect the US labor markets from the standpoint of temporary help services. Autor makes use of a state-level time series aggregate data that follows the employment level and employment characteristics from 1979 to 1995. Autor finds that the implied contract exception to the employment at-will doctrine shows an incremental increase in the usage of temporary hiring services in states that adopt the implied contract exception to their legislature.

Using a smaller sample of data and entropy balancing method we make iterative changes to Autor's regressions to study whether his results can be validated with adjusted data. Entropy balancing is a data pre-processing method that balances control and treatment groups' independent variables prior to regressions. Given that states that adopt and the states that do not adopt the implied contract exception are characteristically very different, entropy balancing could validate Autor's results in a setting where control and treatment states are more comparable. Using entropy balancing and a smaller sample size do not uniformly confirm Autor's hypothesis that implied contract exception cause increase in the usage of temporary hiring services.

Tiivistelmä

Tutkielmani tarkastelee työttömyysturva-lakia tiukentavan implisiittiseen työsopimus-poikkeuksen vaikutusta vuokratyövoiman lisääntyneeseen käyttöön. Tarkastelen David Autorin tukimuksen empiiristä tuloksia sekä dataa, joka on luotu kyseisiä estimointeja varten. Tarkastelen tutkielmassani myös Autorin estimointimenelmiä ja kuinka ne pystyvät erottelemaan implisiittinen sopimus-poikkeuksen vaikutuksen muista työmarkkinoita vaikuttavista osatekijöistä ja shokeista. Sekä tutkimuksen aikana että tutkimuksen jälkeen vuokratyövoiman käyttö perinteisten työsopimusten sijaan on lisääntynyt.

Työni tarkastelee ja työttömyysturva-lakiiin liittyvien muutokaiin liittyviä empiirisiä tutkimuksia sekä niiden tuloksia. Sen lisäksi esittelen employment at-will-doktriinin ja kuinka työntekijää suojelevat poikkeukset doktriiniin vaikuttavat erityisesti vuokratyövoiman käyttöön. Implisiittinen sopimus-poikkeus employment at-will-doktriiniin, joka on osavaltio-kohtainen lisäys lakiin. Implisiittisen sopimuksen johdosta kaikkia pitempiaikaisia työntekijöitä tullee kohdella irtisanomistilanteessa samoin ehdoin kuin vakituisessa työsuhteessa ja työsopimuksen allekirjoittaneita työntekijöitä. Autorin aikasarja-data tarkastelee Yhdysvaltojen työmarkkinoita vuodesa 1979 vuoteen 1995. Autorin tulokset, jotka replikoin työssäni, paljastavat että työttömyysturva-lakia tiukentavat poikkeukset lisäävät vuokratyövoiman käyttöä osavaltioissa, jotka siirtyvät noudattamaan implisiittistä sopimus-poikkeusta.

Tutkimalla pienempää 43 osavaltion otosta ja käyttämällä entropia tasapainotus metodia tarkastelen mikäli Autorin tulokset voidaan replikoida pienempään otokseen jossa kontrolli- ja hoito-ryhmät muistuttavat toisiaan enemmän. Entropia tasapainotus on datan esikäsittely vaihe, joka pyrkii tasapainottmaaan kontrolli ja hoito-ryhmän muuttujat ennen regressiota. Osavaltiot, jotka adoptoivat implisiittinen sopimuksen eroavat osavaltioista jotka eivät ota edellä mainittua poikkeusta lakiin. Tämän johdosta datan esikäsittely on perusteltua. Vertaan estimointieni tuloksia Autorin tuloksiin. Tutkielmani tulokset eivät muuta Autorin tutkimuksen keskeisiä tuloksia. Päädyn tutkielmassani esittämään kuinka estimointi-menetelmän ja muuttujien valinta vaikuttaa keskeisesti Autorin esittämään kausaalisuuteen.

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1   Introduction

Employment Protection Legislature (EPL) is a part of legislature that sets a framework of basic rules and regulation for both the employer[1] and the employee[2]. Over the course of the past century, the trend has been to introduce more stringent EPL throughout the developed countries (Boeri & van Ours, 2008). Empirically, more stringent EPL entails that it is more costly and timely for a firm to discharge a redundant worker. An integral part of the United States EPL is the employment at-will doctrine that defines the contractual relationship between the employer and the employee. Employment at-will doctrine statutes that either the employer or the employee can terminate an ongoing employment spell without a reason. Similarly, workers can leave their employer without repercussions.

Academic literature on EPL's impact on employment has developed noticeably in the last 25 years. Foundation for the economic theory in EPL is presented by Lazear (1990), who introduces a 2-period model on job security provisions and unemployment. Recent empirical research (Acemoglu & Angrist, 2001; Autor et al., 2002; Boeri & Jimeno, 2003; Boeri & Garibaldi, 2007; Bassanini et al., 2009) has brought more understanding to EPL's impact and relation to the modern labor markets. The above-mentioned empirical evaluations study one country's data to study an EPL related shock and its impact on employment. Moreover, cross-country evaluations of EPL's impact on the economy are rare because of differences in labor market institutions and economic trends among countries.

Largely, recent empirical research shows that more stringent EPL affects negatively on employment and it is likely to increase the usage of fixed contracts. Stringent EPL tends to also decrease the worker turnover ratio for firms. In some cases, well-intentioned government labor market interventions may sometimes, in fact, hurt those whom they are intended to help (Acemoglu & Angrist, 2001). Hence, it is important to study how more stringent employment protection will affect the labor markets.

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[1] This thesis uses the terms employer and firm interchangeably

[2] There is no distinction between the terms employee and worker

At the turn of the mid-1900s, public support for the employment at-will doctrine had eroded. Labor force preferred a framework where the firm could not fire a worker without an adequate cause (Lawrence, 1967). Congruently, court decisions on employment began to favor more stringent EPL. Such court rulings are exceptions to the at-will doctrine. Exceptions to the at-will doctrine limit employers' opportunities to implement maleficent practices on the workers. Implied contract exception, which is one of the three distinct exceptions to the employment at-will doctrine, introduces dismissal limitations to workers working under an implied contract. The implied contract exception conditions that a worker hired using an implied, i.e. informal contract, should be treated similarly to a worker with a formal employment contract (Autor, 2003).

This thesis studies Autor's (2003) paper "Outsourcing at Will: The Contribution of Unjust Dismissal Doctrine to the Growth of Employment Outsourcing" and the robustness of his estimations using more recent econometric methods. Autor (2003) data, which is publicly available, are used as the framework to study methods that could enhance the regressions in order to create a robust and randomized setting for estimations. Autor (2003) studies whether there is a correlation between employment outsourcing and implied contract exception to the employment at-will doctrine. In relation to other contractual types of employment, the share of THS employment grows steadily throughout the timespan (1979-1995) of the study. Thus, Autor (2003) studies whether it is possible to disentangle the implied contract's impact on THS employment from other trends and factors in play and to see whether implied contract has an impact on THS employment.

The Difference-in-differences (DID) method is used to evaluate state-level aggregate data on employment and labor force characteristics. Autor (2003) finds that implied contract exception is likely to increase a state's THS employment. The regression design Autor (2003) uses controls for the state-level and year fixed effects with an assortment of other controls for regional, linear, and non-linear variation in the data. Autor (2003) uses a large number of control variables to control for differences among states and yearly variation. A large number of control dummies can increase the likelihood of an overfitted regression. An overfitted regression fits too closely

with the given data points and, therefore, is more likely to approximate random error or noise, rather than causality of the dependent and independent variables. This phenomenon could occur when there are too many parameters fitted into a regression in relation to the number of observations in the data. Autor (2003) controls for state-level, regional and time trends, whereas the majority of these estimations do not control for labor force characteristics.

This thesis studies whether entropy balancing the data will balance the covariate imbalance between the control and the treatment states. Entropy balancing is a data balancing method that is likely to make the data less model-dependent because it adjusts for covariate distribution representation inequalities. Underlining differences in the control and treatment state data could explain why the implied contract variable's treatment effect decreases when labor force demographics are incorporated to Autor's (2003) regression design.

In addition to testing the entropy balancing method to the data, this thesis sets out to improve the specific sample of states used for evaluations. One option is to omit observations that do not fit to the RCT and DID framework. For the first seven states that adopt the implied contract, there are no pre-implied contract data available. Without the pre-treatment employment and THS employment levels for the seven early-adopter states there are no reference points to compare the impact of implied contract. Also, early-adopter states may have adapted to changes in EPL and thus these states should be potentially omitted from the model.

This thesis tests entropy balancing method on the smaller 43 state data in order to find whether the magnitude and standard error of the implied contract on THS employment variable is different to the balanced 50 state data estimations. The argument for the 43 state data is that it is a closer simulation of a randomized controlled trial (RCT) design and it aligns well with the DID method. A smaller data allows for there to be statistics for both pre- and post-shock employment levels for all the 43 states incorporated to the regression. This thesis also studies the 43 state entropy balanced data and whether regressions remains representative with a fewer number of control variables that control for state-level and yearly variation.

Estimations of this thesis show that the labor force characteristics have explanatory power on THS employment and one should not omit the labor force characteristics from regressions. Both the entropy balancing and the smaller 43 state data improve the statistical significance of the implied contract variable. When the entropy balanced 43 state data is used for estimations the treatment effect is greater than it is for the 50 state data. Finally, this thesis tests whether it is possible to decrease the number of control dummy variables and simultaneously maintain the explanatory power of the treatment variable. Smaller number of dummy variables would also decrease the risk of overfitting, given the limited number of observations (Leinweber, 2007).

Overall, both entropy balancing the data and the smaller 43 state data improve the magnitude of the implied contract exception's impact on THS employment. Similarly, the majority of the labor force demographic variables show more explanatory power on the THS employment. The implied contract's impact on the 43 state entropy balanced data is greater, but it also shows that some of Autor's (2003) estimations lose their statistical significance. This thesis shows that the THS employment growth remains consistent even after there are no new implied contract states.

First, this thesis summarizes the contextual background and definitions of employment at will doctrine and the implied contract exception and their relation to THS industry. Second, a look at the economic theory of the EPL and THS employment is presented to describe the framework this thesis proposes. Third, a literature review of the recent empirical research on EPL and its impact employment and productivity is presented. Fourth, the Autor (2003) data are described and summarized. Fifth, this thesis replicates a part of his paper in order to show that the alternations to the iterative regressions of this thesis are comparative to the baseline. Finally, estimations using the entropy balanced 43 state data are presented and compared to their original counterparts.

2   Employment at Will –doctrine and Employment Protection Legislature

The Employment at Will (at-will) doctrine is an essential part of the U.S. employment legislature. At-will doctrine shaped the contractual relationship of the employer and the employee over 150 years ago. One of the most well-known court decisions that was the essential basis for the at-will doctrine dates back to the year 1884. Tennessee Supreme Court declared in 1884 that "men must be left, without interference to buy and sell where they please, and to discharge or retain employees at will for good cause or for no cause, or even for bad cause without thereby being guilty of an unlawful act *per se*. It is a right which an employe [sic] may exercise in the same way, to the same extent, for the same cause or want of cause as the employer…" (Payne v. Western & Atlantic Railroad, 1884).

Morriss (1994) argues that the at-will doctrine is not "a product of nineteenth century industrialization". Morriss's (1994) theory is supported by an empirical evaluation that does not show correlation between industrialization and the adaptation of the at-will doctrine. At the turn of the 20th century, employment at-will gained recognition in the US common law system. The early 1900s European civil law systems do not acknowledge differences in the nature and legality of indefinite employment (Morriss, 1994).

In practice, the employment at will doctrine means that either the employer or the employee can independently terminate the employment contract under most circumstances. It also means that there are no indefinite contracts between the employer and the employee, unless the employment contract specifies one (Morriss, 1994). At the turn of the 20th century, the at-will doctrine was generally in line with the judicial and public consensus (Lawrence, 1967). Historically there are no definite explanations as to why the at-will doctrine was initially adopted. One cannot construct an objective function for court decisions as the doctrine had different levels of enforcement and was adopted in different ways (Morriss, 1994). It is unlikely that there is a judge whose objective function is unbiased on employment at-will decisions and who is not affected by things such as personal preferences and/or common opinion. In the common law system, additions to the law can be created based on a single decision by a court. Because of the nature of

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the common law system, state-level courts and judges can determine their state's stance on a certain matters. In order to the have an unbiased objective function, every judge would have to have an identical set of decision nodes, and they could not be impacted by the public discourse and consensus. Morriss (1994) suggests that the creation of both descriptive and objective function is likely to be impossible. According to Lawrence (1967), the public support for at-will doctrine eroded rapidly in the 1950s. In 1959, California was the first state to make an exception to the at-will doctrine that limited employers' right to terminate a worker. Subsequently, most states followed California's lead, and, by 1992, 46 states recognized both judicial and legislative exceptions to the employment at-will doctrine (Autor, 2003).

In the early 1990s, US courts acknowledged three common law exceptions to the employment at will doctrine. Autor (2003) defines the three exceptions as such: "…breach of an implied contractual right to continued employment, terminations contrary to public policy, and violations of an implied covenant of good faith and fair dealing." The implied contractual exception to employment at-will is usually linked to the 1980 case of *Toussaint v. Blue Cross*, where the Michigan Supreme Court decided to reverse court of appeals verdict and side with the discharged plaintiff who sought compensation for a wrongful discharge.

The plaintiff, Charles Toussaint, accused Blue Cross & Blue Shield (Blue Cross) for a breach of contract. The plaintiff claimed that the internal personnel policy handbook stated Blue Cross's policy of terminating employees without any particular reason. The Michigan Supreme Court decided that the company handbook was an implication of a binding employment contract and favored Toussaint in its decision (Autor, 2003).

In 1981, *Pugh v. See's Candies* ruling in California extended the notion of implied contract to employees who had no written or direct statements of the nature of the employment. This could be the situation when the signals given by the employer define the context of the employment relationship. In the given context, signals of implied contract are implications, such as length of service at work, past promotions, and salary increases. An implied contract can be created on the basis of the treatment of other employees and standard industry practices contracts. Thus, a

worker without a contract who is treated equally to workers with a contract can consider themselves as being under the implied contract exception (Autor et al., 2002).

Collectively, a wave of court rulings caused uncertainty among employers. Firms began to reconsider their discharging practices in order to comply with the exceptions to the at-will doctrine. The first widely covered court battles also initiated numerous unjust dismissal litigation processes. Employees who worked without a contract considered themselves to be under the implied contractual employment. If that had been the case, it would have provided workers with better employment protection. The data on the cost of unjust dismissal cases are quite limited because 96% of all civil cases are settled before jury verdict (Jung, 1997).

Jung (1997) studies a sample of implied contract disputes in California, where 52% of the cases are ruled in favor of the plaintiff. An average punitive fee assigned to the employer after trial is approximately $600,000 (Jung, 1997). For unjust dismissal cases where the defense is victorious, legal fees average at $98,000. In decisions where the court sided with the plaintiff, the legal fees amounted to an average of $220,000 (Dertouzos et al., 1988). For the indirect cost of a potential unjust dismissal litigation process is financially bearing, a forward-looking employer had an incentive to avoid litigations by conducting preventive actions that minimize the risk of unjust dismissal trial.

By the year 1980, court rulings in several states acknowledged and enforced exceptions to the at-will doctrine. Employers were aware of the changes occurring in the employment and unjust dismissal framework (Edelman et al., 1992). Edelman, Abraham and Erlanger (1992) and Autor (2003) conclude that professional law, personnel, and news journals published a wide array of articles on the increased risk unjust dismissal litigations. Qualitatively, personnel journals inflate the threat of wrongful discharge (Edelman et al., 1992). As an example, Edelman et al. (1992) show the extensive usage of hyperbolic terms such as "epidemic, avalanche, pathological, litigation explosion" when the authors describe wrongful discharge trials and their riskiness for an employer.

Personnel—i.e. human resources—articles on wrongful discharge rarely include available statistics to support claims made on wrongful discharge rules and their effects to the employer (Edelman et al., 1992). Law journal articles are more exact on the state-level variation on the receptiveness of wrongful discharge lawsuits. Compared to personnel articles, law review articles and law practice journals are less likely to use a threatening style of writing or hyperbolic terms. Edelman et al. (1992) conclude that employers were widely exposed to both articles on unjust dismissals and the increased probability of litigation as a result of the implied contract. Edelman et al. (1992) also show that, between 1980 and 1985, the number of employment at will clauses on company handbooks increased exponentially. While this occurred in the 1980s, Autor (2003) mentions that it became difficult for firms to "contract around the risk posed by implied contract suits."

2.1   Temporary Help Services Industry

Temporary help services (THS) are a short-term employment solution that is contractually different from traditional open-ended employment solutions. As its moniker conditions, THS employment has no eminent implications of long term employment. Between years 1979 and 1995 THS employment grew 11% annually. Therefore, there cannot be any implications of an implied contract. THS industry works as an intermediate between the worker and the firm. In essence, THS firms hire workers to work for their client firms (Cohany, 1998). The share of THS employment in the US has been steadily growing, beginning from 1980 (Autor, 2003).

Cohany's (1998) survey data from the Current Population Survey shows that in 1997, compared to a traditionally employed, THS workers are more likely to be young, female, and/or minorities. They are also more likely to not be in high school or have a high school or college diploma when compared to their counter-parts in traditional jobs. Approximately 55% of THS workers are women, while women constitute only 47% of the overall labor force. Roughly half of the women who are employed via THS firms raise children at the same time. Cohany (1998) shows that minorities such as blacks and Hispanics are over-represented among the THS-employed when compared to their representation in the labor force. Eighty-percent of THS workers work full

time[3] at the job. Instead of using THS firms for part-time or temporary help, firms appear to use THS firms for a traditional, open-ended full-time employment.

On average, wages paid by THS firms are about two-thirds of the amount thattraditionally-employed workers receive when all else is held equal. Health insurances and pension benefits are rarely provided for THS workers. In 1997, 60% of THS workers expressed their preference towards a traditional employment contract, while others would not express a specific preference between the THS and traditional employment. The median length of employment in a THS position is 6 months. Approximately 23% of the THS-employed work for their employer for more than a year. Companies in manufacturing and the service industry are the likely clients of THS firms (Cohany, 1998). This implies that employers could to mitigate the risk of implied contract exception by using THS (Autor, 2003).

In addition to the implied contract exception, there are two other widely acknowledged exceptions to the employment at-will doctrine: the public policy exception and the implied covenant of good faith and fair dealing (good faith) exception. The public policy exception makes it illegal for an employer to discharge an employee for obeying the law or exercising his constitutional rights. For example, an employee cannot be discharged because of jury duty or other mandatory government duty. The good faith exception prohibits employers from discharging employees to deprive them of future benefits that they have already earned at the job.

Autor (2003) finds that THS employment does not correlate with public policy or good faith exceptions. Both non-THS and THS employment contracts are legally constrained to follow public policy and good faith exceptions. Consequently, firms cannot circumvent the public policy and the good faith exceptions by using THS firms for employment. Federal court rulings also suggest that employer violations of these exceptions are often considered civil right violations. Thus, it is unlikely that THS employment would gain a competitive advantage against the traditional employment due to good faith or public policy exceptions (Autor, 2003)

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[3] Full time employment is defined as 35 hours of weekly employment

3   Economic Theory on Employment Outsourcing and the EPL

EPL is a ubiquitous part of legislature in a majority of developed countries. The level of job loss protection, i.e. stringency, varies from country to country. Typically EPL sets boundaries and requirements for the employer (and the employees) in the matter of dismissal of redundant workers. EPL constitutes of legislative restrictions on employee dismissals and defines financial compensations for discharged workers. In economic theory, EPL usually involves three parties: the employer, the worker, and the judicial system. The US follows the common law doctrine, in which court rulings are an integral part of the EPL because a single court ruling shapes the nature of similar cases in the future (Boeri & van Ours, 2008).

A dismissed employee can dispute the dismissal in a court the will either validate or invalidate the legality of the dismissal. Court decisions on unjust dismissals cases are an integral part of the evolving EPL framework because these decisions will form the labor markets' understanding on the justified and unjustified dismissals. The US follows the common law doctrine at both the federal and state level. As a result of this approach, a single court ruling in the matter of unjust dismissals can be preemptive, which means that judges and juries previous decisions shape the EPL for future cases.

EPL is divided into two distinct financial components: the transfer component and the tax component. The transfer component constitutes of payments assigned to a third party that can be used for "severance payments and for the mandatory advance notice periods…" (Boeri & van Ours, 2008). Severance payments are payments paid to workers who were discharged by the employer. Severance payments are intended to compensate in situations where workers are laid-off. EPL also usually sets an advance notice period that is the specific time period given to the worker before his termination is actually conducted. Tax component payments are payments attributable to legal fees, such as trials costs and the procedural costs related to a discharging a worker (Boeri & van Ours, 2008).

Stringent dismissal legislation functions as a restraint to the firm's ability to freely adjust its workforce according to the labor demand they face. Empirically, when EPL becomes more stringent, it is likely to reduce both the turnover of both old and new employees. In theory, a stringent EPL can be neutral for employment; if the workers are risk-neutral, wages fluctuate freely with no minimum wage and the tax component of the EPL is zero (Lazear, 1991). The three conditions are merely theoretical and are unlikely to be applicable for empirical evaluations. Thus, the number of studies on the impact of EPL is relatively limited (Boeri & van Ours, 2008).

If Lazear's (1991) third assumption of no transfer payments is relaxed, a dynamic evaluation framework is necessary in order to estimate the effect of EPL. Legal fees related to discharges are usually payments made before or after a worker be discharged. These payments do not have a direct impact on the worker *per se* but Boeri and van Ours (2008) show that an increase in EPL fees is likely to reduce job creation. This effect is intuitive because an increase in EPL increases the current and/or future tax payments that the employers have to account for.

According to the OECD report: "Employment Protection Regulation and Labour Market Performance" (2004), among the countries studied, the US has the most lax EPL. OECD's EPL index is synthetically aggregated and requires human interpretation in order to evaluate and quantify the strictness of the EPL (Boeri & van Ours, 2008). As such, the heuristic evaluation of the strictness and coverage of EPL is common. Everything considered, it is impossible to universally compare EPL stringency on a country level. Boeri and van Ours (2008) suggest that the best approach to evaluate EPL is to disentangle the tax and the transfer components; disentangled tax and transfer components could improve our understanding of EPL's implications on employment and labor markets. This would enable one to study the insider-outsider framework where incumbent workers and future workers may have different preferences on the strictness of the EPL (Lazear, 1991).

Boeri and van Ours (2008) define EPL as a formal institution of laws and rules. EPL does not universally cover the entire workforce, because some workers are exempt from the protection it

provides. For example, a stringent EPL for the traditional open-ended employment contracts may increase the incentive for firms to rethink the usage of alternative hiring routes. One option is to hire workers informal or through temporary employment contracts. Most parts of the EPL do not usually cover the self-employed or the temporary workers..

In relation to other type of employment contracts, workers with open-ended employment contracts are generally the most protected part of the workforce. Both the unemployed and people who are about to enter the labor force do not generally benefit from any form of EPL. Stringent EPL decreases firms' incentive to hire workers for contractually to full-time positions. Thus, EPL is likely to extend unemployment spells. Some have argued that the stringent EPL creates secondary informal and temporary job markets that circumvent the enforced worker protection.

The exact impact of EPL on employment and unemployment is ambiguous;there are countries with strict EPL and low unemployment and, conversely, there are other countries with strict EPL and high unemployment (Boeri & van Ours, 2008). EPL is an institution that has a strong tendency to redistribute the economically efficient solution. Cross-country evaluations are rare, because there are no two countries that are inherently comparable. Therefore, empirical evaluations usually focus on within-country studies (Autor, 2003; Boeri & Jimeno, 2003). EPL employers are likely to experience smaller profits unless firms can enforce lower wages to workers to compensate for the insurance payments firms to make to protect their workers. Lazear's (1991) second assumption on flexible wages must, therefore, hold. If wages are not flexible, firms' profits are bound to drop (Boeri & van Ours, 2008).

Exception to at-will doctrine that increases workers' protection is likely to increase firms' firing costs. Therefore, we study whether employers will outsource part of their labor force to THS firms. Autor (2003) uses the framework of Becker's (1964) hypothesis on employment type and human capital, where traditionally-hired workers are more likely to invest firm specific human capital, as opposed to workers hired temporarily through a THS firm. An investment on firm specific human capital is, in practice, a specialization to a task that will increase their productivity only in the firm where the investment is made. These positions often require specialized high-productivity skills and, therefore, such positions are unlikely to be outsourced to THS firms. The argument is that even if the firing cost were to increase, it would not make THS workers more desirable because of the shorter nature of the employment contract. THS workers would be less eager to invest in firm specific skills because they cannot expect to experience the returns associated with the investment. Anecdotal evidence appears to support Becker's (1964) hypothesis (Autor, 2003).

Shulamit Kahn (2000) studies whether firms are able to distinguish positions and occupations that do not require firm specific skills from positions that do require the set of skills;such positions are likely to be outsourced through a THS firm. In some cases, these positions are easily distinguishable. On the other hand, some companies themselves cannot identify the level of corporate-specific knowledge needed for a job position. Kahn also notes that THS firms charge a mark-up fee for their services that can add a wage premium of up to 50% (Autor et al, 1999) to the traditional employee's salary expenditure cost. Another cost associated with the temporary employee solutions is the constant need to train the new temporary employees. Usually, the training exposes THS workers to a minimal amount of institutional and technical knowledge in order to use the same temporary workers repetitively (Kahn, 2000).

Autor (2003) introduces a two-period model for worker and firms' THS employment and EPL that he uses as a framework for his empirical evaluations of US labor markets. The model is built upon a large number of risk-neutral workers (c) and a large number of firms. In the first period, firms and workers find matches where a worker can match with only one firm and workersinvest

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in firm specific skills $s \in U[0, ŝ]$, where the cost of investment is c(s) and the cost increases strictly. At the end of period 1, firms and workers form matches and there is a productivity shock of $\eta \sim U[-z, z]$. Period 2 is the production period when the workers who remain in the firms determine the output Y of:

$$Y = \gamma * s + \eta$$

where $\gamma * s$ is the return to firm-specific capital and $\gamma \geq 0$ if and only if the worker and the firm continue to second period and create an output. Both the shock η, and the return to firm-specific capital $\gamma * s$ are not competitively priced. Autor (2003) assumes a Nash bargaining model where the period 2 wage is influenced by the worker's bargaining power $\beta \in (0, 1)$, the equation for the consequent wage is:

$$w = \beta(\gamma * s + \eta + \phi)$$

in which ϕ is the firing cost associated with workers who will not work in the second period. Firing cost ϕ is not present during the wage bargaining; thus, it is not part of the compensation that is a result of the Coasean bargaining (Coase, 1960). The firing cost is part of the firm's wage expenditure it uses to discharge the worker. Albeit, firing cost does not improve worker's compensation and, therefore, the equilibrium is a result of Coasean bargaining. If a firm uses THS workers, they can discharge the temporary help with no cost $(\phi = 0)$. Autor (2003) emphasizes that the firing cost (ϕ) alone will not create a setting where the THS employment would dominate traditional employment. One has to also assume that firm specific human capital investments done by the worker require non-verifiable but observable commitment and effort from the worker. Employers are able to observe a worker's investment in firm-specific capital imperfectly.

The two-period model factors in the impact of firing cost given by the Nash bargaining solution. Autor (2003) notes that the worker-firm pair will continue their relationship to the second period if and only if there will be a surplus for both parties to continue the employment relationship:

$$Y \geq -\phi$$

and the worker's bargaining power equation is satisfied when $w \geq 0$ and $Y - w \geq -\phi$, which defines a model where workers receive a positive wage and the company makes enough profits to

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offset the firing cost. From equations 1 and 2, one can derive worker's specific capital investment in relation to expected earnings and the cost of the investment:

$$\max E(U) = \boldsymbol{E}(w|w \geq 0)\boldsymbol{P}(w \geq 0) - c(s).$$

We assume that the shock variable η is uniformly distributed, and that the first-order solution for worker's specific capital investment is:

$$c'(s^*) = \frac{\beta\gamma(z + \gamma \times s^* + \phi)}{2z}$$

From the specific capital investment function, one can find an interior solution that exists when $0 < s^* < \hat{s}$ and $\gamma > 0$. The training cost function must also be convex for the interior solution to exist. The solution function observes that a worker's investments in skills increase conjointly with the productivity of the specific capital γ and bargaining power β. Worker's investment in specific capital is also positively correlated with the firing cost ϕ. When the firing costs are high, employers are less likely to discharge their employees. Thus, workers are more likely to make large investments in firm specific capital (Autor, 2003).

Firms face a trade-off between maximization of specific capital investment and minimization of the firing costs. In order to quantify the trade-off Autor (2003) suggests that the expected profitability of a specific capital investment as the function of optimal firing cost. The function is defined as:

$$E[\pi(\phi)] = \frac{(1 - \beta)[z + \gamma \times s(\phi) + \phi]^2}{4z} - \phi$$

in which the $s(\phi)$-function emphasizes the strong relation of firm specific investment and the potential firing cost. Firing cost ϕ is on both sides of the equation. It naturally increases the cost of termination, but also the expected profits from the workers, because they are incentivized to invest in firm specific capital (Autor, 2003). Firms may also introduce positive firing costs ($\phi^*(\gamma) \geq 0$) in order to incentivize their employees to invest in firm specific human capital especially when there could be high returns for their investment (large γ)

$$\frac{\partial^2 \pi}{\partial\gamma\partial\phi} \geq 0$$

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Court-mandated firing cost (ϕ^c) will have an effect on a firm if and only if the court mandated firing cost exceeds optimal firing cost ($\phi^c \geq \phi^*(\gamma)$). When a court mandate increases the firing costs (a shock) for firms, they optimize their employment strategy based on the new level firing costs. If the court mandated firing cost exceeds the optimal firing cost, firms are likely to reconsider their hiring practices. Occupations where the firm-specific human capital investment increases a worker's productivity (large γ), a higher firing cost will not likely result in a surge of outsourcing (Autor, 2003). When there is an increase in the firing costs, firms will compare the relative profitability of outsourcing to the marginal profitability of a capital investment caused by the increased firing costs.

THS firms often provide labor force for low-skilled blue collar and administrative support positions. While these occupations constitute 30% of the overall employment, 63% of the labor force in the aforementioned positions is employed via THS firms. Some white collar occupations in computing and medicine use THS workers, even though the skills needed are can be very technically demanding and often require a degree (Autor, 2003).

Even though some THS positions require very specific and extensive formal training if the skills are applicable throughout the industry, these occupations can be hired via THS firm (Cohany, 1998). To further develop his model, Autor (2003) uses an equation to estimate the correlation between general skills and THS employment penetration in the US data. He estimates the share of THS employment on 485 detailed occupations (j):

$$THSshare_j = \propto + \beta_1 \times Trained_j + \beta_2 Tenure_j + \varepsilon_j,$$

in which Autor (2003) shows that the occupational training is statistically significant and economical determinant of the nature of the type of employment contract . THS firms are prevalent in occupations where the firm does not provide training and the employment spells are shorter.

Autor (2003) estimates that in a given occupation an increase of one standard deviation of on-the-job training will decrease the mean share of THS usage by 25%. In order for on-the-job training to decrease the THS usage, the training has to be formally organized by the firm. Informal on-the-job training does not affect the level of THS employment, for it is more difficult to quantify (Autor, 2003). Portugal and Varejão (2009) find similar results in the Portuguese labor markets. They find that temporary workers, who take part in official on-the-job training, are more likely to be promoted to a full-time position.

4   Relevant Empirical Research

The precursor in the economic literature (Autor et al., 2007) on the EPL and its impact on employment is Edward Lazear's (1990) paper "Job Security Provisions and Employment." Lazear (1990) studies the developed labor markets, where the labor force is generally protected by some magnitude of job security provisions. Such enforced labor market provisions are likely to affect the labor market equilibrium. To study the impact of labor market provisions Lazear (1990) introduces a 2-period theoretical model in which a severance payment requirement is imposed by the government in period 1. This payment is put in effect if the worker is not employed in period 2. The model predicts that the worker pays a fee that he will either get as the severance payment or as a wage that is increased by the magnitude of the severance payment. Lazear's (1990; 1986) 2-period theory has not been used successfully in empirical evaluations

The data Lazear (1990) makes use of is a macro level data set and it has data on 22 developed countries. The data covers the years 1956 and 1984. The data follows measures on employment, average hours worked and gross domestic product. Because of data aggregation certain countries, such as Canada, are dropped from the final evaluation as the number of data points is insufficient. The OLS estimates show that the severance payment legislation that introduce a longer three month wage severance payment reduce employment-to-population ratio by approximately 1%. Therefore, the unemployment rate can also decrease because some discouraged workers may exit the labor force.

Lazear (1990) finds that a severance payment scheme is likely to turn some full-time jobs into either part-time or temporary jobs. This could occur because non-open ended job contracts are not covered by severance payment packages. Lazear's (1990) estimations suggests that extending the severance payment scheme from 1 month to 3 months is bound to transform over 9 million US jobs from full-time to part-time. Lazear (1990) acknowledges the risk of unclear causation, endogeneity, and cross-country differences in the labor markets. These factors affect Lazear's (1990) insomuch that he considers the study to be the antecedent for more refined empirical evaluations of EPL's impact on employment.

Boeri and Jimeno (2003) study the variable enforcement effect on the EPL in the US and Italy. Their evaluations of the US labor markets lack sufficient data for empirical evaluations of the counterfactual control group. Therefore, they focus on the more robust Italian data set. Italian labor markets have a two tier EPL division based on the size of the firm. Firms with more than 15 workers are required to follow the "tutela reale" protection clause on individual dismissals. The clause imposes a range of compensation options for wrongfully discharged workers. The fees imposed on the company are based on the number of workers the company employs.

Workers who are employed by a company with 15 or more workers are covered by the "tutela reale" clause. Workers under "tutela reale" are eligible to seek for reinstatement in the firm with a monetary compensation for the wrongful discharge. The reinstatement option also carries a financial compensation minimum of 5 months of earnings at the job. If the discharged worker does not wish to be reinstated to his old firm, he is entitled for a compensation worth of 15 months of earnings. Firm that employ fewer than 15 workers are exempt from the "tutela reale" clause. Such small firms follow the so-called "tutela obbligatoria" approach in which the compensation for wrongfully discharged worker range from 2 to 6 months of earnings depending on worker's seniority and experience.

Boeri and Jimeno (2003) conclude that companies with more than 15 workers have to endure more severe financial consequences when they are found guilty for wrongful dismissals. This artificial 15 employee barrier will decrease firm's demand to hire the 16th worker. The data used for Boeri and Jimeno's (2003) estimations are collected from the Italian social security records. The social security database gathers firm information from all private employers in Italy; thus, it creates a complete universe of the Italian labor markets. The same social security data is also used for other EPL evaluations (Garibaldi et al., 2003; Schivardi & Torrini, 2003). Boeri and Jimemo's estimations (2003) use yearly data from 1986 to 1995. The sample Boeri and Jimeno (2003) use is likely to over-represent the large firms because small firms are more likely stop operating during the timespan of the study.

Italy's EPL reform of 1990 enforced a minimum severance payment of six months for unjust dismissals to firms with fewer than 15 workers. Before the 1990 reform, small firms were not required to follow the stricter EPL guidelines that bigger companies had to follow. In addition to the 15 worker threshold difference in EPL stringency there is also a simultaneous shock that increases the amount of severance payments for small firms. In order to control for the small company shock, Boeri and Jimeno (2003) use a double-difference approach in order to evaluate both the impact of the 15 worker threshold and the 1990 reform on small firm employment and hiring (Boeri & Jimeno, 2003).

The control group for Boeri and Jimeno's (2003) natural experiment is a sampling of firms that operate in services and employ 15 or more workers. For example, a company operating in services with fewer than 15 workers is under the small-firm "tutela obbligatoria" threshold that experiences the 1990 reform (shock). This allows Boeri and Jimeno (2003) to evaluate and compare the impact of the reform. Garibaldi, Pacelli, and Borgarello (2003) focus on companies with the maximum of 30 workers in order to set the 15 worker threshold in the mean of the sample.

Essentially, Boeri and Jimeno (2003) find that EPL has a mild positive and statistically significant impact on the firm's likelihood to persevere through shocks. More stringent EPL also decreases the turnover ratio of workers. Using the same the data, Schivardi and Torrini (2003) show that firms right below the 15 worker threshold are the least likely firms to grow in size. These findings appear to be in line with their economic theory. They also find that the small firm threshold does correlate with the hiring and firing practices of a firm. Due to the 1990 reform, small firms will also decrease their workforce turnover. Boeri and Jimeno (2003) conclude that empirical evaluations encounter a multitude of interferences, such as economic layoffs that cannot be entirely controlled for.

Autor, Donohue III, and Schwab (2002) study whether at-will doctrine exceptions affect employment and wages in the US labor markets. The econometric method Autor et al. (2002) make use of is the difference-in-differences method to evaluate the social cost of the implied

contract exception. Autor et al. (2002) use monthly Current Population Survey (CPS) data from 1978 to 1999 to measure employment and earnings. CPS consists of a sample of 100,000 adult monthly surveys. Similar to Autor's (2003) findings, Autor et al. (2002) find that the implied contract exception to employment at-will has a negative impact on employment.

Different theories on EPL's effect on employment hypothesize inconsistent results on stringent EPL's effect on employment levels. Lazear (1991) and Blanchard and Katz (1997) show that the impact of increased restrictions on firing costs on unemployment is uncertain. If one makes the assumption of efficient labor markets, an employer-side's firing costs are fully offset by worker-firm bargains. For example, workers can post a bond that is equal to the associated firing costs. Summers (1989) presents a theory, where the workers are willing to take a wage cut equal to the employer's marginal cost of EPL. Empirically, such a Coasean arrangement is rare. Therefore a higher level of employment protection, while valued by the workers, shifts the labor demand curve inward and also shifts the labor supply curve outward. The empirical findings by Autor et al.'s (2002) paper are quite contradicting because their findings suggest that workers do not value increased job security ex ante.

Autor, Donohue, and Schwab (2002) study whether implied contract exception affects employment rate. Autor et al.'s (2002) findings show a statistically significant decline in the employment rate when the implied contract is enforced. After the adoption of implied contract, employment-to-population rate decreases by 1.5 to 2 percentage points in the next two years. The nadir of the drop occurs on the 30 month mark after the shock. Relative to other workers, women who are under 40 years old with below the average level education are most likely to see the negative impact on their employment. The impact of implied contract on employment-to-population in this cohort decrease ranges from 1.0 to 2.7 percentage points. Only one demographic group is not negatively affected by the implied-contract. College-educated 40 year old and older men and women's employment-to-population levels are not affected by the implied-contract.

In order to study the effect of higher firing cost on labor markets using empirically realistic setting, Kugler and Saint-Paul (2004) present the idea of adverse selection in the hiring process to the theory of EPL. Essentially, their innovation is to introduce the theory of adverse selection into the hiring process. In the given context of EPL and employment, adverse selection hypothesizes that an increase in the firing costs would discriminate the unemployed furthermore.

Under the adverse selection theory, one assumes that employers prefer to hire an employed candidate over an unemployed candidate (ceteris paribus). Therefore, employed candidates are more likely to get hired. If we assume that employers behave according to the adverse selection theory, stringent EPL further segregates the employed and the unemployed as it extends the unemployment spells of the unemployed. Kugler and Saint-Paul (2004) test their hypothesis on National Longitudinal Survey of Youth (NLSY) data. NLSY data samples over six thousand persons for the years 1979-1984 and 1996. NLSY focuses on a group of young workers from the age of 17 to 39. The NLSY data are geographically labeled. Geographic labels allow Kugler and Saint-Paul (2004) to distinct the workers who were and were not affected by the implied-contract exception. The NLSY data allow the identification of the employed, unemployed, and also the people who are in the process of job-to-job transition.

When an implied-contract exception is enforced on a state-level, the probit model estimates show that the probability of the unemployed to be re-employed decreases relative to the employed. As an intuitive explanation for the uncertainty on the hiring practices, Kugler and Saint-Paul (2004) refer to a widely cited market of lemons theory (Akerlof, 1970; Gibbons & Katz 1991).

The lemons theory suggests that the type of dismissal will impact the unionized worker's probability to be re-employed. For example, relative to dismissed non-union members, dismissed union members are more likely to be re-employed because unionized workers are discharged "subject to layoff-by-seniority rules," and not based on their competence. Thus, young unionized workers look more desirable, as opposed to young non-unionized workers looking for a job. On the whole, Kugler and Saint-Paul (2004) conclude that high dismissal costs will reduce the flows

out of and into unemployment. Their results are in line to other papers in the field (Boeri, 1999; Bertola & Rogerson, 1996).

Bertola and Rogerson's (1996) paper "Institution and Labor Reallocation" proposes causes for why the European labor markets tend not be as rigid as labor economic theories suggest. When compared to the US labor markets, European labor markets are noticeably more rigid and more stringent EPL (Boeri & van Ours, 2008). These rigidities should result in the worker turnover ratio that is significantly lower in Europe than it is in the US. In fact, the turnover rates in the US and in Europe are surprisingly comparable.

Even though there are similarities in European and US labor market turnover rates, intercontinental evaluations tend to be quite flawed because we cannot assume all else at the labor markets to be equal. For one, European wage negotiations are usually centralized and are inspired by the "equal pay for equal work" principle. The aforementioned salary principle is bound to lead to a system with centralized wage negotiations where workers are uniformly paid salaries. Such wage compression would, in fact, make wages less correlated with economic shocks. Therefore, even when a positive shock is improving the firm's profits, the workers still receive a wage predetermined by the centralized wage negotiations. Bertola and Rogerson (1996) suggest that wage compression is likely to result in "…a higher employer-initiated job turnover…"

Boeri and van Ours (2008) acknowledge that labor markets are comprised of multiple formal and informal institutions. These institutions make it fundamentally impossible to disentangle a single policy (a shock) for evaluation without some level of interference by other factors. On cross-country and intercontinental evaluations, these factors are even more likely to make the results and findings subject to dispute (Bertola & Rogerson, 1996).

Bertola and Rogerson (1996) suggest that a stringent EPL is highly-correlated with stringent wage-compression policies and strong labor market institutions. According to Bertola and Rogerson (1996), European policies are likely to reduce the job-to-job mobility owing to the

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reduced incentive for on-the-job job search. Due to the difficultness to conduct robust empirical evaluations, Bertola and Rogerson (1996) conclude that they do not have enough empirical evidence to support their model.

Good intentioned government interventions can sometimes have the opposite desired impact. Acemoglu and Angrist (2001) study the effect of the Americans with Disabilities Act (ADA) that was meant to improve the labor force participation and employment of those with disabilities. ADA outlawed discrimination based on disabilities and required firms to provide sufficient premises for the disabled people to work in. Firms would have faced noticeable fines if they had not complied with the ADA code when it was put in effect in 1992. During the following 5 years after the ADA was put in effect, 91 thousand ADA violation claims were filed (Acemoglu & Angrist, 2001)

In reality, ADA increases the cost associated with accommodation of disabled workers at the workplace. If firms were to hire workers with disabilities, the firms would also have to familiarize themselves with ADA regulations and adjust to a lower level of productivity. The productivity of a firm could decrease, because ADA requires fair amount of time spent on complying with the criteria The risk of a wrongful dismissal law suit by a worker with a disability is also likely to increase the cost associated with the disabled workers after the ADA came into effect (Acemoglu & Angrist, 2001).The data used to evaluate the impact of the ADA is from the yearly Current Population Survey (CPS) where the CPS participants self-reported their ability or limitation to work. The data was collected from 1988 to 1997 and the subsamples were derived from men and women aged 21 to 58.

The difference-in-differences (DID) estimation method is used to study the ADA. The treatment variable is the interaction of disabled individuals beginning in 1992. Using the DID estimation method, Acemoglu and Angrist (2001) find that disabled men aged 21-39 experience a significant drop in the number of work weeks per year. Disabled women in the same age bracket also experience a noticeable drop in the measure. Conversely, the results in workers aged 40-58

are less clear. Older men decreased the number of weeks worked, but women actually increased the numbers of weeks worked.

Acemoglu and Angrist (2001) continue to develop the standard difference-in-differences estimation method by controlling for state-level variation, firm-size, and other factors that could affect the employment of the disabled. These estimations bring some ambiguity to the initial estimations but they do not factor out the ADA's negative impact on the number of working weeks by the disabled workforce. The ADA does not appear to increase the number of wrongful discharges among the workers with disabilities. The ADA does not induce to the climate of dismissal practices because it is likely cheaper to accommodate disabled workers than it is to face a lawsuit. Some critics have described the ADA to be one of the exceptions that is designed to nullify the traditional employment at will doctrine (Olson, 1997), for it extends the employment protection.

Studies on the correlation of EPL and productivity show that EPL is unlikely to affect aggregate productivity. Autor, Kerr and Kugler (2007) study whether employment protection reduces productivity. This paper is one of the first micro level evaluations where Autor et al. (2007) study whether the decreasing employment volatility affects the productivity negatively. The evaluation data is drawn from Longitudinal Business Database (LBD) and the Annual Survey of Manufacturers (ASM). The LBD is a comprehensive and balanced data set on the dynamics of the manufacturing and non-manufacturing sectors. The ASM allows the continuous following of a subsample of companies followed year-over-year. This allows Autor et al. (2007) to monitor companies within the representative sample continuously.

The findings of Autor et al.'s (2007) study are somewhat surprising. The good faith exception to the at-will doctrine is likely to decrease the employment volatility in states that adopted the exception. Employers are likely to experience costs associated with the implementation of the good will doctrine. If the short run adjustment costs an affected firm's choice on the division of capital and labor inputs, this ultimately impacts the productivity of the firm. Autor et al. (2007) suggest that the good faith exception leads to increasing investments in capital and skill intensive

labor, i.e. the deepening of capital and labor. Thus, labor productivity increases, whereas the total factor productivity (TFP) decreases. Autor et al. (2007) describe their findings as suggestive, and the authors suggest cautious interpretation of the results presented in this study (Autor et al., 2007).

Bassanini, Nunziata and Venn (2009) conducted a cross-country evaluation among the OECD countries on the EPL on dismissal regulation and its impact on productivity. Job productivity growth has been a seminal part of the recent economic growth in developed OECD countries that are studied from 1982 to 2003. Growth in productivity has caused at least half or more of the growth in developed countries. Bassanini et al. (2009) study whether stricter EPL, and especially more stringent employee dismissal regulation, affects productivity negatively.

Empirical evaluations of EPL's impact on productivity are rare. Bassanini et al.'s (2009) paper is one of the first and recent aggregate and industry level studies in the field. In order to create a comparable cross-country evaluation, the time-series evaluations were conducted on an industry-level and focus of the study is on industry-level. The industry-level approach supports the assumption that EPL is not universally binding or similar. EPL is more likely to have different levels of control over different industries. In effect, changes in EPL are more effective if firms actually use layoffs as the primary tool for layoffs (Bassanini et al., 2009).

For their empirical evaluations of EPL and its impact on total factor productivity (TFP), Bassanini et al. (2009) make use of Inklaar et al.'s (2008) data set that is collected from the EUKLEMS database. EUKLEMS provides multiple measures of TFP, which are merged with CPS Displaces Workers Supplement. By using the difference-in-differences method Bassanini et al. (2009) find that mandatory dismissal regulation does not impact the TFP growth of temporary positions. This model does not fully control for cross-country, cross-industry, and time-trend causality. Bassanini et al. (2009) show that for industries where the dismissal regulation is binding, more stringent rules are likely to decrease TFP growth. One can also infer from Bassanini et al.'s (2009) results that aggregate labor productivity growth slows down when EPL becomes more stringent.

European labor market reforms of the 1990s are likely to be a partial cause for new job creation that was not attributable to the economic growth at the time. The employment growth of 1990s contrasted starkly with the 1980s labor markets where many economies grew without new job creation. Boeri and Garibaldi (2007) study the transitional dynamics of two tier EPL reforms on productivity and employment.

European two-tier EPL reforms create a setting where new jobs are more likely to be temporary, rather than permanent. These reforms do not affect incumbent workers. Multiple European countries introduced two-tier reforms in order to improve labor market flexibility and competitiveness. In theory, a law change that decreases the rigidity of the temporary employment legislature will change the way firms use temporary contracts for future hires. The incumbent workforce will remain in the firm and leave the firm over time as they are by the temporary contract workers. During the "honeymoon" period, the employment level will temporarily increase, but will dissipate as soon as the incumbent workers leave the workforce.

Boeri and Garibaldi (2007) make use of an assortment of Italian micro-level employment data to study their hypothesis that changes in EPL do not affect employment growth. Labor Force Survey (LFS) and Work Histories Italian Panel data are used to compile sufficient measures of employment and productivity ex- ante the labor institution reform. Boeri and Garibaldi (2007) make use of a two wave panel data from 1995 to 1997 and from 1998 to 2000. The "Pacchetto Treu" reform gradually expanded the range of temporary contracts firms use to fixed-term contracts as the standard procedure for new worker job entry. Boeri and Garibaldi (2007) find evidence to support the "honeymoon" effect of employment because of fixed-term contracts.

The 'honeymoon" effect period could happen as a result of the growing use of temporary contracts. Temporary contracts are bound to increase employment levels, but only temporarily. The "Pacchetto Treu" reform's impact on productivity is somewhat ambiguous. Boeri and Garibaldi's (2007) estimations suggest that firms that were most active in hiring new workers experienced a greater decline in productivity. Firms that were not as active in hiring new workers

did not experience a similar drop in productivity. Also, the average productivity is likely to recuperate once the employment levels readjust to the normal and lower pre-shock levels. Thus, policymakers are advised to take the "honeymoon" period of increased employment into account when appropriate actions are considered to spur economic growth and employment (Boeri & Garibaldi, 2007).

5   Data

This thesis makes use of Autor's (2003) state-level panel data on employment and labor force demographics. The data set is collected from the Census Bureau's Country Business Pattern (CBP), Bureau of Labor Statistics State, State and Area Employment Statistics, and Current Population Survey (CPS) sources. The data are aggregated to a state-level year-to-year data from 1979 until 1995. Data are available for all 50 states; Washington, District of Columbia is omitted from the sample. As it is a yearly data, the 50 states in the estimations are represented by 17 observations. CBP data is collected in the month of March in each state.

Autor's (2003) data creates a complete universe of the U.S. labor markets and also provides a tally of the workers employed by the THS firms. The number of workers employed by the THS firms is a sum of both full-time and temporary workers. Given the data, we cannot distinguish these sub-groups from one another. Autor (2003) assumes that the share of the full-time workers who for THS is insignificantly small in relation to the number of temporarily employed workers who work the THS firms' clients. The classification of the temporary help services expanded slightly in the 1987 revision of the Standard Industrial Classification System (SIC). The expansion did not impact the THS employment's proportion on a state-level; hence, the yearly data should absorb the significance of the increase (Autor, 2003).

Employment data on non-THS related employment is collected from the Bureau of Labor Statistics State and Area Employment Statistics. The statistics on non-THS related employment enable the evaluation of the share of THS employment in relation to the overall employment. Outgoing Rotation Group (ORG) files of the CPS data are used to create demographic variables in order to control for the state-level characteristics. Labor force demographics on a state-level characterize states' employment, education, age, gender, marital status, and the composition of employment by industry (Autor, 2003).

Estimated state-level union penetration is a hybrid measure of union penetration and the union bargaining power. The penetration measure is derived from Hirsch, Macpherson and Vroman

(2001) paper. Hirsch et al.'s (2001) estimated measure takes account of both the members of the union and the industries where the industry-level union coverage extends to non-union members. Both the union membership and the union coverage have steadily decreased during the timespan of the study. In 1979, 24.1% of the labor force was union members and 27% of the entire labor force was covered. Sixteen years later, in 1995, the respective metrics had decreased to 14.9% and 16.7% (Hirsch et al., 2001).

The data on state-by-time exceptions to the employment at-will are drawn from data sources by Morriss (1995), Postic (1994) and the Bureau of National Affairs (1997). According to Autor (2003), characterization of the common law is not definite nor wholly objective. In order to mitigate the risk of subjectivity, Autor (2003) uses Dertouzos and Karoly's (1992) characterization method. When state court or legislation acknowledges the implied contract in a state, the binary change from control to treatment occurs.

Replications of Autor's (2003) estimations with both entropy balanced and unadulterated data are presented. In addition to the full 50 state sample, a comparative evaluation of using a subsample of 43 states is presented. This is conducted in order to create a cleaner and randomized controlled trial. Using a smaller sample of states we could potentially study states that actually experienced the implied contract shock to the at-will doctrine. This thesis constructs a subsample of 43 states by dropping the first seven states that adopted the implied contract exception[4]. According to Autor (2003) data, seven states had already adopted the exception on 1979 or as early as 1959 (California) and 1972 (Michigan). Usage of the subsample of 43 states allows us to have less diluted ex-shock tenure that could potentially provide more representative and robust results on the impact of implied contract on the THS employment.

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[4] Omitted 7 states: Maine, Illinois, Oklahoma, Idaho, Washington, California and Oregon

6 Methodology

6.1 Difference-in-differences Method

Ashenfelter and Card (1985) introduced the basics to the difference-in-differences (DID) method (with fixed effects), a linear econometric estimation method. The DID method is often used to analyze impact of a treatment or a shock on empirical panel data. The fixed effect model incorporates variables such as a state and region variables that do not change over time. A disciplined DID estimation measures the average treatment effect (ATE). The basic setup of DID involves two groups and two time-periods and the data shows the pre- and post-treatment periods. The basic model for DID as

$$y = \beta_0 + \beta_1 dB + \delta_0 d2 + \delta_1 d2 * dB + u$$

in which y is the outcome variable and $d2$ is the second period dummy. Dummy variable dB controls for potential differences between the control and treatment group. $d2$ variable controls for aggregate factors that affect the growth of the outcome variable y sans policy change. The difference-in-differences coefficient δ_1 is multiplied with the interaction variable $d2 * dB$ which equals to one when for the treated states on the second period. Estimate for difference-in-differences can be estimated as the following subtraction.

$$\hat{\delta}_1 = (\bar{y}B, 2 - \bar{y}B, 1) - (\bar{y}A, 2 - \bar{y}A, 1)$$

It is integral for successful DID setup that only one of the two groups receives treatment. The division into control and treatment group should be randomized, i.e. there should be no self-selection to either group. The treatment occurs in the beginning of the second period and no units should be exposed to the treatment or shock on the first period (Imbens & Wooldridge, 2008). The control group units will never exposed to the treatment. In order to calculate the treatment's average gain over two periods, we subtract the control (non-exposed) group's change from the treatment (exposed) group's change. Imbens and Wooldridge (2008) reiterate that: "…double differencing removes biases in second period comparisons between the treatment and control group…" Autor (2003) uses the DID method to evaluate the implied contract's impact on THS.

One of the most widely cited (Imbens & Wooldridge, 2008; Angrist & Pischke, 2008) empirical DID papers to date is Card and Krueger's (1994) paper on the impact of New Jersey's raise in state-level minimum wage. Card and Krueger (1994) study the state of New Jersey's minimum wage hike's impact on fast-food restaurants that use often use minimum wage employment. The fast-food restaurants surveyed were located right at the border between New Jersey and Pennsylvania. A sample of 410 fast-food restaurants were surveyed along both sides of the New Jersey-Pennsylvania state line. The sample allows Card and Krueger (1994) to study the average treatment effect (of employment) between New Jersey and Pennsylvania. As the data is collected close to the state line, the sample should be more state agnostic. Angrist and Pischke (2008) discuss the outcomes one can factually see. One cannot see what would have happened to employment levels in New Jersey had they not increased the minimum wage. The same counterfactual applies to Pennsylvania; one cannot see what would have happened to fast-food employment in Pennsylvania if they had increased the minimum wage.

Therefore, one must assume that the response to the shock would be the same in both control and treatment states (Angrist & Pischke, 2008). Similarly one must assume that if the shock had not occurred in New Jersey, its employment levels would correlate strongly with the Pennsylvanian employment figures. Card and Kruger's (1994) find that employment in fast-food establishments did not decrease in New Jersey given the higher minimum wage, whereas the fast-food worker employment decreased in Pennsylvania. This finding contradicts the basic notion of the negatively sloping labor demand curve (Card & Krueger, 1994). In order to validate Card and Krueger's (1994) findings on minimum wage, one must assume that both states would have experienced similar employment and economic trends aside from the minimum wage shock that occurred in New Jersey. Card and Krueger (2000) publish a follow-up paper as a response to the public debate on their original paper. Card and Krueger (2000) show that the yearly employment level fluctuates in both states without a strong correlation; given this, Pennsylvania may not be a good control sample for New Jersey.

When one cannot be certain that the control and the treatment groups are equally likely to receive treatment, one option is to adjust or balance the independent covariates that are controlled for. The covariate balance means that the treatment and control groups have identical joint distribution of observed covariate values. Because of the unforgiving nature of the linear regression, even a minor covariate adjustment between the treatment and control group may change the results from the initial estimations. The most basic approach would be to find close matches based on covariate values from the control and treatment group and calculate the ATE among the close matches. This matching method may be useful in small sample sizes, but it has its limitations with larger sample sizes (Imbens & Wooldridge, 2008).

A more refined approach to finding a representative ATE is to match the control and treatment group observations using the propensity score method. Propensity score is an implicit score that is calculated by comparing an observation's independent covariate values to the baseline characteristics (Imbens & Wooldridge, 2008). Given the known covariates and observations that factually received treatment, one can calculate the propensity score and subtract the treatment. The propensity score is also an observation's probability to receive the treatment, thus, ideally, propensity score distributions should be not be different for the treatment and control groups. Austin (2011) suggests that when the propensity score method is implemented into an empirical data, it can mimic certain characteristics of a randomized controlled trial (RCT).

An RCT framework is often considered to be "the gold standard" of program evaluation for empirical studies (Kaptchuk, 1998). RCT are designed to decrease the likelihood of self-selection to be in either control or treatment group. RCT are a common procedure in medicine, where it is easy to randomly assign treatment and control to a group of candidates with homogenous characteristics. Hainmueller (2011) suggests that a covariate balance allow "…researchers "manually" iterate between propensity score modeling, matching, and balance checking until they attain a satisfactory balancing solution.". Manual iteration approach is likely to contradict with the goal of finding stochastically balanced covariates because it relies on assumptions made by the researcher (Hainmueller, 2011). In the matter of propensity, a score researcher can affect the weights used to create the covariate balance. Thus, it is possible to iteratively find a balance

that suits the given agenda. One must determine a model used for the propensity score model. Drake (1993) notes that if the model chosen for the propensity score is misspecified it may create an even greater imbalance between the control and treatment groups. Hainmueller (2011) argues that a propensity score can be an accurate way to correct the imbalance if and only if the model specifications are correct and the sample size is sufficiently large. When the reweighting via the propensity score is done correctly, it makes the causal interpretation more robust, for the exposure to treatment is not confounded by underlining covariates (Lunceford & Davidian, 2004).

6.2   Entropy Balancing and Other Preprocessing Methods for Empirical Data

New refined data preprocessing and balancing methods have recently gained traction, especially for empirical evaluations in social sciences (Watson & Elliot, 2013). Modern preprocessing methods are often used to balance the data prior to estimations. As the various preprocessing methods are fairly recent the academic literature in the field of data balancing is both active and currently developing (Imai & Ratkovic, 2014; Diamond & Sekhon, 2012). Hainmueller (2011) introduces entropy balancing as a powerful data balancing method for observational studies with a binary treatment variable.

Entropy balancing incorporates an improved covariate balance into the sample units using the balanced constraint weight function. The reweighting method can be applied to any standard model with a binary treatment variable. Entropy balancing and other data balancing methods are often used when there is a strong likelihood of selection bias between the control and treatment groups (Watson & Elliot, 2013). The automatic preprocessing step of entropy balancing improves the covariate balance between the treatment and control groups. In fact,   an improved balance ought to make the treatment variable less dependent on the original selection bias that led to the group selection. Unlike the propensity score method, entropy balancing is less model- and researcher-dependent because it orthogonalizes the treatment indicator with respect to the independent covariates. The propensity score method relies on the "manual" and iterative process where the researcher attempts to find an adept balancing solution. While this gives the researcher

more flexibility it also increases the potential risk of error caused by the researcher (Drake, 1993; Diamond & Sekhon, 2012; Hainmueller, 2011).

Weights used for entropy balancing are calculated by minimizing a loss function $H(w)$. In order to minimize the loss function, entropy balancing creates a set of unit adjustment weights $W = [w_i, \ldots, w_{n0}]$, that minimize the entropy distance. The distance is measured as entropy divergence:

$$(H(w_i) = w_i \log(^{w_i}/q_i))$$

between the adjustment weights W and the vector of base weights $Q = [q_i, \ldots, q_{n0}]$. The loss function $H(w)$ calculates the distance between the distribution of the base weights determined by the vector $Q = [q_i, \ldots, q_{n0}]$ and the approximated distribution of control weights defined by $W = [w_i, \ldots, w_{n0}]$. The loss function $H(w)$ is always non-negative because of the given constraints. As a result of the non-negativity of the loss function, the smaller the function value, the shorter the distance. Consequently when $W = Q$, the loss function equals zero (Hainmueller, 2013).

Calculated weights are, then, adjusted as needed in accordance to the balance constraints. The adjustment process keeps balance constraints as close to the base weights as possible. Because of the adjustment process of the given constraints, the reweighted data will retain the maximum amount of information. Hainmueller (2011) allows the researcher to set the base weights uniformly when $q_i = 1/n_1$[5]. Alternatively when one knows already existing sample weights, n_w, base weights can be set accordingly at $q_i = n_w$. Entropy balancing adjustment weights can be calculated to allow alignment of a non-probability sample (n_0) with a reference sample (n_1).

Entropy balancing can be applied for data where each observation is subject to a binary treatment $D_i \in \{1,0\}$ and $D_i = 1$ if the unit receives treatment, $D_i = 0$ if the unit is in the control group one can set up the theory for entropy balancing scheme. In the given scenario, the end goal is to

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[5] Where the n_1 is the reference sample

reweigh the control group's covariate balance than that of the treatment group. Subsequently, one can estimate the population average treatment effect (PATT)

$$\tau = E[Y(1)|D = 1 - E[Y(0)|D = 1]$$

that calculates the difference in mean outcomes between the treated units and reweighed control units. The counterfactual of the calculated mean i.e. the mean difference between reweighed treatment units and control units by

$$E\left[Y\overline{(0)|D} = 1\right] = \frac{\Sigma_{\{i|D=0\}} Y_i w_i}{\Sigma_{\{i|D=0\}} wi}$$

where w_i is defined as control unit's weight. The following weights are subject to minimizing the loss function

$$\min_{w_i} H(w) = \sum_{\{i|D=0\}} h(w_i)$$

is subject to following three constraints that minimize the loss function H(w) to $h(\cdot)$. Loss function minimization calculates a distance metric and $c_{ri}(X_i) = m_r$ which is a set of R balance constraints imposed on the covariates of the reweighted control group. The first constraint of the scheme

$$\sum_{\{i|D=0\}} w_i c_{ri}(X_i) = m_r \ with \ r \in 1, ... R$$

defines the balance constraints defined by the equation. In the equation, m_r is the moment that contains the rth order moment of a given variable X_j, which value is drawn from the target population, i.e. the treatment group.

The following two equations define the normalization constraints

$$\sum_{\{i|D=0\}} w_i = 1 \ and$$

$$w_i \geq 0 \ for \ all \ i \ such \ that \ D=0$$

where the first condition states sum to normalized constant of one. This condition is not binding which means that the exact value is for normalization is arbitrary. Instead of one the value of

n_1 reference sample can be also used as the constraint. The second condition sets up limits to the aforementioned non-negativity constraint. Also, the second constraint is not binding, but it simplifies the understanding of the entropy balancing method (Hainmueller, 2011).

Entropy balancing bears some similarities with the conventional propensity score approach. Similar to the propensity score method, entropy balancing uses a logistic regression to compute the balance checks. These balance checks are used to confirm that they equalize with the covariate distributions. The underlining difference between the propensity score balancing and the entropy balancing is that the entropy balancing's adjustment process is done "backward ." Third, a potentially large number of balance constraints are incorporated in the process. This implies that for all prespecified moments, the covariate distribution of the treatment and control group matches perfectly in the preprocessed data. Fourth, after the prespecified level of covariate balance has been determined, entropy balance finds the "…the set of weights that satisfies the balance constraints but remains as close as possible (in an entropy sense) to a set of uniform base weights to retain information" (Hainmueller, 2011).

According to Hainmueller (2011), entropy balancing has three advantages over the traditional propensity score reweighing. First, entropy balancing allows one to achieve a high degree of covariate balance between the control and the treatment group relatively automatically. Second, entropy balancing retains the valuable information of the balance constraint as it imposed by the researcher. Third, entropy balancing provides a sufficiently versatile scheme as it allows multiple constraints for normalization (Hainmueller, 2011) and the weights are identical for the treatment and control groups (Imai & Ratkovic, 2013). Fourth, the computational optimization units are globally convex and therefore the units are well behaving (Hainmueller, 2011).

Hainmueller (2011) demonstrates the efficiency of the entropy balancing design by comparing the mean squared error (MSE) of his balancing scheme to alternative data preprocessing methods. Both the empirical and simulated Monte Carlo data comparisons are conducted to compare the entropy balancing to following alternatives: difference in means, propensity score matching, Mahalanobis distance matching, genetic matching, and combined computation of

propensity score and Mahalanobis distance matching. Three simulated data sets are also used to demonstrate scenarios where the control and treatment group have distinctly different distribution shapes. In terms of MSE, entropy balancing design provides lower MSEs than any alternative method. Aside from larger sample sizes (1500 observations), entropy balancing performs very well in comparison to alternative matching methods.

In order validate entropy balancing design for empirical data; Hainmueller (2011) compares entropy balancing to the aforementioned list of alternative matching methods. For empirical comparisons Hainmueller (2011) uses the LaLonde (1986) data set. LaLonde's (1986) data constitutes of two separate data sets merged into one. The treatment group is compiled from the first data set while the second data set creates the control group. LaLonde's (1986) data is often used as a test to study and compare various data balancing methods.

The treatment group's data is gathered from the National Supported Work Demonstration (NSW) job training program. The control group of the data set is drawn from the CPS-Social Security Administration file. LaLonde (1986) replaces the NSW's control group with a sample drawn from the CPS file. LaLonde (1986) data are used (Hainmueller, 2011; Diamond & Sekhon, 2012) to study whether it is possible to adjust and or match the control group with the treatment group. The same covariates are used to measure and control for in other observational studies. LaLonde's (1986) data set is considered to be difficult data to balance because the unadjusted difference of control and treatment mean outcomes is very different Also, the covariate between the treatment and control groups is heavily imbalanced because the people chosen for the randomized evaluation have distinctly different covariate values compared to the CPS sample that replaces the original control group (Hainmueller, 2011).

Entropy balancing provides both high and computationally efficient degree of covariate balance. Entropy balancing yields results that are in line with the LaLonde's (1986) experimental target. The treatment effect is also statistically significant (at 95% confidence interval). In terms of MSE, Hainmueller's (2012) entropy balancing also outperforms the genetic matching scheme conducted by Diamond and Sekhon (2012).

Genetic matching is an automated iterative process that checks and improves overall covariate balance. It guarantees an asymptotic convergence to the optimal matched sample. By design, it improves the covariate balance between the control and treatment group (Diamond & Sekhon, 2012). Technically, genetic matching bears many similarities with entropy balancing. Similar to entropy balancing, the genetic matching method's primary focus is to attain a covariate balance between the treatment and control group. Unlike entropy balancing, genetic matching finds uses the nearest neighbor method that is calculated on generalized distance metric.

Genetic matching assigns weights to each covariate that will be reweighed in order to achieve a better covariate balance. Aforementioned covariate weights are calculated by a genetic algorithm that attempts to maximize the covariate balance. Genetic algorithm balances in a broader manner, whereas entropy balancing balance respect to given covariate moments. Because of the different approach to reweighting, compared to genetic matching, entropy balancing weights are less constrained to vary across units (Hainmueller, 2011). Diamond and Sekhon (2012) do not provide a comprehensive outlook on theoretical framework, while Hainmueller (2011) argues that this is due to the profound complexity of the underlining theory of the genetic matching.

Diamond and Sekhon (2012) follow Hainmueller's (2011) approach to test the genetic matching method with both simulated and empirical data to study and compare it against alternative data balancing methods and test whether genetic matching can improve the standard errors for covariates. LaLonde (1986) data is used to study how genetic matching stacks up against alternative methods. NSW job training experiment data set, which only carries 185 treated and 260 control observations, makes it difficult to recreate a control sample from the CPS (Diamond & Sekhon, 2012). The reweighting process of synthetic control group gathered from the CPS becomes even more difficult because the initial NSW experiment sample focuses on former convicts and addicts, high school dropouts, and long-term welfare recipients (LaLonde, 1986).

Diamond and Sekhon (2012) compare the balancing results of genetic matching to logistic regression, Random Forest, and Boosted CART matching. Genetic matching is not compared to

entropy balancing. Their findings conclude that enetic matching is the least bias matching scheme among the selected matching methods. Diamond and Sekhon (2012) conclude that empirical researchers should not attempt a multitude of matching models to balance their data. Unlike Hainmueller (2011), Diamond and Sekhon (2012) foresee that improvements in computational power and machine learning will help to improve covariate matching and covariate reweighing methods.

In conclusion, matching and reweighing observational data methods are still developing to find the most apt balancing of the observational data. Furthermore, there does not appear to be academic consensus on the best method or measure to rank or compare the wide variety of balancing options provided. A measure of this type could ensure the definite comparability of the multitude of alternative reweighting schemes (Diamond & Sekhon, 2012).

7   Results

7.1   Summary Statistics and Data Descriptions

In order to validate the comparability of the adjusted and balanced estimations, a replication of Autor's (2003) summary statistics is presented in Table 1. Replications show identical results (Table 1) to Autor's (2003) summary statistics of the data. Replicated summary statistics confirm that the adjustments made to the data and regression design can be compared to Autor's (2003) findings. Table 1 depicts the number of THS employed and their share of the overall nonfarm employment in the US. All four geographic regions[6] in the US experience both relative and absolute growth of THS employment (Table 1, column 1, 2, 3, 4). In 1979, approximately 432,000 people or 0.6% of the nonfarm-employed worked in THS firms. This number constitutes of both the people who work through a THS firm and the workers who operate and administer the THS firms.

Statistics from the year 1983 show a slight drop on THS employment in the US, which is likely to be in line with the sharp decline in labor demand as a result of the early economic downturn of 1980s (Moy, 1985). After the economic downturn of 1983, Table 1 shows that there is a steady growth trend (Column 5) on both relative and absolute number of workers employed by THS firms. Southern and Western states (Table 1, columns 3, 4) are the most THS labor intensive areas in the US. Southern region of the US the share of THS employment in 1995 is 2.87% of the nonfarm employment (Table 1, column 4). In 1995, the share of THS employment in the US as a whole is 2.39% (Table 1, column 5). Autor (2003) also shows the statistics for THS employment

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[6] The U.S. Census Bureau four regions:

Northeast: Maine, New Hampshire, Vermont, Massachusetts, Rhode Island, Connecticut, New York, New Jersey, Pennsylvania

Midwest: Ohio, Indiana, Illinois, Michigan, Wisconsin, Minnesota, Iowa, Missouri, North Dakota, South Dakota, Nebraska, Kansas

South: Delaware, Maryland, Virginia, West Virginia, North Carolina, South Carolina, Georgia, Florida, Kentucky, Tennessee, Alabama, Mississippi, Arkansas, Louisiana, Oklahoma, Texas

West: Montana, Idaho, Wyoming, Colorado, New Mexico, Arizona, Utah, Nevada, Washington, Oregon, California, Alaska, Hawaii

in 2000 to show that both the absolute THS employment and its share of nonfarm employment rose to 2.95% (Table 1, column 5).

Table 2 and Table 3 summarize the quadrennial labor force demographic covariates to the four statistical regions according to the U.S. Census Bureau (n.d.) four region division. Autor's (2003) labor force demographics control for education level, race, age, gender, and marital status. Labor force controls are aggregated dummy variable controls that they indicate the state-level percentage of a certain characteristic. In the Northeast (Table 2.1, columns 1,2,3) the share of high school graduates in the labor force decrease throughout the study, while the share of college enrolled attendees and college graduates increases. High inter-regional correlation in the education level trends of the labor force is apparent throughout the US (Table 2, columns 1, 2, 3; Table 3, columns 1, 2, 3).

Regional racial demographics of the labor force are distinctly different between the southern states, compared to the other three regions. In 1979, 16 % of the labor force in the southern region is black, whereas the blacks' share of the labor force in other regions range from 3% to 6% (Table 2, column 4; Table 3, column 4). In the Western region, other non-whites constitute 10% of the labor force, while on other regions their share of the labor force is roughly 2% (Table 2, column 5; Table 3, column 5). Age groups' 16 to 24 and 55 and older participation in the labor force decreases over the course of the study (Table 2, columns 6, 7; Table 3, columns 6, 7).

Changes in marital status of the labor force in different regions are relatively small and the small differences converge after 1995 (Table 2, columns 9, 10). Share of married females in the labor force increase over time. The overall share of the married and female in the labor force steadily decline in all regions. This implied that the number of married men in the labor force declines more than the number of married women increases. Over the course of the timespan of the study, the regional marriage levels in the labor force converge to around the 60 percent mark (Table 2, column 10; Table 3, column 10).

This thesis studies whether there are confounding differences between the states that adopt the implied contract and the states that do not[7]. If the 9 never-taker states[8] that do not implement the implied contract exception have different characteristics from the states that do, never-taker states may not represent the counterfactual result. For example, labor force characteristics could determine the overall usage of THS employment. Labor force demographics such education level, age, race, and gender have all been shown to be different between the THS and non-THS jobs (Cohany, 1998). The problem arises with Autor's (2003) estimations of whether the selection to control and treatment states is not randomized. Cohany (1998) shows that the adoption of at-will doctrine is dependent on labor force characteristics. One does not know how the never-taker states would have reacted to the implied contract. Similarly one does not know how the implied contract states would have fared had they not adopted the implied contract.

One way to study the potential differences between the never-taker states and the implied contract states is to look at the covariate-by-covariate demographic means. Figure 1 displays the covariate-by-covariate standardized differences of the never-takers to implied contract states in the Autor (2003) data without adjustments or balancing. Figure 1 bars graph displays estimates for the standardized differences of the means of the controlled labor force characteristics in the Autor (2003) data.

Bars in Figure 1 skewed to the left of the zero point indicate a covariate bias towards the never-taker states. Conversely, covariate bias towards the implied contract states is skewed to the right. Differences in the education level of the implied contract and never-takers are quite significant. Never-taker states' labor force's high school graduate ratio is higher than that of the implied contract states' (Figure 1). Covariates that measure the level of higher education show a bias towards the implied contract states (Figure 1). The standardized mean of the some college - variable in the implied contract states is over half a standard deviation higher than it is in the never-taker states. Implied contract states' labor force is more educated that is shown by the statistics. Share of college graduates (undergraduate degree and higher) in the labor force in the

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[7] During the timespan (1979-1995) of the study

[8] Rhode Island, Pennsylvania, Montana, Delaware, North Carolina, Georgia, Florida, Mississippi, Louisiana

implied contract states than it is in states that do not adopt the implied contract. There is over half a standard deviation of difference in the share of the labor force that received some college education but did not graduate with a degree (Figure 1) between the never-taker states and the implied contract states.

Labor force demographics of the never-taker states and rest of the states are profoundly different. In never-taker states, there is over standard deviation bias toward blacks, whereas implied contract states have a noticeable bias toward other races. There is no bias in the age bracket 16 to 24, but the share of workers aged 55 and older is slightly skewed towards the never-taker states. Females in the labor force are more represented in the never-takers, whereas the share of married females in the labor force is higher in the implied contract group.

Aside from the log of employment and the log of THS employment for the Figure 1, other independent variables in Figure 1 are a measure of state-level aggregate that show the prevalence of each demographic measure in the state. Due to the nature of the panel data, this data cannot be fundamentally divided into control and treatment group because the implied contract is implemented on a state-level and there is not a single shock year when all the states implement the exception. Implied contract states are represented in the control group for the years prior to the adaptation of the contract. In order to find how labor force characteristics differ between the states that do not adopt implied contract (never-taker state) and states that will adopt the implied contract at some point of the study (implied contract states) we graph (Figure 1) the differences by standard deviation. Figure 1 shows the covariate-by-covariate differences in the labor force characteristics between the never-taker states and the implied contract states is distinct.

A few states, such as California, already enforced some level of implied contract before the timespan of the study. In fact, there are seven states that enforced the implied contract in 1979 (Figure 2). Forty-one states adopt the implied contract over the course of the study. Remaining 34 states adopt the implied contract exception between 1980 and 1990. The most noticeable treatment shocks occur in 1983, 1985 and 1987. Autor (2003) addresses the issue of multiple pre- and post-periods, but does not find a strong reason to believe that one states adoption of implied

contract would impact other states' employment and THS employment levels. Therefore, it is impossible to see counterfactual results for both the early-adopter states and the never-taker states unless one can assume that implied contract enforcement is randomly assigned and that one states implied contract adoption does not affect. Given the macroeconomic setting and demographic differences in state-level labor force demographics it is difficult to argue in favor of random assignment of implied contract by state.

7.2   Replicating and Entropy Balancing Autor's (2003) Estimations

In order to authenticate estimations presented in the thesis, a replication of Autor's (2003) Table 5 (columns 1, 2, 3, 4, 5, 6) is presented (Table 4). Estimations of the Table 4 show identical results to Autor's (2003) estimations. Analogous to Autor's (2003) estimations, all the replicated regressions of this thesis control for yearly variation and state-level differences on THS employment by implementing a set of dummy-variables. State-level controls introduce 50 state-specific dummy-variables that control for initial differences on THS employment level. Yearly variation variables controls for the yearly changes on the overall THS employment level in the US.

Autor (2003) estimates a standard difference-in-differences model for   the   log   of   THS employment

$$\ln(THS_{jt}) = \beta_0 + \beta_1 (Common\ Law\ Exceptions_{jt}) + \beta_2 \ln(Nonfarm\ Employment)$$
$$+ \beta_3 (Labor\ Force\ Demographics) + \beta_4 (State\ and\ Regional\ Interactions)$$
$$+ u_j + y_t + e_{jt}$$

where the dependent variable is logarithm of temporary help employment in a state (j) and year (t). Vector of state dummies, u_j, and year dummies, y_t are also presented. This thesis' estimations control for linear state-by-time trends and region-by-year differences.

Columns 1, 2, 3, 4, and 6 of Table 4 also control for state-by-time trends. The state-by-time controls are interaction variables that allow every state to have their own respected THS employment curve. Table 4 studies whether the inclusion of labor force demographic controls affects the statistical significance of the implied contract exception's impact on THS employment. The dependent variable for upcoming estimations is the natural logarithm value of THS employed people in the US. Log of THS nonfarm employment variable is the natural logarithm of the absolute value of employed people in the US who do not work in farming. Jobs in agriculture are often omitted from the statistics because there are In addition to the covariates shown in the Table 4, other two exceptions (good faith and public policy) are also controlled for.

Both exceptions are omitted from the tables because they do not show any impact or significance on THS employment. Logarithm of nonfarm employment appears to have positive impact on THS employment. Depending on the controls used in the model an increase of percentage point in the number of employed would tend increase the number of THS employed by 1 to 2 percent.

Autor (2003) introduces state-by-time interaction controls to the model that are replicated in (Table 4, columns 1, 2, 3, 4, 6). State-by-time[9] covariate allows each state to have a distinct curve for THS employment level. In order to control for non-linear state-level THS employment growth, Autor (2003) uses quadratic state-by-time controls (Table 4, columns 2, 4). Quadratic state-by-time covariate is an interaction variable between state-level control variable and time squared. Overall, region-by-year[10] controls appear to improve the statistical significance of the implied contract's positive effect on THS employment.

Without labor force demographic controls, the implied contract variable remains statistically significant with different variations of state and time related controls (Table 4, columns 1, 2, 3, 4). When the labor force demographic covariates are controlled for (Table 4, column 5), the implied contract becomes statistically insignificant (at 95% confidence interval). As the state-by-time controls are implemented to the model (Table 4, column 6), implied contract variable regains its statistical significance. Demographic covariates appear have a strong impact on the THS employment. If the demographic covariates are omitted from the model, the risk of omitted variable bias could increase and therefore decrease credibility of the results. The higher the level of high school graduates and college attendees in a state, the higher the level of THS employment. States where there are more blacks in the labor force have significantly less THS employment (Table 4, column 5).

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[9] Time by Autor's (2003) definition is a count number that starts at one in the year 1979

[10] Nine regions used in region-by-year variable are the nine divisions of the four major regions. According to the US Census Bureau (n.d.) these divisions i.e. regions are: Pacific, Mountain, West North Central, West South Central, East North Central, East South Central, New England, Middle Atlantic, South Atlantic

Age and gender controls are not statistically significant factors of THS employment (Table 4, columns 5, 6). When state-by-time differences are controlled for, the impact of the two education variables becomes statistically insignificant (Table 4, column 6). State-by-time interaction variables affect the p-value of the high school graduate-covariate that spikes from .013 to .950. Similarly, some college-variable loses its statistical significance when state-by-time interactions are controlled for (Table 4, column 6). Conversely, state-by-time interaction controls improve implied contract exception's (Table 4, columns 5, 6) p-value from .089 to .013 making the treatment effect statistically significant at 95% confidence level.

Table 4 shows that, without state-by-time controls, inclusion of labor force controls decreases the statistical significance of the implied contract and falls below the desired confidence level of 95%. This could be given the fact that there are demographic differences between the state labor forces that adopt the implied contract and states that do not adopt the rule. Table 5 is a replication of the Table 4, where the independent variables are entropy balanced and weighted into the regressions. The balancing covariate, i.e. the binary treatment, is the implied contract exception covariate. Demographic covariates from all the years in which a state did not have an implied contract are balanced to match the demographics of the implied contract observations.

Entropy balancing adds weights to the demographic covariate values in order to correct the imbalance between the control and treatment group (Figure 3). Entropy balancing weight specifications can be adjusted by the researcher. This thesis studies what happens to the treatment effect if the control observations had identical labor force characteristics to that of the treatment observations. Table 5 shows that the p-value and the standard error of the implied contract and other covariates decrease throughout regressions used in Autor's (2003) initial regressions. This implies that there are differences between the treatment and control groups that affect the statistical significance of the initial regressions. Entropy balancing the original Autor (2003) data suggests (ceteris paribus) that both the magnitude and statistical significance changes (Table 4; Table 5). Some regressions experience a higher confidence level (Table 4, columns 1, 6; Table 5, columns 1, 6), whereas the treatment effect drops for one of Autor's models (Table 4, column 2; Table 5, column 2).

This would allow us to conclude that the disparities in labor force demographics between the treatment and control observations that are likely to explain partially the level of state-level THS employment. Entropy balancing also affects the magnitude of the implied contract treatment on the number of THS employed. Without the entropy balancing of data (Table 4), the adoption of the implied contract will increase the number of THS employed by 14 centinepers[11], or approximately 15%.

This thesis studies whether Autor's (2003) 50 state data should be downsized with respect to the data available for evaluations. One option is to drop early-adopter states from the evaluations altogether. For states that enforce the implied contract in 1979, there are no baseline data that from which one can derive the pre-treatment employment levels. Also, some states enforced implied contract exception significantly earlier than 1979. In 1959, California began to enforce common law and statutory exceptions to the employment at will. States that have had years or even two decades to adjust to the implied contract do not fit in the desired RCT framework this thesis attempts to set up. Also, even if the other six early-adopter states enforce implied contract in 1979, there is no pre-shock data available. Because of the insufficient data for the seven states, Table 6 and Table 7 omit the seven early-adopter states[12] of the implied contract are from the regressions. The introduction of the 43 state sample allows the isolation of the implied contract's impact on the employment level for those states that actually adopted the implied contract between 1979 and 1995.

7.3   Subsample Estimations with the Original and Entropy Balanced Data

Table 6 replicates Table 4's regressions without the first seven states that adopt the implied contract without using entropy balancing. One potential issue with Autor's (2003) data is that it includes seven states that enforced the implied contract exception in 1979 (Figure 2). This does

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[11] Henceforth referred as log points

[12] Maine, Illinois, Oklahoma, Idaho, Washington, California and Oregon

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not fare well with the DID method and can potentially dilute the estimations. Early-adopter states have had time to adapt their labor markets to comply with the changes in EPL associated to implied contract exception. Potentially, the change in THS employment had already occurred for the seven states. Also, given the data one does not know early-adopter states' pre-shock employment and THS employment levels. Therefore, the seven early-adopter states are unlikely to provide desirable information on the shock's (implied contract) impact on THS employment.

All else held equal, using the smaller 43 state data, the treatment variable's statistical significance improves noticeably (Table 4, columns 2, 4, 5; Table 6, columns 2, 4, 5). The treatment effect's statistical significance remains the same for the region by year controlled regression (Table 4, column 3; Table 6, column 3). As expected, the magnitude of the treatment effect also increases by few log points (Table 4; Table 6). There is a slight drop in the implied contract p-values for two regressions that does not affect the initial confidence level (Table 6, columns 1, 6; Table 4, column 1, 6). Autor's (2003) most basic regression design that controls for labor force demographics shows a slightly greater implied contract's impact on THS employment.

To further study the characteristics of state-level labor force characteristics, a box-plot graph shows the pre-balanced standardized covariate-by-covariate differences for both 50 state and 43 state data (Figure 3.1 & Figure 3.2). Because of the nature of panel data and the implied contract exception that throughout the 1980s (Figure 2), implied contract states are incorporated into the control group labor force demographics. Figure 3.1 and Figure 3.2 show that the noticeable disparity on covariate means between the treatment and control groups do not change when the early-adopter states are omitted from the estimations. This could imply that the disparity remains in the smaller sample of states because of underlining differences between the never-taker and implied contract states. Covariate means that are greater in non-implied contract observations are skewed to the left and covariate means that greater in implied contract observations are shown as boxes to the right. Aside from log of employment and other non-white worker covariates, all the other covariates are biased towards either group (Figure 3.1; Figure 3.2).

Table 7 demonstrates how the estimations change when the data are rebalanced with the entropy balancing method. Table 7 replicates Table 6's estimations that allow us to compare entropy balanced and non-balanced estimations for the 43 state data. The regression design used in Table 7 is identical to that of the Table 4's design. Moreover, Table 7 shows that the entropy balancing of the demographic covariates does not universally enhance the statistical significance of the implied contract (Table 7; Table 6). When the model controls for state-level and yearly fixed effects, state-by-time trends, and quadratic state-by-time trends, the implied contract becomes statistically insignificant at 95% confidence interval (Table 7, column 2; Table 6, column 2). Similarly, column 4 of Table 7 incorporates the region-by-year controls to the aforementioned column 2's model is not statistically significant (at 95% confidence level). The remaining four regressions (Table 7, columns 1, 3, 5, 6) show a statistically significant treatment effect for the implied contract.

The original 50 state and 43 state data estimations show that, on average, the implied contract exception increases the THS employment by approximately 14 log points. All else held equal, entropy balancing the data (both 50 and 43 state) increase the implied contract's impact on THS employment to 37 log points. Entropy balancing the data increases the magnitude of the implied contract's impact on THS employment by an average of 21 log points. In effect, adoption of the implied contract increases the state-level THS employment by 20 log points. An increase of one log point is approximately an increase of one percent. Thus, entropy balancing of the independent variables appears to increase the impact of the treatment effect by 20% higher. Given the entropy balanced data, a state that adopts the implied contract will experience a 62% higher level of THS employment than that of a non-implied contract state. Using the original Autor (2003) data implied contract will increase the number of THS workers approximately 14%, whereas, with the entropy balanced data, the increase could be as high as 37%.

However, findings of the implied contract's impact on THS employment become less robust when the early-adopter states are omitted from the estimations. Autor (2003) acknowledges that the THS employment growth remains persistent even after there are no new implied contract states. After 1990, there are no implied contract adoptions between 1991 and 1995. Also, when

Autor's (2003) controls for labor force demographics, the statistical significance of the implied contract drops under the 95% confidence level when state-by-time are not controlled for. Hence, Autor (2003) omits the labor force demographics from all but one of the tables on the paper. Not controlling for labor force characteristics leaves out an integral factor in play that could impact the state-level THS employment decision.

Also, R-squared values between .976 and .991 (Table 4) are likely to have too complicated regression design given the number of observations and data. When the R-squared approaches 1, the model covers for the entire variance of the sample which is described as an overfitted model (Leinweber, 2007). Leinweber's (2007) research shows that availability of data can introduce the practice of apophenia, where one can find meaningless correlation between two things. The high R-squared value is a result of consistent use of fixed effects controls that control for state-level and year fluctuation on THS employment.

8   Conclusions

The contractual nature of employment has changed considerably in the past hundred years. In the mid-1800s, employment at-will doctrine shaped and defined the nature of an employment contract in the US. Given that the at-will doctrine is comparatively unique because at the time of its implementation, European countries did not acknowledge the nature of the employment contract. At-will doctrine built upon the premises of flexible labor markets where the employee and employer could terminate the contract without other party's consent.

Over the course of the 1900s, public support for the at-will doctrine diminished in the US (Lawrence, 1967). The expected nature of employment shifted towards more protected work environment where full-time employees expect a certain level of job security. Gradually, states such as California and Michigan began to enforce exceptions to the at-will doctrine before 1979 that is the beginning of Autor (2003) data. Early adopter states are likely to dilute the robustness of Autor's (2003) estimations. The three known exceptions were intended to protect employees from firm initiated unjust actions that could harm the employee. The three distinct exceptions to employment at-will became more popular during the 1980s. From 1980 to 1990, 34 states adopted the implied contract exception. This could allow one to study whether changes such as implied contract exception, in the EPL will cause higher levels of THS employment.

Some scholars suggest that exceptions that limit the statute of the original employment at will doctrine changed the entire paradigm of once flexible US labor markets (Olson, 1997). While the exceptions to employment at will are intended to protect workers from maleficent employer practices, they may in fact cause adverse impact on employment. Empirically, more stringent EPL increases job security and thus decrease the turnover ratio of the workers. Stringent EPL is likely to decrease firm's interest to hire new workers using open-ended contracts because of added restraints to employer's dismissal practices. Under stringent EPL, firms are likely to find new ways to contract workers using short term or temporary employment contracts. Autor (2003) shows that the THS industry's share of overall employment steadily grew consistently throughout

the study. Unless THS industry has created an entirety of new jobs to the economy, it is likely that THS jobs have replaced traditional open-ended jobs.

Autor (2003) sets out to study whether exceptions to the at-will doctrine increase THS employment levels relative to normal open-ended employment. Jobs that require little investments in the firm-specific human-capital can be hired through a THS firm because the number of THS workers can be adjusted more flexibly. Autor (2003) hypothesizes that the growing usage of THS firms is due to increased liability in the matter of wrongful discharge. Only the implied contract exception is considered to be a partial cause to the increase in THS employment. Autor (2003) shows that the introduction of public policy and good faith exceptions to the at-will doctrine is not likely because these employer must follow these practices with their full-time and THS workers. Consequently, Autor (2003) uses the difference-in-differences (with fixed effects) method to study whether the implied contract exception to employment at-will doctrine affects the THS employment.

Autor (2003) suggests that the implied contract states are likely to experience higher levels of THS employment. During the timespan of the study THS, employment in the US grew consistently. Autor (2003) finds that the THS employment grew rapidly even after the year 1992, a year after which there had been no new implied contract exceptions initiated on a state-level. Autor (2003) acknowledges that the growth of THS employment remains constant even after the there are no new implied contract state. As the Figure 2 shows the last implied contract adoption occurred in 1990. Nevertheless, between 1991 and 1995, the share of THS employment of the overall employment increased by 79% (Figure 1). In absolute terms, over a million new THS jobs were created between the years 1991 and 1995.

The regression design Autor (2003) implements makes use of a large number of control dummies to control for yearly, state-level and regional trends and non-linear variation in the data this allows the researcher to control for intrinsic differences among states and regions. State-level differences in THS employment could depend largely on many factors, such as state-specific and regional specialization into certain industries, political environment and labor force

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demographics should be controlled for. Some of the regressions also incorporate state-by-time interaction and quadratic control dummy variables. Because of the limited number of observation, one could add too many variables to a regression. This is bound increase the risk of overfitted regression and increase the risk of apophenia (Leinweber, 2007). While Autor's (2003) regression design intends to control for state-level and yearly differences, it also shows signs of overfitted model.

This thesis studies whether the implied contract and non-implied contract state labor forces are comparable and whether entropy balancing the data improves the statistical significance of Autor's (2003) model. Entropy balancing is data balancing method that sets weights to the independent covariate values in order to correct imbalance between the treatment and control groups. As Autor (2003) omits the labor force characteristics from all but one of his ten models, this thesis studies how the inclusion of labor force controls affects the regressions. Also, the comparative evaluation between the 50 and 43 state data is performed to study whether a smaller sample would improve the robustness of the model. A smaller 43 state can improve the purity of the estimations by omitting states for which there are no pre-implied contract data. We find that covariate-by-covariate standardized means of labor force demographics show that the control and treatment observations are distinctly different from one another.

By entropy balancing Autor's (2003) 50 state data, one can study whether the balancing of the data will correct the imbalance between the control and treatment groups. If the labor force demographics are controlled for, the demographics of both the treatment and control states should be relatively comparable. If one is able to dispute control group's function as a representative control group for the treatment states the validity of the study could be questioned. At the same time, it is impossible to create a clinical trial like setting in which the treatment and control group are identical, even if one can conclude that the implied contract states are different from the never-taker states. This thesis finds that the entropy balancing method decreases the most of the standard errors of the implied contract and independent labor force demographic covariates.

This thesis also studies whether the smaller 43 state data could provide more randomized controlled trial-like framework that can be used for more robust estimations. One issue with the data is the impurity of the point in time when the implied contract shock occurs. According to Autor's (2003) data, there are no pre-implied contract data on nonfarm and THS employment for the seven states. The difference-in-differences evaluation method is based upon the premises that there are data on the pre- and post-shock levels for both the control and treatment observations. Without the pre-treatment statistics on employment and THS employment, the early-adopter observations do not fulfil the DID method's requirements.

Given the available data, the inclusion of early-adopter states can potentially reduce the robustness of the evaluation. California, which is one of the seven early-adopter states, enforced first staged of the implied contract as early as 1959. This means that the labor markets in California had 20 years to adjust to the implied contract exception. Comparatively to the 50 state data, the treatment effect in the 43 state data is greater.

Eleven-percent annual growth rate of THS employment remains consistent even after THS adoption plateaus in the 1990. This thesis studies whether there is a causal relationship between the exceptions to employment at will and the growth of THS industry. As this thesis shows, there is substantial evidence that employers were aware of the changing legal environment. This, in theory, attracts employers to find ways to contract around more stringent EPL. This thesis shows that using the smaller 43 data and entropy balancing the data the independent variables THS employment appears to become more dependent on the selection of different control variables.

More research should be conducted on different data balancing methods and whether one balancing method is more statistically accurate than others. Also, this thesis finds that the whole 50 state data are likely to be statistically less significant as opposed to data that shares more things in common with a RCT. Additional research on EPL's impact on employment and THS employment should be conducted in settings where it is possible to disentangle the shock from other factors in play.

9   Tables and Figures

Table 1: Summary Statistics Replication of the THS employment (1,000s of workers) and the THS's Share of Nonfarm Employment (%) in the US (Autor, 2003)

	(1) Northeast (9 States) (%)	(2) Midwest (12 States) (%)	(3) South (16 States) (%)	(4) West (13 States) (%)	(5) Total (50 States) (%)
1979	114.45	104.43	104.92	109.11	432.93
	0.66	0.51	0.46	0.78	0.58
1983	111.11	75.83	112.02	97.08	396.04
	0.65	0.42	0.48	0.69	0.55
1987	198.89	188.53	234.23	172.43	794.09
	1.00	0.90	0.86	1.01	0.93
1991	203.49	280.29	480.75	260.62	1,225.16
	1.02	1.22	1.61	1.36	1.33
1995	352.27	570.69	970.09	495.53	2,388.87
	1.73	2.12	2.87	2.42	2.39
2000[13]					3,887.0
					2.95

[13] Year 2000's statistics are taken from Autor's (2003) paper because the year 2000 data is not part of the data set provided for public use.

Table 2: Summary Statistics on the Labor Force Demographics by US Census Bureau's Region Division

Table 2.1: Northeast (9 States)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	High School Grad. (%)	Some College (%)	College Degree + (%)	Black (%)	Other (%)	Ages 16-24 (%)	Age > 54 (%)	Female (%)	Married Female (%)	Married (%)
1979	38,31	18,71	19,28	5,07	0,90	23,47	15,27	42,80	24,99	64,61
1983	38,75	20,56	22,02	4,76	1,13	21,80	14,43	43,75	24,92	61,77
1987	37,95	21,45	24,29	5,09	1,37	18,98	13,95	45,56	26,08	61,39
1991	36,97	22,73	27,27	5,38	2,04	15,54	13,02	46,13	25,74	59,29
1995	35,24	25,18	28,97	5,84	2,49	14,95	12,85	47,05	26,19	59,10

Table 2.1: Midwest (12 States)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	High School Grad. (%)	Some College (%)	College Degree + (%)	Black (%)	Other (%)	Ages 16-24 (%)	Age > 54 (%)	Female (%)	Married Female (%)	Married (%)
1979	39,64	21,03	16,27	5,08	1,32	26,13	15,34	41,96	25,12	65,61
1983	40,85	22,63	18,68	5,03	1,30	22,75	14,16	43,49	26,66	65,23
1987	40,76	24,38	19,55	5,32	1,64	19,88	13,49	45,08	27,64	64,63
1991	39,15	25,90	21,74	5,38	2,02	17,61	12,88	46,06	27,11	61,60
1995	35,60	30,68	22,83	5,56	2,05	17,80	12,73	46,95	27,38	60,36

Table 3: Summary Statistics on the Labor Force Demographics by US Census Bureau's Region Division

Table 3.1: South (16 States)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	High School Grad. (%)	Some College (%)	College Degree + (%)	Black (%)	Other (%)	Ages 16-24 (%)	Age > 54 (%)	Female (%)	Married Female (%)	Married (%)
1979	35,71	19,33	15,90	16,25	0,96	23,67	14,20	42,10	26,67	68,92
1983	37,36	20,32	17,55	17,34	1,16	21,88	13,17	43,67	26,94	66,41
1987	38,10	21,77	18,75	18,14	1,47	19,41	12,17	44,87	27,63	65,25
1991	37,34	23,81	20,35	18,30	1,63	16,71	12,05	45,79	26,20	61,62
1995	35,96	27,75	21,85	18,64	2,03	16,48	11,73	46,67	26,60	60,71

Table 3.2: West (13 States)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	High School Grad. (%)	Some College (%)	College Degree + (%)	Black (%)	Other (%)	Ages 16-24 (%)	Age > 54 (%)	Female (%)	Married Female (%)	Married (%)
1979	34,74	26,02	19,27	2,07	9,98	24,77	12,59	41,38	24,97	65,32
1983	35,15	27,79	21,18	2,43	10,03	21,53	11,49	43,00	25,88	63,59
1987	34,57	28,53	22,04	2,59	10,00	18,29	11,74	44,67	27,35	64,09
1991	33,53	29,50	23,97	2,60	9,54	16,13	11,59	44,96	26,19	60,72
1995	31,93	33,10	23,93	2,70	9,48	16,72	11,07	45,51	26,82	60,98

Figure 1: Covariate-by-Covariate Balance in Standardized Difference in Means of the Implied Contract States and Never-taker States

Figure 1: Standardized differences in means of the unadjusted data
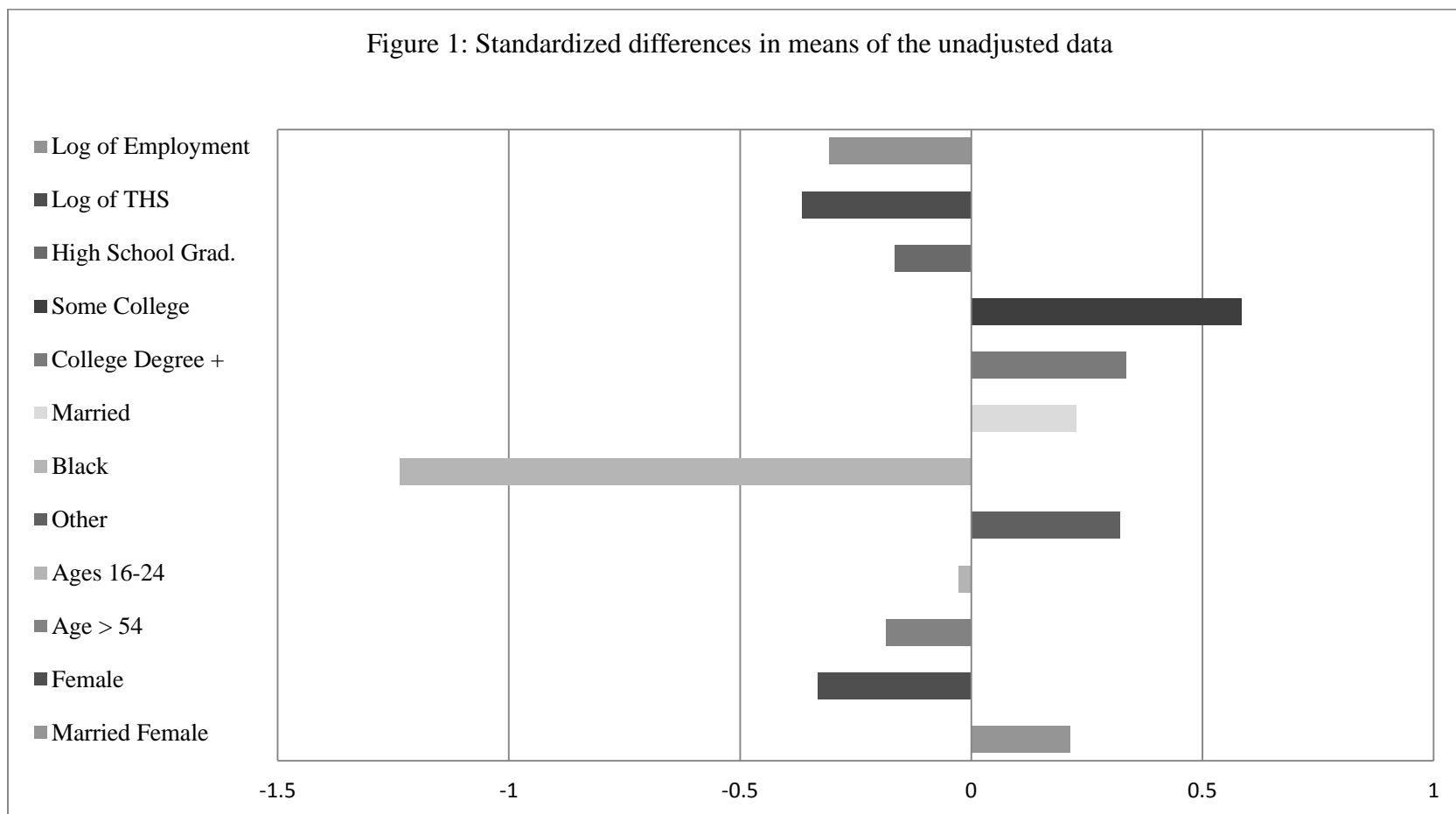
Figure 2: Histogram of the States That Implemented the Implied Contract to the Employment at Will

Yearly Frequency of the Implied Contract Adoption in the US on a State-level
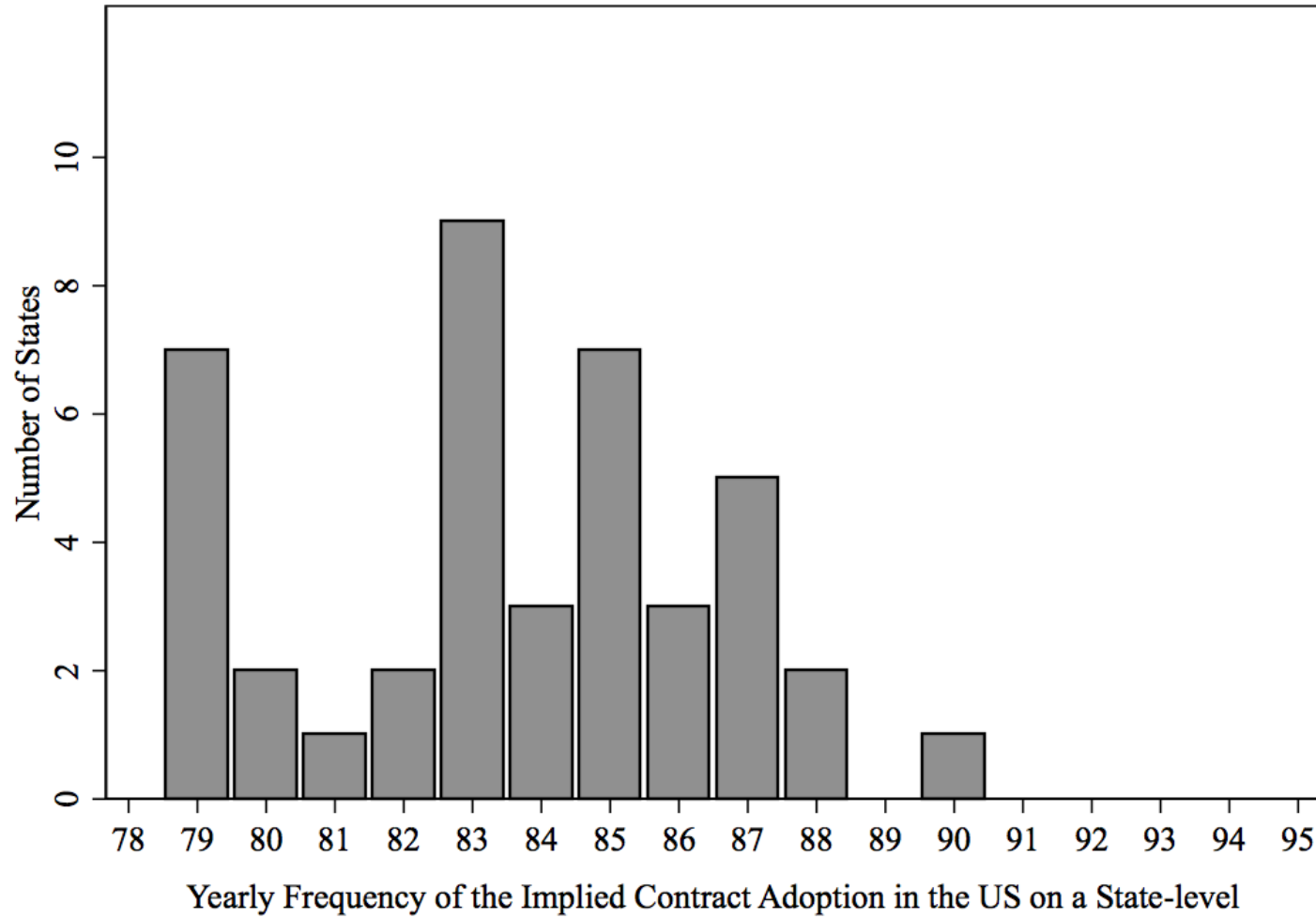
Figure 3: Standardized Difference of Covariate Means Between the Control and the Treatment States
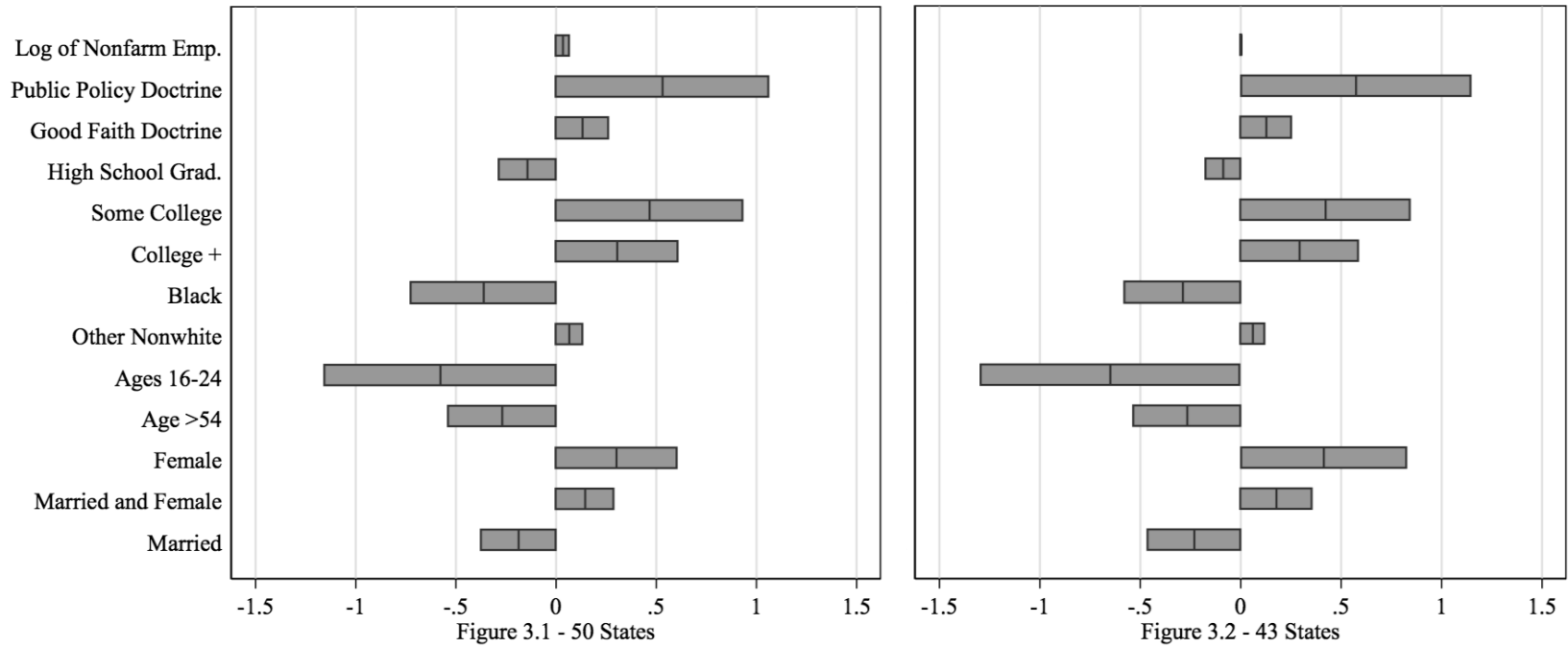

Figure 3.1 - 50 States

Figure 3.2 - 43 States

Table 4: The Implied Contract's Impact on a State Level with Labor Force Demographics

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Implied Contract	0.148**	0.132**	0.174***	0.141**	0.134*	0.145**
Exception	(0.012)	(0.041)	(0.003)	(0.043)	(0.089)	(0.013)
Log of Nonfarm	1.552***	1.588**	1.441**	1.655*	2.008***	1.668***
Employment	(0.001)	(0.017)	(0.016)	(0.076)	(0.000)	(0.000)
High School Grad.					5.595**	0.078
					(0.013)	(0.950)
Some College					6.385***	0.943
					(0.008)	(0.516)
College +					0.044	-1.455
					(0.981)	(0.358)
Black					-3.192**	-2.008*
					(0.026)	(0.098)
Other Nonwhite					-0.524	-0.141
					(0.888)	(0.936)
Ages 16-24					1.859	-0.885
					(0.304)	(0.409)
Age >54					0.657	0.695
					(0.777)	(0.597)
Female					3.093	2.011
					(0.143)	(0.144)
Married and Female					-2.440	-3.307
					(0.690)	(0.217)
Married					1.331	1.572
					(0.692)	(0.387)
State-level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year Controls	Yes	Yes	Yes	Yes	Yes	Yes
State-by-Time Trends	Yes	Yes	Yes	Yes	No	Yes
Quadratic State-by-Time Trends	No	Yes	No	Yes	No	No
Region-by-Year Dummies	No	No	Yes	Yes	No	No
Observations	850	850	850	850	850	850
R-squared	0.989	0.990	0.991	0.993	0.976	0.989

All standard errors are adjusted for clustering at the state level. Robust p-values in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 5: Entropy Balanced replication of the Table 4

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Implied Contract	0.401**	0.363*	0.378***	0.400**	0.365***	0.317**
Exception	(0.017)	(0.089)	(0.006)	(0.048)	(0.007)	(0.025)
Log of Nonfarm	2.274***	1.983***	2.254***	1.863**	2.359***	2.486***
Employment	(0.000)	(0.000)	(0.000)	(0.031)	(0.000)	(0.000)
High School Graduate					1.836	1.012
					(0.339)	(0.589)
Some College					3.826*	0.548
					(0.059)	(0.815)
College +					-2.740	-4.258*
					(0.267)	(0.073)
Black					-4.225*	-3.135*
					(0.068)	(0.089)
Other Nonwhite					1.929	0.850
					(0.483)	(0.580)
Ages 16-24					-0.713	-2.882
					(0.773)	(0.155)
Age >54					-1.655	-2.395
					(0.343)	(0.185)
Female					-0.703	-0.902
					(0.737)	(0.623)
Married and Female					6.806	0.838
					(0.373)	(0.844)
Married					-2.348	0.790
					(0.566)	(0.725)
State-level Control	Yes	Yes	Yes	Yes	Yes	Yes
Year Controls	Yes	Yes	Yes	Yes	Yes	Yes
State-by-Time Trends	Yes	Yes	Yes	Yes	No	Yes
Quadratic State-by-Time Trends	No	Yes	No	Yes	No	No
Region-by-Year Dummies	No	No	Yes	Yes	No	No
Observations	850	850	850	850	850	850
R-squared	0.996	0.997	0.997	0.998	0.993	0.996

All standard errors are adjusted for clustering at the state level. Robust p-values in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 6: Implied Contract's Impact on a State-level without the Early-adopter States (43 state)

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Implied Contract Exception	0.137**	0.136**	0.178***	0.162**	0.161**	0.134**
	(0.020)	(0.035)	(0.003)	(0.024)	(0.038)	(0.023)
Log of Nonfarm Employment	1.327**	1.405*	1.040	1.478	1.804***	1.394**
	(0.015)	(0.096)	(0.149)	(0.207)	(0.001)	(0.011)
High School Graduate					5.697**	-0.524
					(0.023)	(0.674)
Some College					5.697**	-0.524
					(0.023)	(0.674)
					(0.022)	(0.516)
College +					6.516**	0.781
					(0.017)	(0.602)
Black					-0.431	-2.284
					(0.839)	(0.187)
Other Nonwhite					-3.219**	-2.297*
					(0.034)	(0.076)
Ages 16-24					0.255	0.328
					(0.942)	(0.868)
Age >54					1.459	-1.166
					(0.449)	(0.341)
Female					0.199	0.538
					(0.938)	(0.706)
Married and Female					-2.955	-2.018
					(0.673)	(0.456)
Married					1.263	0.867
					(0.744)	(0.663)
State-level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year Controls	Yes	Yes	Yes	Yes	Yes	Yes
State-by-Time Trends	Yes	Yes	Yes	Yes	No	Yes
Quadratic State-by-Time Trends	No	Yes	No	Yes	No	No
Region-by-Year Dummies	No	No	Yes	Yes	No	No
Constant	-11.766*	-13.437	-9.097	-14.669	-24.936***	-12.978**
	(0.060)	(0.164)	(0.283)	(0.290)	(0.004)	(0.030)
Number of States	43	43	43	43	43	43
Observations	731	731	731	731	731	731
R-squared	0.988	0.990	0.991	0.992	0.974	0.988

All standard errors are adjusted for clustering at the state level. Robust p-values in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 7: Entropy Balanced Estimations of Table 6 using the 43 State Data

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Implied Contract	0.375**	0.331	0.370**	0.382*	0.387***	0.298**
Exception	(0.014)	(0.115)	(0.015)	(0.058)	(0.008)	(0.016)
Log of Nonfarm	0.761	1.623**	1.196	0.043	2.149***	1.013
Employment	(0.273)	(0.039)	(0.142)	(0.969)	(0.001)	(0.141)
High School Graduate					1.211	-0.532
					(0.546)	(0.785)
Some College					2.888	-0.423
					(0.155)	(0.851)
College +					-3.328	-5.610**
					(0.178)	(0.018)
Black					-4.964**	-4.034**
					(0.032)	(0.030)
Other Nonwhite					3.403	1.280
					(0.188)	(0.513)
Ages 16-24					-0.899	-3.537*
					(0.725)	(0.098)
Age >54					-1.213	-2.438
					(0.554)	(0.208)
Female					0.054	-1.640
					(0.978)	(0.357)
Married and Female					5.648	0.414
					(0.499)	(0.920)
Married					-2.129	0.531
					(0.641)	(0.819)
State-level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year Controls	Yes	Yes	Yes	Yes	Yes	Yes
State-by-Time Trends	Yes	Yes	Yes	Yes	No	Yes
Quadratic State-by-Time Trends	No	Yes	No	Yes	No	No
Region-by-Year Dummies	No	No	Yes	Yes	No	No
Constant	-19.894***	-16.709	-20.221	-9.371	-20.314***	-14.518*
	(0.006)	(0.136)	(0.115)	(0.632)	(0.001)	(0.092)
Number of States	43	43	43	43	43	43
Observations	731	731	731	731	731	731
R-squared	0.995	0.996	0.997	0.997	0.991	0.995

All standard errors are adjusted for clustering at the state level. Robust p-values in parentheses
*** p<0.01, ** p<0.05, * p<0.1

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