ESSAYS ON LABOR ECONOMICS

by

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ABSTRACT

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Changes in the labor market environment can alter how the market allocates and compensates labor forces. This dissertation examines the impact and relevance of employment protection and local demand on employment and wage by focusing on the employment at-will exceptions and the housing net worth channel.

Employment relationship in the U.S. deviated from its at-will tradition over 1980's by recognizing three major exceptions to the at-will doctrine across states. The First chapter of this dissertation investigates the impact of the at-will exceptions on wage and employment by focusing on the implied contract exception. Using data from a Panel Study of Income Dynamics (PSID) and difference in differences estimation strategy, the analysis identifies the negative impact of the adoption of implied contract exceptions on both employment and wage among the head of household, and the increased return to tenure compensates for the negative impact on wages. Subsample analyses reveal that the negative impact is typically more substantial for the younger age group, non-unionized occupations, and high school graduates without a college diploma. More relevant occupations and industries preserving the main results are classified by keeping the complement group to have insignificant results. Short-lived effects concentrated within the early adoption cohort suggest a non-market response of employers to legal adoption. The main results are robust to a more restricted control group setting and alternative estimation strategy excluding already treated samples from the control group.

The second chapter of this dissertation examines the employment consequence of the local housing net-worth channel by separating tradable and non-tradable employment. Using the state-level tradable and non-tradable employment data from 1990:M1-2019:M12, a stronger relevance is observed between the local housing price and the non-tradable employment relative to the tradable employment. Estimation using the panel VAR shows a stronger and more persistent impact of house price shock on employment relative to personal income or building permits. The response to the house price shock is more distinctive for non-tradable employment by reflecting the local demand channel associated with the local housing market. Out-of-sample forecasting analysis translates the in-sample asymmetry to asymmetric out-of-sample forecasting performance between tradable and non-tradable employment. Including house prices improves the forecasting performance in predicting non-tradable employment, and the performance gain for predicting tradable employment is minimal. This forecasting performance gap is typically more significant for states with inelastic housing supply and volatile house prices.

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Chapter 1: REVISTING LABOR MARKET RESPONSES TO "AT Will" EXCEPTIONS

1.1 Introduction

Employment in the United States has traditionally been terminable at will in the absence of a formal employment contract (either individual or collective). This legal presumption followed the belief that employers and employees have equal legal rights and similar bargaining power. The final quarter of the twentieth century saw a variety of legislative and judicial initiatives that created exceptions to the at will doctrine. These exceptions have been seen as a recognition that employers have greater bargaining power (Muhl 2001). In addition to violation of an explicit contract and discriminatory dismissal, three major judicial exceptions to the at will doctrine have been created almost exclusively at the state level. These include dismissal under an implied contract, when it hurts public policy, and when it breaks a covenant of good faith. The central objective of this study is to reinvestigate the impact of these at will exceptions on employment and wages.

Employment protection law potentially influences a firm's hiring and compensation decisions by increasing dismissal costs. These increased expected costs of employment have been argued to make hiring more expensive and so will be reflected in lower wages, reduced employment or both. Yet, the actual impacts on employment and wages are inconclusive in previous research. The current study follows a broadly similar approach to Autor et al. (2006) who investigated the influence of exceptions with a difference-in-difference methodology. Instead of using aggregated data, I use individual data from the Panel Study of Income Dynamics (PSID).

The use of individual data allows accounting for individual fixed effects to see if workers who remain in the same state experience changes in employment or earnings. This holds constant movements by workers between states that could be a logical response to new exceptions and influence aggregate results. It also allows separate examinations by demographic characteristics that are more difficult or impossible with aggregated data. The argument that such differences might be anticipated is one of the contributions of this paper.

The results confirm Autor et al. (2006) in showing a reduction in employment associated with the advent of the implied contact exception. Indeed, the point estimate is of broadly similar size. It also confirms previous work showing that in an undifferentiated estimate, there appears to be no influence on earnings when clustering errors by state. Yet, disaggregation reveals important patterns not previously confirmed. First, the employment results are largely driven by males and young workers. There appears to be no influence on women and older workers. We suggest this may reflect that the expected cost of breaking a contract with men and younger workers is larger. Second, the impact of legal adoption is concentrated among workers with high school diplomas but without a college degree. There is little or no influence on either high school dropouts or on these who have completed a college degree. Third, wage influences can be identified by including important elements of work history not available in aggregate data. We confirm that workers with low tenure experience a significant reduction in earnings and that accumulated tenure shrinks the size of the reduction. We argue that such tenure results might be anticipated because employment protection limits the opportunity to improve match quality over time. Workers with long tenure have earnings reflecting this match quality. New hires and workers with low tenure remain less selected on match quality and limits on discharge hurts their earnings more (Lazear 1998).

The rest of the article is organized as follows. Section 2 presents a summary of past work and demonstrates how our approach contributes. Section 3 describes the data and the legal variables. Section 4 summarizes the conceptual approach and empirical methodology. Section 5 presents the empirical results on employment and wages of the exceptions. Finally, Section 6. offers concluding remarks.

1.2. Setting the Stage

In this section we first describe the exceptions to the at will employment doctrine in the United States. Second, we review the past literature estimating the consequences of these exceptions. Third, we motivate our decision to return to this literature and describe our valued added.

1.2.1 Exceptions to the At Will Doctrine

In many countries, employment protection takes the form of direct monetary compensation or a mandatory notice period for dismissal. The degree of such protection (which workers are eligible, the amount of the compensation and the length of the notice period) varies across countries (see Heywood et al. 2018). Following English common law, the United States traditionally held that employment is at will and can be terminated by either party. Thus, severance pay, dismissal compensation and extensive notice have not traditionally been mandated in the United States. It remains the case that the U.S. dismissal process is relatively simple according to the OECD Indicators of Employment Protection (OECD 2023). While often identified as the industrialized democracy with the least employment protection, the last quarter of the last century saw growing exceptions in the US to the at will doctrine. Rather than originating with the federal government, the exceptions came over time from individual state governments. Moreover, rather than being legislative, the exceptions typically followed judicial decisions that set precedent for employers and provided causes of relief for workers injured by "wrongful discharge."

While a few states created exceptions as early as the 1950s, most exceptions were recognized between the late 1970s and the early 1990s. These exceptions limit employers' ability to dismiss their workers by defining circumstances of wrongful discharge. These circumstances fall into three categories: (1) the discharge undermines an explicit public policy of the State (public policy exception); (2) the discharge breaks a covenant of good faith and fair dealing (good-faith exception); and (3) the discharge violates an implied contract between the employer and employee. (implied contract exception). We will review the evidence on which of these has been most important empirically but note that that they were often implemented in close succession across a variety of states. This makes identification of influences difficult and requires a suitable empirical strategy.

The public policy exception protects workers from dismissal that undermines well-established public policy including but not limited to a state constitution, statute, or administrative order. For example, an employer cannot discharge a worker who refuses an employer's solicitation of perjury on behalf of the employer. Similarly, it undermines public policy to discharge a worker for performing jury duty or filing a worker's compensation claim for a work-related injury. Walsh et al. (1996) argues that public policy is the longest and most well-recognized legal exception. Yet, despite this, Autor et al. (2006) find very few successful cases under this exception. This, they say, reflects a tendency to narrowly limit the applicability to very clear and explicit statements in constitutions or statues rather than to broad interpretation of what is in the interest of public policy.

The good faith exception has potentially the broadest ramifications as good faith and fair dealing presumably apply to all employment relationships. This exception requires the employer to have “good cause” to discharge or, at least, prohibits an employer from discharging an employee in bad faith. In some cases, the good faith exception recognizes longevity of employment history and past successful service as a ground for maintaining good faith. Yet, in most states and in most applications, the exception is not given a broad conceptual definition. First, the least number of states recognize good faith exceptions. Second, in those states that do, successful cases are typically limited to showing malice or extreme self-interest on the part of the firm. Thus, a classic case would involve the timing of discharge. A worker had done everything to earn a substantial end of year bonus but was discharged a day prior to when the bonus was to be paid so as not to provide the promised bonus. This reflects a far narrower view of good faith.

The implied contract exception is recognized by about 40 states. The implied employment contract can be established when an employer makes a written or oral representation of what constitutes performance satisfactory for retention. The manager may say if you accomplish these four things, you will have done a good job this year. Alternatively, an employee handbook could specify the causes for employment termination. These can be seen as binding implicit contracts. Discharge after achieving the performance the manager said would be satisfactory or for reasons other than those identified in the handbook become wrongful discharge. Even if there are no explicit handbook descriptions or managerial statements, an implied contract could be theoretically established by examining the historical circumstances in which similar employees have or have not been discharged or from an accumulation of less explicit but convincing managerial statements. Indeed, as we will see, this exception has been seen as having made the deepest inroads into the at will doctrine.

As reviewed, wrongful discharge differs in the US from many other countries. First, as emphasized, the concept relies on state court decisions rather than national legislation as is common in Europe. Thus, a given exception may not have the same interpretation across states. Second, there is no government authority, state or national, policing such discharges and no labor law court with unique jurisdiction as in common in continental Europe. Success in a wrongful discharge case in the US requires individual legal action on the part of the discharged worker as a plaintiff.

The exact case creating a given exception in a state is not always clear. This has resulted in several classifications dating when exceptions become law. Among these, we focus primarily on that in Autor et al. (2006). They convincingly emphasize the date of precedent-setting cases rather than of proclaiming cases. In a few key states, this changes timing and so treatment dates. We will, however, briefly discuss the consequences of using other classifications.

1.2.2 Past Theory and Evidence

This subsection first provides a brief description of the theory of why employment protection may influence earnings and employment. It then summarizes some of the European evidence on these predictions. It concludes with a description of the US evidence on employment protections. Again, the European studies largely examine legislated job protections including severance provision and rights to employment. The US evidence examines the judicial exceptions to employment at will.

Employment protection imposes a new cost to discharge. Lazear (1990) predicts that perfectly flexible wages will absorb this cost by lowering the initial wage. This compensates for the new increase in the cost of discharge. Yet, he recognizes that imperfections in the labor market (other government policies, unions, the presence of long-term implicit contracts and so on) can generate combinations of reduced wages and employment. In these cases, the additional cost of discharge increases retention of incumbents while decreasing new workers' chance of being hired. Very similar predictions can be found in Bentolila and Bertola (1990).

Betorla (1992) presents a model emphasizing discounting and natural worker attrition. When these are sufficiently large, the wage reduction could be accompanied by an average increase in employment. The optimal labor demand path is partially myopic, and increased firing costs can increase a myopic firm's worker demand through a rational ratchet effect. Alternatively, Lindbeck et al. (2001) describes a wage advantage of incumbent workers stemming from a better bargaining position obtained by employment protection mandating higher dismissal cost. In this classic insider-outsider view, the wages of insiders need not decline even as the wages of new hires absorb the decline predicted by Lazear (1990). Even this short review makes clear that theoretical predictions can vary as to how large and widespread a decline in either earnings or wages should be anticipated.

Various researchers have investigated the actual impact of employment protection law, and this research yields no consensus. One vein of this research uses the variation in employment protection across countries and time. Much of this focuses on either OECD or European countries. Nickell (1997) and Nickell et al. (1999) directly utilize a ranking of employment protection across OECD countries to examine its relationship to unemployment. They find mostly insignificant associations with durations of unemployment. Blanchard et al. (2000) also developed an index by combining the OECD measure with differences in required severance pay as emphasized by Lazear (1990). Focusing on labor demand shocks, they find that increased employment protection increases unemployment. Belot et al. (2004) focus on unemployment among OECD countries confirming a negative impact of employment protection on unemployment. Garibaldi et al (2005) generate a very similar result in a replication study. Kahn (2007) reports a higher rate of non-employment and incidence of temporary employment that is more pervasive among youth, immigrants, and women population. Addison and Teixeira (2005), and Fialová & Schneider (2009) undertake broadly related examinations on employment but find weak or no influences. Bordon et al. (2018) shows that labor market reform, identified by a reduction in the OECD indicator of employment protection, tends to have a lagged positive impact on employment. Piton and Rycx (2018) simultaneously examine product and labor market deregulation. Using the same OECD indicator, they demonstrate a long-run reduction in unemployment but a short-run increase in unemployment.

Heywood et al. (2018) argue that the influence of employment protection will result in sharply lower wages in those industries that pay a compensating differential due to inherently uncertain employment. Using a measure of layoffs in the US as an inherent industry measure, they confirm this reduction in wages for a thirty-year period across European countries.

It is crucial to emphasize that in this vein of research the employment protection variables do not change much over time. Thus, the variation in employment protection comes largely from cross-sectional differences. This leaves open important questions of country specific heterogeneity,

There has been work on European employment protection that is country specific and focuses on legislative changes. One focus has been the German relaxation of the Protection Against Dismissal Act (PADA) in 2004. This relaxation increased the firm size threshold for protection from 5 to 10 employees. Bauernschuster (2009) uses Diff-in-Diff estimation and finds an increase in hiring activity without any change in dismissal behavior. Bauer et al. (2007) found no significant impact on hiring or dismissal. Priesack (2015) confirms that there is not robust evidence of any influence on the overall job flow rate, on the share of employees on fixed-term contracts or that are temporary agency workers. The change in the legal threshold from 5 to 10 employees may simply have been too modest to identify influences.

Italy has among the most binding employment protection (OECD 2023). Dismissed workers receive mandatory severance payment. They are eligible for compensation for forgone wages and for mandatory reinstatement for unjust dismissal. In addition, hiring temporary workers is extremely difficult. Kugler et al. (2006, 2008) investigate a 1990 reform that newly mandated severance payments to small firms. They confirm a reduction in dismissals and new hires. On net, employment increased for men but decreased for women. Leonardi et al. (2013) investigated the impact of the same reform on wages. They identify a slight reduction in average wages driven by lower entry wages. The wage reduction is concentrated among young workers, blue-collar workers, low-wage workers, and workers in low-employment regions.

Martins (2009) examines the impact of 1989 Portuguese reform in 1989 that allowed greater employment flexibility for small firms. He shows a slight increase in hiring and a significant negative impact on wages that he argues reflects increase employer bargaining power. Van der Wiel (2010) examines a 1999 increase in the dismissal notice period in the Netherlands. Appealing to a model with endogenous wage setting behavior by a monopolistic union (Garibaldi and Violante 2005), he shows that each month of additional notice is associated with almost 3 percent higher wages. Cervini-Plá et al. (2014) examines a 1997 Spanish reform that reduced firing costs. He demonstrates an increase in wages and employment.

As is clear from this review of country specific changes, there is no consensus on the influence of changing employment protection on labor market indicators. The nature of the reform and the country specific institutions likely contribute to the contradictory results. Thus, the problems that confound cross-country results reemerge when trying to generalize across individual country specific studies.

In the U.S., general laws do not dictate severance or individual notice periods. They also do not typically assume all dismissals must be for cause.[1] As emphasized, the adoption of exceptions to at-will employment largely reflects court decisions in each state. Timing the implementation for a given state and exception differs between researchers. Dertouzos et al. (1992), Walsh et al. (1996), and Autor et al. (2003) each develop timing classifications. These timings are used in panels of states and years to make difference-in-difference estimates of the impact of the at-will exceptions on various measures of wages and employment. The studies typically use state-wide employment ratios, unemployment rates and wage measures.

Dertouzos et al. (1992) classifies adoption of exceptions with a tort cause of action (where both compensatory and punitive damage are available for plaintiffs) or a contractual cause of action. They identified 3% decline in aggregate employment for adoption of a tort cause of action with a stronger effect in industries such as service and retail. They use an instrumental variable approach by estimating the probability of adoption using a logistic regression in the first stage. Their findings used data earlier in the process of creating exceptions and have not been reexamined with more recent exceptions. Moreover, Autor et al. (2006) criticizes the identifying variables, unionization share and the change in unemployment rate, as likely to violate the exclusion restriction as they directly influence state employment.

Miles (2000) examines employment and wages using the timing classification developed by Walsh et al. (1996). He finds an increase in temporary employment associated with the

[1] Montana is the exception in which actual laws provide a requirement for just dismissal. In this respect, Montana is explicit in not being an employment at will state.

implied contract exception but no effect on employment, unemployment, or the sectoral mix of employment. He concludes that the labor market consequences of the exceptions are modest. Autor et al. (2006) find no influence from two of the exceptions but find that the implied contract exception significantly reduces the employment population ratio by 0.8% to 1.7%. They find no impact of any of the exceptions on wages. They argue that their legal classification identifying the precedent-setting case is the main reason for the discrepancy in results from Miles (2000). Kugler et al (2004) utilize the timing classification of Autor et al. (2003) to investigate worker flows. The result shows that implementation of the wrongful discharge laws lower the job finding probability of unemployed worker relative to employed worker. This impact is smaller for unionized workers and temporary workers who lost jobs due to the end of contract.

Although Autor et al. (2003) found not influence on wages, they left open the possibility that the influence could be positive. They point out that additional job protection could increase wages by providing workers more bargaining power or incentivizing firms and workers to accumulate more human capital. In later work, Acharya et al. (2014) presents a model of enhanced employees' effort for innovation when employment protection limits firms' ability to hold up the employees' innovative outcomes. Empirically, Bird (2009) shows that the implied contract exception is associated with higher labor expenses and with lower firm profitability. This hints at higher wages for already hired workers. Thus, while Autor et al could not confirm a role for the exceptions on wages, it remains an issue of interest and inquiry.

1.2.3 The Value Added of this Examination

This study relies primarily on the timing classification of Autor et al (2003) to reexamine US evidence on employment and wages.[2] Following from that work, the primary focus is on the implied contract exception although the other exceptions will be discussed. The primary innovation is the use of individual data from the Panel Study of Income Dynamics (PSID). This innovation allows several refinements and explorations beyond those in previous studies which use aggregate data.

First, the individual data provides far greater detail on individual characteristics. This provides the inclusion of important controls such as job tenure not possible in the aggregate data. This also allows examining the influence of the at-will exceptions for heterogeneity. Some differences, such as by gender are straightforward but others cannot be examined without individual data. Of particular importance is the potential role of tenure. Lazear (1990) suggested that the influence of employment protection would be a reduction in the starting wage. Yet, as reviewed in the last section, workers may gain advantages in human capital accumulation, effort provision or bargaining over the course of employment. This suggests that the primary influence of employment protection could be on the slope of the earnings profile, a reduction in starting wages made-up over increased tenure with the firm. This might appear in aggregate data as no general increase in earnings but remains an important labor market consequence. The use of individual data allows an examination of this implication.

An additional example of using the heterogeneity inherent in individual data is the ability to identify union status. Union members have explicit employment rights given by contract. They are not at will employees and under collective bargaining may typically only be terminated

[2] Results using the alternative classifications will, however, be briefly discussed.

for cause. Thus, one would anticipate that exceptions to the at-will doctrine would have little or no influence on their terms of employment.

Second, the use of individual data allows controlling for potential interstate spillover consequences of employment protection. Thus, if employment protection in one state causes workers to move to another state, these differences will be reflected in aggregate difference-in-difference estimates. By concentrating on workers who remain in both the treatment state and the control states before and after treatment, the influence of employment protection excludes the spillover role that movers might play in influencing the aggregate data. As eventually, the exceptions came into being in all but a few states, it may be that spillovers are of only temporary importance.

Third, the use of individual data allows estimating within worker influences. Such fixed effects control for time-invariant but unobservable worker characteristics that influence the wage or the likelihood of employment. This could be critical if patterns of such worker characteristics influence the timing of court or legislative decisions.

1.3. Data and Legal Variables

In what follows the legal variables, and their timing are initially discussed. We use this discussion to identify how we will initially implement a treatment window approach. This is followed by describing the data taken from the PSID.

1.3.1 Legal Variables

This study focuses on the legal classification for the three at-will exceptions developed by Autor et al. (2003). Table 1.1 presents their classification showing the timing for each at will exception by state. All fifty states and the District of Columbia are listed in the table. Note that most, but not all, states created exceptions within the time frame of our time series. California had on exception dating from the 1950s and several states retain no exceptions throughout our time series.

Table 1.1. Law Implementation

State	PP	IC	CGF	State	PP	IC	CGF
Alabama		1987		Nevada	1984	1983	1987
Arizona	1985	1983(84)	1985	New Hampshire	1974	1988	1974(80)
Arkansas	1980	1984		New Jersey	1980	1985	
California	1959	1972	1980	New Mexico	1983	1980	
Colorado	1985	1983		New York		1982	
Connecticut	1980	1985	1980	North Carolina	1985		
Delaware	1992		1992	North Dakota	1987	1984	
District of Columbia				Ohio	1990	1982	
Florida				Oklahoma	1989	1976	1985(89)
Georgia				Oregon	1975	1978	
Idaho	1977	1977	1989	Pennsylvania	1974		
Illinois	1978	1974		Rhode Island			
Indiana	1973	1987		South Carolina	1985	1987	
Iowa	1985	1987		South Dakota	1988	1983	
Kansas	1981	1984		Tennessee	1984	1981	
Kentucky	1983	1983		Texas	1984	1985	
Louisiana			1998	Utah	1989	1986	
Maine		1977		Vermont	1986	1985	
Maryland	1981	1985		Virginia	1985	1983	
Massachusetts	1980	1988	1977	Washington	1984	1977	
Michigan	1976	1980		West Virginia	1978	1986	
Minnesota	1986	1983		Wisconsin	1980	1985	
Mississippi	1987	1992		Wyoming	1989	1985	1994
Missouri	1985	1983(88)		Alaska	1986	1983	1983
Montana	1980	1987	1982	Hawaii	1982	1986	
Nebraska	1987	1983					

PP: Public Policy, IC: Implied Contract, CGF: Covenant of Good Faith
Source: Autor, D. H., Donohue, J. J., & Schwab, S. J. (2006). The Costs of Wrongful-Discharge Laws. *The Review of Economics and Statistics*, 88(2), 211–231.

The original source of law classification includes the month of the legal adoption. As the adoption months had to be ignored to work with the yearly observations of PSID dataset, the distance between the actual adoption date and the post treatment period necessarily becomes uneven across states. The average distance between the adoption month and the PSID post treatment data are 6.61 months for implied contract, 6.23 months for public policy and 4.31 months for good faith. When the adoption months are binary coded into the first and second half of the year, regressing against the alphabetic order of state does not reject the null of random order of adoption. Moreover, this issue is partially addressed in the empirical estimation by comparing the result with different pre and post treatment windows and by excluding the year of treatment as will be described.

Given the year of legal implementation by states, treatment samples collect observations within predetermined treatment windows excluding the year of implementation. Therefore,

$$Treat_{st} = \begin{cases} 1 & if\ (y-p) \leq t < y, or\ y < t \leq (y+p) \\ & 0, otherwise \end{cases} \tag{1}$$

where, s and t is state and time, y is the year of legal implementation, and p is pre and post implementation period for the treatment window. For most of the analysis, both 3 and 4 years pre and post treatment windows are used. In the case of an individual who moves to other states within the treatment window, observations after the relocation will be excluded from the treatment sample. As emphasized, individual data allow removing the influence of any migration between states caused by the exceptions.

Given the treatment window above, construction of the control sample is discussed. The initial construction mirrors that in Autor et al. (2002). The full control sample includes observations from states that created no new exceptions of any kind within the treated states treatment window. As in Autor et al., both already treated and not-yet treated observations are eligible to be in the control sample. Recognizing the issues of heterogeneity and timing in a rollout design (Goodman-Bacon 2021), two alternatives are also explored. The first alternative control group excludes observations from already treated states. Thus, the control includes observations from never-treated states and from states treated after the treatment window. The second alternative control group includes only observations from the never treated states.[3]

In the case of implied contract in Arizona and of good faith in Oklahoma, the court overturned the previous decision within a treatment window. As in Autor et al. (2003), these two cases are not dealt with differently than the others. First, the affected observations are vanishingly small, only about 0.15% of the total observations in the dataset. Second, the response to the implementation and the removal may not be symmetrical suggesting inquiry remains interesting. Finally, their exclusion changes none of our substantive conclusions.

1.3.2 PSID Data

To estimate the impact of the legal implementation on wages and employment, we utilize the Panel Study of Income Dynamics (PSID). The PSID is an annual longitudinal survey of roughly 5000 households since 1968. To incorporate the bulk of the exceptions, we use data from 1976 to 1997. The survey is largely performed on a household basis, and a consistent wage series is available only for the head of each household. Since the survey questions are also more detailed about the head of household, the analysis is limited to heads of households.[4] The full sample contains 18929 individuals with 166686 observations. The wage data excludes observations with very extreme values or that have missing data on the hourly wage, yearly work hours, or job tenure. The resulting sample is 14476 individuals and 96895 observations. This subsample is slightly younger and more likely to be both married and male. The survey questions

[3] Control samples are collected within the union of all treatment windows to exclude control observations without matching treatment sample. Thus, if a certain year is outside of all treatment windows, the year will be excluded from the analysis.
[4] Therefore, individual data can be incomplete if an individual is the household head only in some years but not others.

for earnings are retrospective; therefore, data in survey year t refers to year t-1 earnings. To account for this, the analysis focuses on the earning year rather than the survey year.

Table 1.2 reports the descriptive statistics for the full and sub-sample. The hourly wage is defined as the gross annual income divided by the annual hours worked. The gross annual income includes the pre-tax amount of wage, salaries, bonuses, overtime, commissions, professional practice, and the labor part of the farm and business income. As the annual hours worked include work hours for all occupations, the gross annual income also includes non-wage components such as a bonus in the variable. An individual is classified as employed if the person is currently employed or only temporarily laid off, and all other observations are classified as a not-working which includes unemployed, retired, and permanently disabled individuals as well as students and those engaged in exclusively in household production. The unemployed are those not working but currently looking for work.

Job tenure is typically surveyed. If a direct measure is unavailable, missing values are imputed based on job starting year and month. As noted by Brown (1992), the tenure variable is not routinely consistent over time. The reported job tenure may not accord with the calendar year due to the reporting error, rounding error, or in the case returning to the previous occupation with accrued tenure. Brown (1992) suggests identifying job change based on changes in reported tenure. Marcotte (1998) compared several suggested job change identification methods and found they are broadly consistent. Since the source of reporting error is not unique and rounding issues are quite common, multiple layers of correction based on already corrected value is unavoidable to construct a completely consistent tenure variable from which to identify job changes. As our application does not require an accurate detection of job change and is more interested in simply tenure, the tenure variable is only corrected more by the missing value imputation.

Table 1.2. Descriptive Statistics

Full Sample	**Mean**	**Std. Dev.**	**N**
Hourly Wage (Log)	2.067978	0.834053	128508
Employment (Ratio)	0.696597	0.459729	166686
Tenure (month)	61.2735	104.9921	165477
Male (Ratio)	0.691276	0.461968	166686
Married (Ratio)	0.561637	0.496188	166686
Education (Year)	13.29542	11.52262	166686
Age (year)	43.13963	16.93118	166686
Experience (year)	17.40696	19.93205	166686
Male			
Hourly Wage (Log)	2.172941	0.811176	97398
Employment (Ratio)	0.780805	0.413703	115226
Tenure (month)	70.57124	109.9214	114186
Married (Ratio)	0.809036	0.393062	115226
Education (Year)	13.35866	10.57713	115226
Age (year)	41.72141	15.56523	115226
Experience (year)	18.57521	19.18922	115226
Female			
Hourly Wage (Log)	1.739364	0.819159	31110
Employment (Ratio)	0.508045	0.49994	51460
Tenure (month)	40.57451	89.68947	51291
Married (Ratio)	0.007676	0.087276	51460
Education (Year)	13.15383	13.39894	51460
Age (year)	46.31522	19.27368	51460
Experience (year)	14.7911	21.27126	51460
Wage Sample	**Mean**	**Std. Dev.**	**N**
Hourly Wage (Log)	2.204791	0.6440136	96895
Tenure (month)	93.44494	95.35792	96895
Male (Ratio)	0.7742608	0.4180704	96895
Married (Ratio)	0.6469477	0.4779213	96895
Education (Year)	13.77183	10.28883	96895
Age (year)	37.3572	11.80334	96895
Experience (year)	14.48813	15.7365	96895

Source: Panel Study of Income Dynamics, public use dataset.

As the sample is limited to the heads of households, some of the needed questions are asked only of such heads. Consequently, the sample is disproportionately male. Moreover, most of the female sample consists of singles, widowed, and divorced women as married couples almost always report the husband as the head of the household. This creates a noticeable discrepancy between male and female heads of household samples. Average tenure and experience are smaller for the female sample even as the female sample is older than the male even as it has a similar year of education. The observed wage difference in hourly wage is 43%. The employment-population ratio is 50% for the female sample, while it is 78% for the male sample.

1.4. Empirical Methodology

As made clear, at will exceptions in the U.S., do not create explicit dismissal costs such as automatic severance pay. Dismissals increase costs only if dismissed workers bring suits for damages. Moreover, even this expected cost may be reduced by the firm revising or stipulating contractual elements that reduce the likelihood of successful suits. Nonetheless, a reduction in employment, even if temporary, is expected for the implementation of the at will exceptions.

Similarly, the wage may also fall to reflect the increased dismissal cost. The reduced likelihood of dismissal may encourage workers to accumulate more human capital and potentially put more effort into their work. This could alter the tenure wage profile. Kugler et al. (2004) showed that increased dismissal costs can increase the quality of employed workers as the firm becomes more selective in hiring. This could imply that workers with successful employment elsewhere become more valuable relative to unemployed or new job seekers. This could also have an influence on tenure wage profile.

The empirical investigation identifies the impact of the exceptions on employment and hourly wage in adopted states using the difference in difference design. Autor et al. (2006) argue that converting the long panel to a short panel mitigates the issue that comparing samples with a decade or more time differences. Moreover, Edelman et al. (1992) emphasizes that firms can respond to the exceptions by revising contractual terms, manuals and dismissal procedures over time. This suggests that the impact could be concentrated around the time of implementation making a short panel more appropriate.[5] The short panel approach matches the pre and post period for all implementation cases by states. Thus, the methodology resembles the stacked difference in differences approach, though it does not explicitly separate each adoption and duplicate control observations.

For the impact on employment, the regression equation estimates a linear probability model:

$$e_{ijst} = \beta_1 Treat_{st} + \beta_3(Treat_{st} \times Post_{st}) + \beta_4 Postpost_{st} + \beta' X_{ist} + \gamma_s + \delta_t + \pi_j + \phi_i + \epsilon_{ijst} \quad (2)$$

where e_{ijst} is the employment status of the individual i in region j and state s at time t. $Treat_{st}$ is the indicator for the treated sample within the 3 or 4 years pre and post treatment window excluding the year of implementation. $Post_{st}$ indicates the sample within the 3 or 4 year post period of legal adoption, and the $Postpost_{st}$ is the dummy for posttreatment period specifying the sample outside of the treatment window in treated states. (explain why this is in) X includes the individual level control variables including the marital status, years of education, age and labor market experience. γ_s, δ_t, π_j, ϕ_i, are the vectors of state, time, region, and individual dummies to account for the unobserved factors affecting the outcome variable. In this specification, β_3 is the coefficient of interest identifying the impact on employment.

The large sample identified in the earlier section is used in the estimation of this equation. Consequently, the equivalent of two aggregate measures can be examined. By identifying the employment status as employment relative to any other status, it can represent the individual

[5] Indeed, Autor et al. (2006) experiments with longer pre and post periods showing smaller influences. One of their explanations is the extremely high legal costs in cases in the initial years.

based equivalent of the employment population ratio. By identifying employment relative to only those employed or looking for work, it represents the individual equivalent of the employment rate (the complement of the unemployment rate). Each of these will be estimated.

The influence on wages is necessarily estimated on the smaller sample of the employed and follows the framework above except with a change of dependent variable:

$$\begin{gathered} w_{ijst} = \beta_1 Treat_{st} + \beta_3(Treat_{st} \times Post_{st}) + \beta_4 Postpost_{st} \\ +\beta' X_{ist} + \gamma_s + \delta_t + \pi_j + \phi_i + \epsilon_{ijst} \end{gathered} \tag{3}$$

where w_{ijst} is the natural log of the hourly wage for individual i. Similarly, β_3 represents the impact of the law implementation on log hourly wage which can be interpreted as an approximate percent change.

For the impact on wage, we also examine the heterogenous effect across the tenure profile by using the triple difference term interacting job tenure and legal implementation. The model specification is

$$\begin{gathered} w_{ijst} = \beta_1 Treat_{st} + \beta_3(Treat_{st} \times Post_{st}) + \beta_4 Postpost_{st} + \tau_1 tenure_{it} + \tau_2 tenure_{it}^2 \\ +\tau_3(tenure_{it} \times Treat_{st}) + \tau_4(tenure_{it} \times Treat_{st} \times Post_{st}) + \gamma_s + \delta_t + \pi_j + \phi_i + \epsilon_{ijst} \end{gathered} \tag{4}$$

where and $tenure_{it}$ is the job tenure in years for individual i at time t. The coefficient of the triple interaction (τ_4) captures the change in return to tenure generated by the legal exception. To further account for the heterogeneity in the return to tenure across states, a model including state and tenure interaction is also estimated.

In the case of both examining employment and wages, estimates will be run on the subsample of males. This reflects the fact that they are disproportionately heads of households and that males typically have greater labor force attachment and so may face a greater cost to employment protection. The males will be further divided to focus on those who are youngest. If low tenure workers are most hurt by employment protection, this may be evident in a group that is 30 and younger. An additional examination focuses briefly on union workers suggesting they are less influenced by employment protection. This seems sensible as collective contracts have historically provided contractual protection and have long been recognized as not part of the at will doctrine. For both employment and wage, the same estimation is performed by using the subsamples distinguished by the union status of each observation.

Finally, alternative control groups designed because of the roll out design will be examined. Our estimation strategy is not exactly the typical application of two-way fixed effect (TWFE); however, it is similar enough to share the issue with the heterogeneous treatment effects as the control group includes observations from already treated states. If the treatment effect is not constant over time, the difference in differences estimation will be the treatment effect on treated net of the evolution of the treatment effect within the control group. The estimated result can have an opposite sign if the evolution of the treatment effect for the already treated group overwhelms the treatment effect on the treated. We expect the influence of this bias should be limited as the impact of the at-will exceptions may be temporary and not prevalent outside of our treatment windows (Edelman et al. 1992, Autor et al. 2006). To examine this, we constructed alternative control groups excluding the observations from already treated states. Also, the pattern of individual state results over time will be illustrated as part of this process.

The short panel approach builds a treatment window around each year. It excludes observations treated at any year within the window other than the treatment date being examined. Consequently, the main analysis is limited to the short-term impact of the treatment. Given the roll-out design, investigating the long-term impact within the same framework would have trouble finding suitable controls. Moreover, balancing the post treatment period for each adoption cohort in longer windows faces data availability problems. The never treated control group allows investigating the pattern and evolution of the impact across adoption cohorts and time. Using the never treated control group, we largely replicate our main analysis. In addition, we investigate the influence of the adoption timing and the impact over an extended time span by separating the early and late adoption cohorts. Nonetheless, we recognize the limitations of our method for isolating long run influences.

1.5. Results

The initial results for employment and wage outcomes are presented in subsections 5.1 and 5.2. Identical analyses are performed for both the full sample and the subsamples constructed by age, education, and union status. Results for racial sub-samples have been estimated but are omitted as they do not present any significant differences. It can be provided upon request.

1.5.1 Initial Employment Results

Table 1.3 presents estimates of equation (2) to examine employment. Panel A summarizes the estimate where the dependent variable is employment, but the alternative is any form of not working. This is the equivalent of an employment-population ratio. Results are shown capturing clustered robust standard errors across individuals and, alternatively, across states. The estimated influence of the implied contract exception appears with a routinely negative coefficient but while significant with individual clustered errors, is never significant with state clustered errors. Panel C undertakes the same estimate on the full sample using employment as the dependent variable, but the alternative is being unemployed, not working but looking. This is equivalent to the complement of the unemployment rate. Again, there is no statistical significance with the state clustered errors.

Panels B and D estimate the identical specification but limit the sample to men. The results appear larger in size and our now routinely statistically significant for all estimates regardless of clustering technique. Adoption of the implied-contract exception reduces the employment-population ratio by 1.4 percent within the 3-year window and by 1.5 percent within the 4-year window. The same adoption reduces the employment rate by 1.1 percent within the 3 and 4 windows. These estimates results are roughly comparable to those presented in Autor et al. (2006).

Table 1.3. Estimation result of the impact of Implied contract law on Employment

Panel A. Full, Not Working	(1)	(2)	Panel B. Male, Not Working	(1)	(2)
Treatment window	3 years	4 years	Treatment window	3 years	4 years
Treat × Post	-0.0107	-0.0127	Treat × Post	-0.0137	-0.0149
t stat_pid	(-1.92)*	(-2.18)**	t stat_pid	(-2.19)**	(-2.31)**
t stat_State	(-1.31)	(-1.53)	t stat_State	(-1.97)*	(-2.06)**
State/Indiv. FE	Y	Y	State/Indiv. FE	Y	Y
N	122911	136363	*N*	84937	94109
Panel C. Full, Unemployed	(1)	(2)	Panel D. Male, Unemployed	(1)	(2)
Treatment window	3 years	4 years	Treatment window	3 years	4 years
Treat × Post	-0.0069	-0.0087	Treat × Post	-0.0107	-0.0113
t stat_pid	(-1.58)	(-2.07)**	t stat_pid	(-2.38)**	(-2.65)***
t stat_State	(-1.06)	(-1.45)	t stat_State	(-2.17)**	(-2.46)**
State/Indiv. FE	Y	Y	State/Indiv. FE	Y	Y
N	92958	102993	*N*	70616	78044

Notes: Not working means that the alternative to working is any form of not working while unemployed means that the alternative to working is not working but looking for work.
* p<0.10, ** p<0.05, *** p<0.01

The male results may differ and deserve prominence for at least two reasons. First, recall that female head of households are unusual in the PSID and often reflect the absence of a husband and so may represent a qualitatively different sample. Second, male workers have longer tenure and the likelihood that they may eventually be dismissed would be higher all else equal. Shorter-term women workers may quit the firm earlier and more often all else equal. Thus, the expected dismissal costs could be larger for men.

Table 1.4 presents the employment consequence of the implied-contract exception by age group. Theory suggests that older workers with successful employment histories may become more valuable because of increased dismissal costs. Moreover, younger workers may have a longer expected tenure increasing the odds of eventual dismissal. The younger group consists of samples aged 20 to 30 years old, and the older group includes other observations up to 65 years old. The estimate shows a stronger impact on the younger age group for both employment outcomes. Panels A and B show that the impact of the implied contract on the employment population ratio is statistically significant and that the estimated negative impact is greater by 1.3 to 1.7 percentage points for the younger group. The difference is greater when the employment ratio is the outcome. Panels C and D show that adoption reduces the employment ratio by 3.8 percent within the 3-year window and by 3.5 percent within the 4-year window. At the same time, the impact on the older age group is not statistically significant for all treatment window and clustering techniques.

Table 1.4. Estimation result of the impact of Implied contract law on Male Employment by Age group

Age 20 to 30			**Age 31 to 65**		
Panel A. Not Working	(1)	(2)	Panel B. Not Working	(1)	(2)
Treatment window	3 years	4 years	Treatment window	3 years	4 years
Treat × Post	-0.0350	-0.0356	Treat × Post	-0.0181	-0.0193
t stat_pid	(-2.45)**	(-2.32)**	t stat_pid	(-2.25)**	(-2.32)**
t stat_State	(-1.99)*	(-1.96)*	t stat_State	(-2.20)**	(-2.36)**
State/Indiv. FE	Y	Y	State/Indiv. FE	Y	Y
N	23235	24796	*N*	52961	59486
Panel C. Unemployed	(1)	(2)	Panel D. Unemployed	(1)	(2)
Treatment window	3 years	4 years	Treatment window	3 years	4 years
Treat × Post	-0.0376	-0.0352	Treat × Post	-0.00755	-0.00798
t stat_pid	(-2.90)***	(-2.78)***	t stat_pid	(-1.44)	(-1.56)
t stat_State	(-2.47)**	(-2.56)**	t stat_State	(-1.37)	(-1.58)
State/Indiv. FE	Y	Y	State/Indiv. FE	Y	Y
N	22322	23805	*N*	46922	52702

* p<0.10, ** p<0.05, *** p<0.01

Table 1.5 shows the estimation results by education level. The negative employment impact observed in the previous analysis is largely concentrated among those with a high school diploma but without a college degree. The legal adoption reduces the employment-population ratio by 1.9 to 2.2 % within this middle education group. The impact is negligible for the group without a high school diploma or the group with a college degree. This suggests that implied the contract exception is irrelevant for the low skilled jobs of high school dropouts. It may also suggest that workers with college degrees are less likely to be replaceable, especially in 1980s,. The analysis with the unemployment sample yields almost identical results.

Table 1.5. Estimation results of the impact of Implied contract law on Male Employment by Education

Not Working	(1)	(2)	(3)	(4)	(5)	(6)
Education	Less than 12		12 to 15		16 and over	
Treatment window	3 years	4 years	3 years	4 years	3 years	4 years
Treat × Post	-0.00391	-0.00475	-0.0192	-0.0217	-0.00072	0.00036
t stat_pid	(-0.27)	(-0.31)	(-2.36) **	(-2.59) ***	(-0.06)	(-0.03)
t stat_State	(-0.23)	(-0.27)	(-2.27) **	(-2.57) **	(-0.07)	(-0.03)
State/Indiv. FE	Y	Y	Y	Y	Y	Y
N	22672	24470	45458	50579	16807	19060

* p<0.10, ** p<0.05, *** p<0.01

1.5.2 Initial Wage Results

Table 1.6 presents estimates of equations (3) and (4) to identify the impact of the implied-contract exceptions on log hourly wages. Job tenure acts as an additional control variable for equation (3) (Panels A and B), while equation (4) (Panels C and D) includes job tenure and the triple interaction between the treatment and job tenure. For panels A and B, the impact of the adoption reduces the wage by 1.5 percent for the full sample and 1.8 to 1.9 percent for the male sample within 3 and 4-years treatment window, respectively. However, the result is statistically significant only for the individual-level standard error clustering, and it is never significant with the more common state-level clustering.

Panels C and D of Table 1.6 show the estimation result with the interaction term between treatment and tenure, and all estimates of the coefficient of interest are statistically significant for both individual and state-level standard error clustering. The triple interaction term allows us to identify the heterogeneous influence of the wrongful discharge law over the job tenure dimension. The positive coefficient for the triple interaction term represents the additional return to tenure attached to the legal implementation. As Panel C shows, the implied contract exception lowers the wage by 3.3 to 3.5 percent but this is partially recovered by the *increased* return to tenure of 0.2 percent per year of job tenure.

Table 1.6. Estimation result of the impact of Implied contract law on Hourly Wage

Panel A. Full	(1)	(2)	Panel B. Male	(1)	(2)
Treatment window	3 years	4 years	Treatment window	3 years	4 years
Treat × Post	-0.0145	-0.0150	Treat × Post	-0.0190	-0.0175
t stat_pid	(-1.98)**	(-2.00)**	t stat_pid	(-2.29)**	(-2.06)**
t stat_State	(-1.15)	(-1.12)	t stat_State	(-1.43)	(-1.24)
State/Indiv. FE	Y	Y	State/Indiv. FE	Y	Y
Triple interaction	N	N	Triple interaction	N	N
N	71659	79270	*N*	55457	61228
Panel C. Full	(1)	(2)	Panel D. Male	(1)	(2)
Treatment window	3 years	4 years	Treatment window	3 years	4 years
Treat × Post	-0.0330	-0.0350	Treat × Post	-0.0418	-0.0415
t stat_pid	(-3.15)***	(-3.39)***	t stat_pid	(-3.50)***	(-3.51)***
t stat_State	(-2.12)**	(-2.07)**	t stat_State	(-2.73)***	(-2.43)***
Treat × Post × Tenure	0.00214	0.00224	Treat × Post × Tenure	0.00255	0.00261
t stat_pid	(3.27)***	(3.53)***	t stat_pid	(3.58)***	(3.71)***
t stat_State	(2.98)***	(2.60)***	t stat_State	(4.00)***	(3.19)***
State/Indiv. FE	Y	Y	State/Indiv. FE	Y	Y
Triple interaction	Y	Y	Triple interaction	Y	Y
N	71659	79270	*N*	55457	61228
Panel E. Full	(1)	(2)	Panel F. Male	(1)	(2)
Treatment window	3 years	4 years	Treatment window	3 years	4 years
Treat × Post	-0.0320	-0.0328	Treat × Post	-0.0414	-0.0400
t stat_pid	(-2.98)***	(-3.08)***	t stat_pid	(-3.37)***	(-3.27)***
t stat_State	(-2.03)**	(-1.90)*	t stat_State	(-2.69)***	(-2.31)**
Treat × Post × Tenure	0.00212	0.00218	Treat × Post × Tenure	0.00247	0.00254
t stat_pid	(3.25)***	(3.45)***	t stat_pid	(3.47)***	(3.62)***
t stat_State	(2.92)***	(2.48)***	t stat_State	(3.87)***	(3.09)***
Tenure × State	Y	Y	Tenure × State	Y	Y
State/Indiv. FE	Y	Y	State/Indiv. FE	Y	Y
Triple interaction	Y	Y	Triple interaction	Y	Y
N	71659	79270	*N*	55457	61228

* p<0.10, ** p<0.05, *** p<0.01

For the male sample, the adoption of an implied contract reduces the hourly wage by 4.2 percent and increases the value of an additional year of job tenure by 0.26 percent. Given the sample average tenure of 7 years, the negative impact of the adoption mechanically becomes 2.4

percent reduction on average, and it becomes zero when the job tenure reaches 15 to 16 years. For tenure beyond that, the implication would be an increase in earnings associated with the exception. This supports the view that increased dismissal decreases wages but also makes long tenure workers more valuable.

Our empirical strategy is a variation of the two-way fixed effect estimation modified by adopting the treatment window to limit the influence of observations far from the adoption timing. As our treatment sample is limited to observations within the treatment window, our empirical specification has the separate indicator for the treatment sample to address censored samples and distinguish the post treatment sample only within the treatment window. The control sample was also collected within the matched treatment window, but it is not possible to make a universal pre and post treatment distinction for the control sample due to the staggered legal adoption. Our empirical specification introduces the triple interaction with the tenure variable into this adoption cohort sample. To account for the possibility of state-by-state heterogeneity in the return to tenure, Panels E and F of Table 1.6 present estimation results with the full set of tenure interactions with state dummies. The results show that allowing this heterogeneity across states does not change our main result.[6]

Table 1.7 presents the impact of the implied contract on hourly wages by age group. The estimated result suggests that the significant influence of adoption is concentrated within the older age group. The point estimates on the treatment are of the same general magnitude so this may simply reflect sample size and the differences in precision. Moreover, for the triple interaction, there is far less variation in tenure, so the lack of an influence might be anticipated.

[6] Simultaneously allowing a full set of interactions of tenure with the year dummies greatly attenuates the results but, as we will show, even this saturated model returns similar results in several of our robustness checks.

Table 1.7. Estimation result of the impact of Implied contract law on Male Wage by Age group

Panel A. Age 20 to 30	(1)	(2)	Panel B. Age 31 to 65	(1)	(2)
Treatment window	3 years	4 years	Treatment window	3 years	4 years
Treat × Post	-0.0202	-0.0271	Treat × Post	-0.0317	-0.0312
t stat_pid	(-0.67)	(-0.98)	t stat_pid	(-2.11)**	(-2.12)**
t stat_State	(-0.53)	(-0.79)	t stat_State	(-2.25)**	(-1.96) *
Treat × Post × Tenure	0.0000648	0.00147	Treat × Post × Tenure	0.00179	0.00156
t stat_pid	(0.01)	(0.38)	t stat_pid	(2.21)**	(1.94)*
t stat_State	(0.02)	(0.46)	t stat_State	(2.86)***	(2.04)**
Tenure × State	N	N	Tenure × State	N	N
State/Indiv. FE	Y	Y	State/Indiv. FE	Y	Y
Triple interaction	Y	Y	Triple interaction	Y	Y
N	18053	19215	*N*	36554	41053
Panel E. Age 20 to 30	(1)	(2)	Panel F. Age 31 to 65	(1)	(2)
Treatment window	3 years	4 years	Treatment window	3 years	4 years
Treat × Post	-0.0263	-0.0323	Treat × Post	-0.0293	-0.0284
t stat_pid	(-0.86)	(-1.15)	t stat_pid	(-1.91) *	(-1.87) *
t stat_State	(-0.70)	(-0.95)	t stat_State	(-2.06) **	(-1.76) *
Treat × Post × Tenure	-0.0000889	0.00211	Treat × Post × Tenure	0.00165	0.00145
t stat_pid	(-0.02)	(0.54)	t stat_pid	(2.05) **	(1.82) *
t stat_State	(-0.03)	(0.63)	t stat_State	(2.59) **	(1.89) *
Tenure × State	Y	Y	Tenure × State	Y	Y
State/Indiv. FE	Y	Y	State/Indiv. FE	Y	Y
Triple interaction	Y	Y	Triple interaction	Y	Y
N	18053	19215	*N*	36554	41053

* p<0.10, ** p<0.05, *** p<0.01

Table 1.8 reports the estimation results by education level. As with the employment results, high school graduates without a college degree are the most relevant subgroup. Adopting the implied contract decreases the wage of the high school graduate by 4.9% and increases the return to job tenure by 0.34%. The wages of workers without college appear to be reduced substantially but with only 10 percent statistical significance given the state fixed effects. The change in return to tenure for workers without high school is essentially zero. As with employment, the wages of workers with a college degree are simply not responsive to the implied contract exception. In total the wage results confirm that the college educated do not seem sensitive to the employment-at-will exception.

Table 1.8. Estimation results of the impact of Implied contract law on Male Wage by Education

	(1)	(2)	(3)	(4)	(5)	(6)
Education	Less than 12		12 to 15		16 and over	
Treatment window	3 years	4 years	3 years	4 years	3 years	4 years
Treat × Post	-0.0601	-0.0565	-0.0487	-0.0489	-0.0137	-0.0164
t stat_pid	(-2.18) **	(-2.08) **	(-3.15) ***	(-3.24) ***	(-0.051)	(-0.061)
t stat_State	(-1.99) *	(-1.91) *	(-2.83) ***	(-2.56) **	(-0.051)	(-0.062)
Treat × Post × Tenure	0.00132	0.00127	0.00340	0.00336	0.00229	0.00287
t stat_pid	(0.91)	(0.87)	(3.57) ***	(3.64) ***	(1.44)	(1.82) *
t stat_State	(0.95)	(0.89)	(3.68) ***	(3.22) ***	(1.43)	(1.79) *
State/Indiv. FE	Y	Y	Y	Y	Y	Y
Triple interaction	Y	Y	Y	Y	Y	Y
N	11068	11808	32410	35888	11979	13532

* p<0.10, ** p<0.05, *** p<0.01

Table 1.9. Estimation result of the Impact of Implied Contract Law on Male Wage by Union Status

Panel A. Union	(1)	(2)	Panel B. Non-Union	(1)	(2)
Treatment window	3 years	4 years	Treatment window	3 years	4 years
Treat × Post	-0.00707	-0.0133	Treat × Post	-0.0438	-0.0374
t stat_pid	(-0.34)	(-0.64)	t stat_pid	(-3.12) ***	(-2.74) ***
t stat_State	(-0.44)	(-0.91)	t stat_State	(-2.74) ***	(-2.11) **
Treat × Post × Tenure	-0.0000940	0.000416	Treat × Post × Tenure	0.0030	0.00267
t stat_pid	(-0.08)	(0.37)	t stat_pid	(3.38) ***	(3.04) ***
t stat_State	(-0.11)	(0.53)	t stat_State	(3.15) ***	(2.22) **
Tenure × State	N	N	Tenure × State	N	N
State/Indiv. FE	Y	Y	State/Indiv. FE	Y	Y
Triple interaction	Y	Y	Triple interaction	Y	Y
N	14218	14941	*N*	38766	40851
	(1)	(2)	Panel F. Non-Union	(1)	(2)
Treatment window	3 years	4 years	Treatment window	3 years	4 years
Treat × Post	-0.0140	-0.0164	Treat × Post	-0.0427	-0.0357
t stat_pid	(-0.63)	(-0.73)	t stat_pid	(-2.98) ***	(-2.56) **
t stat_State	(-0.80)	(-0.98)	t stat_State	(-2.68) ***	(-2.03) **
Treat × Post × Tenure	0.0000375	0.000484	Treat × Post × Tenure	0.00306	0.00260
t stat_pid	(0.03)	(0.42)	t stat_pid	(3.42) ***	(2.97) ***
t stat_State	(0.04)	(0.59)	t stat_State	(3.20) ***	(2.15) **
Tenure × State	Y	Y	Tenure × State	Y	Y
State/Indiv. FE	Y	Y	State/Indiv. FE	Y	Y
Triple interaction	Y	Y	Triple interaction	Y	Y
N	14218	14941	*N*	38766	40851

* p<0.10, ** p<0.05, *** p<0.01

1.6. Potential Problems with the Roll-out Design

Even if our empirical approach does not follow a typical two-way fixed effect (TWFE), it shares features that can potentially create bias. Our analysis allows observations from a treated state to be in the control sample if their treatment is outside the treatment window. If there exists a heterogeneous treatment effect by time, this potentially yields a biased estimate. It remains true that our short panel dataset limits such bias by censoring the treatment observation within treatment windows, but the potential problem remains. Newly proposed estimation techniques remedy the issue with TWFE by eliminating comparisons with previously treated observations (Callaway and Sant'Anna (2021), Sun and Abraham (2021)). Thus, we re-estimate our short panel design using only the never treated as control group. This results in a potentially cleaner comparison between treated and untreated groups at the cost of sample size.

Panel A of Table 1.10 and Table 1.11 replicate our baseline male sample results for employment and wage presented in Tables 1.3 and 1.6. The point estimates closely mimic the original result with similar statistical significance, even if the sample size is close to one-third of the original estimation due to the loss of many control observations. [7] The adoption of the implied contract exception is estimated to reduce employment by 1.5 to 1.6% and wages by 3.2 to 4%. Also, additional job tenure is compensated 0.19 to 0.29% more after the adoption. These results support the original estimates with the full sample, and it is evident the estimates are not driven by the undesirable comparison embedded in a roll-out design.

[7] Subsample analysis with the never-treated control group also presents similar coinciding results with the original estimates using the full sample. The estimates largely mimic Table 1.4, 1.6 and 1.7.

Table 1.10. Estimation result of the impact of Implied contract law on Male Employment with Never Treated Control Group

Panel A. Employment	(1)	(2)
Treatment window	3 years	4 years
Treat × Post	-0.0150	-0.0155
t stat_pid	(-1.75)*	(-1.81)*
t stat_State	(-2.41)**	(-2.38)**
State/Indiv. FE	Y	Y
N	28263	36579
Panel B. Union	(1)	(2)
Treatment window	3 years	4 years
Treat × Post	-0.00358	0.00492
t stat_pid	(-0.17)	(-0.23)
t stat_data	(-0.16)	(-0.25)
t stat_State	(-0.25)	(-0.32)
State/Indiv. FE	Y	Y
N	6391	7980
Panel C. Non-Union	(1)	(2)
Treatment window	3 years	4 years
Treat × Post	-0.0188	-0.0191
t stat_pid	(-1.64)	(-1.55)
t stat_data	(-2.24) *	(-2.43) **
t stat_State	(-2.26) **	(-2.07) **
State/Indiv. FE	Y	Y
N	19098	23497

* p<0.10, ** p<0.05, *** p<0.01

Table 1.11. Estimation results of the impact of Implied contract law on Male Hourly wage with Never Treated Control Group

	(1)	(2)	(3)	(4)	(5)	(6)
Treatment window	3 years	4 years	3 years	4 years	3 years	4 years
Treat × Post	-0.0395	-0.0359	-0.0395	-0.0386	-0.0322	-0.0325
t stat_pid	(-2.60)***	(-2.48)**	(-2.59) ***	(-2.64) ***	(-1.89) *	(-1.97) **
t stat_State	(-2.20)**	(-2.06)**	(-2.19) **	(-2.24) **	(-1.91) *	(-1.81) *
Treat × Post × Tenure	0.00291	0.00256	0.00278	0.00257	0.00187	0.00195
t stat_pid	(2.96)***	(2.80)***	(2.92) ***	(2.85) ***	(1.47)	(1.61)
t stat_State	(3.21)***	(2.83)***	(3.01) ***	(2.84) ***	(2.00) *	(2.21) **
Tenure × State	**N**	**N**	**Y**	**Y**	**Y**	**Y**
Tenure × Year	**N**	**N**	N	N	**Y**	**Y**
State/Indiv. FE	Y	Y	Y	Y	Y	Y
Triple interaction	Y	Y	Y	Y	Y	Y
N	18284	23784	18284	23784	18284	23784

* p<0.10, ** p<0.05, *** p<0.01

Panel B and C of Table 1.10 present the employment implication of the implied contract by union status. In contrast to the wage implication by union status presented in Table 1.9, estimating the impact on employment by union status requires additional thought. Union members frequently give up membership upon becoming unemployed or leaving the labor force. It represents a substantial cost with no benefit. Therefore, the change in employment by union status is not easily tracked by simply examining the union status of the employed and unemployed. As an alternative, we identify workers with and without union membership a year before legal adoption. Another obstacle is constructing a consistent control sample with staggered adoption timing. Since the adoption timing is not unique, a control observation at a certain year can be both pre and post treatment observation relative to different treated states. To build a consistent control sample across adoption years, we construct a separate dataset for each adoption timing and aggregate those datasets by appending all of them. Though any not-treated observations can be included in each control sample, we limit the control sample to observations from the never-treated states for all three at-will exceptions by excluding not-yet-treated observations from states that will eventually be treated. Appending sub-datasets unavoidably duplicate control observations because observations can be included in multiple sub-datasets. Since the not yet treated observations are disproportionately larger for earlier years of the sample period, we exclude them to avoid the aggravating issue with the over representation.

While necessarily imperfect, the estimation result shows that the adoption of the implied contract has a statistically significant negative consequence only for non-union workers. On average, non-union workers are 1.9% more likely to lose their jobs within 3 to 4 years of the legal adoption. As union workers are typically covered by employment contracts and seen as a pre-existing exemption to employment at will, this seems sensible.

For additional validation of this result, efforts were made to yield a comparable result using the technique developed by Callaway and Sant'Anna (2021). Their methodology estimates an adoption cohort and time-specific average treatment effect, ATT(g, t), for all periods t and adoption cohorts g by using the canonical 2×2 design and the control group contains only the never treated (or not yet treated) observations.

$$ATT(g,t) = E\left(y_{i,t} - y_{i,g-1} \middle| Group_i = g\right) - E\left(y_{i,t} - y_{i,g-1} \middle| Group_i = Never\ Treated\right) \quad (5)$$

$$ATT(g,t) = E\left(y_{i,t} - y_{i,t-1} \middle| Group_i = g\right) - E\left(y_{i,t} - y_{i,t-1} \middle| Group_i = Never\ Treated\right) \quad (6)$$

As equation (5) shows, if $g \leq t$, the canonical model estimates the average treatment effect on treated at time t, relative to pre-treatment time (g-1). If the time t is the pre-treatment period (g>t), it estimates the ATT following equation (6), which should be insignificant in the absence of the pre-trend. The typical aggregation takes the weighted average of ATT(g, t) across desired dimensions with the weight based on the number of treated observation used for each 2×2 estimates.

Table 1.12. ATT on Employment, Aggregated by the Distance from Implementation Timing

	Coefficient	Std. err.	z	P>|z|	[95% Conf.	Interval]
Pre_avg	-0.00691	0.0027	-2.55	0.011	-0.0122	-0.0016
Post_avg	-0.01634	0.0071	-2.32	0.021	-0.0302	-0.0025
T-4	-0.02192	0.0066	-3.30	0.001	-0.0350	-0.0089
T-3	0.00038	0.0068	0.06	0.955	-0.0130	0.0137
T-2	-0.00558	0.0065	-0.85	0.393	-0.0184	0.0072
T-1	-0.00051	0.0067	-0.08	0.939	-0.0136	0.0126
T	-0.00338	0.0064	-0.53	0.598	-0.0160	0.0092
T+1	-0.01699	0.0074	-2.29	0.022	-0.0315	-0.0025
T+2	-0.01352	0.0088	-1.58	0.115	-0.0303	0.0033
T+3	-0.02462	0.0099	-2.48	0.013	-0.0441	-0.0052
T+4	-0.02320	0.0107	-2.17	0.030	-0.0442	-0.0022

Table 1.12 presents the averaged ATT(g, t) for each distance from the adoption timing. Given the distance $d = t - g$, the reported result shows the weighted average of ATT (g, t) for each value of d. Though it shows a statistically significant pre-treatment coefficient at d=-4, immediate pre-treatment periods do not show any significant pre-trend for the adoption on average. The average ATT for the post-treatment period is typically negative and often significant. Table 1.13 presents average ATT(g,t) for 3 and 4 years post-treatment window to

match the earlier work. It suggests that the employment to population ratio declines 1.8% within 3 years and 1.9% within 4 years of implied contract exception. These estimates are largely consistent with our earlier results, ranging from a 1.4 to 1.6% reduction in the employment-population ratio.

Figure 1.1. ATT on Employment, Aggregated by the Distance from Implementation Timing

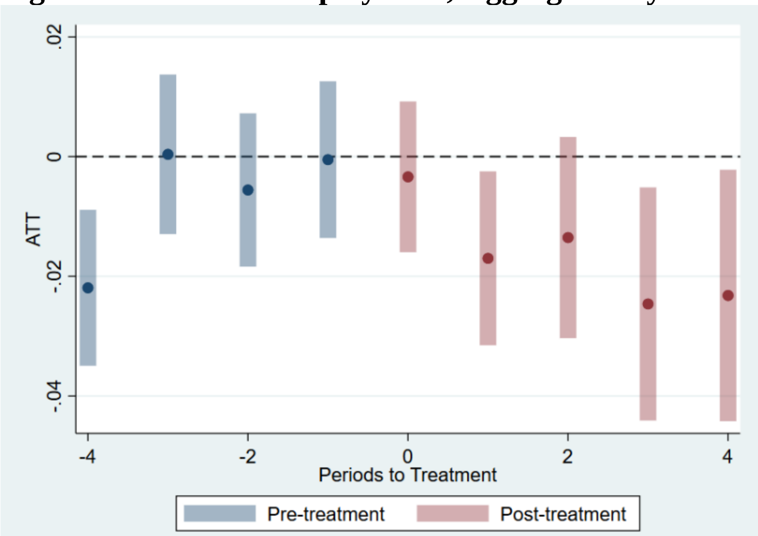

Table 1.13. Impact on Employment Aggregated within a post adoption interval

	Coefficient	Std. err.	z	P>|z|	[95% Conf.	Interval]
ATT(1,3)	-0.0183	0.007507	-2.43	0.015	-0.03298	-0.00355
ATT(1,4)	-0.0194	0.007799	-2.49	0.013	-0.03469	-0.00411

The Callaway and Sant'Anna's estimation procedure does not allow an approachable estimation of triple interaction terms, and a wage analysis without the triple interaction with tenure does not detect any significant impact on wages which also conforms with our earlier estimations.

Nonetheless, the combination of first using the never treated control and then adopting the Callaway and Sant' Anna approach argues that there remains a series of interesting and meaningful results. The implied contract exception seems to be influential.

1.7. The Pattern of Results Over Time

There are several reasons to anticipate that the influence of adoption may attenuate over the rollout period. This is a different point than that just covered involving the potential estimation issues associated with the rollout design. Instead, we anticipate that states later to adopt may see smaller influences than those that have adopted earlier. First, as emphasized employers are known to have responded to the exceptions by changing everything from hiring offers to employee handbooks to make clear that no contract was implied. While a relatively new strategy in the early 1980s, it was widely followed across by employers in virtually all states by the end of the rollout period. This was driven in part by large multi-state employers adopting standardized materials and procedures as the rollout period advanced.

This development suggests violations of the no-anticipation assumption and the stable unit treatment value assumption (SUTVA). The no-anticipation assumption requires one's future treatment plan does not affect the pre-treatment outcome, and SUTVA requires the outcome to depend only on its own treatment status. As Dertouzos et al. (1992) use the adoption rate of the surrounding state as a predictor of the adoption probability, adoption timing might well be anticipated. Moreover, the spillover of defensive measures into untreated states implies that the future treatment plan likely did influence the pre-treatment outcome. In short, the scope for the exception to influence labor economic variables could be reduced later in our window of observation.

In the previous section, we presented the result without the comparison between late treated and already treated group to avoid potential bias. Tables 1.12 and 1.13 continue to follow this approach. They present the employment and wage results by adoption cohorts to explore the possibility of declining influence in the role of the exception. Again, so as not to confuse the concern about adoption cohorts with that of the potential problems of the roll out design, we continue to use as the control the sample of never treated states in all estimates. We construct an early adoption cohort by identifying the observations that are treated by 1985. If an observation is in a state adopting the law after 1985, it is in the later adoption cohort. This division creates two relatively similar size cohorts with descriptive statistics that are also similar.

Table 1.14. Estimation result of the impact of Implied contract law on Male Employment by Adoption Cohort

Panel A 1980-1985	(1) Never Treated 3 years pre-post	(2) Never Treated 4 years pre-post	(3) Not yet treated 3 years pre-post	(4) Not yet treated 4 years pre-post
Treat × Post	-0.0251	-0.0265	-0.0166	-0.0165
t stat_pid	(-2.10)**	(-2.29)**	(-1.61)	(-1.57)
t stat_State	(-2.21)**	(-2.17)**	(-1.58)	(-1.49)
State/Indiv. FE	Y	Y	Y	Y
N	16764	22446	27309	33141
Panel B 1986-1992	**(1) Never Treated 3 years pre-post**	**(2) Never Treated 4 years pre-post**	**(3) Not yet treated 3 years pre-post**	**(4) Not yet treated 4 years pre-post**
Treat × Post	-0.0109	-0.0117	-0.0107	-0.0160
t stat_pid	(-0.76)	(-0.83)	(-0.77)	(-1.19)
t stat_State	(-1.13)	(-1.09)	(-0.85)	(-1.14)
State/Indiv. FE	Y	Y	Y	Y
N	10918	13984	13113	16482

* p<0.10, ** p<0.05, *** p<0.01

It seems clear that the employment results differ by cohort. Table 1.14 shows a 2.5 to 2.7% reduction in the employment-population ratio for early adoption cohorts when the control group is the never-treated observation. This is substantially larger than the influences estimated in Table 1.10 for the entire never treated sample. Importantly, the later adoption cohorts do not respond significantly to the implied contract exception. The coefficients are less than half the size of the earlier cohort and are far from significance. Results with the not-yet treated control group fail to show a definite distinction between adoption cohorts, even if the point estimate suggests a moderated difference. It is likely that the already treated states have more in common with the not-yet treated states relative to the never treated states.

Table 1.15. Estimation results of the impact of Implied contract law on Male Hourly Wage by Adoption Cohort

Never Treated Control Sample: Panels A, B						
Panel A. 1980-1985	(1)	(2)	(3)	(4)	(5)	(6)
Treatment window	3 years	4 years	3 years	4 years	3 years	4 years
Treat × Post	-0.0588	-0.0498	-0.0558	-0.0495	-0.0547	-0.0475
t stat_pid	(-2.88)***	(-2.60)***	(-2.71)***	(-2.56)***	(-2.31)**	(-2.15)**
t stat_State	(-2.17)**	(-1.97)*	(-1.98)*	(-1.92)*	(-2.27)**	(-1.86)*
Treat × Post × Tenure	0.00305	0.00280	0.00291	0.00275	0.00282	0.00263
t stat_pid	(2.41)**	(2.51)**	(2.41)**	(2.53)**	(1.80)*	(1.79)*
t stat_State	(2.53)**	(2.71)**	(2.37)**	(2.78)***	(2.13)**	(2.65)**
Tenure × State	**N**	**N**	**Y**	**Y**	**Y**	**Y**
Tenure × Year	**N**	**N**	N	N	**Y**	**Y**
N	11172	15016	11172	15016	11172	15016
Panel B. 1986-1992	(1)	(2)	(3)	(4)	(5)	(6)
Treatment window	3 years	4 years	3 years	4 years	3 years	4 years
Treat × Post	-0.0131	-0.0104	-0.0147	-0.0125	0.000517	-0.00577
t stat_pid	(-0.43)	(-0.36)	(-0.48)	(-0.43)	(0.01)	(-0.17)
t stat_State	(-0.54)	(-0.38)	(-0.60)	(-0.45)	(0.03)	(-0.26)
Treat × Post × Tenure	0.0000635	-0.000670	0.00000616	-0.000725	-0.00193	-0.00154
t stat_pid	(0.04)	(-0.37)	(0.00)	(-0.40)	(-0.73)	(-0.60)
t stat_State	(0.05)	(-0.48)	(0.00)	(-0.51)	(-0.67)	(-0.78)
Tenure × State	**N**	**N**	**Y**	**Y**	**Y**	**Y**
Tenure × Year	**N**	**N**	N	N	**Y**	**Y**
N	6850	8730	6850	8730	6850	8730
Not Yet Treated Control Sample: Panels C, D						
Panel C. 1980-1985	(1)	(2)	(3)	(4)	(5)	(6)
Treatment window	3 years	4 years	3 years	4 years	3 years	4 years
Treat × Post	-0.0413	-0.0445	-0.0333	-0.0391	-0.0246	-0.0278
t stat_pid	(-2.20)**	(-2.37)**	(-1.75)*	(-2.08)**	(-1.11)	(-1.26)
t stat_State	(-1.37)	(-1.55)	(-1.09)	(-1.41)	(-0.81)	(-0.97)
Treat × Post × Tenure	0.00325	0.00301	0.00310	0.00304	0.00203	0.00176
t stat_pid	(3.17)***	(2.98)***	(2.87)***	(2.94)***	(1.35)	(1.20)
t stat_State	(3.14)***	(3.13)***	(2.84)***	(3.24)***	(1.37)	(1.75)*
Tenure × State	**N**	**N**	**Y**	**Y**	**Y**	**Y**
Tenure × Year	**N**	**N**	N	N	**Y**	**Y**
N	18128	21953	18128	21953	18128	21953
Panel D. 1986-1992	(1)	(2)	(3)	(4)	(5)	(6)
Treatment window	3 years	4 years	3 years	4 years	3 years	4 years
Treat × Post	-0.00417	-0.00650	-0.00421	-0.00610	0.0141	0.00370
t stat_pid	(-0.15)	(-0.23)	(-0.15)	(-0.22)	(0.43)	(0.11)
t stat_State	(-0.16)	(-0.23)	(-0.16)	(-0.21)	(0.60)	(0.13)
Treat × Post × Tenure	-0.000235	-0.000877	-0.000417	-0.000911	-0.00276	-0.00216
t stat_pid	(-0.14)	(-0.51)	(-0.24)	(-0.51)	(-1.17)	(-0.92)
t stat_State	(-0.18)	(-0.69)	(-0.32)	(-0.72)	(-1.20)	(-1.08)
Tenure × State	**N**	**N**	**Y**	**Y**	**Y**	**Y**
Tenure × Year	**N**	**N**	N	N	**Y**	**Y**
N	8164	10208	8164	10208	8164	10208

* p<0.10, ** p<0.05, *** p<0.01

The wage responses also seem to be driven by early adoption states. Table 1.15 shows a noticeable difference between early and later adoption cohorts. The adoption of implied contract is estimated to reduce the wage by 4.1 to 5.9% and to increase the coefficient on an additional year of job tenure by 0.28 to 0.33%. In the later adoption cohort, the main treatment effect and the triple interaction with tenure are statistically insignificant and only a small fraction of the size estimated for the early cohort. In the case of wages both never-treated and not-yet-treated control groups yield a similar comparison confirming that the earlier results persist only in the early adoption cohort.

Importantly, including a full set of additional tenure interactions with each state and each year yields a similar pattern to the result shown in Table 1.6, but the estimation result tends to be more stable with the never-treated control samples.

Table 16. Estimation results of the Impact of Implied Contract on Employment periods by periods for Early Adoption cohort (1980-1985)

Panel A: 2years window								
Pre	T-1, T-2	T-1, T-2	T-1, T-2	T-1, T-2	T-1, T-2	T-1, T-2	T-1, T-2	T-1, T-2
Post	T+1, T+2	T+2, T+3	T+3, T+4	T+4, T+5	T+5, T+6	T+6, T+7	T+7, T+8	T+8, T+9
Treat × Post	-0.0250	-0.0189	-0.0268	-0.0120	-0.0109	-0.0152	-0.0136	-0.0116
t stat_pid	(-1.96) **	(-1.21)	(-1.41)	(-0.59)	(-0.47)	(-0.61)	(-0.52)	(-0.44)
t stat_State	(-2.15) **	(-1.63)	(-1.70) *	(-0.67)	(-0.54)	(-0.62)	(-0.58)	(-0.71)
N	11821	12191	13261	14279	15216	15899	15861	15785
Panel B: 1 year window								
Pre	T-1	T-1	T-1	T-1	T-1	T-1	T-1	T-1
Post	T+1	T+2	T+3	T+4	T+5	T+6	T+7	T+8
Treat × Post	-0.0296	-0.0240	-0.0283	-0.0183	-0.0150	-0.0093	-0.0117	0.0024
t stat_pid	(-2.01) **	(-1.32)	(-1.30)	(-0.76)	(-0.58)	(-0.33)	(-0.39)	(-0.08)
t stat_State	(-2.07) **	(-2.25) **	(-1.76) *	(-0.80)	(-0.74)	(-0.36)	(-0.36)	(-0.11)
N	6732	7083	7426	8474	9157	9796	10253	10101

* p<0.10, ** p<0.05, *** p<0.01

Table 1.16 presents the period-by-period employment comparison results for early adoption cohorts. We use shorter treatment windows for this analysis because averaging over longer periods can dilute the pattern of the outcome evolution. The estimation result shows diminishing employment impact over time, and the pattern becomes more noticeable with the year-to-year comparison. Overall, the impact tends to be persistent up to 4 years after the adoption and gradually diminishes in the later periods.[8]

This difference between cohorts supports the "on-the-ground" reports that employers reacted to the initial exceptions. As mentioned, it became common to rewrite employee manuals to make sure employment agreements made explicit that no contract was implies and to teach managers to avoid any language that would imply anything to the contrary in all verbal and written communications including evaluations. Moreover, as suggested, firms that crossed state borders made such practices common in both states with and without exceptions. Consequently, we take the results by adoption cohort to suggest that while early adopters may have responded with changes in employment and wages, later adopters anticipated the exception and so did not make such responses. Moreover, the suggestion that even among the early cohort the results may diminish over time suggests that similar reactions may have happened among the early adopters. Increasingly, manuals, language and communications were changed to avoid any implication of a contract and the effects on employment diminished.

1.8. Robustness Exercises

For the sake of completeness, we contrast the results using the preferred timing and the implied contract exception with other timings and exceptions. Results tables for these robustness exercises are presented in chapter 1 appendix. Tables A1.1 to A1.4 present the estimation results replicating Tables 1.3 and 1.5 focusing on the public policy and covenant of good faith exceptions. They use the adoption timing of those exceptions developed by Autor et al. (2006) as did our examination of the implied contract exception.

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[8] A similar approach for wage estimation does not show the diminishing pattern. It suggests that employment consequences are likely to be a temporary layoff or hiring freeze while the impact on wages tends to be more persistent.

The court's recognition of the public policy exception varies by state, and many states narrowly limit the public policy exception to protect employees against termination for failure to engage in illegal activity explicitly violating the constitution and statutes. The narrow recognition of the exception maybe be reflected in the fact that Autor (2006) found no employment consequences. Tables A1.1 and A1.2 show the estimated impact of the public policy exceptions on employment and hourly wage. Despite our different methodology and focus on individual data, the impact on employment is close to zero and statistically insignificant across samples and employment outcomes. The impact on wage is also statistically insignificant, and the point estimates are smaller than those associated with the implied contract exception.

The covenant of good faith exception has a wide range of potential applicability as it nominally requires employers to be fair in their employment relationships. However, the actual application of the exception has typically been limited to only cases where the employer deliberately dismisses workers to avoid a promised employment benefit or payments. Also, interestingly, the least number of states adopted the good faith exception. Our estimation results, presented in Tables A1.2 and A1.4, reflect this limited applicability and again confirms with individual data the finding of Autor et al (2006). The point estimates are without statistical significance and often take the opposite sign of those estimated when focusing on the implied contract exception.

Tables A1.5 to A1.8 present estimates using the alternative legal timing developed by Walsh (1996) and Dertouzos et al. (1992). As emphasized, the adoption of three at-will exceptions is identified by analyzing the relevant court cases and the researcher's judgment can greatly affect the outcome. Walsh and Detouzos's legal timings are modestly different from Autor et al. (2006). Indeed, the adoption timing is within 3 years for approximately two-thirds of the cases. As our only statistically significant results come from the implied contract exception, and as it is generally recognized to be the most telling exception, we limit our use of these two alternative timings to that exception.

Despite the modest differences in timing, the alternatives yield different results in our short panel approach. Tables A1.5 and A1.6 use the two alternative timings to examine employment influences and find none. Tables A1.7 and A1.8 estimate wage influences and the coefficients are mostly close to zero, can take the opposite sign and routinely lack of statistical significance.

Tables A1.9 and A1.10 provide the state-by-state estimation results for both wage and employment outcomes using the never-treated control group. The results are organized chronologically. While they show heterogeneity, they detail the summary provided in the cohort adoption estimates. The earlier estimates are more likely to be negative and significant than are those that come later.

Table A1.12 presents the results on an increasingly narrow set of occupations. Categories classifying occupations and industries are based on the 1970 Census classification and are listed in Table A1.11. Analysis results within each occupation code are mostly insignificant and unstable. Thus, Table 1.12 undertakes an exercise to identify the groups of occupations in which our results are concentrated. In panels A and B of Table A1.12, we exclude professional, sales, laborer, and farm-related occupations. Service workers are further removed from the sample in panels C and D, and Panels E and F exclude the transportation category to collect occupations associated with larger companies with relatively smaller performance payments. Thus, in the end Table A1.12 estimates the results on only four broad occupations: Administrators and Managers Except Farm, Clerical and Kindred Workers, Craftsmen and Kindred Workers, and

Operatives (non-transport). Despite a much smaller sample, the pattern of sizes and significance remain with evidence that this group of workers face both employment and wage consequences from the at-will exception. Importantly, the complement (all occupations other than these four) does not show any significant consequences from the at will exceptions.

A similar narrowing by industry does not generate a similar concentration of results (See Table A1.13). The results suggesting an occupation-specific impact coincide with our subsample analysis performed by age and education. The occupations identified tend to have the age and education levels that we found as critical.

Table A1.14 presents the impact of implied contract on employment and wage variability. The measures of variability are the employment and wage variability are the squared and absolute deviation from the individual mean. A positive coefficient implies an increase in variability and, one might speculate, adjustment activities associated with the new exception. As the outcome variable for employment is binary coded by 0 and 1, the estimation result for the employment variability with the individual fixed effect is identical between the squared deviation and the absolute deviation. The estimation results presented in panels A and B show the estimated coefficients are consistently positive but are typically statistically insignificant. Panels C to F present the impact of legal adoption on wage variability. Legal adoption is associated with increased wage variability, while job tenure tends to attenuate the impact. These results hint that a consequence of the new exception is increased adjustment by firms and workers. As such, they remain consistent with our main results showing negative impacts of the adoption on employment and wages as well as job tenure compensating for the negative wage impact.

These robustness exercises confirm that the labor market influences that we have reported on male employment and on earnings are unique to the Autor timing and to the implied contract exception. Moreover, while we clearly find stronger results among the earlier adopters using Autor's timing, no significant results could be found using earlier cohorts in the results presented using alternative timings.

1.9. Conclusions

In this article, we revisit the impact of exceptions to the employment at will doctrine widely adopted across the U.S. from the late 70's to the early 90's. We are the first to combine individual level data extracted from PSID and the difference in difference estimation strategy. The exceptions have been thought to protect incumbent workers by increasing the separation cost for the firm. Yet, previous theoretical predictions have been in conflict. An increase in separation cost may suppress both the dismissal of incumbents and the hiring of entrants with an uncertain net prediction on employment. Similarly, that same increase in separation costs may decrease wages to reflect decreased hiring benefits to the firms or it increase wages by providing greater bargaining power or the incentive to accumulate human capital. The empirical literature does find routine and convincing evidence of labor market consequences.

We primarily use the legal timing developed in Autor et al. (2006) and focus on the implied contract exception. We use individual data to control for worker fixed effects, to control for variables not typically available in previous aggregate studies and to vary the heterogeneity examination by some of those characteristics.   The probability of employment for the male sample declines from 1.4 to 1.5 percent. The impact on employment is concentrated on the younger worker group aged from 20 to 30. The stronger impact on younger workers implies that hiring reductions focus on those with the longest expected tenure. The results also suggest that workers with high school diplomas without a college degree are the group facing the consequences of the exception. Finally, the impact of the legal adoption is not detected for the union workers while non-union employment behaves in a similar way to our main analysis.

We deviate from previous simple examinations of earnings to include the role of tenure with the firm. The estimated impact on wage for the male sample shows a 4.2 percent reduction in wage but with an additional return to tenure of 0.26 percent. Thus, the additional return to tenure can eliminate the negative impact on wages if the job tenure is around 16 years. The estimation result by age group suggests that the impact is concentrated within the old workers and workers without college degree. When we separate the union and non-union workers, the wage of union workers is much less responsive to the legal adoption, potentially due to the explicit labor contract established by collective bargaining.

Given the multiple adoption timings across states, we face the problems associated with "rollout designs." The empirical strategy is exposed to the potential bias stemming from comparing recently treaded workers to a combination of not treated and long-ago treated workers. To address this issue, we estimated our result using only never treated observations which looked very similar. Though the short panel estimation setup could not be perfectly matched, we also utilized the strategy developed by Callaway and Sant'Anna (2021). While limited to the employment results, the timing-by-timing result and the aggregate averaged within post-treatment periods, yield very similar estimates to our main result.

Given employers operating in many states and potential spillovers across states, later adoption may be associated with anticipation. especially for the later adoption cohort. We separate early and later adoption cohorts. The overall results are much stronger and larger for early adoption cohorts. In addition, there is at least modest evidence that the influence of adoption even in the early cohort may not persist long-term. The combination of anticipation and adaptation suggests and important but temporary influence.

Further robustness exercises are performed using other at will exceptions and alternative legal timings. Like Autor et al. (2006), the public policy and the covenant of good faith exceptions do not yield a significant impact. Alternative legal timings also fail to identify significant impacts on wage or employment. The impact of at will exceptions in the U.S. is relevant only to the implied contract exceptions with the timings of Autor et al. (2006).

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Chapter 2: The Local Employment Effect of House Prices: Evidence from U.S. States

2.1. Introduction

Housing is one of the most significant household assets, and the housing price naturally plays a key role in determining household consumption. Household consumption is repeatedly reported to be more responsive to housing wealth relative to financial wealth, and it also determines the borrowing capacity of the household under liquidity constraints. Through these wealth and collateral channels, the housing market plays a key role in determining local demand and local employment.

The housing market is the central component of the Great Recession from 2007 to 2009, and the accompanying large decline in employment arouses attention to the implication of the housing net worth channel on employment. Mian et al. (2013) identified a higher average marginal propensity to consume for the poorer and more levered locations which are in line with the locations experiencing a larger collapse in the housing market. Mian and Sufi (2014) examine the local-level linkage between the local house price and employment by separating the impact on tradable and non-tradable employment. Employment in sectors producing non-tradable output relies more heavily on local demand relative to the tradable sectors. Therefore, the local demand suppressed by a decline in local housing prices asymmetrically impacts employment in non-tradable sectors. By exploiting the county-level data between 2007 and 2009, they found a relatively larger reduction in employment associated with the decline in housing net worth for the non-tradable sector.

Given the reduction in consumer demand through the housing net worth channel, Giroud and Mueller (2017) reveal the firm-level characteristics that induce greater employment consequences in response to the demand shock stemming from changes in local house prices.

Using the establishment level data in the non-tradable sector and ZIP code and county level house price, they find that more highly levered firms show a larger reduction in employment, and counties with highly levered firms tend to experience greater job loss within the non-tradable sector. This result suggests a disproportionately stronger employment adjustment in the non-tradable sector against a local house price shock more pervasive to highly levered firms. Using the data before the Great Recession, Adelino et al. (2015) report an increase in employment within small firms induced by rising house prices and home-equity-based borrowing. Since small firms are more likely to rely on local demand relative to large companies, this result also suggests a greater non-tradable employment response to local housing prices.

The cross-sectional studies focused on the Great Recession have a limited ability to explain the dynamics of a shock in the housing market on other economic indicators. Since the house is one of the biggest parts of household wealth, house price becomes a significant driver for the business cycle. When the housing price cycle turns, the housing market frictions, including the transaction costs, taxes, or the specific structure of the mortgage market, can delay adjustment or collapse in the housing market. Also, the supply in the housing market can take a substantial amount of time and may not be responsive to a demand shock due to relevant policies or geographical conditions in the area. Given the significant influence of the housing market on the macroeconomy and various frictions affecting the magnitude and timing of such influence, our investigation of the relationship between employment and the housing market must be performed in a dynamic framework.

This paper contributes to the current literature by examining the impact of the housing market on tradable and non-tradable employment with the dynamic framework. It also will be based on a newly developed state-level employment time series in both tradable and non-tradable sectors in addition to the time series of house prices, personal income, and building permits by states. The data covers from 2001:M1 to 2019:M12 across 45 U.S. states with monthly frequency. Specifically, our analysis aims to verify the asymmetric consequence of the shock in the local housing market on tradable and non-tradable employment and to identify the persistency of the influence of the housing market on both types of employment. It will also reveal how those dynamic impacts are different from the impact of other potential endogenous variables; income and building permits. Firstly, we utilize a panel vector autoregression model (PVAR) to investigate the magnitude and the persistence of house price shocks on employment. Though the panel structure of the data allows us to account for the state-by-state heterogeneity while estimating the average impact across states, a state-level analysis is also important to validate the result and to study the aspects of heterogeneity. The second step of our analysis compares the out-of-sample forecasting performance of house prices for tradable and non-tradable employment by using a state-by-state approach. This will verify the asymmetric relevance of house prices on tradable and non-tradable employment while accounting for the possible heterogeneity across states.

To compare the impact of house prices on tradable and non-tradable employment, the identical setup of the PVAR model is estimated by alternating the employment variable. The model includes the state-fixed effect to account for the state-level heterogeneity and other unobservable characteristics, and four main variables are log-differenced for the purpose of detrending and the variable stationarity. The PVAR estimation shows the relationship between our variables under the dynamic framework, and the impulse response function would reveal the magnitude and persistency of the unexpected shock to house price, income, and building permit on two different types of employment. For the second approach, we measure the forecasting performance of the model by the root mean squared error, and it is compared between the autoregressive (AR) model and the vector autoregressive (VAR) model with house price. In the case that house price has a predictive power on employment, the VAR model with house price will outperform the AR model. Also, if the house price is more relevant to non-tradable employment, the performance gap between the AR and VAR models will be more significant for non-tradable employment. To verify that the housing net worth channel is the source of the observed difference in forecasting performance, the same set of forecasting exercises is performed by using the real personal income and the building permit. Lastly, we examine the housing supply elasticity and price volatility to investigate the relevance between those institutional characteristics and the predictability difference across tradable and non-tradable employment.

Within the model, the result of the PVAR model estimation suggests that the employment consequence of the house price shock is more significant and persistent compared to the personal income and the building permits. Point estimates for the income and building permits are often statistically insignificant and display very short-lived impulse responses. The comparison between PVAR models shows a coinciding result with Mian and Sufi (2014). House prices display a stronger and more persistent impact on non-tradable employment than tradable employment in terms of point estimates and impulse response. The result of the second approach confirms that the above tendencies are preserved for the state-by-state forecasting performance comparison. For all forecasting horizons and the majority of states, including house prices, brings a superior improvement in the forecasting performance for non-tradable employment. The forecasted VAR model using non-tradable employment outperforms its AR benchmark while the same model with tradable employment presents a similar forecasting performance to its benchmark. The forecasting result with the personal income and building permits does not show a noticeable difference in forecasting performance across the employment type. Also, the forecasting performance displays a significant heterogeneity across states for the model with house price while the income and building permit yield more uniform forecasting performance across states. Given the greater predictability of house prices for non-tradable employment, the gap between tradable and non-tradable is greater for states with less elastic housing supply and greater housing price volatility. This reaffirms that the house price carries information more relevant to non-tradable employment likely through the housing net worth channel.

The remainder of the paper proceeds as follows. Section 2 presents a literature review, and section 3 provides a description of the dataset. Section 4 presents the estimation results of the PVAR model and compares the forecasting performance. Finally, section 5 concludes.

2.2. Literature Review

There is a vast literature in regional economics that attempts to explain the movements in statelevel employment and regional disparities in labor market conditions. Some of the key drivers of regional employment include vacancy rate (Beveridge curve), national unemployment rate (cyclical sensitivity model), migration (migration-based models), region-specific non-accelerating inflation rates of unemployment (Phillips curve-type set-up), one or more lags of employment itself, housing market conditions, among others (Holzer 1993; Pehkonen and Tervo 1998; Layard et al. 2005; Payne 1995; Kerr et al. 1998; Mian et al. 2013; Vega and Elhorst 2016). Elhorst (2003) provides a good comprehensive theoretical and empirical overview of regional unemployment differentials.

One of the topics in the related macroeconomics literature that has attracted significant attention, especially after the recent financial crisis, is the nexus between housing markets and employment conditions. A deteriorating housing market conditions, for example, negatively affect the household's balance sheet. This, in turn, reduces consumer demand either through the wealth channel or tighter borrowing constraints due to falling collateral value of housing.[9] In either case, the drop in demand gets reflected in lower employment.

More importantly, the literature that studies the labor market conditions across states does not distinguish employment based on the source of demand (local or national) in different industries, even while improvements in employment forecasts are particularly evident during recessions (Rapach and Strauss 2009; Barnichon et al. 2012). The distinction between tradable and non-tradable components of employment has proved to be crucial as shown by Mian and Sufi (2014). They use a cross-sectional approach and show that the effects of housing markets on employment are significant and they are related to the nature of employment. The use of time-series data allows us to look at the relationship between housing market and labor market conditions for a much longer sample period than the period of housing bust studied by Mian and Sufi (2014). Although the housing bust period is of significant interest, it is equally, if not more, important to examine the impact of the housing market boom that preceded the bust.

We build on the work of Mian and Sufi (2014) and extend the analysis of the impact of house prices on tradable and non-tradable employment in a dynamic setting. For this purpose, we first perform a PVAR analysis. Following the contribution by Holtz-Eakin et al. (1988), PVAR models have become a standard tool for analyzing multivariate time-series in a panel context.

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9 Several studies find that the housing market's wealth effects are bigger than the financial asset's wealth effect (Case et al. 2005; Hurst and Stafford 2004; Kishor 2007; Campbell and Cocco 2007; Cooper 2009; Kermani 2012). Studies have also explored the importance of collateral lending channel and find that small businesses in areas with greater increases in house prices experienced stronger growth in employment than large firms in the same areas and industries (Adelino et al. 2015).

One of the strengths of PVAR models is that multiple variables can be simultaneously treated as endogenous. PVAR model allows us to examine the persistence of different types of shocks on tradable (non-tradable) employment growth. In the second step of our analysis, we examine the forecasting performance of house price for different types of employment. It is important to note here that the existing literature on the housing-employment nexus does not provide any evidence on the predictive ability of house prices for future movement in jobs growth for different states. Most of the existing studies are largely limited to in-sample analysis of the relationship between the housing markets and employment conditions. These studies do not provide any guidance on out-of-sample predictive ability of different variables. For example, in forecasting U.S. state-level employment growth with 11 potential predictors, including house prices, Rapach and Strauss (2012) show that improved out-of-sample forecasts can be obtained by amalgamating different econometric approaches.[10] This implies that the in-sample approach likely provides only a partial understanding of the impact of housing markets on employment and a more holistic understanding would emerge from testing out-of-sample forecasting connections. Following the insights from the existing literature, we also test the out-of-sample forecasting power of personal income and building permits for tradable employment and non-tradable employment. The examination of the predictive power of these variables will also provide a test for the local demand channel that postulates a differential impact of house prices on non-tradable employment as compared to tradable employment. Unlike house price growth, changes in real personal

10 Similarly, in the case of housing markets, the in-sample analysis reveals that several factors like income, interest rate, and industrial production share a common trend with house prices (Holly and Jones 1997; Englund and Ioannides 1997; Egert and Mihaljek´ 2007; Adams and Fuss¨ 2010; Kishor and Marfatia 2017). However, in out-of-sample forecasting OECD house prices, among several macroeconomic variables, interest rates hold the most significant forecasting power (Kishor and Marfatia 2018). Rapach and Strauss (2009) also highlight the important differences in the forecastability of real house price growth across U.S. states.

income growth and building permits are not directly associated with changes in net worth, and hence may not generate the local demand channel arising out of changes in household balance sheets.

2.3. Data Description

In this study, we use monthly data on tradable and non-tradable employment, and house prices for 45 U.S. states for a sample period that runs from 2001:M1 to 2017:M12.[11] The employment data is sourced from Current Employment Statistics (CES)[12]. This data is available at a monthly frequency for two-digit North American Industry Classification System or NAICS industries.[13] In particular, we consider total employment in the private sector for all 19 two-digit industries. Following Mian and Sufi (2014), each industry is categorized as tradable or non-tradable industry. Table 2.1 reports the industry classifications used in this paper.[14] The state-level house price data is obtained from Freddie Mac, and is also available at the Federal Reserve Bank of St. Louis' FRED database. House price data is for all transactions and are not seasonally

11 We do not include Delaware, Hawaii, Maine, Rhode Island, and Wyoming in our sample because of data availability issues.

12 The CES is built on the back of the Quarterly Census of Employment and Wages (QCEW), pulling its survey sample from the QCEW universe, and benchmarking to QCEW every year. The primary difference between the two is that the surveybased estimates of CES allow for more timely data releases, with estimates published less than a month after the reference period, while QCEW's population data allow for much more granular geographic and industry data.

13 Disaggregated time-series data at state level is available only at 2-digit level.

14 Mian and Sufi (2014) use two different methods to classify industries. Based on "retail and world trade" classification, industries are categorized as tradable, non-tradable, construction, and others. An industry is tradable if total exports plus imports are greater than \$500M. Non-tradable industries are the retail sector and restaurants. Construction industries include construction, real estate, and land development, and the remainder industries are categorized as other. The second method - "Geographical Concentration" classification - is based on geographical concentration. Mian and Sufi (2014) argue

adjusted. We seasonally adjust the data using Census X-12 methodology before performing the analysis.[13] To get real house prices for each state, we deflate nominal house prices by PCE price index. Our real house price growth measure is monthly percentage change and is annualized. PCE price index is also available from the FRED database.

Table 2.1. Classification of Industries

All Industries	Tradable	Non-tradable
11-Agriculture,forestry,fishing	✓	
21-Mining,quarrying and oil and gas	✓	
22-Utilities		✓
23-Construction		✓
31-33-Manufacturing	✓	
42-Wholesale trade	✓	
44-45- Retail trade		✓
48-49- Transportation and warehousing		✓
51-Information	✓	
52-Finance and insurance	✓	
53-Real state-rental and leasing		✓
54-Professional and technical services	✓	
56-Administrative and waste services		✓
61-Education services	✓	
71-Art,Entertainment recreation	✓	
72-Accommodation and food services		✓

The table reports classification of tradable and non-tradable industries.

Apart from house prices, another proxy of housing market conditions is new building permits. Several studies highlight the role of building permits in driving economic activity across the U.S. (Leamer 2007; Ghent and Owyang 2010; Strauss 2013). Strauss (2013), for example, finds that building permits which are closely related to consumer expectations significantly lead economic activity in all the U.S. states. Since economic activity is linked with consumers' income, this implies a possible linkage of employment conditions with income and building permits. Personal income data, as well as new private housing units authorized by building permits data are obtained from the FRED database. Real income is calculated by using PCE index as deflator. All variables are in monthly growth rates and are annualized.

Table 2.2. Correlation Between Employment Cycle and House Price Cycle

State	Tradable	Non-tradable	P-value	State	Tradable	Non-tradable	P-Value
Alabama	0.6066	0.4194	0.00	Nebraska	-0.1571	0.0622	0.00
Alaska	0.0913	0.1240	0.57	Nevada	0.6899	0.7629	0.00
Arizona	0.3934	0.5282	0.04	New Hampshire	-0.0015	0.3058	0.00
Arkansas	0.3828	0.4694	0.01	New Jersey	0.1445	0.2225	0.04
California	0.4124	0.5893	0.00	New Mexico	0.5243	0.6089	0.00
Colorado	0.1271	0.3395	0.00	New York	0.0635	0.0991	0.33
Connecticut	0.0547	0.2720	0.00	North Carolina	0.5519	0.5623	0.51
Florida	0.7132	0.8567	0.00	North Dakota	0.2089	0.4450	0.00
Georgia	0.5655	0.6194	0.00	Ohio	0.0266	0.1195	0.00
Idaho	0.7886	0.8174	0.01	Oklahoma	-0.0344	0.1650	0.00
Illinois	0.0079	0.4635	0.00	Oregon	0.6553	0.7287	0.00
Indiana	0.1742	0.1707	0.86	Pennsylvania	0.0914	0.1971	0.00
Kansas	0.0211	0.0345	0.69	South Carolina	0.4532	0.5697	0.00
Kentucky	0.1624	0.2324	0.01	South Dakota	0.2088	0.1701	0.4
Louisiana	0.0277	0.1300	0.00	Tennessee	0.5229	0.5008	0.13
lowa	0.0133	0.2061	0.00	Texas	0.1836	0.4176	0.00
Maryland	0.4116	0.5153	0.00	Utah	0.8111	0.8217	0.34
Massachusetts	-0.3558	0.0413	0.00	Vermont	0.2936	0.2392	0.28
Michigan	0.5538	0.5452	0.61	Virginia	0.3492	0.4479	0.00
Minnesota	0.1969	0.3446	0.00	Washington	0.2174	0.7629	0.00
Mississippi	0.5568	0.4804	0.00	West Virginia	0.2699	0.2919	0.56
Missouri	0.2614	0.3461	0.00	Wisconsin	0.0924	0.3108	0.00
Montana	0.5968	0.6127	0.49				

This table presents the correlation between the cycle of non-tradable employment and house prices as well the correlation between tradable employment cycles and house price cycles. To get the cycles, we use Hodrick Prescott filter. P-values from the correlation comparison test of Meng et al. (1992) are reported in the last column of each panel.

To get an overview of the in-sample relationship between house prices and the disaggregated employment, we first explore the cyclical correlation patterns between these variables by decomposing them into a trend and a cycle using the Hodrick-Prescott filter. We decompose the logarithm of the level of employment and level of real house price using the default monthly smoothing parameter $\lambda = 14400$. Table 2.2 reports the correlation between the house price cycle and the cycle of both tradable and non-tradable employment. Results clearly show a strong positive correlation between non-tradable employment and house price cycle for most of the states. The correlation between employment cycles and house price cycle, on average, is 0.40 for non-tradable employment and 0.29 for tradable employment. The last column in Table 2.2 shows p-values from Meng et al. (1992) correlation comparison test. The results do suggest significant differences in correlation for most of the states. It is also worth highlighting the significant heterogeneity present in the correlation patterns between house prices and non-tradable employment cycle, with the lowest correlation in the case of Massachusetts (0.04) and highest in the case of Florida (0.86). For almost all the states, non-tradable employment cycle has a higher correlation with house price cycle as compared to tradable employment cycle providing preliminary evidence on the local demand channel hypothesis.

2.4. Empirical Analysis

2.4.1 Panel VAR Analysis

Given the correlation analysis that house price is more strongly correlated with non-tradable employment, we attempt to formulate a more comprehensive result using the PVAR model. After Holtz-Eakin et al. (1988) introduce the standard estimating and testing procedure, it has been used as a standard tool for multivariate time-series analysis incorporating panel data. Our PVAR model is constructed by using 4 endogenous variables including employment, income, building permit, and house price. The identical analysis will be duplicated by alternating employment variables between tradable and non-tradable employment. The estimation results and the impulse response analysis will reveal the impact of unexpected shock to personal income, building permit, and house price on employment and the persistency of each impact. Also, the comparison between tradable and non-tradable employment will allow us to verify the significance of the housing net worth channel reported in previous research.

Without any restrictions on contemporaneous effects between variables, we consider the system:

$$By_{it} = u_i + C_1 y_{it-1} + \cdots + C_p y_{it-p} + e_{it}, \qquad i = 1 \cdots n\,, t = 1 \cdots T$$

where B is the matrix of contemporaneous coefficient, u_i is state-specific fixed effect, C_p are matrix of lag coefficients, and e_{it} are white noise disturbances. In our case, n=45 (the number of states) and T=234 (length of time-series). Multiplication of the inverse of B on both side yields the reduced form VAR representation of the system:

$$y_{it} = \mu_i + A_1 y_{it-1} + \cdots A_p y_{it-p} + \varepsilon_{it}$$

where $A_p=B^{-1}C_p$ and $\varepsilon_{it}=B^{-1}e_{it}$. Therefore, the estimation of the reduced form VAR allows correlation between contemporaneous disturbances if B is not restricted to be diagonal. In our specification, we explicitly include terms for unobserved individual fixed effect rather than assuming those individual effects are uncorrelated with our endogenous variables. We further assume that the coefficients matrix and covariance matrix of residual are homogeneous across states after accounting for the state-specific fixed effects. This assumption allows to stack the data from different states in the same column for pooled estimation of the VAR model and impulse response. One of the issues with estimating auto regressive model with fixed effect is inconsistency stems from correlation between lagged endogenous variables and error term. Nickell (1981) analytically showed that the bias of the within estimator can be approximated to $plim(\hat{\gamma}^{FE} - \gamma) \approx -\frac{1+\gamma}{T-1}$ for fixed T, and random effect model or first difference transformation does not remedy the bias. As shown in bias approximation, the bias asymptotically approaches to zero because the correlation between endogenous regressor and the time mean of error is diluted as T grows. In our application, the time-series is sufficiently large to limit the influence of the potential bias of using fixed-effect estimator.[15]

[15] Various GMM based approach to correct the bias is developed in the context of the short panel cases. (Anderson and Hsiao (1982), Arellano and Bond (1991), and Blundell and Bond (1998)) For long panel application, loss of efficiency or exponentially increasing moment conditions are not desirable.

Impulse response analysis allows us to trace the time path of the shocks on each variable in the system through the moving average representation of the model. Since the estimation of the reduced form does not fully recover the structural coefficients, it is required to impose identification restrictions. We follow the recursive ordering that forces asymmetric contemporaneous impact between variables by placing the real variables (employment, personal income) ahead of housing variables (building permit, house price). This allows real variables to have a contemporaneous impact on housing variables while housing variables have only a lagged impact on real variables. Reverse ordering of the variable is also considered for robustness check, but it only makes a small numerical difference and does not have a substantial impact on our results.

Table 2.3. Panel unit root test

	Im, Pesaran and Shin	ADF-Fisher Chi Square
Non-tradable(level)	0.172	0.762
Tradable (Level)	0.455	0.808
House Price (Level)	1.00	0.999
Income (level)	1.00	1.00
Building Permit (Level)	0.456	0.988
Non-tradable(1*st* difference)	0.00	0.00
Tradable (1*st* difference)	0.00	0.00
House Price (1*st* difference)	0.00	0.00
Income (1*st* difference)	0.00	0.00
Building Permit (1*st* difference)	0.00	0.00

The table reports P-values for the panel unit root tests.

Given the sufficiently long length of time-series for each panel, the panel unit root test is performed for all variables in level and in the first difference of the variable using Im et al. (2003) and ADF-Fisher Chi-square test. Both types of the test have a null hypothesis of non-stationarity (unit root), and Table 2.3 reports the p-values of the test statistics. The results of the unit root test suggest that all variables have unit root in level while they become stationary in first differenced form at all conventional significance levels. Table 2.4 presents the parameter estimation results of the employment equation in our PVAR model. The model is estimated with the first differenced data; therefore, each variable can be read as the growth rate of the variable for each period. The optimal lag length of the model is chosen by utilizing AIC and BIC, and the suggested lag length of the model is 1. House price has a statistically significant effect on both types of employment, and more importantly, the effect is much stronger for employment in non-tradable sectors. Building permits have a significant positive impact only on non-tradable employment, while the effect of personal income is significant only for tradable employment.

Table 2.4. Panel VAR estimates

	Tradable Employment Growth	Non-tradable Employment Growth
Tradable Employment Growth Lags	-0.03 (2.81)	-0.181 (18.32)
Income Growth Lags	0.001 (1.96)	0.002 (1.31)
Building Permit Growth Lags	0.731 (1.64)	2.435 (3.24)
House Price Growth lags	0.140 (7.81)	0.225 (7.27)
R-squared	0.01	0.03

The table reports parameter estimates from the panel VAR model. T-statistics are reported in parentheses.

Figures 2.1 to 2.3 report the impulse response function derived from the estimated PVAR model. They provide a graphical visualization of the interrelationship between variables in the system, and we focus on the response of employment variables to fit our research purposes. To normalize the magnitude of the shock for each variable, each figure tracks the 60-month response against a unit standard deviation shock with a 95% confidence interval. Figure 2.1 shows the impulse response of tradable employment growth to unexpected shocks in house price, building permits, and personal income growths, and it does not display contemporaneous response due to the variable ordering used for this analysis. Figure 2.2 presents the same set of impulse responses for non-tradable employment, and Figure 2.3 compares the previous impulse response of tradable and non-tradable employment growth on the same scale.

Figure 2.1. Impulse response functions of tradable employment from the panel VAR model

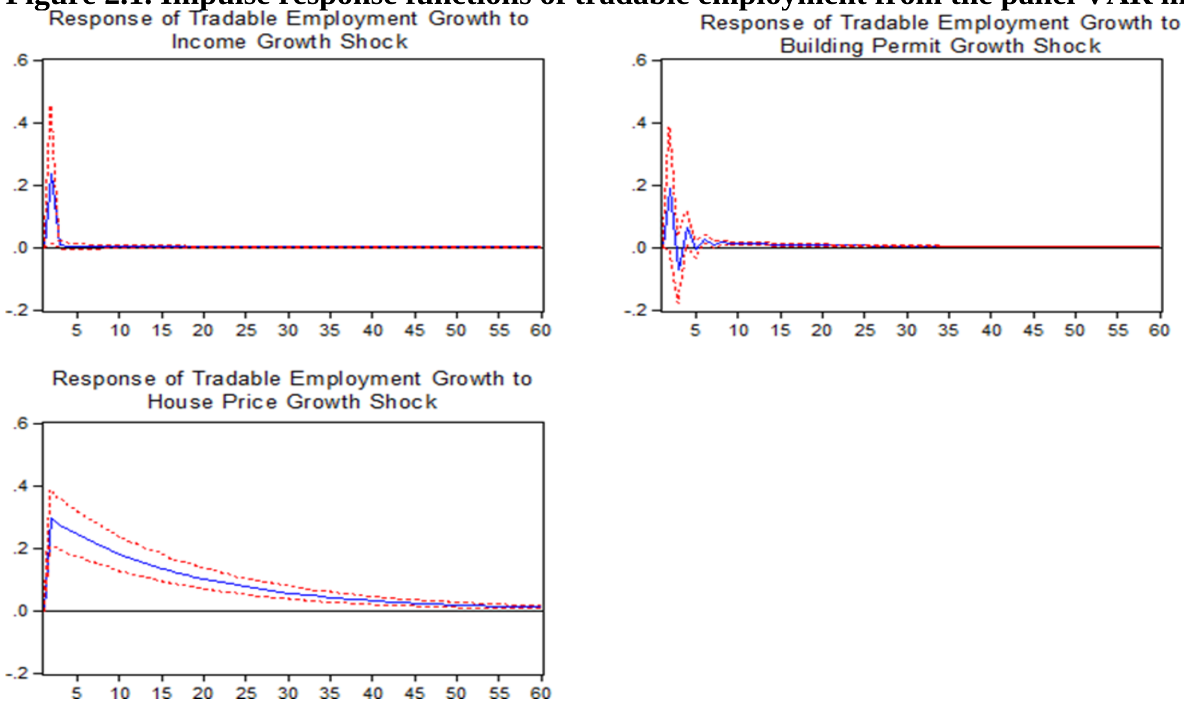


Figure 2.2. Impulse response functions of non-tradable employment from the panel VAR model

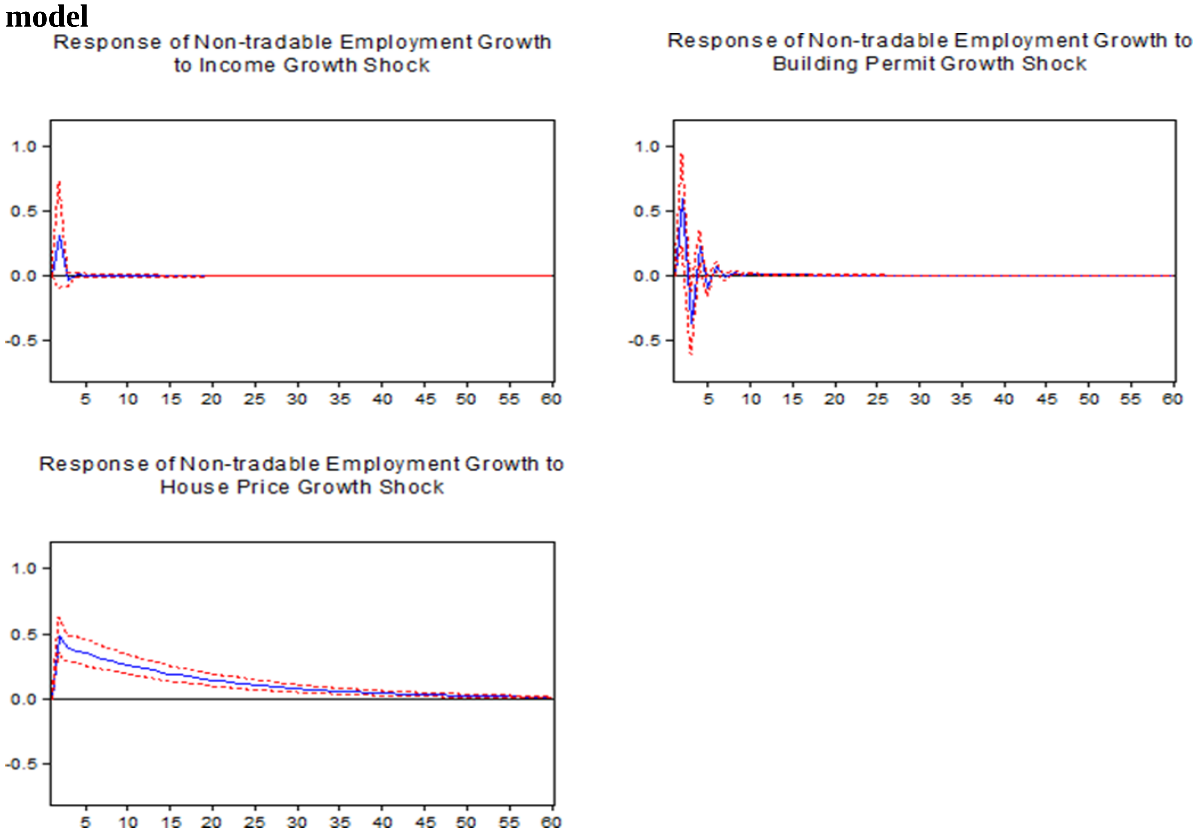
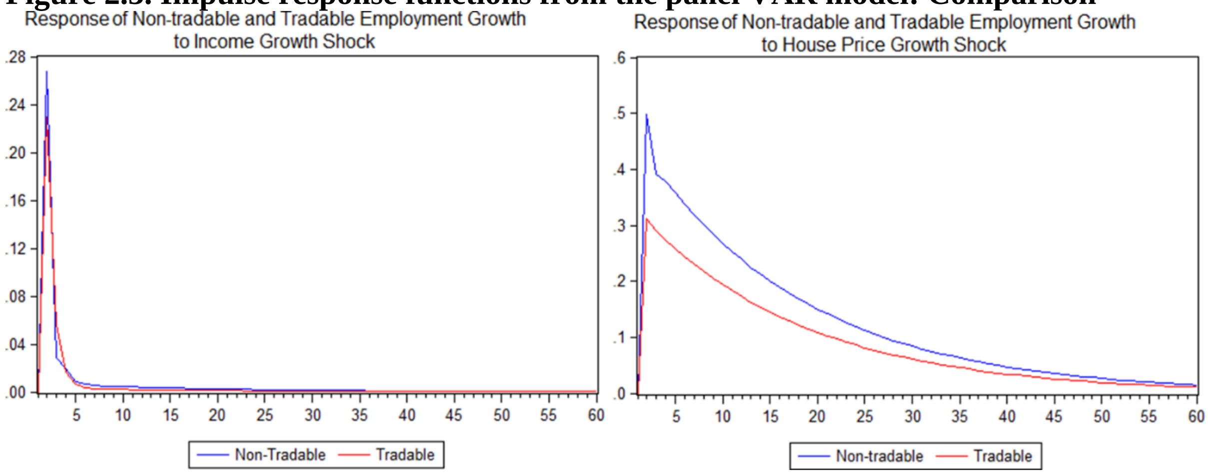


Figure 2.3. Impulse response functions from the panel VAR model: Comparison



Overall, employment consequences of personal income and building permits are limited. Though positive shocks from income and building permit create a positive response of the employment growth, the employment response is short-lived, and the magnitude of the response is mostly not statistically different from zero even in the early periods. In contrast, a positive shock in house prices leaves a long-lasting positive response of employment growth. The impact on employment decays over time, but they are statistically significant for the entire 60-month horizon. Given the persistence of the housing market frictions, this result coincides with the slow recovery of Japan from the asset bubble or the U.S. economy from the Great Recession. As Figure 2.3 shows, a unit standard deviation shock in house prices generates a much stronger response of the non-tradable employment growth relative to the tradable employment case. Specifically, the initial response of the non-tradable employment growth is 0.46 units while the corresponding response of tradable employment growth is only 0.31. This difference in responses is still persistent between two variables after the 5-year time horizon.

The impulse response analysis from the PVAR model is interesting and instructive for our analysis. There are two noteworthy findings. First, non-tradable employment responds more to housing variables than tradable employment. Housing variables include building permits and house prices. However, only the response to a house price shock is persistent and lasts for more than 5-years in our analysis. The mechanical explanation of persistence, or lack thereof, lies in the time-series property of these data series. High persistence of real house price growth leads to a persistent effect. In other words, if there is an unexpected shock to house price growth, both non-tradable and tradable employment responds significantly. Since house price shock is expected to last long, the responses of different employment variables are also expected to last long. Building permits or income growth on the other hand do not show high persistence, and that is why we do not observe a long-lasting effect of these shocks. Intuitively speaking, a change in house price affects household balance sheets and that in turn has an impact on collateral and leverage of households as well as firms. This creates a higher impact on employment in the non-tradable sector since demand for the non-tradable sector cannot be diversified away as in the case of tradables.[16]

2.4.2 Forecasting Analysis

2.4.2.1 Methodology

The PVAR analysis presented above is limited to in-sample analysis of the relationship between the housing markets and employment conditions. It does not provide any guidance on out-of-sample predictive ability of different variables. In addition, the PVAR analysis provides us the average effect of different types of shocks for the U.S. economy. It is clear, however, that a significant degree of heterogeneity exists in the housing market at state level. Given that we have data along the time dimension that is of significant size, we can extend the analysis to an out-of-

[16] Incomplete markets and credit constraints make it difficult to fully insure against local labor market shocks. While labor mobility can mitigate the impact of local shocks, there is mounting evidence that the movement of labor across regions in the aftermath of shocks is sluggish and, at best, incomplete, such as those originating from falling house prices. See, for example, Asdrubali et al. (1996); Del Negro (2002), and Lustig and Van Nieuwerburgh (2010).

sample forecasting for each state separately. To examine the predictive power of house price growth for tradable and non-tradable employment growth in different states, we estimate vector autoregression (VAR) models with house price growth and tradable employment and non-tradable employment growth. In addition, we test the forecasting power of two other macroeconomic variables - income and building permits - by estimating separate bivariate VAR models of employment growth and these variables. We specify and estimate the following VAR(p) model:

$$Y_t = C + \Phi_1 Y_{t-1} + \cdots + \Phi_p Y_{t-p} + \epsilon_t \qquad (1)$$

where $Y_t = (E_t, X_t^n)$, p is the order of VAR model, and Φ's are the coefficient matrices. E_t represents tradable employment in one specification and non-tradable in other specifications. In each specification, X_t^n is one of the n potential macroeconomic predictors of employment. This includes the growth rate of real house prices, real income growth, and changes in building permits. The optimal number of lags is chosen using the Bayesian information criterion (BIC).

The h-step-ahead out-of-sample forecasts of tradable and non-tradable employment is obtained separately by estimating separate VAR models. Thus, the one-step-ahead forecast from a VAR(2) model estimated at time t can be represented as:

$$\begin{bmatrix} \hat{E}_{t+1|t} \\ \hat{X}^n_{t+1|t} \end{bmatrix} = \begin{bmatrix} c_1 \\ c_2 \end{bmatrix} + \begin{bmatrix} \phi^1_{11} & \phi^1_{12} \\ \phi^1_{21} & \phi^1_{22} \end{bmatrix} \begin{bmatrix} E_{t|t} \\ X^n_{t|t} \end{bmatrix} + \begin{bmatrix} \phi^2_{11} & \phi^2_{12} \\ \phi^2_{21} & \phi^2_{22} \end{bmatrix} \begin{bmatrix} E_{t-1|t} \\ X^n_{t-1|t} \end{bmatrix} \qquad (2)$$

We use the forecasts from previous period to generate h-period ahead forecasts iteratively. Equation 1 is estimated for an initial in-sample window of size T_0 and the out-of-sample forecasts of employment movements $\hat{E}_{t+h|t}$ are generated recursively by adding one observation and re-estimating at each stage. Our benchmark model is a parsimonious AR(1). The forecasts obtained from the VAR model are then compared with the forecasts obtained from the benchmark AR(1) model using a similar recursive procedure. A popular metric of the ratio of root mean squared error (RMSE) from the benchmark AR(1) model and the VAR model is used to compare the forecasting performance of the two models.

2.4.2.2 Forecasting Results

To examine the predictive power of house price growth for tradable and non-tradable employment growth in different states, we estimate VAR models of employment growth with real house price growth. Our first estimation sample begins in 2001:M1 and runs through 2006:M12. Our first forecasts cover the period 2007:M1-2007:M12. We then move ahead one month, re-estimate the VAR model and forecast for sample period 2007:M2-2008:M1, etc. Our final set of forecasts, for 2019:M1-2019:M12, would have been prepared in 2018:M12. We consider different monthly horizon forecasts until h=12. We also examine the average over the next 12 months. These averages are also used in the analysis to get around the noise associated with monthly projections. The optimal number of lags is chosen using the Bayesian information criterion.[17]

The results for this out-of-sample forecasting exercise are shown in Table 2.5. There are several interesting findings from this exercise. In the majority of the states (28/45), the root mean square errors (RMSE) for non-tradable employment forecasts using house price information are lower compared to RMSE from the AR(1) benchmark model.[18,19] In case of tradable employment growth forecasts from the VAR model dominate AR(1) in 17 states. If we look at the average of

17 The results are qualitatively similar if we use AIC criterion.

18 For h=avg (1-12) forecast horizon.

19 We also calculate the ratio of mean absolute error (MAE) and the results are qualitatively similar. For sake of brevity, we do not report the results in the paper but are available upon request.

all the states (the third to last row in Table 2.5), we find that the VAR model for h=1-12 forecast horizon dominates AR(1) model for both tradable and non-tradable employment growth implying the usefulness of house prices in forecasting employment growth on average. However, as can be seen from the table, the improvement in tradable employment is only 0.24 percent, whereas the improvement in non-tradable employment is 5 percent.[20]

The most interesting results are obtained when we compare the forecasting performance of house prices for tradable and non-tradable employment. In 80% of the states, the relative performance of house price in predicting non-tradable employment is better than tradable employment growth at shorter horizons (h=1, 2). This pattern is not different for longer horizon forecasts. In predicting (h=1-12 horizon) non-tradable employment in California, Colorado, Florida, Georgia, Idaho, Maryland, Michigan, Nevada, New Mexico, Oregon, and Utah the forecasting performance of the VAR model is higher by more than 5% over the benchmark AR(1) model. The maximum forecasting gains are in predicting non-tradable employment in Nevada (89%), Florida (59%), and California (29%). These are also the states with more volatile housing markets. We explore the relationship between house price volatility and forecasting gains in the next section.

These results of improvement in out-of-sample forecasts of non-tradable employment compared to tradable employment are consistent with the findings of Mian and Sufi (2014). They found that the 2007-2009 drop in employment in U.S. states was highly correlated with house prices across different MSAs. The underlying idea is that if changes in house prices affect local employment through changes in consumer demand, then variation in house prices should explain

20 We also used Clark and West (2006) to test if the forecasting improvement over AR(1) is significant. In case of nontradable employment growth, we reject the null of equal predictive accuracy at all significance levels, whereas we could not reject the null in case of tradable employment.

(regional) variation in employment primarily in the non-tradable sector, where demand by households is local. In contrast, variation in house prices should not correlate strongly with variation in employment in the tradable sector, where demand is national or global.

Table 2.5. Forecasting performance [2007-2019]: House Prices

Predictor Housing Prices	Forecasted Variable													
	Tradable Employment							Non-Tradable Employment						
State	h=1	h=2	h=3	h=6	h=9	h=12	h=Avg	h=1	h=2	h=3	h=6	h=9	h=12	h=Avg
Alabama	1.0042	0.9890	1.0047	1.0538	0.9915	0.9770	1.0203	1.0216	1.0314	1.0453	1.0275	1.0018	0.9884	1.0377
Alaska	0.9619	0.9698	0.9673	0.9729	0.9778	0.9845	0.8515	1.0014	1.0029	0.9983	0.9963	0.9987	1.0004	0.8976
Arizona	0.9041	0.9609	0.9857	1.0054	1.0045	1.0006	1.0313	1.0605	1.0125	1.0086	1.0174	1.0149	1.0146	1.1016
Arkansas	1.0176	1.0028	0.9786	0.9890	1.0137	0.9733	0.9873	1.0232	1.0114	0.9941	1.0039	1.0117	0.9965	1.0331
California	1.0144	1.0170	1.0198	1.0163	1.0112	1.0035	1.0369	1.0612	1.0916	1.1255	1.1430	1.1360	1.1221	1.2946
Colorado	0.9959	0.9912	0.9792	0.9655	0.9693	0.9620	0.9520	1.0266	1.0435	1.0576	1.0384	1.0204	1.0123	1.0744
Connecticut	0.9798	0.9715	0.9559	0.9327	0.9278	0.9180	0.8501	1.0031	0.9932	0.9900	0.9791	0.9751	0.9658	0.9260
Florida	1.0701	1.0930	1.1396	1.1404	1.1772	1.1276	1.4003	1.1586	1.1385	1.1789	1.1924	1.1547	1.0857	1.5925
Georgia	1.0396	1.0627	1.0834	1.0500	1.0230	0.9928	1.0856	1.1094	1.1295	1.1700	1.0868	1.0296	0.9835	1.1812
Idaho	1.0334	1.0532	1.0556	1.0331	1.0440	0.9952	1.1179	1.1071	1.1603	1.1701	1.1289	1.0619	1.0318	1.2950
Illinois	0.9993	1.0004	1.0000	0.9982	0.9977	0.9967	0.9855	1.0187	1.0196	1.0178	0.9930	0.9476	0.9227	0.7942
Indiana	1.0269	1.0535	1.0507	1.0297	1.0047	0.9982	1.0443	1.0200	1.0294	1.0267	1.0020	0.9980	0.9952	1.0028
Kansas	0.9896	0.9842	0.9829	0.9901	0.9891	0.9918	0.9503	0.9915	0.9955	0.9956	0.9971	0.9940	0.9954	0.9582
Kentucky	1.0182	1.0117	1.0041	0.9986	0.9984	0.9972	1.0010	1.0307	1.0077	1.0023	0.9899	0.9918	0.9946	0.9733
Louisiana	0.9915	0.9929	0.9833	0.9797	0.9869	0.9850	0.9428	0.9871	0.9930	0.9942	0.9984	0.9988	0.9989	0.9893
Iowa	1.0187	1.0084	0.9924	0.9872	0.9899	0.9864	0.9607	0.9940	0.9975	0.9937	0.9955	0.9948	0.9937	0.9581
Maryland	1.0110	1.0096	1.0060	0.9986	1.0055	1.0045	1.0335	1.0197	1.0285	1.0190	1.0196	1.0118	0.9940	1.1132
Massachusetts	0.9737	0.9219	0.9155	0.9284	0.9097	0.9033	0.8489	0.9941	0.9933	0.9906	0.9836	0.9782	0.9733	0.9184
Michigan	1.0847	1.0815	1.0591	1.0473	1.0260	1.0158	1.1655	1.0461	1.0712	1.0691	1.0486	1.0276	1.0091	1.0977
Minnesota	0.9788	0.9731	0.9652	0.9448	0.9348	0.9219	0.8870	1.0219	1.0245	1.0238	1.0091	0.9927	0.9811	1.0102
Mississippi	1.0049	1.0051	1.0062	1.0080	0.9880	0.9894	0.9984	1.0154	1.0205	1.0204	1.0018	0.9936	0.9925	1.0154
Missouri	0.9994	1.0041	0.9975	0.9854	0.9734	0.9526	0.9507	1.0210	1.0301	1.0382	1.0101	0.9956	1.0025	1.0206
Montana	1.0335	1.0414	1.0404	1.0054	0.9663	0.9810	1.0085	1.0356	1.0410	1.0131	1.0421	0.9764	0.9736	1.0465
Nebraska	0.9948	0.9950	0.9938	0.9904	0.9935	0.9932	0.9419	1.0001	0.9933	0.9909	0.9953	1.0069	0.9982	1.0147
Nevada	1.1133	1.1605	1.1883	1.2213	1.2223	1.2144	1.4911	1.1628	1.2207	1.2858	1.2906	1.2976	1.2441	1.8957
New Hampshire	0.9735	0.9670	0.9598	0.9416	0.9248	0.9107	0.8757	1.0160	1.0093	1.0085	1.0027	1.0013	0.9908	0.9955
New Jersey	0.9897	0.9843	0.9751	0.9538	0.9298	0.9192	0.8773	1.0101	1.0122	1.0107	0.9899	0.9718	0.9509	0.9571
New Mexico	1.0074	1.0366	1.0136	1.0025	1.0116	1.0175	1.0442	1.0487	1.0869	1.0768	1.0586	1.0376	1.0170	1.1942
New York	0.9924	0.9744	0.9667	0.9642	0.9599	0.9678	0.9155	0.9922	0.9838	0.9829	0.9799	0.9780	0.9790	0.8872
North Carolina	0.9945	1.0030	1.0173	1.0077	0.9894	0.9774	0.9978	1.0093	1.0394	1.0472	1.0007	0.9752	0.9763	1.0045
North Dakota	0.9996	1.0139	1.0101	1.0022	0.9963	0.9952	0.9968	1.0333	1.0458	1.0301	1.0125	1.0130	1.0040	1.0438
Ohio	1.0044	1.0244	1.0354	1.0048	0.9952	0.9847	1.0107	1.0166	1.0237	1.0185	0.9993	1.0013	0.9927	1.0035
Oklahoma	0.9785	0.9836	0.9883	0.9957	0.9930	0.9941	0.9850	0.9717	0.9942	0.9922	1.0116	1.0046	1.0036	0.9946
Oregon	1.0196	1.0280	1.0285	1.0095	1.0097	0.9700	1.0461	1.0495	1.0295	1.0494	1.0770	1.0220	0.9524	1.0774
Pennsylvania	0.9934	0.9926	0.9747	0.9556	0.9497	0.9574	0.9187	0.9973	1.0006	0.9923	0.9845	0.9770	0.9754	0.9223
South Carolina	1.0006	1.0036	1.0100	0.9938	0.9809	0.9794	0.9808	1.0310	1.0367	1.0582	1.0024	0.9908	0.9756	1.0248
South Dakota	1.0040	0.9951	1.0023	0.9946	0.9920	0.9847	0.9724	0.9915	0.9998	0.9988	1.0035	1.0036	0.9980	0.9909
Tennessee	1.0204	1.0405	1.0558	1.0384	1.0217	1.0098	1.0626	1.0424	1.0409	1.0406	1.0417	0.9912	0.9703	1.0341
Texas	0.9926	0.9964	0.9830	0.9663	0.9624	0.9725	0.9586	1.0173	1.0322	1.0283	1.0156	1.0036	1.0043	1.0399
Utah	1.0346	1.0387	1.0422	1.0751	1.0547	1.0017	1.1223	1.1065	1.1480	1.1650	1.1472	1.0633	0.9664	1.2302
Vermont	0.9898	0.9868	0.9852	0.9879	0.9855	0.9918	0.9497	1.0040	1.0022	1.0044	1.0042	1.0083	1.0130	1.0435
Virginia	1.0029	1.0000	0.9971	0.9771	0.9733	0.9646	0.9441	1.0101	1.0340	1.0283	0.9979	0.9866	0.9793	1.0300
Washington	0.9930	0.9987	0.9988	0.9990	0.9991	0.9981	0.9821	1.0448	1.0672	1.1183	1.1359	1.0758	1.0091	0.7891
West Virginia	0.9990	1.0052	1.0002	0.9958	0.9910	0.9965	0.9946	0.9995	1.0029	1.0085	1.0005	1.0016	0.9918	0.9803
Wisconsin	0.9964	0.9969	0.9860	0.9548	0.9476	0.9474	0.9274	1.0198	1.0153	1.0006	0.9879	0.9830	0.9768	0.9452
AVG	1.0055	1.0094	1.0086	1.0021	0.9954	0.9868	1.0024	1.0298	1.0375	1.0418	1.0321	1.0155	0.9999	1.0540
MIN	0.9041	0.9219	0.9155	0.9284	0.9097	0.9033	0.8489	0.9717	0.9838	0.9829	0.9791	0.9476	0.9227	0.7891
MAX	1.1133	1.1605	1.1883	1.2213	1.2223	1.2144	1.4911	1.1628	1.2207	1.2858	1.2906	1.2976	1.2441	1.8957

The table presents, for each forecast horizon (h), the ratio of RMSE from the benchmark AR(1) model with changes in housing Prices to RMSE from the VAR model in predicting tradable and non-tradable employment. The estimation sample for the first forecast is 2001:M1-2006:M12, which is recursively updated for the forecast sample 2007:M1-2019:M12.

Given that we find consistent improvement in forecasting performance of non-tradable employment over tradable employment when house price growth is used as a predictor, one would naturally be tempted to argue that it may be due to the overall demand channel in those states and not due to the demand arising out of the net worth channel. To examine this, we also compare the forecasting performance of these two types of employment growth by using housing building permits and personal income growth as predictors. In addition to house prices, housing building permits are a good proxy for the overall housing market conditions in different states, but at the same time are not expected to generate asymmetric demand effects on tradable and non-tradable employment. Similarly, real personal income growth is used as a proxy for overall business cycle indicator in these states. The results of forecasting performance from income growth and changes in housing permits are presented in Table 2.6 and 2.7, respectively. Results in Table 2.6 show that real personal income growth provides positive forecasting gains in predicting employment in a few states. In states and forecasting horizons where this is not the case, the forecasting advantage of the benchmark AR(1) model over the VAR model is not more than 10% for tradable employment growth.

There are at least two noteworthy results that emerge when we compare the forecasting performance of house prices and income. First, the size of forecasting errors of non-tradable employment forecasts from the VAR with house prices is systematically lower compared to the ones from the VAR with personal income growth. In fact, the maximum forecasting gain relative to an AR(1) model in predicting non-tradable employment using income is 15% (Tab. 6), whereas it is 89% when house price growth is used as a predictor (Tab. 5). More importantly, unlike house prices, we find that personal income growth does a better job predicting tradable employment growth than non-tradable employment. For h=1-12 forecasting horizon, on average, the inclusion of personal income growth in a VAR model improves forecasts of tradable employment growth by 2%, whereas it worsens the forecasting performance of non-tradable employment growth by approximately 1%.

Table 2.6. Forecasting performance [2007-2019]: Income

Predictor Income	Forecasted Variable													
	Tradable Employment							Non-Tradable Employment						
State	h=1	h=2	h=3	h=6	h=9	h=12	h=Avg	h=1	h=2	h=3	h=6	h=9	h=12	h=Avg
Alabama	0.9646	0.9594	0.9931	0.9911	0.9811	0.9941	0.9752	0.9859	0.9857	0.9971	0.9968	0.9836	1.0003	0.9752
Alaska	0.9236	0.9579	0.9902	0.9969	1.0148	0.9796	0.9958	1.0065	1.0065	1.0063	1.0019	1.0011	1.0026	0.9500
Arizona	0.9616	0.9883	1.0014	1.0002	1.0007	0.9995	1.0044	1.0358	1.0042	1.0038	1.0016	1.0026	0.9980	0.9968
Arkansas	1.0019	1.0098	0.9970	0.9997	1.0012	0.9986	0.9940	0.9989	1.0161	1.0153	0.9952	1.0008	0.9999	1.0110
California	0.9732	1.0033	0.9961	1.0637	1.0051	1.0083	1.0659	0.9843	0.9877	1.0056	1.0322	1.0037	1.0021	1.0524
Colorado	1.0071	1.0319	1.0382	1.0161	1.0043	0.9944	1.0200	0.9847	1.0154	1.0131	1.0032	1.0018	1.0009	1.0269
Connecticut	0.9989	0.9950	0.9991	1.0001	0.9981	0.9980	0.9987	1.0009	1.0006	0.9989	1.0011	0.9997	0.9998	1.0105
Florida	0.9882	1.0124	1.0750	1.1167	1.0108	1.0339	1.1371	1.0044	0.9979	1.0180	1.0259	1.0100	1.0013	1.0647
Georgia	0.9973	1.0211	1.0454	1.0213	1.0091	1.0251	1.0374	1.0032	1.0548	1.0500	1.0144	1.0350	1.0298	1.0563
Idaho	1.0072	1.0495	1.0621	1.0282	1.0081	1.0065	1.0666	1.0051	0.9843	0.9701	1.0494	1.0010	1.0081	1.0468
Illinois	1.0044	1.0010	1.0022	0.9998	0.9995	0.9994	0.9964	1.0251	1.0579	1.0693	1.0101	1.0005	0.9972	0.8306
Indiana	1.0193	1.0402	1.0406	1.0159	0.9984	0.9960	1.0149	1.0150	1.0333	1.0225	1.0136	0.9963	0.9982	0.9942
Kansas	1.0270	1.0120	1.0255	1.0161	1.0068	1.0010	1.0411	1.0201	1.0242	0.9966	0.9976	1.0011	0.9978	0.9733
Kentucky	1.0045	1.0192	1.0224	1.0088	0.9934	0.9967	1.0049	1.0108	1.0253	1.0179	0.9917	0.9932	0.9958	0.9743
Louisiana	0.9798	0.9879	0.9976	0.9935	0.9986	0.9982	0.9877	0.7965	0.8205	0.8843	0.9698	0.9913	0.9998	0.8222
Iowa	1.0222	1.0239	1.0167	1.0241	1.0011	1.0004	1.0357	0.9995	1.0143	1.0180	1.0019	1.0027	1.0001	1.0241
Maryland	0.9890	1.0047	1.0113	1.0132	1.0012	1.0087	1.0514	0.9974	0.9956	1.0037	1.0091	0.9941	1.0024	1.0439
Massachusetts	0.9840	0.9540	1.0055	1.0307	0.9999	0.9879	1.0087	0.9929	1.0059	1.0153	1.0067	0.9982	0.9927	1.0055
Michigan	0.9665	1.0109	1.0272	0.9779	0.9900	0.9955	0.9911	0.9979	1.0178	1.0157	1.0071	0.9971	0.9978	0.9625
Minnesota	1.0387	1.0705	1.0925	1.0164	1.0072	1.0091	1.0599	1.0329	1.0354	1.0292	1.0011	1.0018	1.0007	1.0297
Mississippi	0.9944	1.0053	1.0061	0.9987	1.0003	1.0013	1.0001	0.9951	1.0059	1.0011	0.9971	0.9979	0.9990	1.0028
Missouri	1.0048	1.0123	1.0209	1.0043	0.9937	0.9972	1.0040	0.9987	1.0093	0.9970	0.9966	0.9969	0.9974	0.9766
Montana	0.9963	1.0456	1.0444	1.0312	0.9997	1.0170	1.0627	1.0047	1.0328	0.9932	1.0133	0.9980	0.9958	1.0302
Nebraska	1.0079	1.0076	1.0078	1.0048	1.0016	0.9969	1.0350	0.9916	1.0013	1.0043	1.0042	1.0024	1.0106	1.0320
Nevada	1.0070	1.0403	1.0649	1.0658	1.0524	1.0384	1.1099	0.9794	1.0240	1.0712	1.0879	1.0724	1.0437	1.1540
New Hampshire	0.9867	1.0031	1.0193	1.0134	1.0049	1.0028	1.0248	1.0093	1.0014	1.0018	1.0055	1.0002	0.9994	1.0007
New Jersey	0.9909	1.0063	1.0107	0.9976	0.9235	0.8769	0.9053	0.9952	1.0222	1.0341	1.0003	1.0009	0.9842	1.0109
New Mexico	1.0064	1.0030	1.0006	1.0026	0.9982	0.9994	1.0002	0.9850	0.9910	0.9884	0.9981	0.9965	0.9972	0.9895
New York	0.9972	0.9886	1.0046	1.0241	1.0076	1.0101	1.0318	0.9958	1.0041	1.0076	1.0049	1.0026	1.0024	0.9932
North Carolina	0.9934	0.9837	0.9992	0.9970	0.9904	0.9908	0.9858	1.0010	0.9943	1.0029	0.9816	0.9816	0.9802	0.9626
North Dakota	1.0163	1.0498	1.0619	1.0205	1.0021	1.0034	1.0329	1.0345	1.0579	1.0539	0.9963	1.0138	1.0034	1.0478
Ohio	1.0074	1.0128	1.0203	1.0207	1.0114	1.0073	1.0220	1.0061	1.0112	1.0103	1.0100	0.9994	0.9996	1.0044
Oklahoma	1.0965	1.1529	1.1361	1.0470	0.9992	0.9784	1.0617	1.0209	1.0232	1.0130	1.0131	1.0015	0.9795	0.9774
Oregon	1.0201	1.0279	1.0025	1.0314	0.9903	0.9986	1.0227	0.9974	0.9695	0.9811	0.9966	1.0003	0.9953	0.9957
Pennsylvania	1.0034	1.0189	1.0229	1.0254	0.9950	0.9994	1.0236	0.9971	1.0261	1.0283	1.0049	0.9933	0.9979	1.0059
South Carolina	1.0085	1.0391	1.0509	1.0213	1.0095	1.0006	1.0278	1.0077	1.0215	1.0106	1.0016	0.9992	0.9999	1.0104
South Dakota	1.0113	1.0372	1.0670	1.0121	1.0182	1.0117	1.0662	1.0049	1.0051	1.0038	0.9980	1.0044	1.0021	0.9914
Tennessee	0.9878	0.9849	0.9909	1.0368	1.0137	1.0069	1.0182	0.9927	1.0301	1.0512	1.0176	1.0022	0.9995	1.0119
Texas	1.0001	0.9735	1.0009	0.9802	0.9290	0.9225	0.9217	0.9911	0.9928	0.9942	1.0051	0.9871	0.9510	0.9626
Utah	0.9463	0.9548	0.9852	1.0519	0.9844	0.9996	1.0012	0.9281	0.9556	1.0061	1.0031	0.9982	0.9966	1.0075
Vermont	1.0009	1.0090	1.0125	1.0075	0.9955	0.9952	1.0026	0.9977	1.0020	1.0065	1.0007	0.9985	0.9990	1.0053
Virginia	1.0090	1.0164	1.0416	1.0194	0.9851	1.0072	1.0334	1.0001	1.0258	1.0097	1.0087	0.9794	1.0009	1.0227
Washington	0.9973	0.9995	0.9997	1.0001	1.0000	1.0000	1.0013	1.0070	1.0060	1.0057	1.0048	1.0015	0.9993	0.7381
West Virginia	1.0103	1.0133	1.0252	1.0081	1.0021	0.9937	1.0092	1.0004	1.0020	1.0025	0.9989	0.9978	0.9995	0.9762
Wisconsin	1.0112	1.0084	1.0097	1.0059	0.9986	1.0058	1.0140	1.0142	1.0189	1.0006	1.0001	0.9987	0.9991	0.9858
AVG	0.9993	1.0122	1.0232	1.0168	0.9986	0.9976	1.0201	0.9968	1.0071	1.0095	1.0062	1.0009	0.9991	0.9944
MIN	0.9236	0.9540	0.9852	0.9779	0.9235	0.8769	0.9053	0.7965	0.8205	0.8843	0.9698	0.9794	0.9510	0.7381
MAX	1.0965	1.1529	1.1361	1.1167	1.0524	1.0384	1.1371	1.0358	1.0579	1.0712	1.0879	1.0724	1.0437	1.1540

The table presents, for each forecast horizon (h), the ratio of RMSE from the benchmark AR(1) model with changes in income to RMSE from the VAR model in predicting tradable and non-tradable employment. The estimation sample for the first forecast is 2001:M1-2006:M12, which is recursively updated for the forecast sample 2007:M1-2019:M12.

Evidence from Table 2.7 suggests that changes in building permits, among all the variables, has the least incremental information in forecasting employment. In fact, the RMSE from the VAR model that contains building permits are almost at par with the benchmark AR(1) model. The forecasting performance of building permits is similar to disposable income growth in one dimension. Unlike house prices, changes in building permits also do not have superior predictive power for nontradable employment growth as compared to tradable employment growth.

Table 2.7. Forecasting performance [2007-2019]: Building permits

Predictor	**Forecasted Variable**													
Housing permits	**Tradable Employment**							**Non-Tradable Employment**						
State	h=1	h=2	h=3	h=6	h=9	h=12	h=Avg	h=1	h=2	h=3	h=6	h=9	h=12	h=Avg
Alabama	1.0104	1.0062	1.0064	1.0014	0.9999	0.9995	1.0043	0.9915	0.9958	1.0101	0.9983	0.9996	0.9992	0.9962
Alaska	0.9641	0.9958	1.0054	1.0012	1.0014	1.0015	1.0079	0.9978	0.9991	0.9999	1.0004	1.0006	1.0006	0.9331
Arizona	0.9981	1.0003	0.9999	1.0001	1.0001	1.0001	1.0011	0.9922	0.9986	0.9996	1.0000	0.9998	0.9992	0.9900
Arkansas	0.9952	0.9996	1.0030	1.0011	1.0014	1.0011	1.0027	0.9914	0.9968	0.9977	1.0003	0.9998	0.9998	1.0021
California	0.9936	1.0012	1.0001	1.0002	1.0001	1.0001	1.0000	0.9959	1.0015	1.0006	0.9998	0.9994	0.9992	1.0229
Colorado	0.9843	1.0066	0.9962	0.9992	0.9959	0.9925	0.9953	0.9928	1.0019	1.0001	1.0010	1.0010	1.0008	1.0201
Connecticut	0.9894	1.0018	0.9979	0.9994	0.9991	0.9990	0.9974	0.9962	1.0096	1.0014	0.9998	1.0001	1.0006	1.0116
Florida	0.9939	0.9864	0.9977	0.9987	0.9991	0.9988	1.0002	0.9991	0.9966	0.9984	0.9987	0.9986	0.9983	1.0315
Georgia	0.9939	1.0031	1.0006	1.0017	1.0013	1.0010	1.0021	0.9955	1.0059	1.0000	0.9999	0.9996	0.9995	1.0087
Idaho	0.9856	1.0018	0.9999	0.9996	0.9996	0.9994	0.9994	0.9860	0.9945	0.9965	0.9975	0.9979	0.9978	1.0209
Illinois	0.9809	0.9820	0.9953	1.0010	1.0008	0.9999	0.9993	0.9897	1.0026	0.9981	0.9993	0.9986	0.9984	0.8256
Indiana	0.9909	1.0025	1.0024	1.0013	1.0011	1.0010	1.0023	0.9911	0.9966	1.0002	0.9987	0.9984	0.9984	0.9772
Kansas	0.9954	1.0021	0.9997	1.0002	1.0000	0.9997	0.9997	0.9825	0.9959	1.0011	1.0000	0.9999	0.9998	0.9753
Kentucky	0.9899	0.9934	0.9989	0.9999	1.0000	0.9997	0.9997	0.9938	0.9984	1.0004	0.9997	0.9996	0.9996	0.9858
Louisiana	0.9938	0.9986	1.0018	0.9999	0.9999	1.0000	1.0001	0.9946	1.0020	1.0016	1.0002	1.0002	1.0000	1.0142
Iowa	0.9947	1.0019	0.9992	1.0000	0.9997	0.9996	0.9994	1.0070	1.0046	0.9998	0.9996	0.9998	0.9998	0.9956
Maryland	0.9945	0.9957	1.0012	0.9996	0.9999	0.9997	0.9989	1.0075	1.0041	1.0035	0.9995	0.9998	0.9998	1.0359
Massachusetts	0.9945	1.0056	1.0014	1.0000	0.9979	0.9973	1.0016	0.9900	0.9965	0.9963	0.9996	0.9995	0.9996	0.9958
Michigan	0.9952	0.9878	0.9955	0.9980	0.9980	0.9981	0.9951	0.9985	0.9915	1.0004	1.0001	1.0001	1.0001	0.9528
Minnesota	0.9810	1.0058	0.9995	1.0045	1.0040	1.0039	1.0091	1.0018	1.0027	1.0018	0.9996	0.9996	0.9994	1.0005
Mississippi	1.0056	1.0029	1.0080	1.0029	1.0030	1.0029	1.0070	0.9908	0.9981	1.0001	1.0000	0.9997	0.9996	1.0077
Missouri	0.9813	0.9988	1.0013	0.9999	0.9998	0.9997	1.0007	1.0036	0.9999	1.0002	0.9997	0.9996	0.9995	0.9853
Montana	0.9904	1.0045	1.0019	1.0010	1.0007	1.0005	1.0038	0.9989	0.9987	0.9995	0.9990	0.9990	0.9989	1.0099
Nebraska	0.9961	1.0042	1.0014	1.0009	1.0004	1.0004	1.0043	0.9930	0.9991	1.0007	0.9999	0.9998	0.9996	1.0160
Nevada	0.9925	0.9968	0.9961	0.9981	0.9968	0.9954	0.9953	0.9849	0.9887	0.9994	0.9986	0.9984	0.9982	1.0050
New Hampshire	0.9938	1.0014	0.9985	1.0004	1.0001	1.0000	1.0000	0.9932	0.9962	1.0014	1.0000	0.9999	0.9999	0.9835
New Jersey	0.9912	1.0006	0.9958	0.9998	0.9982	0.9983	0.9956	0.9969	1.0011	0.9976	0.9997	0.9995	0.9996	1.0002
New Mexico	0.9965	1.0009	0.9995	1.0001	1.0000	1.0000	1.0000	0.9987	0.9928	1.0034	0.9991	0.9994	0.9995	1.0061
New York	0.9934	1.0033	0.9999	1.0040	1.0042	1.0039	1.0096	0.9962	1.0003	0.9989	1.0004	1.0003	1.0000	0.9764
North Carolina	0.9979	1.0024	1.0085	1.0106	1.0125	1.0129	1.0171	0.9947	0.9998	1.0008	0.9998	1.0002	1.0002	1.0060
North Dakota	0.9949	1.0045	0.9998	1.0008	0.9996	0.9996	1.0018	0.9986	0.9996	1.0051	0.9999	1.0000	0.9999	1.0022
Ohio	0.9977	0.9874	0.9986	1.0073	1.0100	1.0108	1.0116	1.0007	0.9934	0.9993	0.9996	0.9999	0.9998	0.9901
Oklahoma	0.9887	0.9919	0.9993	0.9986	0.9988	0.9987	0.9974	1.0007	0.9995	0.9984	0.9989	0.9992	0.9994	0.9631
Oregon	0.9890	1.0008	1.0000	1.0005	1.0005	1.0006	1.0007	0.9911	0.9985	1.0018	0.9981	0.9993	0.9984	1.0083
Pennsylvania	0.9940	1.0028	0.9999	0.9997	0.9990	0.9990	0.9978	0.9954	1.0006	0.9988	0.9995	0.9993	0.9992	0.9891
South Carolina	1.0169	1.0012	1.0094	1.0039	1.0039	1.0038	1.0091	0.9963	1.0026	1.0026	1.0009	1.0010	1.0013	1.0116
South Dakota	1.0014	1.0007	1.0000	0.9994	0.9991	0.9989	0.9983	1.0045	1.0148	1.0037	1.0005	1.0001	1.0000	0.9794
Tennessee	1.0045	0.9973	1.0048	1.0094	1.0113	1.0111	1.0150	0.9927	0.9970	1.0009	1.0003	1.0009	1.0007	0.9973
Texas	0.9931	1.0012	0.9976	0.9960	0.9924	0.9908	0.9908	0.9911	1.0021	0.9994	0.9988	0.9977	0.9976	1.0003
Utah	0.9956	1.0015	1.0027	1.0001	0.9999	0.9994	1.0004	0.9957	0.9933	0.9985	0.9979	0.9993	0.9996	1.0196
Vermont	0.9405	0.9908	0.9872	1.0004	0.9991	0.9988	0.9927	1.0201	1.0262	0.9961	0.9987	1.0000	0.9998	1.0008
Virginia	1.0008	0.9979	0.9949	1.0006	1.0002	1.0003	0.9995	0.9695	0.9979	0.9988	0.9998	0.9997	0.9996	1.0262
Washington	0.9981	0.9995	1.0000	0.9999	0.9999	0.9999	0.9992	0.9873	0.9897	0.9959	0.9966	0.9983	0.9983	0.7339
West Virginia	0.9772	0.9994	0.9991	1.0000	1.0000	1.0002	1.0009	0.9751	0.9493	1.0069	1.0006	1.0004	0.9999	0.9844
Wisconsin	1.0034	1.0009	1.0170	1.0045	1.0051	1.0046	1.0090	0.9928	0.9957	1.0072	1.0002	0.9998	0.9999	0.9839
AVG	0.9923	0.9994	1.0005	1.0010	1.0008	1.0005	1.0016	0.9946	0.9984	1.0005	0.9995	0.9996	0.9995	0.9885
MIN	0.9405	0.9820	0.9872	0.9960	0.9924	0.9908	0.9908	0.9695	0.9493	0.9959	0.9966	0.9977	0.9976	0.7339
MAX	1.0169	1.0066	1.0170	1.0106	1.0125	1.0129	1.0171	1.0201	1.0262	1.0101	1.0010	1.0010	1.0013	1.0359

The table presents, for each forecast horizon (h), the ratio of RMSE from the benchmark AR(1) model with changes in housing permits to RMSE from the VAR model in predicting tradable and non-tradable employment. The estimation sample for the first forecast is 2001:M1-2006:M12, which is recursively updated for the forecast sample 2007:M1-2019:M12.

Overall, the evidence shows that among house prices, income, and housing permits the forecasting gains are maximum in the case of house price growth. In addition, house price growth, unlike income and building permits, provides distinct forecasting gains in predicting non-tradable employment as compared to tradable employment. In forecasting non-tradable employment, the average gains from using house price information are higher across all the horizons as compared to the case when the forecasted variable is tradable employment. In fact, on average, the inclusion of building permits in a VAR model worsens the forecasting performance of non-tradable employment growth by 1.1% and keeps the forecasts of tradable employment growth unchanged.

2.4.2.3 Prediction Heterogeneity, Supply Elasticity and House Price Volatility

We find that not only does real house price growth have superior predictive power for non-tradable employment growth, there is also significant heterogeneity in predictability across different states. Since local demand channels arising out of changes in net worth operate through changes in house prices, it would be an interesting exercise to see if predictive power of house price across different states is related to housing supply elasticity. To examine the relationship between predictive power and housing elasticity, we use the geography-based measure of supply elasticity of Saiz (2010). There are two issues with using this housing supply elasticity in our context. First, the supply elasticity data is unavailable at the state level. Second, these measures are cross-sectional and hence there is no time variation. To take into account the first problem, we calculate the state level housing supply elasticity by weighting the supply elasticity of MSA in each state by its population. This population weighted supply elasticity estimates are reported in the appendix.[21] To get around the second problem, we exploit the cross-sectional variation in the predictability across different states.

In order to examine the role of housing supply elasticity, we calculate the difference in predictability of non-tradable and tradable employment across states for h=1-12 forecast horizon. The predictability is measured by the ratio of RMSE of the AR(1) model with those of the VAR

21 Alternatively, one could argue that the analysis in this paper should be performed at MSA level. However, the data constraint is more binding at the MSA level.

models. After taking the difference, we take the average across time. This provides us a measure of the superiority of house prices in predicting non-tradable employment over tradable employment on average for the forecast sample for each state. We find that the correlation between this average difference in the predictability and supply elasticity is -0.25. When we regress this predictability difference measure on supply elasticity, we find that 10% of the variation in this difference can be explained by supply elasticity measure and the coefficient on the supply elasticity is significant at 5% and 10% significance levels (See Table 2.8). These results suggest that most states with inelastic housing supply based on geography are also the states with higher predictability of non-tradable employment when house prices are used as a predictor. This makes intuitive sense as the net worth channel will have a bigger impact on states with inelastic housing supply. We also examine the relationship between supply elasticity and predictability of house prices by looking at the relationship between house price volatility and the difference in predictability. The measure of volatility is simply the variance of the real house price growth for each state in our sample period. The regression result of predictability difference on this volatility measure is shown in Table 2.8. We find that the volatility measure explains 35 percent of the variation in the difference in predictability measure and is significant at all levels of significance.

Table 2.8. Regression estimates

	Dependent Variable: DIFF		
	Coefficient	Prob.	R-squared
Saiz Elasticity	-0.015	0.031	0.10
Volatility of house prices	0.149	0.000	0.35

The table reports the results of regression of the dependent variable (DIFF) on Saiz elasticity and on volatility of house prices. The dependent variable (DIFF) in each regression is the difference of the RMSE ratio of forecast horizon (h=1- 12) for non-tradable and tradable employment forecast. The standard errors are Heteroskedasticity and Autocorrelation Consistent.

2.5. Conclusions

In this paper, we investigate the impact of house prices on tradable and non-tradable employment in the dynamic setting. Since the shocks in a housing market are more persistent and permanent, it also has stronger consumption consequences relative to financial wealth. (Kishor 2007). Given this positive effect of housing wealth on consumption, the local housing market has a more significant impact on the non-tradable sector than the tradable sector, as Mian and Sufi (2014) identified the stronger effect of the change in local house prices on non-tradable employment compared to the tradable employment. Our estimation results of the panel VAR model and the impulse response analysis support the result of previous research by identifying a stronger and more persistent impact of house price shock on non-tradable employment. In contrast, building permits and personal income display a short-lived and mostly insignificant impact on both types of employment.

We also attempt to verify the asymmetric relevance of the local housing market and tradable and non-tradable employment by comparing the out-of-sample forecasting performance. If the local housing market carries information more relevant to non-tradable employment, the gain in forecasting performance should be much greater for non-tradable employment when the house price is included in the model. Our forecasting analysis presents the result strongly supporting those anticipations. For the majority of states, a model with house prices outperforms the AR benchmarks in predicting non-tradable employment, and this enhanced forecasting performance is much less noticeable for tradable employment. Additionally, we examine the factors affecting the difference in forecasting performance between employment types. Our results suggest the forecasting performance gap tends to be greater for the states with inelastic housing supply and greater house price volatility.

References: Chapter 2

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Appendix A: Chapter 1 Appendix

Appendix A1: Examination of the Other Exceptions (Adoption Timing of Autor et al.)

Table A1.1. Estimation result of the Public Policy law on Employment

Panel A. Full, Not Working	(1)	(2)	Panel B. Male, Not Working	(1)	(2)
Treatment window	3 years	4 years	Treatment window	3 years	4 years
Treat × Post	0.00179	-0.00112	Treat × Post	0.00850	0.00717
t stat_pid	(0.31)	(-0.19)	t stat_pid	(1.32)	(1.07)
t stat_State	(0.22)	(-0.14)	t stat_State	(1.42)	(1.20)
State/Indiv. FE	Y	Y	State/Indiv. FE	Y	Y
N	130553	133460	*N*	90155	92198
Panel C. Full, Unemployed	(1)	(2)	Panel D. Male, Unemployed	(1)	(2)
Treatment window	3 years	4 years	Treatment window	3 years	4 years
Treat × Post	0.00582	0.00418	Treat × Post	0.00783	0.00792
t stat_pid	(1.29)	(0.97)	t stat_pid	(1.70)	(1.83)
t stat_State	(0.84)	(0.58)	t stat_State	(1.28)	(1.27)
State/Indiv. FE	Y	Y	State/Indiv. FE	Y	Y
N	98482	100743	*N*	74762	76379

Notes: Not working means that the alternative to working is any form of not working while unemployed means that the alternative to working is not working but looking for work.
* p<0.10, ** p<0.05, *** p<0.01

Table A1.2. Estimation result of the Covenant of Good Faith law on Employment

Panel A. Full, Not Working	(1)	(2)	Panel B. Male, Not Working	(1)	(2)
Treatment window	3 years	4 years	Treatment window	3 years	4 years
Treat × Post	0.0111	0.0107	Treat × Post	0.0200	0.0163
t stat_pid	(0.98)	(0.93)	t stat_pid	(1.61)	(1.30)
t stat_State	(1.01)	(0.79)	t stat_State	(1.98)*	(1.32)
State/Indiv. FE	Y	Y	State/Indiv. FE	Y	Y
N	114794	104178	*N*	78835	71405
Panel C. Full, Unemployed	(1)	(2)	Panel D. Male, Unemployed	(1)	(2)
Treatment window	3 years	4 years	Treatment window	3 years	4 years
Treat × Post	0.0118	0.00834	Treat × Post	0.0124	0.0122
t stat_pid	(1.32)	(0.98)	t stat_pid	(1.32)	(1.36)
t stat_State	(1.44)	(1.02)	t stat_State	(1.41)	(1.33)
State/Indiv. FE	Y	Y	State/Indiv. FE	Y	Y
N	86425	78262	*N*	65215	58958

Notes: Not working means that the alternative to working is any form of not working while unemployed means that the alternative to working is not working but looking for work.
* p<0.10, ** p<0.05, *** p<0.01

Table A1.3. Estimation result of the impact of Public Policy law on Hourly Wage

Panel A. Full	(1)	(2)	Panel B. Male	(1)	(2)
Treatment window	3 years	4 years	Treatment window	3 years	4 years
Treat × Post	-0.00747	-0.00520	Treat × Post	-0.0115	-0.00698
t stat_pid	(-0.96)	(-0.65)	t stat_pid	(-1.30)	(-0.77)
t stat_State	(-0.55)	(-0.40)	t stat_State	(-0.78)	(-0.49)
State/Indiv. FE	Y	Y	State/Indiv. FE	Y	Y
Triple interaction	N	N	Triple interaction	N	N
N	75342	77261	*N*	58337	59755
Panel C. Full	(1)	(2)	Panel D. Male	(1)	(2)
Treatment window	3 years	4 years	Treatment window	3 years	4 years
Treat × Post	-0.0103	-0.0133	Treat × Post	-0.0199	-0.0208
t stat_pid	(-0.93)	(-1.23)	t stat_pid	(-1.59)	(-1.69) *
t stat_State	(-0.53)	(-0.71)	t stat_State	(-0.99)	(-1.05)
Treat × Post × Tenure	0.000257	0.000910	Treat × Post × Tenure	0.000931	0.00156
t stat_pid	(0.33)	(1.27)	t stat_pid	(1.08)	(1.97) **
t stat_State	(0.27)	(0.90)	t stat_State	(1.00)	(1.57)
State/Indiv. FE	Y	Y	State/Indiv. FE	Y	Y
Triple interaction	Y	Y	Triple interaction	Y	Y
N	75432	77261	*N*	58337	59755

* p<0.10, ** p<0.05, *** p<0.01

Table A1.4. Estimation result of the impact of Covenant of Good Faith law on Hourly Wage

Panel A. Full	(1)	(2)	Panel B. Male	(1)	(2)
Treatment window	3 years	4 years	Treatment window	3 years	4 years
Treat × Post	0.0122	0.0199	Treat × Post	0.00198	0.0102
t stat_pid	(0.78)	(1.28)	t stat_pid	(0.11)	(0.58)
t stat_State	(0.93)	(1.34)	t stat_State	(0.15)	(0.70)
State/Indiv. FE	Y	Y	State/Indiv. FE	Y	Y
Triple interaction	N	N	Triple interaction	N	N
N	66422	60048	*N*	51167	46191
Panel C. Full	(1)	(2)	Panel D. Male	(1)	(2)
Treatment window	3 years	4 years	Treatment window	3 years	4 years
Treat × Post	0.0179	0.0256	Treat × Post	-0.00134	-0.0268
t stat_pid	(0.80)	(1.19)	t stat_pid	(-0.05)	(-0.74)
t stat_State	(1.40)	(1.78) *	t stat_State	(-0.07)	(-1.06)
Treat × Post × Tenure	-0.00100	-0.00121	Treat × Post × Tenure	-0.00116	-0.000960
t stat_pid	(-0.50)	(-0.67)	t stat_pid	(-0.50)	(-0.46)
t stat_State	(-0.75)	(-2.01) *	t stat_State	(-1.20)	(-1.66)
State/Indiv. FE	Y	Y	State/Indiv. FE	Y	Y
Triple interaction	Y	Y	Triple interaction	Y	Y
N	66422	60048	*N*	51167	46191

* p<0.10, ** p<0.05, *** p<0.01

Appendix A2: Examination of Other Law Coding

Table A1.5. Estimation result of the Implied Contract law on Employment (by Walsh)

Panel A. Full, Not Working	(1)	(2)	Panel B. Male, Not Working	(1)	(2)
Treatment window	3 years	4 years	Treatment window	3 years	4 years
Treat × Post	0.00115	0.00139	Treat × Post	-0.00459	-0.00438
t stat_pid	(0.20)	(0.23)	t stat_pid	(-0.71)	(-0.65)
t stat_State	(0.14)	(0.16)	t stat_State	(-0.60)	(-0.53)
State/Indiv. FE	Y	Y	State/Indiv. FE	Y	Y
N	106109	111149	*N*	73649	77015
Panel C. Full, Unemployed	(1)	(2)	Panel D. Male, Unemployed	(1)	(2)
Treatment window	3 years	4 years	Treatment window	3 years	4 years
Treat × Post	0.00240	0.00291	Treat × Post	-0.00299	-0.00275
t stat_pid	(0.52)	(0.64)	t stat_pid	(-0.63)	(-0.60)
t stat_State	(0.33)	(0.45)	t stat_State	(-0.49)	(-0.46)
State/Indiv. FE	Y	Y	State/Indiv. FE	Y	Y
N	80124	83860	*N*	61378	64065

Notes: Not working means that the alternative to working is any form of not working while unemployed means that the alternative to working is not working but looking for work.

* p<0.10, ** p<0.05, *** p<0.01

Table A1.6. Estimation result of the Implied Contract law on Employment (by Dertouzos)

Panel A. Full, Not Working	(1)	(2)	Panel B. Male, Not Working	(1)	(2)
Treatment window	3 years	4 years	Treatment window	3 years	4 years
Treat × Post	-0.000614	0.0000713	Treat × Post	-0.0102	-0.00992
t stat_pid	(-0.10)	(0.01)	t stat_pid	(-1.53)	(-1.44)
t stat_State	(-0.07)	(0.01)	t stat_State	(-1.23)	(-1.14)
State/Indiv. FE	Y	Y	State/Indiv. FE	Y	Y
N	103142	108645	*N*	71402	75055
Panel C. Full, Unemployed	(1)	(2)	Panel D. Male, Unemployed	(1)	(2)
Treatment window	3 years	4 years	Treatment window	3 years	4 years
Treat × Post	-0.000585	-0.000996	Treat × Post	-0.00549	-0.00601
t stat_pid	(-0.12)	(-0.21)	t stat_pid	(-1.11)	(-1.28)
t stat_State	(-0.08)	(-0.16)	t stat_State	(-0.99)	(-1.19)
State/Indiv. FE	Y	Y	State/Indiv. FE	Y	Y
N	77768	81868	*N*	59293	62211

Notes: Not working means that the alternative to working is any form of not working while unemployed means that the alternative to working is not working but looking for work.
* p<0.10, ** p<0.05, *** p<0.01

Table A1.7. Estimation result of the impact of Implied Contract law on Hourly Wage (by Walsh)

Panel A. Full	(1)	(2)	Panel B. Male	(1)	(2)
Treatment window	3 years	4 years	Treatment window	3 years	4 years
Treat × Post	0.00634	0.00228	Treat × Post	0.00489	0.000869
t stat_pid	(0.85)	(0.30)	t stat_pid	(0.58)	(0.10)
t stat_State	(0.55)	(0.19)	t stat_State	(0.40)	(0.07)
State/Indiv. FE	Y	Y	State/Indiv. FE	Y	Y
Triple interaction	N	N	Triple interaction	N	N
N	60885	63695	*N*	47519	49657
Panel C. Full	(1)	(2)	Panel D. Male	(1)	(2)
Treatment window	3 years	4 years	Treatment window	3 years	4 years
Treat × Post	-0.0000310	-0.00725	Treat × Post	-0.00562	-0.0122
t stat_pid	(-0.00)	(-0.67)	t stat_pid	(-0.46)	(-1.00)
t stat_State	(-0.00)	(-0.52)	t stat_State	(-0.40)	(-0.81)
Treat × Post × Tenure	0.000583	0.000831	Treat × Post × Tenure	0.000984	0.00114
t stat_pid	(0.87)	(1.24)	t stat_pid	(1.34)	(1.56)
t stat_State	(0.92)	(1.11)	t stat_State	(1.55)	(1.50)
State/Indiv. FE	Y	Y	State/Indiv. FE	Y	Y
Triple interaction	Y	Y	Triple interaction	Y	Y
N	60885	63695	*N*	47519	49657

* p<0.10, ** p<0.05, *** p<0.01

Table A1.8. Estimation result of the impact of Implied Contract law on Hourly Wage (by Dertouzos)

Panel A. Full	(1)	(2)	Panel B. Male	(1)	(2)
Treatment window	3 years	4 years	Treatment window	3 years	4 years
Treat × Post	-0.00278	-0.00261	Treat × Post	-0.00822	-0.00614
t stat_pid	(-0.36)	(-0.32)	t stat_pid	(-0.94)	(-0.68)
t stat_State	(-0.20)	(-0.17)	t stat_State	(-0.55)	(-0.38)
State/Indiv. FE	Y	Y	State/Indiv. FE	Y	Y
Triple interaction	N	N	Triple interaction	N	N
N	59020	62248	*N*	45864	48286
Panel C. Full	(1)	(2)	Panel D. Male	(1)	(2)
Treatment window	3 years	4 years	Treatment window	3 years	4 years
Treat × Post	-0.0161	-0.0148	Treat × Post	-0.0283	-0.0237
t stat_pid	(-1.42)	(-1.30)	t stat_pid	(-2.20) **	(-1.84) *
t stat_State	(-0.92)	(-0.79)	t stat_State	(-1.51)	(-1.20)
Treat × Post × Tenure	0.00137	0.00108	Treat × Post × Tenure	0.00205	0.00164
t stat_pid	(1.91)	(1.53)	t stat_pid	(2.65) ***	(2.12) **
t stat_State	(1.50)	(1.05)	t stat_State	(2.31) **	(1.59)
State/Indiv. FE	Y	Y	State/Indiv. FE	Y	Y
Triple interaction	Y	Y	Triple interaction	Y	Y
N	59020	62248	*N*	45864	48286

* p<0.10, ** p<0.05, *** p<0.01

Appendix A3: State by State estimates

Table A1.9. Estimation result of the impact of Implied contract law on Male Employment by State (Never Treated Control group)

			Panel A 3year Pre-Post		
State	Year	TE	t_pid	t_state	N
Michigan	1980	-0.0396	(-1.20)	(-11.09)	2918
New Mexico	1980	-0.0391	(-0.38)	(-2.27)	1632
Tennessee	1981	-0.0570	(-1.32)	(-7.59)	2130
New York	1982	-0.0041	(-0.15)	(-0.44)	2902
Ohio	1982	-0.0222	(-0.75)	(-1.71)	3020
Arizona	1983	-0.0527	(-1.01)	(-2.79)	2138
Colorado	1983	0.0081	(0.26)	(0.45)	2085
Kentucky	1983	-0.0043	(-0.12)	(-0.17)	2237
Minnesota	1983	-0.0377	(-0.98)	(-2.73)	2098
Missouri	1983	0.0293	(0.90)	(1.51)	2508
Nebraska	1983	-0.0259	(-0.63)	(-5.69)	1892
Nevada	1983	-0.0717	(-0.52)	(-1.59)	1771
South Dakota	1983	0.0094	(0.11)	(0.43)	1837
Virginia	1983	-0.0366	(-1.27)	(-1.99)	2622
Alaska	1983	-0.1370	(-1.13)	(-10.31)	1728
Arkansas	1984	-0.0449	(-1.15)	(-2.12)	2392
Kansas	1984	0.0732	(0.70)	(4.15)	1823
North Dakota	1984	-0.1620	(-1.35)	(-14.51)	1758
Connecticut	1985	-0.0114	(-0.20)	(-1.57)	2039
Maryland	1985	-0.0079	(-0.25)	(-0.52)	2841
New Jersey	1985	-0.0353	(-1.09)	(-2.41)	2542
Texas	1985	-0.0473	(-1.75)	(-3.29)	3596
Vermont	1985	0.7540	(17.23)	(14.91)	1802
Wisconsin	1985	-0.0264	(-0.55)	(-1.97)	2111
Wyoming	1985	-0.0015	(-0.08)	(-5.91)	1812
Utah	1986	-0.0663	(-0.88)	(-3.89)	2430
West Virginia	1986	-0.0675	(-0.77)	(-4.18)	2349
Hawaii	1986	0.4310	(2.13)	(13.41)	2261
Alabama	1987	0.0138	(0.32)	(1.05)	3011
Indiana	1987	0.0019	(0.05)	(0.12)	3377
Iowa	1987	0.0364	(1.23)	(3.03)	3248
Montana	1987	0.0913	(3.26)	(3.47)	2628
South Carolina	1987	-0.0264	(-1.00)	(-1.87)	4230
Massachusetts	1988	0.0186	(0.56)	(1.12)	3789
New Hampshire	1988	-0.0168	(-0.18)	(-1.26)	3097
Mississippi	1992	-0.0212	(-0.73)	(-0.89)	4558

Table A1.10. Estimation result of the impact of Implied contract law on Male Hourly Wages by State (Never Treated Control group)

State	Year	TE	t_pid	t_state	TE × Tenure	t_pid	t_state	N
Michigan	1980	-0.0584	(-0.86)	(-1.23)	0.0028	(0.68)	(2.51)	1809
New Mexico	1980	0.1660	(0.74)	(2.58)	-0.0201	(-0.93)	(-1.31)	1046
Tennessee	1981	-0.1940	(-1.98)	(-5.02)	0.0093	(1.19)	(5.20)	1440
New York	1982	0.0142	(0.25)	(0.70)	-0.0011	(-0.34)	(-1.68)	1935
Ohio	1982	-0.2040	(-3.44)	(-8.22)	0.0079	(2.75)	(8.09)	2002
Arizona	1983	0.0489	(0.48)	(1.49)	-0.0097	(-1.23)	(-3.18)	1374
Colorado	1983	-0.1330	(-1.14)	(-7.82)	0.0070	(1.09)	(1.50)	1397
Kentucky	1983	-0.0761	(-0.80)	(-13.18)	0.0024	(0.51)	(1.33)	1468
Minnesota	1983	-0.0840	(-1.17)	(-5.23)	0.0038	(0.94)	(1.06)	1390
Missouri	1983	0.0025	(0.03)	(0.34)	-0.0057	(-0.99)	(-3.49)	1614
Nebraska	1983	-0.2720	(-1.85)	(-38.26)	0.0158	(1.12)	(3.09)	1198
Nevada	1983	-0.3230	(-1.01)	(-9.90)	0.0193	(0.29)	(2.11)	1146
South Dakota	1983	-0.0907	(-1.03)	(-2.61)	0.0006	(0.10)	(0.18)	1193
Virginia	1983	0.0438	(0.65)	(3.74)	-0.0025	(-0.52)	(-2.64)	1796
Alaska	1983	0.2650	(1.45)	(50.49)	-0.0515	(-1.35)	(-13.10)	1130
Arkansas	1984	-0.1120	(-1.27)	(-8.19)	0.0024	(0.41)	(0.87)	1510
Kansas	1984	0.0133	(0.10)	(3.13)	-0.0109	(-0.53)	(-3.21)	1195
North Dakota	1984	-0.7870	(-1.73)	(-23.56)	0.1030	(1.52)	(33.51)	1153
Connecticut	1985	0.1980	(2.31)	(6.21)	-0.0090	(-1.80)	(-2.88)	1368
Maryland	1985	-0.0494	(-0.81)	(-6.25)	0.0030	(0.84)	(2.45)	1889
New Jersey	1985	0.0008	(0.02)	(0.10)	0.0007	(0.21)	(0.35)	1692
Texas	1985	-0.1610	(-2.87)	(-6.34)	0.0044	(1.18)	(1.62)	2340
Vermont	1985	0.0000	(.)	(.)	0.0000	(.)	(.)	797
Wisconsin	1985	-0.0531	(-0.83)	(-3.48)	0.0018	(0.50)	(1.10)	1406
Wyoming	1985	0.0177	(0.05)	(0.07)	-0.0368	(-0.66)	(-4.67)	1184
Utah	1986	-0.1260	(-1.14)	(-2.07)	0.0005	(0.06)	(0.09)	1532
West Virginia	1986	0.1540	(1.24)	(2.48)	-0.0201	(-2.22)	(-4.42)	1454
Hawaii	1986	-0.6170	(-3.67)	(-23.80)	0.0689	(1.32)	(9.47)	1424
Alabama	1987	0.0459	(0.77)	(8.16)	-0.0015	(-0.41)	(-0.49)	1836
Indiana	1987	-0.0994	(-1.50)	(-4.98)	0.0022	(0.51)	(0.95)	2140
Iowa	1987	0.0045	(0.04)	(1.11)	0.0015	(0.22)	(0.43)	1946
Montana	1987	0.0000	(.)	(.)	0.2330	(1.61)	(6.17)	1584
South Carolina	1987	-0.0071	(-0.14)	(-0.43)	-0.0011	(-0.32)	(-0.74)	2618
Massachusetts	1988	0.0150	(0.24)	(0.94)	0.0008	(0.23)	(0.35)	2195
New Hampshire	1988	0.0720	(0.53)	(1.85)	0.0033	(0.09)	(0.75)	1791
Mississippi	1992	-0.0016	(-0.02)	(-0.07)	-0.0012	(-0.25)	(-0.67)	2695

Appendix A4: Occupation, Industry Sub-Sample, and Volatility

Table A1.11. Occupation and Industry Codes

	Occupation		Industry
1	Professional, Technical, and Kindred Workers	1	Agriculture, Forestry, and Fisheries
2	Administrators and Managers Except Farm	2	Mining
3	Sales Workers	3	Construction
4	Clerical and Kindred Workers	4	Manufacturing
5	Craftsmen and Kindred Workers	5	Transport., Commu., and Other Pub. Utilities
6	Operatives, Except Transport	6	Wholesale and Retail Trade
7	Transport Equipment Operatives	7	Finance, Insurance, and Real Estate
8	Laborers, Except Farm	8	Business and Repair Services
9	Farmers and Farm Managers	9	Personal Services
10	Farm Laborers and Farm Foremen	10	Entertainment and Recreation Services 1
11	Service Workers, Except Priv. Household	11	Professional and Related Services
		12	Public Admin.

PSID occupation and industry classification system based on 1970 Census classification

Tabel A1.12. Estimation results of the impact of Implied contract on Male Wage by Occupation (Grouped)

Panel A. 2, 4, 5, 6, 7, 11	(1)	(2)	Panel B. 2, 4, 5, 6, 7, 11	(1)	(2)
Treatment window	3 years	4 years	Treatment window	3 years	4 years
Treat × Post	-0.0496	-0.0421	Treat × Post	-0.0405	-0.0282
t stat_pid	(-3.44) ***	(-3.03) ***	t stat_pid	(-2.67)***	(-1.89) *
t stat_State	(-3.05) ***	(-2.51) **	t stat_State	(-2.20)**	(-1.50)
Treat × Post × Tenure	0.00302	0.00267	Treat × Post × Tenure	0.00184	0.00112
t stat_pid	(3.54) ***	(3.23) ***	t stat_pid	(2.08) **	(1.27)
t stat_State	(4.96) ***	(3.60) ***	t stat_State	(2.86) ***	(1.55)
Tenure × State, Tenure × Year	N	N	Tenure × State, Tenure × Year	Y	Y
N	37029	40995	*N*	37029	40995
Panel C. 2, 4, 5, 6, 7	(1)	(2)	Panel D. 2, 4, 5, 6, 7	(1)	(2)
Treatment window	3 years	4 years	Treatment window	3 years	4 years
Treat × Post	-0.0441	-0.0347	Treat × Post	-0.0367	-0.0225
t stat_pid	(-2.88) ***	(-2.36) **	t stat_pid	(-2.27) **	(-1.43)
t stat_State	(-3.07) ***	(-2.35) **	t stat_State	(-2.30) **	(-1.44)
Treat × Post × Tenure	0.00308	0.00264	Treat × Post × Tenure	0.00213	0.00131
t stat_pid	(3.52) ***	(3.10) ***	t stat_pid	(2.33) **	(1.43)
t stat_State	(5.37) ***	(3.91) ***	t stat_State	(3.44) ***	(2.00) *
Tenure × State, Tenure × Year	N	N	Tenure × State, Tenure × Year	Y	Y
N	32749	36218	*N*	32749	36218
Panel E. 2, 4, 5, 6	(1)	(2)	Panel F. 2, 4, 5, 6	(1)	(2)
Treatment window	3 years	4 years	Treatment window	3 years	4 years
Treat × Post	-0.0459	-0.0348	Treat × Post	-0.0411	-0.0266
t stat_pid	(-2.81) ***	(-2.21) **	t stat_pid	(-2.38) **	(-1.57)
t stat_State	(-2.77) ***	(-2.12) **	t stat_State	(-2.34) **	(-1.60)
Treat × Post × Tenure	0.00344	0.00280	Treat × Post × Tenure	0.00275	0.00188
t stat_pid	(3.76) ***	(3.13) ***	t stat_pid	(2.84) ***	(1.91) *
t stat_State	(4.55) ***	(3.45) ***	t stat_State	(3.70) ***	(2.55) **
Tenure × State, Tenure × Year	N	N	Tenure × State, Tenure × Year	Y	Y
N	28662	31688	*N*	28662	31688

Numbers assigned to each panel refer to the occupation code specified in Table A11.
* p<0.10, ** p<0.05, *** p<0.01

Table A1.13. Estimation results of the impact of Implied contract on Male Wage by Industry (Grouped)

Panel A. 1, 4, 5, 6, 9	(1)	(2)	Panel B. 1, 4, 5, 6, 9	(1)	(2)
Treatment window	3 years	4 years	Treatment window	3 years	4 years
Treat × Post	-0.0412	-0.0423	Treat × Post	-0.0336	-0.0319
t stat_pid	(-2.55) **	(-2.70) ***	t stat_pid	(-1.97) **	(-1.88) *
t stat_State	(-2.50) **	(-2.52) **	t stat_State	(-1.81) *	(-1.69) *
Treat × Post × Tenure	0.00248	0.00269	Treat × Post × Tenure	0.00167	0.00155
t stat_pid	(2.82) ***	(3.18) ***	t stat_pid	(1.82) *	(1.69) *
t stat_State	(3.22) ***	(3.09) ***	t stat_State	(2.00) *	(1.69) *
Tenure × State, Tenure × Year	N	N	Tenure × State, Tenure × Year	Y	Y
N	30608	33791	*N*	30608	33791
Panel C. 4, 5, 6	(1)	(2)	Panel D. 4, 5, 6	(1)	(2)
Treatment window	3 years	4 years	Treatment window	3 years	4 years
Treat × Post	-0.0375	-0.0364	Treat × Post	-0.0313	-0.0275
t stat_pid	(-2.29) **	(-2.29) **	t stat_pid	(-1.81) *	(-1.59)
t stat_State	(-2.46) **	(-2.29) **	t stat_State	(-1.82) *	(-1.55)
Treat × Post × Tenure	0.00220	0.00233	Treat × Post × Tenure	0.00157	0.00140
t stat_pid	(2.48) **	(2.72) ***	t stat_pid	(1.70) *	(1.51)
t stat_State	(2.89) ***	(2.74) ***	t stat_State	(1.99) *	(1.55)
Tenure × State, Tenure × Year	N	N	Tenure × State, Tenure × Year	Y	Y
N	28589	31553	*N*	29295	32326

Numbers assigned to each panel refer to the industry code specified in Table A11.
* p<0.10, ** p<0.05, *** p<0.01

Table A1.14. Estimation results of the impact of Implied contract on Employment and Wage Variability

Panel A. Not working Squared Deviation	(1)	(2)	Panel B. Unemployed Squared Deviation	(1)	(2)
Treatment window	3 years	4 years	Treatment window	3 years	4 years
Treat × Post	0.00425	0.00629	Treat × Post	0.00268	0.00285
t stat_pid	(1.42)	(2.05) **	t stat_pid	(1.02)	(1.13)
t stat_State	(1.02)	(1.38)	t stat_State	(1.09)	(1.19)
N	84937	94109	*N*	70616	78044
Panel C. Wage Squared Deviation	**(1)**	**(2)**	**Panel D. Wage Squared Deviation**	**(1)**	**(2)**
Treatment window	3 years	4 years	Treatment window	3 years	4 years
Treat × Post	0.0459	0.0514	Treat × Post	0.0445	0.0509
t stat_pid	(3.06) ***	(3.42) ***	t stat_pid	(2.88) ***	(3.21) ***
t stat_State	(1.91) *	(1.71) *	t stat_State	(1.93) *	(1.79) *
Treat × Post × Tenure	-0.00242	-0.00220	Treat × Post × Tenure	-0.00246	-0.00262
t stat_pid	(-2.89) ***	(-2.71) ***	t stat_pid	(-2.77) ***	(-2.90) ***
t stat_State	(-2.48) **	(-1.80) *	t stat_State	(-2.30) **	(-2.00) *
Tenure × State, Tenure × Year	N	N	Tenure × State, Tenure × Year	Y	Y
N	55457	61228	*N*	55457	61228
Panel E. Wage Absolute Deviation	**(1)**	**(2)**	**Panel F. Wage Absolute Deviation**	**(1)**	**(2)**
Treatment window	3 years	4 years	Treatment window	3 years	4 years
Treat × Post	0.0229	0.0236	Treat × Post	0.0256	0.0258
t stat_pid	(2.23) **	(2.37) **	t stat_pid	(2.40) **	(2.45) **
t stat_State	(1.34)	(1.16)	t stat_State	(1.54)	(1.32)
Treat × Post × Tenure	-0.00077	-0.00055	Treat × Post × Tenure	-0.00131	-0.00122
t stat_pid	(-1.20)	(-0.90)	t stat_pid	(-1.92) *	(-1.80) *
t stat_State	(-1.17)	(-0.77)	t stat_State	(-1.62)	(-1.36)
Tenure × State, Tenure × Year	N	N	Tenure × State, Tenure × Year	Y	Y
N	55457	61228	*N*	55457	61228

* p<0.10, ** p<0.05, *** p<0.01

Appendix B: Chapter 2 Appendix

Tables A2.1. Forecasting performance (2007:M1-2020:M6): House prices

Predictor	**Forecasted Variable**													
House Prices	**Tradable Employment**							**Non-Tradable Employment**						
State	h=1	h=2	h=3	h=6	h=9	h=12	h=Avg	h=1	h=2	h=3	h=6	h=9	h=12	h=Avg
Alabama	1.0289	1.0096	0.9927	1.0043	0.9976	0.9967	1.0256	0.9876	0.9988	0.9970	0.9995	0.9996	0.9994	1.0616
Alaska	0.9857	0.9886	0.9876	0.9897	0.9917	0.9942	0.8856	0.9989	0.9999	1.0006	1.0002	1.0005	1.0006	1.0158
Arizona	0.9077	0.9623	0.9861	1.0052	1.0043	1.0006	1.0315	1.0368	1.0051	1.0039	1.0108	1.0086	1.0085	1.0953
Arkansas	1.0183	1.0076	0.9971	0.9952	1.0041	0.9919	0.9960	1.0113	1.0012	1.0001	0.9997	1.0014	0.9994	1.0469
California	1.0019	1.0012	1.0035	1.0025	1.0020	1.0006	1.0279	0.9788	0.9796	1.0051	1.0056	1.0055	1.0049	1.2021
Colorado	0.9962	0.9924	0.9967	0.9954	0.9952	0.9939	0.9626	1.0299	0.9962	1.0006	1.0020	1.0006	1.0002	1.0789
Connecticut	0.9938	0.9946	0.9944	0.9912	0.9904	0.9889	0.8885	0.9912	0.9971	0.9991	0.9999	0.9992	0.9986	1.0218
Florida	1.0315	1.0353	1.0249	1.0289	1.0364	1.0285	1.3103	1.0756	1.0016	1.0117	1.0154	1.0132	1.0083	1.3919
Georgia	1.0331	1.0038	1.0053	1.0075	1.0049	0.9957	1.0634	1.0445	0.9894	1.0024	1.0045	1.0026	0.9977	1.1135
Idaho	1.0382	1.0147	1.0159	1.0084	1.0090	0.9913	1.1035	1.0551	0.9284	1.0120	1.0104	1.0020	1.0003	1.2293
Illinois	0.9990	1.0003	1.0000	0.9983	0.9979	0.9969	0.9857	0.9054	0.8372	1.0004	0.9997	0.9986	0.9962	0.9222
Indiana	0.9745	0.9848	1.0032	0.9993	0.9999	0.9995	1.0195	0.9628	0.9856	1.0006	0.9988	0.9995	0.9997	1.0128
Kansas	0.9924	0.9928	0.9925	0.9972	0.9969	0.9975	0.9576	0.9900	0.9961	0.9992	0.9999	0.9996	0.9996	0.9839
Kentucky	0.9905	0.9817	0.9969	0.9998	1.0002	0.9995	0.9965	1.0013	0.9988	0.9973	0.9992	0.9999	0.9995	1.0016
Louisiana	0.9965	0.9970	0.9946	0.9934	0.9963	0.9954	0.9544	0.9931	0.9966	0.9992	0.9992	1.0002	0.9998	1.0511
Iowa	0.9971	0.9976	0.9936	0.9952	0.9966	0.9960	0.9654	0.9919	0.9997	0.9996	0.9999	0.9997	0.9998	1.0055
Maryland	0.9982	1.0009	1.0014	0.9984	1.0019	1.0008	1.0190	0.9863	0.9918	1.0019	0.9997	1.0016	0.9995	1.1013
Massachusetts	1.0011	0.9959	0.9911	0.9917	0.9871	0.9861	0.8832	0.9564	0.9504	0.9997	0.9995	0.9995	0.9993	1.0406
Michigan	1.0124	1.0034	1.0070	1.0049	1.0033	1.0013	1.1053	0.9914	1.0008	1.0009	1.0006	1.0007	1.0001	1.0810
Minnesota	0.9919	0.9912	0.9903	0.9872	0.9850	0.9826	0.9040	0.9801	0.9917	1.0000	0.9998	0.9996	0.9997	1.0404
Mississippi	1.0029	1.0002	0.9989	1.0010	0.9984	0.9987	1.0001	0.9922	0.9966	1.0002	1.0002	0.9995	0.9994	1.0286
Missouri	1.0058	0.9981	0.9949	0.9944	0.9955	0.9893	0.9546	1.0032	0.9990	0.9992	0.9989	1.0001	0.9994	1.0363
Montana	1.0277	1.0048	0.9967	1.0003	0.9932	0.9951	1.0012	1.0496	1.0001	0.9956	1.0014	0.9985	0.9980	1.0621
Nebraska	0.9954	0.9951	0.9961	0.9959	0.9972	0.9971	0.9504	0.9871	0.9933	0.9986	0.9997	1.0005	0.9997	1.0345
Nevada	1.0555	1.0688	1.0127	1.0250	1.0263	1.0262	1.3553	0.9780	0.9704	1.0032	1.0094	1.0107	1.0099	1.3857
New Hampshire	0.9950	0.9944	0.9928	0.9898	0.9865	0.9840	0.8959	1.0046	0.9965	0.9986	1.0001	1.0001	0.9989	1.0374
New Jersey	0.9968	0.9967	0.9965	0.9954	0.9926	0.9912	0.9124	0.9879	0.9885	0.9994	1.0001	0.9995	0.9985	1.0599
New Mexico	0.9887	0.9950	0.9904	0.9939	0.9969	0.9962	1.0229	1.0180	1.0068	0.9995	1.0024	1.0002	0.9969	1.0874
New York	1.0009	0.9992	0.9979	0.9963	0.9960	0.9970	0.9451	0.9457	0.9429	0.9998	0.9997	0.9997	0.9997	1.0674
North Carolina	1.0230	1.0010	0.9980	0.9941	0.9996	0.9939	0.9975	1.0169	1.0011	1.0002	0.9962	1.0001	0.9972	1.0163
North Dakota	1.0134	1.0191	1.0199	1.0081	1.0020	1.0013	1.0059	1.0494	1.0333	1.0155	1.0034	1.0036	1.0022	1.0539
Ohio	0.9932	0.9942	1.0001	0.9983	0.9983	0.9978	1.0007	0.9422	0.9353	1.0000	0.9997	1.0000	0.9997	1.0071
Oklahoma	0.9873	0.9886	0.9920	0.9989	0.9965	0.9980	0.9878	0.9855	0.9936	0.9988	1.0018	1.0002	1.0006	0.9978
Oregon	1.0152	1.0035	1.0074	1.0020	1.0032	0.9916	1.0414	1.0508	1.0302	0.9984	1.0028	1.0009	0.9978	1.0922
Pennsylvania	1.0002	0.9992	0.9975	0.9966	0.9965	0.9970	0.9452	0.9840	0.9987	0.9999	0.9997	0.9997	0.9997	1.0226
South Carolina	0.9938	0.9928	0.9985	0.9966	0.9984	0.9950	0.9796	1.0033	0.9894	1.0024	0.9991	0.9997	0.9977	1.0193
South Dakota	1.0044	0.9950	0.9955	0.9971	0.9957	0.9939	0.9761	0.9972	0.9996	0.9997	1.0003	1.0003	0.9999	1.0183
Tennessee	1.0239	0.9937	0.9931	1.0024	1.0019	0.9981	1.0493	1.0201	0.9912	0.9960	1.0009	0.9990	0.9955	1.0315
Texas	0.9958	0.9942	0.9929	0.9917	0.9920	0.9938	0.9655	1.0200	0.9987	0.9980	1.0005	1.0000	1.0001	1.0422
Utah	1.0678	1.0276	0.9972	1.0174	1.0149	0.9978	1.1088	1.0470	1.0497	1.0058	1.0159	1.0072	0.9927	1.1813
Vermont	0.9998	0.9973	0.9973	0.9990	0.9986	0.9990	0.9655	1.0079	0.9974	0.9994	1.0002	1.0002	1.0002	1.1063
Virginia	1.0046	0.9967	0.9968	0.9963	0.9953	0.9926	0.9551	0.9921	0.9910	0.9997	1.0008	0.9990	0.9981	1.0445
Washington	0.9929	0.9984	0.9986	0.9989	0.9991	0.9981	0.9819	1.0102	1.0092	1.0014	1.0049	1.0046	0.9996	0.8490
West Virginia	1.0011	1.0085	1.0043	0.9983	0.9923	0.9973	0.9954	0.9996	1.0035	1.0091	1.0012	1.0022	0.9935	0.9927
Wisconsin	0.9937	0.9961	0.9957	0.9907	0.9886	0.9898	0.9372	0.9736	0.9858	0.9997	0.9994	0.9989	0.9993	0.9945
AVG	1.0037	1.0003	0.9986	0.9994	0.9990	0.9966	1.0004	1.0008	0.9899	1.0011	1.0018	1.0013	0.9997	1.0615
MIN	0.9077	0.9623	0.9861	0.9872	0.9850	0.9826	0.8832	0.9054	0.8372	0.9956	0.9962	0.9985	0.9927	0.8490
MAX	1.0678	1.0688	1.0249	1.0289	1.0364	1.0285	1.3553	1.0756	1.0497	1.0155	1.0159	1.0132	1.0099	1.3919

The table presents, for each forecast horizon (h), the ratio of RMSE from the benchmark AR(1) model with real house price growth to RMSE from the VAR model in predicting tradable and non-tradable employment. The estimation sample for the first forecast is 2001:M1-2006:M12, which is recursively updated for the forecast sample 2007:M1-2020:M12.

Table A2.2. Forecasting performance [2019:M1-2020:M6]: House Prices

Predictor	**Forecasted Variable**													
House Price	**Tradable Employment**							**Non-Tradable Employment**						
State	h=1	h=2	h=3	h=6	h=9	h=12	h=Avg	h=1	h=2	h=3	h=6	h=9	h=12	h=Avg
Alabama	1.0315	1.0122	0.9902	0.9954	0.9984	0.9999	1.0258	0.9867	0.9975	0.9947	0.9982	0.9995	1.0000	1.1033
Alaska	1.0005	1.0002	1.0002	1.0000	1.0000	0.9999	0.9951	0.9987	0.9997	1.0008	1.0006	1.0007	1.0007	1.0776
Arizona	0.9937	0.9931	0.9952	1.0000	0.9983	0.9980	0.9617	1.0003	0.9937	0.9966	1.0003	0.9985	0.9988	0.9977
Arkansas	1.0187	1.0092	1.0042	0.9967	1.0008	0.9995	1.0109	1.0103	1.0002	1.0006	0.9994	1.0006	0.9997	1.0753
California	1.0003	0.9987	1.0009	1.0004	1.0005	0.9999	1.0017	0.9774	0.9770	1.0009	1.0005	1.0004	1.0000	1.1021
Colorado	0.9962	0.9925	0.9986	0.9999	0.9998	0.9996	0.9980	1.0299	0.9947	0.9985	1.0004	0.9997	0.9995	1.0914
Connecticut	0.9953	0.9976	0.9998	1.0001	1.0000	1.0000	0.9999	0.9909	0.9972	0.9994	1.0005	1.0000	0.9997	1.0862
Florida	1.0229	1.0231	0.9974	0.9998	1.0009	1.0005	0.9981	1.0687	0.9915	0.9988	1.0007	1.0009	1.0004	1.0339
Georgia	1.0322	0.9945	0.9912	0.9979	1.0006	0.9961	0.9142	1.0415	0.9829	0.9945	0.9996	1.0007	0.9984	0.9889
Idaho	1.0409	0.9920	0.9919	0.9908	0.9862	0.9872	0.7541	1.0507	0.9105	0.9967	0.9971	0.9946	0.9953	0.8917
Illinois	0.9947	0.9987	1.0004	1.0000	1.0019	0.9993	0.9904	0.9037	0.8344	0.9997	0.9999	1.0008	0.9994	1.0971
Indiana	0.9721	0.9802	0.9989	0.9962	0.9992	0.9996	0.9417	0.9608	0.9836	0.9992	0.9986	0.9996	0.9999	1.0314
Kansas	0.9933	0.9957	0.9959	0.9995	0.9996	0.9996	1.0024	0.9899	0.9962	0.9994	1.0000	1.0000	0.9998	1.0186
Kentucky	0.9886	0.9797	0.9961	0.9996	1.0004	0.9997	0.9827	1.0001	0.9983	0.9970	0.9996	1.0003	0.9998	1.0437
Louisiana	0.9986	0.9988	0.9998	0.9997	1.0005	1.0001	1.0031	0.9934	0.9968	0.9995	0.9993	1.0002	0.9998	1.0762
Iowa	0.9872	0.9927	0.9942	0.9988	0.9999	1.0006	0.9805	0.9918	0.9998	0.9998	1.0000	0.9999	1.0001	1.0441
Maryland	0.9962	0.9995	1.0007	0.9986	1.0014	1.0003	1.0084	0.9850	0.9902	1.0011	0.9988	1.0011	0.9997	1.0949
Massachusetts	1.0034	1.0041	1.0017	1.0016	0.9997	1.0000	1.0051	0.9559	0.9496	0.9999	0.9999	1.0001	1.0000	1.0886
Michigan	1.0039	0.9942	1.0005	0.9993	1.0003	0.9995	0.9577	0.9906	0.9997	0.9996	0.9996	1.0002	0.9999	1.0726
Minnesota	0.9944	0.9955	0.9966	0.9985	0.9990	1.0002	0.9890	0.9787	0.9905	0.9990	0.9994	0.9999	1.0006	1.0652
Mississippi	1.0028	0.9996	0.9977	0.9999	0.9997	0.9997	0.9860	0.9912	0.9954	0.9990	1.0000	0.9999	0.9999	1.0539
Missouri	1.0070	0.9969	0.9944	0.9963	1.0005	0.9981	0.9634	1.0028	0.9979	0.9978	0.9985	1.0003	0.9993	1.0559
Montana	1.0268	0.9982	0.9885	0.9984	0.9989	0.9972	0.9591	1.0501	0.9983	0.9948	0.9995	0.9996	0.9991	1.1061
Nebraska	0.9959	0.9953	0.9977	0.9998	0.9996	0.9997	0.9934	0.9865	0.9933	0.9991	1.0000	1.0000	0.9998	1.0726
Nevada	1.0516	1.0606	0.9933	1.0019	1.0028	1.0024	1.0407	0.9750	0.9666	0.9951	1.0006	1.0015	1.0015	1.1416
New Hampshire	0.9982	0.9992	0.9992	0.9998	0.9998	0.9996	0.9946	1.0041	0.9959	0.9981	0.9999	1.0000	0.9993	1.0611
New Jersey	0.9974	0.9980	0.9989	1.0002	1.0003	0.9998	0.9986	0.9874	0.9880	0.9992	1.0003	1.0002	0.9997	1.1124
New Mexico	0.9824	0.9819	0.9826	0.9897	0.9916	0.9894	0.9563	1.0166	1.0028	0.9953	0.9991	0.9981	0.9958	0.9695
New York	1.0014	1.0009	1.0004	0.9993	0.9995	0.9997	0.9988	0.9455	0.9425	1.0000	1.0000	1.0000	1.0000	1.1022
North Carolina	1.0253	1.0007	0.9947	0.9904	1.0007	0.9965	0.9616	1.0172	0.9990	0.9972	0.9957	1.0019	0.9987	1.0381
North Dakota	1.0203	1.0230	1.0276	1.0133	1.0076	1.0063	1.1001	1.0517	1.0313	1.0126	1.0026	1.0024	1.0011	1.1114
Ohio	0.9927	0.9923	0.9970	0.9973	0.9983	0.9991	0.9633	0.9411	0.9333	0.9994	0.9996	0.9999	0.9999	1.0115
Oklahoma	0.9915	0.9921	0.9948	1.0014	0.9997	1.0011	1.0107	0.9868	0.9935	0.9996	1.0007	0.9998	1.0003	1.0035
Oregon	1.0135	0.9934	0.9986	0.9987	1.0004	1.0018	1.0198	1.0508	1.0302	0.9964	0.9995	0.9999	1.0003	1.1219
Pennsylvania	1.0005	0.9996	0.9989	0.9995	0.9999	0.9999	0.9899	0.9839	0.9987	1.0000	0.9999	1.0000	1.0000	1.0579
South Carolina	0.9933	0.9915	0.9965	0.9963	1.0015	0.9974	0.9537	1.0015	0.9866	0.9987	0.9985	1.0003	0.9992	1.0115
South Dakota	1.0045	0.9950	0.9916	0.9978	0.9982	1.0000	1.0080	0.9976	0.9995	0.9998	1.0000	1.0000	1.0000	1.0402
Tennessee	1.0243	0.9889	0.9850	0.9956	0.9979	0.9951	0.9421	1.0191	0.9884	0.9931	0.9976	0.9995	0.9974	1.0193
Texas	0.9961	0.9939	0.9950	0.9995	1.0017	1.0004	1.0037	1.0201	0.9977	0.9970	0.9999	0.9999	0.9998	1.0498
Utah	1.0763	1.0242	0.9812	0.9922	0.9958	0.9941	0.8962	1.0437	1.0413	0.9878	0.9979	0.9983	0.9972	0.9693
Vermont	1.0009	0.9985	0.9987	1.0002	1.0002	0.9999	0.9980	1.0080	0.9973	0.9993	1.0001	1.0000	0.9999	1.1194
Virginia	1.0049	0.9961	0.9967	1.0002	0.9996	0.9982	0.9810	0.9915	0.9894	0.9983	1.0010	0.9997	0.9991	1.0611
Washington	0.9888	0.9830	0.9882	0.9939	0.9976	0.9963	0.9635	1.0092	1.0070	0.9956	0.9979	1.0001	0.9981	1.0222
West Virginia	1.0582	1.0952	1.1115	1.0522	1.0221	1.0137	1.0413	0.9956	1.0131	1.0220	1.0126	0.9985	1.0418	0.9609
Wisconsin	0.9934	0.9959	0.9973	0.9982	0.9979	0.9995	0.9781	0.9725	0.9848	0.9996	0.9998	0.9996	1.0001	1.0478
AVG	1.0069	1.0010	0.9991	0.9997	1.0000	0.9992	0.9827	0.9990	0.9879	0.9989	0.9999	0.9999	1.0004	1.0534
MIN	0.9721	0.9797	0.9812	0.9897	0.9862	0.9872	0.7541	0.9037	0.8344	0.9878	0.9957	0.9946	0.9953	0.8917
MAX	1.0763	1.0952	1.1115	1.0522	1.0221	1.0137	1.1001	1.0687	1.0413	1.0220	1.0126	1.0024	1.0418	1.1416

The table presents, for each forecast horizon (h), the ratio of RMSE from the benchmark AR(1) model with changes in housing Prices to RMSE from the VAR model in predicting tradable and non-tradable employment. The estimation sample for the first forecast is 2001:M1-2018:M12, which is recursively updated for the forecast sample 2019:M1-2020:M6.