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Essays on the Changing Nature of Business Cycle Fluctuations: A State-Level Study of Jobless Recoveries and the Great Moderation

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by

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Abstract

The behavior of several important macroeconomic variables has changed dramatically over the past several business cycles in the U.S. These changes, which began around the mid-1980s, have been viewed as somewhat puzzling given the stark contrast they exhibit to earlier post-war data. The movement of output and employment has historically been highly correlated throughout the different phases of the business cycle. However, this changed with the economic recovery of 1991. Since then, periods of output recovery have been accompanied by periods of prolonged job loss. These periods have come to be known as "jobless recoveries". Several competing explanations for this phenomenon have come forth, however, all face similar limitations. To date, there has been no method presented to quantify a period of jobless recovery. This makes comparisons across business cycles difficult and also prevents formal statistical testing of the proposed explanations. This study creates a meaningful measure of a jobless recovery which can be used to test these hypotheses. Furthermore, jobless recoveries have only been studied using the national aggregate data. This neglects potentially valuable information which may exist in the cross-section between states. Using the jobless recovery measure, a state-level empirical analysis is conducted to determine which, if any, of the existing explanations of jobless recoveries are supported by the data. It has also been noted that the growth of output has experienced dramatic changes over roughly the same period. The broad decline in the volatility of output since the mid-1980s, named the Great Moderation, has become the subject of a large literature. However, the literature has examined mostly data at the national-level. Using a proxy of quarterly output, this paper provides state-level evidence of the Great Moderation and shows that large, cross-state differences exist in the degree to which each state experiences the Great Moderation. Explanations for why the Great Moderation exists in the national data are examined to see how well they explain the observed cross-state differences in the evolution of output volatility.

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Introduction

The three most recent U.S. business cycles have seen dramatic departures from earlier cycles with respect to the volatility and co-movements of several macroeconomic variables. Chief among these are the decline in volatility of aggregate output growth and the divergence of the growth rates of employment and output. Employment growth has historically followed GDP growth very closely, and the nature of the relationship between output and labor was thought to be well understood. However, in recent business cycles, employment growth has been negative for extended periods into the economic recovery. These jobless recoveries have puzzled economists and given birth to a literature which seeks to explain their emergence.

To date, the work on jobless recoveries has been constrained in at least two significant ways. The first is the lack of a comprehensive measure capable of capturing the magnitude of a given jobless recovery. Such a measure is desirable in order to make comparisons across business cycles and across different economies. Without a comprehensive jobless recovery measure, one cannot perform the statistical analysis necessary to test the existing hypotheses on the causes of jobless recoveries. This first constraint is addressed in the first chapter of this dissertation. A comprehensive measure for a jobless period is developed and then constructed for the nation and the fifty individual states.

The second factor which has limited previous work on jobless recoveries is the lack of cross-sectional analysis. Past research has focused only on the national time-series data, which provides at best three instances of jobless recoveries in the post-war U.S. This limitation is the focus of the second chapter of this dissertation. A panel study is conducted using state-level data from 1960-2012. This provides fifty times the observations for each business cycle allowing for much more robust statistical results. The state-level data, along with the newly developed jobless recovery measure from chapter one, is used to test several of the existing hypotheses on the causes of jobless recoveries.

Finally, chapter three of this dissertation addresses a similar problem in the literature surrounding the Great Moderation. The Great Moderation is the name given to the period of significant decline in output volatility in the United States beginning around 1984. While many have examined the national time-series data, few have analyzed output volatility across economies. Chapter three conducts some empirical tests of the leading theories on the Great Moderation using all fifty states. Thus, each chapter of this dissertation examines some recent change in the movements of variables over the business cycle which is not well understood and uses the statistically richer, state-level data to examine the competing hypotheses.

Chapter 1: The Measurement and Nature of Jobless Recoveries in the U.S.

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Abstract

In the average recovery prior to 1990 for the post-war U.S., positive growth in output was accompanied by positive growth in employment. However, in the three most recent business cycles, the positive growth rate of output following the cyclical trough has been accompanied by significant periods of continued job loss, causing economists to label these periods "jobless recoveries." While a sizable literature on this topic has developed, testing of proposed hypotheses has been constrained by the lack of a meaningful way to measure the degree or severity of a jobless recovery. As a result, there is little, if any, formal statistical tests of these hypotheses. We construct a general measure of the magnitude of a jobless recovery which exhibits many desirable properties for answering questions regarding the nature of this recent phenomenon. In addition to the national data for the U.S., we also apply our measure to the individual states, creating a database that allows for cross-sectional study of the jobless recovery problem.

1 Introduction

”You take my life when you do take the means whereby I live”

- *The Merchant of Venice*, William Shakespeare (1600)

The issue of employment has long been one of the primary concerns of economics. The behavior of aggregate employment during the business cycle was believed to be quite well understood until recently. In the average recovery prior to 1990 for the post-war United States, positive growth in output was accompanied by positive growth in employment. However, in the three most recent recessions, the positive growth rate of output following the cyclical trough has been accompanied by significant periods of continued job loss, causing economists to label these periods “jobless recoveries” (Groshen and Potter, 2003; Schreft and Singh; 2003; Aaronson et al., 2004; Berger, 2012). As stated by Schreft and Singh, a recovery is considered to be jobless “if the growth rate of employment in a recovery is not positive,” and this definition is consistent throughout the literature. Thus, if the economy is experiencing a recovery in output, yet there is no positive growth in employment, then this recovery is classified as jobless.

This recent phenomenon is somewhat puzzling considering the remarkably strong historical correlation between output and employment. Between 1960 and 1990, business-cycle expansions in the USA came together with almost simultaneous increases in employment. But sometime around the year 1990, this macroeconomic relationship changed, and in all of the economic recoveries observed after that date, output growth was accompanied by extended periods of continued job losses. In fact, the average correlation between quarterly changes in output and quarterly changes in employment observed during business cycle expansions decreased from a strong 0.522 before 1990 to a much weaker 0.076 after 1990.[1]

[1]The correlation was calculated by comparing the first difference in the log-values of non-farm employment and GDP strictly during business cycle expansions as defined by the National Bureau of Economic Research (NBER). We calculated the correlation for each

These periods of positive output growth and negative (or zero) growth in employment are the subject of a recent literature that attempts to understand their emergence.

Several alternative hypothesis exist about what may be causing the jobless recoveries. Berger (2012), for example, argues that the drop-off in union power experienced in the 1980's has lead businesses to become more productive during recessions and necessitate less workers during expansions, thus creating a jobless recovery. Groshen and Potter (2003) and Garin et al. (2011) focus instead on the relocation of jobs across industries or regions. They argue that the recent jobless recoveries result from the relocation of employment from shrinking, unproductive sectors to expanding, productive ones which require less workers. Faberman (2008) and DeNicco and Laincz (2013), in turn, have shown that jobless recoveries can be traced back to the broad decline in the volatility of economic aggregates beginning in 1984 (known as the Great Moderation). Others like Koenders and Rogerson (2005) and Bachmann (2011) provide an explanation based on employer's labor hoarding behavior and unusually long expansionary periods; while yet others like Aaronson et al. (2004b) consider the recent rise in health care costs as a potential cause.

However, the joblessness of recent recoveries in the United States is an issue deserving a great deal more attention than it is currently receiving. Economists cannot take lightly the divergent trend between output and employment. The very foundations of macroeconomic policy hinge on the premise that policies which stimulate aggregate output growth will also add jobs to the economy. It is in *The General Theory of Employment, Interest, and Money* that Keynes remarks, "To dig holes in the ground, paid for out of savings, will increase, not only employment, but the real national dividend of useful goods and services." Politicians and economists alike have made careers out of the assumption that fiscal policy can simultaneously achieve these dual objectives. Yet the data seem to suggest an evolution of the relationship between these two variables over time, implying a diminished, or at least, increasingly delayed, impact of policy on the labor market. Research efforts aimed at better

particular period using quarterly data and report the averages: 0.522 for the period covering 1960-1990, and 0.076 for the post 1990 years. Employment data comes from the Bureau of Labor Statistics, GDP data comes from the Bureau of Economic Analysis.

understanding this relationship and the reasons behind a weaker correlation of output and employment are paramount to current and future macroeconomic policy decisions.

Unfortunately, our ability to test the existing hypotheses has been constrained by two important limitations: 1. The lack of comprehensive measures capable of quantifying the extent or severity of a jobless recovery; which hinders our ability to generate positive statements and compare across business cycles. 2. The lack of cross-sectional statistical analysis at the state or regional level; which prevents us from conducting tests that cannot be performed using time-series data alone.

To grasp the importance of the first limitation, consider a simple comparison between the jobless recoveries of 2001 and 2008. After the economic recovery of 2001 started, it took 21 months and 1,078,000 jobs lost for employment to reach its lowest point and start growing again. In comparison, after the recovery of 2008 started, it took 8 months and 1,259,000 jobs lost for employment to accomplish that same feat.[2] Thus, if one looks at the *time it takes* for employment to join the expansionary cycle, the jobless recovery of 2001 can be said to be worse than that of 2008. But if one looks at the *amount of jobs lost* during the recovery, then the recovery of 2001 can be said to be better than that of 2008. One would like to discuss whether jobless recoveries are becoming more or less pronounced, but one cannot do so without a more comprehensive measure.

In similar fashion, to recognize the importance of the second limitation, consider the problem of testing a particular hypotheses about the causes of jobless recoveries. If it were true, for example, that the advent of just-in-time hiring practices are responsible for the emergence of jobless recoveries, as suggested recently in a paper by Panovska 2012, then we should expect these type of recoveries to be more prevalent or severe in places where just-in-time employment practices are more widespread. But it is impossible to conduct such a test using aggregate, national data alone. Cross-sectional studies are better suited for that task and can help improve our understanding.

[2]Total Non-Farm employment data from US Bureau of Economic Analysis was used to compute these numbers.

Our paper is concerned with these constraints. In the paper, we first propose a single, comprehensive measure of jobless recoveries. The proposed measure maps the percent of jobs lost, the length of time over which that job loss is observed, and the simultaneous changes in output that occur, into an easy-to-calculate number that we label "the jobless recovery depth" or *JRD*. We illustrate the properties of this measure using quarterly, time-series data at the national level for the USA, as is standard in the literature. We then compute the measure independently for all 50 states and all business cycles since 1960 and these calculations are made available to the public for future research.[3]

In order to compute our *JRD* measures, quarterly data on output and employment is required. For the most part, such data is available from the Bureau of Economic Analysis (BEA) and the Bureau of Labor Statistics (BLS). When computing the *JRD* values at the state level, however, we were faced with the problem of not having a valid source for quarterly, state-level GDP statistics.[4] We thus resorted to using data on the states' personal income accounts (earnings by place of work account in particular), also from the BEA, as an approximation. At the annual frequency, the average correlation coefficient between the states' GDP levels and the states' earnings by place of work is 0.9977. Of course, we cannot evaluate whether such a strong correlation is also observed at the quarterly frequency (quarterly, state-level GDP measures do not exist), but the evidence we examine suggests earnings by place of work are indeed a good approximation for the states' GDP levels.

Our results at the national level indicate jobless recoveries began with the expansion of 1991 and became increasingly severe after that. More specifically, we find an increase of 204% in the national *JRD* measure between the 1991 and the 2001 recoveries, and a 142% increase between the 2001 recovery and the still on-going recovery of 2008. Thus, using our comprehensive *JRD* measure, any questions of whether jobless recoveries are indeed taking place at the national level, or whether a significant change in the aggregate GDP-employment

[3]The *JRD* state-level database and accompanying code are available on Dr. Fabio Mendez's website, http://evergreen.loyola.edu/fmendez1/www/

[4]No source for quarterly, state-level, GDP statistics is currently available. Although the BEA is expected to produce state-level, quarterly GDP measures in the near future.

relation took place around 1990, are settled. Interestingly, our results also indicate that the sharp change observed in the 1990's was preceded by a mild but noticeable trend in the *JRD* dating back to 1975; a finding which has been previously overlooked but might provide valuable information regarding the causes of jobless recoveries.

In addition, a completely new set of insights arises when the state-level *JRD* measures are studied. To begin with, our results indicate that the jobless recovery phenomena is not a nation-wide occurrence, but a local event confined within a cluster of states that expands slowly from the 1991 recovery to the recoveries of 2001 and 2008. This finding underlines the importance of using cross-sectional statistical analysis as a complement for the type of aggregate, time-series studies currently available in the literature and makes it possible for one to test the validity of alternative hypothesis about jobless recoveries in a completely different way.

The jobless recovery measure derived in this paper will allow future research to make real progress in understanding the nature and causes of jobless recoveries in the United States. This, in turn, will open the door to a better understanding of how macroeconomic policy fulfills its dual objective in today's economy. The goals of this paper, however, are to present a general form of the *JRD* measure and then construct the measure using data for the nation and the individual states. Furthermore, we discuss the construction of our measure and its resulting strengths and weaknesses for application in future work. Although we leave the formal testing of current jobless recovery hypotheses for future work, we discuss in this paper what is learned from simple inspection of our measure alone. As already mentioned, we see that jobless recoveries at the national level became obvious in 1991, but have been monotonically increasing in severity since 1975. We also find that jobless recoveries have existed for certain states in each business cycle since 1960, long before the phenomenon appeared in the national aggregate data. Furthermore, we see that not all states experience jobless recoveries, even when they appear at the national level. Finally, the magnitude of jobless recoveries varies widely across states and time.

The remainder of the paper is organized as follows: Section 2 presents evidence on the existence of jobless recoveries, Section 3 discusses the national and state-level data used and modifications made to them, Section 4 introduces the Jobless Recovery Depth (JRD) measure that we propose in this paper and illustrates its properties using both national and state-level data, Section 5 shows there is significant variation in the jobless recovery experiences across states, and Section 6 concludes.

2 Evidence of Jobless Recoveries at the National Level

In this section, we present some evidence on the existence of jobless recoveries. We begin by taking the definition of a jobless recovery that is commonly found in the literature and applying it to past recessions, including the Great Recession. We then establish that each of the three most recent recessions has been followed by a jobless recovery, consistent with the literature. Following sections will present some additional tools for measuring the "joblessness" of any given economic recovery. We will apply these measures to the post-war U.S. data to determine the length and severity of joblessness in each recovery, and to detect any possible trends.

The recovery following the 1990-91 recession was the first in post-war U.S. history to be labeled jobless, and it was followed by another jobless recovery after the 2001 recession. The joblessness of these two recoveries has been documented in the literature (Groshen and Potter, 2003; Schreft and Singh; 2003; Aaronson et al., 2004). As stated by Schreft and Singh, a recovery is considered to be jobless "if the growth rate of employment in a recovery is not positive," and this definition appears to be consistent with the literature as a whole. Thus, if the economy is experiencing a recovery in output, yet there is no positive growth in employment, then we classify that recovery as jobless. Berger (2012) also provides evidence that these two recoveries were jobless, while extending his analysis to include the Great Recession of 2008-2009.

The business cycle is characterized by periods of economic contraction and economic growth. The trough of a business cycle is the point at which the contraction ends and the expansion begins. Thus, a recovery begins at the trough of a business cycle, and ends when the previous peak is once again attained. In order to determine whether or not a given cycle contains a jobless recovery, one must consider how the economy gains or loses jobs immediately following the trough. Figure 1 simply plots total nonfarm employment for the U.S. in the post-war era. Periods of recession are shaded in gray, meaning that recoveries begin where the shaded areas end. From this figure, we see that the post-1990 recessions appear to differ from the typical post-war recessions in that employment does not turnaround immediately following the start of a recovery. Rather we observe periods of continued decline or stagnation in employment extending well beyond the end of the recession. In pre-1990 business cycles, positive growth in employment lagged the positive growth in output at the start of a recovery by at most one quarter. In many cases, employment began its recovery in the same quarter as output. The movement in these two series was highly correlated in both the recession and recovery phases of the cycle. Beginning with the recovery in 1991, we observe a change, where these two series still move together during periods of recession, but then diverge for significant lengths of time into the recovery. (Individual plots of both employment and output for each post-1960 recession can be found in Appendix A.)

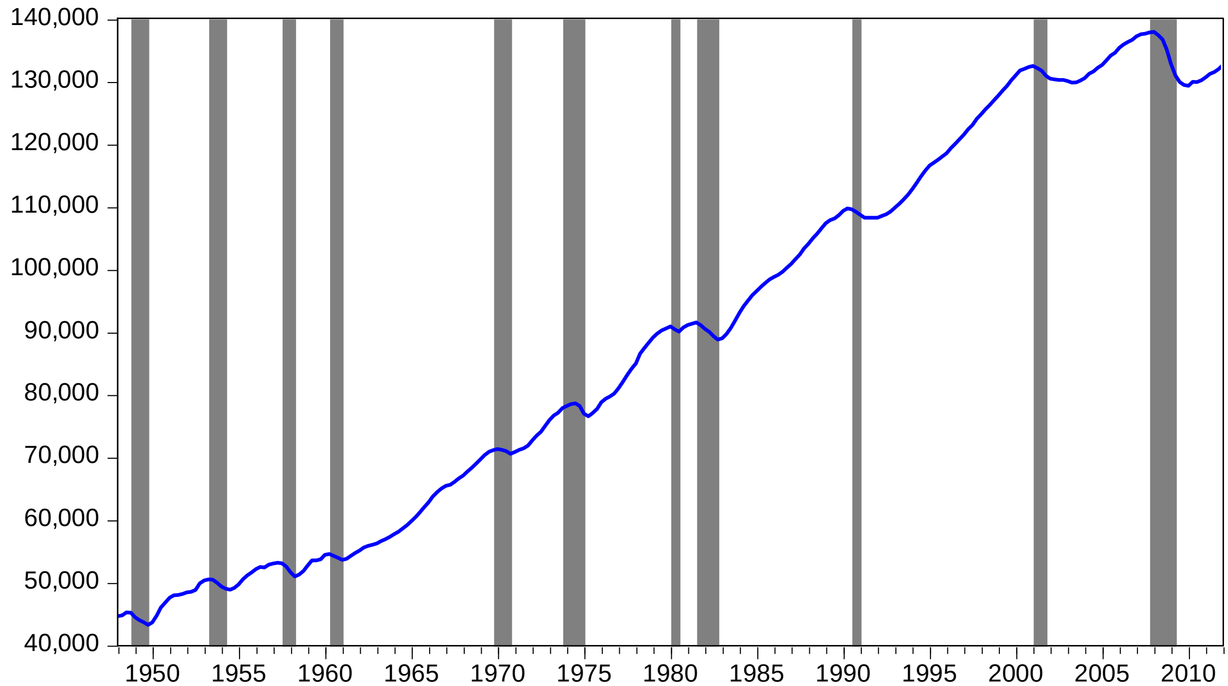


Figure 1: Total Nonfarm Employment (thousands). The shaded areas indicate NBER defined recessions. Source: U.S. Bureau of Labor Statistics

As previously stated, in order to determine whether or not a given cycle contains a jobless recovery, one must consider how the economy gains or loses jobs immediately following the trough. Using total nonfarm payroll employment data from the Bureau of Labor Statistics Current Employment Statistics (CES) for the post-war era, we plot the growth path of employment around the troughs of each recession in Figure 2. We normalize employment at the time of the trough to one for each cycle. The four series plotted are each of the three most recent recessions and the average of the post-war recessions from 1960 up through the 1980s. Figure 2 depicts the degree to which employment continued to decline, relative to the start of the recovery, as well as how long it took to begin adding jobs, and how long it took for jobs to fully recover to their pre-recovery and pre-recession levels. From this figure, a quick visual examination of the data shows quite clearly that the three post-1990 recessions were each accompanied by jobless recoveries. At the same time, we are able to see how different these jobless recoveries have been from the average post-war recovery. This is highly suggestive that these recoveries have indeed been jobless, and that jobless recoveries may be the new norm as proposed by Schreft and Singh (2003). It should be further noted how the jobless recoveries differ from one another when comparing the relative magnitude of continued job loss, and the duration of joblessness. An examination of this figure may also lead one to ask whether the condition of joblessness is a phenomenon that is worsening over time, and if so, in what way?

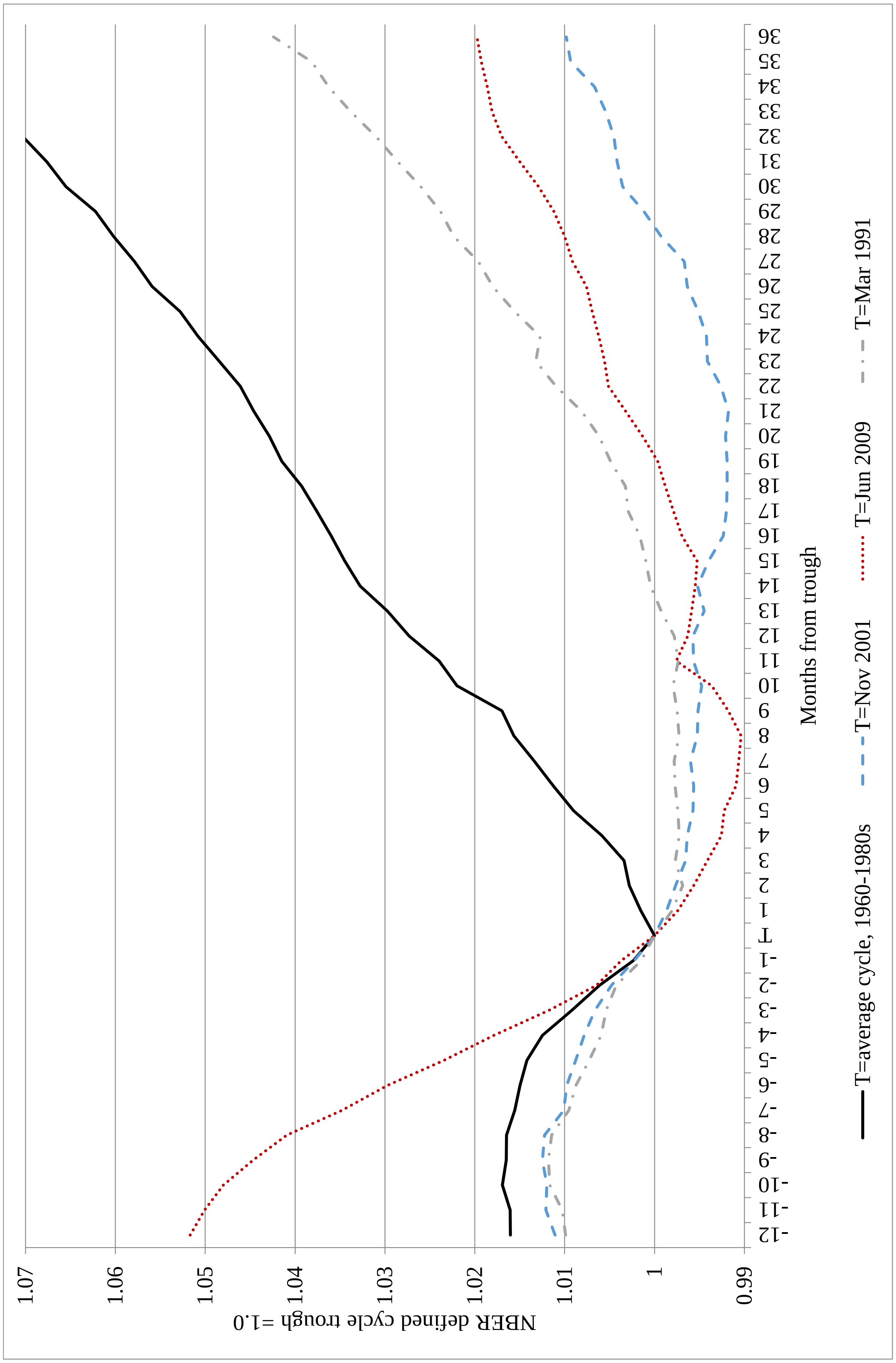


Figure 2: Source: U.S. Bureau of Labor Statistics; author's calculations

3 Description of the Data

3.1 National Level

The national data for the U.S. used in this paper comes from two main sources. The national employment data for the U.S. comes from the Bureau of Labor Statistics (BLS). The BLS databases include data on total employment, total hours, and hours per worker, among others, from 1947 to 2012. As a measure of total employment, the seasonally adjusted total nonfarm employment as reported by the Current Employment Statistics (CES) survey is used, consistent with the literature (Schreft and Singh, 2003; Aaronson, et al., 2004; Berger, 2012).

As a measure of national output, the quarterly real GDP data comes from the Bureau of Economic Analysis (BEA). This series is in 2005 chained dollars and is seasonally adjusted. Monthly and quarterly dates for peaks and troughs in the business cycle are taken from the National Bureau of Economic Research (NBER) Business Cycle Dating Committee, the accepted authority on business cycle dating. Using real GDP as the measure of output in this paper is appropriate as it is one of the main measures of economic activity considered by this committee in determining the dates of recessions and expansions.

For both total nonfarm employment and quarterly real GDP, analysis will only be done including the years 1960 to 2012[5]. Although data for nonfarm employment and GDP are available going back to 1947, there were significant changes made in both statistics that make comparisons between the pre-1960 and post-1960 periods potentially problematic. Bailey (1958) discusses how revisions made to the industrial classification system effect BLS employment statistics. He notes that, beginning in 1960, "all national employment statistics published by the U.S. Department of Labor's Bureau of Labor Statistics will be revised according to a new classification system." He continues to emphasize the potential issues by

[5]Although national GDP data for 2013 became available just prior to the completion of this draft, it was still not available at the state level. Thus, 2013 data has not been incorporated into this draft.

stating, ”The extensive revision of the coding structure will have a sizable impact on the continuity of a number of the BLS series, since the composition of many individual industries has changed significantly.” Also, between 1947 and 1960, the BEA went through several comprehensive revisions, resulting in statistical, definitional, and presentational changes. This presents a potential issue for both the employment and GDP series before 1960. In addition, choosing to work only with the data beginning in 1960 or later is consistent with the extant literature on jobless recoveries (Berger, 201; Groshen and Potter, 2003; Schreft and Singh, 2003).

Aaronson, Rissman, and Sullivan (2004) provide a very clear and detailed description of the BLS's two major employment surveys: the payroll survey coming from the Current Employment Statistics, and the household survey from the Current Population Survey. Both are monthly surveys and designed to be nationally representative. Those interested in a detailed description of the respective survey methods, the quantity of households or establishments surveyed, what is actually being counted as employment, and the methods for extrapolating these survey results to the whole population should refer to their paper. They detail potential flaws and biases that exist in each survey, and conclude by stating their opinion that the payroll survey (from the Current Employment Statistics) is generally the more accurate of the two. In addition, the majority of the existing work done in the area of jobless recoveries has used the CES. Therefore, employment data from the CES is used throughout the paper.

3.2 State Level

State-level employment data is also taken from the BLS. Monthly total non-farm employment data for each state is available from 1960-2012, however it is not seasonally adjusted. In order to get a seasonally adjusted series of employment for each state over the desired sample period, we seasonally adjust the data using the X12 ARIMA seasonal adjustment program from the United States Census Bureau.

Recall that GDP was used as a measure of output at the national level. However, state-level GDP data coming from the BEA Regional Economic Accounts and is only available annually from 1963-2012. Annual data does not allow one to properly observe the changes in variables throughout the business cycle. Since we need data that is at least available at a quarterly frequency, we must find a proxy for GDP at the state level that is available at the desired frequency.

Personal income data by state is reported on a quarterly basis by the BEA. One of these components, called *earnings by place of work*, was chosen as our proxy of state output. According to the BEA, "Earnings by place of work is the sum of Wage and Salary Disbursements, supplements to wages and salaries and proprietor's income. BEA presents earnings by place of work because it can be used in the analysis of regional economies as a proxy for the income that is generated from participation in current production." Thus, we feel that earnings by place of work has the potential to be a reasonably strong proxy for state output. Henceforth, *earnings by place of work* will be referred to as simply *earnings* for short.

Additional adjustments must be made to the *earnings* data to make the series more comparable to the measure of output used at the national level (GDP), and to allow for meaningful comparison across time and states. The *earnings* data is nominal and not seasonally adjusted. We first seasonally adjust the *earnings* data for each state using the X12 ARIMA process discussed above. The nominal, seasonally adjusted series is then converted into real earnings using the GDP deflator. This provides a real, seasonally adjusted *earnings* measure for each state which can be used as a proxy for output.

Other proxies for output face challenges either in the frequency or range of the available data. For instance, GDP by state is available over the desired range, but only at an annual frequency. Data on commercial electricity consumption by state, which is believed to be highly correlated with production, is avaiable monthly, but only as far back as 1990. Since both of these alternative proxies have their shortcomings in the context of this particular study, they cannot be used here.

The data seem to support the claim of the BEA that *earnings* by place of work may proxy well for production. The average correlation coefficient between annual state GDP levels and annual state earnings by place of work is 0.9977. Thus, at the state level, the correlation between GDP and our proxy seems very strong when using the annual data. Of course, we cannot evaluate whether this is also true when using quarterly data (quarterly, state-level GDP measures do not exist); but we still made an effort to document the quarterly correlation at the national level. National data for both GDP and earnings by place of work are available at a quarterly frequency and have a correlation of 0.7272. Both the annual state-level correlations and the quarterly national-level correlations suggest that *earnings* is indeed a reasonable proxy for GDP.

In addition, given that for the purpose of calculating the JRD we require an approximation for the percentage changes in GDP and not for the GDP levels themselves, we also looked at how annual changes in earnings at the state level correlate with annual changes in state-level GDP. We conducted standard OLS regressions between the state-level, annual changes in GDP and the corresponding state-level annual changes in *earnings*. In these regressions, *earnings* are significant at the 1% level for all 50 states and explain about 75.6% of the observed variation in GDP, on average (the average R-squared for the 50 regressions was 0.756).

4 The Jobless Recovery Depth and Other Measures of Jobless Recoveries

4.1 Unsophisticated Measures of Duration

Although evidence has been provided on the existence of jobless recoveries, there has been little to no attempt made to measure them in a meaningful way. Questions regarding the severity of a jobless period and whether there is a discernible trend or pattern over time are difficult to answer without meaningful measures. Using the definition of a jobless recovery from Schreft and Singh (2003), recall that a recovery is considered to be jobless "if the growth rate of employment in a recovery is not positive." This definition is consistent with the related literature. We begin by constructing a simple measure out of this definition:

merely counting the number of months or quarters that a given recovery was jobless. This is accomplished by calculating the number of quarters or months where positive output growth was accompanied by nonpositive employment growth, once again using the NBER defined cycle troughs as the start of a recovery. This is reported in Figure 3 using national data. The results from counting the number of jobless quarters are redundant, so only monthly measures are reported here.

This simple definition we have taken from the literature for a jobless recovery generates nothing more than a simple indicator variable. At any given point in time, a recovery is either jobless, or it's not; a 1, or a 0. The issue with creating a binary variable to use in our analysis of jobless recoveries is that, apart from duration, it tells us nothing about how these jobless periods have differed from one another. (It should be noted that the simple measure of duration this provides is alone an improvement over the previous research on jobless recoveries). Comparing a 1 to a 1 in different business cycles suggests these jobless periods are the same. Does it seem likely that all periods of time defined as jobless are equal? The data clearly suggest otherwise, yet with this simple indicator variable, we glean no additional information. This simple classification neglects important details in the movements of these variables over time. One example is that it fails to account for the relative magnitude of job losses and gains. In fact, the losses to total employment incurred over the jobless period following a recover may not be regained for many months or even years. This may be accompanied by strong or weak growth in aggregate output, and the weakness of the labor market relative to output growth is lost on a binary variable. Apart from producing the simple measures of duration reported in Figure 3, this indicator variable for jobless recoveries can tell us little else. Yet there has been no previous attempt made to move away from so restrictive a definition of jobless recoveries.

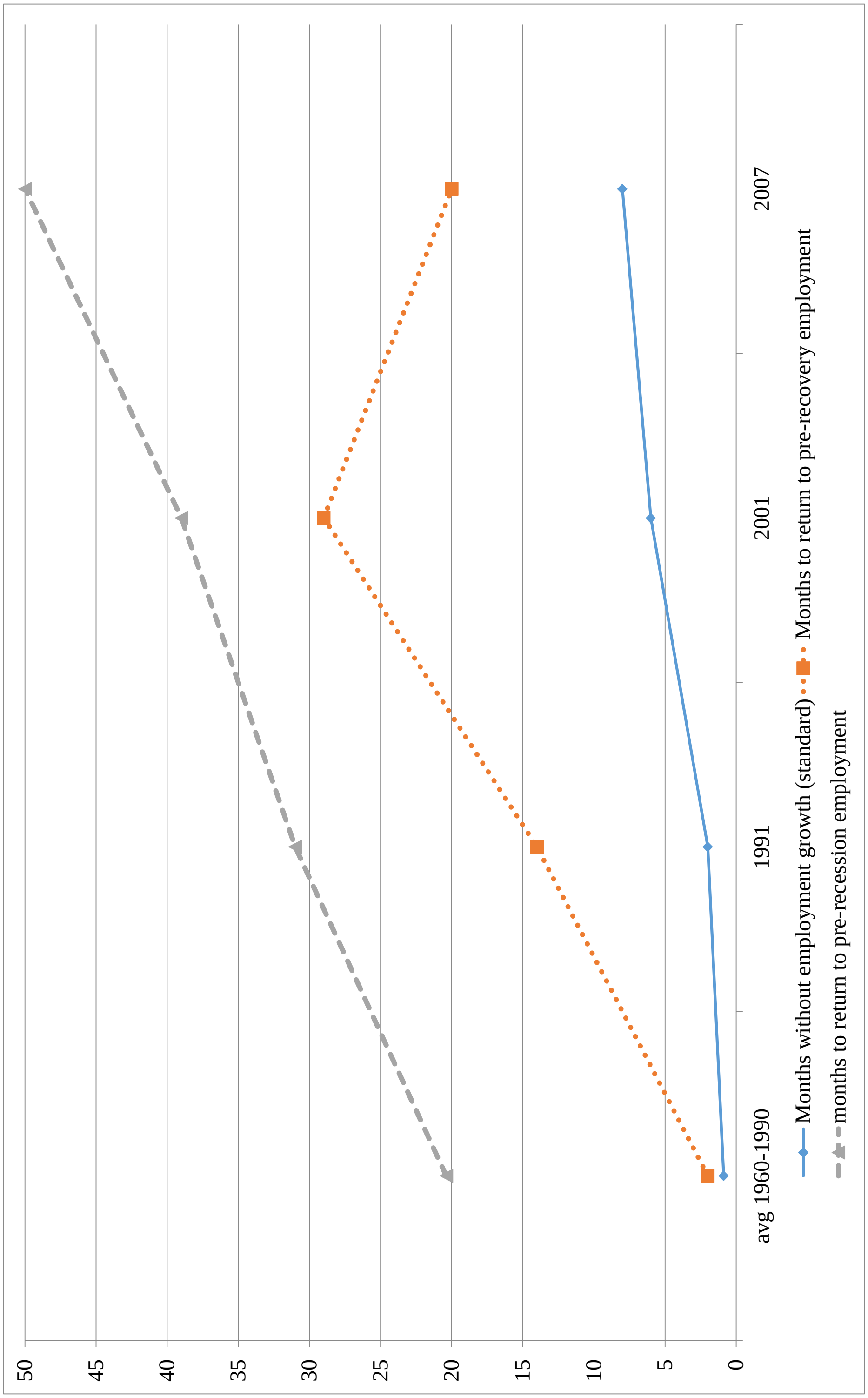


Figure 3: Unsophisticated measures of duration using monthly data

For example, in the recovery following the Great Recession, there were only three jobless quarters according to this aforementioned definition. However, it took eight quarters for employment to regain its pre-recovery level. Meaning that two years after output began to recover; jobs had experienced zero net growth relative to the start of said recovery. Could one not also argue then that this whole period of time could be considered jobless? We see that the determination of how long joblessness lasts during a recovery depends very strongly on the interval of time being considered. If instead of using quarterly data, one used annual or monthly data as the interval of time, one might find that relatively longer or shorter periods fall under the jobless recovery label currently being used in the literature. Thus, measuring the length of time it takes for employment to reach a positive net gain relative to the start of the recovery may be an informative measure for joblessness as well. This measure is also presented in Figure 3. Moreover, we feel it is meaningful to quantify the length of time it takes for total employment to return to its pre-recession peak, in other words, how long it takes for employment to make a full recovery. This count is also presented in Figure 3. Inspecting Figure 3, we see that according to all of these measures the post-1990 recoveries have been jobless. Additionally, we see that most of these measures suggest a trend towards recoveries with an increasingly long duration of joblessness over time. This provides further evidence of a change in the economy away from the historical relationship between output and labor.

4.2 The Relative Job Loss

Although meaningful, these simple counting measures offer only a glimpse of what can be gained from quantifiably measuring jobless recoveries. We now propose a new measure of employment during the business cycle that should be much more informative. In the macroeconomic and econometrics literatures, there is a useful measure for gauging the depth of a recession at any point in time known as the Current Depth of Recession (CDR). CDR was first proposed by Beaudry and Koop (1993). CDR is defined as the gap between the economy's historical maximum level of output, and its current level. It is given by, $CDR_t = max[Y_{t-j}]_{j \geq 0} - Y_t$, , where Y is the natural log of output. The Current Depth of Recession is a very nice measure in the sense that its construction is exceedingly simple, yet it contains a great deal of information. Since it is calculated using logs of the data, it displays the depth as a percentage variation from the historical maximum. This allows for clean comparisons across business cycles, where examining data in levels can clearly be misleading. Furthermore, CDR itself contains information pertaining to the length of time of a recession, and the length of time of a recovery in a manner that is easy to discern from a simple inspection of the data.[6]

By applying a similar methodology, we construct a comparable measure for the employment time series which we will name simply Job Loss (JL_t). Following the CDR literature, the JL_t variable is formed by calculating the gap in the historical maximum value of employment, and the current value. We make one minor adjustment. Instead of comparing each point in time to the current historical maximum value over the entire series, we restrict the maximum to the within-cycle maximum. This is done to insure that values from one business cycle are not being compared to maximum values from previous business cycles. In a few instances, it is actually the case that the historical maximum occurs in an earlier cycle, significantly distorting the measure.

For this reason, we introduce our measure using a generic, standard representation of a business cycle where output growth is negative during recessions and positive during expansions. Using this representation, a full business cycle is defined as the period of time that begins on the date that marks the initiation of the economic decline, continues over the trough and the subsequent expansion, and ends when the economy stops expanding and another cycle begins. We arbitrarily choose to label the moment when the recovery begins as "$t = 0$". Similarly, we choose to represent the beginning of the business cycle as "$t = t_{begin}$", and its end as "$t = t_{end}$". That is, at any time $t \in (t_{begin}, t_{end})$, we measure job losses as $JL_t = max\{L_j\}_{j \in (t_{begin}, t)} - L_t$; where L represents the logarithm of the employment level and JL_t stands for "job loss at time t". Where the "peak to peak" business

[6]For evidence in favor of using CDR in time-series analysis, see Jansen and Oh (1999)

cycle intervals employed for the beginning and end dates are those established by the National Bureau of Economic Research (the intervals are: [1960q2, 1969q4), [1969q4, 1973q4), [1973q4, 1980q1), [1980q1, 1981q3), [1981q3, 1990q3), [1990q3, 2001q1), [2001q1, 2007q4), and [2007q4, 2012q4[7])).

The JL_t measure is plotted in Figure 4. As far as we are aware, this is the first and only attempt that has been made to apply such a measurement methodology to the employment series. This innovative measure allows us to address many of the shortcomings of relying on the simple definition of a jobless recovery used in the previous literature. However, As noted by Gali, Smets and Wouters (2012), jobless recoveries cannot be measured by drops in employment alone; but by changes in employment relative to the concurrent changes in output. So if two recoveries generate identical job losses, but one takes place during a period of strong output growth while the other takes place during a period of moderate output growth, then the desirable jobless-recovery measure should distinguish between these two different experiences. Arguably, when jobless recoveries are characterized by stronger output growth, the job losses experienced should be weighted heavier, and the measure should take on greater values.

Thus, we apply our modified version of Beaudry and Koop's measure to output, and name the new variable Output Loss (OL_t). That is, at any time $t \in (t_{begin}, t_{end})$, we measure output losses as $OL_t = max\{Y_j\}_{j\in(t_{begin},t)} - Y_t$; where Y represents the logarithm of output[8] and OL_t stands for "output loss at time t". Plotting both JL_t and OL_t together in Figure 4, we see supporting evidence of the existence of jobless recoveries shown in the previous section. In each pre-1990 recession, growth in employment lagged growth in output by at most one quarter. Here, we gain additional information as we also see that full recovery in employment lagged full output recovery by at most one quarter. This simply confirms the remarkable strength of the historical correlation between employment and output. The change after 1990 is once again obvious. Employment growth lags output growth by much longer periods

[7]The last business cycle is ongoing, and the next peak has not yet been established. Here, we use the last quarter for which data is available

[8]given here by seasonally adjusted quarterly real GDP in 2005 chained dollars.

of time, with the economy continuing to shed jobs in some cases even after output has fully recovered. Note that there is no significant or noticeable change in the correlation between output and employment during the recession phase of the business cycle. The change that has taken place in the economy over the last several business cycles seems to have explicitly affected the correlation of these two series during recoveries alone.

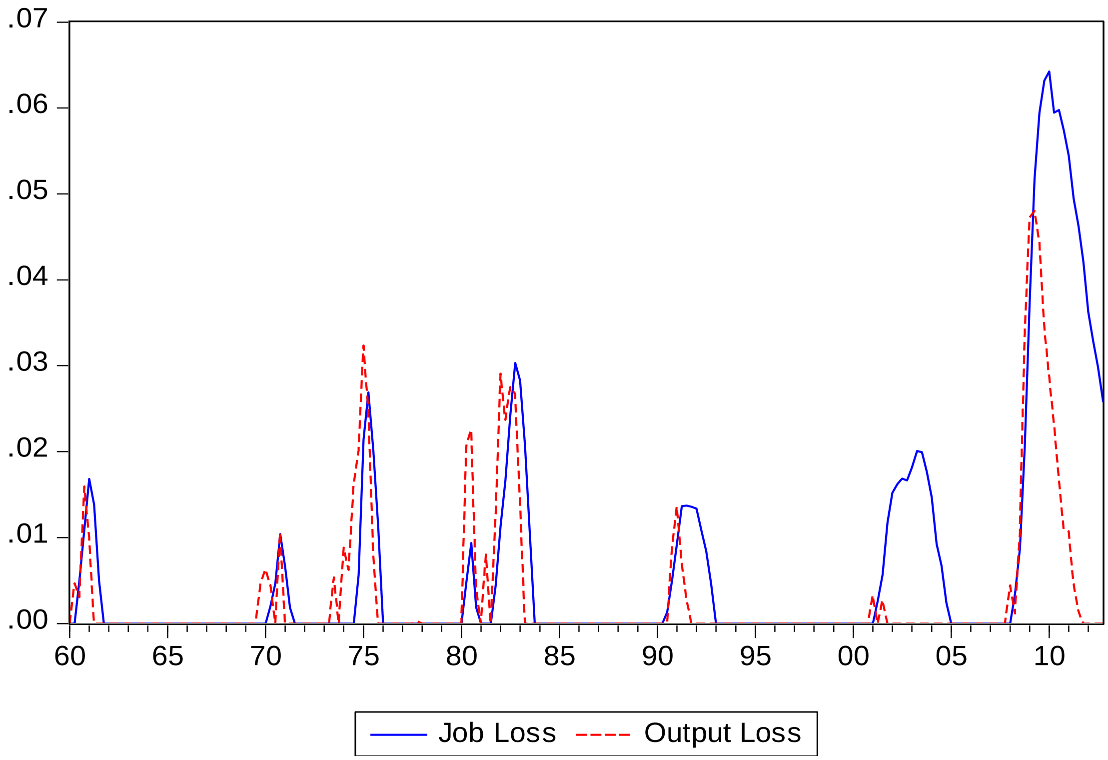


Figure 4: This figure illustrates the percentage changes in employment and output, relative to the peak of their corresponding cycle. Output data used are real, seasonally-adjusted, quarterly GDP series from the Bureau of Economic Analysis (BEA). Employment data used are total non-farm employment series from the Bureau of Labor Statistics (BLS). Seasonally adjusted series are provided monthly by the BLS and are aggregated here to a quarterly frequency using the 3-month average.

The ability to aid in answering questions regarding the severity, length, and trend of jobless recoveries is perhaps the most important application of the newly constructed JL_t measure. However, as we will show in the following section, we believe it may also be valuable in testing the existing theories on the causes of jobless recoveries. Inspecting Figure 4, it appears that the last three recessions have been increasing in joblessness as measured by JL_t. However, as already mentioned, the real question of interest is not just the depth of joblessness, but how much of that joblessness is not explained by the current depth of recession. For example, in the 1975 recovery, the JL_t was larger than in the jobless recoveries beginning in 1991 and 2001. However, the recovery beginning in 1975 has not been considered jobless. To account for the fact that jobs naturally decline more when the decline in output is larger, consider the Relative Job Loss (RJL_t),

$$RJL_t = [max\{L_j\}_{j\in(t_{begin},t)} - L_t] - [max\{Y_j\}_{j\in(t_{begin},(t-1))} - Y_{t-1}]$$

Here, Y_t is given by the natural log of output, L_t is given by the natural log of total nonfarm employment, and RJL_t stands for the relative job loss at time t. This index is displayed from 1960-2012 in Figure 5. We use the one-quarter lag of OL_t in order to be consistent with the fact that employment has historically lagged output by about one quarter, (Koenders and Rogerson, 2005). Also, when one does not use the one quarter lagged value of OL_t, one gets a large one-quarter spike appearing in the difference between the two series at the peak and trough of each business cycle. Rather than have these large, one-quarter long spikes in our measure for each business cycle, we choose to lag our output loss measure by one quarter.

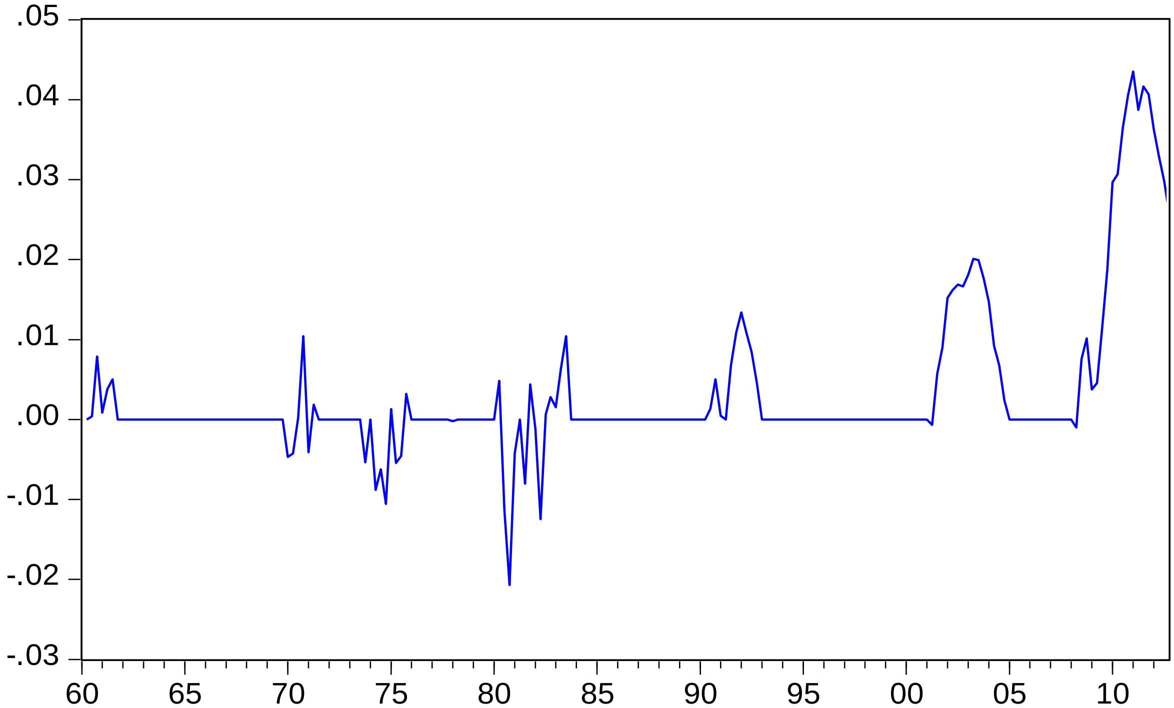


Figure 5: The Relative Job Losses (RJL_t), the difference between JL_t and OL_{t-1} from 1960 to 2012

How does one interpret RJL_t? Recall that both OL_t and JL_t were derived from natural logs of the data, so that we were already dealing with percentage deviations from historical maximums at any given time. From a mere visual inspection of the recent Great Recession, we see that RJL_t reaches a value of approximately .04. This tells us that employment at this point in time was about 4 percentage points further from full recovery than output was one quarter earlier. Remember, we normally expect employment to lag output by about a quarter. Let's examine the exact values for each measure. During the Great Recession, RJL_t reached a maximum value of .043 in the first quarter of 2011. At this time, JL_t was .053, meaning total employment in the U.S. was 5.3% lower in the first quarter of 2011 than at it's pre-recession peak. OL_t was .01 a quarter earlier, meaning that GDP on the other hand, was only 1% lower than its pre-recession peak. This implies that relative to output's distance to full recovery, employment was still 4.3 percentage points behind. This is the Relative Job Loss. From Figure 5 we see that employment is progressively lagging behind output by greater magnitudes and greater lengths of time. The newly constructed variable RJL_t suggests that jobless recoveries are growing more severe in magnitude, or depth, as well as in duration. Most interestingly, this pattern seems to extend back to the 1970s. Using only the unsophisticated binary definition of a jobless recovery discussed previously, one fails to noticed the divergence in the behavior of output and employment that was occurring well before the first universally recognized jobless recovery.

Figure 6 presents a cleaner view of the depth and duration of each recession from 1960-2012. The measure of depth being plotted comes from our newly constructed index, RJL_t. The depth values in the figure are the greatest level of relative joblessness for each business cycle, that is, the peaks from Figure 5. For duration, RJL_t was also used. It is a simple count of the number of consecutive quarters that had a positive RJL_t in each business cycle. It should be noted, however, that total nonfarm employment in the United States has still not fully recovered at the time of this writing. Therefore, the last observation for duration in Figure 6 will continue to grow as new data becomes available. Also, a comparison of the duration graph and Figure 3 shows that our newly constructed index for joblessness is consistent with our more elementary counting measures of duration. The main addition in value is in the measure of depth, but importantly, RJL_t does not contradict Figure 3 as a measure of duration.

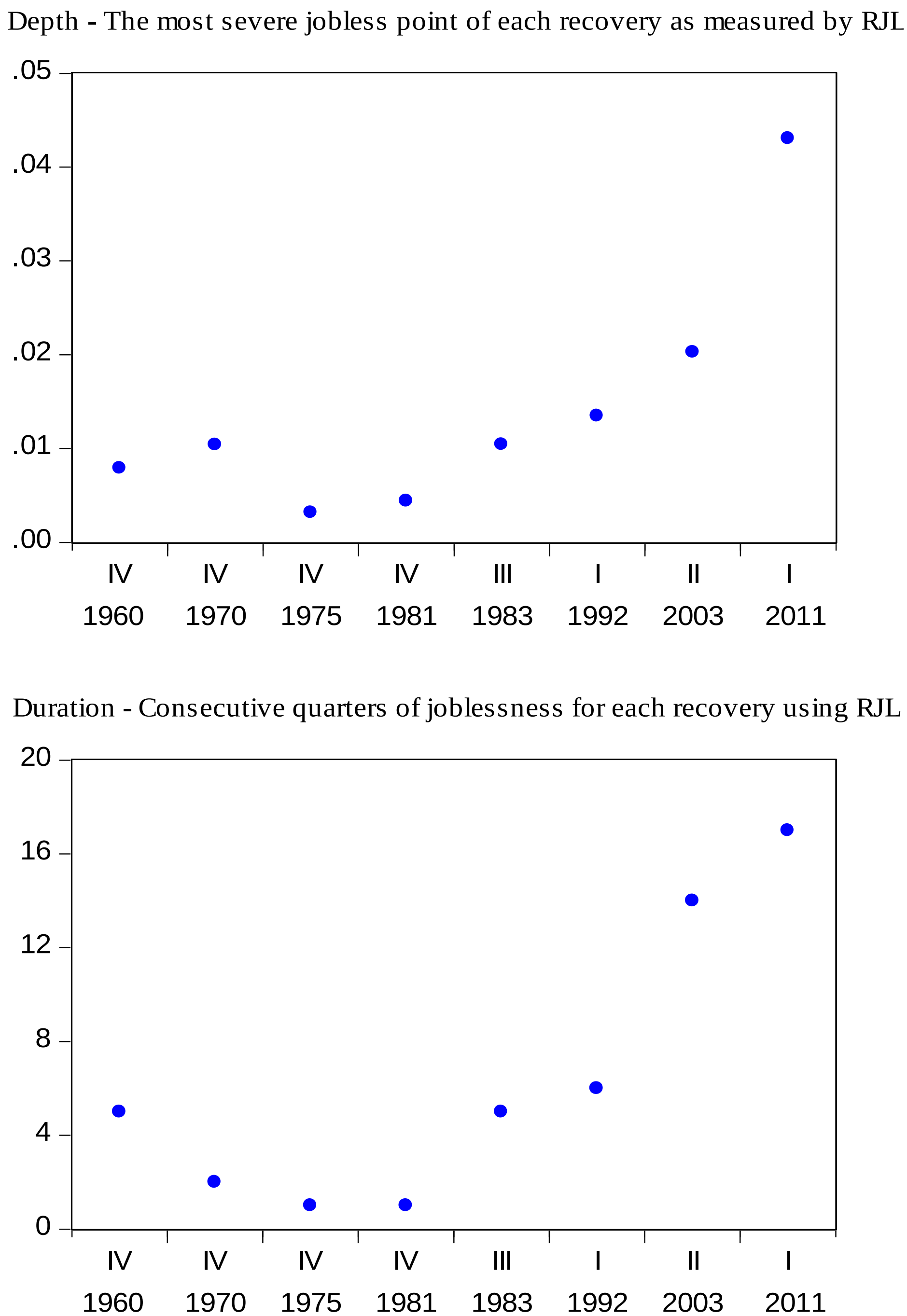


Figure 6: Depth and duration of joblessness, as measured by RJL

As mentioned before, a meaningful measure for joblessness that is not a binary variable may allow for more accurate analysis of the existing theories regarding the causes of jobless recoveries. Moreover, for certain types of analysis, one may wish to use a continuous variable in place of the RJL_t variable which is not continuous. Recall that by design, periods that are not jobless return a value of zero for RJL_t. The jobless periods alone return positve values, which are themselves continuous, and vary in magnitude given the relative changes in output and employment. It is possible to construct similar variables that are continuous. These corresponding continuous measures for both JL_t and OL_t, as well as RJL_t, are presented in Appendix B of this paper.

4.3 The Jobless Recovery Depth

As shown in figure 4, the relative job loss (the vertical distance between curves) takes on the expected values. That is, the difference between the employment and output losses is close to zero at all points in time before 1990 and increasingly positive for the expansionary periods that begin around 1991, 2001, and 2008. But figure 4 not only shows the two lines have separated vertically; it also shows they have separated horizontally. This growing horizontal distance between the lines indicates that employment losses have become more resilient; that while employment losses before 1990 tended to disappear at the same time as the corresponding output losses, they tend to linger for much longer after 1990.

Thus, when constructing the ideal jobless recovery measure, one should make sure to incorporate this additional time dimension. We do that here by measuring the difference between the employment and output losses cumulatively, throughout the duration of the cycle.[9] The result is our jobless recovery depth (JRD) measure, which we formally define as follows:

$$JRD = \sum_{t\,=\,t_{begin}}^{t\,=\,t_{end}} [max\{L_j\}_{j\in(t_{begin},t)} - L_t] - [max\{Y_j\}_{j\in(t_{begin},(t-1))} - Y_{t-1}]$$

[9]Panovska 2012 discusses how the changes in labor market variables and other real variables that characterized jobless recoveries can be observed throughout the entire cycle and not just the recovery. Koenders and Rogerson 2005 point out how it is not optimal to compare across business cycles while using the recovery part of the cycle alone, specially when the downturns that precede those recoveries have been different.

Several properties of the *JRD* must be noted. First, that since employment has historically followed output with a one-period lag, it makes sense to use lagged output values instead of current values. We do so in all of our calculations. Second, that since the *JRD* is not affected by the duration of the cycle per-se, it can be easily compared both across cycles and across regions. And third, that the *JRD* does not differentiate between pronounced and sustained output or job losses (although it could be modified to do so). Thus, in our proposed measure, a business cycle with severe but short-lived job losses may yield the same *JRD* than another cycle with mild but long-lived job losses.

Finally, it is also important to point out that the *JRD* measure is responsive to all the elements that define a jobless recovery in a non-controversial manner: a ceteris paribus increase in the percentage of jobs lost during the cycle increases the *JRD*, a ceteris paribus increase in the output growth experienced during the cycle increases the *JRD*, and a ceteris paribus increase in the time it takes for the employment to recover also increases the *JRD*. These responses show that our measure is consistent with the literature and the data regarding the characteristics of past jobless recoveries in the U.S.

At the national level, the *JRD* measure can be easily calculated using the same BEA data introduced before. In fact, one may deduct the national-level *JRD* values by measuring the total area between the lines in figure 4, for each respective time period. For simplicity, however, the corresponding national-level *JRD* values are plotted directly in figure 7. The reason why the *JRD* measure takes both positive and negative values is simple. In any given quarter, a negative value arises when the output loss is greater than the loss of of employment. That is, when employment performs relatively better than output. Conversely, positive values arise when employment has performed poorly relative to output, as one may expect from the typical jobless recovery. Thus, since the *JRD* measure aggregates these differences over the length of a the cycle, it may also take on positive or negative values.

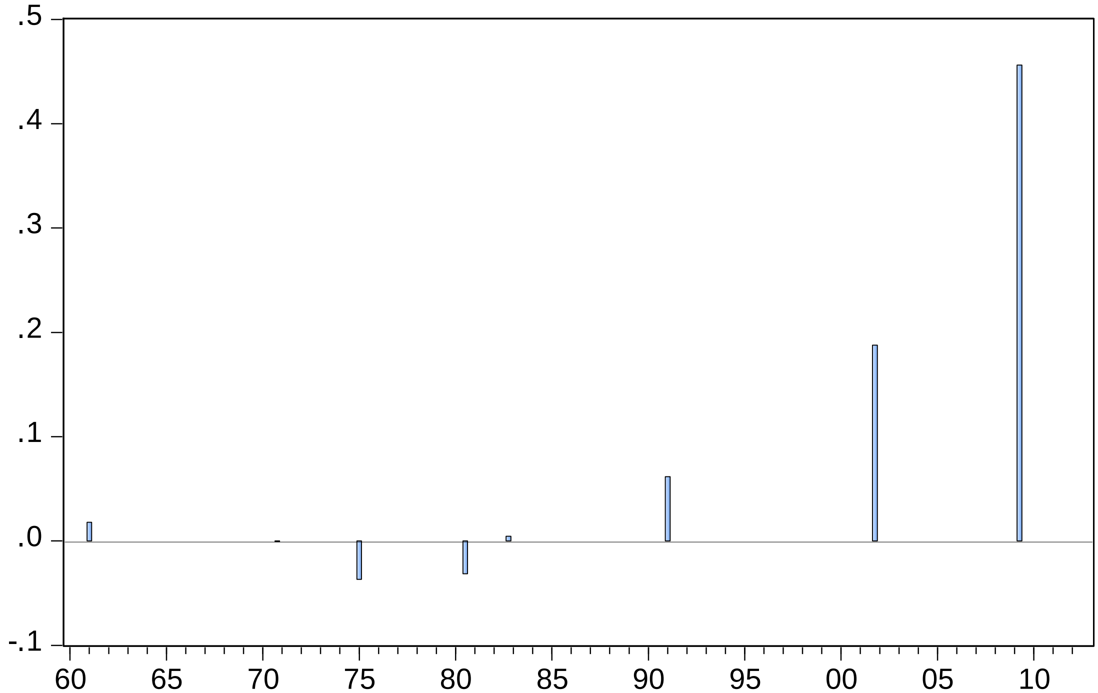


Figure 7: This figure illustrates the jobless recovery depth measure using national data for the USA. Output data used are real, seasonally-adjusted, quarterly GDP series from the Bureau of Economic Analysis (BEA). Employment data used are total non-farm employment series from the Bureau of Labor Statistics (BLS). Seasonally adjusted series are provided monthly by the BLS and are aggregated here to a quarterly frequency using the 3-month average.

As shown in figure 7, the *JRD* provides a clear answer to the question described in the introduction about whether the jobless recovery of 2008 was more or less severe than that of 2001. The *JRD* of 2008 is much greater than the *JRD* of 2001. Figure 7 also shows there has been a marked increase in the *JRD* measure at the national level since 1990. In our calculations, the largest *JRD* value for all pre-1990 recessions was .0179, while that for the ongoing expansion is up to 25 times larger already (the *JRD* for the 2009 recession up to quarter 4 of 2012 was .4561). Interestingly, when both the positive and negative values of the *JRD* are considered, the increase in the *JRD* observed after the 1990's seems to follow a trend that starts some 20 years earlier. In fact, as illustrated in figure 7, national-level *JRD* values have increased in every subsequent cycle since 1975. This observation then suggests that for the empirical study of the causes of jobless recoveries, it is important to secure data dating back as many years as possible.

The *JRD* is not the only evidence that the relationship between output and employment was changing well before 1991. Figure 8 shows the rolling correlation between the log differences of GDP and employment from 1960 to 2012. This is a centered rolling correlation with a window size of 8 years. From Figure 8 we see that the correlation between output and employment has been declining since the 1970s. This provides further suggestive evidence that there was in fact a change in the nature of the relationship between output growth and employment growth well before the first observed jobless recovery in 1991. This suggests that the *JRD* measure is uncovering something true about the co-movements of these two variables throughout past business cycles, and that the trend observed in figure 7 is not a aberration. However, it should also be noted that the rolling correlation is sensitive to the choice of window size, and the 8 year window was selected to be consistent with the literature (Berger, 2012).

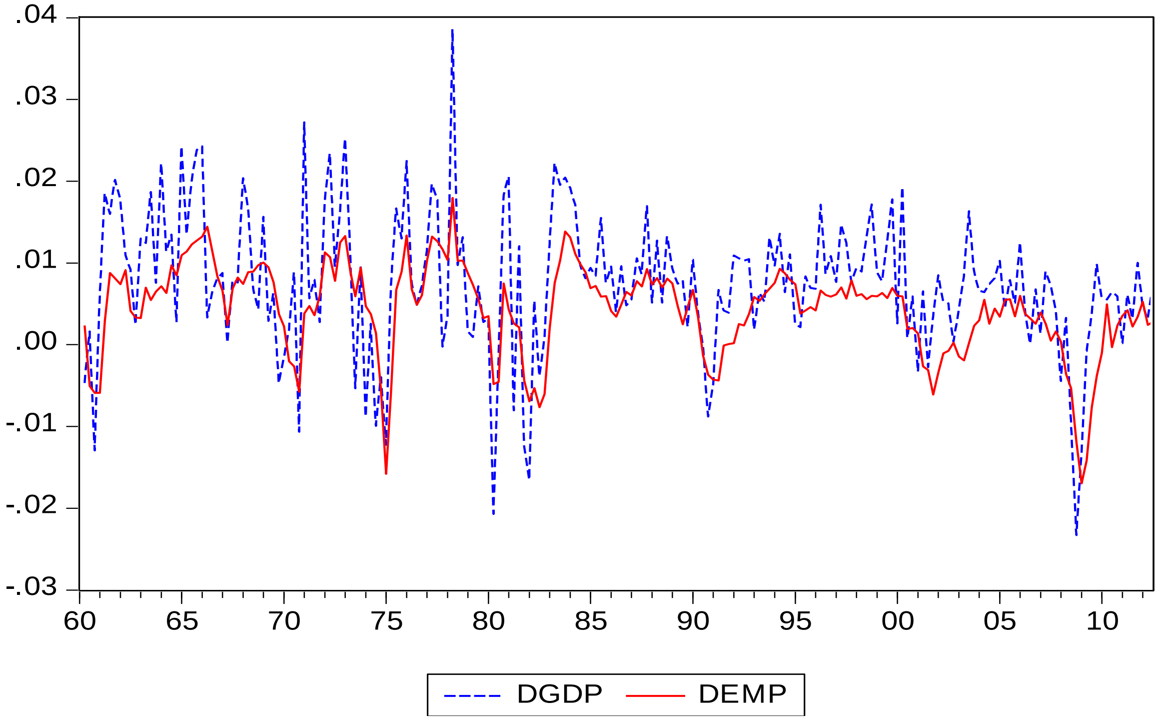


Figure 8: Ablove: log difference of GDP and total nonfarm employment. Below: rolling correlation: GDP and EMP. (window size of 8 years)

4.4 Construction of the *JRD* at the State Level

Next, we wish to examine the cross-sectional properties of jobless recoveries. In order to do that, we calculate *JRD* values at the state level for each of the NBER classified business cycles on record since 1960. As explained before, the calculation of the *JRD* requires one to define the business cycle peak dates (t_{begin}, t_{end}) and calculate both, the employment and output losses $((max\{L_j\}_{j\in(t_{begin},t)} - L_t)$ and $(max\{Y_j\}_{j\in(t_{begin},t)} - Y_t)$, respectively), observed during those periods. To define the state-level, business cycle peak dates (t_{begin}, t_{end}), we simply use the "peak to peak" intervals established by the NBER for the aggregate economy as the common dates for all states. So that if $t_{begin} = a$ and $t_{end} = b$ at the national level, then we set $t_{begin} = a$ and $t_{end} = b$ for all individual states. At first glance, this way of choosing the business cycle dates for the individual states may seem problematic. At close inspection, however, it is safe to say that states enter and exit business cycles in synchrony with the national economy.

We compare the dates of the peaks and troughs for the nation provided by the NBER to the state level output series we are using (*earnings by place of work*) in order to judge how closely most state economies follow the national economy. We examine the 3 most recent business cycles and find that 93% of the time, state troughs are within 2 quarters of the national date, and 79% of the time, state peaks are within 2 quarters of the national date. The more important number for the purposes of our study is the proximity of the troughs. By design, our measure captures information in the recovery phase of the business cycle, or immediately following the trough. State trough dates close to the national dates suggest recoveries are beginning at similar times. Peak dates are important since we are defining our business cycles as peak-to-peak intervals. However, in most cases, output and employment series have already recovered to pre-recession levels well before the following peak, meaning that the *RJL* measure is simply zero for several periods at the end of each cycle. Thus, even if use of the national peak dates from the NBER causes our measure to omit 1, 2, or even 3 quarters (in extreme cases) of expansion, it is unlikely to change our measure much, if at all. (92% of state peaks are within 4 quarters of the national date).

Our further argument for using the national business cycle dates for the construction of our state measures is that it is better than the alternative. Attempts to define unique business cycles for each state create several problems. First, how does one choose to define a cycle, and will this definition allow for comparison to the national definition? Is a recession defined as two consecutive quarters of output decline? Such a definition does not even apply to the national dates (see 2001). Using the national dates at the very least eliminates error in attempting to fit all states into a single definition of how a business cycle is defined. Importantly, sticking with the eight cycles from 1960-2012 for all states helps to maintain a balanced panel. An arbitrary attempt at state-by-state business cycle dating may provide seven cycles over our sample for one state, and nine for another, while an equally arbitrary alternative measure may produce a completely different number of business cycles for each state.

To construct the *JRD* for each state, we require output and employment data at a quarterly frequency. This presents some challenges, especially with respect to the output series, but suitable proxies are available as discussed in detail in section 3. Thus, we proceed with our state-level *JRD* calculations using total nonfarm employment from the BLS for our employment series and *earnings by place of work* as our output proxy.

Now that we have selected quarterly employment and output series for each state, we construct the *JRD* for each state according to the same *JRD* equation used previously for the national data. Given that we now possess a measure of a jobless recovery that is state specific, we can examine several unique aspects of jobless recoveries which have not previously been studied. Section 5 displays the state-level *JRD* data and further discusses the new insights into jobless recoveries which can be gleaned from a cross-sectional examination of the data.

5 Cross-sectional properties of jobless recoveries

Although the phenomenon of jobless recoveries has been documented at the aggregate level for the United States, it is also informative to consider the relationship between disaggregated jobs and output. By considering the variables of interest for several smaller groups, for example, states, we are able to glean some additional information regarding the conditions surrounding observed instances of jobless recoveries. Examination of the cross section allows one to answer a wider range of questions regarding periods of prolonged joblessness in addition to an increased number of statistical tools for analyzing the data. One could examine different kinds of groups other than U.S. states, such as regions, MSAs, counties, cities, or even different industries using the same methodology to construct the measures presented here. However, as a first attempt to study cross-sectional properties of jobless recoveries, we will be focusing on state-level data. We encourage future research to study variations in JRD using output and employment data at the industry and MSA levels.

It should first be noted that significant differences do and have existed across states regarding the relationship between labor and output. Without these differences, a cross-state study would be fruitless. However, we see that jobless recoveries vary a great deal from place to place in duration, magnitude, etc. In fact, initial analysis of the data shows that although jobless recoveries are fairly new in the aggregate, they have existed within certain states in every post-war business cycle. Figure 9 plots our newly constructed measure, JRD, for all 50 states for the last 8 business cycles. The data suggest that a considerable difference exists across states and across time.

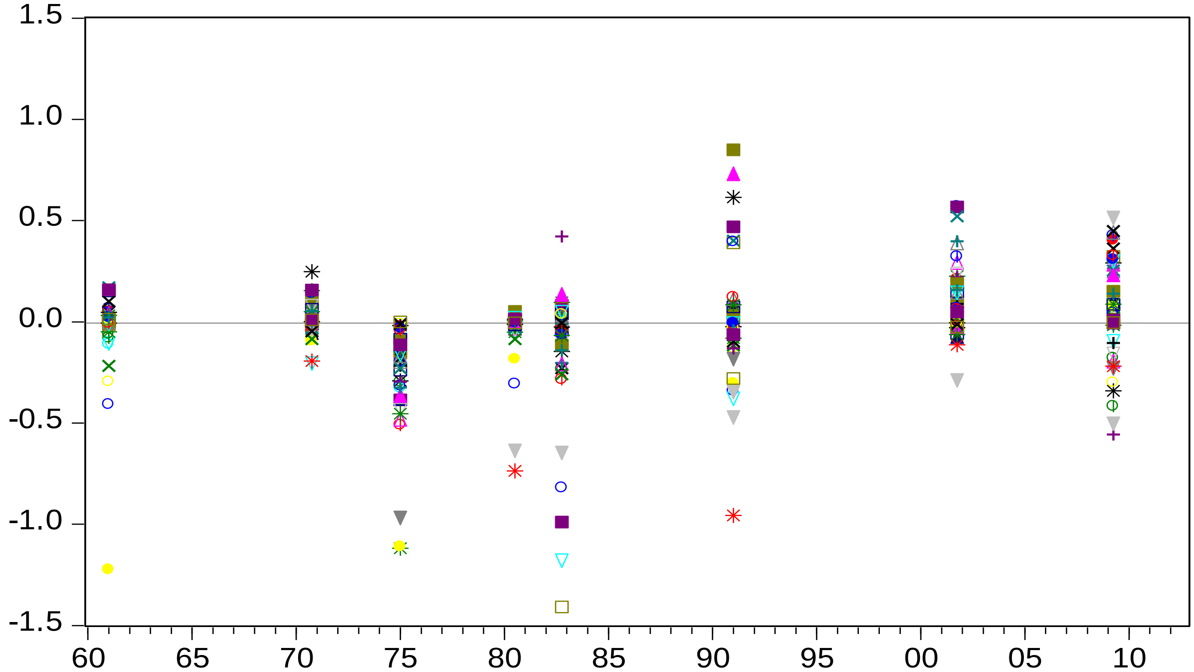
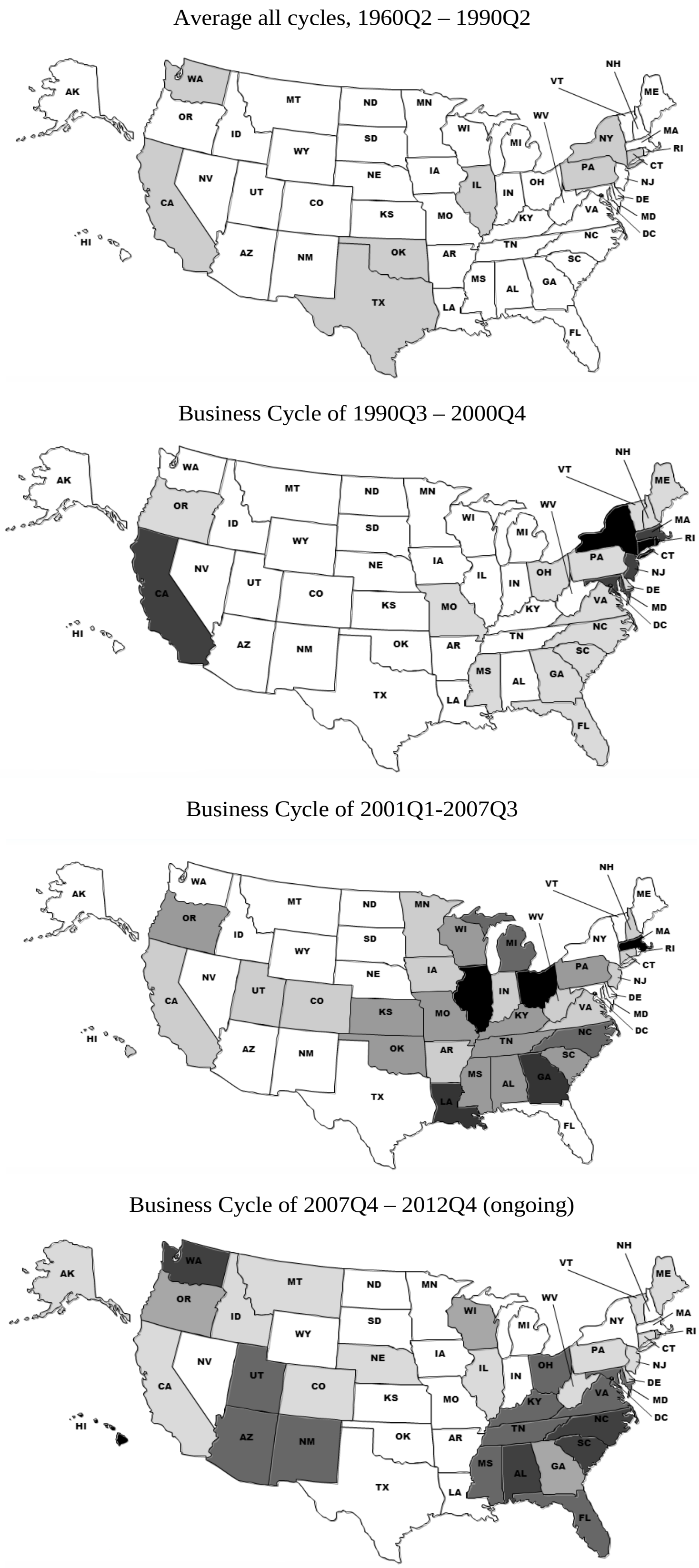


Figure 9: *JRD* for each state for each business cycle since 1960.

An alternative way to visually examine the cross-state differences in jobless recoveries is presented in Figure 10. Here, shaded maps of the U.S. show the employment recovery gap for each state over time, where a darker shade corresponds to a higher *JRD*, or a more severe jobless recovery. Once again we see vast differences across both states and time. Recall the previous discussion of how the *JRD* measure is able to take on both positive and negative values. Just as we observed some business cycles with negative *JRD* values in the national data, we observe several states with negative values at different points in time. This can be observed in Figure 9. However, the maps of the United States in Figure 10 treats both negative and zero values of *JRD* the same. This is due to the fact that both negative and zero values correspond to cycles which had traditional, non-jobless, recoveries.

More specifically, the maps of the United States are shaded by assigning each state into one of six categories. States are reassigned for each business cycle, and the category to which a state is assigned is determined by the value of its *JRD* variable. The first category is by far the largest, and includes all states which have JRD values less than or equal to zero. As discussed, a negative or zero value for JRD corresponds to a business cycle in which employment performed relatively well compared to output. These business cycles are not considered to experience any degree of a jobless recovery, and are placed into category 0, which receives a white color, representing no degree of jobless recovery. The states which have positive values of JRD are assigned to one of five categories according to the following scale: category 1 for JRD values between (0, .125), category 2 for values between [0.125, .25), category 3 for values between [.25, .50), category 4 for values between [.50, .75), and category 5 for values between [.75, infinity). Each category is shaded in gray-scale where darker states correspond to higher categories, or a higher degree of jobless recovery. The shading for all six categories is depicted in the legend of Figure 10.



JRD Range	Shade
(-∞, 0]	
(0, .125)	
[.125, .25)	
[.25, .375)	
[.375, .50)	
[.50, ∞)	

Figure 10: Maps of all 50 U.S. states shaded according to their Jobless Recovery Depth (JRD), with darker states having a higher JRD

It is also informative to inspect the distribution of state *JRD* values for each business cycle. Histograms for all eight post-1960 business cycles are displayed in Figure 11. From the histograms we see a fairly clear evolution over time. Nearly all of the business cycles prior to 1991 saw very few or zero states with *JRD* values which were positive. The fast majority of values were negative and close to zero. However, distributions are skewed somewhat to the left, with some large negative-value outliers. Then, beginning in 1991, there is a movement in the distribution to the right that has been increasing over each business cycle. We see an increasing number of positive *JRD* values (jobless recoveries) and the distribution even becomes skewed to the right. Finally, the most recent recovery, following the Great Recession, shows a bimodal distribution with both peaks in the positive range.

One may also wish to examine the entire distribution over all eight cycles at once. This is shown in Figure 12. Once again, we see large negative values from older business cycles and positive values coming from more recent cycles. A look at the basic summary statistics of the state-level *JRD* in Table 1 points to the same pattern. All of this evidence suggests that jobless recoveries are becoming increasing severe over time.

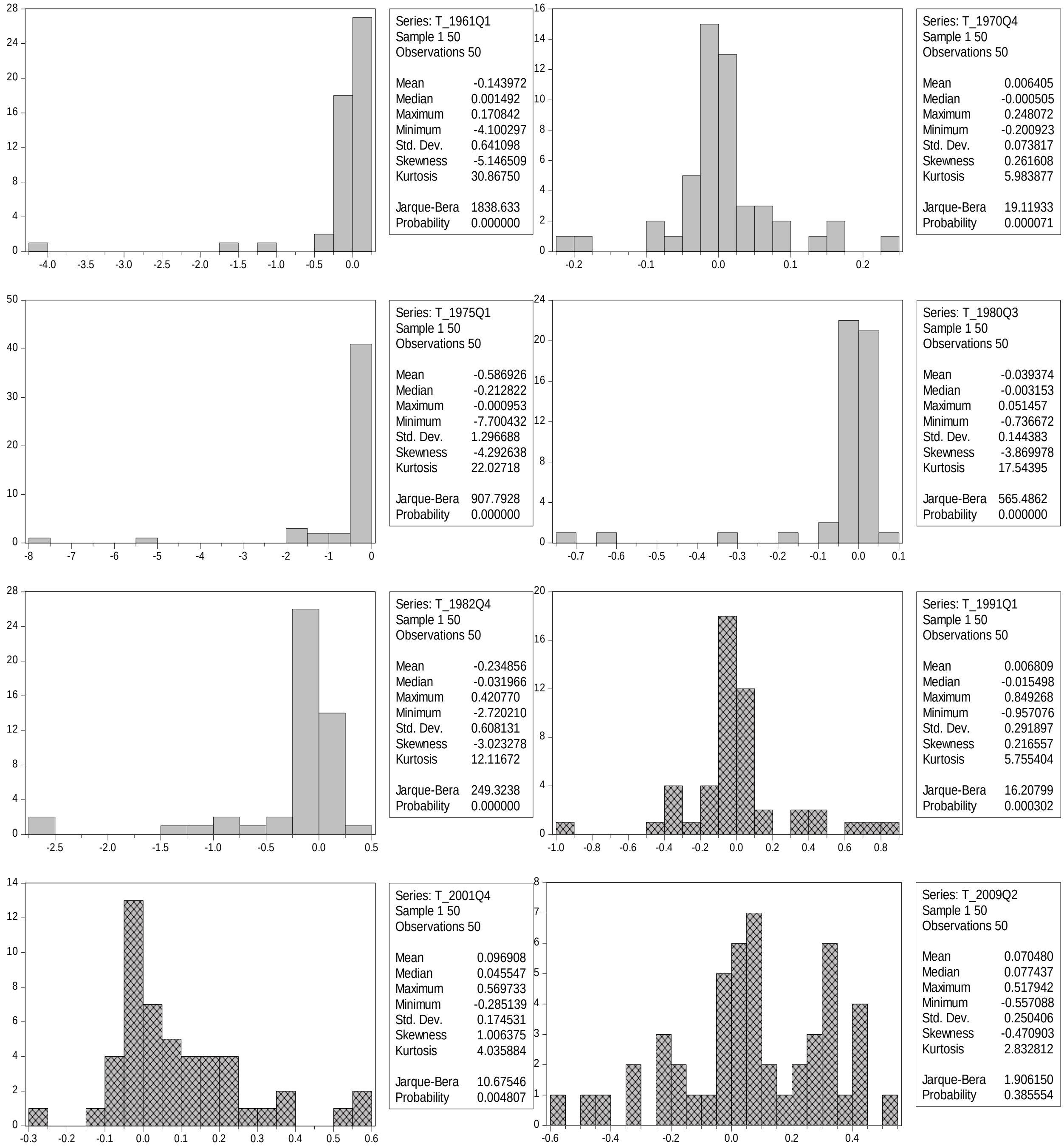


Figure 11: Histograms for the distribution of state JRD values over each business cycle since 1960. The hatched histograms correspond to periods of jobless recovery at the national level.

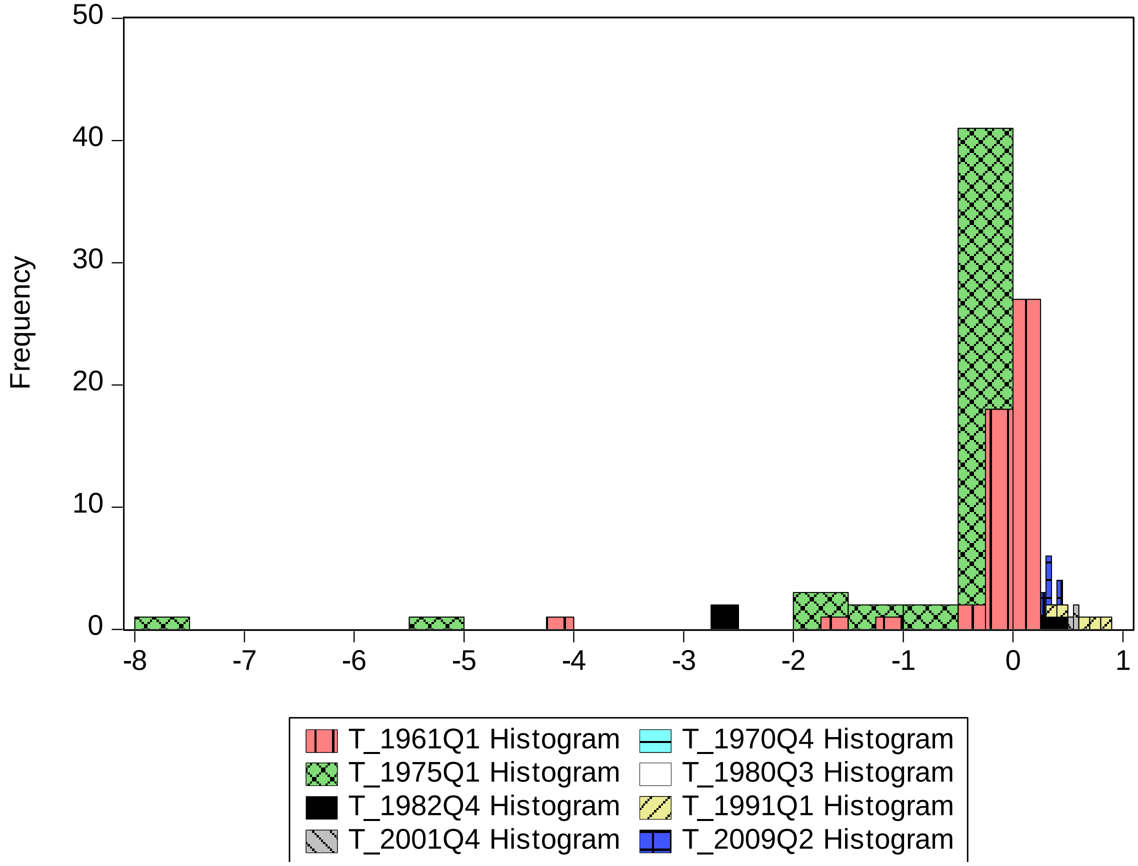


Figure 12: Single histogram of all JRD values for all states over all eight business cycles.

Table 1: **JRD Summary Statistics.** Basic summary statistics for the state-level *JRD* measures, covering the years 1960-2012. Column one shows statistics for the average business cycles before 1990. In turn, column 2 shows statistics for the cycle that begun in the third quarter of 1990, column 3 for the cycle that begun in the first quarter of 2001, and column three for the cycle that begun in the fourth quarter of 2007

JRD	Pre-1990 average	1991	2001	2009
Mean	-0.196	0.007	0.099	0.078
Median	-0.010	-0.011	0.048	0.082
Standard Dev.	0.724	0.289	0.1732	0.254
Minimum	-7.700	-0.957	-0.285	-0.557
Maximum	0.420	0.849	0.570	0.518
Sample size	250	50	50	50

6 Concluding Remarks

We have taken initial steps to address two major limitations that have existed to date in the jobless recovery literature; a lack of meaningful measures capable of generating positive statements about jobless recoveries, and a lack of studies considering cross-sectional evidence. We demonstrate how current methods of measuring and analyzing jobless recoveries fail to capture the multi-dimensional characteristics of these events such as relative depth and duration. We propose a measure which incorporates all of these dimensions into a single index which we have named the Jobless Recovery Depth (*JRD*). This measure possesses important properties which allow it to capture the essential information present during a jobless period. Upon defining this measure, we proceeded to construct a database for the

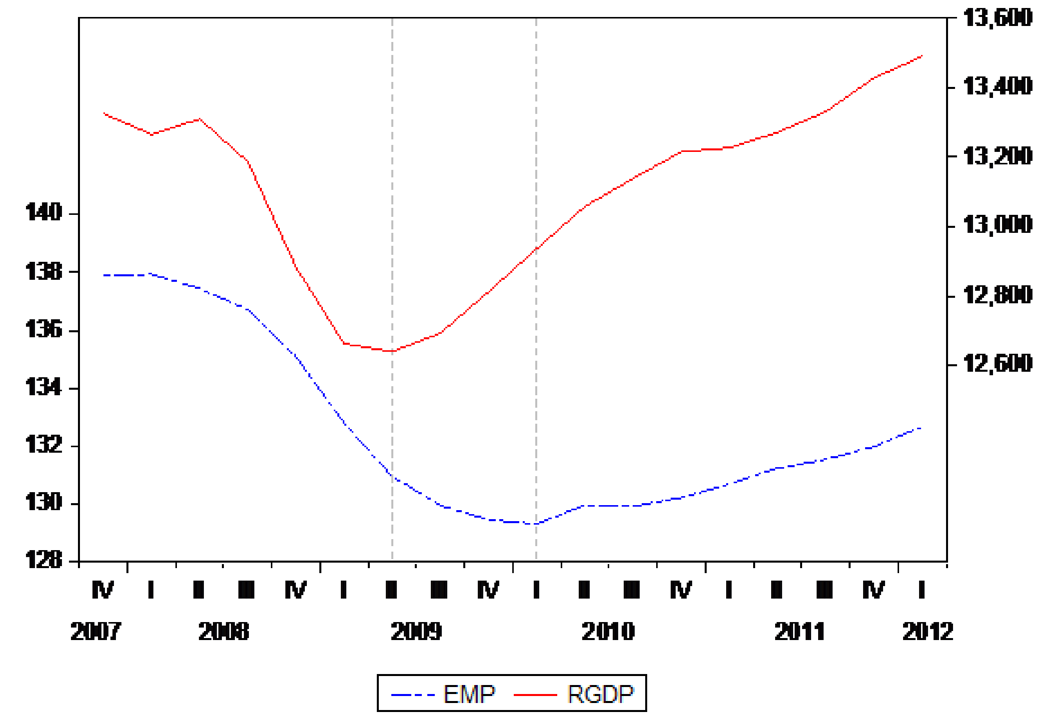
JRD index for each state since 1960. This database was made publicly available [10] for use in future research and will be updated regularly by the authors. The ability to perform cross-sectional and panel analysis using a multi-dimensional measure of jobless recoveries will greatly assist the empirical testing of the existing and forthcoming hypotheses on the causes of jobless recoveries in the United States.

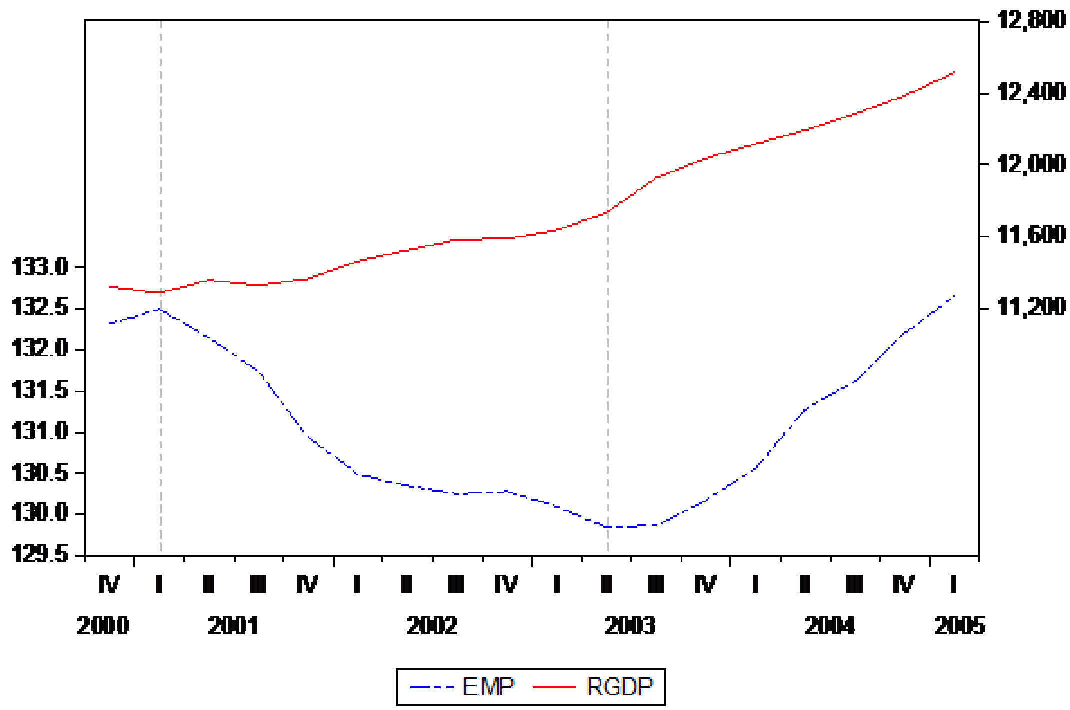
From inspection of the *JRD* measure, we are able to make several observations in answer to questions which have previously not been answered. At the national level, our measure is consistent with the consensus in the literature that the jobless recovery problem began in 1991. However, it also suggests that the magnitude of the jobless recovery problem is monotonically increasing in the U.S. since approximately 1975. This has important implications for future work in this area, suggesting candidate explanations for this phenomenon should be consistent these newly uncovered facts as well. Also, our measure implies that if current trends continue, we can expect an even more severe episode of joblessness in the recovery phase of the next U.S. business cycle. If such a trend continues, it may have major implications regarding the ability of classical macroeconomic policy to satisfy its dual objectives of output and employment growth.

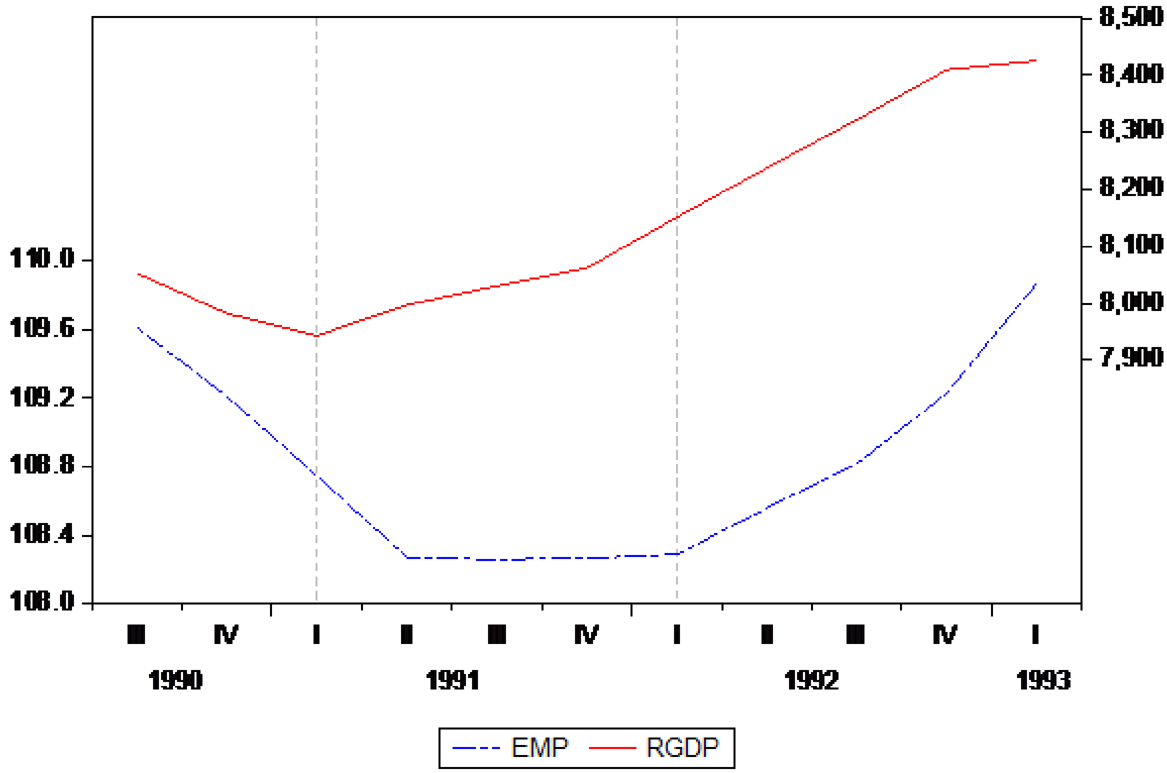
Furthermore, we find that jobless recoveries have existed in the United States prior to 1991, at least at the state level, which fact has not previously been explored. More specifically, jobless recoveries have existed in at least some states for *every* business cycle since 1960. We find that although jobless recoveries are observed in the national data, they are not a nation-wide problem. Several states fail to experience jobless recoveries at all, and the extent of the problem in each state varies widely. The large differences which are observed across states and across time suggests that future cross-sectional research of jobless recoveries at the state-level is promising.

[10] The *JRD* state-level database and accompanying code are available on Dr. Fabio Mendez's website, http://evergreen.loyola.edu/fmendez1/www/

A Additional Figures Depicting Jobless Recoveries at the National Level



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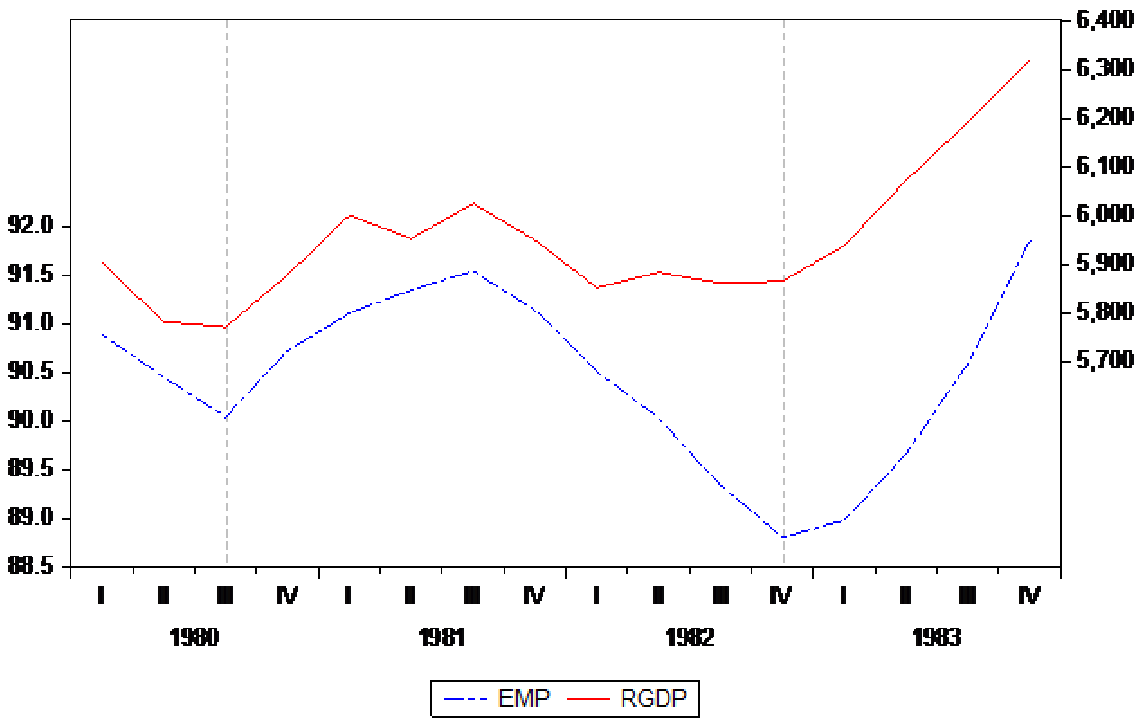
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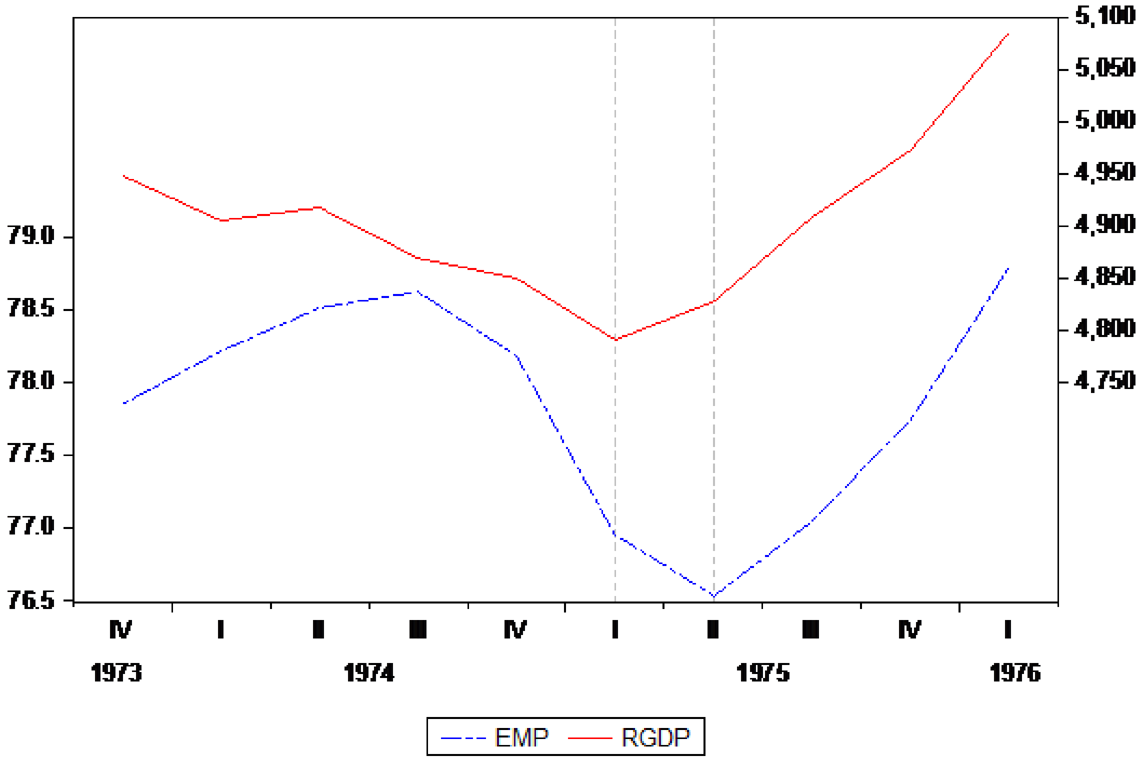

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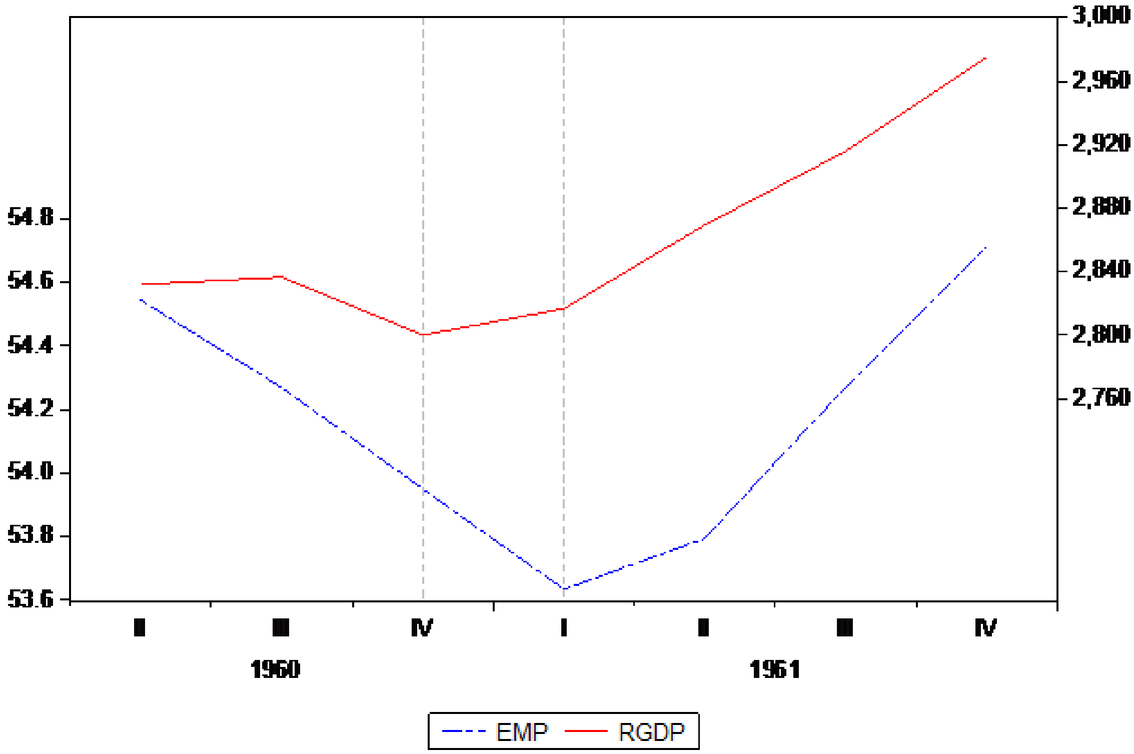

B Continuous counterparts to JL, OL, and RJL

Let $JLA_t = L_t - max[L_{t-j-1}]_{j \geq 0}$. Here, JLA stands for "Job Loss - alternate" where the alternate measure is a continuous counterpart to the JL measure presented in the body of the paper that reported zeros for many observations. JLA presents two changes from JL. First, we have flipped the order around so that the historical maximum value is now being subtracted from the current value. This generates jobless spikes in a downward direction rather than an upward direction. Secondly, the historical maximum is restricted to the period before the current period. This way, even if the current period is indeed the historical max, it will not be subtracted from itself and we will not get observations with a value of zero. We use an identical specification to construct a continuous measure for output which we will call OLA (output loss - alternate). JLA and OLA are displayed in Figure 13.

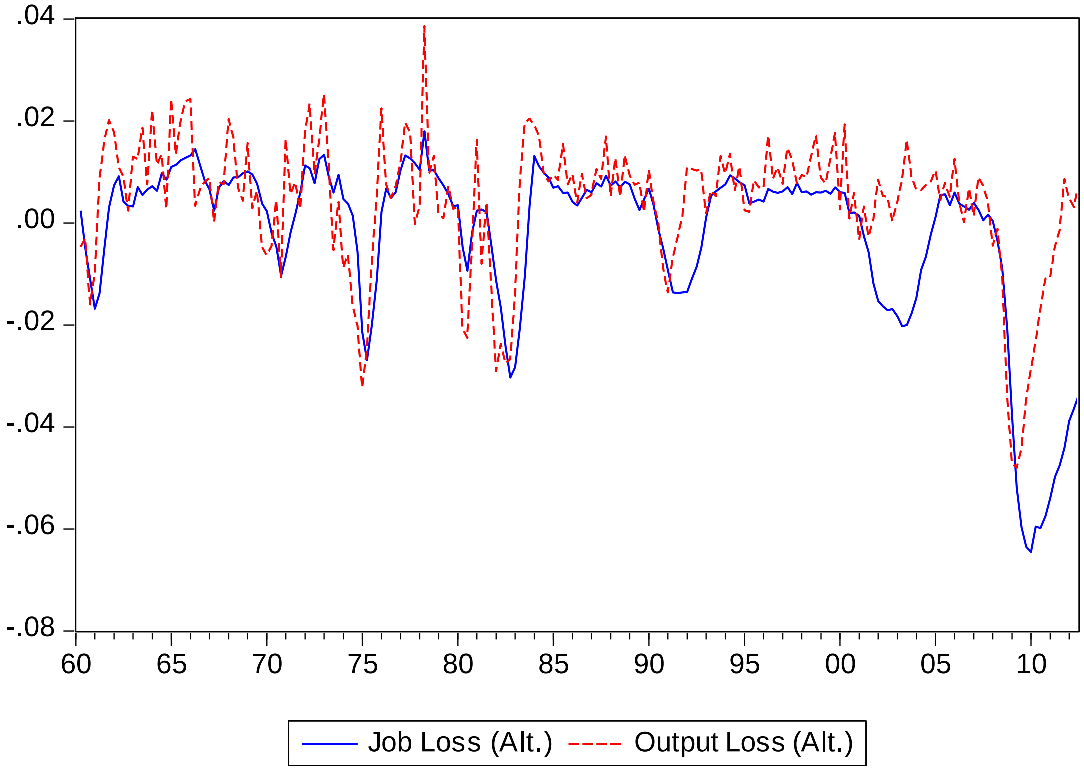

Figure 13: Job Loss - alternate and Output Loss - alternate, continuous measures similar to the discontinous Job Loss and Output Loss which were presented in the body of the paper.

Now that we have continuous measure for Job Loss and Output Loss, we can construct a measure for the Relative Job Loss that is also continuous. Let RJLA = JLA(t)-OLA(t), where RJLA stands for "Relative Job Loss - alternate." We have now constructed a measure for the relative degree of joblessness for any period between 1960 and 2012 that is continuous. RJLA can be found in Figure 14. These measures may be used in future statistical analysis desiring a quarterly measure of relative joblessness without the issue of so many observations being equal to zero.

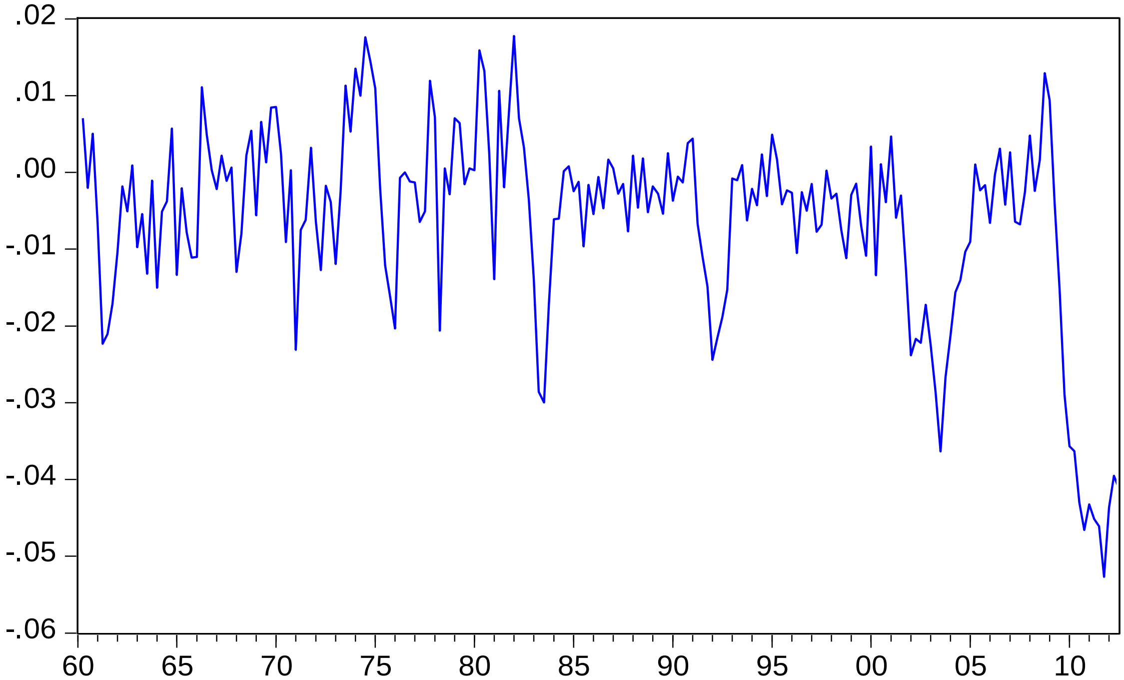

Figure 14: The continuous measure for the Relative Job Loss (RJLA)

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Chapter 2: The Causes of Jobless Recoveries: Cross-sectional Evidence from the 50 United States

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Dissertation Committee:

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Abstract

It has now been well documented that the three most recent business cycles have been accompanied by jobless recoveries. The literature on this topic presents many competing theories as to the cause of this problem. We carefully review the literature to determine what the proposed causes of the jobless recovery problem are. We then perform a cross-sectional analysis of these theories using the newly constructed state-level jobless recovery measure, the "jobless recovery depth". In what is the first statistical test of several of these hypotheses, we find the state-level evidence to be consistent with theories that link jobless recoveries to changes in union power, labor hoarding behavior, and the advent of the great moderation; but not consistent with theories that link them to increased health care costs or income inequality.

1 Introduction

Up until about 1991, business-cycle expansions in the post-war USA came together with almost simultaneous increases in employment. However, this macroeconomic relationship seems to have changed following the recession of 1991, and in all of the economic recoveries observed after that date, output growth has been accompanied by extended periods of continued job losses. Such periods of positive output growth accompanied by zero employment growth have been labeled "jobless recoveries" and are the subject of a recent literature that attempts to understand their emergence.

There have been many competing explanations put forth following the seminal paper of Groshen and Potter (2003). The proposed causes of the jobless recovery phenomenon range from declining union power (Berger, 2012), to increasing health care costs (Aaronson, et al, 2004a; Wessel, 2004), to the relocation of labor across industries (Groshen and Potter, 2003; Garin, et al., 2011), along with at least a dozen others. Although the existence of jobless recoveries has been well established in this literature, there has been little to no movement towards establishing a consensus regarding the cause(s) of this recent change. Although there are many separate theories, there has been very little empirical work done in the area of jobless recoveries and almost no formal statistical testing of these hypotheses.

The lack of econometric work in this field has clearly lagged behind the theory. Our ability to test the many competing hypothesis has been constrained by two important limitations: 1. The lack of comprehensive measures capable of quantifying the extent or severity of a jobless recovery; which hinders our ability to generate positive statements and compare across business cycles. 2. The lack of cross-sectional statistical analysis at the state or regional level; which prevents us from conducting tests that cannot be performed using time-series data alone.

With the formation of the *Jobless Recovery Depth (JRD)* measure in Chapter 1 of this dissertation, the first constraint has been eliminated. This paper is concerned with the second constraint: the lack of cross-sectional statistical analysis. In this paper, we discuss the many different proposed causes of jobless recoveries in the literature, and then seek to create variables using state-level data that will allow us to test these hypotheses. Using these as explanatory variables, and the newly computed state-level *JRD* as our dependent variable, we conduct a cross-sectional study of jobless recoveries which puts several of the existing hypothesis to the test. In some cases, the lack of state-level data prevents us from being able to formally test these hypotheses. We discuss the availability of state-level data and the set of usable explanatory variables in this paper. As far as we are aware, this is the first attempt to study the jobless recovery problem at the state level.

We conduct a series of Ordinary Least Squares (OLS), fixed-effects, and random-effects regressions in which our state-level JRD measure is entered as the dependent variable and a series explanatory variables for which we are able to obtain data are entered as independent variables. Through these regressions, we find evidence in support of arguments that link jobless recoveries to declines in union membership, unusually lengthy expansionary periods between cycles, and reduced output volatility. In contrast, our findings fail to find a robust correlation between jobless recoveries and either income inequality or health care costs[1].

The analysis performed in this paper is an important first step towards narrowing down the large pool of candidate explanations of jobless recoveries. As we show, there are many proposed explanations in the literature to date, but the lack of empirical work means we have been unable to reject any of these hypotheses previously. Our analysis, although not completely comprehensive, finds support for some of these theories, while helping to eliminate some of the others. Future research must continue to test the remaining theories on the causes of jobless recoveries in order to focus in on the true cause(s).

The remainder of the paper is organized as follows: Section 2 provides an extensive review of the literature on the causes of jobless recoveries, Section 3 discusses those independent variables for which state-level data is available and reviews the properties of the dependent variable, the *JRD*, Section 4 discusses the state-level data used for construction of the ex-

[1]A series of regressions with a smaller sample size also failed to find evidence to support theories of labor relocation and household mobility.

planatory variables, Section 5 presents our cross-sectional analysis of the likely causes of jobless recoveries, and, finally, Section 6 concludes.

2 Survey of the Literature on Jobless Recoveries

If anything can be said from a review of the literature on jobless recoveries, it is that there is clearly no consensus as to the cause. While a handful of candidate explanations have received more attention than others, no clear front runner has emerged. Given the lack of empirical testing to date, one cannot ignore any of the previously proposed explanations of the jobless recovery problem. In this section, we set out to introduce the reader to the wide range of theories which have been put forth in the literature and which cannot yet be rejected. Table 1 provides a concise summary of the different proposed causes of jobless recoveries and the supporting research. The remainder of the section examines each theory in slightly more depth. However, the empirical analysis in this paper deals only with those theories for which state-level data is available. Readers who are solely interested in the data used for the empirical analysis and the results may wish to skip over this section, or simply review Table 1.

In the most widely cited paper on jobless recoveries, Groshen and Potter (2003) examine how structural change, which they define as the permanent shifts in the distribution of workers throughout the economy, helps to explain why job growth stalled during the recovery beginning in 2001. The first type of structural change they consider is the prevalence of permanent job losses in place of temporary job losses. In the case of a temporary layoff, both the employer and employee expect the relationship to resume when demand increases. In some instances, the employer may even help the employee file for unemployment benefits so that they will be able to wait until demand increases and return to their old job. A permanent layoff however completely breaks the tie between employer and employee. This requires that both parties must expend a great deal more effort to find a new employee or a new job. Groshen and Potter find that pre-1990, temporary layoffs spiked significantly

Table 1: **Summary of the Literature.** A summary of the proposed causes of jobless recoveries with supporting research papers.

Proposed Causes	**Authors**
Increased repositioning of jobs	Groshen and Potter (2003)
across industries	Aaronson, Rissman, and Sullivan (2004b)
	Elsby, Hobijn, and Sahin (2010)
Increased repositioning of jobs	Schultze (2004)
across regions	Garin, Pries, and Sims (2011)
	Blanchflower and Oswalt (2013)
Increasing length of economic	Koenders and Rogerson (2005)
expansions	Bachmann (2011)
	Berger (2012)
Declining output volatility	Faberman (2008)
(Great Moderation)	Engemann and Owyang (2010)
	Garin, Pries, and Sims (2011)
	DeNicco and Laincz (2013)
Increasing health care costs	Aaronson, Rissman, and Sullivan (2004a)
	Wessel (2004)
Declining union power	Berger (2012)
Rising income inequality	Cynamon and Fazzari (2014)
	Krugman (2014)
Increase use of just-in-time	Schreft and Singh (2003)
employment	Bachmann (2011)
	Panovska (2013)
Increase in permanent layoffs	Groshen and Potter (2003)
	Garin, Pries, and Sims (2011)
Greater job polarization	Jaimovich and Siu (2012)
Inadequate aggregate demand	Aaronson, Rissman, and Sullivan (2004a)
Inadequate labor supply	Schweitzer (2003)
	Aaronson, Rissman, and Sullivan (2004a)
Increasing real wage rigidity	Shimer (2010)
Increasing average labor	Gali and van Rens (2010)
productivity	Elsby, Hobijn, and Sahin (2010)
Credit market shocks	Calvo, Coricelli, and Ottonello (2012)
Short and shallow recessions	Bachmann (2011)
Slow diffusion of new technology	Andolfatto and MacDonald (2006)

with each recession, but that this was not the case in either of the recessions after 1990, suggesting there may have been a structural change in how firms lay off employees. Garin, Pries, and Sims (2011) also note the correlation between jobless recoveries and increases in temporary layoffs.

Groshen and Potter (2003) considered another form of structural change as well: the repositioning of jobs from one industry to another. For their analysis, they examined how jobs adjusted in each of the major U.S. industries during the business cycle. If job losses or gains within a given industry during a recession are quickly reversed when the economy recovers, then these employment changes are considered to be cyclical. Likewise, if jobs continue to grow or decrease within in industry well into the recovery, then these changes are considered structural. They find that structural change in jobs moving from some industries to others was significantly more important in the post-1990 recoveries. Elsby, Hobijn, and Sahin (2010) also discuss structural change across industries, however, they find that unemployment outflows are actually converging across U.S. industries, not diverging, contrary to the hypothesis of Groshen and Potter. This explanation of jobless recoveries is examined yet again by Aaronson, Rissman, and Sullivan (2004b), who find no evidence to support Groshen and Potter's claim, using different method to measure sectoral reallocation.

Schreft and Singh (2003) look at how specific hiring patterns in the U.S. have changed over time and how these changes distinguish the recent jobless recoveries from the typical recoveries of the past. They find that in the recent jobless recoveries that firms have come to rely more heavily on just-in-time employment practices. That is, firms are substituting more flexible labor inputs such as overtime and temporary and part-time workers, in place of less flexible labor inputs. Schreft and Singh reach this conclusion by comparing employment growth during the recovery phase following the recessions of the early 1990's and early 2000's to the average of past business cycles from 1960 to 1989. They find that at least the first year of both post-1990 recoveries were indeed jobless, and accompanied by a significant increase in the use of just-in-time employment methods, which had not been seen in the typical recovery. Finally, they argue that the use of such employment practices gives the firm greater flexibility in employing labor. Firms can therefore expand production given short notice to meet anticipated increases in demand but are still able to wait for indications that this escalation in demand will be sustained. Although they note this correlation, Schreft and Singh provide no clear reason as to why these just-in-time employment techniques are such a recent development. Using data for hours worked in place of employment, they also show that the jobless recoveries in the U.S. have been "hourless" recoveries as well.

This finding is at odds with the finding of Panovska (2013) who finds that hours have seen a significant increase during the recent periods of jobless recovery. Panovska argues that firms have substituted away form adding new workers and have simply caused their existing employees to work longer hours. Bachmann (2011), also argues that firms with increase hours worked and not employees when the economic recovery is a fairly weak one.

Berger (2012) presents a quantitative model where firms are able to restructure during recessions. This model includes worker heterogeneity, endogenous entry and exit, labor adjustment costs and aggregate shocks. Unlike previous models in this area, Berger's model is able to explain both the joblessness of recoveries and the recent countercyclicality of labor productivity to the business cycle, while still being a good fit for the entire post-war period. Berger argues that one major cause of the structural change observed in the 1980s is the significant decline in union power, which allowed firms to fire employees more selectively. Specifically, he presents cross-state and cross-industry tests of how output and productivity were affected by decreasing union coverage, while ignoring the issue of how joblessness may be related to union power. Berger performs a cross-state statistical test of the relationship between declining union coverage rates and the correlation of output and labor productivity. However, he does not extend his statistical analysis of the union power hypothesis to the question of jobless recoveries, only to the question of the recent countercyclicality of labor productivity.

Garin, Pries, and Sims (2011) argue that jobless recoveries are just one of several important changes in the nature of economic fluctuations in recent years. They point to four other important facts about the economy that have arisen along with jobless recoveries: decreased output volatility (the Great Moderation), labor productivity becoming acyclical, a decline in the importance of the "efficiency wedge" relative to the "labor wedge", and the dramatic decline in temporary layoffs over the last three recessions. The authors contend that these changes are all related in some way and call for a common explanation. The main hypothesis of the paper is that reallocative shocks now play a more important role in the economy relative to aggregate shocks. They argue that the relocation of labor across regions and industries will take time, and employment will recover more slowly than before. However, others (Schultze, 2004; Blanchflower and Oswalt, 2005) have argued that it is actually decreases in labor mobility (relocation) that have contributed to jobless recoveries.

The correlation between the Great Moderation and jobless recoveries was one noted not only by Garin, Pries, and Sims (2011), but by several others as well. Faberman (2008) performs structural break tests for job flows in and out of the market, looking for a break in the year 1984, the year commonly associated with the start of the Great Moderation. Engemann and Owyang (2010) and DeNicco and Laincz (2013) also argue that the decline in output volatility may have caused the subsequent jobless recoveries.

Koenders and Rogerson (2005) and Bachmann (2011) put forth a new explanation for the jobless recovery phenomenon that focuses on dynamics within growing organizations and the inter-temporal substitution of organizational restructuring. Their analysis implies that slower employment growth following a recession is a characteristic of unusually long expansions. Longer expansions lead to more labor hoarding on the part of firms and a greater need to restructure following economic downturn. Firms will wait until recessions, when the cost is relatively low, to shed their less productive workers. This provides the firm with a period, during recovery, where only the productive employees remain following the restructuring and there is little to no need to add more labor to meet increases in demand.

Gali and van Rens (2010) also seek to explain multiple changes in business cycle phenomena with a single model. They hypothesize that increases in labor market productivity are driving the acyclicality of labor productivity and the increased volatility of employment. Lower costs of adjustment lead to less labor hoarding, producing more acyclical labor productivity. However, this explanation is not consistent with the recent business cycle phenomenon of jobless recoveries. Lower labor adjustment costs cannot explain both of these facts simultaneously. Elsby, Hobijn, and Sahin (2010) also point out the correlation between jobless recoveries and increased average labor productivity. However, whether increased labor productivity is a potential cause of jobless recoveries or a product of jobless recoveries has not been established.

Aaronson, Rissman, and Sullivan (2004a) point out that increases in productivity alone do not explain why jobs are not being added to the economy. Rather, the fact that productivity increases have not been accompanied by proportional gains in aggregate demand is the real issue as it applies to the jobs market. The authors suggest that a series of shocks following the recessions of 1990 and 2000 may have led to unusually slow AD growth. Possible shocks responsible for this include: concerns over terrorism, hangover from the fall in the stock market, the credit crunch resulting from the savings and loan crisis, and revelations of poor corporate governance.

While several other theories have focused on factors affecting labor demand (just-in-time employment, health care costs, etc.), Aaronson, Rissman, and Sullivan (2004a) point out that changes in labor supply are another potential explanation. They consider labor force participation rates as a proxy for labor supply and show that the recent jobless recoveries have experienced considerably lower labor force participation rates than previous recoveries. This finding is consistent with the work of Schweitzer (2003). However, Aaronson, et al. are quick to mention that labor force participation rates may be a poor proxy for labor supply as they may reflect the expectation of finding a job, not the willingness to work.

Calvo, Coricelli, and Ottonello (2012) consider how shocks to the credit market may result in a sluggish labor market leading to the recent jobless recoveries. They find that the labor market adjusts much more slowly in recessions where the primary driving force is a disruption in the credit market, as opposed to the recessions not brought on by credit market shocks. In their model, recessions induced by credit market shocks can either result in a jobless recovery or in persistently low real wages, depending upon the pattern of inflation during the recession. They find that high inflation is correlated with low real wages during a recovery, while low inflation is correlated with joblessness during a recovery.

Aaronson, Rissman, and Sullivan (2004a) and Wessel (2004) present two arguments as to why increases in health care costs could possibly be responsible for jobless recoveries. The first is that higher health care costs have pushed the total cost of labor above equilibrium, reducing labor demand. The second argument suggests that any increase in fixed costs per worker, which do not vary with hours worked or base salary, is likely to have an adverse effect on the hiring of new employees. Data on health care costs show increases over the time period associated with jobless recoveries.

Jaimovich and Siu (2012) argue that jobless recoveries are correlated with increasing job polarization. Job polarization is a term that refers to the disappearance of jobs in the middle of the skill distribution. The majority of the decline in middle-skill occupations occurs during times of economic downturn. They conclude that jobless recoveries in middle-skill occupations account for jobless recoveries in the aggregate. This is supported by the fact that recoveries are not jobless in high-skill or low-skill occupations. Also, job polarization was not present in the economy prior to the age of jobless recoveries.

Shimer (2010) examines how jobless recoveries can be generated by real wage rigidities. He shows that in a neoclassical growth model, following a shock, if wages are permitted to be flexible then they will fall as the economy grows back to trend. On the other hand, if wages are perfectly inflexible, or rigid, then the shock will result in a permanent decline in employment and output relative to trend. Extending the neoclassical model to allow for firms to recruit more labor, a search model, Shimer shows that even under the condition of rigid wages, the effects of the shock will eventually disappear. It should be noted that the data for the recovery period following the Great Recession was very limited at the time this paper was written, as it included data only up through the second quarter of 2010. Also, the model in the paper captures the declines in both employment and output, yet it says little about their divergent behavior of the two during the recovery phase, which is really the heart of the jobless recovery problem. Also, the paper only presents data beginning in

1990. Rather than address the causes of jobless recoveries, the paper seems to focus more on the recession phase of the past three business cycles, while the recovery phase has been identified as the period of interest in the literature.

Andolfatto and MacDonald (2006) argue that jobless recoveries are in fact the exact prediction of neoclassical theory under conditions of new technologies unevenly impacting different sectors of the economy and being slow to diffuse. It is their contention that jobless recoveries are the result of many different types of shocks and mechanisms that are all interacting at a given moment in time. However, they contend that technology shocks are a primary driving force. They set up a RBC model where GDP growth is driven by advances in technology that raise total factor productivity, however this technology does not affect the entire economy evenly nor instantaneously. This model yields labor and income results that are quantitatively similar to those observed during the recent jobless recoveries.

Bachmann (2011) investigates the jobless recoveries after the 1991 and 2001 recessions. While the majority of explanations have been based on the concept of structural change in the economy, Bachmann focuses on how a cyclical mechanism has contributed to the generation of these jobless recoveries. He builds upon the previous work of Hansen and Sargent and computes a DSGE model of heterogeneous establishments that use both an intensive margin (hours worked) and an extensive margin (employment) of labor services. When establishments face adjustment costs to employment, then following a short and shallow recession the need to hire new employees is relatively weak. Also, the model shows that firms will increase average hours per worker in the earlier portion of a weak recovery, delaying new hires. Bachmann concludes that this is a fairly good fit to the U.S. data. However, although the paper has been updated as recently as 2011, it does not include any data for the Great Recession, which may present some problems given that this recession was neither short nor shallow and yet a jobless recovery still ensued.

3 State-Level Variables

In this section, we will discuss the construction of both dependent and independent variables at the state level. First, we review the properties of our dependent variable, the "jobless recovery depth" (*JRD*), and discuss its usefulness in a cross-sectional study such as this. We then discuss which of the aforementioned candidate causes of jobless recoveries has the necessary data available at the state level for the construction of an explanatory variable. As we will see, the state-level data is somewhat limiting, and not all of the theories that have been presented in the literature can be tested in a cross-state analysis. However, several of the leading explanations can be tested. For those potential causes where data does exist, we will provide a slightly deeper review of the literature in order to understand what the theory tells us concerning the expected direction of each variable's effect on the magnitude of a jobless recovery. We also discuss the sources of all state-level data, and any manipulations made to the data prior to the econometric analysis.

3.1 Dependent Variable

Recall from Chapter 1 of this dissertation, that one of the ways in which empirical work in the area of jobless recoveries has been constrained is that no meaningful measure of the magnitude of a jobless recovery has been created. However, this constraint was eliminated with the introduction of the *JRD* measure in Chapter 1[2]. The *JRD* has several important properties that make it a strong variable for gauging the multiple aspects of jobless recoveries. Specifically, a ceteris paribus increase in the percentage of jobs lost during the cycle increases the *JRD*, a ceteris paribus increase in the output growth experienced during the cycle increases the *JRD*, and a ceteris paribus increase in the time it takes for the employment to recover also increases the *JRD*. These responses show that our measure is consistent with the literature and the data regarding the characteristics of past jobless recoveries in the

[2]Descriptions of the what the *JRD* is, how it was constructed at the state level, and the state-level data used are all laid out in full in Chapter 1 of this dissertation. Please refer back to Chapter 1 to answer any questions regarding the *JRD* measure.

U.S.

Table 2: ***JRD* Summary Statistics.** Basic summary statistics for the state-level JRD measures, covering the years 1960-2012. Column one shows statistics for the average business cycles before 1990. In turn, column 2 shows statistics for the cycle that begun in the third quarter of 1990, column 3 for the cycle that begun in the first quarter of 2001, and column three for the cycle that begun in the fourth quarter of 2007

JRD	Pre-1990 average	1991	2001	2009
Mean	-0.196	0.007	0.099	0.078
Median	-0.010	-0.011	0.048	0.082
Standard Dev.	0.724	0.289	0.1732	0.254
Minimum	-7.700	-0.957	-0.285	-0.557
Maximum	0.420	0.849	0.570	0.518
Sample size	250	50	50	50

It is also important to note that there is a great deal of variation across states and time in the severity of jobless recoveries as measured by the *JRD*. If not for this variation, a cross-state study such as this would be fruitless. However, we see that there are truly dramatic differences across time and space. Table 2 shows basic summary statistics for the *JRD* variable for all 50 states. Similar to what has been previously shown in the national data, jobless recoveries appear to be growing worse over time. Also, the range of *JRD* values, and their standard deviation, indicate that states have experienced joblessness very differently from one another over time.

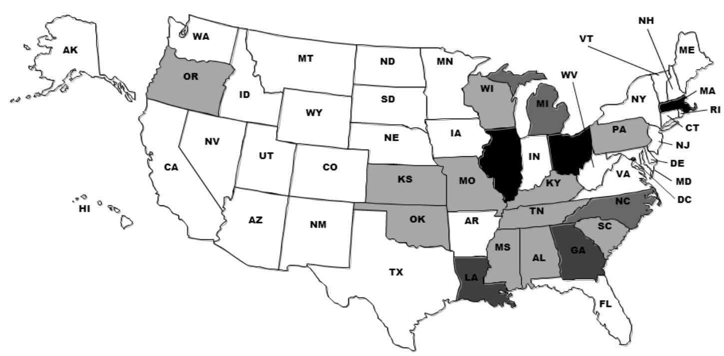
This is further evidenced by inspecting the maps in Figure 1. Here, each state is shaded according to its *JRD* value, where a darker shade indicates a more severe period of joblessness. From these maps, we see that jobless recoveries are not a nation-wide phenomenon, but rather a somewhat localized event, occurring in certain states, often clustered around one another. This observation is completely lost using the national data alone. However, it is also clear that many of the states exhibiting jobless recoveries in Figure 1 are experiencing fairly mild instances of joblessness. Several fall into the lowest category and are shaded with the lightest shade of gray. In order to show that jobless recoveries truly are somewhat localized events, we do a robustness check by eliminating all of the “mild” jobless recoveries from the map. In other words, we do not want our findings to be watered down by including several states that experience only mild instances of joblessness, so we replace the lowest shade of gray with white in Figure 2. Even when we eliminate the most mild instances of jobless recoveries, we still see a great deal of variation across time and state, with jobless recoveries clustered in certain regions.

Figure 1: Maps of all 50 U.S. states shaded according to their *JRD*, with darker states having a higher *JRD*

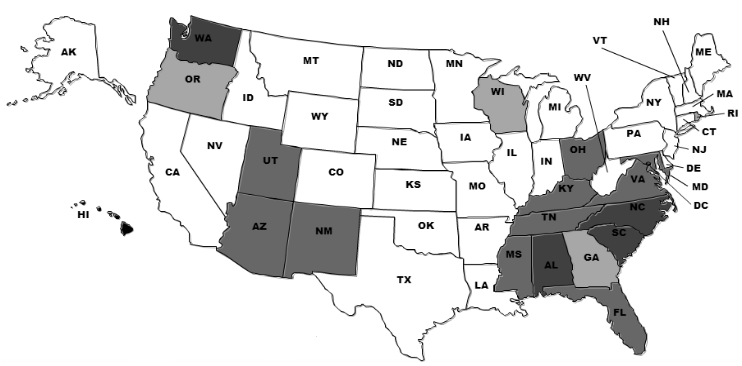
Average all cycles, 1960Q2 – 1990Q2

Business Cycle of 1990Q3 – 2000Q4

Business Cycle of 2001Q1-2007Q3



Business Cycle of 2007Q4 – 2012Q4 (ongoing)



JRD Range	Shade
(-∞, 0]	
(0, .125)	
[.125, .25)	
[.25, .375)	
[.375, .50)	
[.50, ∞)	

Figure 2: Maps of all 50 U.S. states shaded according to their *JRD*, with darker states having a higher *JRD*

3.2 Independent Variables

Although we would like to test each of the theories on the causes of jobless recoveries, the availability of data at the state level constrains our analysis. In this section, we discuss those candidate explanations for which state-level data does exist. We then further discuss the existing research involving those theories for which we have data, in order to determine the expected direction of the effect each will have on our measure for the severity of a jobless recovery, the *JRD*. We summarize the theoretical arguments built around each candidate cause separately, but the description of the data we collected in order to test them is provided in the next section.

One of the proposed causes of a jobless recovery for which state-level data is available is the decline in union power. According to Berger (2012), declining union power across the U.S. has been the major contributing factor to the jobless recovery phenomenon. His argument is based upon the restrictions faced by firms when firing their employees. As unions lose power, firms are able to be more selective in their firing decisions, allowing them to keep employees based on factors such as performance and not seniority. Then, when the economy recovers and demand increases, firms already have employees which are a good match, eliminating some of the incentive to hire new employees. This differs from times when unions are more powerful, and senior employees often keep their jobs even if they aren't the most productive. In this case, firms have an incentive to hire high-productivity individuals during periods of economic recovery to compensate for their long-tenured workforce that may not be entirely productive. The resulting hypothesis is that as union power decreases, the magnitude of the jobless recovery increases.

Koenders and Rogerson (2005), Bachmann (2011), and Berger (2012) argue that the length of an expansionary period leading up to a recession affects the degree to which jobs lag output when the recovery begins. Koenders and Rogerson argue that certain inefficiencies regarding labor build up in an organization over time. Since restructuring labor requires a diversion of resources away from current production, firms lack an incentive to do so until times of economic decline when the opportunity cost of restructuring is relatively low. Longer periods of economic expansion would therefore be expected to generate greater labor inefficiencies. This will result in a significant number of jobs being shed during the recession with little reason for newly streamlined firms to add back inefficient labor. The hypothesis which is generated from this argument is that the longer the preceding period of economic expansion, the more severe the jobless recovery.

To test whether the decline in union power or the length of the expansionary periods that precedes a cycle can in fact explain the emergence of jobless recoveries, we collected data on three related variables: the state-specific *length of expansion*, the states' *union membership* rate, and the states' *forced unionism* legislation[3]. To construct our length of expansion variable, we simply utilize the seasonally-adjusted, real earnings data and count the number of quarters of expansion in output that take place prior to the beginning of each cycle. That is, we count the number of quarters in the previous cycle for which output exhibited positive growth.

In order to construct the union membership variable, in turn, we utilized union membership rates for each state from Hirsh et al. (2001). They combine data from the BLS Current Population Survey and the Directory of National Unions and Employee Associations and estimate the percentage of nonagricultural wage and salary employees who are union members, including in the public sector, from 1964 to 2012. The data is reported on an annual basis but for the purposes of our study, a cycle-specific measure is required. We then used the average fraction of non-farm employees covered under a collective bargaining agreement during the entire cycle as the relevant measure of union membership. Finally, to construct the forced unionism variable, we created a dummy variable using data from the national right to work legal defense foundation. For each particular state and cycle, this dummy equals 1 if force-unionism legislation is in place at the beginning of the cycle and

[3]Some states have adopted legislation which allows employees the "right to work" for a firm with a unionized labor force, without having to pay union fees. This is known as right-to-work law and is believed to decrease a union's power as it can no longer force those covered under the collective bargaining agreement to pay their membership dues. States which have not adopted such legislation are referred to as "forced unionism" states.

zero otherwise.

Another one of the candidate causes we are able to study at the state level is the advent of a much documented decrease in the volatility of aggregate economic variables; a shift known as the "great moderation". Authors like Engemann and Owyang (2010), Faberman (2008), and DeNicco and Laincz (2013) have presented convincing evidence showing the emergence of jobless recoveries at the aggregate level coincides with the beginning of the great moderation. Despite the evidence they provide, however, very few explanations can be found in the literature as to why this could be the case. The only exception that we are aware of is the paper by Garin, Pries and Sims (2011). We will test the hypothesis that lower output volatility will increase the magnitude of a jobless recovery.

Garin, Pries and Sims (2011) use a labor reallocation model and show that a decline in the importance of aggregate shocks relative to re-allocative shocks can simultaneously account for both the jobless recoveries and the great moderation. In their model, re-allocative shocks precipitate the relocation of workers "across sectors, professions, or geographic regions" and, since displaced workers are more likely to experience lengthier unemployment spells and steeper learning curves for their subsequent occupations, the relocation process may lead to a jobless recovery. The explanation put forth by Garin, Pries and Sims 2011 is justified by the evidence presented in Foerster et al (2011); which shows re-allocative shocks have indeed become relatively more important than aggregate shocks. But it is at odds with an empirical literature which fails to find a direct link between the relocation of workers and jobless recoveries. In one of the earlier studies, Groshen and Potter (2003) presented evidence suggesting job relocations across industries may be responsible for jobless recoveries. Later studies by Aaronson et al (2004a), Aaronson et al. (2004b), and Panovska (2013), however, have failed to support that hypothesis. Others like Blanchflower and Oswald (2013) actually suggest that greater labor mobility is positively correlated with employment.

We would like to test whether the correlation found at the aggregate level between jobless recoveries and the great moderation, can also be found at the state-level. For that purpose, we construct a state-level measure of output *volatility* for the cycle. More specifically, we compute the absolute values of the first-differences in the quarterly growth rates of *earnings* (a measure roughly equivalent to one of the measures of volatility used by McConnell and Perez-Quiros (2000) using U.S. GDP data, in their seminal paper that documents the Great Moderation) and take the average of these values over the entire cycle as our measure of volatility. The smaller the volatility, the greater the moderation. Unfortunately, we were unable to obtain adequate state-level data documenting neither the extent nor the type of workers' relocations, so a deeper test of the arguments put forth by Garin, Pries and Sims (2011) was not possible. In the appendix at the end of the paper we present an attempt to fill that gap using what limited state-level data is available.[4]

Finally, the last two theories which we are able to test with state-level data are the rise in *health care costs* and the increase in *income inequality* that have taken place in the USA in recent decades as candidate causes of jobless recoveries. Aaronson, et al. (2004) and Wessel (2004) argue that rapidly increasing health care costs have played an important role in firms being reluctant to add new employees. Specifically, increases in health care costs can be reflected as increases in fixed costs per worker, regardless of salary level or the number of hours worked. This provides an incentive for firms to squeeze as much work as possible out of their existing employees, for example through working longer hours, and to hire part-time and temporary employees who are not going to incur these fixed costs for the firm. Importantly, higher health care costs, as argued here, could lead to firms relying more heavily on just-in-time employment practices as stated by Schreft and Singh (2003). State-level data on part-time and temporary employment do not appear to exist for the sample period of our study, but health care cost data does exist over a significant portion of that period. If health care costs are driving both jobless recoveries and just-in-time employment, this helps to fill the hole left by not having the just-in-time employment data. From these arguments, the expectation is that greater increases in health care costs will result in greater jobless recoveries, and more income inequality will result in greater jobless recoveries. While

[4]As shown in Table 8 in the Appendix, in the reduced-sample regressions, the estimated coefficients for the homeownership and relocation variables are always insignificant. Thus, our results suggests that relocation and household mobility do not significantly explain variations in the magnitude of jobless recoveries.

Cynamon and Fazzari (2014) and Krugman (2014) argue that jobless recoveries are at least partially due to the drag in demand that is generated by the rising inequality of income. We are not aware, however, of any papers that formally test these arguments.

We attempt to quantify the burden imposed on firms by health care costs at the state level, by utilizing data provided by the Office of the Actuary of the Center for Medicare and Medicaid services (CMS). The CMS estimates Health Care expenditure by State of Provider for each state. The CMS reports the sum of total revenues generated by all health-care related industries. These estimates are reported annually from 1980-2009 (so they constrain our sample a little) and are not adjusted for inflation. We use the GDP deflator to calculate real, per-capita health care expenditures for each state, and then take the annual average over each cycle. In turn, in order to track changes in income inequality at the state level, we borrow data from Frank (2008) and record the corresponding Gini coefficients for each state at the start of each cycle. Frank (2008) constructs Gini coefficients using adjusted gross income from the IRS's *Statistics of Income* publication.

To summarize, the set of explanatory variables representing our list of "candidate causes" for which we have state-level data, includes measures of union membership, forced-unionism, output volatility, length of preceding expansion, income inequality, and health-care costs[5]. We were able to gather data going back to 1960 for almost all variables, with some exceptions. For the length of expansion variable, for example, since our data does not go beyond 1960, we are unable to compute this number for the cycle that begins in 1960 and we are forced to work with a reduced sample whenever the length of expansion variable is included. A similar problem occurred with the health-care costs. Summary statistics for all explanatory variables are displayed in Table 3; ordered according to the number of observations we were able to obtain. Health care costs are reported in thousands of dollars throughout the paper.

[5]Relocation and household mobility variables were available, but only over a smaller range and are analyzed in the Appendix

Table 3: **Additional Summary Statistics.** Basic summary statistics for the explanatory variables for all cycles considered

Variable	n	Mean	S.d.	Min	Max
UNION MEMBERSHIP	400	18.18	8.85	3.10	42.10
FORCED UNIONISM	400	0.61	0.49	0.00	1.00
VOLATILITY	400	0.17	0.12	0.01	0.98
INCOME INEQUALITY	400	0.52	0.06	0.41	0.70
LENGTH OF EXPANSION	350	20.89	10.88	1.00	41.00
HEALTH CARE COSTS (1=$1000)	250	4.36	1.91	1.5	9.45

4 Data Description

One unique aspect of our empirical analysis is the use of business cycle-specific variables. That is, we use a simple value to represent the entire business cycle. The *JRD* is a cycle-specific measure by definition; it sums the difference between output losses and employment losses over the entire cycle. Since the *JRD* is our dependent variable, we need independent variables that are cycle-specific as well. Although we have already mentioned where most of the data comes from for the construction of these variables, it is important to understand exactly how each cycle-specific independent variable was constructed. This section explains all the relevant details of the data including where is came from, what adjustments were made, and how this data was then turned into a cycle-specific measure for use in our analysis.[6]

As a measure of union power, union membership rates for each state are taken from BLS's Current Population Survey, and the Directory of National Unions and Employee Associations, combined and reported by Barry Hisch and David Macpherson at unionstats.com. This database provides union membership density from 1964-2012. This data is reported on an annual basis and represents the percentage of non-farm employees covered by a collective bargaining agreement. However, to match this measure of union power appropriately with

[6]A description of the data for our relocation and household mobility variables is found in the Appendix.

the number of time periods of our jobless recovery measure, we need only one value for each business cycle. We break the sample into the 8 business cycles identified by the NBER from 1960-2012 and take the average union membership density over the cycle. Thus, our measure of union power is the within-cycle average percentage of non-farm employees covered under a collective bargaining agreement.

The eight NBER defined business cycles over our sample period are the "peak to peak" business cycle intervals, and are reported here (the intervals are: [1960q2, 1969q4), [1969q4, 1973q4), [1973q4, 1980q1), [1980q1, 1981q3), [1981q3, 1990q3), [1990q3, 2001q1), [2001q1, 2007q4), and [2007q4, 2012q4[7])). At first glance, this way of choosing the business cycle dates for the individual states may seem problematic. At close inspection, however, it is safe to say that states enter and exit business cycles in synchrony with the national economy. We compare the dates of the peaks and troughs for the nation provided by the NBER to the state level output series we are using (*earnings by place of work*) in order to judge how closely most state economies follow the national economy. We examine the 3 most recent business cycles and find that 93% of the time, state troughs are within 2 quarters of the national date, and 79% of the time, state peaks are within 2 quarters of the national date[8]. Thus, it seems that the state economies typically move in reasonably close synchrony to the national economy, helping to justify the use of national business cycle dates for the construction of all the state-level variables.

For information regarding which states have passed right-to-work legislation and the dates such legislation was instituted, we referred to the National Right To Work Legal Defense Foundation's website, NRTW.org. A simple dummy variable is constructed for each state for each business cycle. A value of 0 is given a forced-unionism state or to a state which adopts right-to-work legislation during the given cycle. A value of 1 is given to a state with right-to-work legislation beginning at any point prior to the current cycle.

[7]The last business cycle is ongoing, and the next peak has not yet been established. Here, we use the last quarter for which data is available

[8]92% of state peaks are within 4 quarters of the national date

Length of expansionary period was calculated using the same series to approximate state output that has been used throughout the paper, *earnings by place of work* from the BEA regional accounts. *Earnings* was seasonally adjusted and converted into real terms. Length of expansionary period is simply the number of quarters of expansion in output prior to a recession. That is, each state will report a number of quarters of expansion for a given business cycle which comes from the previous expansionary period. Thus, we do not have this measure for the first cycle beginning in 1960 as it would require the use of data beginning in 1958, which data is intentionally excluded from this study.

For claims regarding changes in output volatility associated with the Great Moderation, we construct a measure of output volatility, once again using our proxy for output, *earnings by place of work*. To capture how volatile state output has been, we begin by calculating the growth rate of *earnings*. We then take the first difference of the growth rate in order to see where large jumps occur in the growth rate of output. We take the absolute value of this number as we are not concerned with the direction of the volatility. Now that we have a quarterly measure of output volatility for each state, which roughly follows McConnell and Perez-Quiros (2000), we construct business cycle specific measures. Each business cycle's output volatility is simply the average volatility over the entire cycle. In short, that is the average of the absolute value of the first difference of the growth rate of *earnings by place of work*.

The data seem to support the claim of the BEA that *earnings* may proxy well for production. The average correlation coefficient between annual state GDP levels and annual state *earnings by place of work* is 0.9977. Thus, at the state level, the correlation between GDP and our proxy seems very strong when using the annual data. Of course, we cannot evaluate whether this is also true when using quarterly data (quarterly, state-level GDP measures do not exist); but we still made an effort to document the quarterly correlation at the national level. National data for both GDP and *earnings by place of work* are available at a quarterly frequency and have a correlation of 0.7272. Both the annual state-level correlations and the quarterly national-level correlations suggest that *earnings* is indeed a reasonable proxy for

GDP.

In addition, given that for the purpose of calculating the *JRD* we require an approximation for the percentage changes in GDP and not for the GDP levels themselves, we also looked at how annual changes in earnings at the state level correlate with annual changes in state-level GDP. We conducted standard OLS regressions between the state-level, annual changes in GDP and the corresponding state-level annual changes in *earnings*. In these regressions, *earnings* are significant at the 1% level for all 50 states and explain about 75.6% of the observed variation in GDP, on average (the average R-squared for the 50 regressions was 0.756).

Health Care by State of Provider is an estimate of the total revenues received by health care providers for providing health care goods and services within a state for a given year. These estimates are provided by the Office of the Actuary of the Centers for Medicare and Medicaid Services. The main source of health care data used to construct these estimates comes from the quinquennial Economic Census. This census collects data every five years for each state by industry using the North American Industry Classification System (NAICS). The State of Provider estimates use data from the following industries: Hospital Care, Physician and Clinical Services, Other Professional Services, Dental Services, Home Health Care, Nursing Care Facilities and Continuing Care Retirement Communities, Prescription Drugs and Other Non-Durable Medical Products, Durable Medical Products, and Other Health, Residential, and Personal Care. The Office of the Actuary uses the Economic Census data by industry along with some individual hospital and agency data made available for federal hospitals and agencies within states. Total revenue for each health care industry along with hospital and state specific values from federal institutions are summed and controlled to the National Health Expenditure Accounts. Thus, Health Care by State of Provider is an estimate of total revenue for health care goods and services, not just those paid for by those using Medicare and Medicaid.

For our health care costs measure, we take total revenue for health care goods and services as reported by the Center for Medicare and Medicaid services as Health Care by State of

Provider and convert it into real terms using the GDP deflator. To account for the size of each state, we then construct real per capita Health Care by State of Provider, using state population statistics from the U.S. Census Bureau. It is our hope that his measure will be correlated with employer-specific health care costs.

Finally, for our measure of income inequality at the state level, we use the publicly available data provided by Frank (2008). Frank constructs Gini coefficients using adjusted gross income from the IRS's *Statistics of Income* publication. We use the corresponding Gini coefficients for each state at the start of each cycle as our cycle-specific measure of income inequality. We choose the start of cycle Gini and not the cycle-average Gini to avoid issues of endogeneity.

5 Empirical Analysis and Results

Using the state-level *JRD* measures as the dependent variable and elements from our list of "candidate causes" as explanatory variables, we conduct a series of OLS, fixed-effect, and random effects regressions. A first set of results can be found in Tables 4 and 5. Table 4 presents the estimated coefficients for simple bivariate OLS regressions. Table 5 presents the estimated coefficients for multivariate OLS regressions, where candidate causes are added to the list of explanatory variables one by one. Robust standard errors were used in all cases.

By and large, the results from Tables 4 and 5 are consistent with the theoretical arguments discussed previously: declines in union membership, lengthier expansions predating the cycles, lower output growth volatility, and higher health care costs are all positively and significantly associated with more severe jobless recoveries. These correlations are robust across different econometric specifications used and to the inclusion of lagged *JRD* values (not shown). Greater income inequality is also positively associated with the *JRD* measure, but the coefficient is not statistically different from zero once other independent variables are included in the regression.

Table 4: Bivariate OLS Regressions.

	1	2	3	4	5	6
Union Membership	-0.003					
	(-1.52)					
Forced Unionism		0.186**				
		(2.50)				
Volatility			-3.44***			
			(-4.50)			
Income Inequality				0.56**		
				(2.03)		
Expansion					0.014***	
					(4.88)	
Health-care						0.045***
						(4.34)
R-sqr	0.002	0.022	0.461	0.003	0.070	0.06
N	400	400	400	400	350	250

T-statistics in parenthesis,***significant at 1%, **significant at 5%, *significant at 10%.

Table 5: Multivariate OLS Regressions

	1	2	3	4	5	6
Union Mem.	-0.011***	-0.011***	-0.005***	-0.008****	-0.014***	-0.008**
	(-4.25)	(-4.84)	(-2.05)	(-2.88)	(-4.69)	(-2.36)
Forced Union	0.293***	0.164***	0.136***	0.174***	0.210***	0.010**
	(3.27)	(3.76)	(3.31)	(3.75)	(3.99)	(2.45)
Volatility		-3.40***	-3.44***	-3.35***	-3.243***	-1.846***
		(-4.52)	(-4.55)	(-3.60)	(-3.57)	(-4.65)
Income Ineq.			0.98***	1.31**		-0.63
			(2.37)	(2.28)		(-1.33)
Expansion				0.005****	0.006***	0.005***
				(3.16)	(3.92)	(3.50)
Health-care						0.035**
						(2.48)
Constant	-0.078	0.558***	-0.013	-0.324	0.414**	0.40
	(-1.35)	(4.72)	(-0.06)	(-1.29)	(2.41)	(1.58)
R-sqr	0.041	0.4821	0.4389	0.484	0.4749	0.4906
N	400	400	400	350	350	250

T-statistics in parenthesis, ***significant at 1%, **significant at 5%, *significant at 10%.

Table 6: Fixed-Effects Regressions

	1	2	3	4	5	6
Union Membership	-0.014***	-0.017***	-0.009**	-0.020***	-0.026***	-0.016**
	(-3.74)	(-4.50)	(-2.11)	(-3.52)	(-4.61)	(-2.06)
Forced Unionism	0.174***	0.247***	0.276***	0.260***	0.248***	0.315***
	(3.63)	(3.68)	(3.65)	(3.88)	(3.81)	(9.78)
Volatility		-2.897***	-2.90***	-3.04***	-3.02***	-1.585***
		(-3.23)	(-3.23)	(-3.48)	(-3.50)	(-3.14)
Income Inequality			0.947**	0.620		-0.12
			(2.29)	(1.26)		(-0.22)
Expansion				0.006***	0.006**	0.006***
				(4.21)	(4.60)	(3.92)
Health-care						0.005
						(0.32)
Constant	0.056	0.553***	-0.09	0.134	0.549**	0.209
	(0.97)	(3.00)	(-0.34)	(0.48)	(2.39)	(0.68)
N	400	400	400	350	350	250

T-statistics in parenthesis, ***significant at 1%, **significant at 5%, *significant at 10%.

However, the forced-unionism dummy is also significantly correlated with the *JRD* measure; but the sign of the coefficient is opposite to what we expected initially. A potential explanation for the positive correlation between the *JRD* measures and the forced-unionism dummy could be that differences between right-to-work and forced-unionism laws change the nature of the bargaining between firms and workers away from employment warranties and towards other objectives. But we have no evidence to support that claim. Furthermore, the fact that forced-unionism legislation is likely to be related to other, potentially omitted, state-level characteristics makes it possible for the OLS estimates to suffer from endogeneity biases and calls for the use of fixed-effects techniques instead of OLS.

The estimated coefficients from the fixed-effects regressions are provided in Table 6. As shown in this table, the results obtained from the fixed effects regressions do not vary much from those obtained in the OLS regressions. The one exception is that the estimated

Table 7: Random-Effects Regressions

	1	2	3	4	5	6
Union Membership	-0.013***	-0.010***	-0.006**	-0.008***	-0.014***	-0.008**
	(-3.77)	(-4.41)	(-2.14)	(-2.65)	(-4.52)	(-2.58)
Forced Unionism	0.28***	0.164***	0.136***	0.174***	0.210***	0.010**
	(2.26)	(4.80)	(4.50)	(5.45)	(5.81)	(2.52)
Volatility		-3.40***	-3.44***	-3.35***	-3.24***	-1.846***
		(-4.69)	(-4.72)	(-4.85)	(-4.78)	(-4.48)
Income Inequality			0.983**	1.31***		-0.632
			(2.39)	(2.70)		(-1.35)
Expansion				0.006***	0.006***	0.005***
				(3.93)	(4.83)	(3.91)
Health-care						0.035**
						(2.47)
Constant	-0.03	0.558***	-0.013	-0.324	0.414***	0.406
	(-0.48)	(4.36)	(-0.07)	(-1.49)	(2.82)	(1.79)
N	400	400	400	350	350	250

T-statistics in parenthesis, ***significant at 1%, **significant at 5%, *significant at 10%.

coefficient on the health care costs variable now becomes insignificant and does not appear to have an effect on the magnitude or depth of jobless recoveries. For comparison purposes, the estimated coefficients from the random effects regressions are also provided in Table 7. The results are again very similar with the exception of the health care costs variable which appears significantly correlated with the *JRD* in the random effects specification. A Hausman specification test for this last regression in column 6, however, suggests the use of fixed effects is more appropriate, consistent with our prior beliefs about the correct specification.

6 Conclusion

While there are many competing theories as to what may cause jobless recoveries, it is clear that the empirical work necessary to complement the existing research has lagged behind. The reason, at least in part, is that the few empirical studies that have been carried out to date have utilized national level data and have been forced to work with a small sample size, typically including only two instances of jobless recoveries (or three if you count the ongoing cycle). The state-level data we have gathered here, in contrast, provides us with 50 times the instances of jobless recoveries. This gives us a unique opportunity to exploit the variation found across states and help create tests for the alternative theories. We believe we have convincingly shown that jobless recoveries are a somewhat localized event with strong variation across states and time. We encourage future research to exploit this variation in the magnitude of jobless recoveries at the state level, and to extend the use of the *JRD* measure to other cross-sectional analyses as well. Some examples might be a study of jobless recoveries at the MSA or industry levels.

Previous research has presented only weak empirical results, in large part due to the lack of a comprehensive measure which allows positive statements about jobless recoveries to be made. Without such a measure, any efforts to quantify the effects of independent variables on the severity of jobless recoveries are fruitless. We believe the Jobless Recovery

Depth (JRD) measure can serve that purpose and can help settle many of the debates that surround jobless recovery questions in ways which were not possible before. Our claim is that such types of comprehensive measures are necessary for the study of multidimensional events like the so called jobless recoveries and that the JRD is one simple, intuitive alternative. We have displayed how this measure might be used to conduct empirical analysis that will help narrow down the pool of competing explanations in the literature, moving us closer to an understanding of the factors truly correlated with jobless recoveries.

A Appendix: Additional Results

We would like to provide a deeper understanding of how relocation correlates with jobless recoveries, and about the potentially different effects of job-change-related relocation, career-change-related relocation, and geographic-move-related relocation. For that purpose, we collected data on related variables. These variables are less than ideal and their use reduces our sample size significantly. We explain each of them in turn.

We collected data on the states' *home-ownership* rate; which has been shown to have a statistical link to both, decreased labor mobility and increased joblessness (see Blanchflower and Oswald 2013). Home-ownership is defined as the percentage of housing units occupied by owner at the beginning of a cycle. The data was taken from the United States Census Bureau It is available every year after 1983 and once every 10 years going back to 1900. However, data available every ten years cannot be used for our analysis. So, we construct a cycle-specific measure, using the average home-ownership rates over the cycle for the four most recent business cycles only.

We also constructed a *job-relocation* measure at the state level, as the sum of the total job destruction and total job creation observed in a state during a particular cycle, averaged over the duration of the cycle. State-level, quarterly job destruction and job creation statistics are reported by the BLS in their Business Employment Dynamics database. The BLS does not specify the particular industries in which jobs are being gained or lost, but an overall measure of all turnover across all industries. Since the data reported starts with quarter 3 of 1992, our job relocation measure for the 1990-2001 business cycle was built without data on the first 7 quarters. Data on all previous cycles is not available

Finally, a *geographical-relocation* variable was derived from the Statistics of Income Migration Data reported by the Internal Revenue Service (IRS). The IRS tracks the number of exemptions claimed on tax returns filed each year where the state of residence has changed for that filer relative to the previous year. Inflows are the total number of exemptions entering the state from another state; outflows are the total number of exemptions leaving the state for another. We construct our geographical relocation measure by adding the inflows and outflows reported by the states in any particular year and taking the average of these annual numbers over the duration of the cycle. The data was obtained from the Tax Foundation website (taxfoundation.org). Admittedly, the tax exemptions data allows us to gauge relocation across states, but ignores any relocation activity generated within states.

The data we obtained covers from 1993 to 2010 only and fails to capture migration occurring in the first 2 years of the 1990-2001 business cycle and the most recent two years of the present cycle. As before, when constructing the measure for the cycles, we applied the average taken over the available data to the entire cycle. Omitting the first two years of the 1990-2001 cycle likely biases our relocation measure down, because interstate migration tends to be higher during the recession (which occurs at the start of the cycle). While omitting the last two years of the current cycle likely biases our measure up. On average, interstate migration has been declining steadily over the sample period and jobless recoveries have been shown to get more severe at the same time. Thus, we would expect the incomplete data to bias any estimated correlations between the *JRD* and the geographical relocation measures towards zero.

Table 8 shows the results of the corresponding OLS, fixed-effects and random-effects regressions that include the homeownership and worker relocation measures in the set of explanatory variables. The inclusion of the additional variables reduces the sample size from 400 to 200 observations when the homeownership variable is included, and from 400 to 150 observations when the relocation variables are included. For that reason, we present the two sets of regressions separately. As shown in the table, when these new variables are introduced, the results obtained for the other variables are similar to those discussed in the main body of the paper. The estimated coefficients for the homeownership and relocation variables, however, are always insignificant. Thus, the evidence from our reduced-sample analysis suggests that relocation and household mobility do not significantly explain variations in the magnitude of jobless recoveries.

Table 8: Reduced Sample Regressions.

	1	2	3	4	5	6
	OLS	FE	RE	OLS	FE	RE
Union Membership	-0.003	0.02	-0.003	-0.003	-0.011	-0.003
	(-1.21)	(0.53)	(-1.2)	(-0.59)	(-0.55)	(-0.63)
Forced Unionism	0.079**	0.20***	.079**	0.075	0.078	0.075*
	(2.55)	(7.20)	(2.43)	(1.66)	(1.27)	(1.90)
Volatility	-0.976***	-0.455	-0.96***	-1.2***	-1.25***	-1.2***
	(-4.04)	(-1.37)	(-3.34)	(-3.14)	(-3.23)	(-3.47)
Income Inequality	-0.171	-0.07	-0.165	-1.06*	-0.376	-1.06*
	(-0.33)	(-0.09)	(-0.3)	(-1.71)	(-0.33)	(-1.92)
Expansion	0.009***	0.007***	0.009***	0.004*	0.002	0.004*
	(4.44)	(3.01)	(4.02)	(1.93)	(0.86)	(1.95)
Health-care Costs	0.025**	0.033*	0.024**	0.004	-0.003	0.004
	(2.19)	(1.86)	(2.25)	(0.26)	(-0.09)	(0.27)
Home Ownership	-0.002	-0.002	-0.002	-0.004	-0.009	-0.004
	(-0.86)	(-0.37)	(-0.97)	(-0.88)	(-0.97)	(-0.96)
Geo. Relocation				0.0005	-0.038	0.005
				(0.21)	(-1.74)	(0.27)
Job Relocation				0.0006	0.017**	0.0006
				(0.38)	(2.09)	(0.54)
Constant	0.13	-0.16	0.131	1.02	1.53	1.02
	(0.33)	(-0.28)	(0.31)	(1.58)	(1.40)	(1.88)*
R-sqr	0.275	0.14	0.27	0.25	0.01	0.25
N	200	200	200	150	150	150

T-statistics in parenthesis, ***significant at 1%, **significant at 5%, *significant at 10%.

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Chapter 3: A State-Level Investigation into the Causes of the Great Moderation

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Abstract

In recent years, a good deal of attention has been given to the broad decline in output volatility in the United States, commonly referred to as the Great Moderation. Several competing theories have been suggested, including changes in policy, decreased volatility of the durable goods sector, and a change in the nature of aggregate shocks. Nearly all studies of the Great Moderation in the United States have focused on national output data. This paper examines the changes in output volatility at the state level using a quarterly proxy of state-level GDP. Our findings support research linking the Great Moderation to reductions in volatility in durable goods, but do not support theories suggesting changes in interstate banking laws are responsible.

1 Introduction

The broad decline in output volatility beginning in the mid 1980's is a subject that has received considerable attention in the literature over the last 15 years. While it is nearly universally acknowledged that a Great Moderation in output occurred during this time, there remains a good deal of debate as to why. Most explanations fit into one of three categories: good policies, good practices, or good luck. While proponents of the improved policy argument focus mostly on monetary policy and the recent reduction in inflation volatility that accompanies the reduced fluctuations in output, those who argue for good practices point out that the reduction in volatility is not universal, but rather concentrated in specific sectors, such as durable goods. However, the literature surrounding each argument, by and large, fails to utilize the rich resource of state-level data to test their claims. We attempt to shed further light on some of the factors which may have contributed to the Great Moderation by examining all 50 state economies from the period of 1960-2012.

It is important to understand the characteristics and timing of a structural break in output volatility for a number of reasons. One such example on the empirical front is that linear models of output growth that cover the break period will be misspecified. Another popular method for testing theories against the data is to compare the moments of the data of calibrated models with the moments of real-world data. However, the presence of a structural break over the sample period will affect how second and higher moments of output growth need to be calculated. Also, the signal-to-noise ratio of Markov-switching models of business-cycle fluctuations will be decreased if a constant variance is used as shown by McConnell and Perez-Quiros (2000). There are obvious policy implications as well, since changes in output growth that were once considered to be relatively small may now be thought of as large, which may affect optimal policy decisions in response to these fluctuations. Also, recent research has linked declines in output volatility to the problem of jobless recoveries experienced in recent U.S. business cycles (Mendez and Reber, 2014).

In this paper, we closely examine the findings linking the Great Moderation to a decline in volatility stemming from the durable goods sector (McConnell and Perez-Quiros, 2000; Blanchard and Simon, 2001; Kahn, et al., 2002; Owyang, et al., 2008) and to the advent of interstate banking in the U.S. (Morgan, et al., 2004; Owyang et al., 2008). Studies of the relationship between durable goods volatility and total output volatility have mostly focused on the issue at the national level. However, the nation provides only one series of date with which to test this theory. The states, on the other hand, provide fifty times the data, allowing for a much richer empirical analysis. One other research paper, by Owyang, et al. (2008), attempts an empirical study of the Great Moderation at the state level, however, their specification does not use an output measure, but rather utilizes state-level employment data. We discuss the issues with using employment data in place of output, especially over the period of the Great Moderation, and show that a suitable alternative to proxy for state output does exist.

We closely follow the empirical methodology put forth by McConnell and Perez-Quiros in their seminal paper from 2000 which documents the structural break in U.S. output volatility in the first quarter of 1984. We document similar structural breaks for many of the individual states, with a great deal of heterogeneity in the timing of the estimated break dates. Moreover, we are able to replicate their analysis showing that the durable good sector is a driving force behind the Great Moderation with the state-level data. Our findings strongly suggest that breaks in volatility in the growth contribution of durable goods and the growth rate of durable goods are one of the main factors contributing to the decline in output volatility on the whole.

Having documented the structural break in output volatility at the state level, we then apply our findings to the research spearheaded by Morgan, et al. (2004) linking the Great Moderation to the proliferation of interstate banking laws. Comparing our results to the dates when states first allowed interstate banking, we are able to show how output volatility in each state may have been effected by this deregulation. However, the data suggests that the advent of interstate banking does a poor job of explaining the break in output volatility at the state level, and fails to match up with the timing of the national break. We do not examine how other policy changes, such as monetary policy changes, may have influenced output volatility at the state level in this paper, but urge future research to do so.

The rest of the paper is organized as follows: Section 2 discusses the proposed causes of the Great Moderation as found in the literature, Section 3 discusses the data and shows that there is a great deal of variation at the state-level worth exploiting, Section 4 conducts our empirical analysis and the results are discussed, and Section 5 concludes.

2 Literature on The Great Moderation

One of the first papers to document the decline in output volatility in the national data, McConnell and Perez-Quiros (2000), put forth the idea that the rapid decrease in GDP could be driven by volatility reduction in just one, or a few, of the components of GDP. They investigated this idea and found evidence suggesting that the Great Moderation is largely attributable to reductions in volatility of durable goods. Future research continued to test their theory, with others finding evidence that the durable goods sector was a major contributing factor to the Great Moderation (Blanchard and Simon, 2001; Kahn et al., 2002; Summers, 2005; Owyang et al., 2008). McConnell and Perez-Quiros also find that other sectors did not experience a similar decline in volatility, strongly suggesting that changes in the nature of fluctuations in durable goods are the driving factor in understanding where the decline in output has come from. Others, such as Blanchard and Simon (2001), also found evidence that inventory investments have seen a dramatic decline in volatility.

Another proposed explanation for the change in output volatility is the increase in bank integration across states (Morgan, et al., 2004; Owyang, et al., 2008) First proposed by Morgan, et al. (2004), this theory explains how a movement away from small, state-based banking systems to an integrated nation-wide banking system may have had an affect upon output fluctuations. For instance, as businesses face difficult economic times in one state, they may require increased bank capital. However, the banks within that state are likely to feel the stress of the poor economic conditions as well, making them ill-equipped to assist in-state businesses. If regulation stipulates that no out-of-state banking options are available, this could lead to further economic decline as well as possible bank failures. However, opening up state boarders to banking services may help alleviate these sorts of regional issues and stabilize the changes in output.

Yet another of the potential causes of the Great Moderation is an improvement in monetary policy (Boivin and Giannoni, 2006; Summers, 2005; Cogley and Sargent, 2005; Stock and Watson, 2002) The argument is that there has been a sort of regime shift in U.S. monetary policy that is responsible for the reduction in output volatility (Sims and Zha, 2006). As the Fed has changed the way it responds to inflation and the output gap, with less willingness to try to fine tune output and more focus on price stability, this has reduced fluctuations in aggregate output growth. One argument against the claim that good monetary policy is responsible for the Great Moderation is the fact that the components of output have been differentially affected by this change in volatility. More specifically, an argument has to be made as to why the durable goods sector seems to be at the heart of the Great Moderation when policy has no theoretical differential effect across industries.

Finally, it has been suggested that "good luck" or a fortunate change in the nature of aggregate shocks has led to the widespread reduction in volatility (Stock and Watson, 2002; Ahmed, et al., 2004; Smets and Wouters, 2007; Justiano and Primiceri, 2006). Two of the most commonly identified shocks which are potentially behind the Great Moderation are energy shocks and productivity shocks. However, Giannone, et al. (2008) review the literature on the "good luck" hypothesis and conclude that the models which pointed to shocks as the driving force behind the Great Moderation were excessively naive, and that omitted variable problems caused the variance of the shocks to be overestimated.

3 The Data

Studies of the Great Moderation have typically considered the problem at the national level. Usually, the measure of national level output used for analysis is real GDP. However, state-level GDP data coming from the BEA Regional Economic Accounts is only available annually from 1963-2012. This is the data used by Morgan, et al. (2004) in their small sample size, state-level study of the Great Moderation and bank deregulation. However, annual data does not allow one to observe the fluctuations in variables throughout the business cycle. It is desirable to have a measure of output that is at least available at a quarterly frequency. Therefore, we set out to find a proxy for GDP at the state level that is available at the desired frequency.

In the only other state-level empirical study of the Great Moderation of which we are aware, Owyang, et al. (2008) also abstain from using annual state GDP data due to it's limitations in capturing output volatility over the business cycle. Their solution is to use state level employment data coming from the BLS. Using employment data in place of output, they estimate structural break dates for each state from which they draw conclusions regarding the proposed causes of the Great Moderation. However, the use of employment data in place of output is clearly problematic. Perhaps the most important reason is that the behavior of state-level employment and output have been diverging in recent business cycles as documented in Mendez and Reber (2014). The continued decline in employment after output recovery begins, known as a jobless recovery, has been well documented at the national level and has been shown to correspond to the timing of the Great Moderation. Mendez and Reber also show that this is true for many of the individual states, suggesting that employment makes for a poor measure of output volatility for the period associated with the Great Moderation.

We show that a suitable state-level proxy for output does exist. Personal income data by state is reported on a quarterly basis by the BEA. One of these components, called *earnings by place of work*, was chosen as our proxy of state output. According to the BEA, "Earnings by place of work is the sum of Wage and Salary Disbursements, supplements to wages and salaries and proprietor's income. BEA presents earnings by place of work because it can be used in the analysis of regional economies as a proxy for the income that is generated from participation in current production." Thus, we feel that Earnings by place of work may be a reasonable proxy for state output.

The data seem to support the claim of the BEA, and justify out use of *earnings* as a proxy for state-level output. The average correlation coefficient between annual state GDP levels and annual state *earnings by place of work* is 0.9977. Thus, at the state level, the correlation between GDP and our proxy seems very strong when using the annual data. Of course, we cannot evaluate whether this is also true when using quarterly data (quarterly, state-level GDP measures do not exist); but we still made an effort to document the quarterly correlation at the national level. National data for both GDP and *earnings by place of work* are available at a quarterly frequency and have a correlation of 0.7272. Both the annual state-level correlations and the quarterly national-level correlations suggest that *earnings* is indeed a reasonable proxy for GDP.

In addition, given that for the purpose of this paper we will estimate equations using the growth rates of output in percentage terms, we also look at how annual changes in earnings at the state level correlate with annual changes in state-level GDP. We conducted standard OLS regressions between the state-level, annual growth rates in GDP and the corresponding state-level annual growth rates in *earnings*. In these regressions, *earnings* are significant at the 1% level for all 50 states and explain about 75.6% of the observed variation in GDP, on average (the average R-squared for the 50 regressions was 0.756). This further supports our use of *earnings* as a proxy of state-level GDP.

Additional adjustments must be made to the *earnings by place of work* to make the series more comparable to the measure of output used at the national level (GDP), and to allow for meaningful comparison across time and states. The *earnings* data is nominal and not seasonally adjusted. We first seasonally adjust the *earnings* data for each state using the X12 ARIMA process provided by the U.S. Census Bureau. The nominal, seasonally adjusted series is then converted into real earnings using the GDP deflator. This provides a real, seasonally adjusted *earnings* measure for each state which can be used as a proxy for GDP. Importantly, this measure is available over the desired period, 1960:Q1-2012:Q4[1],

[1]2013 state-level *earnings* data was not available at the time this paper was written

and at the desired quarterly frequency. Other potential proxies for state GDP failed to meet either the range of frequency requirements.

Data on durable goods production at the state level is taken from the BEA Regional Accounts as well. State-level GDP by industry is only available at an annual frequency. As mentioned before, annual data is a poor choice for modeling volatility in output and for testing for the presence of structural breaks. However, *earnings* data from the durable goods sector is reported in the state personal income accounts at a quarterly frequency. We use this data from 1960:Q1-2012:Q4. It should be noted that a continuous series is only available for 46 of the 50 states, as 4 of the states have significant periods of missing values[2]. The durable goods earnings data is seasonally adjusted using the same X12 ARIMA seasonal adjustment process, and is then converted to real terms using the GDP deflator. This real, seasonally-adjusted series for durable goods is used to construct the growth rate of durable goods variable, and the growth contribution of durable goods variable used later in the paper.

With this state-level data in hand, we first wish to visually inspect the data to identify an periods of seemingly dramatic change in volatility. The existence of the Great Moderation in the data is often displayed by plotting the growth rate of real GDP over time. The national data is plotted in Figure 1. In like manor, we plot the growth rate of real *earnings* (our GDP proxy), for all 50 states in Figure 2. Inspection of the data shows what appears to be a noticeable drop in the volatility of output growth for many of the individual states which mirrors the change in the national-level data.

[2]The 4 states which are omitted from our durable goods calculations due to missing values are Alaska, Hawaii, Rhode Island, and Wyoming.

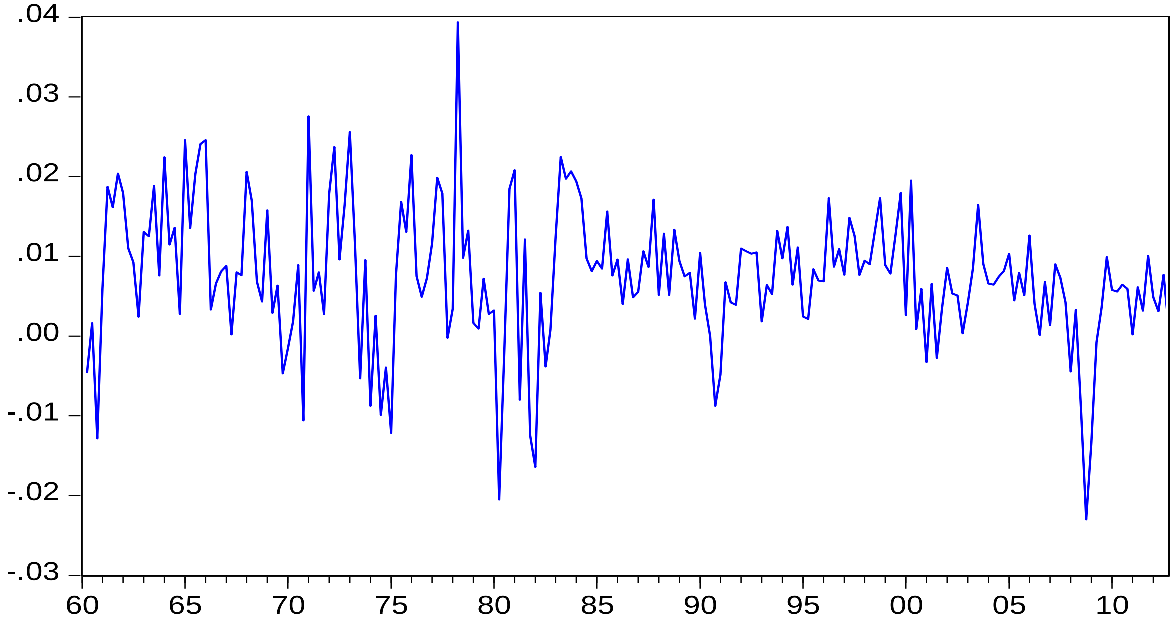

Figure 1: National growth rate of output using quarterly real GDP.
Source: Bureau of Economic Analysis; Author's calculations

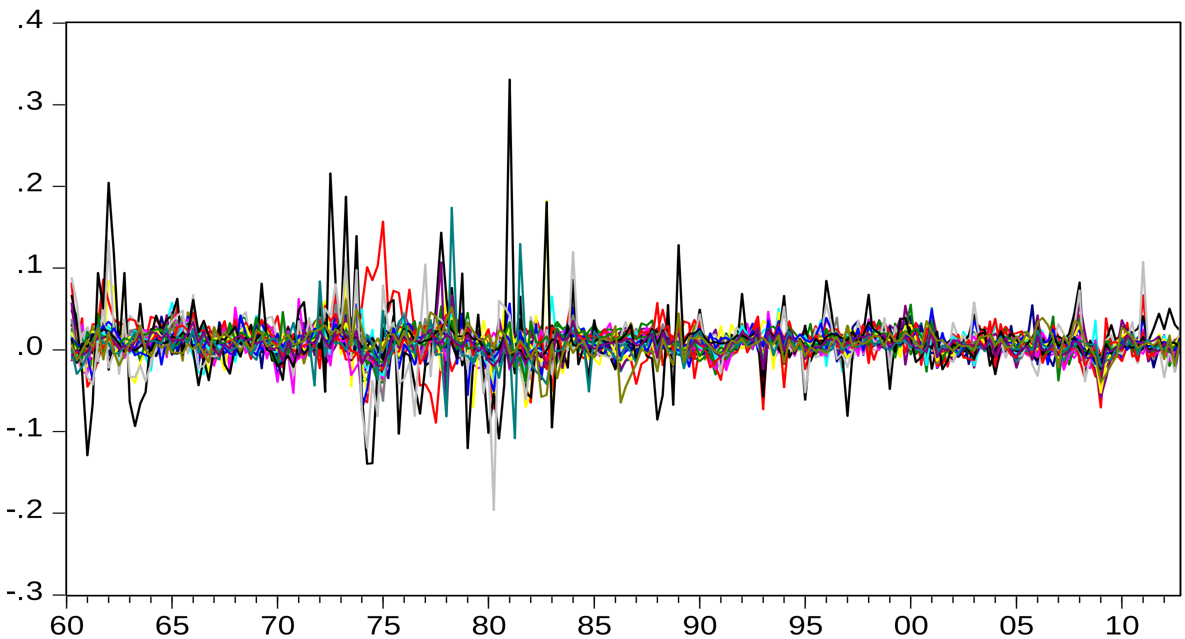

Figure 2: State-level growth rate of output using real quarterly *earnings by place of work*.
Source: Bureau of Economic Analysis; Author's calculations

However, it is very difficult to see exactly how many states are experiencing a Great Moderation by plotting all 50 series at once. Instead, consider the standard deviation of output for the two periods before and after the start date of the Great Moderation most commonly referenced in the literature, 1984:Q1. By taking the difference in the standard deviation of output from the pre-1984 period to the post-1984 period, we are able to see how the change in volatility for each state compares to the nation as a whole. This is shown in Figure 3. Interestingly, when we impose the national start date for the Great Moderation we see a great deal of variation across states. The decline in output volatility is greater than the national decline for some states, while other states actually see an increase in volatility after 1984. We see that 18 states experienced a "moderation" in output volatility greater in magnitude than that of the U.S., 24 states experienced a "moderation" smaller in magnitude than the U.S., and 9 states actually experienced increased in volatility around the date of the national moderation. Clearly, imposing the national start date for each state is not appropriate, and we will test for the break date in each state individually. However, the point remains that the states vary greatly one from another and provide a great opportunity for testing the separate theories on the causes of the Great Moderation.

The final piece of information our study requires is the date of banking deregulation for each state. Data on the year that each state began allowing interstate banking is taken from Morgan, et al. (2004). They report the year in which out-of-state bank entry was first allowed for each state. These are the years presented later in Table **??** of the paper, and are used for making comparisons between interstate banking dates and estimated structural break dates.

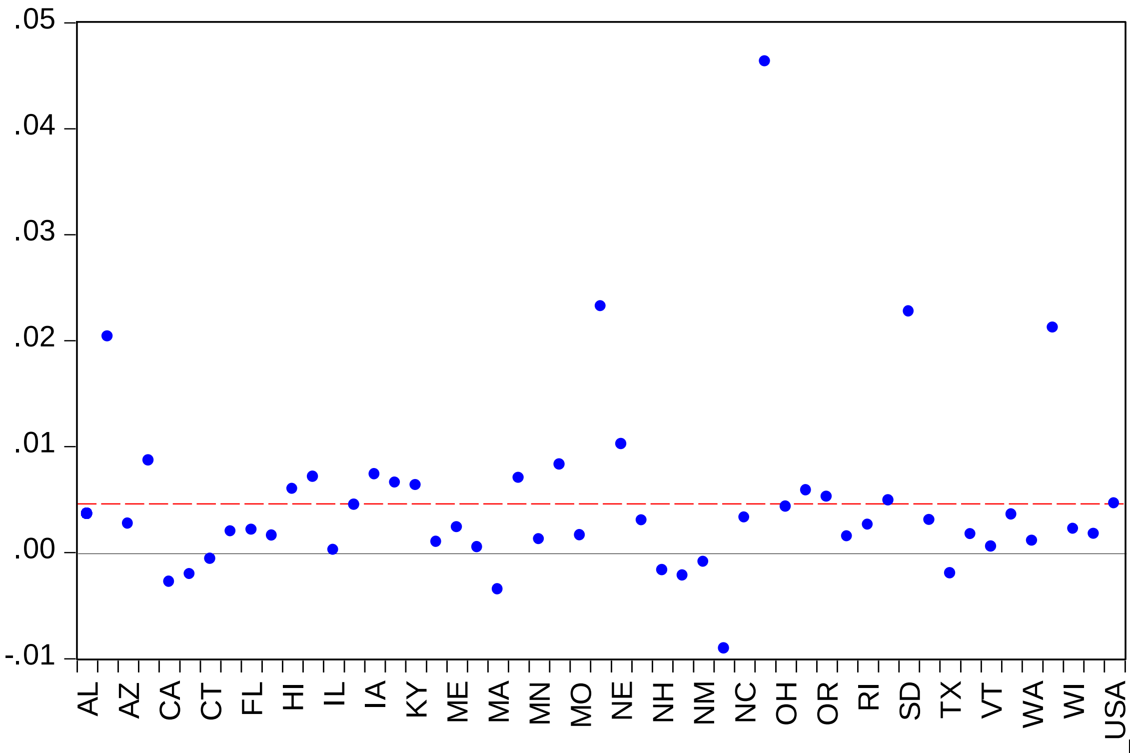

Figure 3: Difference in standard deviation of output growth from the pre-1984 and post-1984 periods. The dashed line represents the value for the United States as a whole. 9 states have negative values, suggesting an increase in output volatility between the two periods. 24 had a "moderation" smaller in magnitude than the nation, and 18 had a "moderation" greater in magnitude than the nation as a whole.

4 Emperical Analysis and Results

4.1 Estimating Structural Breaks in Output

In order to utilize the state-level output data to help explain the Great Moderation, we follow closely the methodologies used in the past for analyzing the national data. However, finding evidence to support a theory with one series of data (national output) could be an aberration, whereas a finding coming from fifty series of state-level output data would be much more robust. Perhaps the biggest innovation of this paper is the extension of existing tests to new, state-level data. We first set out to estimate the timing of structural breaks in output volatilty for each state using our state-level GDP proxy, *earnings*. Then, we examine well the finding that the decline in volatility emanates from the durable goods sector holds up under an analysis of our larger data set. Next, the claim that the advent of interstate banking caused the Great Moderation is put to the test.

Following the seminal work of McConnell and Perez-Quiros (2000), we employ the Qaundt-Andrews Breakpoint Test to determine whether there is a structural break in output volatility over our sample period for equation (1) for each state. Output growth is modeled as an AR(1) process[3]. The Quandt-Andrews test (Andrews, 1993; Andrews and Ploberger, 1994) performs a series of individual Chow breakpoint tests (Chow, 1960) for every observation between the beginning and end dates in the sample, τ_1 and τ_2. The test statistics from the individual Chow tests, k in number, are then summarized into a single test statistic which is used to test against the null hypothesis of no structural break over the sample (between τ_1 and τ_2).

$$\Delta y_t = \mu + \phi \Delta y_{t-1} + \varepsilon_t \tag{1}$$

There are two statistics saved from each of the individual Chow breakpoint tests: the Wald F-statistic, and the Likelihood Ratio F-statistic. However, for linear equations these two F-statistics are identical. All of the resulting F-statistics from the Chow tests can then be summarized into three test statistics comprising the Quandt-Andrews test statistics: the Maximum or Sup statistic which takes the maximum of the individual Chow F-statistics as found in equation (2), the Exp test statistic as in equation (3), and the Ave statistic which is an average of the individual Chow F-statistics, as found in equation (4). The Maximum test statistic is typically used to indicate the most likely breakpoint over the sample.

[3]The lag lengths selection for each state was done using the partial autocorrelation functions and confirmed using the SIC and AIC lag length criteria. For 46 of the 50 states, a 1 period lag cannot be rejected at the 5% level. Two states (Alaska and Indiana) are AR(2), and two states (North Carolina and Washington) are AR(3). Equation 1 is adjusted appropriately for these longer lags when performing analysis on these four states.

$$MaxF = \max_{\tau_1 \leq \tau \leq \tau_2} (F(\tau)) \tag{2}$$

$$ExpF = ln \left(\frac{1}{k} \sum_{\tau = \tau_1}^{\tau_2} exp \left(\frac{1}{2} F(\tau) \right) \right) \tag{3}$$

$$AveF = \frac{1}{k} \sum_{\tau = \tau_1}^{\tau_2} F(\tau) \tag{4}$$

These three test statistics have non-standard distributions. Approximate asymptotic p-values were provided by Hansen (1997) and are used in our analysis. However, these distributions degenerate as one as the endpoints, τ_1 or τ_2, are approached. Thus, it is standard practice to eliminate several of the observations near the beginning and end of the sample. This is commonly referred to as "trimming" and a "trimming" of 15% is the norm, where 7.5% of the observations at the start of the sample and 7.5% of the observations at the end of the sample are excluded. This is also consistent with the "trimming" percentages used my McConnell and Perez-Quiros (2000).

The results of our breakpoint tests are found in Table 1[4]. Here, we see what one might have expected after inspecting Figure 2. While the data supports the findings at the national level that there has been a break in output growth, we see that the dates of this break vary widely across different places in the U.S. Furthermore, there are several states with break points after the year 2000. These are more likely structural breaks in which output volatility is increasing as we approach the more volatile period towards the end of the sample. In order to gain a better understanding of how state-level volatility declines compare to the nation as whole, we reduce our sample size to 1960:Q1-2004:Q4. The resulting break point estimates are shown in Table 2. The end point of 2004 is selected for two reasons. The first is to

[4]For Table 1, the test statistic used for statistical significance of the break point is the Maximum test statistic from equation (2). All three Quandt-Andrews test statistics and their accompanying p-values are reported in a larger table in the Appendix.

eliminate many of the more volatile recent years from the sample. Remember that 7.5% of the observations on each end of the sample will be trimmed when performing the Quandt-Andrews test, so this will eliminate nearly all of the new millennium, thus removing the time period over which significant increases associated with the end of the Great Moderation are likely to be seen. Also, this reduced sample matches up with the timing of Morgan, et al.'s research from 2004 on interstate banking. Thus, the reduced sample may provide a nice test of how well their theory held up at the time their paper was written.

From Tables 1 and 2 we see that many states experienced statistically significant structural breaks in output growth over the respective sample periods. Many states experienced breaks prior to the national date of 1984:Q1, and many experienced breaks after. The range over which state-level breaks are observed is quite staggering. This makes our next finding regarding changes in the volatility of durable goods all the more surprising.

4.2 Durable Goods

Now, we wish to examine whether or not a decline in output volatility in the durable goods sector is driving the state-level Great Moderations we are observing. This has been one of the most popular explanations found in the literature (McConnell and Perez-Quiros, 2000; Blanchard and Simon, 2001; Kahn et al., 2002; Summers, 2005; Owyang et al., 2008). Following McConnell and Perez-Quiros (2000), we estimate equation (1) again, with two separate durable goods measures in place of y: the growth contribution of durables and the growth rate of durables. The growth contribution is measured as durable goods' share of nominal output (*earnings*) in period *t-1* times the real growth rate of durable goods in period *t*. McConnell and Perez-Quiros argue that a break in the growth contribution of durable goods signals a potentially causal role for declining aggregate volatility. A break in the growth rate of the durable goods sector indicates that the break in the growth contribution is coming from reduced volatility *within* the durables sector, not just a change in durables share of output.

Table 1: **Structural Breaks for Each State by Year, 1960:Q1-2012:Q4.** The results of the Maximum test statistic of the Quandt-Andrews break point test using p-values from Hansen (1997).

Year	1	2	3	4	5
1968	AR***				
1970	HI***				
1972	ME**	MS***			
1973	FL***	VA**			
1974	ID*	NC	SC*		
1975	AK***	KS*	SD		
1978	NE	WV***			
1979	KY**				
1980	IA*	MN*			
1981	ND**	TX	WY		
1982	MT***	OK***			
1983	WI *				
1985	OH**				
1986	MI*				
1987	TN**				
1988	AL***	IL**	IN	LA***	NY***
1989	PA***				
1990	MD***				
1991	NJ***				
1992	MA***				
2000	CA***	GA**	OR**	VT***	WA***
2001	AZ*	CO***	CT**	UT**	
2004	MO***	NH***	RI*		
2005	DE***	NV**	NM**		

***significant at 1%, **significant at 5%, *significant at 10%.

Table 2: **Structural Breaks for Each State by Year, 1960:Q1-2004:Q4.** The results of the Maximum test statistic of the Quandt-Andrews break point test using p-values from Hansen (1997).

Year	1	2	3	4	5	6	7
1966	AR***						
1969	VA*						
1970	HI***						
1972	ME***	MS**					
1973	DE	FL***					
1975	AK*	KS*	SC	SD			
1977	KY						
1978	NE	NM***	OK***	TN**			
1980	IA						
1981	ND**	WV**					
1982	MN**	MT**					
1986	ID**	IN**	MI**				
1988	AL***	NY***					
1989	OH***	PA***					
1990	MD***						
1991	MO*	NJ***					
1992	AZ**	CO***	CT*	IL***	LA***	MA***	NV***
1993	CA**	GA***	RI**	WA*			
2000	NH***	NC*	OR**	TX	VT***	WI**	
2001	UT***						

***significant at 1%, **significant at 5%, *significant at 10%.

The same Quandt-Andrews tests are repeated for each state again, using the growth rate and growth contribution of durable goods to estimate equation (1). The results are displayed in Table 3. Once again, the break dates are those coming from the Maximum test statistic in equation (2), and statistical significance using Hansen (1997) p-values is reported. Several of the states have significant breaks in the growth contribution of durables very close to the timing of the national break date of 1984:Q1. We see that 28 of the 46 states (or about 61%) have statistically significant break points for both growth contribution and growth rate of durable goods that are within 2 quarters of one another. Thus, we have evidence suggesting that variation *within* the durable goods sector is actually experiencing a dramatic change in volatility over our sample period, with nearly simultaneous breaks in the growth contribution driving aggregate output volatility declines.

Perhaps even more compelling than Table 3, are the histograms of the break dates in durable goods' growth contribution and growth rate found in Figure 4. The distribution appears to be roughly normal with a large peak in 1984. Also, we have very similar shapes in the distribution of breaks for both variables, suggesting that volatility within the durable goods sector, not just in its share of output, may be driving the observed declines in state-level volatility. This finding is consistent with the findings of McConnell and Perez-Quiros, and derives greater strength from the use of a much larger data set.

State	Break in Growth Contribution	Break in Growth Rate	Matching Breaks
AL	1988Q2*	1988Q2	
AZ	1987Q2***	1987Q2***	✓
AR	1978Q2***	1978Q2***	✓
CA	2000Q1***	2000Q1***	✓
CO	1981Q3***	1981Q3**	✓
CT	1976Q***	1976Q3***	✓
DE	1994Q1	2000Q1	
FL	1988Q2***	1982Q2***	✓
GA	1992Q4***	1992Q4***	✓
ID	1992Q2***	1992Q2**	✓
IL	1968Q2	1985Q3**	
IN	1973Q1	1991Q4	
IA	1979Q3*	1976Q3*	
KS	1984Q1***	1984Q1***	✓
KY	1984Q2*	1984Q2*	✓
LA	1988Q2***	1988Q2***	✓
ME	1980Q1***	1980Q1***	✓
MD	1969Q4	2001Q2***	
MA	1984Q4***	1984Q4**	✓
MI	1972Q4**	1973Q1	
MN	1984Q4***	1984Q4***	✓
MS	1978Q2**	1977Q4***	✓
MO	1968Q4*	1994Q3	
MT	1984Q2***	1984Q2**	✓
NE	1984Q1***	1984Q1***	✓
NV	1984Q1***	1984Q1***	✓
NH	1987Q4***	2000Q1***	

NJ	1980Q1**	1980Q1**	✓
NM	2000Q3**	2000Q3**	✓
NY	1968Q4	1993Q2**	
NC	1975Q1**	2000Q3***	
ND	1994Q4**	1975Q1	
OH	1980Q3**	1989Q1**	
OK	1981Q2**	1981Q2***	✓
OR	1979Q3	1979Q3	
PA	1989Q1*	1989Q1***	✓
SC	1989Q1***	1988Q4***	✓
SD	1986Q3***	1990Q1	
TN	1984Q1***	1984Q1***	✓
TX	1988Q2***	1988Q2***	✓
UT	1987Q4	1987Q4	
VT	1978Q2***	2001Q1***	
VA	1988Q4***	1988Q4***	✓
WA	1986Q4***	1983Q3***	
WV	1984Q3***	1984Q3**	✓
WI	1984Q1*	1984Q1**	✓

***significant at 1%, **significant at 5%, *significant at 10%.

Table 3: Estimated break dates in the growth contribution and growth rates of durable goods, 1960:Q1-2012:Q4

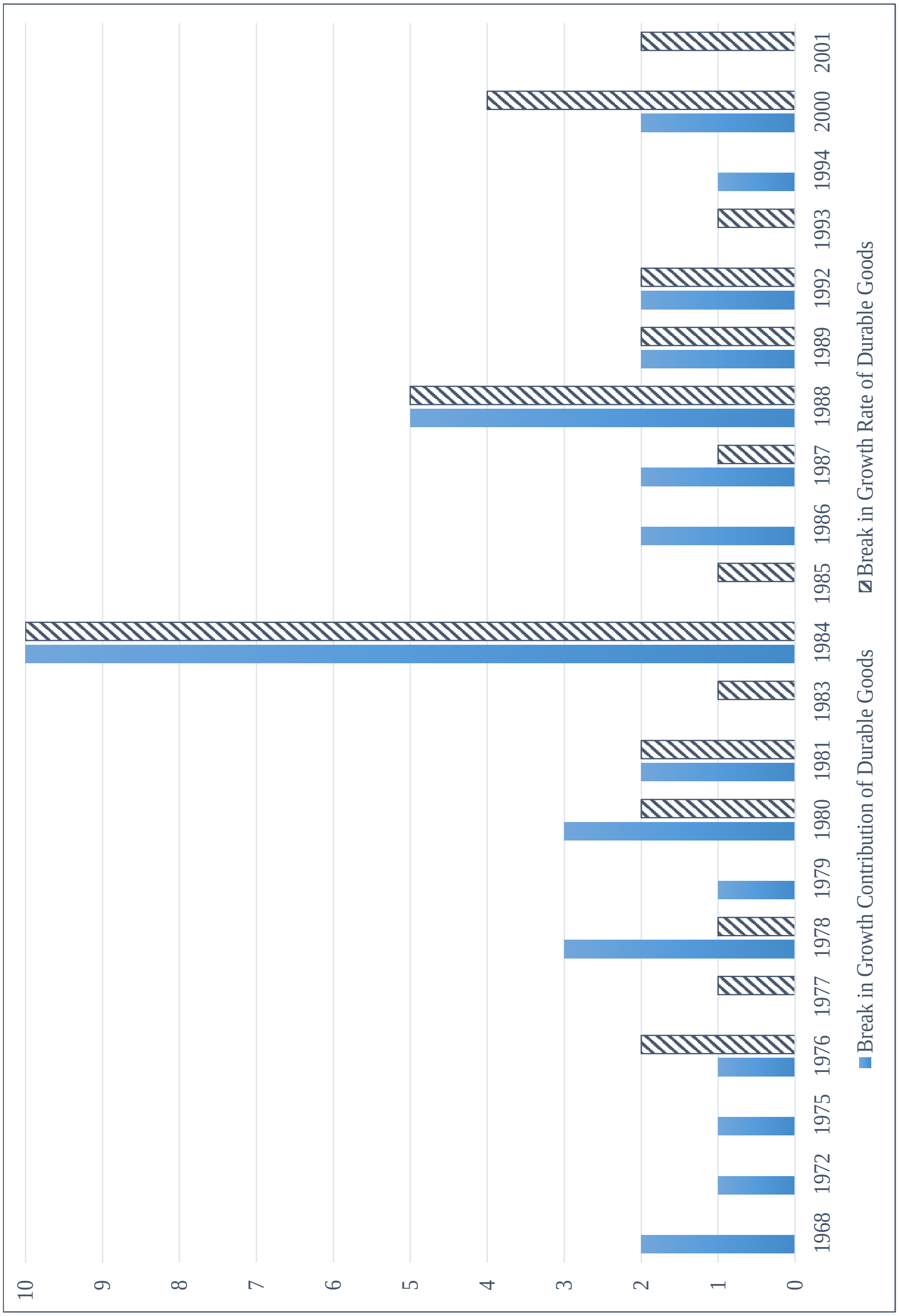

Figure 4: Distribution of estimated break dates in the growth contribution and growth rates of durable goods, 1960:Q1-2012:Q4

4.3 Interstate Banking

In their study of bank integration and output volatility in state business cycles, Morgan, et al. (2004) find that increases in bank integration across states is correlated with a significant decline in output volatility. Thus, from their findings one would expect a measure of bank integration to be significantly and positively correlated with fluctuations in output[5]. Also, one would expect the proposed "cause" to occur prior to the change in volatility. One can compare the dates of the structural breaks in output to the dates in which interstate banking was allowed to see if this hypothesis holds.

Taking the years that interstate banking was first allowed in each state from Morgan, et al. and our estimated structural break dates from Table 1, we are able to see whether the deregulation allowing interstate banking came before or after the break in output growth. If it came before, then this is consistent with the story told by Morgan, et al. However, if the change in banking law came after the structural change, then this does not support their claim. These dates are presented in Table 4, and a histogram showig the distribution of state break dates and deregulation dates in found in Figure 6. However, simply checking that interstate banking came before the break is not enough to claim support for Morgan, et al.'s theory. The structural break dates for several states occur in the year 2000 or later. These breaks do not nearly match up with the national break date of 1984, and as mentioned before, these break dates may be the result of volatility increases toward the end of our sample. In each of these cases, the break in output is occurring many years after the start of interstate banking, often a decade or more later. This is hardly evidence that the switch to interstate banking is the cause of these breaks. Thus, we argue that states with break dates in the 2000's also fail to support Morgan, et al. and are removed from the tally of states which support their hypothesis in Column 5. Also, given that it may be too easy to find a structural break using Chow tests, especially at the 10% level, we consider only those

[5]These degree of bank integration measures used by Morgan, et al. (2004) were the "interstate asset ratio" and the "other state asset ratio", however, these were only available annually from 1976-1994. The sample is quite small, and due to the problems of using annual data already discussed, we elected not to use this data.

states which had breaks at the 5% level or higher. States which fail to support the Morgan hypothesis at the 5% level are removed from the final tally reported in Column 6.

Considering only those states where the deregulation and structural break occur in the appropriate order, the break occurs before 2000, and the estimated break is significant at the 5% level as supporting Morgan's hypothesis, we are left with only 10 of the 50 states that meet these criteria. That is, using our full sample, 1960:Q1-2012:Q4, only 20% of states support the interstate banking theory of the Great Moderation. From an inspection of the histograms in Figure 6, we see quite clearly that many states experienced a break in output prior to the deregulation of interstate banking laws, and many states experienced a break long afterwards, with only a few state break dates corresponding to the period of deregulation. Also, the majority of states starting allowing interstate banking after the national date of the Great Moderation.

As discussed previously, some of these later break points, after 2000, may be the result of volatility increases near the end of our sample period. Once again, we reduce the sample to consider only 1960:Q1-2004:Q4, which dates help to eliminate the breaks arising from increasing volatility and also match up with the timing of Morgan's initial publication. Such an exercise can only make it easier to find support for Morgan's theory since it is eliminating many of the break dates on the far right of the distribution, far removed from the timing of bank deregulation. The results from this slightly reduced sample are found in Table 5, with accompanying histograms in Figure 5. Although there are a few more states which now seem to follow the Morgan hypothesis, we still fail to see much support for interstate banking. The number individual states which now have a structural break in output occurring after the change in banking laws which is significant at the 5% level is 19, up from 10 in our full sample. However, the histograms tell the same story as before; most states experienced a structural break in output volatility well before or well after interstate banking began.

State	Interstate Banking	Break Point	Order	Pre-2000's	5% sig.
AL	1987	1988Q1***	✓	✓	✓
AK	1982	1975Q2***			
AZ	1986	2001Q2			
AR	1989	1968Q3***			
CA	1987	2000Q4***	✓		
CO	1988	2001Q2***	✓		
CT	1982	2001Q2**	✓		
DE	1988	2005Q1***	✓		
FL	1985	1973Q4***			
GA	1985	2000Q3**	✓		
HI	1997	1970Q3***			
ID	1985	1974Q2*			
IL	1986	1988Q2**	✓	✓	✓
IN	1986	1988Q1*	✓	✓	
IA	1991	1980Q3*			
KS	1992	1975Q2*			
KY	1984	1979Q2**			
LA	1987	1988Q1***	✓	✓	✓
ME	1978	1972Q3**			
MD	1985	1990Q2***	✓	✓	✓
MA	1983	1992Q1***	✓	✓	✓
MI	1986	1986Q1*	✓	✓	
MN	1986	1980Q3*			
MS	1988	1972Q2***			
MO	1986	2004Q4***	✓		
MT	1993	1982Q2***			
NE	1990	1978Q3			

NV	1985	2005Q1*	✓		
NH	1987	2004Q4***	✓		
NJ	1986	1991Q2***	✓	✓	✓
NM	1989	2005Q1**	✓		
NY	1982	1988Q2***	✓	✓	✓
NC	1985	1974Q2			
ND	1991	1981Q1**			
OH	1985	1985Q3**	✓	✓	✓
OK	1987	1982Q3***			
OR	1986	2000Q4*	✓		
PA	1986	1989Q2***	✓	✓	✓
RI	1984	2004Q1*	✓		
SC	1986	1974Q2*			
SD	1988	1975Q1			
TN	1985	1987Q2**	✓	✓	✓
TX	1987	1981Q4			
UT	1984	2001Q3**	✓		
VT	1988	2000Q3***	✓		
VA	1985	1973Q3**			
WA	1987	2000Q2***	✓		
WV	1988	1978Q3***			
WI	1987	1983Q2*			
WY	1987	1981Q1			

***significant at 1%, **significant at 5%, *significant at 10%.

Table 4: Interstate Banking and Breaks in Output, 1960:Q1-2012:Q4

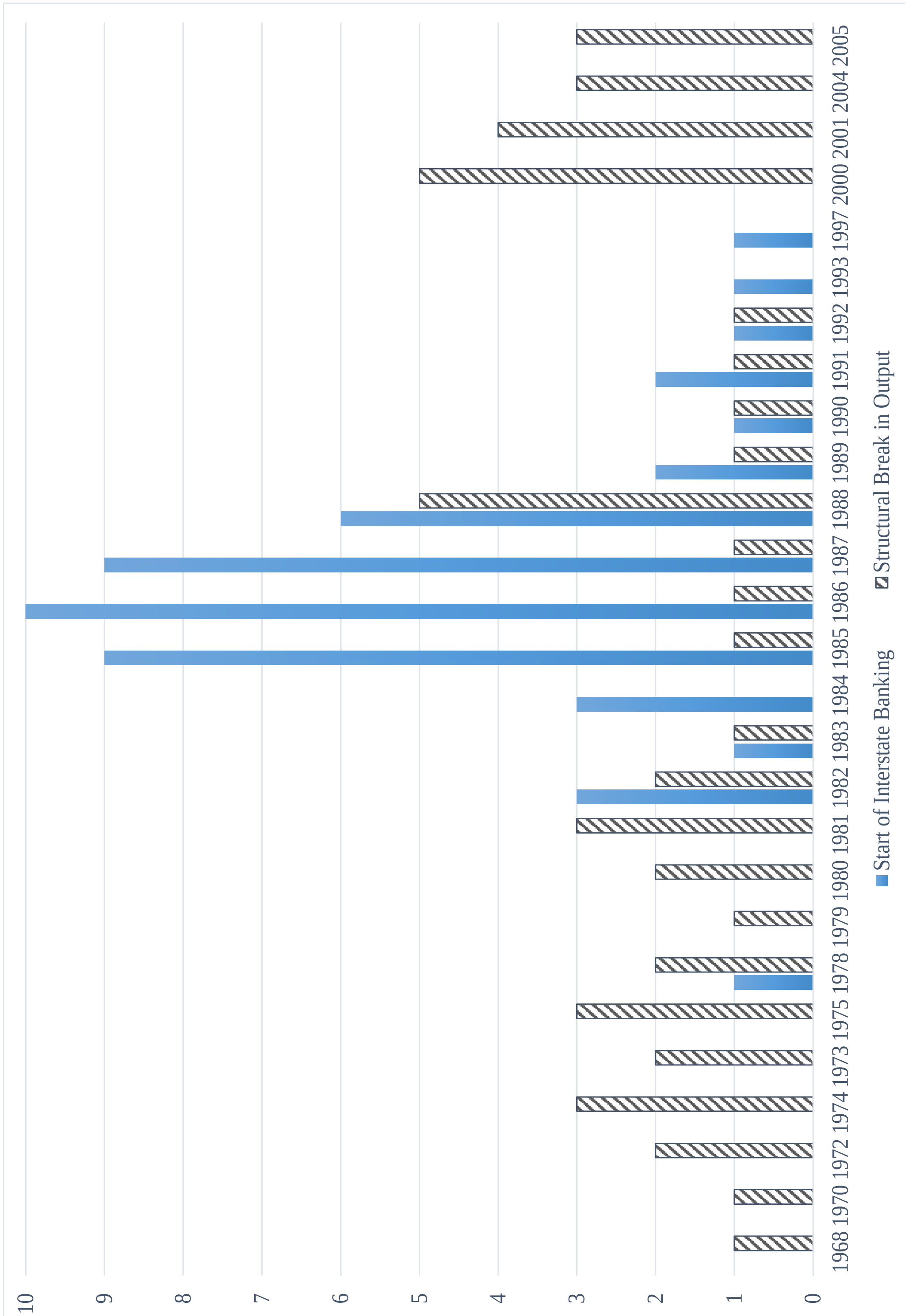

Figure 5: Distribution of estimated break dates and interstate banking dates: 1960:Q1-2012:Q4

5 Conclusion

We have shown that the Great Moderation is not only observed in the aggregated national data, but at the state level as well. Furthermore, there is a great deal of heterogeneity in the break points in output volatility across states. Theories which attempt to explain the cause of the decline in output volatility at the national level should also be able to explain the state-level decline in volatility. Using a quarterly proxy of state output, and durable goods, we perform Quandt-Andrews unknown breakpoint tests for each state. Our results strongly support the findings in the literature linking the Great Moderation to a break in output volatility in the durable goods sector. However, our results do not support theories linking the Great Moderation to the advent of interstate banking.

Past empirical work on this topic has failed to utilize the rich resource of state-level data to put the existing theories to the test. This paper provides an initial attempt to take some of these theories to the state-level data, using a quarterly state output proxy. We encourage future research to continue this effort by testing theories of improved monetary policy and changing nature of aggregate shocks using the more abundant data from the states. These results will shed further light on the true cause(s) of the Great Moderation, and improve our understanding of how output growth changes over time.

A Additional figures from reduced-sample analysis

State	Interstate Banking	Break Point	Order	Pre-2000's	5% sig.
AL	1987	1988Q1***	✓	✓	✓
AK	1982	1975Q2*			
AZ	1986	1992Q4**	✓	✓	✓
AR	1989	1966Q3***			
CA	1987	1993Q2**	✓	✓	✓
CO	1988	1992Q1***	✓	✓	✓
CT	1982	1992Q1*	✓		
DE	1988	1973Q4			
FL	1985	1973Q4***			
GA	1985	1993Q1***	✓	✓	✓
HI	1997	1970Q3***			
ID	1985	1986Q3**	✓	✓	✓
IL	1986	1992Q1***	✓	✓	✓
IN	1986	1986Q1**	✓	✓	✓
IA	1991	1980Q3			
KS	1992	1975Q2*			
KY	1984	1977Q3			
LA	1987	1992Q3***	✓	✓	✓
ME	1978	1972Q3***			
MD	1985	1990Q2***	✓	✓	✓
MA	1983	1992Q1***	✓	✓	✓
MI	1986	1986Q1**	✓	✓	✓
MN	1986	1982Q4**			
MS	1988	1972Q2**			
MO	1986	1991Q4*	✓	✓	✓
MT	1993	1982Q2**			

NE	1990	1978Q3			
NV	1985	1992Q1***	✓	✓	✓
NH	1987	2000Q2***	✓	✓	
NJ	1986	1991Q2***	✓	✓	✓
NM	1989	1978Q4***			
NY	1982	1988Q2***	✓	✓	✓
NC	1985	2000Q4*	✓		
ND	1991	1981Q1**			
OH	1985	1989Q2***	✓	✓	✓
OK	1987	1978Q1***			
OR	1986	2000Q4**	✓	✓	
PA	1986	1989Q2***	✓	✓	✓
RI	1984	1993Q1**	✓	✓	✓
SC	1986	1975Q2			
SD	1988	1975Q1			
TN	1985	1978Q3**			
TX	1987	2000Q2			
UT	1984	2001Q2			
VT	1988	2000Q3***	✓	✓	
VA	1985	1969Q2*			
WA	1987	1993Q1*	✓		
WV	1988	1981Q4**			
WI	1987	2000Q4**	✓	✓	
WY	1987	1983Q1			

Table 5: Interstate Banking and Breaks in Output, 1960:Q1-2004:Q4

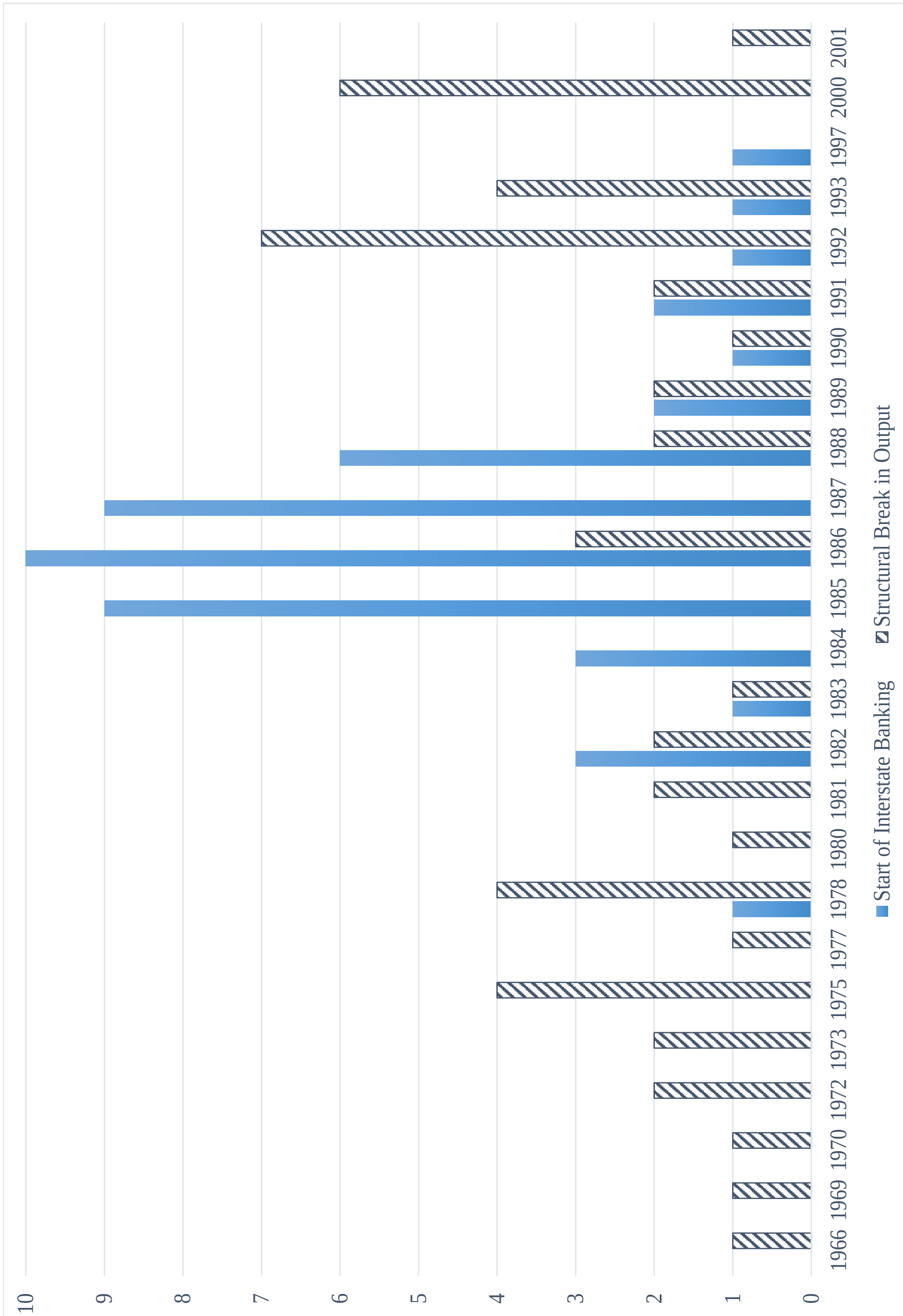

Figure 6: Distribution of estimated break dates and interstate banking dates: 1960:Q1-2012:Q4

State	Break Date	Max (p-values)	Exp (p-values)	Ave (p-values)
AL	1988Q1	18.090 (0.0028)	6.229 (0.0027)	7.635 (0.0058)
AK	1975Q2	11.303 (0.0556)	3.521 (0.0359)	6.327 (0.0142)
AZ	1992Q4	14.925 (0.0117)	4.376 (0.0151)	4.209 (0.0686)
AR	1966Q3	21.251 (0.0007)	7.499 (0.0009)	6.875 (0.0097)
CA	1993Q2	13.283 (0.0241)	3.733 (0.0289)	5.377 (0.0283)
CO	1992Q1	15.53 (0.0090)	4.656 (0.0115)	5.309 (0.0297)
CT	1992Q1	9.997 (0.0948)	2.744 (0.0809)	3.133 (0.1593)
DE	1973Q4	7.378 (0.2573)	1.637 (0.2563)	2.347 (0.2953)
FL	1973Q4	15.858 (0.0078)	5.116 (0.0074)	6.965 (0.0091)
GA	1993Q1	24.735 (0.0001)	9.457 (0.0002)	9.488 (0.0064)
HI	1970Q3	15.634 (0.0086)	5.391 (0.0057)	7.674 (0.0056)
ID	1986Q3	13.728 (0.0198)	4.982 (0.0092)	6.883 (0.0096)
IL	1992Q1	34.945 (0.0000)	13.492 (0.0000)	11.177 (0.0007)
IN	1986Q1	11.993 (0.0417)	3.868 (0.0252)	4.327 (0.0626)
IA	1980Q3	8.237 (0.1878)	1.858 (0.2098)	3.049 (0.1701)
KS	1975Q2	10.306 (0.0837)	2.478 (0.1075)	1.795 (0.4505)
KY	1977Q3	8.461 (0.1726)	1.957 (0.1886)	2.637 (0.2352)
LA	1992Q3	22.466 (0.0004)	8.983 (0.0003)	10.107 (0.0013)
ME	1972Q3	21.358 (0.0006)	6.615 (0.0019)	2.206 (0.3294)
MD	1990Q2	20.317 (0.0010)	7.182 (0.0012)	9.301 (0.0021)
MA	1992Q1	52.294 (0.0000)	22.061 (0.0000)	8.947 (0.0025)
MI	1986Q1	12.012 (0.0414)	4.269 (0.0168)	5.595 (0.0241)
MN	1982Q4	13.668 (0.0204)	5.109 (0.0074)	7.543 (0.0062)
MS	1972Q2	12.795 (0.0297)	4.322 (0.0160)	4.179 (0.0702)
MO	1991Q4	10.539 (0.0761)	3.042 (0.0591)	4.026 (0.0791)
MT	1982Q2	14.609 (0.0135)	4.011 (0.0218)	4.715 (0.0465)
NE	1978Q3	8.171 (0.1925)	1.727 (0.2418)	2.592 (0.2438)

NV	1992Q1	18.635 (0.0022)	6.464 (0.0022)	7.029 (0.0087)
NH	200Q2	22.924 (0.0001)	7.648 (0.0000)	7.192 (0.0004)
NJ	1991Q2	27.086 (0.0000)	10.532 (0.0000)	9.401 (0.0000)
NM	1978Q4	12.231 (0.0093)	3.427 (0.0085)	4.908 (0.0084)
NY	1988Q2	27.239 (0.0000)	8.887 (0.0000)	8.453 (0.0000)
NC	2000Q4	7.161 (0.0962)	1.497 (0.0990)	1.700 (0.1533)
ND	1981Q1	8.729 (0.0473)	2.058 (0.0490)	2.436 (0.0738)
OH	1989Q2	12.603 (0.0078)	4.304 (0.0020)	6.704 (0.0010)
OK	1978Q1	19.504 (0.0003)	5.284 (0.0002)	7.647 (0.0002)
OR	2000Q4	10.134 (0.0248)	1.828 (0.0651)	1.585 (0.1735)
PA	1989Q2	19.148 (0.0003)	6.218 (0.0000)	7.825 (0.0001)
RI	1993Q1	8.621 (0.0497)	1.999 (0.0527)	2.532 (0.0676)
SC	1975Q2	6.431 (0.1331)	0.993 (0.1986)	1.591 (0.1724)
SD	1975Q1	4.662 (0.2851)	0.502 (0.4389)	0.701 (0.4979)
TN	1978Q3	10.863 (0.0176)	3.256 (0.0073)	5.773 (0.0033)
TX	2000Q2	1.157 (0.3513)	0.878 (0.2363)	1.529 (0.1847)
UT	2001Q2	1.256 (0.9606)	0.141 (0.9177)	0.248 (0.8887)
VT	2000Q3	30.249 (0.0000)	10.584 (0.0000)	14.445 (0.0000)
VA	1969Q2	8.252 (0.0588)	1.035 (0.1867)	0.935 (0.3703)
WA	1993Q1	7.675 (0.0764)	2.100 (0.0466)	3.127 (0.0400)
WV	1981Q4	10.788 (0.0183)	1.926 (0.0577)	2.262 (0.0871)
WI	2000Q4	8.679 (0.0484)	2.506 (0.0284)	4.094 (0.0175)
WY	1983Q1	5.453 (0.2038)	0.725 (0.3006)	1.105 (0.3010)

Table 6: Quandt-Andrews breakpoint test results all 50 states: 1960:Q1-2012:Q4

State	Break Date	Max (p-values)	Exp (p-values)	Ave (p-values)
AL	1988Q1	16.232 (0.0066)	5.143 (0.0072)	8.553 (0.0032)
AK	1975Q2	23.026 (0.0009)	7.069 (0.0049)	8.743 (0.0071)
AZ	2001Q2	9.861 (0.1001)	2.851 (0.0723)	4.231 (0.0674)
AR	1968Q3	21.331 (0.0006)	7.559 (0.0009)	7.260 (0.0074)
CA	2000Q4	16.604 (0.0055)	3.952 (0.0231)	4.939 (0.0392)
CO	2001Q2	25.932 (0.0001)	8.322 (0.0005)	6.033 (0.0175)
CT	2001Q2	11.678 (0.0476)	3.206 (0.0498)	4.366 (0.0608)
DE	2005Q1	16.364 (0.0062)	4.742 (0.0106)	5.767 (0.0212)
FL	1973Q4	24.249 (0.0002)	8.610 (0.0004)	10.472 (0.0011)
GA	2000Q3	14.291 (0.0155)	4.295 (0.0164)	5.903 (0.0192)
HI	1970Q3	20.351 (0.0010)	7.034 (0.0013)	9.099 (0.0023)
ID	1974Q2	11.042 (0.0620)	2.323 (0.1269)	3.642 (0.1067)
IL	1988Q2	13.533 (0.0216)	4.533 (0.0130)	5.643 (0.0232)
IN	1988Q1	11.888 (0.1081)	3.793 (0.0733)	5.966 (0.0555)
IA	1980Q3	11.088 (0.0608)	2.537 (0.1010)	3.666 (0.1047)
KS	1975Q2	11.130 (0.0598)	2.796 (0.0766)	2.332 (0.2987)
KY	1979Q2	13.391 (0.0230)	3.773 (0.0277)	4.648 (0.0192)
LA	1988Q1	19.568 (0.0014)	7.518 (0.0009)	9.268 (0.0021)
ME	1972Q3	14.282 (0.0156)	3.854 (0.0255)	4.568 (0.0520)
MD	1990Q2	22.633 (0.0003)	7.936 (0.0006)	9.588 (0.0017)
MA	1992Q1	16.998 (0.0046)	5.475 (0.0053)	4.919 (0.0398)
MI	1986Q1	10.184 (0.0879)	3.432 (0.0394)	5.996 (0.0180)
MN	1980Q3	11.241 (0.0571)	3.889 (0.0246	5.987 (0.0181)
MS	1972Q2	16.427 (0.0060)	5.536 (0.0050)	5.189 (0.0325)
MO	2004Q4	24.945 (0.0001)	9.907 (0.0002)	12.437 (0.0004)
MT	1982Q2	15.567 (0.0088)	4.329 (0.0159)	3.475 (0.1217)
NE	1978Q3	9.496 (0.1157)	1.862 (0.2089)	2.063 (0.3675)

NV	2005Q1	13.215 (0.0248)	4.005 (0.0219)	4.462 (0.0416)
NH	2004Q4	22.511 (0.0004)	7.659 (0.008)	6.231 (0.0152)
NJ	1991Q2	20.538 (0.0009)	7.149 (0.0012)	8.253 (0.0039)
NM	2005Q1	12.395 (0.0352)	3.659 (0.0311)	4.969 (0.0383)
NY	1988Q2	15.484 (0.0092)	4.224 (0.0176)	5.425 (0.0273)
NC	1974Q2	11.740 (0.2297)	3.199 (0.2633)	4.290 (0.3565)
ND	1981Q1	11.762 (0.0459)	2.487 (0.1066)	2.535 (0.2550)
OH	1985Q3	14.215 (0.0160)	5.264 (0.0064)	7.662 (0.0057)
OK	1982Q3	38.578 (0.0000)	16.271 (0.0000)	14.123 (0.0002)
OR	2000Q4	11.319 (0.0553)	3.332 (0.0436)	4.839 (0.0423)
PA	1989Q2	22.934 (0.0003)	8.329 (0.0005)	9.378 (0.0020)
RI	2004Q1	11.272 (0.0563)	2.880 (0.0701)	3.899 (0.0873)
SC	1974Q2	10.037 (0.0933)	3.101 (0.0555)	5.469 (0.0264)
SD	1975Q1	5.199 (0.5276)	0.716 (0.7034)	1.129 (0.7201)
TN	1987Q2	13.932 (0.0182)	4.966 (0.0085)	8.165 (0.0041)
TX	1981Q4	7.603 (0.2373)	1.749 (0.2360)	2.723 (0.2196)
UT	2001Q3	11.718 (0.0468)	2.974 (0.0635)	2.702 (0.2237)
VT	2000Q3	28.124 (0.0000)	9.694 (0.0002)	14.081 (0.0002)
VA	1973Q3	14.037 (0.0173)	4.557 (0.0127)	6.665 (0.0112)
WA	2000Q2	22.120 (0.0048)	6.901 (0.0097)	9.051 (0.0180)
WV	1978Q3	19.815 (0.0013)	5.106 (0.0075)	2.524 (0.2572)
WI	1983Q2	10.184 (0.0879)	3.375 (0.0417)	5.259 (0.0308)
WY	1981Q1	6.946 (0.2997)	1.429 (0.3335)	1.821 (0.4416)

Table 7: Quandt-Andrews breakpoint test results all 50 states: 1960:Q1-2004:Q4

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Conclusion

Since the double-dip recession of the early 1980's, there have been significant changes in the nature of several important business cycle fluctuations. This work has focused on two of those recent changes which have been less well understood; the Great Moderation, and jobless recoveries. These phenomena are similar in that they have both generated a seizable literature which has posited many competing explanations for their advents, yet no consensus has been reached over time. With both jobless recoveries and the Great Moderation, past empirical work has focused almost entirely on the national time-series data for the U.S., resulting in a relatively small number of observations.

This dissertation has sought movement towards a better understanding of the causes of both jobless recoveries and the Great Moderation by using state-level data to test the existing hypotheses on their respective causes. The use of data from the fifty state economies provides a much greater number of observations which may be desirable for statistical work. In both cases, the panel analysis results find support for some hypotheses, and fail to find support for others. This helps to narrow down the field of competing explanations for both the jobless recovery and Great Moderation phenomena, hopefully leading to a better understanding of the true causes of each.