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IRVINE

Essays in Policy Evaluation

DISSERTATION

submitted in partial satisfaction of the requirements  
for the degree of

DOCTOR OF PHILOSOPHY

in Economics

by

Patrick James Harold Button

Dissertation Committee:  
Professor David Neumark, Chair  
Professor Marianne Bitler  
Professor Jan Brueckner

2015



# DEDICATION

I dedicate this dissertation to my parents, Gene and David Button. I could not have gotten this far without both of you. I have asked much of you to survive this ordeal and you have never hesitated to go above and beyond what was required to make sure I was taken care of. I hope I can repay your kindness and patience.

I would like to thank my dissertation committee for all their support. I am particularly indebted to David Neumark for being my role model for so many years. I never felt like a real economist until I was able to work with you. I can't imagine what my life would be like had I not had the privilege of working with you. I thank Marianne for being an inspiration to me as someone who strives to achieve the most rigorous standards in research. I hope to continue to achieve rigorous empirical standards in my research by asking myself: "What would Marianne do?" I thank Jan Brueckner for sharing his wisdom with me, especially when I was writing my paper on tax incentives for the film industry. I could not have made my foray into urban economics without you.

I would also like to thank my partner, Dylan Moore, for putting up with me during the final stages of my dissertation and during the job market. You have a lot of patience and character because I am sure that was not easy! I feel honored that you were a part of this.

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# CURRICULUM VITAE

Patrick James Harold Button

## Education

Ph.D. Economics, University of California, Irvine, 2015

Advisors: David Neumark (Chair), Marianne Bitler, Jan Brueckner

Research Fields: Public, Labor, and Urban Economics, Applied Econometrics

M.A. Economics, University of Toronto, 2010

B.A. Honours Economics, University of Regina, 2008

## Employment

July 2014 -	Assistant Professor of Economics, Tulane University
2014 - 2015	Haynes Lindley Doctoral Dissertation Fellow
2014 & 2012	Instructor, Teaching Assistant Professional Development Program
2012 - 2014	Research Assistant, David Neumark
2011	Lecturer, University of Regina
2010	Economist, Industry Canada

## Peer-Reviewed Publications

“Model Uncertainty and Model Averaging in Regression Discontinuity Designs” (2015), forthcoming in *Journal of Econometric Methods*

“Did Age Discrimination Protections Help Older Workers Weather the Great Recession?” (2014) (with David Neumark), *Journal of Policy Analysis and Management*, 33(3): 566-601.

## Manuscripts Under Review

“Does Protecting Older Workers from Discrimination Make It Harder to Get Hired? Evidence from Disability Discrimination Laws” (With David Neumark and Joanne Song), revisions requested at *Research on Aging*

## Manuscripts in Preparation

“Do Tax Incentives Affect Business Location? Evidence from Motion Picture Production Incentives” (Job Market Paper)

“Testing for Age Discrimination: Obtaining More Convincing Evidence from Field Experiments” (With David Neumark and Ian Burn)

“Expanding Disability Discrimination Laws to Those With Less Severe Impairments: Evidence from California’s Prudence Kay Poppink Act”

## Conference and Seminar Presentations

- 2015      The RAND Corporation (01/29)  
            University of Wisconsin - Stout (02/03)  
            Huron University College at the University of Western Ontario (02/09)  
            Tulane University (02/18)
- 2014      Southern Economic Association (11/22)  
            Association for Public Policy Analysis & Management (11/06)  
            Canadian Economic Association (06/01)
- 2013      AEA Conference on Econ. Ed. (Discussant) (05/30)

## Local Talks

- 2014      UCI Center for Economics & Public Policy (12/01)  
            UCI Applied Microeconomics Colloquium (4/29, 10/7)  
            UCI IMBS Graduate Conference (5/23)
- 2012      UCI Econometrics Colloquium (5/30)
- 2008      U. of Regina Putting Theory Into Practice Conference (04/15)

## Grants and Awards

### University of California, Irvine

- 2014-2015 Haynes Foundation Doctoral  
Dissertation Fellowship (\$20,000)
- 2014 Order of Merit: Kathy Alberti Prize for  
Outstanding Research & Service
- 2014 AGS Travel Grants
- 2013-2014 UCI Center for Economics &  
Public Policy Fellow
- 2012-2014 Summer Research Fellowship
- 2012-2013 Pedagogical Fellow
- 2012 UCLA Labor and Employment  
Research Fund Mini-Grant
- 2010-2015 Social Sciences Tuition Fellowship

### University of Toronto

- 2008-2009 Joseph-Armand Bombardier Canada  
Graduate Scholarship (\$17,500)
- 2008-2009 University of Toronto Fellowship
- 2008-2009 Mary H. Beatty Fellowship

### University of Regina

- 2008 Graduation with High Honours
- 2006-2007 Academic Gold Scholarships
- 2007 David L. Beattie Scholarship  
in Economics
- 2006 Dean's Honours List
- 2005 Centennial Merit Scholarship

## **Professional Service**

Journal Referee

Research on Aging  
Economics of Governance  
Association for Institutional Research (x2)

University of California, Irvine

2013- Graduate Economics Council  
2012- TA Workshop Committee Chair  
2013 Organizer, Economics Student Colloquium  
2012-2013 Organizer, Mock Candidacy Exams

University of Regina

2007-2008 University Senate Member  
2007-2008 Senate Appeals Committee Member  
2006-2008 Co-Pres. of Econ. Students' Assoc.  
2006-2008 Students' Union Board Member  
2006-2008 LGBT Centre Board Member

## **Teaching as Instructor**

2012 - Social Sciences Teaching Workshop Series, UC Irvine  
2012 - Teaching Assistant Consultant, UCI TLTC  
2014, 2012 Teaching Assistant Professional Development Program, UC Irvine  
2011 Intermediate Microeconomics I, University of Regina

## Teaching Assistant

University of California, Irvine

Intro Microeconomics (Fall 2011), Intro Stats for Economists (Spring 2011), Managerial Econ (Winter 2011), Introductory Macroeconomics (Fall 2010)

University of Toronto

Microeconomics for Commerce (Fall 2008, Winter 2009, Fall 2009)

University of Regina

Co-Manager, Economics Tutorial Centre (2006-2007), Intermediate Macro (Fall 2007), Intermediate Micro (Winter 2007), Intro Micro (Fall 2006, Summer 2006)

## Pedagogical Development

2012-2013 Pedagogical Fellows Program

A selective year long faculty development program covering course construction, active learning, online courses, classroom technology, diversity, mentoring, and many other topics.

## Teaching Publications

*“Think-Pair-Share: Where Does the Unemployment Rate Come From?”* in *The Ultimate Guide to Teaching Macroeconomics* (2014) by Wayne Geerling, Lee Coppock, Kim Holder, and James Tierney.

## Languages

English (Native), French (Intermediate)  
STATA, MATLAB, L<sup>A</sup>T<sub>E</sub>X, VBA, X-12-ARIMA, gnuplot

# ABSTRACT OF THE DISSERTATION

Essays in Policy Evaluation

By

Patrick James Harold Button

Doctor of Philosophy in Economics

University of California, Irvine, 2015

Professor David Neumark, Chair

This dissertation contains three independent papers (chapters). Chapter 1 argues that researchers that use a regression discontinuity design with parametric controls for the assignment variable face significant uncertainty as to the appropriate model. I argue that researchers should incorporate this model uncertainty into their inference through model averaging. I show in a Monte Carlo experiment that Frequentist model averaging, compared to pretesting, leads to more precise estimates and less Type I error. Chapter 2 studies the impact of state-level tax incentives for the film industry, which have exploded since the early 2000s. I study how these incentives affect filming location and the number of jobs and businesses in the film industry. I seek to answer the question: can these incentives create a local film industry? I find that while these incentives have large effects on filming location, they almost never lead to increases in jobs and employment in the film industry. Chapter 3 studies the Prudence Kay Poppink Act, effective 2001, which broadened California's disability discrimination in employment law. This expanded coverage to those with less severe impairments. I study how this expansion affected labor market outcomes for the disabled by comparing the disabled in California before and after the Act to the disabled in other states during the same time period. I augment this difference-in-difference methodology by adding a comparison to the non-disabled as well. I find that this expansion of legal protections increased employment and possibly increased wage and salary rates.



# Chapter 1

## Model Averaging and Model Uncertainty in Regression Discontinuity Designs

### 1.1 Abstract

Parametric (polynomial) models are popular in research employing regression discontinuity designs and are required when data are discrete. However, researchers often choose a parametric model based on data inspection or pretesting. These approaches lead to standard errors and confidence intervals that are too small because they do not incorporate model uncertainty. I propose using Frequentist model averaging to incorporate model uncertainty into parametric models. My Monte Carlo results show that Frequentist model averaging leads to mean square error and coverage probability improvements over pretesting. An application to Lee (2008) shows how this approach works in practice, and how conventionally selected models may not be ideal.

## 1.2 Introduction

Model uncertainty was and continues to be a popular topic of research in statistics and econometrics (Claeskens and Hjort 2008; Leeb and Pötscher 2005; Burnham and Anderson 2002; Buckland, Burnham and Augustin 1997; Chatfield 1995; and Pötscher 1991). Even with the expansive literature on this topic, many researchers fail to deal adequately with model uncertainty or even address it, to the point where Breiman (1992) describes it as the “quiet scandal” in statistics.

With the resurgence of regression discontinuity design (RDD) as a popular quasi-experimental method (Cook, 2008), many researchers have applied the design to make causal inference. Using a polynomial to control for the assignment variable (also called the running or forcing variable) in RDD is a popular strategy and is required when the assignment variable is discrete (Lee, 2008). However, most researchers select their model without any clear justification, often by inspecting scatter plots of the data or pretesting. Model uncertainty is poorly addressed in all of these approaches, leading to standard errors and confidence intervals that are too small. Model uncertainty is prevalent in RDD, as there is often little evidence or theory to suggest a proper functional form to control for the assignment variable.

To better address model uncertainty, I propose using Frequentist model averaging (FMA) to select polynomial controls for the assignment variable. This strategy averages estimates over several models, weighting each model based on its relative likelihood. Statistical theory suggests that this improves mean square error, leads to more accurate standard errors and confidence intervals, and decreases Type I error (Claeskens and Hjort 2008; Hjort and Claeskens 2003). I confirm that the benefits suggested by theory apply in regression discontinuity designs by running several Monte Carlo experiments. I then apply FMA to a notable application: estimating the incumbency advantage in US House of Representatives elections (as in Lee 2008; Lee and Lemieux 2010). This application shows how estimates

and model selection weights vary across a large set of feasible polynomial models, and how conventionally selected models may not be the best for this data.

### 1.3 A Brief Review of the RDD Literature

Suppose the following simple model for a sharp RDD with a binary treatment:

$$\begin{aligned} Y &= \tau T + f(X) + \epsilon \\ T &= 1[X \geq c] \end{aligned} \tag{1.1}$$

where  $Y$  is the outcome of interest,  $X$  is the assignment variable,  $f(X)$  is a usually unknown function of  $X$ , and  $\tau$  is the “average treatment effect” at the cut-off of  $c$  (Lee and Lemieux, 2010). In a sharp RDD, treatment status,  $T$ , is perfectly determined by  $X$  exceeding the cut-off  $c$ . For a fuzzy RDD, an additional unknown function of  $X$  determines treatment. From this point forward I only discuss the sharp RDD case.

Properly controlling for the unknown functions of the assignment variable in RDD models has a critical impact on inference. Failing to control for the curviness of  $f(X)$  leads to a biased estimate of  $\tau$  (Lee and Lemieux, 2010). There are many approaches to select a functional form for  $f(X)$ , but the Frequentist literature advocates two approaches: local linear regression and polynomial regression.<sup>1</sup> The two are discussed below for sharp RDD.

---

<sup>1</sup>The only explicitly Bayesian RDD papers I found were Rau (2011) and Chib and Greenberg (2014), both of whom used penalized regression splines.

### 1.3.1 Local Linear Regression

Local linear regressions are of the form:

$$Y = \alpha + \tau T + \beta_l(X - c) + (\beta_r - \beta_l)T(X - c) + \epsilon, \quad \text{where } c - h \leq X \leq c + h \quad (1.2)$$

This approach fits a separate linear regression for each side of the cut-off using only the observations within the bandwidth  $h$ . As  $n \rightarrow \infty, h \rightarrow 0$ , giving consistency, however, continuity of  $X$  is required for this.

Non-parametric methods, such as local linear regression, are not feasible when the assignment variable is discrete, as the bandwidth cannot be shrunk beyond a certain point (Lee, 2008). Examples of discrete assignment variables are common. The most common is age in years, such as in Card, Dobkin and Maestas (2009) or Oreopoulos (2006).

### 1.3.2 Polynomial Regression

Polynomial controls for the assignment variable are popular among researchers even though the literature recommends local linear regression for continuous assignment variables. Even though polynomials may not be ideal for continuous assignment variables, Lee and Lemieux (2010) argue that they should be used anyways as a robustness check to ensure that inference is not dependent on one particular modeling strategy.

Polynomial models are of the form:

$$Y = \alpha + \tau T + \sum_{i=1}^p \beta_i(X - c)^i + \epsilon \quad (1.3)$$

In addition, a bandwidth restriction is sometimes used such that  $X \in [c - h, c + h]$  as in Equation (1.2). Equation (1.3) represents the functional form under the assumption that one

functional form best fits the entire support of  $X$ . This is the case if there is no discontinuity in the slope, concavity, or higher-order derivatives in the polynomial control at the cut-off  $c$ . Relaxing this assumption gives:

$$Y = \alpha + \tau T + \sum_{i=1}^p \beta_i (X - c)^i + \sum_{i=1}^q \beta_{ri} T (X - c)^i + \epsilon \quad (1.4)$$

where  $r$  represents that the second polynomial term is only fitted to the right side of the cut-off. This term determines the extent to which there is a separate polynomial term for each side of the cut-off. If  $p = q$  then this represents an extreme case where each side of the cut-off has its own polynomial control of order  $p$ . If  $q = 0$  then one polynomial is used to control for the entire range of  $X$ .

Lee and Lemieux (2010) note that if there are thought to be discontinuities in derivatives of  $f(X)$  at the cut-off, and the restricted model in Equation (1.3) is used, then the estimate of  $\tau$  will be biased. For this reason, they recommend using the model in Equation (1.4). If there are no discontinuities in derivatives of  $f(X)$  then Equation (1.3) is a preferred model since it does not require the estimation of extra terms that would be unnecessary and would decrease precision. Also the degree of freedom that was applied to  $q$  could have been used to increase  $p$  instead. Since it is not obvious which approach is best in most cases, it is clear that researchers face uncertainty and trade-offs in model selection.

As shown in the next section, there are a variety of plausible models in the polynomial family that are generally overlooked. Defaulting to one over others based on convenience, heuristics, assumptions, or pretesting ignores a significant amount of model uncertainty since the “true” model that generated the data is almost never known.

## 1.4 Problems with Model Selection in RDD

Selecting the best polynomial order that balances bias and variance is crucial if a polynomial model is used. Generally this means minimizing mean square error. Under-fitting gives too much bias relative to variance, and over-fitting inflates variances by estimating extra parameters that are not that useful. In either case, there is an increase in mean square error. In addition to uncertainty in the polynomial order, researchers must decide between the model in Equation (1.3), with one polynomial, or Equation (1.4), with one polynomial for each side. In addition, one can imagine plausible cases that lie within the two above extremes of Equations (1.3) and (1.4), or cases where  $q \geq p$ . For example, if only the slope of  $f(X)$  changes discontinuously at the cut-off, but not higher-order derivatives, then only the term  $\beta_r T(X - c)$  in the second summation term of Equation (1.4) is necessary. If a researcher is only considering 3<sup>rd</sup> degree polynomials or less, and no restrictions on the relationship between  $p$  and  $q$  are made, then there are 16 possible models, with many more if higher order polynomials are considered. Selecting a polynomial model to get the bias-variance trade-off “just right” is difficult in practice as there are so many options and the approaches commonly used, such as visual inspection or pretesting, are problematic.

### 1.4.1 Pretesting

Lee and Lemieux (2010) recommend selecting an optimal polynomial order using the Akaike Information Criterion (AIC) (Akaike, 1974). Polynomial models of several orders are run, and the AIC is calculated for each model. The polynomial order that minimizes the AIC is used. Other forms of pretesting are also possible, such as from other information criteria or t-tests.

Selecting a polynomial order by pretesting, and reporting this pretest, is an honorable

way to admit that there is model uncertainty. However, it is not without many problems. The interpretation of regression results are different if pretesting is used, as the distribution of the estimates are conditional on the results of the earlier pretest. Properties of pretest estimator are difficult to know, adding another layer of uncertainty (Davidson and Mackinnon, 1993). Most importantly, Cohen (1965) shows that pretest estimators are inadmissible with respect to conventional loss functions, such as the commonly used quadratic loss. Greene (2008) notes that pretest estimators are biased. The interested reader should see Giles and Giles (1993) and Wallace (1977) for excellent backgrounds on the effects of pretesting.

### 1.4.2 Data Inspection

Lee and Lemieux (2010) suggest examining non-parametric scatter plots of the data to try to assess the curvature of  $f(X)$  to determine polynomial forms that seem reasonable. Bins of  $X$  are selected, and local averages of  $Y$  (and again with  $T$  for a fuzzy RDD) are plotted for each bin. Fitted values from some models are sometimes added to investigate how well this model controls for the unknown  $f(X)$ . This is a transparent way to visualize the data, but it is a poor model selection strategy.

Model selection based on data inspection is arguably a form of pretesting. The model selected would be, in an unclear fashion, a function of some sort of subjective test based on how the sample of data is presented and then visually interpreted. Scatter plots with wider or narrower bins may present the data differently, particularly if there is significant variance in the assignment variable. Comparing the fit of a proposed polynomial model to a scatter plot may be helpful to determine when a particular model possibly under fits, but it says nothing about over fitting. More importantly it ignores the fact that as the sample size increases model parameters should also increase.

## 1.5 The Impacts of Model Uncertainty

Chatfield (1995), Draper et al. (1987), and Hodges (1987) note that there are three main sources of uncertainty in statistical inference:

- (1) uncertainty about the structure of the model;
- (2) uncertainty about model parameter estimates, assuming the structure is known;
- (3) unexplained random variation in observables after accounting for (1) and (2).

(1) is often ignored by applied researchers; it is covered more indirectly by some through robustness checks. On the other hand, (2) and (3) are almost always addressed. The ignorance that many researchers have with respect to model uncertainty is shocking given the large literature on the topic.<sup>2</sup> Breiman (1992) describes this ignorance as the “quiet scandal” in statistical research.

Those who ignore model uncertainty implicitly assume that their selected model is the “true” one that generated the data. This leads to standard errors that are too small and confidence intervals that are too tight. For hypothesis testing, Hjort and Claeskens (2003) note that under model uncertainty a hypothesis rejected at an announced 5% significance level might have actually been tested at a higher level. This leads to an increase in Type I error. These problems resulting from model uncertainty are a particular problem in regression discontinuity design, where there is often little theoretical evidence or prior knowledge to suggest an optimal functional form for the assignment variable.

These problems are not necessarily solved through typical robustness checks. Since there is still model uncertainty under alternative models, the standard errors and confidence

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<sup>2</sup>See, for example, Leeb and Pötscher (2005), Buckland, Burnham and Augustin (1997), Chatfield (1995), and Pötscher (1991).



intervals are still too small. Robustness checks may not even include appropriate alternatives, particularly if the originally selected model was poor. For example, if a quadratic polynomial control for  $f(X)$  is used when the sample size is large, leading to a poor bias-variance trade-off, then trying the nearby linear and cubic models does not do much better at having an optimal bias-variance trade-off, or addressing model uncertainty. Also, it is crucial for researchers to have one main model that leads to the best estimate of the treatment effect. Since this estimate is the main one that is reported, discussed, and used by consumers of research, it is best for it to be as accurate as possible and thus more robust to model uncertainty.

## 1.6 An Introduction to Model Averaging

Model averaging has a large and rapidly growing literature. Hansen and Racine (2012), Liang et al. (2011), Moral-Benito (2015), Claeskens and Hjort (2008), Hansen (2007), Hjort and Claeskens (2003), Burnham and Anderson (2002), and Buckland, Burnham and Augustin (1997) contribute to the Frequentist literature, while Moral-Benito (2015), Hoeting et al. (1999), and Raftery, Madigan and Hoeting (1997) cover the Bayesian literature.<sup>3</sup> Although model averaging will not entirely solve problems relating to model uncertainty, it leads to improved inference, particularly in regression discontinuity designs, where there is significant uncertainty over the functional form for the assignment variable. In classical statistical analysis, model averaging sidesteps the requirement to assume a “true” model, as it smooths estimators over several feasible models. In addition to model averaging being particularly useful in cases where there is little information to suggest the true model, as is common in the RDD case, model averaging is also useful when there is a significant amount of model selection instability. Yuan and Yang (2005) define this as the sensitivity of

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<sup>3</sup>Model averaging is also related to shrinkage estimation, which Hjort and Claeskens (2003) discuss.

parameter estimates (determined after model selection) to minor perturbations of the data.<sup>4</sup>

Researchers generally select a model and base inference on a single estimator,  $\hat{\tau}$ , while the actual estimator, as shown by Moral-Benito (2015), depends on the model chosen:

$$\hat{\tau} = \begin{cases} \hat{\tau}_1 & \text{for model 1} \\ \hat{\tau}_2 & \text{for model 2} \\ \vdots & \\ \hat{\tau}_M & \text{for model } M \end{cases} \quad (1.5)$$

Where  $M$  is the number of possible models under consideration. In the RDD case, this set of  $M$  models for  $f(X)$  follow those in Equation (1.4):

$$f(X) = \sum_{i=1}^p \beta_i (X - c)^i + \sum_{i=1}^q \beta_{ri} T(X - c)^i \quad (1.6)$$

The restriction  $q \leq p$  provides the least restrictive set of models to consider of polynomial order  $p$  or lower, and  $q = p$  or  $q = 0$  provide the most restrictive set. Rewriting the above estimator for  $\hat{\tau}$  from Equation (1.5) gives:

$$\hat{\tau} = \sum_{m=1}^M \tilde{\omega}_m \hat{\tau}_m \quad (1.7)$$

$$\text{where } \tilde{\omega}_m = \begin{cases} 1 & \text{if model } m \text{ is selected} \\ 0 & \text{otherwise} \end{cases}$$

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<sup>4</sup>Yuan and Yang (2005) develop an index that measures perturbation instability in estimation, which they call PIE. They suggest a rule of thumb that model averaging should be used over model selection when  $PIE \geq 0.5$ , while values less than 0.4 suggest a good model selection method is likely to work better.

Model averaging smooths weights across models such that:

$$\hat{\tau} = \sum_{m=1}^M \omega_m \hat{\tau}_m \tag{1.8}$$

where  $0 \leq \omega_m \leq 1 \forall m$  and  $\sum_{m=1}^M \omega_m = 1$

There are several strategies to determine weights. For Frequentist approaches, Buckland, Burnham and Augustin (1997) use smoothed Akaike information criterion (s-AIC) and smoothed Bayesian information criterion (s-BIC) weights. More modern approaches include Mallows model averaging (MMA) (Hansen, 2007), which uses Mallows’  $C_p$  and Jackknife model averaging (JMA) (Hansen and Racine, 2012), which is based on “leave one out” cross-validation. All four are presented below.

### 1.6.1 Smoothed AIC and BIC

FMA weight selection is most commonly based on information criteria of the form:

$$I_m = -2\ell_m + \varphi_m \tag{1.9}$$

Where  $\ell_m$  is the maximized log likelihood function of the  $m^{\text{th}}$  model, and  $\varphi_m$  is the value of the penalty term for model  $m$ , which is based off its  $k$  parameters. In pretesting, the model that minimizes the information criterion is selected.

$\varphi_m$  is most commonly in the form of the Akaike Information Criterion (AIC)(Akaike, 1974),  $\varphi_m = 2k$ , or the Bayesian Information Criterion (BIC)(Schwarz, 1978),  $\varphi_m = k \log(n)$ . The BIC has a stiffer penalty for additional parameters and thus favors less parameterized

models compared to the AIC. These AIC and BIC weights are also referred to as smoothed AIC (s-AIC) and smoothed BIC (s-BIC).

Buckland, Burnham and Augustin (1997) suggest using the AIC and BIC to construct model weights as follows:

$$\omega_m = \frac{\exp(-\frac{1}{2}I_m)}{\sum_{j=1}^M \exp(-\frac{1}{2}I_j)} \quad (1.10)$$

where  $m$  is one possible model out of a set of  $M$  possible models.<sup>5</sup>

## 1.6.2 Mallows Model Averaging

Hansen (2007) proposes model weight selection for homoskedastic linear regression models based on minimizing the Mallows criterion ( $C_p$ ). This involves selecting the vector of weights,  $W = (\omega_1, \dots, \omega_M)'$  to minimize:

$$C_p(W) = W' \bar{\epsilon}' \bar{\epsilon} W + 2\sigma^2 K' W \quad (1.11)$$

where  $K = (k_1, \dots, k_N)'$  is the  $M \times 1$  vector of the number of parameters in the  $M$  candidate models and  $\bar{\epsilon}$  is the  $n \times M$  matrix of residuals from each model such that  $\bar{\epsilon} = (\hat{\epsilon}_1, \dots, \hat{\epsilon}_M)$ . Minimization of  $C_p(W)$  can be done through quadratic programming, subject to the earlier non-negativity and summation constraints on the model weights in Equation (1.8).<sup>6</sup>

Hansen (2007) shows that the Mallows Model Averaging (MMA) is asymptotically

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<sup>5</sup>Claeskens and Hjort (2008) note that, for computational reasons, it is preferable to work with:

$$\omega_m = \frac{\exp(-\frac{1}{2}\Delta I_m)}{\sum_{j=1}^M \exp(-\frac{1}{2}\Delta I_j)}$$

where  $\Delta I_j = I_j - I_{min}$  and  $I_{min}$  is the model with the lowest information criterion score.

<sup>6</sup>See Bruce Hansen's <http://www.ssc.wisc.edu/~bhansen/progs/progs.ma.html> (accessed September 24, 2014) for code to calculate Mallows  $C_p$  and cross-validation weights.

optimal, although this is dependent on the assumption that residuals are homoskedastic. In his Monte Carlo experiment, MMA out performs s-AIC and s-BIC, although this was not in an RDD case.

### 1.6.3 Jackknife Model Averaging

Building on Hansen (2007), Hansen and Racine (2012) introduce weight selection, based on “leave one out” cross-validation, for linear regression models with unknown but bounded heteroskedastic residuals. Called Jackknife Model Averaging (JMA), Hansen and Racine (2012) show that it performs better than MMA in a Monte Carlo experiment when heteroskedasticity occurs. In JMA the vector of weights,  $W = (\omega_1, \dots, \omega_M)'$ , is chosen to minimize the following cross-validation criterion:

$$\begin{aligned}
 CV(W) &= (Y - \tilde{Y}(W))'(Y - \tilde{Y}(W)) \\
 \text{where } \tilde{Y}(W) &= \sum_{m=1}^M \omega_m \tilde{Y}_m \\
 \text{and } \tilde{Y}_m &= (\tilde{Y}_m^1, \dots, \tilde{Y}_m^n)'
 \end{aligned} \tag{1.12}$$

$\tilde{Y}_m^i$  is the predicted value of Y,  $\hat{Y}_m^i$ , computed under model  $m$  but with the  $i^{\text{th}}$  observation deleted. Minimization of  $CV(W)$  can again be done through quadratic programming, subject to the earlier non-negativity and summation constraints on the model weights.

### 1.6.4 Standard Errors

Conventional standard errors do not incorporate model uncertainty, leading to standard errors that are too small, confidence intervals that are too tight, and ultimately to increased

Type I Error. Buckland, Burnham and Augustin (1997) provide a strategy for constructing standard errors using FMA that inflates the conventional standard errors based on the amount of model uncertainty. These standard errors closely resemble those in Bayesian model averaging (Moral-Benito, 2015). These standard errors are constructed as follows:

$$SE_{BBA} = \sum_{m=1}^M \omega_m \sqrt{Var(\hat{\tau}_m) + (\hat{\tau}_m - \hat{\tau})^2} \quad (1.13)$$

Where  $Var(\hat{\tau}_m)$  is the variance estimated for each model separately using the preferred method (e.g., clustering, heteroskedastic robust standard errors), and  $\hat{\tau}_m - \hat{\tau}$  measures the deviation of each model  $m$  from the model averaging estimate of  $\hat{\tau}$ , capturing model uncertainty.

## 1.7 Monte Carlo Experiment

Although Monte Carlo analysis has been done to compare pretesting to FMA (Claeskens and Hjort, 2008) it has not been done for FMA in an RDD context. Although I expect *a priori* based on theory and earlier empirical evidence that FMA will lead to improved inference in RDD, it is more convincing to show this explicitly. I use two different data generating processes (DGP) to compare pretesting under the AIC, BIC, Mallows  $C_p$ , and “leave one out” cross-validation to their model averaging counterpoints (s-AIC, s-BIC, MMA, JMA).

The mean bias, standard deviation (SD), and root mean square error (RMSE) are

computed as:

$$\begin{aligned}
 Bias &= \bar{\hat{\tau}} - \tau = \bar{\hat{\tau}} - 1 \\
 SD &= \frac{1}{10000} \sum_{i=1}^{10000} (\hat{\tau}_i - \bar{\hat{\tau}})^2 \\
 RMSE &= \sqrt{(bias)^2 + (SD)^2} \\
 \text{where } \bar{\hat{\tau}} &= \frac{1}{10000} \sum_{i=1}^{10000} \hat{\tau}_i
 \end{aligned} \tag{1.14}$$

Coverage probability is calculated as the probability that the true value of  $\tau$ , which is set to one, lies in the constructed 90% or 95% confidence interval for  $\hat{\tau}$ . Confidence intervals for the pretest estimators are constructed in the common fashion of  $\hat{\beta} \pm 1.64\sqrt{\hat{\sigma}^2(X'X)^{-1}}$  (90%) or  $\hat{\beta} \pm 1.96\sqrt{\hat{\sigma}^2(X'X)^{-1}}$  (95%). For model averaging estimators, standard errors are constructed following Buckland, Burnham and Augustin (1997)'s procedure, which incorporates model uncertainty.

### 1.7.1 Data Generating Process 1

This DGP comes from Sun (2005), and was later used by Rau (2011). This DGP is particularly good at simulating various  $f(X)$  functions seen in RDD applications because it has a built in parameter that varies smoothness. By varying this parameter, I can see how the benefits of model averaging are mediated by the amount of curvature of the assignment variable.

This DGP involves the following simple sharp RDD:

$$\begin{aligned} Y_i &= \tau T_i + f(X_i) + \epsilon_i \\ T_i &= 1[X_i \geq 0] \end{aligned} \tag{1.15}$$

where  $\epsilon_i$  is i.i.d standard normal and  $f(X_i)$  is:

$$\begin{aligned} f(X_i) &= X_i + X_i^2 + X_i^3 + |X_i|^s \text{ if } X_i \geq 0 \\ f(X_i) &= X_i + X_i^2 + X_i^3 - |X_i|^s \text{ if } X_i < 0 \end{aligned} \tag{1.16}$$

The parameter  $s$  controls the curvature of the last portion of the DGP. Values of  $\frac{1}{2}$ ,  $\frac{3}{2}$ ,  $\frac{5}{2}$ , and  $\frac{7}{2}$  are considered, where  $\frac{1}{2}$  has the most curvature.

Since polynomial regression is more useful in situations with discrete data, I give  $X$  a binomial distribution of the form:

$$X \sim 0.1B(400, 0.5) - 200 \tag{1.17}$$

This distribution has mean zero, variance of one, and discrete  $X_i$  values at 0.1 increments. I use a sample size of 500 as in Sun (2005).



The set of models considered is:

$$\begin{aligned} Y_i &= \alpha + \tau T_i + \sum_{j=1}^p \beta_j (X_i)^j + \epsilon_i \\ T_i &= 1[X_i \geq 0] \end{aligned} \tag{1.18}$$

where  $p \leq 13$

Tables 1.1 and 1.2 shows the bias, standard deviation, root mean square error (RMSE), and coverage probabilities (90% and 95%) using a sample size of 500 with 10,000 repetitions. Bias, standard deviation, and RMSE are always higher for lower values of  $s$ , which have an  $f(X)$  function with more curvature. Similarly, coverage probabilities are always further from the intended value for lower values of  $s$ . RMSE always decreases when model averaging is used over pretesting, regardless of the curvature,  $s$ , of  $f(X)$ . The RMSE benefit from model averaging does not seem to vary with  $s$ , either in absolute differences or relative differences (percent decrease). The RMSE decrease is primarily driven by decreases in standard deviation, which is always lower under model averaging. The bias results are mixed and do not suggest a difference between pretesting and model averaging. Coverage probabilities at both the 90% and 95% are much closer to the intended level for model averaging in all cases. The coverage benefit from model averaging is larger for smaller values of  $s$ , where there is more curvature. For  $s = \frac{1}{2}$ , coverage probability is between 2.39 to 7.03 percentage points closer to the intended 95% level under model averaging, while for  $s = \frac{7}{2}$  this range is 0.19 to 1.94. s-BIC performs the best in standard deviation, RMSE, and coverage in all cases except  $s = \frac{1}{2}$ .

Figure 2.1 shows the distribution of models chosen by AIC and the average s-AIC model weights for the  $s = \frac{3}{2}$  case. The fact that there is so much variance in the model chosen by pretesting is not surprising, as this shows up in Tables 1.1 and 1.2 in the form of a larger

standard deviation. The amount of smoothing done by model averaging seems to reduce the variability in  $\hat{\tau}$  caused by noise.

## 1.7.2 Data Generating Process 2

In DGP 1, the true model did not exist within the subset of models considered. Consider the case where the true model lies within the subset of models being considered. In this case if the pretest chooses the correct model then it would perform better than model averaging. This DGP, although unrealistic since the true model is almost never known, stacks the deck against model averaging. Finding benefits for model averaging in this circumstance would provide even more support for its use.

Suppose the same sharp RDD in Equation (1.16) above is considered, with the same distribution of  $X_i$  and  $\epsilon_i$  and the same number of repetitions (10,000). I use the same sample size of 500, but I also use 5,000 to consider a case where the benefits of model averaging may be even smaller due to more data.

In this DGP,  $f(X_i)$  is generated as:

$$f(X_i) = \sum_{p=0}^H X_i^p \tag{1.19}$$

where  $H$  is randomly selected for each Monte Carlo repetition to be equal to either 0, 1, 2, 3, 4, or 5 with equal probability. The same set of models is considered as in Equation (1.19) except with the restriction that  $p \leq 7$ . This allows both pretesting and model averaging to overshoot and select a 6<sup>th</sup> or 7<sup>th</sup> degree polynomial control when neither is appropriate.

Table 1.3 shows the bias, standard deviation, RMSE, and coverage probabilities (90% and 95%) under samples sizes of 500 and 5,000. Even in this data generating process which has much less model uncertainty, and even in the high observation case ( $n = 5000$ ), model

averaging beats out pretesting on standard deviation, RMSE, and coverage probability. The decrease in RMSE from model averaging is smaller in the  $n = 5000$  case, but the percent decrease in RMSE is roughly the same for both sample sizes. Coverage improvements from model averaging are also similar for both sample sizes. Model averaging has slightly better bias results, beating pretesting three out of four times, but the bias is small and the differences are minor, suggesting again that the gains in RMSE come from reduced standard deviations. s-BIC again performs the best in standard deviation, RMSE, and coverage.

## 1.8 Application

I apply FMA to U.S. House of Representatives election data as used in Lee, Moretti and Butler (2004), Lee (2008), and Lee and Lemieux (2010).<sup>7</sup> I use this data to put my application in the context of Lee and Lemieux (2010), which also discusses model selection in RDD. This is a panel data set of U.S. House election results from 1946 to 1998. I use this data, as did the three papers above, to estimate the incumbency advantage: the electoral advantage received in the next election by an incumbent relative to a non-incumbent. A causal estimate is achieved by exploiting a regression discontinuity design: comparing parties in districts who just barely won and barely lost elections and how they fare in the next election. Here I measure the incumbency advantage as the percent increase in the two-party (Republican and Democratic) vote share.

My regression model is:

$$DemVote_{i,t+1} = \alpha + \tau T + f(DemVote_{i,t}) + \epsilon_{i,t} \quad (1.20)$$

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<sup>7</sup>The data I use comes from Lee, Moretti and Butler (2004) (see <http://eml.berkeley.edu/~moretti/data3.html> (accessed September 24, 2014)), although all three papers above use almost identical data.

where  $i$  is an electoral district and  $t$  is the year. The assignment variable here is  $DemVote$ , which is the proportion of the two-party vote that was won by the Democratic candidate. This variable ranges from 0 to 1, and a value of 0.5 or above means that the Democratic candidate won ( $T = 1$ ), otherwise the Republican candidate won ( $T = 0$ ). For an easier comparison to the results of similar regressions in Lee, Moretti and Butler (2004), Lee (2008), Lee and Lemieux (2010), I do not include controls. As in Lee (2008), standard errors are clustered at the state-district-decade level.

The set of functions for  $f(DemVote_{i,t})$  that I consider are the same as those in Equation (1.4) for a polynomial order of up to seven, and with the restriction that  $q \leq p$ . In equation form this is:

$$f(DemVote_{i,t}) = \sum_{i=1}^p \beta_i (DemVote_{i,t} - 0.5)^i + \sum_{i=1}^q \beta_{ri} T (DemVote_{i,t} - 0.5)^i \quad (1.21)$$

In this application I do not use a bandwidth restriction.<sup>8</sup>

Table 1.4 presents the estimated incumbency effect using model averaging, along with the average values of  $p$  and  $q$ . Standard errors are again clustered on state-district-decade but also incorporate model uncertainty following Buckland, Burnham and Augustin (1997)'s procedure. The estimates range from 0.0854 to 0.0119, with s-AIC, MMA, and JMA being very similar. The weighted average values of  $p$  range from 5.04 to 5.62 and for  $q$  they range from 0.57 to 2.43. Not surprisingly, s-BIC prefers fewer parameters, especially for  $q$ , as the BIC has a larger penalty for the addition of parameters.

Table 1.5 presents the estimated incumbency effect and its associated standard error (clustered on state-district-decade) for all the regression models considered, each run sepa-

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<sup>8</sup>Practitioners face the choice between using a larger bandwidth and higher polynomial order to control for  $f(X)$ , or selecting a smaller bandwidth and lower polynomial order. Lee and Lemieux (2010) shows the interplay between bandwidth and polynomial order. A recent working paper by Gelman and Imbens (2014) suggests that higher polynomial orders could be problematic, and that restricting the bandwidth and using local linear regression is a better strategy. However, the ability use a bandwidth restriction is limited when data is discrete (Lee, 2008).

rately The estimate varies significantly based on the model chosen, from about 0.07 to 0.13 (ignoring the large estimate of 0.33 for the model with no control for  $f(DemVote_{i,t})$ ). However, the treatment effect is pronounced and robust in this application and all estimates are statistically significant, unlike in many regression discontinuity designs where results are not this consistent. According to the AIC, Mallows  $C_p$ , and “leave one out” cross validation, the best model is  $p = 5$  and  $q = 2$ , which receives the largest model averaging weight of 22.1%, 66.6%, or 69.1% respectively. The BIC prefers  $p = 5$  and  $q = 0$ , with a model weight of 50.1%.

Models with the restriction that  $p = q$  do not perform well in this application. The model used in Lee (2008), for example, was  $p = 4$  and  $q = 4$  and this receives a model weight of 8.2% for AIC, and negligible weight for the BIC, Mallows  $C_p$ , and “leave one out” cross validation. Choosing  $p = q = 3$  is common in applications, and in this application it receives no weight from model averaging based for any information criteria. Similarly, models that restrict  $q$  to be zero also perform poorly with the exception of  $p = 5$  and  $q = 0$ , which forms half the weight for s-BIC, but very little weight for other criteria. Although this application does not reflect all situations, it does suggest that considering models between the restrictive  $q = p$  and  $q = 0$  extremes is worthwhile.

## 1.9 Conclusion

Model uncertainty cannot be ignored in regression discontinuity designs if researchers want their inference to be as credible as possible. A correctly specified functional form for the assignment variable is required to get an unbiased estimate of treatment effects and accurate confidence intervals. This issue is the most critical in polynomial (parametric) models, as assumptions must be made about the underlying relationship between the assignment variable and the outcome variable (and treatment status in the fuzzy RDD case). These polynomial

models are preferable over non-parametric models when the assignment variable is discrete, as consistency cannot be achieved by shrinking the bandwidth (Lee, 2008). However, there is little *a priori* evidence to suggest the ideal polynomial control out of the overwhelming number of possibilities to use. The standard model selection practices, such as pretesting or visual inspection of scatter plots, are not ideal. They result in too much Type I error as standard errors and confidence intervals are too small. Robustness checks do not necessary solve this problem.

Frequentist model averaging (FMA) addresses model uncertainty directly. It does not assume a particular data generating process and more readily deals with model uncertainty by averaging across several plausible models. FMA has significant support in the statistics and econometrics literature. Several model averaging estimators outperform pretest estimators asymptotically and in Monte Carlo experiments (Claeskens and Hjort, 2008). My Monte Carlo experiments, which compare pretesting to model averaging in RDD, show that model averaging reduces the variability of discontinuity estimates and leads to less mean square error and better coverage probability compared to pretesting. My application to estimating incumbency advantages in the US House of Representatives, as in Lee (2008) and Lee and Lemieux (2010), shows how FMA works in practice, and also shows that conventionally selected models may not be ideal. FMA is recommended for researchers using RDD whenever they use polynomial models instead of, or in addition to, non-parametric methods such as local linear regression.

Model averaging using the BIC performs the best in all of my Monte Carlo experiments. For this reason I recommend it, but I also suggest model averaging using other approaches to ensure that results are robust.

## 1.10 References

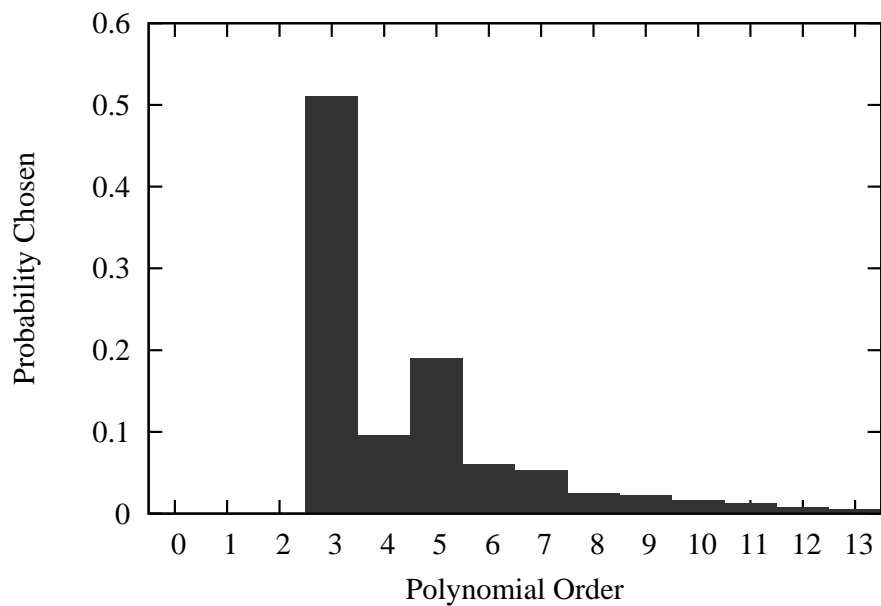
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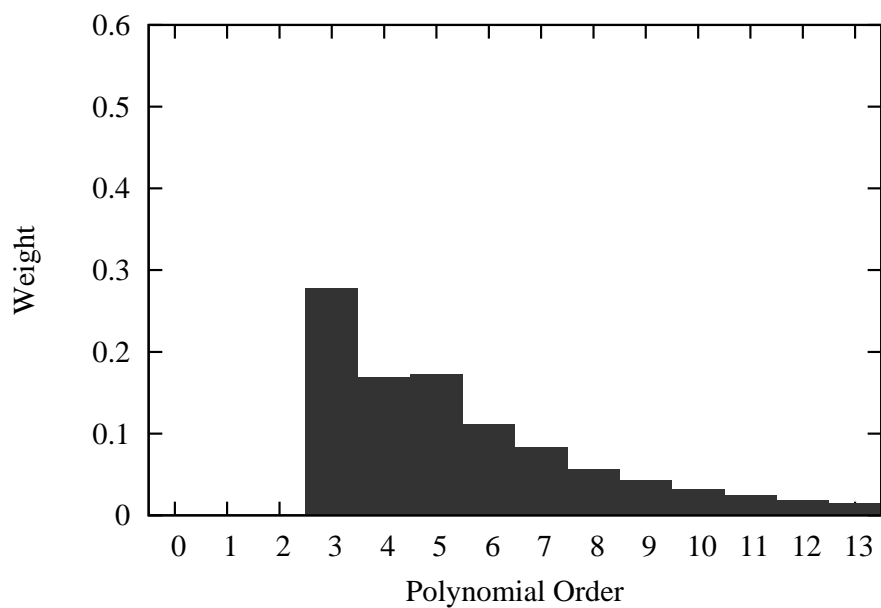


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Figure 1.1: Models and Weights Chosen for the AIC for Data Generating Process 1 with  $s = \frac{3}{2}$



(a) Model Selection Probabilities (AIC)



(b) Average Model Weights (s-AIC)

This figure presents model selection probabilities under pretesting using the AIC (a) and average model weights under  $s$ -AIC model averaging (b) for each polynomial order considered for the control function,  $f(X)$ , of the assignment variable. These are based on 10,000 repetitions of the Monte Carlo experiment with data generating process 1 with  $s = \frac{3}{2}$ .

Table 1.1: Monte Carlo Results - Data Generating Process 1 - Part (a)

	AIC	s-AIC	BIC	s-BIC	$s = \frac{1}{2}$ $C_p$	MMA	CV	JMA
Bias	-0.0117	-0.0121	-0.0100	-0.0099	-0.0060	-0.0082	-0.0055	-0.0146
Standard Deviation	0.3592	0.3472	0.2745	0.2727	0.3353	0.3086	0.3186	0.3027
Root Mean Square Error	0.3594	0.3474	0.2747	0.2729	0.3353	0.3086	0.3186	0.3027
Coverage (90%)	0.6960	0.7674	0.7222	0.7467	0.7018	0.7718	0.7130	0.7917
Coverage (95%)	0.7915	0.8618	0.8132	0.8371	0.7969	0.8630	0.8090	0.8779

	AIC	s-AIC	BIC	s-BIC	$s = \frac{3}{2}$ $C_p$	MMA	CV	JMA
Bias	0.0017	0.0015	0.0015	0.0016	-0.0033	-0.0051	-0.0036	-0.0114
Standard Deviation	0.2369	0.2264	0.1978	0.1945	0.2310	0.2110	0.2240	0.2097
Root Mean Square Error	0.2369	0.2264	0.1978	0.1945	0.2311	0.2111	0.2241	0.2100
Coverage (90%)	0.8647	0.9153	0.8808	0.8905	0.8642	0.9143	0.8685	0.9231
Coverage (95%)	0.9224	0.9608	0.9364	0.9439	0.9262	0.9596	0.9286	0.9632

Results are based on at least 10,000 repetitions. The AIC, BIC, Mallows  $C_p$ , and CV use confidence intervals constructed as  $\hat{\tau} \pm t\sqrt{\hat{\sigma}^2(X'X)^{-1}}$ , where  $t$  is the t-statistic for 90% or 95% significance. s-AIC, s-BIC, MMA, and JMA use confidence intervals constructed using Buckland, Burnham and Augustin (1997)'s procedure that incorporates model uncertainty. The variable  $s$  captures the curvature of  $f(X)$ , the underlying function of the assignment variable  $X$  (see Equation (1.16)). Larger values have less curvature. The true discontinuity value is one in all cases. Table 1.2 is a continuation of this table and represents results for  $s = \frac{5}{2}$  and  $s = \frac{7}{2}$ .

Table 1.2: Monte Carlo Results - Data Generating Process 1 - Part (b)

	AIC	s-AIC	BIC	s-BIC	$s = \frac{5}{2}$ $C_p$	MMA	CV	JMA
Bias	0.0022	0.0029	0.0031	0.0030	-0.0015	-0.0027	-0.0005	-0.0088
Standard Deviation	0.2339	0.2241	0.1958	0.1923	0.2285	0.2106	0.2227	0.2096
Root Mean Square Error	0.2339	0.2241	0.1958	0.1923	0.2285	0.2106	0.2227	0.2098
Coverage (90%)	0.8661	0.9207	0.8790	0.9041	0.8674	0.9142	0.8746	0.9220
Coverage (95%)	0.9265	0.9634	0.9330	0.9516	0.9260	0.9603	0.9304	0.9651

	AIC	s-AIC	BIC	s-BIC	$s = \frac{6}{7}$ $C_p$	MMA	CV	JMA
Bias	0.0005	0.0008	0.0023	0.0021	-0.0014	-0.0018	0.0001	-0.0050
Standard Deviation	0.2471	0.2376	0.2153	0.2091	0.2383	0.2201	0.2309	0.2174
Root Mean Square Error	0.2471	0.2376	0.2153	0.2091	0.2383	0.2201	0.2309	0.2174
Coverage (90%)	0.8684	0.9155	0.8681	0.8945	0.8733	0.9123	0.8821	0.9248
Coverage (95%)	0.9314	0.9609	0.9275	0.9469	0.9296	0.9578	0.9346	0.9635

See the notes to Table 1.1.

Table 1.3: Monte Carlo Results - Data Generation Process 2

	n = 500							
	AIC	s-AIC	BIC	s-BIC	$C_p$	MMA	CV	JMA
Bias	-0.0010	-0.0009	-0.0012	-0.0012	0.0037	0.0045	0.0034	0.0061
Standard Deviation	0.1900	0.1838	0.1675	0.1667	0.1898	0.1792	0.1890	0.1801
Root Mean Square Error	0.1900	0.1838	0.1675	0.1667	0.1898	0.1792	0.1891	0.1802
Coverage (90%)	0.8663	0.9191	0.8866	0.8980	0.8692	0.9162	0.8722	0.9248
Coverage (95%)	0.9281	0.9632	0.9399	0.9503	0.9305	0.9597	0.9328	0.9657

	n = 5000							
	AIC	s-AIC	BIC	s-BIC	$C_p$	MMA	CV	JMA
Bias	0.0000	-0.0001	0.0002	0.0001	0.0002	0.0000	0.0003	0.0003
Standard Deviation	0.0584	0.0552	0.0516	0.0514	0.0580	0.0551	0.0577	0.0551
Root Mean Square Error	0.0584	0.0552	0.0516	0.0514	0.0580	0.0551	0.0577	0.0551
Coverage (90%)	0.8727	0.9163	0.8947	0.9021	0.8735	0.9115	0.8755	0.9158
Coverage (95%)	0.9294	0.9609	0.9423	0.9486	0.9300	0.9567	0.9308	0.9608

See notes to Table 1.1. This data generating process selects a polynomial order of 0 to 5 with equal probability, reflecting a more unlikely case where the true model lies within the set of models considered. 10,000 repetitions are used and the sample size is either 500 or 5,000. The true discontinuity value is one in all cases.

Table 1.4: Incumbency Effect Estimates from Frequentist Model Averaging

	(1)	(2)	(3)	(4)
	s-AIC	s-BIC	MMA	JMA
	0.0889	0.1009	0.0894	0.0891
	(0.0090)	(0.0119)	(0.0092)	(0.0092)
$\bar{p}$	5.62	5.04	5.12	5.12
$\bar{q}$	2.43	0.57	1.59	1.64

This sample is from Lee, Moretti and Butler (2004), which uses the same sample of U.S. House election results from 1946 to 1998 as Lee (2008) and Lee and Lemieux (2010). The entire bandwidth of data (9,474 observations) is used. Standard errors, clustered on state-district-decade, are presented in parentheses. The models considered, and their weights, are presented in Table 1.5 and Equation (1.21).  $\bar{p}$  and  $\bar{q}$  are the weighted average values of  $p$  and  $q$  across all models.

Table 1.5: Estimated Incumbency Effect Estimates and Model Weights for Various Models for  $f(X)$

p	q							
	0	1	2	3	4	5	6	7
0	0.3337 (0.0048) 0/0% 0/0%							$\hat{\tau}$
						Key:	$\hat{se}$	
							$w_{s-AIC}/w_{s-BIC}$	
							$w_{MMA}/w_{JMA}$	
1	0.1163 (0.0049) 0/0%	0.1281 (0.0050) 0/0%						
	0.6/1.1%	0.2/0%						
2	0.1284 (0.0051) 0/0%	0.1254 (0.0051) 0/0%	0.0719 (0.0056) 0/0%					
	0/0%	0/0%	0/0%					
3	0.0783 (0.0051) 0/0%	0.0794 (0.0051) 0/0%	0.1090 (0.0074) 0/0.4%	0.1115 (0.0075) 0/0%				
	4.8/4.4%	0/0%	0/0%	0/0%				
4	0.0802 (0.0051) 0/0%	0.0798 (0.0052) 0/0%	0.1113 (0.0075) 0/0%	0.1070 (0.0077) 0/0%	0.0837 (0.0087) 8.2/0.1%			
	0/0%	0/0%	0/0%	0/0%	0/0%			
5	0.1012 (0.0061) 0.1/50.1%	0.1037 (0.0061) 3.2/38.0%	0.0851 (0.0083) 22.1/7.2%	0.0863 (0.0084) 10.0/0.1%	0.0886 (0.0101) 3.9/0%	0.0871 (0.0102) 1.7/0%		
	2.3/0%	0/0%	66.6/69.1%	0/0%	0/0%	0/0%		
6	0.1026 (0.0062) 0.1/0.7%	0.1004 (0.0063) 8.7/2.8%	0.0874 (0.0085) 12.0/0.1%	0.0881 (0.0086) 5.0/0%	0.0865 (0.0102) 1.9/0%	0.0829 (0.0102) 2.5/0%	0.0848 (0.0113) 0.9/0%	
	0/0%	25.4/25.4%	0/0%	0/0%	0/0%	0/0%	0/0%	
7	0.0933 (0.0067) 1.2/0.4%	0.0953 (0.0067) 9.8/0.1%	0.0890 (0.0094) 4.6/0%	0.0884 (0.0094) 1.8/0%	0.0812 (0.0111) 0.9/0%	0.0848 (0.0111) 0.9/0%	0.0823 (0.0127) 0.4/0%	0.0815 (0.0128) 0.1/0%
	0/0%	0/0%	0/0%	0/0%	0/0%	0/0%	0/0%	0/0%

See the notes to Table 1.4. Standard errors, clustered on state-district-decade, are presented in parentheses. Below the standard errors are the model weights for s-AIC and s-BIC, followed by MMA and JMA. The models considered here are described in Equation (1.21).  $p$  refers to the polynomial order for a polynomial function that spans the entire support of the data, while  $q$  refers to the polynomial order for a polynomial function that only spans the right side of the discontinuity. Bolded model weights indicate the most preferred models for AIC, MMA, JMA ( $p = 5, q = 2$ ), and BIC ( $p = 5, q = 0$ ).

## Chapter 2

# Do Tax Incentives Affect Business Location? Evidence from Motion Picture Production Incentives

### 2.1 Abstract

Incentives for motion picture production are a recent and popular economic development incentive among U.S. states. I estimate the impacts of state-level motion picture production incentives on filming location, establishments, and employment in the motion picture production industry. Filming locations are highly substitutable while locations for establishments in motion picture production are not substitutable due to agglomeration economies. This provides two very different cases to see how tax incentives affect business location. I quantify impacts on filming location, establishments, and employment using two difference-in-differences methodologies: panel regression analysis and synthetic control case studies of New Mexico and Louisiana, who adopted aggressive incentives early. For incentive data, I created a database of all state incentives from 1980 to 2012 through legal research. For filming location, I use the Internet Movie Database (IMDb.com), which provides 189,598 location choices, and for employment and establishment counts I use the Quarterly Census of Employment and Wages (QCEW). I find that most incentives have a moderate effect on



filming location but almost no effects on employment or establishments. These results show that incentives affect location decisions when locations are more substitutable, as in filming, but not otherwise. These results also imply that motion picture production incentives cannot create a local film industry.

## 2.2 Introduction

States provide many incentives to encourage firms to choose their state for business. The methods used to provide these incentives vary, but common strategies include tax credits, upfront cash grants, financing, enterprise zones, and state taxation rates in general. Reviews of the literature by Wasylenko (1999), Buss (2001), and Arauzo-Carod, Liviano-Solis and Manjon-Antolin (2010) note that the effect of incentives on location is still ambiguous. Some studies find at least moderate positive effects of incentives on location (Bartik 1985; Bartik 1989; Bartik 1991; Walker and Greenstreet 1991; Papke 1991; Strauss-Kahn and Vives 2009), while others find a small positive effect or no effect at all (Schmenner 1982; Plaut and Pluta 1983; Carlton 1983; Schmenner, Huber and Cook 1987; Blair and Premus 1987; Dabney 1991; Tannenwald 1996; Lee 2008).

Motion picture production incentives (MPPIs) are currently one of the most pervasive and aggressive types of economic development incentives. MPPIs refers to a broad set of incentives for the motion picture industry provided by governments, typically state and provincial governments, but also federal governments (e.g., Canada). These subsidies are typically tax credits or cash rebates that reduce the cost of qualifying production expenses by about 18-20% on average. The first state to adopt an MPPI of this type was Arkansas in 1983, and this peaked at 42 states, plus the District of Columbia, in July 1, 2011, with the majority of these incentives being adopted in the mid 2000s, as shown in Figure 2.1. In addition to states adopting these incentives for the first time, states with existing incentives

often amended them to make them more attractive, which is reflected in increasing subsidy rates over time, shown in Figure 2.2. The variation in MPPIs is large relative to the variation in other economic development incentives, so studying these incentives can tell us quite a bit about the impacts they have on business location decisions and economic development.

Filming location decisions differ from firm or plant location decisions because filming location is far less sensitive to the characteristics of each location. The majority of scenes can be shot anywhere. Although filmmakers often require some scenes at iconic landmarks or at city-identifying locations, filmmakers are very crafty at faking location. For example, Toronto often stands-in as New York City or Chicago (even in the film *Chicago*), and filmmakers will even build New York City subway stations facades or dress up an existing subway station<sup>1</sup> for Toronto to pass as New York. Using an establishing shot, such as that of a landmark or downtown cityscape, is also a common strategy to signal location when the scenes are filmed elsewhere.<sup>2</sup> The ability of filmmakers to fake location has only increased as technology has improved, allowing more realistic computer-generated backgrounds or imagery. Filmmakers are also less sensitive, relative to most firms, to the skills of the workforce. When filmmakers choose to film in a region without an established film industry, they typically bring their high skilled workers (e.g., principal actors, directors, and managers) with them, and hire locally for less skilled workers (e.g., camera operators, minor actors, carpentry) (Tannenwald 2010; Luther 2010).

What matters much more for filmmakers is cutting costs and it is becoming increasingly common for costs concerns to trump creative concerns in selecting filming locations (Christopherson and Rightor, 2010).<sup>3</sup> Filming location decisions are often decided by management

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<sup>1</sup>See <http://www.canadacool.com/location/toronto-secret-subway-stop/> (accessed October 14, 2014) and <http://urbantoronto.ca/news/2014/06/photo-day-disguise> (accessed October 14, 2014).

<sup>2</sup>This was used heavily by political dramas and comedies set in D.C. but that did not film there (“Scandal” in California, “House of Cards” and “Veep” in Maryland). See <http://www.gwhatchet.com/2014/02/02/from-house-of-cards-to-scandal-d-c-fights-for-more-real-screentime/> (accessed October 14, 2014).

<sup>3</sup>See also <http://www.filminglocations.com/press-releases/PressRelease.aspx?Id=25> (accessed October 14, 2014)

based on a combination of local labor costs and available tax incentives. Filmmakers are told to change their scripts or settings to fit the location, either by changing the setting in the script entirely, or just changing the scene such that the original script location seems plausible.<sup>4</sup> Independent filmmakers who seek financing are expected to have tax incentives arranged already before pursuing private financing.<sup>5</sup>

On the other hand firms in general are more concerned with the unique features of each location. Previous studies, summarized more exhaustively by Arauzo-Carod, Liviano-Solis and Manjon-Antolin (2010), have found that many city or region-specific factors matter, such as agglomeration economies (e.g., Strauss-Kahn and Vives 2009), wages (e.g., Strauss-Kahn and Vives 2009), skills or education of the labor force (e.g., Blair and Premus 1987), city population or density (e.g., Strauss-Kahn and Vives 2009), land price and availability (e.g., Papke 1991), energy costs (e.g., Plaut and Pluta 1983), building costs (e.g., Schmenner, Huber and Cook 1987), accessible markets for customers or suppliers (e.g., Crozet, Mayer and Mucchielli (2004)), union activity or labor laws (e.g., Bartik 1985), climate (e.g., Schmenner, Huber and Cook 1987), local economic conditions (e.g., Coughlin, Terza and Arromdee 1991), and public amenities such as public safety, infrastructure, or education (e.g., Fisher 1997). For motion picture production firms, they are also particularly interested in the unique skills and characteristics of the local labor force and the network of firms that provide inputs into production (Storper and Christopherson 1987; Christopherson and Storper 1989; Batt et al. 2001; Scott 2005; Neff, Wissinger and Zukin 2005; Christopherson and Rightor 2010). These factors restrict the set of locations that firms can choose. Firms often consider tax incentives in their location decisions only at the last step to help decide between a few finalists (Schmenner, Huber and Cook 1987; Blair and Premus 1987). Because locations are more substitutable for filming location than for firm location, if incentives do affect location choice then we are bound to see it in how MPPIs affect filming location.

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<sup>4</sup>See, for example, <http://online.wsj.com/articles/SB10001424052748703816204574489153078960792> (accessed October 13, 2014).

<sup>5</sup>See <http://independentfilmblog.com/why-film-investors-dont-want-you> (accessed October 13, 2014).

To quantify the impacts of MPPIs on the film industry and location choice, I compiled a database of all MPPIs from 1980 to 2012 at the U.S. state level. Not only does this data include the entire universe of these incentives, and their histories, it also includes most of the characteristics of these incentives. This allows me to estimate impacts by the type of production inputs that are subsidized (resident labor, non-resident labor, non-labor expenditure), which ends up being important.

I combine this MPPI database with two sources of data to estimate the impacts of MPPIs on filming location, and employment and establishments in motion picture production. For filming location, I use data collected from the Internet Movie Database (IMDb.com), which contains detailed information on almost three million motion picture productions, of which I match 189,598 productions to a state of filming during my sample period. For establishments and employment, I use the Quarterly Census of Employment and Wages (QCEW) provided by the Bureau of Labor Statistics. I investigate impacts specifically for motion picture production (NAICS Code 512110 / SIC Code 7812), excluding irrelevant industries, such as motion picture exhibition or distribution.

I estimate the impacts of MPPIs using two difference-in-differences regression methodologies: panel regression analysis, which pools all states to estimate average impacts, and the Abadie, Diamond and Hainmueller (2010) synthetic control, which provides case studies of New Mexico and Louisiana's incentives. Intuitively, both these statistical approaches compare states before they adopt an MPPI to after, comparing this before-and-after to the same before-and-after for similar control states that do not adopt MPPIs. This provides a much more convincing causal estimate of the impacts of MPPIs by controlling for both time-invariant state characteristics and national trends in motion picture production, both of which would bias the estimated impact of MPPIs if they were not controlled for.

These results suggest that MPPIs have a moderate to large effect on filming location. For the case study of New Mexico from 2002 and 2008, productions increased by 28.1 pro-

ductions per year on average, which is a 41.5% increase compared to the “business as usual” case. Productions increased in Louisiana as well, but I find no statistically significant effects for Louisiana.

The filming results using the entire panel of state incentives are more moderate. A one percentage point increase in the subsidy rate for workers who are state residents leads to a 1.5 percent increase in the number of filmed productions if the incentive is a cash rebate, grant, or refundable tax credit, or 1.1 percent if it is a transferable tax credit. For the average MPPI, this is a 16.1% increase in productions over five years since incentive adoption, or a 2.9% increase each year.<sup>6</sup> The subsidy rate to workers who are non-residents of the state and the subsidy rate for non-labor expenditures have no positive effects.

However, MPPIs have no effect on the location of establishments in motion picture production. While it may be surprising that the large effects on filming location do not translate into establishments, it is likely because agglomeration effects keep motion picture production establishments in major film centers such as the “30-mile zone” in greater Los Angeles. These agglomeration effects in motion picture production are well documented (Storper and Christopherson 1987; Batt et al. 2001; Lukinbeal 2004; Scott 2005; Florida, Mellander and Stolarick 2012). The benefits of large networks of skilled and diverse workers, and firms that specialize in inputs to filming, make it less beneficial to locate anywhere else. These results suggest that incentives can strongly affect location decisions of firms when locations are substitutable, but they will have no effect when unique characteristics of locations really matter, such as when the forces of agglomeration are strong.

There is only a weak connection between these incentives and employment in motion picture production. There is no employment effect on average, but there is a statistically significant employment effect in the very short term attributable to the subsidy for state

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<sup>6</sup>This is calculated over all refundable or transferable MPPIs using estimates from Table 2.6, Column (4), and multiplying them by the average values of the policy variables.

residents workers. However, this positive effect is almost exactly offset by the negative effects of the subsidy for non-labor inputs. This suggesting a substitution effect, where employment can only increase when it is subsidized more than the alternative of non-labor inputs. The lack of employment effects for the majority of MPPIs could be explained by the fact that local hires for filming are often temporary and less skilled jobs (Tannenwald 2010; Luther 2010), however all of these jobs are counted in my data so it is surprising that I do not find an effect on these. I do find employment effects in the case study of New Mexico (870 jobs on average per year), but not for Louisiana. However, since my data includes jobs of individuals brought in temporarily from out of state, it is unclear what portion of these jobs created were for locals. It is also unclear what proportion are part-time versus full-time, although most are temporary.

These results also tell us that while MPPIs strongly influence where films are shot, they are not effective at creating a local film industry. The employment effects are non-existent, small, or short-term and there is no effect on establishments in motion picture production.

## 2.3 Data

To quantify the impacts of MPPIs on productions, employment, and establishments, I use three sources of data. First is a unique panel database I compiled of MPPIs, second is Internet Movie Database (IMDb.com) data on filming location, and third is Quarterly Census of Employment and Wages (QCEW) data on employment and establishments in motion picture production.

## 2.3.1 Motion Picture Production Incentives Database

There are different types of MPPIs. The most common type, which is the focus of this paper, gives a set percentage of a motion picture production’s “qualified expenditure” back to the production company in either tax credits or as a cash rebate. The other two types are sales and use tax exemptions or rebates and tax credits for investment in a motion picture production facility or capital program. From here forward, I use the term MPPIs to refer exclusively to the type I analyze, the tax credits and cash rebates for qualified expenditure on motion picture production. I focus on these because they are the most common and the strongest incentives.

Appendix 2 contains tables describing all major aspects of the MPPIs available from January 1, 1980 to September 1, 2013<sup>7</sup> in all 50 states and the District of Columbia. This database was compiled primarily by locating the relevant law, via statutes in WestLaw, and confirming changes in legislation over time using notes provided by WestLaw and by locating the actual acts passed, through HeinOnline, that amended this law. In rare cases, supplementary sources such as government websites or consulting firm websites<sup>8</sup> were used to confirm details that were not codified explicitly in law. In the vast majority of cases my research matches those sources.

### 2.3.1.1 Variation in MPPI Characteristics

There is a large amount of variation in MPPIs over several different dimensions. The most important dimension is what is considered “qualified expenditure”: what spending can be claimed for an incentive and can receive a subsidy. In addition to what is considered qualified expenditure, different categories of qualified expenditure (the payroll of state residents,

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<sup>7</sup>Although I started my search at 1980, there does not appear to be any MPPIs before 1980.

<sup>8</sup>See, for example, <http://www.entertainmentpartners.com/incentives> (accessed April 15, 2014).

the payroll of non-residents, and non-labor expenditure) receive different subsidies.<sup>9</sup> Some MPPIs are cash rebates or grants, while others are tax credits. These tax credits can be either refundable, transferable, or be carried forward and applied to future tax liability, or some combination of all three. Finally, MPPIs differ in their rules and restrictions, such as the maximum payout or the minimum expenditure required in order to qualify. My analysis incorporates the most important aspects of MPPIs: their subsidy rates (for the payroll of state residents, non-residents, and non-labor expenditure) and if the incentive is refundable or transferable.

Table 2.1 presents summary statistics for the MPPI database. About one fifth of all state-year observations from 1980 to 2012 are with active MPPIs. About two thirds of MPPI observations are for MPPIs that are what I call “Refundable”: cash rebates, grants, and refundable tax credits, while nearly one quarter (23%) are tax credits that are transferable only, and the rest are tax credits that are neither refundable nor transferable (about 6%). All MPPIs subsidize the wages or salaries of workers who are state residents<sup>10</sup>, and all but seven states, for some period of time, subsidize non-labor expenditure (95% of observations). Almost two third (64%) of state-year observations with MPPIs subsidize non-resident labor. Average subsidy rates are between 18% and 20% and these have increased over time (Figure 2.2).

### **Categories of Qualified Expenditure and their Rates**

There are three categories of expenditure that can be subsidized by MPPIs: the payroll of state residents, the payroll of non-residents, and non-labor expenditures. Non-labor expenditure includes a broad list of spending on inputs into motion picture productions such as set construction, wardrobe, photography, sound, lighting, rental fees, transportation, cater-

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<sup>9</sup>On top of these different subsidy rates, there are also bonus rates, which can be awarded for productions of a certain budget or for productions that locate in areas outside a major city.

<sup>10</sup>Except the District of Columbia’s incentive did not subsidize above-the-line workers who were district residents.



ing, and lodging. Advertising and distribution are not included. States usually have very similar definitions of non-labor expenditure if it is included in qualified expenditure (and it almost always is, 95% of the time, as Table 2.1 shows.). On the other hand, the treatment of payroll tends to differ most widely. 23 states, as of September 1, 2013, have the same rates for the payroll of residents and non-residents, ten give residents a better rate, and nine exclude non-residents entirely.

### **Refundability**

In some cases, tax credits, cash rebates, and grants are equivalent, each leading to the same dollar reduction in production costs. However, if a motion picture production receives tax credits in excess of its tax liabilities, which is very common, then tax credits could differ from cash rebates and grants depending on if the tax credits are refundable or transferable. If a tax credit is refundable, it can be sold back to the state, though this is sometimes at a discounted rate. A similar but weaker feature is if the credit is transferable, so that it can be sold to other firms with tax liabilities to the state through intermediary brokers. These brokers typically take a cut of 20 to 30% of the credit (Luther 2010; Christopherson and Rightor 2010) and the buyer of the credit is typically a large corporation with little to no connection to the film industry (Tannenwald 2010; Christopherson and Rightor 2010). So refundable and transferable MPPIs should be identical, except for this cut. In my analysis use a variable called *Refund* which is equal to the refundability rate if the MPPI is a refundable tax credit (this rate is typically 100%, but occasionally somewhat less, like 90%), and equal to 0.75 if the MPPIs a transferable tax credit, to reflect the average cut taken when the transferable credit is sold, and zero for tax credits that are neither refundable nor transferable.<sup>11</sup>

The last remaining option if the excess tax credit cannot be refunded or transferred is

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<sup>11</sup>My results are robust to assuming  $Refund = 0.7$  or  $Refund = 0.8$  for transferable tax credits. These results are available from the author upon request.

to carry forward excess tax credits to offset tax liability in future years. Almost all MPPIs over time have allowed this. Carrying the tax credit forward is only useful for production companies that expect to film again in the state in the future, so companies that do not expect to return cannot pocket any tax credits that exceed their tax liabilities. I code these MPPIs using a variable called *Neither* and control for them in my analysis.

### **2.3.1.2 Variation over Time in MPPIs**

In addition to the large amount of variation across states in the characteristics or strengths of their MPPIs, there is a significant amount of variation in these incentives over time. Figure 2.1 shows the number of states with MPPIs over time. The first state to create an incentive was Arkansas in 1983, but this was only a mild cash rebate of 5%. States gradually started adding MPPIs on an annual basis until a peak of 43 states with MPPIs in July 2011.

In addition to the growth in the number of MPPIs, there has also been growth in how aggressive they are. Figure 2.2 shows the median subsidy rates for each type of expenditure (resident labor, non-resident labor, non-labor expenditure). Over time, the general trend was for all three rates to increase. Rates for non-resident labor are typically lowest, driven mostly by the nine states that do not include it as qualified expenditure in their MPPIs.

### **2.3.1.3 Variation across States in MPPIs**

Figure 2.3 presents a map of states with active MPPIs during two time periods: January 1, 2003 (top figure), which is before the majority of states adopted MPPIs, and September 1, 2013 (bottom figure), the end of my sample period. As the figure shows, most states have adopted an MPPI at some point during this time period. The exceptions include Delaware, Nebraska, Nevada, New Hampshire, North Dakota, and South Dakota. There are also four

states (Arizona, Iowa, Kansas, and Vermont) that once had an MPPI, but have since removed it, and they no longer have one as of September 1, 2013. MPPIs are popular in all types of states, but it seems as though they are less popular in smaller states, or states that already have lower taxation rates (e.g., Nevada, New Hampshire).

### 2.3.2 The Internet Movie Database (IMDb.com)

The Internet Movie Database (IMDb) at IMDb.com is an online database with information on motion picture productions. IMDb includes information of 2,813,543 titles.<sup>12</sup> Similar to Wikipedia, IMDb users can browse and update information on a wide variety of motion pictures. IMDb is incredibly popular; it is the 44th most popular website globally, or 28th most popular in the U.S., according to its Alexa rank.<sup>13</sup>

I use text-based data files provided by IMDb<sup>14</sup> to extract a sample of the IMDb motion picture productions that include all productions with a release date from 1981 to 2013 that list a filming location in a U.S. state. This sample includes 189,598 productions. I use the release year to estimate the filming year, by assuming the filming year was one year before the release year.<sup>15</sup>

Table 2.2 presents summary statistics for the IMDb data. On average there are 95 productions filming in each state and year, but this varies significantly across states and time. As expected, much of these productions are shot in typical film states, such as California and New York. Figure 2.4 presents the number of productions by release year. Much of the productions are more recent, likely because of the increase in popularity of IMDb over time,

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<sup>12</sup>See <http://www.imdb.com/stats> (accessed March 15, 2014).

<sup>13</sup>This rank is as of March 15, 2014. See <http://www.alexa.com/siteinfo/imdb.com/> (accessed March 15, 2014). Alexa ranks websites based on a combination of page view and unique site users.

<sup>14</sup>See <http://www.imdb.com/interfaces> (website accessed, and data extracted on January 17, 2014).

<sup>15</sup>The results, available from the author, are similar for an assumption of two years. I use the release year as an estimate of the production year because the exact production date is listed for fewer than one quarter of the productions listed on IMDb.

but also because the number of motion picture productions is also increasing over time.

Since IMDb's data comes from users, whether they be production companies or fans, there is the question of how reliable this data is for estimating the impacts on filming location. The fact that some productions are missing will not lead to bias in my estimated impacts so long as having more productions associated with a state on IMDb is not related to MPPIs, except through the effect of MPPIs on filming location. Suppose for example, that my analysis shows that MPPIs are associated with a 5% increase in filming. This could be driven by some combination of an actual effect, more filming in states with an MPPI, or a spurious effect, where there are more productions listed as filming in states with MPPIs after their adoption, due only to more people adding productions to IMDb. Although this spurious effect seems rare, I perform a test for this using Google Trends data on Google searches for IMDb, to see if searching for IMDb.com (which should be related to editing IMDb) at the state level is associated with MPPIs. I find no relationship between searching for IMDb.com and MPPIs. This is discussed further in an appendix available from the author. Although this test does not rule out this possible source of bias, I consider it very unlikely to have occurred.

### **2.3.3 QCEW Employment and Establishment Data**

The Quarterly Census of Employment and Workers (QCEW), collected by the Bureau of Labor Statistics, provides data on employment and establishment<sup>16</sup> counts that are specific to the motion picture production industry. I use employment estimates only for individuals employed at a private business establishment.

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<sup>16</sup>The BLS defines an establishment, relative to a firm or company, as “. . . a single economic unit, such as a farm, a mine, a factory, or a store, that produces goods or services. Establishments are typically at one physical location and engaged in one, or predominantly one, type of economic activity for which a single industrial classification may be applied. A firm, or a company, is a business and may consist of one or more establishments, where each establishment may participate in different predominant economic activity.” See <http://www.bls.gov/cew/cewfaq.htm> (accessed May 22, 2014).

Individuals appear in the QCEW employment estimates if they are covered under the unemployment insurance system, which covers 99.7% of all employment. Although some workers hired to work on a motion picture production may be seasonal, temporary, or contract workers, including performers, these groups are almost always covered under the unemployment insurance system, and thus appear in QCEW employment estimates.

The QCEW data reports employment and establishment counts at different levels of industry specificity, based on the six digit North American Industry Classification Code (NAICS) system and the four digit Standard Industry Classification (SIC) system. The most specific data on motion picture production are at the six digit NAICS level (512110) and four digit SIC level (7812), which are entitled “motion picture and video production”. This data does not include motion picture distribution or exhibition, or sound recording, all of which are included in less specific industry levels. I use SIC data (7812) from 1988 to 2000, and NAICS data (512110) from 2001 to 2012. The SIC 7812 and NAICS 512110 data match perfectly, although this is not true of every industry.<sup>17</sup>

Because filming is mobile and contract based, some workers may relocate. In determining the state to attribute this employment to, the BLS applies four tests, in order.<sup>18</sup> The first test, called the localization test, considers the employment services as allocated to a state “. . . if they are performed both in state and out of the state but those performed out of the state are incidental to the services performed within the state.” If the localization test does not determine the state to attribute the employment to then the next two tests allocate the employment by the place of direction and control of the employee or the employer. The implications of this method of allocating employment, for workers that relocate, is that employment estimates from this data include some temporary jobs of non-residents in addition to the jobs of residents, although it is not clear exactly how many non-residents appear in

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<sup>17</sup>See <http://www.census.gov/epcd/ec97brdg/E97B1512.HTM> (accessed May 22, 2014).

<sup>18</sup>See <https://labor.ny.gov/formsdocs/ui/ia116.3.pdf> (accessed May 22, 2014), which describes this process.

the estimates.

Christopherson and Rightor (2010) suggest that this employment data is inappropriate because "... [this] point-in-time data cannot give a reliable indication of full-time, full-year employment." (p. 343) This is a valid criticism. For example, suppose I estimate that a particular MPPI creates 100 jobs. This could be either 100 full-year jobs or 400 jobs lasting an average of three months each. Given that filming typically lasts only a few months, and rarely longer than a year, many of the jobs that would be created would not be full-year. Full-year jobs are more associated with established motion picture production firms and are a better indication of an established film industry. What is more of a problem is that the QCEW data does not allow me to determine if the jobs are full-time or part-time, so any employment effects that I estimate are a mixture of full-time and part-time jobs. Since I do not find employment effects in most cases, this issue is less important.

Table 2.2 presents summary statistics for employment and establishment estimates from 1988 to 2012. On average, there are 3,387 employees and 284 establishments per state and year, although this varies widely from 10 to 122,773 employees, or 3 to 6,313 establishments. The small number of employees relative to establishments suggests that most establishments have fewer than a dozen employees. This table also presents summary statistics for the sales tax controls (from the Tax Foundation<sup>19</sup>) and the corporate tax controls, from Wilson (2009) (for 1980 to 2006) and Moretti and Wilson (2014) (for 2007 to 2012).

## 2.4 Synthetic Control Case Study Methodology

I conduct case studies of New Mexico and Louisiana's incentives from 2002 to 2008 using the Abadie, Diamond and Hainmueller (2010) synthetic control for New Mexico and

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<sup>19</sup>See <http://taxfoundation.org/article/state-sales-gasoline-cigarette-and-alcohol-tax-rates> (accessed April 15, 2014)

Louisiana. These states both adopted MPPIs early, in 2002, and their incentives have been widely discussed as alleged “success stories”.<sup>20</sup>

The synthetic control methodology compares the treated state (New Mexico or Louisiana) with a synthetic version of it, made up of a weighted combination of states without an MPPI. The weights are chosen so that the synthetic control and treated state best match in all three outcome variables (productions, employment, or establishments) in the period before the MPPI is adopted (1988 to 2001).<sup>21</sup> More specifically, the weights minimize the mean squared prediction error (MSPE) in the pre-treatment period between the actual and synthetic states in all variables used to match. The synthetic control state can be compared to the actual state in the period after the incentive takes effect to get an estimate of its effect over time, net of a “business as usual” trend that is reflected in the synthetic control state.

States are only eligible to be used as control states if they have not had an MPPI during the entire sample period. There is a trade-off between how many years post-incentive adoption I can analyze, and how large the set of eligible control states is, as many states adopted MPPIs in the late 2000s and cannot be used as a control state if the analysis is extended longer. More control states to choose from can improve the synthetic control. I chose to end the analysis at 2008, which gives seven years of results. Ending in 2008 provides 11 control states (AL, DE, KY, NE, NV, NH, ND, OH, SD, UT, and VA). While California could also be used as a control state, I exclude it because it is so unique. Ending in 2012, however, would only provide six control states (DE, NE, NV, NH, ND, SD).

Some QCEW employment and establishment counts are missing for certain years, typically in small states where they are suppressed for protect confidentiality. For the purposes of creating Figures 2.5 to 2.7, these missing years imputed by taking the average value of

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<sup>20</sup>See, for example, Ernst & Young (2009) for discussion of New Mexico and <http://usatoday30.usatoday.com/life/movies/news/2010-08-03-louisianafilm03'CV'N.htm> (accessed October 13, 2014) for discussion of Louisiana (nicknamed “Hollywood South” by some).

<sup>21</sup>In also include the corporate tax rate as a matching variable to control for these rates and their changes. Since sales tax rate data is only available starting in 2000, this variable cannot be included.

the following and preceding years. These synthetic control figures do not normally include confidence intervals. Since a typical imputation method would only help with creating more accurate confidence intervals, there is no added benefit to imputation for these figures. Where imputation could be important is in calculating the mean squared prediction error ratios as in Table 2.4. Imputation procedure in this case has not been established but I am working on a strategy. So this is forthcoming. Note that the IMDb.com productions data is not missing in this manner, so no imputation is necessary.

### **2.4.1 MPPIs in New Mexico and Louisiana, 1980 - 2008**

Louisiana adopted a tax credit of 10% for all production spending, effective July 1, 2002. A base investment of \$300,000 was required to be eligible, and an investment of at least \$1.5m increased the tax credit rate to 15%. The requirement for this bonus 5% was increased to at least \$8m starting on January 1, 2004. This tax credit was initially neither refundable nor transferable but transferability was added effective July 1, 2003. Effective January 1, 2006, the tax credit was increased to 35% for resident labor, and 25% for non-resident labor and non-labor expenditure, and the bonus 5% rate was removed.

New Mexico adopted a 15% refundable tax credit for resident labor and non-labor expenditure (but not non-resident labor) effective January 1, 2002. No minimum expenditure was required for eligibility. Effective July 1, 2003, non-resident labor became eligible for the 15% subsidy. Effective January 1, 2005, a bonus 5% subsidy was available for resident labor and non-labor expenditure if the production was a TV series with at least 60% of the below-the-line crew payroll paid to New Mexico residents. Effective January 1, 2006, the subsidy rates to resident labor and non-labor expenditure were increased to 25%, and the non-resident labor subsidy was removed.

Since the incentives programs in New Mexico and Louisiana were not constant for the



entire period analyzed (2002 to 2008), it is not possible to determine if estimated impacts are due to delayed impacts of the original incentive packages or the amendments to those packages, particularly the 2006 amendments that increased subsidy rates significantly in both states.

## 2.5 Synthetic Control Case Study Results

### 2.5.1 Productions

Table 2.3 presents how each control state is weighted to create each synthetic control, for both treated states (New Mexico and Louisiana), and for each outcome variable. New Mexico's synthetic control for production is made up of 45.7% Nevada, 25.3% Delaware, 21.1% Ohio, and 8.0% Utah. Louisiana's is 53.1% Kentucky, 19.9% Nevada, 17.5% Delaware, and 9.4% Ohio. These weights are very similar for employment and establishments.

Figure 2.5 presents the time series of the number of productions for New Mexico and Louisiana, relative to their synthetic control states. The figure suggests large increases in productions in both states starting in about 2005 and peaking in 2007. This is a jump from a pre-treatment period average of 14.9 productions per year in New Mexico and 28.6 productions per year in Louisiana, to a post-treatment period average of 95.7 and 111.4 productions per year, respectively. This is a similar average increase for New Mexico (80.9) as for Louisiana (82.8) but the percent increase is much larger for New Mexico.

However, this uncontrolled increase is not causal. As the time series for the synthetic controls show, filming would have been rising in both states without any interventions, but not to the same degree for New Mexico. For Louisiana there does not appear to be any effect on productions relative to the synthetic control. In fact, the average productions

in the post-treatment period is actually slightly below the synthetic control (111.4 versus 115.0). Relative to the synthetic control, New Mexico gained an average of 28.1 productions per year, rather than 80.9 productions, representing a still sizable 41.5% average increase in productions relative to the “business as usual” case represented by the synthetic control in the post-treatment period.

## 2.5.2 Employment

Figure 2.6 presents the time series of employment in motion picture production for New Mexico and Louisiana. Both states have a similar trajectory of employment before and after MPPIs compared to for productions. Employment starts increasing rapidly starting in about 2005 for both states, which matches the increase in productions. The average increase in employment, net of the synthetic control is 870 jobs in New Mexico and 930 jobs in Louisiana. For New Mexico this is a massive 217% average increase relative to the “business as usual” case shown by the synthetic control, but only a 120% increase for Louisiana. But note that the synthetic control fits New Mexico better than Louisiana in the pre-treatment period, suggesting that it is more likely for New Mexico than for Louisiana that the results are causal. This is discussed in greater detail in Section 6.4.

This is a large increase in employment relative to the pre-treatment period for both states. However, two problems with the QCEW employment data suggest that the employment increase is less dramatic. First, these employment counts include those relocated from out-of-state to work on productions. Thus not all jobs are created for locals. Second, the QCEW employment counts do not distinguish between full and part-time jobs, suggesting that some unclear proportion of these jobs are part-time.

### 2.5.3 Establishments

Figure 2.6 presents the time series for establishments. The results here are much different compared to productions and employment. Both states show an increase in establishments from 1988 to about 1998, a decrease from about 1998 to 2004, and then a slightly more rapid increase from 2004 to 2008. The increase in 2004 matches the increase in productions and employment, which also started to rapidly increase after about 2004. Net of the synthetic control, the average increase in establishments is 15.9 in New Mexico, but a decrease of 3.8 in Louisiana.

### 2.5.4 Inference

The Abadie, Diamond and Hainmueller (2010) synthetic control does not provide for inference through typical standard errors or confidence intervals, but it does provide an alternative method of inference. Inference is conducted through placebo tests where the control states are deemed “treated” and their time series plots, with their synthetic control counterparts, are compared to those in Figures 2.5 to 2.7. Then, for each of these placebo tests, a ratio of the post-“treatment” to pre-“treatment” mean squared prediction error is calculated as follows:

$$MSPERatio_i = \frac{MSPE_{Post - Treatment}}{MSPE_{Pre - Treatment}} = \frac{\frac{1}{7} \sum_{t=2002}^{2008} (ActualY_{it} - SyntheticY_{it})^2}{\frac{1}{14} \sum_{t=1988}^{2001} (ActualY_{it} - SyntheticY_{it})^2} \quad (2.1)$$

These ratios can then be compared to the same ratios for the actual treated states, New Mexico and Louisiana. These ratios indicate how well the synthetic control matches

the actual state in the post-treatment period relative to in the pre-treatment period. Large ratios for New Mexico and Louisiana suggest results that are more causal. Large ratios can occur in two ways: either there is a large treatment effect so the MSPE Post-Treatment (numerator) is high, or there is a good fit between the actual and synthetic state in the pre-treatment period so the MSPE Pre-Treatment (denominator) is low. If control states have a similarly high ratio to the treated states then this suggests that the results for the treated states may have been spurious.

Figure 2.8 presents a series of placebo tests for the production results in Figure 2.5. Each series presented here is the gap between the actual state and its synthetic control, divided by the average in the actual state during the pre-treatment period (1988 to 2001), and then multiplied by 100 to create a percentage. Positive (negative) values indicate that the actual state's production levels were higher (lower) relative to the synthetic control. Dividing the gap by the average number of productions in the pre-treatment period allows the interpretation to be the percentage increase (or decrease) in productions in that year relative to the average in the pre-treatment period, net of what is predicted by the synthetic control. Presenting each series in this manner also allows states to be more easily compared, since absolute gap would be larger for larger states than for smaller states, independent of treatment effects or the fit of the synthetic control. The darker and thicker lines show the same series for New Mexico and Louisiana from Figure 2.5, but recalculated as this relative gap. The gray lines represent the control states (AL, DE, KY, NE, NH, OH, SD, UT, and VA). The control states of Nevada and North Dakota are excluded from this figure because they had the highest and lowest (respectively) filming levels out of all the control states, which means that no combination of control states could adequately match them.

Figure 2.8 suggests that the increase in productions in New Mexico exceeded many of the control states, while Louisiana blends into the pack. New Mexico's gap of about 800% in 2007 stands out as a clear outlier, suggesting that there is likely a causal effect here, but

this can be confirmed more formally by comparing MSPE ratios, discussed below.

Figure 2.9 presents a similar figure, but for employment instead of productions. Compared to Figure 2.8, Nevada is instead included here, but Utah is removed as it had the largest average employment for a control group in the pre-treatment period. New Mexico and Louisiana perform relatively better here. Both states dominate the control states starting in 2005, although only New Mexico's employment increase stands out. Nevada's gap is the highest for a control state and closely follows Louisiana from 2005 onward. The similarity between Nevada and Louisiana suggests that the increase in employment in Louisiana may also have just been due to a faster rate of increase in employment that is independent of incentives.

Figure 2.10 presents the figure for establishments. Compared to Figure 2.8, Nevada is included but Ohio is excluded instead. Both states perform poorly here, with Nevada and Utah showing somewhat larger increases in establishments. This suggests a lack of statistical significance.

Table 2.4 presents the more formal way to conduct inference. It presents the ratio of the post-treatment period mean squared prediction error to the pre-treatment period mean squared prediction error. This table includes the two control states left out of the earlier placebo figures (Figures 2.8, 2.9, and 2.10).<sup>22</sup> New Mexico performs well for productions and employment, with ratios that exceed the 11 control states. This is partly because New Mexico has larger estimated impacts compared to the control states, but also because the synthetic New Mexico fits the actual New Mexico well. However, New Mexico only has the third largest ratio for establishments. In contrast, Louisiana's synthetic control fits much worse in the pre-treatment period. This difference of fit is apparent in the earlier figures, which show the synthetic control tracking New Mexico (and many other control states) closer than for

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<sup>22</sup>Since no states can create a proper synthetic control for these states, both the pre- and post-treatment period MSPEs are equally elevated, but this problem does not adversely affect the ratio of the two (Abadie, Diamond and Hainmueller, 2010).

Louisiana. Considering the ratios for the 11 control states, the probability of estimating a ratio for productions or employment at least as large as that of New Mexico, under a random permutation of the intervention is  $1/12 = 0.083$ , suggesting statistical significance at the 10% level (Abadie, Diamond and Hainmueller, 2010). However, for establishments, New Mexico's rank of third does not provide statistical significance ( $4/12 = 0.333$ ). For Louisiana, none of the estimates are statistically significant.

However, there are another 31 control states that adopted an MPPI partway through the treatment period of 2002 and 2008. It is possible to calculate the post-treatment MSPE for these states as well, but with a few years missing.<sup>23</sup> These ratios are presented in Appendix 1 Table 2.14 and the associated figures are presented in Appendix 1 Figures 2.11, 2.12, and 2.13.

Comparing the ratios for New Mexico and Louisiana to the original 11 control states and these additional 31 states shows that New Mexico's ratios are always still the largest for productions and employment, which suggests p-values of  $1/43 = 0.023$ . New Mexico's ratio for establishments drops from third to fourth, but with the additional 31 states this provides a p-value of  $4/43 = 0.093$ , providing statistical significance at the 10% level. Louisiana's ratios for productions and establishments continue to rank too low to guarantee statistical significance. Louisiana's ratio for employment drops from second to third, giving a p-value of 0.070 and providing statistical significance at the 10% level. Drawing evidence from both sets of p-values, there is evidence at either the 10% or 5% level for increased productions and employment in New Mexico following its incentive, but very weak evidence (at the 10%

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<sup>23</sup>It is unclear if using these additional 31 controls states to calculate p-values is ideal. Having years missing from these control states will affect the MSPE ratio calculations in two ways. First, it will increase sampling variation and likely increase the post-treatment MSPE since there are fewer years to average over to smooth out any noisy jumps in MSPE that could occur in any given year. Second, it decreases MSPE because the longer the post-treatment period, the larger the post-treatment MSPE is because the actual and synthetic state tend to drift apart. It is unclear which of these two effects dominate and there is no literature that guides this decision. I present the p-value results with the additional 31 control states anyways, but these should not be viewed as necessarily more accurate than the p-values using only the 11 control states that exist for the entire sample.

level only for the second set of p-values) for increased establishments. For Louisiana, the only evidence of an effect is weak evidence (10% level for the second set of p-values only) of an employment increase.

There are a three reasons why the impacts of New Mexico's MPPI were larger and more statistically significant than for Louisiana. First, it was relatively easy to create a synthetic control for New Mexico, such that New Mexico had relatively low pre-treatment period MSPEs. This was the opposite case for Louisiana and partly explains why New Mexico's MSPE ratios were larger than Louisiana's, even if the treatment effects were similar, such as for employment.

Second, Louisiana's MPPI was likely weaker from 2002 to 2006 relative to New Mexico. Louisiana's incentive wasn't made transferable or refundable until transferability was added effective July 1, 2003, whereas New Mexico's incentive was always a refundable tax credit. So New Mexico's program was clearly dominant from 2002 to July 2003. From July 2003 to the 2005, New Mexico's subsidy rates were weakly dominant. Both states had the same rates for large budget productions but Louisiana's subsidy rates were 10%, rather than 15%, for all other productions. Also, throughout the treatment period Louisiana required a minimum budget of \$300,000 to receive the incentive, while New Mexico had no budget requirement. This likely has the most implications for the number of filmed productions. Conversely, Louisiana's MPPI was more generous than New Mexico's from 2006 to 2008, although Louisiana still had a minimum budget requirement. Louisiana's program appears to have been weaker relative to New Mexico during approximately the same period that estimated effects for productions and establishments are actually negative (2002 to 2005).

Third, Louisiana's treatment effects could have been larger in the absence of Hurricane Katrina, a strong Category 3 hurricane which hit Louisiana on August 29th, 2005. This was the costliest and one of the deadliest hurricanes to ever hit the United States.<sup>24</sup> While

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<sup>24</sup>See <http://www.nhc.noaa.gov/pdf/TCR-AL122005%20Katrina.pdf> (accessed February 13, 2015)

it is difficult to determine how many productions, jobs, or businesses were displaced or affected, Figures 2.5 (Productions) and 2.7 (Establishments) do not show any decreases post Katrina. Figure 2.6 shows a small decrease in employment (96 jobs) between 2005 and 2006. Louisiana actually has a similar time series of productions, employment, and establishments as New Mexico, but the difference for Louisiana is that there is no treatment effect net of the synthetic control for productions. So the question is actually if these variables would have grown at a faster rate for Louisiana in the absence of Hurricane Katrina, which is difficult to know.

In summary, it appears that at least for the case study of New Mexico, its MPPI is linked to a large increase in productions and employment in the motion picture production industry, but this did not translate into the creation of business establishments.

## 2.6 Panel Regression Methodology

The IMDb.com data, which provides the number of productions by state and year, is count data. There are zero productions filmed some states (typically small ones) and years, according to the IMDb. Given this, a log-linear model is not feasible, so a Poisson regression model is best suited to this data.<sup>25</sup> making this condition irrelevant.

I consider four different regression models. The first two estimate average effects, first by grouping almost all MPPIs together (Equation 2.2) and then by controlling for the major differences among MPPIs: the refundability rate and the subsidy rates for workers who are state residents, non-residents, and for non-labor expenditure (Equation 2.3). Controlling for these rates is important since they vary significantly among states, and states do not always

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<sup>25</sup>Although the Poisson regression model requires the assumption that the mean and variance of  $Y$  are equal,  $Var(Y_{it}) = E(Y_{it})$ , this condition only affects the standard error estimates. However, this assumption is not required when estimating clustered standard errors. See <http://blog.stata.com/tag/log-linear-regression/> (accessed May 22, 2014) and Wooldridge (2010) and Cameron and Trivedi (2009).



subsidize each category of expenditure at the same rate. The first regression is as follows:

$$\begin{aligned}
 Productions_{it} = \exp(\alpha + \beta_1 Refundable\ or\ Transferable_{it} \\
 + \beta_2 Neither_{it} + X_{it}\Phi + \delta_i\varphi + \mu_t\tau + \epsilon_{it}) \quad (2.2)
 \end{aligned}$$

$Productions_{it}$  is the number of productions filmed in state  $i$  at time  $t$ , where  $t$  is annual from 1980 to 2012.  $Refundable\ or\ Transferable_{it}$  is an indicator variable for if the MPPI is a cash rebate, grant, refundable tax credit or transferable tax credit.  $Neither_{it}$  is an indicator variable covering the remaining MPPIs, which are neither refundable nor transferable.

$X_{it}$  is a set of covariates, which includes state taxation rates. These state taxation rates include sales taxes (2000 to 2012) from the Tax Foundation<sup>26</sup> and the effective top corporate tax rate (1980 to 2012) from Wilson (2009) (for 1980 to 2006) and Moretti and Wilson (2014) (for 2007 to 2012)<sup>27</sup>.

I choose not to include state-specific linear time trends as controls because they could cause bias. Meer and West (2013) present Monte Carlo evidence that confirms that if treatment effects appear in growth rates instead of a level shift, then state-specific linear time trends will bias estimates towards zero. I do present regressions where state-specific linear time trends are used in Appendix 1 (see Tables 2.15 to 2.20), but I do not discuss these results except here to mention that the results are similar with state-specific linear time trends, just somewhat weaker.

$\delta_i$  are state fixed effects which control for time-invariant state characteristics and average production levels by state. For example, without state fixed effects, larger states are

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<sup>26</sup>See <http://taxfoundation.org/article/state-sales-gasoline-cigarette-and-alcohol-tax-rates> (accessed April 15, 2014). Since my regressions include state fixed effects, missing state sales tax data from before 2000 implies that any changes in sales tax rates from before 2000 are not controlled for, but state fixed effects control for the time-invariant differences between states in sales tax rates.

<sup>27</sup>This database was graciously provided by Daniel Wilson. It did not include data for 2012, which I collected via the Tax Foundation. See <http://taxfoundation.org/article/state-corporate-income-tax-rates> (accessed April 15, 2014). The effective rate uses the top corporate tax rate, but adjusts for the deductibility of federal taxes from state taxable income, and vice-versa.

compared to smaller states, so the effects of MPPIs may get confounded with the fact that larger states are more likely to have MPPIs (as shown by Leiser 2013).  $\mu_t$  are time fixed effects which control for the average number of productions each year across all states. These control for national trends in motion picture production that affect all states. Since motion picture production has been increasing over time, excluding time fixed effects would confuse this trend with the adoption of MPPIs, which has also been increasing over time. These time fixed effects also control for policies that affect all states, such as *The American Jobs Creation Act of 2004*, which included a broad set of tax credits, including those that applied to motion picture production.

As is suggested for studies that use state-level policy variation, I cluster my standard errors at the state level (Bertrand, Duflo and Mullainathan, 2004). This allows for arbitrary patterns of serial correlation within states and heteroskedasticity across states, resulting in more accurate estimates of the standard errors, and more accurate inference.

In a separate regression I then replace the *Refundable or Transferable*<sub>it</sub> indicator variable with control variables for the three subsidy rates (*Resident*<sub>it</sub>, *Non-Resident*<sub>it</sub>, and *Non-Labor*<sub>it</sub>), interacting each of these subsidy rates with *Refund*, which captures how much of the incentive filmmakers can receive beyond their tax liabilities. *Resident*<sub>it</sub>, *Non-Resident*<sub>it</sub>, and *Non-Labor*<sub>it</sub> are the subsidy rates, on a 0 to 100 scale, for resident labor expenditure, non-resident labor expenditure, and non-labor expenditure, respectively. *Refund* = 1 for cash rebates, grants, and fully refundable tax credits. A few tax credits are refundable at a slightly reduced rate, so *Refund* is set equal to this rate. For transferable tax credits, a cut of 20-30% of the credit is taken by brokers when the credit is sold to a firm with tax liabilities (Christopherson and Rightor 2010; Luther 2010), so I set *Refund* = 0.75

for transferable tax credits<sup>28</sup> This regression with policy variables is as follows:

$$\begin{aligned}
 Productions_{it} = & \exp(\alpha + \beta_1 Resident_{it} \times Refund_{it} + \beta_2 Non-Resident_{it} \times Refund_{it} \\
 & + \beta_3 Non-Labor_{it} \times Refund_{it} + \beta_4 Neither_{it} + X_{it}\Phi + \delta_i\varphi + \mu_t\tau + \epsilon_{it}) \quad (2.3)
 \end{aligned}$$

The coefficients on these subsidy rates represent the average percent increase in productions after the respective subsidy rate is increased by one percentage point.

In the next two regressions, I estimate effects over time, rather than just average effects, by adding lagged policy effects up to six years after an adoption or strengthening of an MPPI. I present estimated effects for up to one year, up to three years, and up to five years after adoption (or strengthening) of an MPPI by summing the lagged effects. For example, the effects for up to five years would be the sum of the contemporaneous effect ( $j = 0$ ) to the lag for the fifth year ( $j = 5$ ). However, note that I set the lagged variables equal to zero if the MPPI is no longer active. This is to avoid estimating a lagged effect when the policy is no longer active. I include a lag of six years so that the fifth lag only includes impacts in the fifth year. In these regressions I also include identical lags of the sales and corporate tax rate controls.

My rationale for investigating lagged effects is that Equations 2.2 and 2.3 capture the average effect over time after adoption of an MPPIs. This average effect may mask changes in the effectiveness of MPPIs over time. For example, it may take a year for the MPPIs to become effective, if they are not anticipated, and effects may build up over time as the industry establishes itself in states that adopt MPPIs. Although a longer lag length than five years would be preferred to investigate longer-term effects, most MPPIs were adopted or strengthened between 2003 and 2012, so a lag length beyond eight years is not feasible. As the lag length increases, fewer states contribute to the estimation of these lagged effects,

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<sup>28</sup>My results very similar assuming  $Refund = 0.7$  or  $Refund = 0.8$  and are available upon request.

and five years was the most reasonable. The regressions with lagged effects are as follows:

$$\begin{aligned}
 Productions_{it} = \exp(\alpha + \sum_{j=0}^6 [\beta_{1j} Refundable\ or\ Transferable_{it} + \beta_{2j} Neither_{it}] \\
 + X_{it}\Phi + \delta_i\varphi + \mu_t\tau + \epsilon_{it}) \quad (2.4)
 \end{aligned}$$

$$\begin{aligned}
 Productions_{it} = \exp(\alpha + \sum_{j=0}^6 [\beta_{1j} Resident_{i,t-j} \times Refund_{i,t-j} \\
 + \beta_{2j} Non-Resident_{i,t-j} \times Refund_{i,t-j} + \beta_{3j} Non-Labor_{i,t-j} \times Refund_{i,t-j} \\
 + \beta_{4j} Neither_{i,t-j}] + X_{it}\Phi + \delta_i\varphi + \mu_t\tau + \epsilon_{it}) \quad (2.5)
 \end{aligned}$$

Since the employment and establishment count data do not include zeros, it is possible to use a log-linear regression model instead of a Poisson. The same regressions as Equations 2.2 to 2.5 are run except as log-linear and with either employment or establishment counts instead of production counts as the outcome variable.

## 2.7 Panel Regression Results

### 2.7.1 Number of Productions

Table 2.5 presents estimates for the number of productions. Column (1) presents results from Equation 2.2, and Columns (2), (3), and (4) present results for Equation 2.3. These results show that MPPIs that are refundable or transferable strongly affect filming location. The effect is about 35% for the average MPPI, with most of the effect being accrued in up to one year after adoption. There is no clear trend for the few MPPIs that are neither transferable nor refundable.

Table 2.6 presents estimates using Equation 2.4 [Column (1)] and 2.5 [Columns (2), (3), and (4)], which control for the subsidy and refundability rates. These results suggest that the positive filming location effects are driven only by the subsidy to workers who are state residents. However, the effects are smaller here. The coefficient on *Resident*  $\times$  *Refund* in Column (1) suggests that a one percentage point increase in the resident labor subsidy rate increases filming by 1.47%. However, only the estimated effects for up to three years and up to five years for the resident subsidy rate are statistically significant at the 5% level or more, so it is only possible to conclude that this rate affects filming but not until at least three years later. The fact that only the resident subsidy rate matters here makes sense given that local hiring is usually about half the filming budget.

These impacts on filming are much smaller than for New Mexico, which had an average increase in filming of 66.1%. For example, the average MPPI (multiplying the sample average values of each rate variable and *Refund* by the coefficients in Column (4) of Table 2.6) gives a 15.6% increase in production over five years, relative to when the incentives were adopted. This is an 2.9% average increase per year after adoption, while for New Mexico this was 8.8% per year. It is not surprising that the effect for New Mexico is larger, as it represents the best-case scenario, where fairly strong incentives were adopted early.

## 2.7.2 Employment

Tables 2.7 and 2.8 present estimates for employment. For MPPIs on average (Table 2.7), there is no impact for MPPIs that are refundable or transferable, but there appears to be a strong negative effect for MPPIs that are neither refundable nor transferable. This is puzzling since these weak incentives should have either a small positive or non-existent impact and not a negative one. These estimates may reflect that, because only 6% of MPPIs are of this type, these estimates are picking up employment changes in only a few states for a short

period of time.

Table 2.8 does however show a positive and statistically significant effect of the resident subsidy on employment up to one year after adoption. This can be driven both by the increased filming that is attributed to the resident labor subsidy, but it would also make hiring locals more attractive relative to other inputs (hiring non-residents, non-labor expenditures). However, this positive effect from the resident labor subsidy is offset by an equivalent negative and statistically significant estimate for the non-labor subsidy rate. Since almost all (97%) of MPPIs subsidize both resident labor and non-labor expenditure, the employment effects are weak for most MPPIs (a 1.8% increase for the average MPPI up to one year after adoption, again using the average subsidy rates). This suggests that there is some substitution effect, as a higher subsidy to non-labor expenditure decreases the hiring of locals.

### **2.7.3 Establishments**

Tables 2.9 and 2.10 present estimates for establishments. The results for MPPIs that are neither refundable nor transferable are negative again, but other than this there are no statistically significant estimates in either direction. MPPIs do not appear to have any positive effect on establishments.

### **2.7.4 Effects by Time of Adoption**

MPPIs that are adopted earlier, such as the MPPIs in New Mexico and Louisiana, adopted in 2002, may have been more effective than MPPIs adopted later, since there were fewer other states with MPPIs to compete with. To investigate this, I ran the same regressions as Equations 2.2 and 2.3, but with the indicator variable for a refundable or transferable MPPI being interacted with a time variable equal to the year (normalized to be zero for the

first year of the panel) that the MPPI was adopted. The results, available upon request, show that none of the variables with the time-variable interactions are statistically significant at the 5% level. Thus there do not appear to be larger effects for incentives that were adopted earlier.

### 2.7.5 Effects by State Population

Since there are strong forces of agglomeration facing motion picture production establishments, larger states with larger existing film industries may be more attractive both for filming and establishment location, making MPPIs in these states more effective. To investigate this, I ran the same regressions as Equations 2.2 and 2.3 above, but with the indicator variable for a refundable or transferable MPPI interacted with state population estimates from 2000, in millions. For these regression, I exclude California and New York since they both outliers given that they both have established film industries.

The only regression with statistically significant results for the population interactions is that of Equation 2.3 for establishments. This is shown in Table 2.11, which is similar to Table 2.9. These results suggest that there are increases in establishments for refundable and transferable MPPIs, but only in small states. Column (1), for example, shows that an increase in state population in 2000 of one million decreases establishments by about 2.55%. The estimate without the population interaction, which represents the estimate if population were zero, is 0.168, or a 16.8% increase in establishments. Therefore, the sum of the two is positive for states with a 2000 population of Massachusetts (6.1 million) or less, but negative thereafter. This result is likely because less populous states have smaller film industries, so even small increases in establishments can be large percent increases in establishments.

## 2.8 Exogeneity of Incentives

As with any research that compares states that have adopted policies to those that have not, the adoption of those policies may be endogenous to certain state features or trends. This endogeneity could be present in the adoption of incentives, amending of incentives, and in the strength of the chosen incentive. Leiser (2013) investigates if certain state features predict initial adoption of MPPIs. She notes that the pattern of initial MPPI adoption best fits a “bandwagon” theory of policy adoption, whereby states are influenced to adopt MPPIs based on the number or previous adopters and not based on the features of their state or if neighboring states have adopted MPPIs.

Leiser (2013)’s investigation of state features or trends that predict initial MPPI adoption found that few features mattered, except the initial size of the film industry in the state. This is not surprising, as the film industry, led by the Motion Picture Association of America, has been lobbying for these incentives for years (Christopherson and Rightor, 2010), and these lobby efforts are more powerful when the film industry is larger. Leiser (2013) notes that, contrary to what might be commonly believed, adoption of an MPPI is not positively correlated with a neighboring state adopting an MPPI. One might think that neighboring states would see each other as competitors, as they share common geography, weather, and other similarities, but they do not seem to react to each other’s policies.

My research methodology should avoid the endogeneity discovered by Leiser (2013), where states with larger existing film industries were more likely to adopt MPPIs, or adopt them earlier. Since I use state fixed effects, I identify the impact of MPPIs by using within-state variation. The existing size of the film industry in each state is controlled for through the fixed effects.

Where endogeneity could be problematic is if there are differential trends in motion picture production by state. If, independent of MPPI adoption, states with MPPIs experience



a different growth of rate in motion picture production than those states without MPPIs, then my analysis may confuse these differential state trends with the effects of MPPIs. Suppose, for example, an Ashenfelter’s Dip, where states adopt or amend an MPPI in reaction to a negative (or positive) trend in motion picture production. If negative (positive) trends predict adoption or strengthening of MPPIs, then my estimated impacts of MPPIs could be biased downward (upward).

To investigate this, I regress my main policy variables (*Refundable or Transferable*,  $Refund \times Resident$ ,  $Refund \times NonResident$ ,  $Refund \times NonLabor$ ) on six years of leads of each outcome variable (productions, employment, establishments). This is 12 regressions total (four policy variables by three outcomes). For productions, where a Poisson regression is used, this is shown in Equation 2.6, where  $PolicyVariable_{it}$  is replaced with one of the four policy variables listed above. For employment and establishments this regression is the same, except log-linear.

$$PolicyVariable_{it} = \exp(\alpha + \sum_{j=0}^6 \beta_j Productions + X_{it}\Phi + \delta_i\varphi + \mu_t\tau + \epsilon_{it}) \quad (2.6)$$

Results of these regressions are presented in Table 2.12 for *Refundable or Transferable* and Table 2.13 for the rate variables. While most lags are statistically insignificant, there are two patterns the merit discussion. It appears that a negative shock five years before adoption is predictive of adoption (for productions and establishments as shown in Table 2.12) and is predictive of stronger MPPIs (in five out of nine cases in Table 2.13). This relationship is strongest for employment and establishments, occurring in three out of four regressions, but this only occurs once for productions. While the regression results for each subsidy rate should be similar, as subsidy rates co-vary significantly, the fact that this trend with the fifth lead occurs for more than one variable suggests that it may not be random.

Supposing that it is true that five year leads are predictive, what impact would this have on my estimates? If negative shocks persist in the treatment period, then my estimates are biased downward. This is likely more of an issue for employment and establishments than for productions since the fifth lead is only predictive once for productions. Given that only one other lead is negative and statistically significant at the 5% level, it appears that this could be a reaction to a temporary shock long before policy adoption. While these tests point to a negative bias to my employment and establishment results, the bias is likely negligible or unlikely because there is no clear Ashenfelter's Dip.

The only other trend is that the first lead is predictive for all three rate variables for employment, at the 5% or 10% level of significance. This seems odd in comparison to the fifth lead, which is of the opposite sign, and suggests that the effects of the fifth lead could be offset. If this first lead result is taken as true, then positive employment shocks predict more generous MPPIs in the following year. This would suggest an upward bias in my estimated employment effects if positive employment shocks continued independently of MPPIs. An upward bias is less worrisome in this case, as it suggests that my employment results could be an upper bound and this upper bound is almost always zero since few MPPIs have positive employment effects. In summary, there does appear to be some endogeneity from my tests but the effects of this endogeneity are likely negligible. This is, of course, with the caveat that there is no exact test of endogeneity so we cannot know this with certainty.

## 2.9 Conclusion

Motion picture production incentives (MPPIs) have become wildly popular at the U.S. state level since about the early 2000s. Because of the large variation across time and states in these incentives, studying them can tell us a great deal about how tax and economic development incentives work. The film industry is also a good industry to study. Filming

is incredibly footloose; filming locations are readily substitutable, so cost matters the most. Management often chooses the filming location based on the incentives available, taking the location choice away from the creative management. On the other hand, location choices for motion picture production firms are more sensitive to the particular characteristics of location, such as local labor and input markets. I estimate the impact of MPPIs on these two divergent cases.

To estimate the impacts of MPPIs on filming location, establishments, and employment, I combine a database I created on motion picture production incentives (MPPIs) from 1980 to 2012 with data on filming locations from the Internet Movie Database (IMDb.com) for the same time period, and employment and establishment count data for motion picture production from the Quarterly Census of Employment and Wages (QCEW) from 1988 to 2012. I use two difference-in-differences methodologies: a panel regression, which estimates the average effects of MPPIs across all states, and two Abadie, Diamond and Hainmueller (2010) synthetic control case studies for New Mexico and Louisiana's incentives from 2002 to 2008. The case studies tell us what the effects of MPPIs should be in the best case, since these were early and strong adopters of incentives, while the panel regression results tell us what the effects are on average across all states.

The case studies of New Mexico and Louisiana show large effects on filming and employment, but only for New Mexico are these effects statistically significant. For New Mexico, the average number of productions increased by about 28 per year, representing a 41.5% increase compared to the "business as usual" case without incentives. Similarly, employment in New Mexico increased by 870 jobs, or a massive 217% increase relative to without incentives. However it is not clear what proportion of these jobs are full-time jobs, or jobs for New Mexico residents rather than out-of-state residents flown in. There is little evidence for an effect on business establishments.

In the panel regression results, which estimate effects across all states, I find that the

subsidy rate for workers who are state residents is positively associated with the number of productions being filmed. For the average MPPI this is a 16.1% increase in filming over five years since the incentive was adopted, or a 3% increase each year. However, very few MPPIs have a positive impact on employment or establishments.

These results show that most incentives have a moderate effect on filming. Since filming is so footloose, this tells us that we can only expect these moderate effects for similarly footloose industries where locations are more substitutable. The fact that incentives do not affect the location decisions of motion picture production establishments suggests that incentives have no impact when firms value the unique characteristics of each location, such as local labor and input markets, or where there are agglomeration economies.

These results suggest that MPPIs simply relocate productions and cannot help a local industry establish. Obviously employment and establishments in motion picture production are not the only metrics that determine if the local industry is created, but these are the most important. This should prompt state policymakers to reconsider MPPIs if establishing a local film industry is their primary goal. I suggest two courses of action for policymakers. The first is amending existing MPPIs to remove subsidies to workers who are not state residents and to remove subsidies to non-labor expenditure. Neither of these are associated with any positive effects. The second is to remove MPPIs entirely. Which course of action is more desirable for policymakers depends on if the benefits of MPPIs are still worth the cost. The benefits are only the spending injection from filming, which could affect other industries such as the hospitality industry and catering, but does not seem to affect the local film industry. From a national perspective, it is even more likely that MPPIs are not worth the cost since a significant portion of the increased filming due to MPPIs likely comes at the expense of established film industries, such as industry in greater Los Angeles.

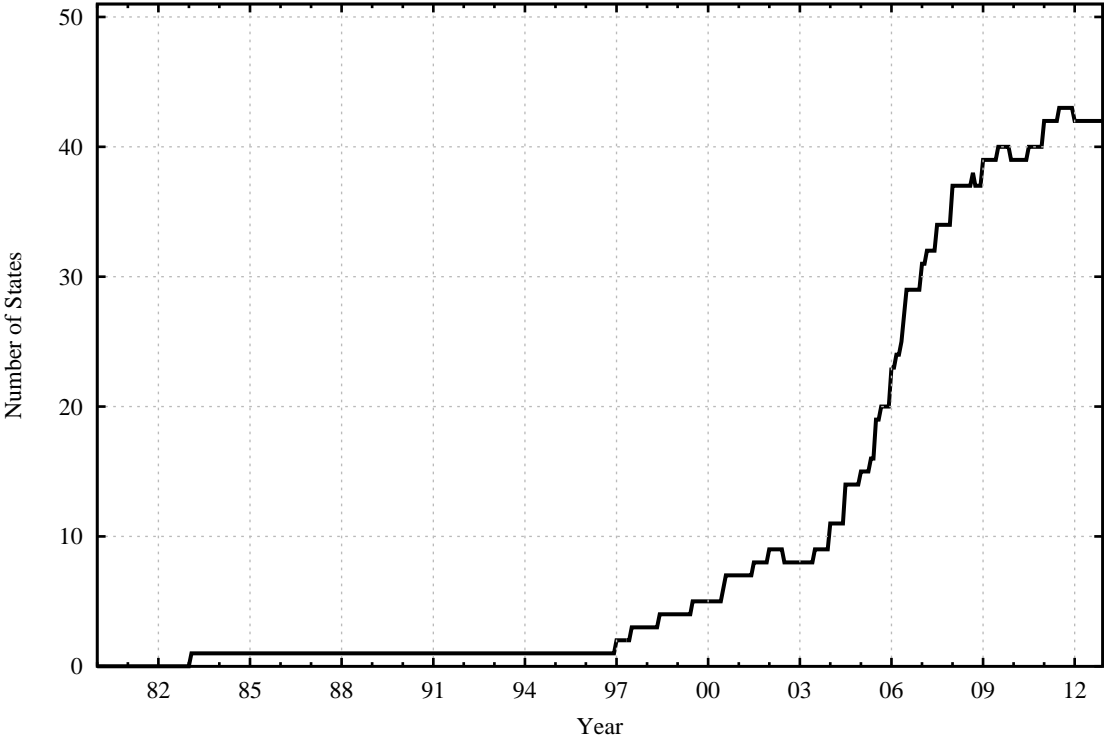
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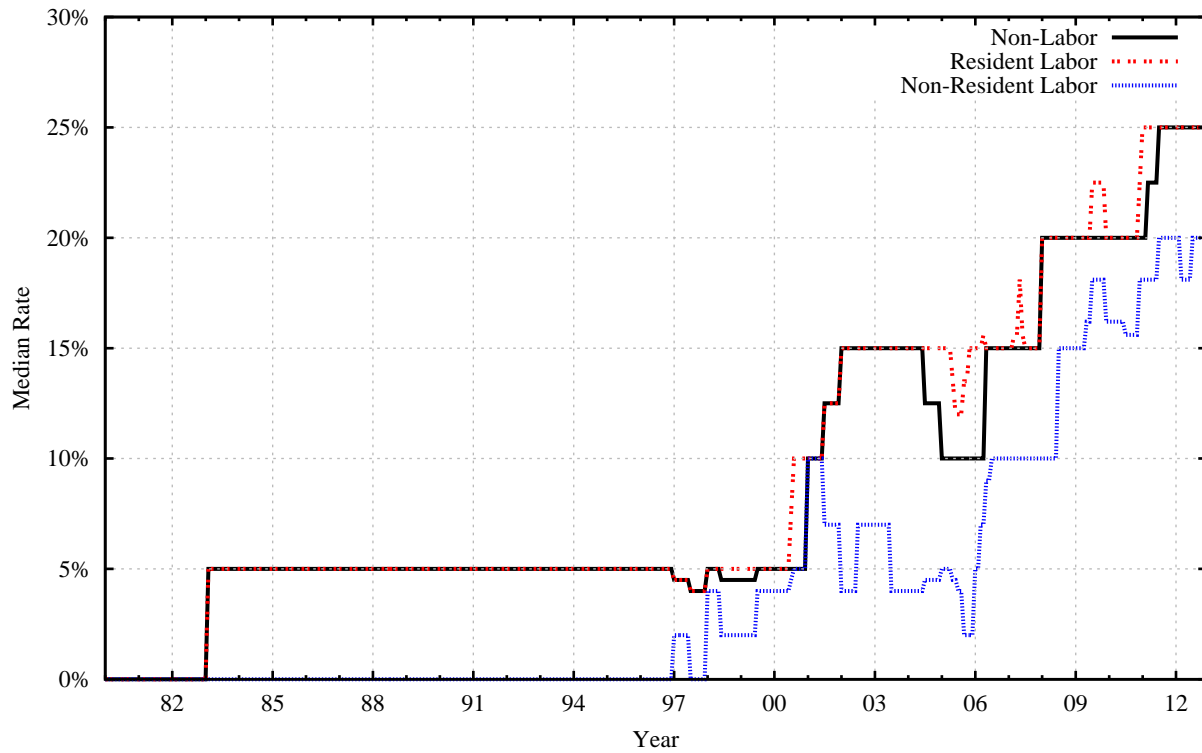
Figure 2.1: Number of States with a Motion Picture Production Incentive (MPPI)



Motion Picture Production Incentives (MPPI) here include only cash rebates, grants, or tax credits for motion picture production, and do not include states with only sales tax exemptions, or other small incentives.



Figure 2.2: Median Qualified Expenditure Rates over Time



Medians are calculated only over the set of states with active MPPIs. States with an MPPI that does not cover a particular type of qualified expenditure (typically non-resident labor) is included as a zero in the calculation.

Figure 2.3: States with MPPIs (black) as of January 1, 2003 (top) and September 1, 2013 (bottom)

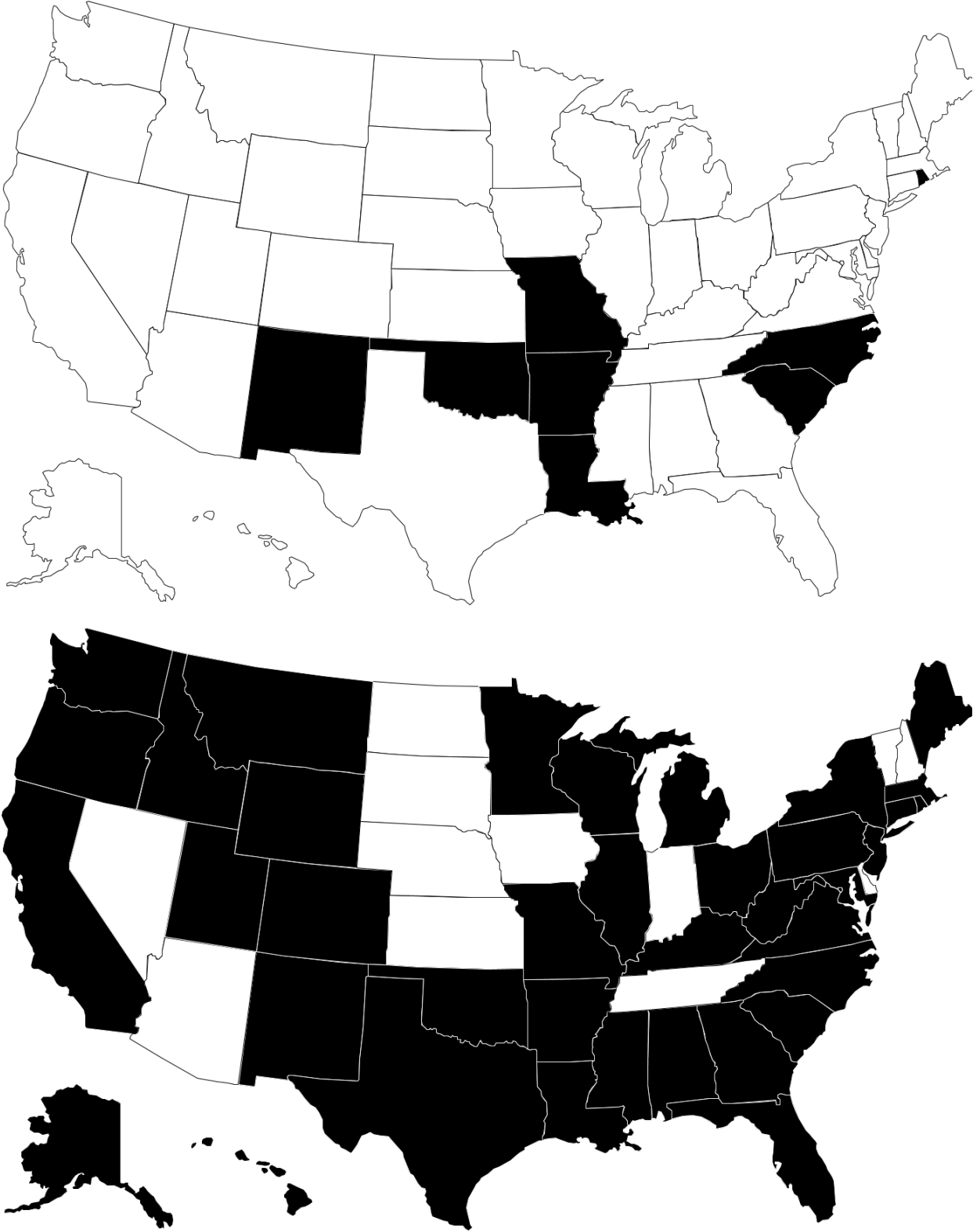
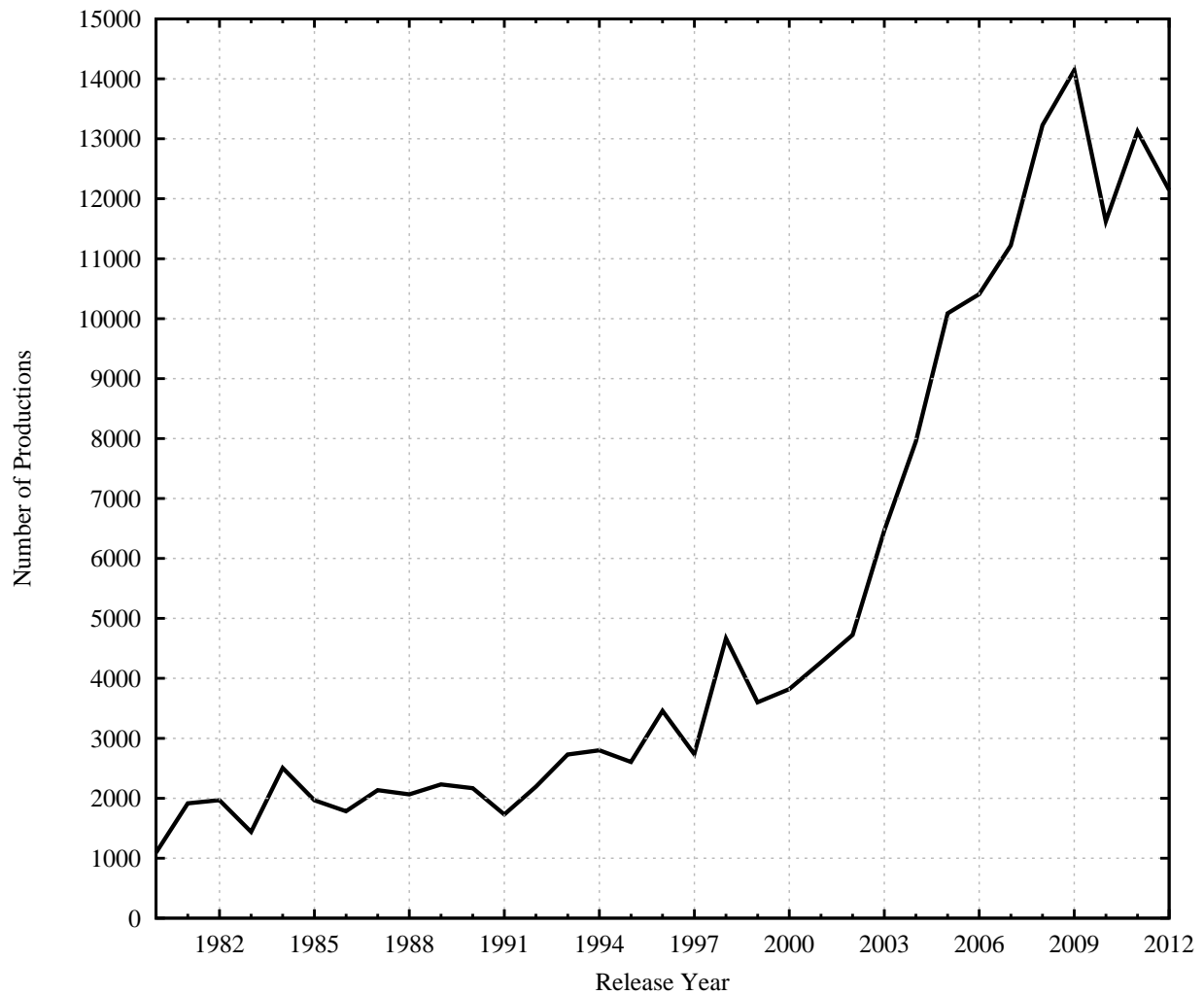
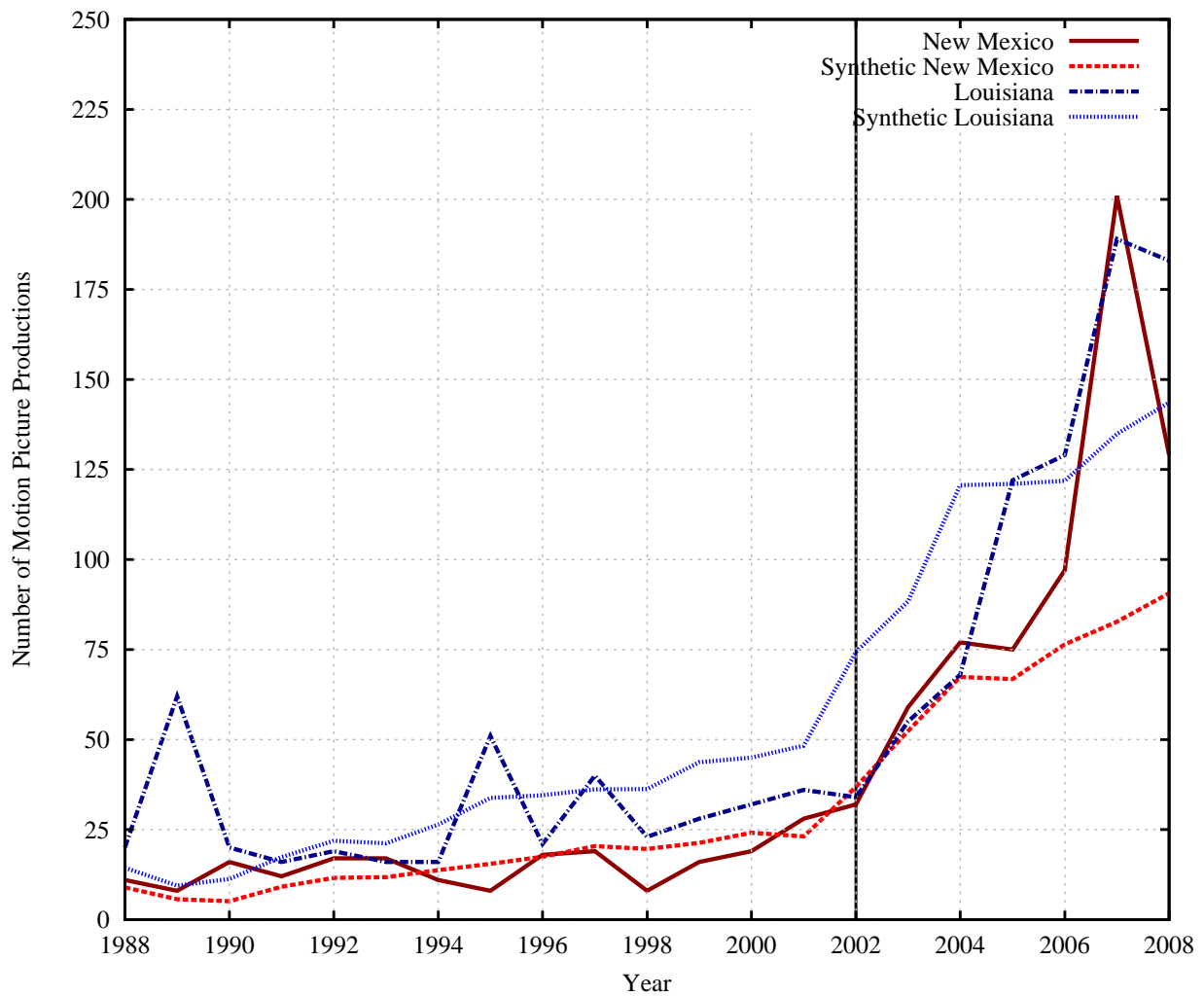


Figure 2.4: Number of Filmed Productions by Release Year



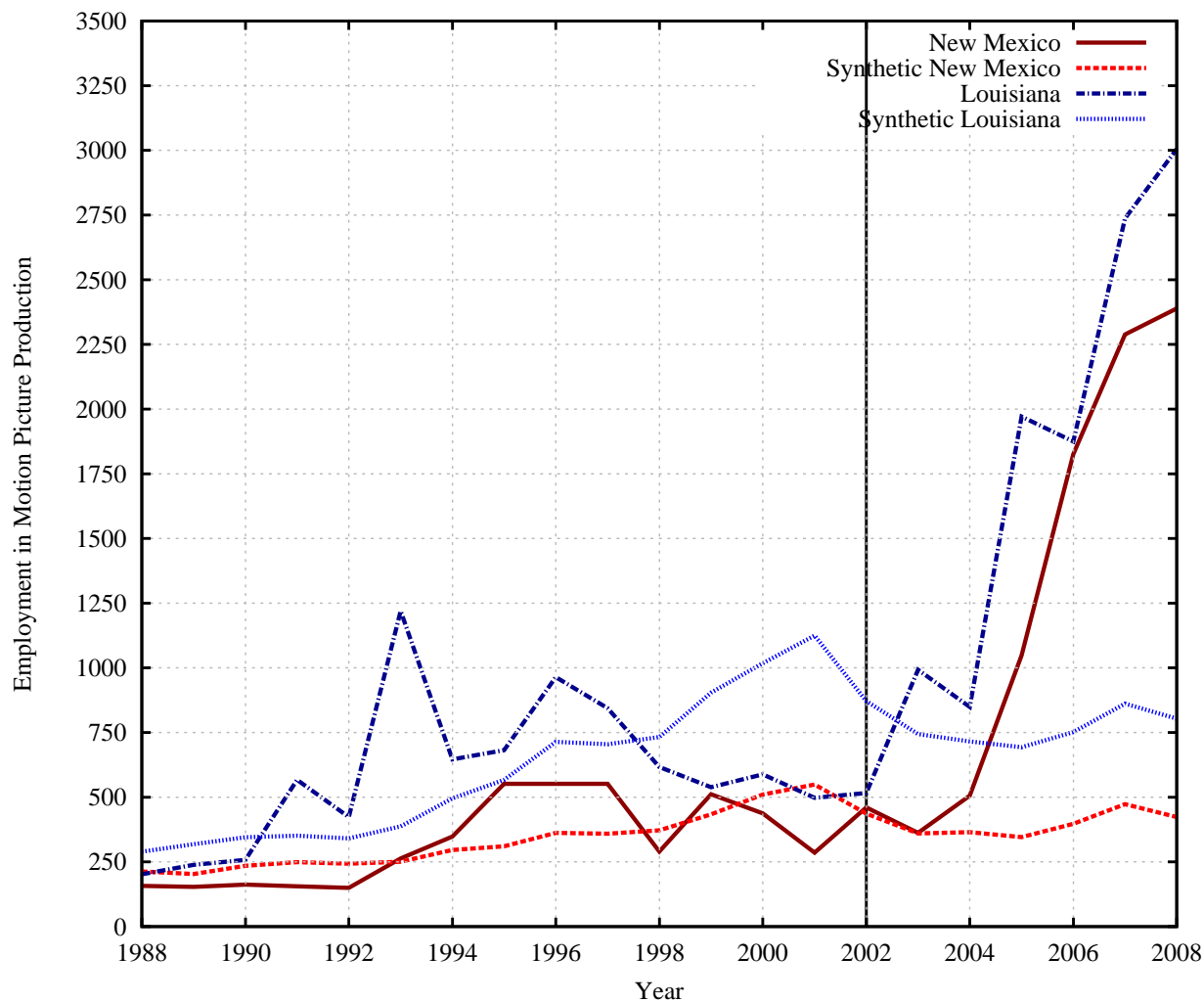
This sample includes all productions on IMDb.com with a release year from 1980 to 2012 that list a state of filming. This is 189,598 productions over all release years. This data was downloaded from IMDb.com on January 17, 2014.

Figure 2.5: Number of Filmed Productions in New Mexico and Louisiana Relative to Synthetic Controls



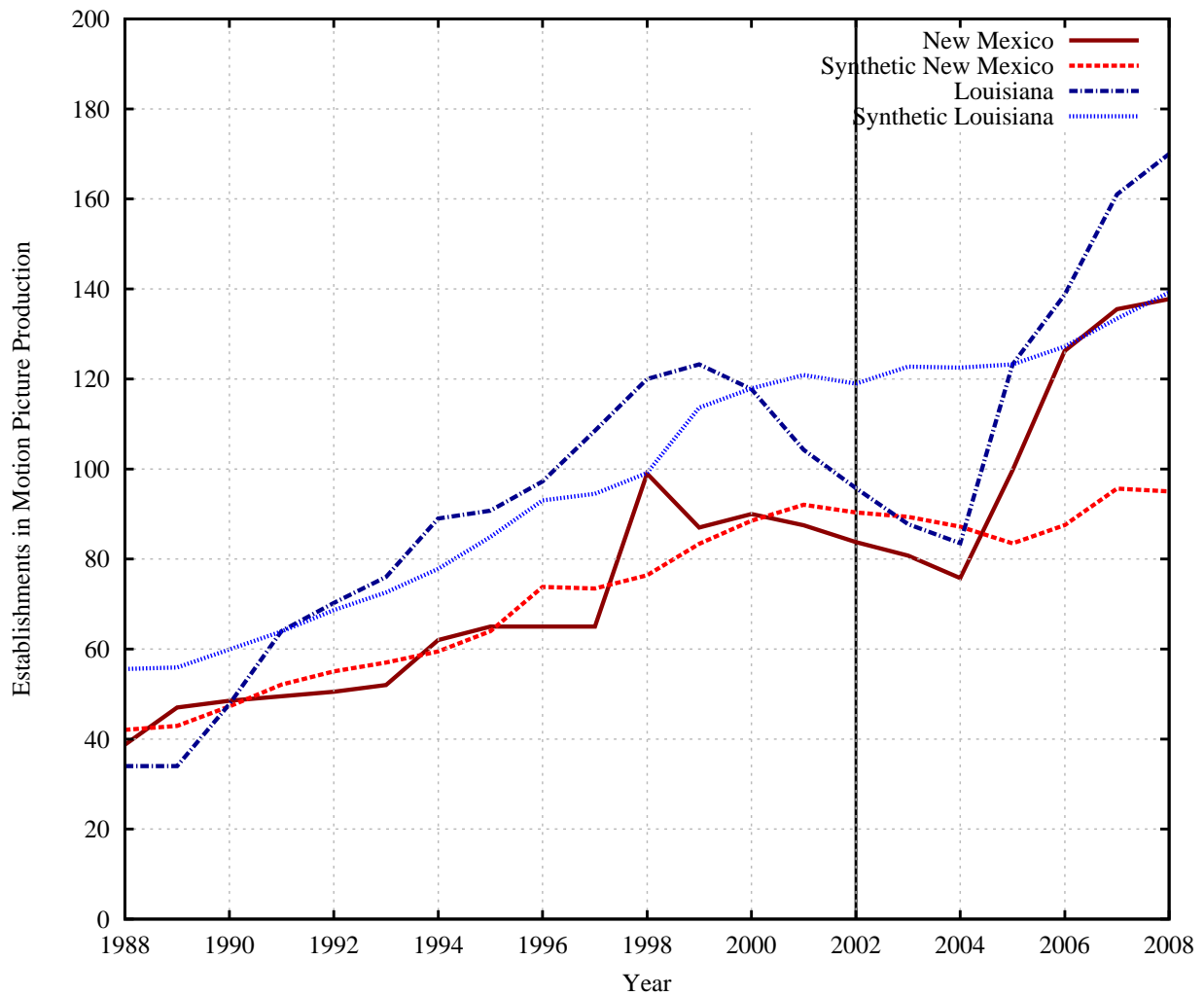
This figure presents the time series of the number of productions for two states, New Mexico and Louisiana, that adopted a motion picture production incentive in 2002. The time series for these states are compared to their respective synthetic control states, constructed following Abadie, Diamond and Hainmueller (2010). The control states are made up of a convex combination of states (AL, DE, KY, NE, NV, NH, ND, OH, SD, UT, and VA) without an incentive during the entire sample period (1988 to 2008). Weights for each control state are determined so that the synthetic control best matches the actual state in the pre-treatment period (1988 to 2001) through minimization of mean squared prediction error. See Table 2.2 and Figure 2.4 for more information on the IMDb.com data.

Figure 2.6: Employment in New Mexico and Louisiana Relative to Synthetic Controls



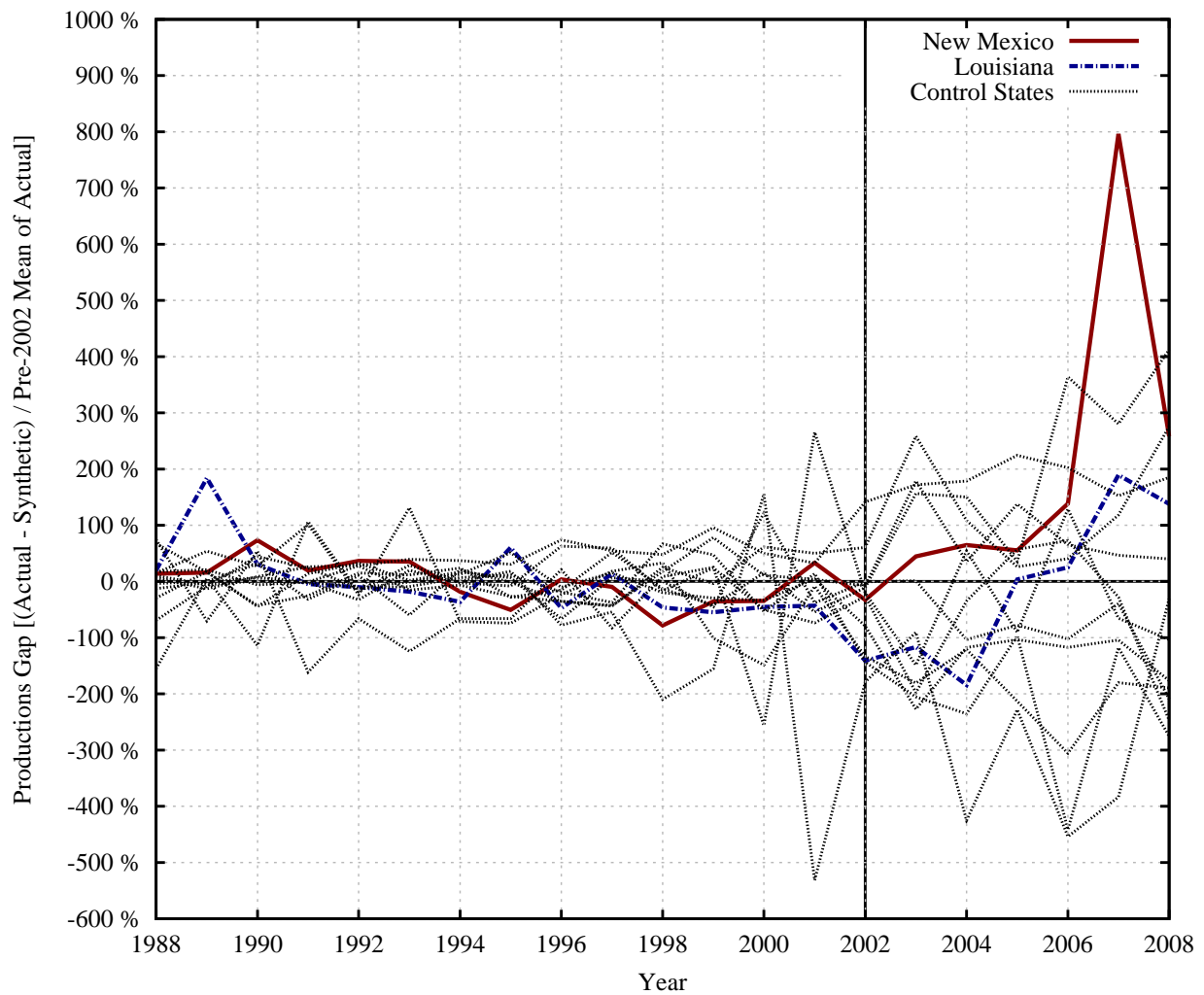
See notes to Figure 2.5. Employment estimates come from the Quarterly Census of Employment and Wages (QCEW) and are for the motion picture production industry (NAICS 512110 / SIC 7812) only. See Table 2.2 for more information on the QCEW data.

Figure 2.7: Establishments in New Mexico and Louisiana Relative to Synthetic Controls



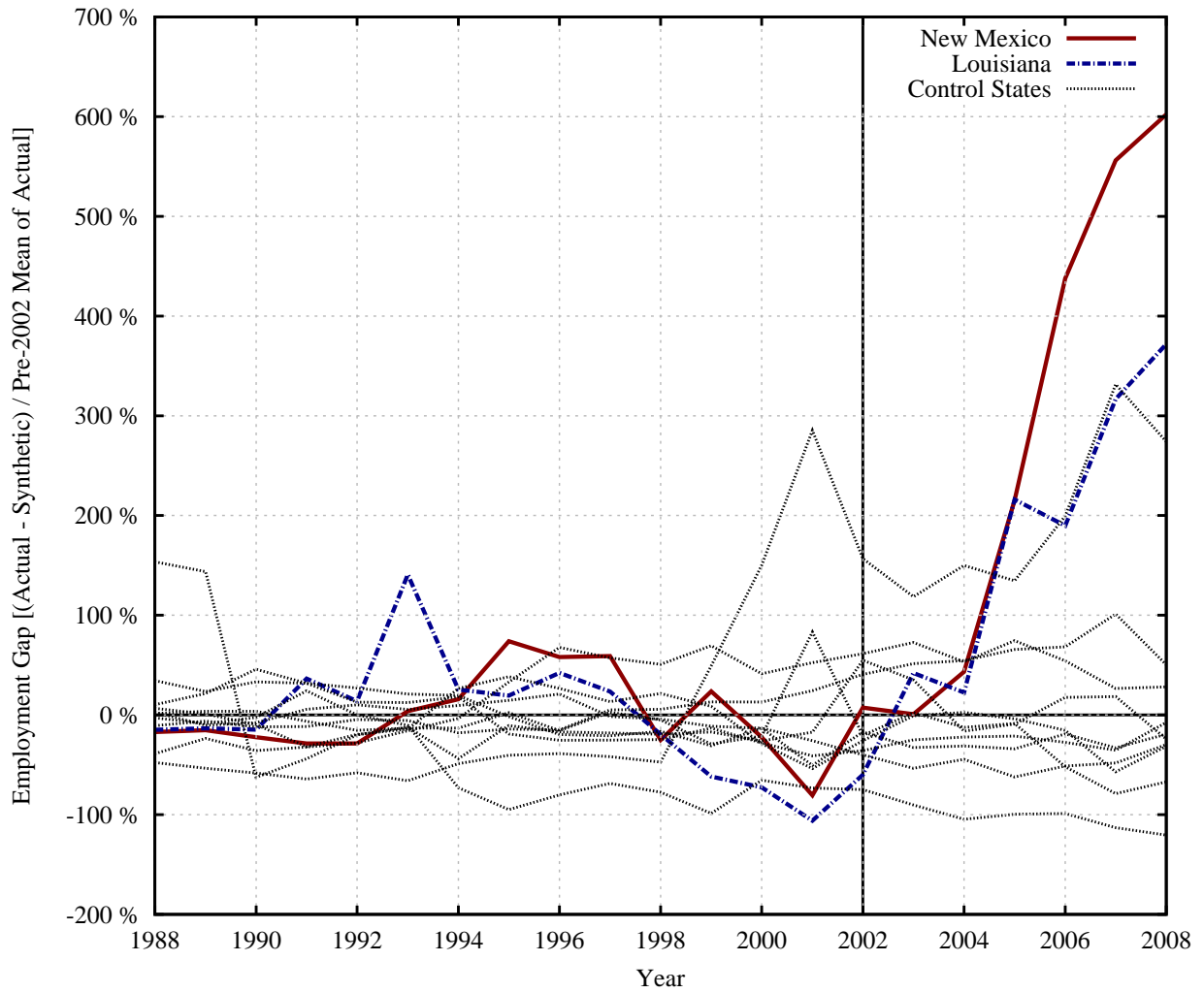
See notes to Figures 2.5 and 2.6. Establishment estimates come from the QCEW and are for the motion picture production industry (NAICS 512110 / SIC 7812) only.

Figure 2.8: Synthetic Control Placebo Tests for Number of Filmed Productions, Control States



This figure presents the gap between the number of productions in the state and in the synthetic state, expressed as the difference between the actual and synthetic state, divided by the average number of productions in the state during the pre-treatment period (1988 to 2001), and then multiplied by 100 to create a percentage. Positive values suggest that the actual state performed better than its synthetic state, and negative values suggest worse. Dividing the gap by the average before treatment allows the interpretation to be the percentage increase (decrease) in productions in that year relative to the average in the pre-treatment period, net of what is predicted by the synthetic control. In addition to the series for New Mexico and Louisiana. Also presented are the series for the control states (AL, DE, KY, NE, NH, OH, SD, UT, and VA), but excluding Nevada and North Dakota. These are excluded because they had the highest and lowest average number of productions, respectively, so no combinations of states could create an adequate synthetic control for them, leading to uninformative plots.

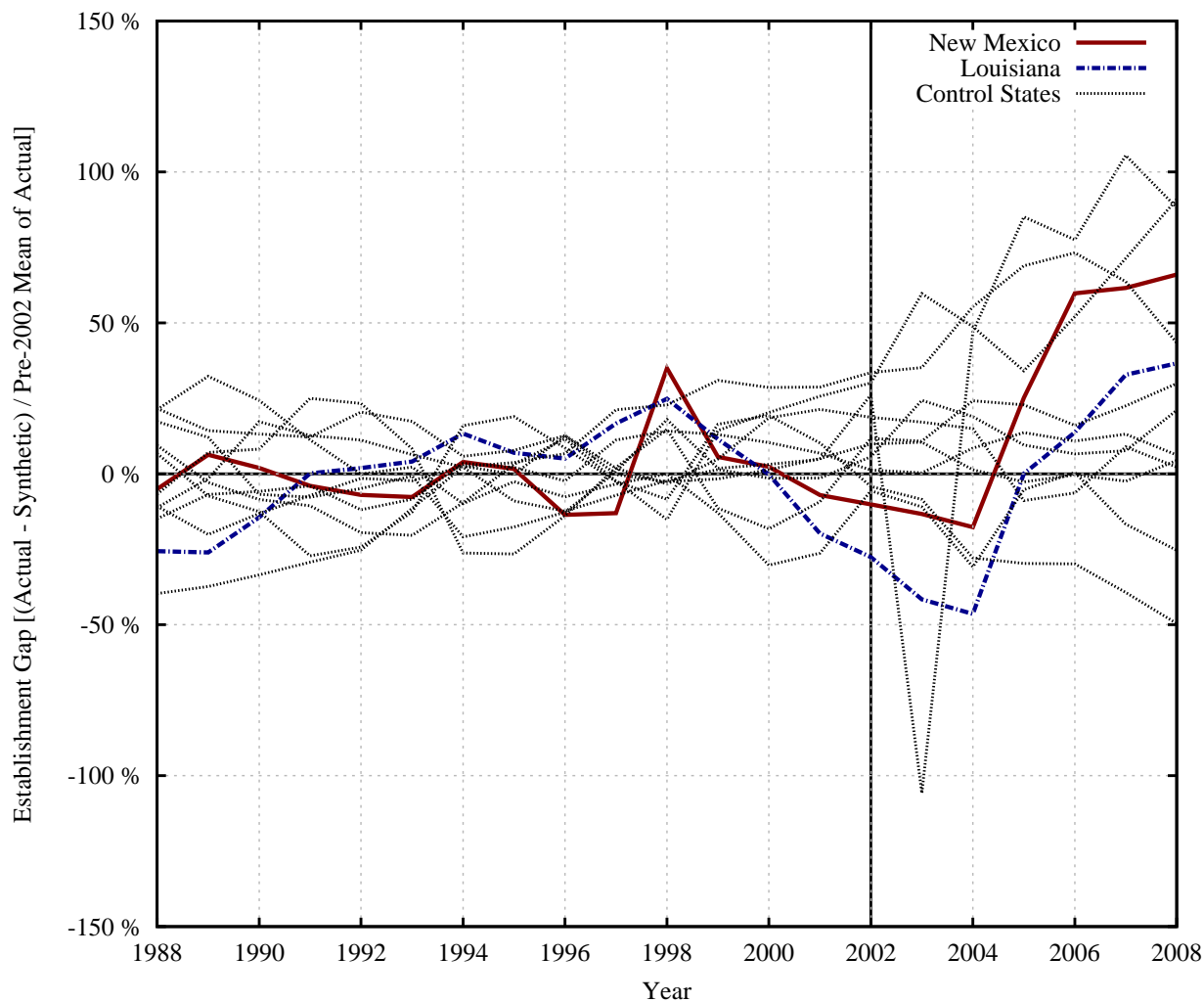
Figure 2.9: Synthetic Control Placebo Tests for Employment, Control States



See notes to Figures 2.8 and 2.6. Utah is missing instead of Nevada, as Utah had the highest employment in the pre-treatment period.



Figure 2.10: Synthetic Control Placebo Tests for Establishments, Control States



See notes to Figure 2.8 and 2.7. Ohio is missing instead of Nevada, as Ohio had the highest employment in the pre-treatment period.

Table 2.1: Summary Statistics for MPPI Database

Variable	Mean	Std. Dev.	Min	Max	Obs
Active MPPI	0.203	0.396	0	1	1,683
For Observations with an Active MPPI:					
Refundable	0.708	0.456	0	1	359
Transferable	0.234	0.424	0	1	359
Neither	0.067	0.224	0	1	359
Only Resident Labor Subsidized	0.025	0.157	0	1	359
Both Labor Types Subsidized	0.028	0.165	0	1	359
Only Resident and Non-Labor Subsidized	0.324	0.469	0	1	359
All Three Subsidized	0.623	0.485	0	1	359
Resident Labor Rate (if subsidized)	18.41	10.71	2.5	50	359
Non-Resident Labor Rate (if subsidized)	19.04	10.20	2.5	50	233
Non-Labor Rate (if subsidized)	18.02	10.41	2.5	50	339

This sample is from 1980 to 2012 and observations are at the state and year level. For MPPI changes that took effect partway through the year, policy variables are set to be a weighted average between the old and new policy based on how many months each was in effect. This data was compiled through my own legal research. See Appendix 2 for more information on this data.

Table 2.2: Summary Statistics for Outcome and Control Variables

Variable	Mean	Std. Dev.	Min	Max	Obs
# of Productions (1980-2012)	95	344	0	4,412	1,683
Employment (1988-2012)	3,387	14,063	10	122,773	1,199
Establishments (1988-2012)	284	752	3	6,313	1,212
Sales Tax (2000-2012)	4.87	1.88	0	8.25	663
Corporate Tax (1980-2012)	6.67	2.75	0	13.80	1,683

All data is at the state and year level (annual averages). QCEW data uses estimates for the motion picture production industry using NAICS 512110 (from 2001 to 2012) and SIC 7812 (from 1988 to 2000). NAICS 512110 and SIC 7812 overlap perfectly, allowing their combination. The production estimates from IMDb includes all productions with a release date from 1981 to 2013 that have a filming location attributable to a U.S. state. This is 189,598 productions, summed by state and year to create 1,683 observations. Sales tax data is from the Tax Foundation. Corporate tax rate data is from Wilson (2009) (1980-2006) and Moretti and Wilson (2014) (2007-2012).

Table 2.3: Control State Weights for Synthetic Control Case Studies

State	<u>New Mexico</u>			<u>Louisiana</u>		
	Productions	Employment	Establishments	Productions	Employment	Establishments
AL	0%	0%	0%	0%	0%	0%
DE	17.5%	16.1%	16.6%	25.3%	27.2%	2.5%
KY	53.1%	55.5%	54.9%	0%	0%	0%
NE	0%	0%	0%	0%	0%	0%
NV	19.9%	19.4%	19.3%	45.7%	43.5%	41.9%
NH	0%	0%	0%	0%	0%	26.8%
ND	0%	0%	0%	0%	0%	0%
OH	9.4%	7.8%	8.8%	21.1%	20.5%	19.5%
SD	0%	0%	0%	0%	0%	0%
UT	0%	0%	0%	8.0%	8.9%	9.3%
VA	0%	1.3%	0.5%	0%	0%	0%

The weights here form the synthetic control groups in Figures 2.5, 2.6, and 2.7. Weights are determined such that the MSPE between the actual and synthetic states is minimized in the pre-treatment period. MSPE is minimized over all three outcome variables (productions, employment, establishments), and also corporate tax rates.

Table 2.4: MSPE Ratios for Treated and Control States

State	Productions	Employment	Establishments
Treated States			
NM	68.0 (1st)* [1st]**	77.8 (1st)* [1st]**	13.8 (3rd) [4th]*
LA	4.7 (10th) [29th]	14.2 (2nd) [3rd]*	4.5 (5th) [15th]
Controls for Entire Sample			
AL	7.7	1.1	3.1
DE	0.6	2.8	1.6
KY	50.3	2.7	1.2
NE	42.7	3.1	1.0
NV	15.8	4.5	17.0
NH	5.3	1.1	4.1
ND	2.9	1.1	5.7
OH	28.9	3.9	0.2
SD	7.1	0.2	50.1
UT	42.7	1.7	4.6
VA	12.4	26.1	1.0

This table presents mean square prediction error (MSPE) ratios for the two treated states (NM and LA) and the control states that appear for the entire sample. See Table 2.14 for the MSPE ratios for the 31 states that adopted incentives between 2002 and 2008. In parenthesis under the ratios for NM and LA is the rank of each ratio relative to the 11 control states, and below this in brackets is the same rank but over the 11 control states plus the additional 31 states listed in Table 2.14. For each of these rankings a \* means the estimate is statistically significant from zero at 90% level and \*\* means at the 95% level.

Table 2.5: Effect of MPPIs on Number of Productions Filmed

	Average Effect (1)	Up to 1 Year (2)	Up to 3 Years (3)	Up to 5 Years (4)
Refundable or Transferable	0.3505*** (0.1112)	0.2858*** (0.0734)	0.2248*** (0.0730)	0.2235** (0.1029)
Neither	-0.0868 (0.0562)	0.0273 (0.1244)	0.2230*** (0.0821)	-0.182*** (0.0530)

The regression results presented here are from Equation 2.2 for column (1) and Equation 2.3 for columns (2), (3), and (4). The estimates reported in Columns (2), (3), and (4) are sums of the coefficients on the contemporaneous policy variables and through to the one year lag, the three year, and the five year lag. The level of observation is state and year. This regression uses all IMDb.com productions which list a state of filming and a release year from 1981 to 2013. The year of filming is assumed to be one year prior to the release year. This sample is generated from 189,598 productions, which are then summed by state and year to create 1,683 observations. Standard errors are clustered at the state level. \* = statistically significant from zero at 90% level, \*\* = 95%, \*\*\* = 99%.

Table 2.6: Effect of MPPIs on the Number of Productions Filmed by Subsidy Rates

	Average Effect (1)	Up to 1 Year (2)	Up to 3 Years (3)	Up to 5 Years (4)
Resident $\times$ <i>Refund</i>	0.0147* (0.0076)	0.0107 (0.0076)	0.0248*** (0.0093)	0.0345*** (0.0131)
Non-Resident $\times$ <i>Refund</i>	-0.0065 (0.0066)	-0.0082 (0.0062)	-0.0110 (0.0069)	-0.0204* (0.0107)
Non-Labor $\times$ <i>Refund</i>	0.0005 (0.0079)	0.0038 (0.0095)	-0.0134 (0.0106)	-0.0121 (0.0152)

See notes to Table 2.5. The regression results presented here are from Equation 2.4 for column (1) and Equation 2.5 for columns (2), (3), and (4). *Refund* is a variable that equals one if the MPPI is a cash rebate or grant, equals the refundability rate for refundable tax credits (which is usually 100%), and 0.75 for a transferable tax credit, and zero otherwise. The coefficients for *Neither* are similar to in Table 2.5 and are not presented here. Each subsidy rate is on a 0 to 100 scale, so coefficient estimates, once they are multiplied by 100, represent the percent increase in the number of productions from a one percentage point increase in the respective subsidy rate.

Table 2.7: Effect of MPPIs on Employment in Motion Picture Production

	Average Effect (1)	Up to 1 Year (2)	Up to 3 Years (3)	Up to 5 Years (4)
Refundable or Transferable	0.0327 (0.0939)	0.0369 (0.0675)	0.0621 (0.1063)	0.1743 (0.1499)
Neither	-0.3523*** (0.1223)	-0.4187** (0.1739)	-0.7801** (0.0821)	-0.7021** (0.2179)

See notes to Table 2.5. The regression results presented here are from Equation 2.2 for column (1) and Equation 2.3 for columns (2) to (4), using employment as the outcome variable and running a log-linear regression instead of a Poisson regression. Employment estimates are for motion picture production (NAICS 512110 / SIC 7812) from the QCEW. These estimates are annual from 1988 to 2012, created by taking annual averages of the monthly data. Refundable or transferable refers to any MPPI that is a cash rebate, grant or a tax credit that is either refundable or transferable. Neither refers to tax credits that are neither refundable nor transferable. There are 1,198 observations.

Table 2.8: Effect of MPPIs on Employment in Motion Picture Production by Subsidy Rates

	Average Effect (1)	Up to 1 Year (2)	Up to 3 Years (3)	Up to 5 Years (4)
Resident $\times$ <i>Refund</i>	0.0093 (0.0076)	0.0140** (0.0054)	0.0158 (0.0130)	0.0234 (0.0183)
Non-Resident $\times$ <i>Refund</i>	-0.0016 (0.0106)	0.0006 (0.0053)	-0.0031 (0.0107)	0.0033 (0.0222)
Non-Labor $\times$ <i>Refund</i>	-0.0057 (0.0088)	-0.0143*** (0.0044)	-0.0088 (0.0127)	-0.0113 (0.0206)

See notes to Tables 2.7, 2.5, and 2.6. The regression results presented here are from Equation 2.4 for column (1) and Equation 2.5 for columns (2) to (4), using employment as the outcome variable and running a log-linear regression instead of a Poisson regression.

Table 2.9: Effect of MPPIs on Establishments in Motion Picture Production

	Average Effect (1)	Up to 1 Year (2)	Up to 3 Years (3)	Up to 5 Years (4)
Refundable or Transferable	-0.0184 (0.0557)	0.0013 (0.0443)	0.0044 (0.0586)	0.0617 (0.0816)
Neither	-0.1446* (0.0850)	-0.1539 (0.1134)	-0.3297*** (0.0515)	-0.3867** (0.0863)

See notes to Table 2.5. The regression results presented here are from Equation 2.2 for column (1) and Equation 2.3 for columns (2) to (4), using establishments as the outcome variable and running a log-linear regression instead of a Poisson regression. Establishment estimates are for motion picture production (NAICS 512110 / SIC 7812) from the QCEW. These estimates are annual from 1988 to 2012, created by taking annual averages of the monthly data. There are 1,211 observations.

Table 2.10: Effect of MPPIs on Establishments in Motion Picture Production by Subsidy Rates

	Average Effect (1)	Up to 1 Year (2)	Up to 3 Years (3)	Up to 5 Years (4)
Resident $\times$ <i>Refund</i>	0.0010 (0.0041)	0.0012 (0.0033)	0.0010 (0.0068)	-0.0005 (0.0098)
Non-Resident $\times$ <i>Refund</i>	0.0003 (0.0037)	-0.0014 (0.0025)	0.0022 (0.0038)	0.0031 (0.0069)
Non-Labor $\times$ <i>Refund</i>	-0.0036 (0.0042)	-0.0018 (0.0031)	-0.0030 (0.0056)	0.0010 (0.0092)

See notes to Tables 2.5, 2.6, and 2.9.

Table 2.11: How State Population Mediates the Establishment Effects

	Average Effect (1)	Up to 1 Year (2)	Up to 3 Years (3)	Up to 5 Years (4)
Refundable or Transferable	0.0542 (0.0692)	0.1683** (0.0711)	0.1596** (0.0731)	0.246*** (0.0913)
$\dots \times Population$	-0.0130 (0.0101)	-0.0254** (0.0114)	-0.0231** (0.0111)	-0.0260** (0.0115)

See notes to Table 2.9. These are the same regressions used in Table 2.9, except with the added variable where the indicator variable for a refundable or transferable MPPI is interacted with *Population*, the state's estimated population in 2000, in millions. *Population* is not included separately since it varies only by state and is absorbed by the state fixed effects.

Table 2.12: Endogeneity Test: Regressing the Indicator for a Refundable or Transferable MPPI on Outcomes before Adoption

Years Before Adoption	Productions	Employment	Establishments
One	0.0012* (0.0007)	0.0411 (0.0476)	0.0880 (0.1375)
Two	0.0009* (0.0005)	0.0093 (0.0337)	-0.0331 (0.0862)
Three	-0.0001 (0.0006)	-0.0011 (0.0551)	-0.0302 (0.0803)
Four	0.0000 (0.0337)	-0.0222 (0.0299)	0.1968** (0.0929)
Five	-0.0014** (0.0006)	-0.0512 (0.0355)	-0.1712** (0.0782)
Six	-0.0008 (0.0012)	-0.0371 (0.0513)	-0.0278 (0.1095)

See notes to Tables 2.5, 2.7, and 2.9. These estimates are based off Equation 2.6 for productions and an identical, but log-linear, regression for employment and establishments.



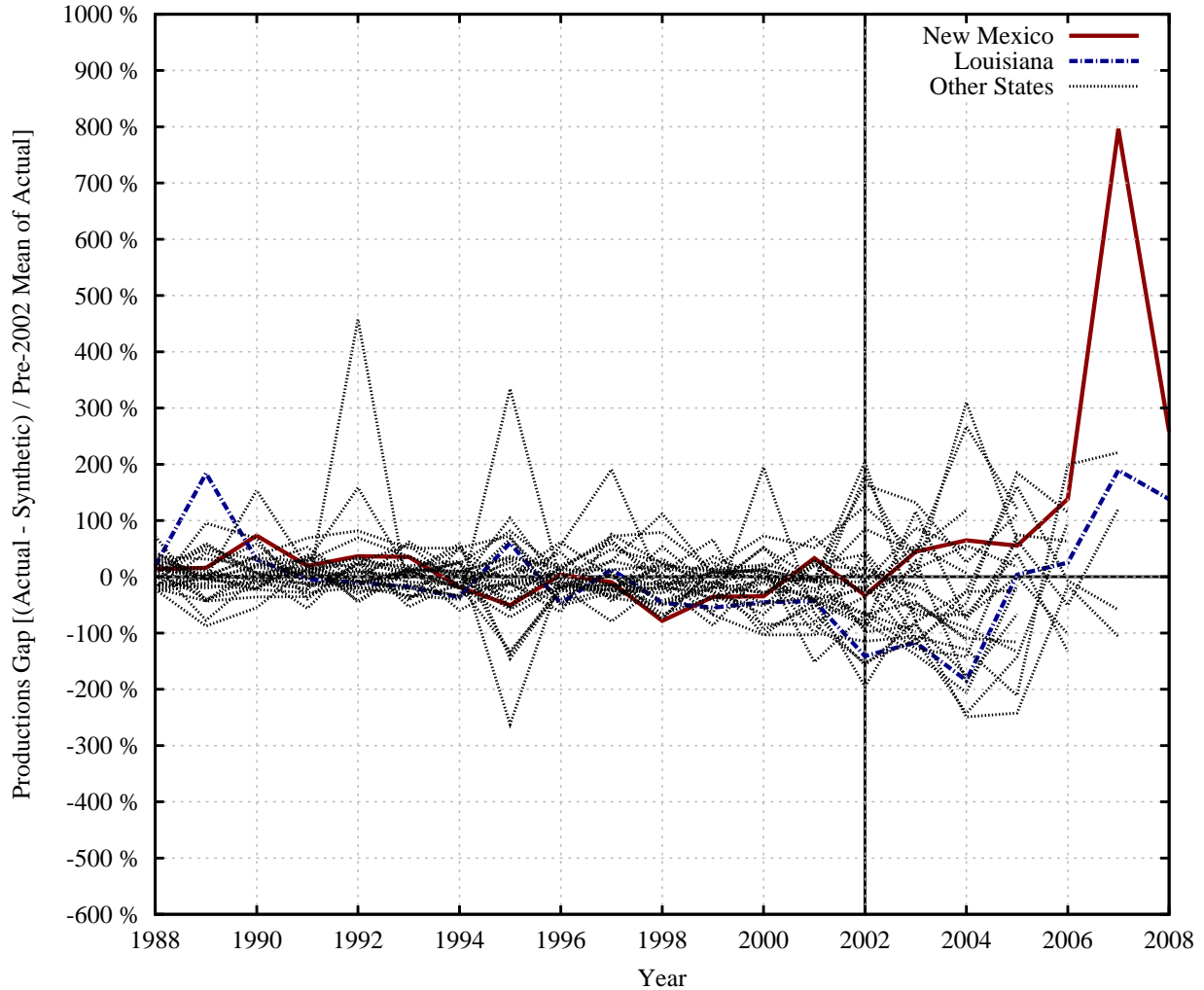
Table 2.13: Endogeneity Test: Regressing the Subsidy Rate Variables on Leading Outcomes

Years Before Adoption	Productions	Employment	Establishments
<i>Refund × Resident</i>			
One	0.0004 (0.0005)	2.1747** (0.906)	4.1843 (3.7576)
Two	0.0004 (0.0003)	0.8871 (0.6949)	-2.3388 (2.0450)
Three	0.0003 (0.0005)	-0.4740 (0.7822)	-1.9137* (1.0311)
Four	0.0003 (0.0006)	-0.8049 (0.5650)	1.3960 (2.1376)
Five	-0.0004 (0.0007)	-2.1009*** (0.6614)	-5.0299*** (1.2807)
Six	-0.0007 (0.0011)	-1.2660 (0.9900)	-2.6443 (2.2916)
<i>Refund × Non – Resident</i>			
One	0.0003 (0.0005)	2.1120** (0.937)	2.8802 (2.5210)
Two	0.0001 (0.0003)	0.2342 (0.6013)	-1.7553 (1.2383)
Three	-0.0001 (0.0006)	-0.8252 (0.6837)	-0.7095 (1.0426)
Four	-0.0004 (0.0008)	-0.3798 (0.4859)	-1.6215 (1.1767)
Five	0.0007 (0.0008)	-1.2077** (0.5820)	-0.5320 (2.1864)
Six	-0.0002 (0.0011)	-0.9130 (1.019)	-0.9420 (2.7141)
<i>Refund × Non – Labor</i>			
One	0.0004 (0.0005)	1.4335* (0.8419)	3.4686 (3.2423)
Two	0.0004 (0.0003)	0.5681 (0.5864)	-2.0347 (1.8488)
Three	0.0003 (0.0006)	-0.5498 (0.6647)	-2.1950** (1.0030)
Four	0.0004 (0.0006)	-0.9297* (0.5248)	1.0521 (2.1074)
Five	-0.0003 (0.0007)	-1.7540*** (0.5715)	-4.8214*** (1.3462)
Six	-0.0006 (0.0011)	-0.7723 (0.9953)	-3.2402 (2.1883)

See notes to Table 2.12.

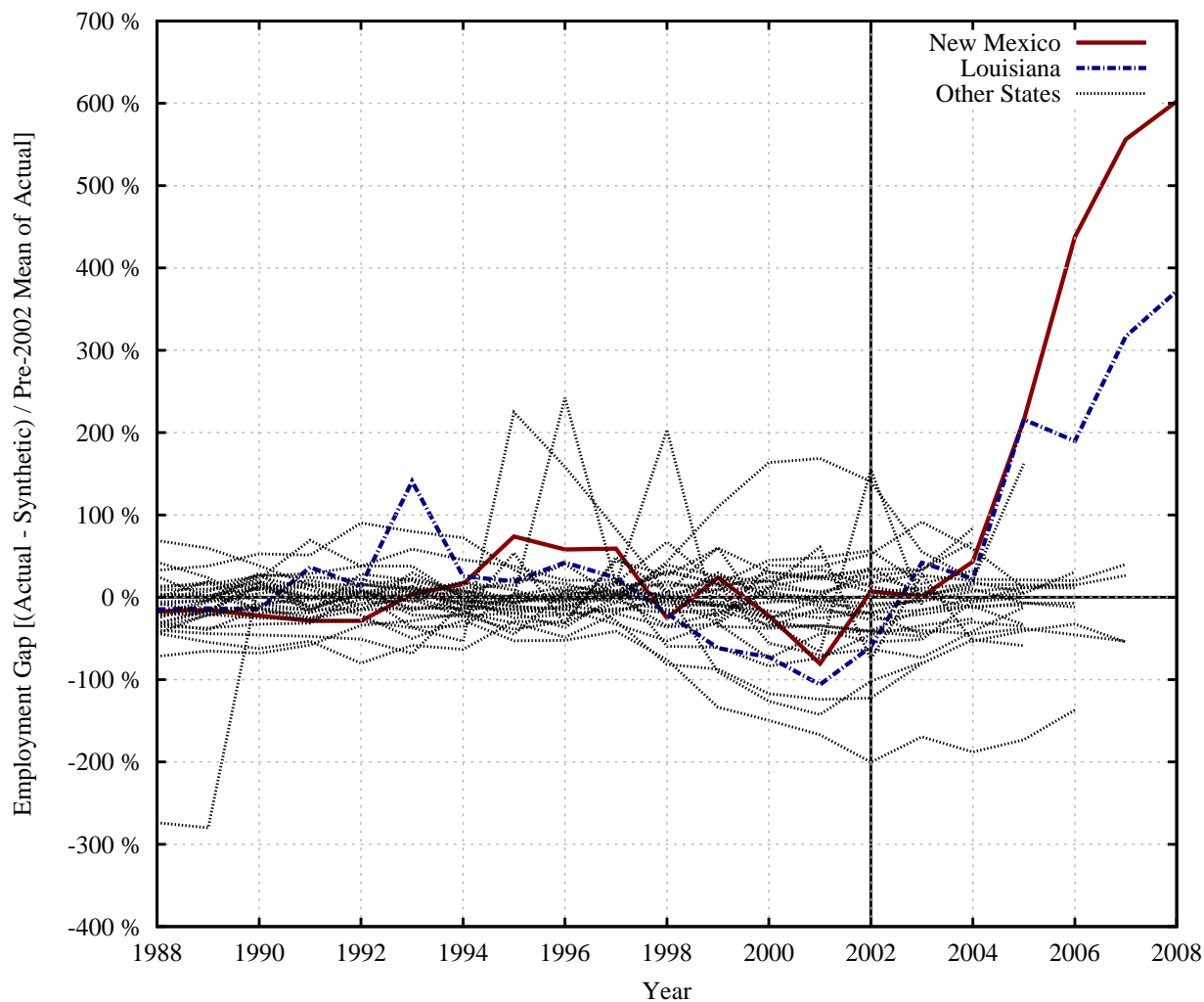
## 2.12 Appendix 1: Additional Figures and Tables

Figure 2.11: Synthetic Control Placebo Tests for Productions, Non-Control States



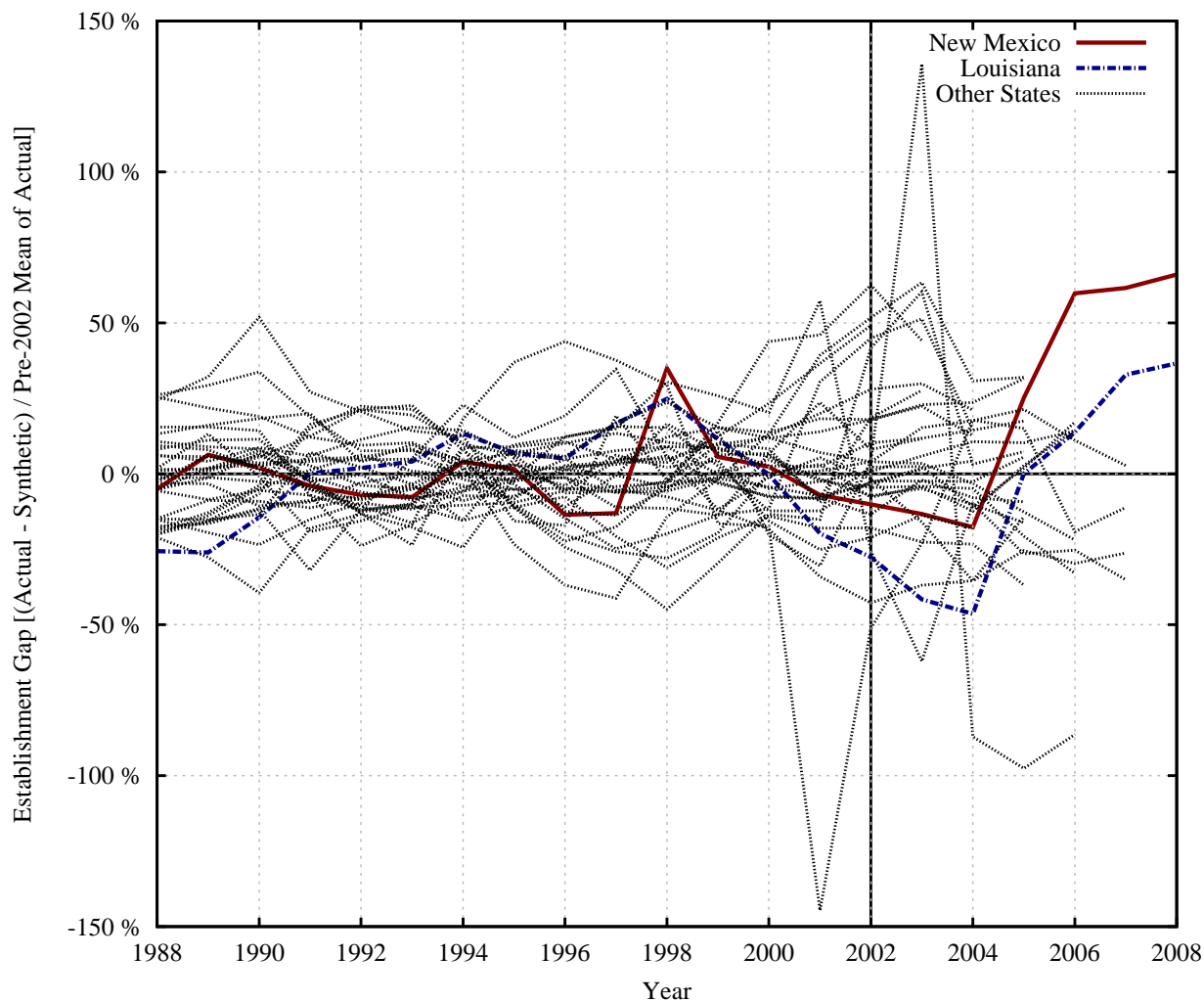
See notes to Figure 2.8. Instead of presenting the series for the control states, presented here are the series for states that did not make up the control group because they adopted an MPPI sometime during the post-treatment period of 2002 and 2008. The 28 states presented here are AK, AZ, CO, CT, DC, FL, GA, ID, IL, IN, IA, KS, ME, MD, MA, MI, MN, MS, MT, NJ, OR, PA, SC, TN, VT, WA, WV, and WI. New York and Texas are not presented because they had the highest number of productions and no control states could create a proper synthetic control for them. Wyoming is also not presented here for similar reasons because it had the lowest number of productions.

Figure 2.12: Synthetic Control Placebo Tests for Employment, Non-Control States



See notes to Figures 2.8 and 2.9. Wyoming is included in this figure.

Figure 2.13: Synthetic Control Placebo Tests for Establishments, Non-Control States



See notes to Figures 2.8 and 2.10 Wyoming is included in this figure.

Table 2.14: Placebo Test Ratios for States Adopting Incentives Partway Through 2002 and 2008

		<u>Treated States</u>								
State		Productions	Employment		Establishments					
NM		68.0 [1st]**	77.8 [1st]**		13.8 [4th]*					
LA		4.7 [29th]	14.2 [3rd]*		4.5 [15th]					
		<u>Other States</u>								
State	Year	Productions	Employment	Establishments	State	Year	Productions	Employment	Establishments	
AK	2008	6.0	1.4	0.3	MI	2007	13.9	0.2	6.7	
AZ	2006	0.6	2.6	0.1	MN	2006	12.2	1.2	1.3	
CO	2006	14.5	0.3	2.5	MS	2004	0.6	0.9	5.2	
CT	2006	14.2	0.4	1.7	MT	2005	11.3	0.5	6.1	
DC	2007	5.1	3.8	1.6	NJ	2005	0.5	15.3	1.7	
FL	2003	0.2	1.1	11.1	NY	2004	3.7	1.7	0.9	
GA	2005	0.6	2.0	3.6	OR	2005	6.1	2.3	7.1	
ID	2008	6.1	0.2	2.0	PA	2004	0.1	0.2	1.0	
IL	2004	6.1	3.1	0.4	SC	2004	8.0	9.4	6.4	
IN	2007	4.2	0.5	2.0	TN	2006	20.1	1.3	3.8	
IA	2007	10.2	2.1	1.2	TX	2005	15.6	0.8	1.5	
KS	2007	1.8	0.2	3.1	VT	2006	6.5	1.3	1.8	
ME	2006	4.1	11.9	4.5	WA	2006	24.5	4.3	6.4	
MD	2005	0.4	9.8	0.1	WV	2008	12.3	0.6	1.7	
MA	2006	0.9	9.7	3.1	WI	2008	5.1	1.2	5.1	
					WY	2007	16.8	1.8	36.5	

See notes to Table 2.4. For the 31 states presented here that adopted incentives between 2002 and 2008, MSPE is calculated up to the year before incentives were adopted, which is the year before the year presented in the second column. There are six states (AR, HI, MS, NC, OK, RI) not eligible for a placebo synthetic control because they had an MPPI for the entire post-treatment period.

Table 2.15: Effects on Number of Productions Filmed, Average over all MPPIs, Using State-Specific Linear Time Trends

	Average Effect (1)	Up to 1 Year (2)	Up to 3 Years (3)	Up to 5 Years (4)
Refundable or Transferable	0.0081 (0.0330)	0.0528 (0.0503)	-0.0156 (0.0664)	-0.0048 (0.0991)
Neither	0.1136*** (0.0191)	0.1198** (0.0533)	0.1319*** (0.0286)	-0.7293*** (0.0553)

The only difference between this table and Table 2.5 is the addition of state-specific linear time trends as a controls.

Table 2.16: Effects on the Number of Productions Filmed, Using State-Specific Linear Time Trends

	Average Effect (1)	Up to 1 Year (2)	Up to 3 Years (3)	Up to 5 Years (4)
Resident $\times$ <i>Refund</i>	0.0020 (0.0018)	0.0073* (0.0044)	0.0167*** (0.0062)	0.0256** (0.0111)
Non-Resident $\times$ <i>Refund</i>	0.0002 (0.0031)	-0.0065 (0.0046)	-0.0012 (0.0046)	-0.0077 (0.0090)
Non-Labor $\times$ <i>Refund</i>	-0.0025 (0.0032)	-0.0016 (0.0055)	-0.0202*** (0.0069)	-0.0245* (0.0127)

The only difference between this table and Table 2.6 is the addition of state-specific linear time trends as a controls.

Table 2.17: Effects on Employment in Motion Picture Production, Average over all MPPIs, Using State-Specific Linear Time Trends

	Average Effect (1)	Up to 1 Year (2)	Up to 3 Years (3)	Up to 5 Years (4)
Refundable or Transferable	0.0504 (0.0500)	0.1117** (0.0456)	0.1632* (0.0875)	0.2059 (0.1374)
Neither	-0.4455*** (0.1467)	-0.3073** (0.1299)	-0.7253*** (0.0542)	-0.6544*** (0.1216)

The only difference between this table and Table 2.7 is the addition of state-specific linear time trends as a controls.

Table 2.18: Effects on Employment in Motion Picture Production by Incentive Rate and Category, Using State-Specific Linear Time Trends

	Average Effect (1)	Up to 1 Year (2)	Up to 3 Years (3)	Up to 5 Years (4)
Resident $\times$ <i>Refund</i>	0.0073 (0.0038)	0.0056 (0.0040)	-0.0017 (0.0104)	-0.0049 (0.0130)
Non-Resident $\times$ <i>Refund</i>	0.0025 (0.0051)	0.0025 (0.0051)	-0.0021 (0.0100)	-0.0025 (0.0189)
Non-Labor $\times$ <i>Refund</i>	-0.0018 (0.0051)	-0.0034 (0.0055)	0.0123 (0.0130)	0.0171 (0.0185)

The only difference between this table and Table 2.8 is the addition of state-specific linear time trends as a controls.

Table 2.19: Effects on Establishments in Motion Picture Production, Average over all MPPIs, Using State-Specific Linear Time Trends

	Average Effect (1)	Up to 1 Year (2)	Up to 3 Years (3)	Up to 5 Years (4)
Refundable or Transferable	0.0061 (0.0263)	0.0192 (0.0204)	0.0241 (0.0270)	0.0600 (0.0450)
Neither	-0.1778 (0.1189)	-0.1045 (0.1021)	-0.3251*** (0.0619)	-0.4601*** (0.0650)

The only difference between this table and Table 2.9 is the addition of state-specific linear time trends as a controls.

Table 2.20: Effects on Establishments in Motion Picture Production by Incentive Rate and Category, Using State-Specific Linear Time Trends

	Average Effect (1)	Up to 1 Year (2)	Up to 3 Years (3)	Up to 5 Years (4)
Resident $\times Refund$	-0.0005 (0.0014)	0.0004 (0.0018)	-0.0028 (0.0049)	-0.0030 (0.0068)
Non-Resident $\times Refund$	-0.0028 (0.0022)	-0.0026 (0.0019)	-0.0005 (0.0033)	-0.0035 (0.0051)
Non-Labor $\times Refund$	0.0040** (0.0017)	0.0038 (0.0023)	0.0076 (0.0054)	0.0123* (0.0073)

The only difference between this table and Table 2.10 is the addition of state-specific linear time trends as a controls.



## Appendix 2: Detailed History of Motion Picture Production Incentives in U.S. States

The tables in this appendix are organized as follows:

**From, To:** Indicates the time period that this particular version of the MPPI was active. The same program often appears over several lines as either an act makes changes or as an existing part of the act takes effect. The dates listed are those for which the program is in effect with this specific criteria, and not necessarily the dates over which the law is in effect, since the statute may specify particular dates for when the program or certain program criteria are active. A dash in the “To” field indicates that this particular iteration of the program is still active as of September 1, 2013.

**Expenditure Rates:** These are the rates applied to the three main types of expenditure: in-state non-labor expenditure (e.g., set construction, wardrobe, rentals), resident labor (payroll for residents of the state), and non-resident labor. The fourth column presents bonus rates, if they are available. Bonus rates either apply to all “qualified expenditure” or only apply to certain types (e.g., payroll of students). Unless otherwise stated, the bonus rates apply to all expenditure.

**Refundable, Transferable, Carry Forward:** Indicates if the credit is refundable or transferable, and indicates the years the credit can be carried forward, if applicable. If the MPPI is a cash rebate instead of a tax credit, then “Rebate” is written across all three lines, as these characteristics are irrelevant (the cash rebate is roughly equivalent to a refundable tax credit). Some incentives are described as grants, so “Grant” is used similarly in the tables and is identical in practice to “Rebate”.

**Rules & Restrictions:** Lists any restrictions to qualified expenditure, minimum or maximum expenditures required for eligibility, or other restrictions.

**Statute:** Lists the legal citation for the statute that contained this MPPI.

**Act:** List is the act that either created this MPPI or amended it.

In comparing one state's program over time using these tables, all information above applies unless otherwise stated. So all rows after the first row for the state only mention changes. A blank field indicates no change from above, while a non-empty cell indicates a change. In almost all cases, each state has one program at a time. When there are two programs active at one time (as manifested by two separate statutes, each with a program), then program changes are listed chronology, so the above row may be information the other program. If this is the case then all cells will be filled in with information. The two programs can be distinguished by the different statutes that will be listed. This is the case for Louisiana, for example, which had a separate program for "digital interactive media".

List of abbreviations:

ATL: "Above-the-line" workers. Refers to principal actors, producers, writers, and directors.

BTL: "Below-the-line" workers. Refers to all workers that are not above-the-line.

	From	To	Expenditure Rates							Rules & Restrictions	Statute	Act
			In-State Non-Labor	Resident Labor	Non-Resident Labor	Bonus Rates	Refundable	Transferable	Carry Forward			
AL	01-Jan-09	18-Apr-12	25%	35%	25%	N/A	Yes	No	No	Expenditure of \$0.5m to \$10m to qualify. For soundtrack projects: \$50k to \$300k.	Ala. Code §41-7A-40 to -48	Act No. 2009-145
	14-Jun-11	18-Apr-12								At least \$0.5m, or \$50k for a music video or soundtrack. A series is now considered one production. Max. \$10m in qualified expenditure could be claimed		Act No. 2011-695
	19-Apr-12	-								Max. \$20m in qualified expenditure could be claimed		Act No. 2012-212
AK	02-Sep-08	30-Jun-13	30%	40%	30%	N/A	No	Yes	6 Years	≥ \$100k over 24 months	§43.98.030, §44.33.232 et seq.	SLA 2008, ch. 63
	01-Jul-13	-		50%	5% (ATL), 30% (BTL)			Yes*		≥ \$75k over 36 months	§43.98.030, §44.25.100 et seq.	SLA 2012, ch. 51
AZ	01-Jan-06	23-May-07	10%/15%/20%		0%	N/A	No	Yes	5 Years	Min. \$250k. Need > \$1m for 15% rate, > \$3m for 20%. Several productions could be grouped to meet expenditure requirements. Residents must be paid at least \$5k to qualify. Max. benefit of \$5m, \$7m in 2008, \$8m in 2009, \$9m in 2010. ≥ 25% of FT employees must be residents. 35% in 2007, and 50% after	§41-1517, §43-1163	2005 Ch. 317, 2006 Ch. 222
	24-May-07	31-Dec-10	20%/30%		0%					Need > \$1m for 30% rate		2007 Ch. 225

\*At 75% rate.

	From	To	Expenditure Rates					Refundable	Transferable	Carry Forward	Rules & Restrictions	Statute	Act
			In-State Non-Labor	Resident Labor	Non-Resident Labor	Bonus Rates							
AR	25-Feb-83	13-Apr-87	5%	5%	0%	N/A				Rebate	> \$1m within 12 months	§84-4805 to -4806	Acts of 1983, Act 276
	14-Apr-87	07-Apr-91									Pre/post production allowed	§26-4-206 to -208	Acts of 1987, Act 1032
	08-Apr-91	30-Jun-93									> \$1m within 12 months or \$0.5m within 6		Acts of 1991, Act 989
	03-Apr-09	31-Jul-13	15%	15% (ATL), 25% (BTL)	15%						> \$1m within 6 months. Only employees earning < \$0.5m included.	§15-4-2003 to -2008	Acts of 2009, Act 816
	01-Aug-13	-									Amended to > \$200k within 6 months.		Acts of 2013, Act 496
CA	01-Jan-11	-	20%	20%	20%	5%*	No	No†	5 Years		Feature films or TV Series: \$1m-\$75m, "Movies of the Week" or mini-series: >0.5m. ≥ 75% of production days in CA. Credits allocated by lottery.	CA Rev. & Tax. §17053.85, §23685	Stats.2009-2010, 3rd Ex.Sess., c.17
CO	05-Jun-06	30-Jun-09	10%	10%	0%	N/A				Rebate	≥ \$100k if production originated in CO, \$1m otherwise. ≥ 75% of both expenditure and payroll must be spent in CO.	§24-46-105.8	Laws 2006, Ch. 336
	01-Jul-09	17-May-10									Only up to \$3m per employee eligible	§24-48.5-201 to -203.	Laws 2009, Ch. 419
	18-May-10‡	31-Dec-10			10%						Minimums amended to \$100k or \$250k. In-state production must include ≥ 25% residents.		Laws 2010, Ch. 232
	01-Jul-12	-	20%	20%	20%						Amended to ≥ 50%.	§24-48.5-114 to -116	Laws 2012, Ch. 186

\* 25% rate for indie films or TV series that filmed all prior seasons outside CA.

† Only transferable for indie films.

‡ Effective for productions that apply on or after this date.

		Expenditure Rates										
	From	To	In-State Non-Labor	Resident Labor	Non-Resident Labor	Bonus Rates	Refundable	Transferable	Carry Forward	Rules & Restrictions	Statute	Act
CT	01-Jan-06	31-Dec-06	30%	30%	30%	N/A	No	Yes	3 Years	≥ \$50k	§12-217JJ	P.A. 06-83
	01-Jan-07	31-Dec-08	30%	30%	30%					Compensation in excess of \$15m per individual not qualified.		P.A. 07-236
	01-Jan-09	31-Dec-10	30%	30%	30%					Half of out-of-state expenditure eligible for credit if it is used in the state.		P.A. 07-236
	01-Jan-10	30-Jun-10	10%/15%/30% for all							Min. \$100k. Need ≥ \$0.5m for 15%, > \$1m for 30%. Compensation limit amended to \$20m. Out-of-state spending no longer eligible. ≥ 50% of principal photography days of post-production costs must be within the state.		P.A. 09-3
	01-Jul-10	-								Amended to 25% of principal photography days, 50% of post-production costs, or ≥ \$1m in post-production costs.		P.A. 10-107
DE	Never											
DC	14-Mar-07	14-Oct-09	10%*	10%*	0%	N/A		Rebate		≥0.5m exp. and 5 filming days	§§39-501 & -502	D.C. Act 16-649, Act 17-381
	15-Oct-09	-	21%/42%†	0% (ATL), 30% (BTL)	42% (ATL), 0% (BTL)					≥ \$250k expenditure		D.C. Act 18-207

\* Lesser of 10% of qualified expenditure or a sales and use tax exemption.

† 42% for expenditures subject to DC taxation, 21% otherwise.

		Expenditure Rates									
	From	To	In-State Non-Labor	Resident Labor	Non-Resident Labor	Bonus Rates	Refundable	Carry Forward Transferable	Rules & Restrictions	Statute	Act
FL	01-Jul-03	30-Jun-05	15%	15%	0%	5%*			Min. \$850k expenditure required. Excludes top two highest paid actors. Max. benefit of \$2m for a motion picture, \$450k for a production ≥ 90 min., \$150k for a production < 90 min., \$25k for a music video or commercial, \$15k for an industrial or educational film.	§288.1254	Ch. 2003-81
	01-Jul-05	30-Jun-07							Max. benefit amended to \$2m for all cases, with \$200k maximum for each of the bonus rates.		Ch. 2005-234
	01-Jul-07	30-Jun-10	10%/15%†		0%	2-7%‡			Can claim up to \$400k in compensation per resident (\$200k for digital media products). Min. expenditure required of \$625k, \$300k for digital media products, \$100k for a commercial or music video, with \$0.5m spent on commercials or music videos within the fiscal year. Max. payouts amended to \$8m for productions in the general queue (\$0.5m for a commercial or music video) and \$1m in the digital media products queue. Above runtime requirements removed. At least 50% of cast and BTL crew must be residents. Other eligibility restrictions apply for the independent queue.		Ch. 2007-125

\* 5% of gross revenues for the first 12 months for companies that relocate to FL and bonus 5% for qualified expenditures for “digital media effects” companies in FL. Max. \$200k awarded for each of these bonus rates.

† 10% rate for productions in the digital media queue, 15% for productions in the general or “Independent Florida filmmaker” queue.

‡ +2% for “family friendly” productions, +5% for off-season production for productions in the general queue.

		Expenditure Rates										
	From	To	In-State Non-Labor	Resident Labor	Non-Resident Labor	Bonus Rates	Refundable	Transferable	Carry Forward	Rules & Restrictions	Statute	Act
FL	01-Jul-10	30-Jun-11	20%	20%	0%	5-10%*	No†	Yes	5 Years	Credits not redeemable until July 1, 2011. Cast and crew residency requirement amended to 75% for digital media, and increased for non-digital media to 60% effective July 1, 2012.		Ch. 2010-147
	01-Jul-11	-				5-35%‡				Maximum benefit of 30% of actual qualified expenses.		Ch. 2011-76
GA	01-Jan-05	31-Dec-07	9%	12%	9%	2-8%¶	No	Yes	5 Years	Min. \$0.5m in expenditure required. Max. \$0.5m in compensation per employee could be claimed.	§48-7-40.26	Laws 2005, Act 356
	01-Jan-08	-	30%#	30%	30%					Max. benefit of \$5m. Above compensation limit only applies to W-2 employees (not 1099).		Laws 2008, Act 469
HI	01-Jan-97	30-Jun-06	Up to 4%	Up to 4%	Up to 4%	2%	Yes	No	No		§235-17	Laws 1997, ch. 107
	01-Jul-06	30-June-13	15%	15%	15%	5%◇				Min. \$200k expenditure required		Laws 2006, ch. 88
	01-Jul-13	-	20%	20%	20%							Laws 2013, ch. 89

\* Both family friendly and off-season bonuses now +5% each.

† The statute states that this tax credit is refundable at a 90% rate, but this is not funded, so in practice this tax credit is not refundable.

‡ +5% for "family friendly", +5% for off-season, +5% if at least 2/3 of filming in an under-utilized region, +5% for expenditure at a qualified production facility in FL if at least half the principal photography occurs there, +15% on compensation for students or recent graduates of a film-related program.

¶ +2% if expenditure exceeds \$20m for multiple TV projects, +3% if base investment in GA ≥ \$20m, +3% for investments in less developed counties.

# Technically all these rates are 20%, with +10% if a GA promotional logo is included in the finished product.

|| Up to 6% for transient accommodations.

◇ +5% for counties other than Honolulu.

	From	To	Expenditure Rates				Refundable	Transferable	Carry Forward	Rules & Restrictions	Statute	Act
			In-State Non-Labor	Resident Labor	Non-Resident Labor	Bonus Rates						
ID	01-Jan-08	-	20%	20%	20%	N/A			Rebate	≥ \$200k expenditure required. Max. benefit of \$0.5m. At least 20% crew must be ID residents. 25% for July 1, 2010 to June 30, 2011; 30% for July 1, 2011 to June 30, 2012; 35% thereafter.	§67-4728	S.L. 2008, ch. 350
IL	01-Jan-04	31-Dec-04	0%	25%	0%	N/A	No	No	No	Only first \$25k in wages per employee qualify. Excludes the salaries of the two highest paid employees of the production. For productions < 30 min. must spend ≥ \$50k, otherwise \$100k.	35 ILCS 15/10	P.A. 93-0543
	01-Jan-05	30-Apr-06							Yes	5 Years		P.A. 94-0171
	01-May-06	31-Dec-08	20%	20%								P.A. 94-0817
	01-Jan-09	-	30%	30%								P.A. 95-1006
IN	01-Jul-07	31-Dec-11	15%	15%	0%	N/A	Yes	No	No	Compensation paid to directors, producers, screenwriters, or actors only included if they were IN residents. Min. expenditure of \$100k, or \$50k for an audio recording, music video, advertisement, or internal media. Max. benefit of \$900k.	§6-3.1-32	P.L. 235-2007

\* +10% for wages of employees who are IL residents and reside in areas of high poverty or unemployment.

† +15% for wages of employees who are IL residents and reside in areas of high poverty or unemployment.



	From	To	Expenditure Rates							Carry Forward	Rules & Restrictions	Statute	Act
			In-State Non-Labor	Resident Labor	Non-Resident Labor	Bonus Rates	Refundable	Transferable					
IA	01-Jan-07	17-May-09	25%	25%	0%	N/A	No	Yes	No	Does not include compensation for directors, producers, or cast members other than extras.	§15.391 to .393	Acts 2007 (82 G.A.) ch. 162	
	18-May-09	23-Nov-09*	Up to 25%	Up to 25%						The salaries of the principal producer, director, and cast member could be counted if they were IA residents and their compensation fell below a caps that was a function of expenditure.		Acts 2009 (83 G.A.) ch. 109	
KS	01-Jan-07	31-Dec-08	30%	30%	30%	N/A	No	No	3 Years	Expected expenditure of at least \$100k, or \$50k if < 30 mins.	§79-32,257-260	Laws 2007, Ch. 184	
	01-Jan-09	31-Dec-10	0%	0%	0%					Program suspended for tax years 2009 and 2010.		Laws 2009, Ch. 142	
	01-Jan-11	31-Dec-12	30%	30%	30%								
KY	01-Jul-10	-	20%	20%	20%	N/A	Yes	No	No	≥ \$0.5m expenditure required for a motion picture, \$200k for a commercial, \$50k for a documentary. Max. \$100k can be claimed for each actor, director, producers, and writers.	§148.542 et seq.	2009 Ch. 1	

\* Program was suspended due to allegations of fraud.

		Expenditure Rates								Rules & Restrictions	Statute	Act
From	To	In-State Non-Labor	Resident Labor	Non-Resident Labor	Bonus Rates	Refundable	Transferable	Carry Forward				
LA	01-Jun-98	30-Jun-00	0%	10%/20%*	0%	N/A	No	No	No		R.S. 47:1121 et seq.	1998 Act No. 55
	01-Jul-02	31-Dec-05								Does not include salaries $\geq$ \$1m.		2002 Act No. 1
	01-Jul-02	30-Jun-03	10%	10%	10%	5%†	No	No	10 Years	$\geq$ \$300k of base investment required. This was defined as 85% of the funds actually invested in the state.	R.S. 47:6007	2002 Act No. 6
	01-Jul-03	31-Dec-03						Yes		Minimum amended to $\geq$ \$300k of actual expenditure.		2003 Act No. 1240
	01-Jan-04	31-Dec-05				5%‡						2003 Act No. 1240
	01-Jan-06	31-Dec-06	25%	35%	25%	0%				Salaries of \$1m or more not eligible for the extra 10% bonus for LA resident compensation.		2005 Act No. 456
	01-Jan-06	30-Jun-09	10-20% for all¶			0%	No	Yes	10 Years	Certified projects in "Digital Interactive Media" only.	R.S. 47:6020	2005 Act No. 346
	01-Jan-07	30-Jun-09	25%	35%	25%	0%	Yes			Salaries of \$1m or more not eligible for the extra bonus for LA resident compensation.	R.S. 47:6007	
	01-Jul-09	-	30%		30%		Yes◇					2009 Act No. 478
	01-Jul-09	10-July-11	25%	35%	25%		No			Certified projects in "Digital Interactive Media" only.	R.S.47:6020	2009 Act No. 478
	11-July-11	-					Yes					2011 Act No. 415

\* 20% rate if aggregate payroll  $>$  \$1m.

† +5% if base investment  $>$  \$1m.

‡ +5% if base investment  $>$  \$8m.

¶ 20% for first two years following certification, 15% for the third and fourth years, 10% for the fifth and sixth years.

|| At 72% rate, 74% from January 1, 2009 to June 30, 2009.

◇ At 85% rate.

		Expenditure Rates										
	From	To	In-State Non-Labor	Resident Labor	Non-Resident Labor	Bonus Rates	Refundable	Transferable	Carry Forward	Rules & Restrictions	Statute	Act
ME	29-Mar-06	27-Sep-11	10%	12%	10%	N/A		Rebate		Only \$1m in compensation per employee could be claimed. $\geq$ \$250k expenditure over 12 months required. Rebate could not exceed tax liabilities.	5 MRSA §13090-L, 36 MRSA §5219-Y, 36 MRSA c. 919-A, §6901 & §6902.	P.L. 2005, c. 519
	28-Sep-11	-	5%									
MD	01-Jul-05	30-Jun-07	0%	50%	0%	N/A		Rebate		Only the first \$25k per employee could be claimed. Employees earning $\geq$ \$1m not included. Max. benefit of \$2m per production.	Art. 83A, Subtit. 18, §5-1801 et seq.	Acts 2005, c. 96
	01-Jul-07	30-Sep-08†	Up to 25%	Up to 25%	Up to 25%					Restrictions on compensation removed.		Acts 2007, c. 87
	01-Jul-11	-	25%	25%	25%	2%*	Yes	No	No		Article - Tax - General, §10-729 et seq.	Acts 2011 c. 516, Acts 2013, c. 28
MA	01-Jan-06	31-Dec-06	25%	20%	20%	N/A	No	Yes	5 Years	$\geq$ \$250k expenditure within 12 months required for eligibility. Max. benefit of \$7m. Tax credit for non-payroll expenditures allowed only if either $\geq$ 1/2 of expenditures or 1/2 the principal photography days occurred in MA. Salaries above \$1m not considered.	MA ST Ch. 62	Laws 2005 Ch. 158
	01-Jan-07	-	25%	25%	25%		Yes‡			$\geq$ \$50k expenditure required.		Laws 2007, Ch. 63

\* +2% for a TV series.

† Repealed by Acts 2008, c. 306

‡ At 90% rate.

		Expenditure Rates										
	From	To	In-State Non-Labor	Resident Labor	Non-Resident Labor	Bonus Rates	Refundable	Transferable	Carry Forward	Rules & Restrictions	Statute	Act
MI	01-Jan-07	31-Dec-09*	12%/16%/20%†		0%	-	Yes	No	No	Only \$100k per employee considered. $\geq$ \$200k in expenditure required.	MCL 205.54cc	P.A. 2006, No. 657
	21-Dec-10	30-Sep-11	40%	30%	30%	2%‡		Yes		Only \$2m per employee considered. $\geq$ \$50k in expenditure required.	MCL 208.1455	P.A. 2010, No. 312
	01-Oct-11	31-Dec-12	27%	32%	25%	3%¶		Rebate		The producer fees that could be claimed were capped at 5% of the payroll of Michigan personnel, or 10% if the producer was a MI resident.		P.A. 2011, No. 291
	01-Jan-13	-			20%					Other rates set to change in 2014 and 2015.		
MN	01-Jul-97	31-Dec-00	5%	5%	5%	N/A		Rebate		Max. benefit of \$100k	116J.543	Laws 1997, c. 200
	01-Jan-01	30-Jun-02	10%	10%	10%							Laws 2001, 1st Sp., c. 4
	01-May-06	30-Jun-07	Up to 15%	Up to 15%	Up to 15%						116U.26	2006 Ch. 282
	30-May-08	30-Jun-10	Up to 20%	Up to 20%	Up to 20%					$\geq$ \$5m expenditure within 12 months required.		2008 Ch. ?
	01-Jul-10	22-May-13	Up to 15%	Up to 15%	Up to 15%	5%				Minimum removed.		2010 Ch. 215
	23-May-13	-	Up to 20%	Up to 20%	Up to 20%	5%◇				Only \$100k in compensation per individual could be claimed.		HF729

\* Repealed by P.A. 2009, No. 78

† 16% if expenditure  $\geq$  \$1m, 20% if  $\geq$  \$10m, but only the first \$10m receives this 20% rate.

‡ +2% if production occurs in a "core community".

¶ +3% if production occurs in a "core community".

|| +5% if either production is located outside the metropolitan area or expenditure exceeds \$5m.

◇ Same as above except \$1m instead of \$5m.

		Expenditure Rates										
	From	To	In-State Non-Labor	Resident Labor	Non-Resident Labor	Bonus Rates	Refundable	Transferable	Carry Forward	Rules & Restrictions	Statute	Act
MS	01-Jul-04	12-Mar-07	10%	10%	0%	N/A	No*	No	10 Years*		§57-89-3 et seq.	2004 Ch. 528
	13-Mar-07	08-May-08	20%/25%/30%†		10%				Rebate	Non-resident compensation only included for workers who made < \$1m. Max. benefit of \$5m.		2007 Ch. 324
	09-May-08	16-Mar-11	20%	25%	20%					Up to \$1m in compensation per employee can be claimed. ≥ \$20k in expenditure required. Max. benefit of \$8m.		2008 Ch. 524
	17-Mar-11	11-Apr-13	25%	30%	25%					≥ \$50k in expenditure required.		2011 Ch. 453
	12-Apr-13	-				5%‡				Max. benefit of \$10m		2013 Ch. 490
MO	01-Jul-99	27-Aug-04	Up to 50%	Up to 50%	Up to 50%	-	No	Yes	5 Years	Min. ≥ \$300k in expected expenditure. Max. benefit of \$0.5m and one project per company per year.	135.75	L.1998 S.B. No. 827
	28-Aug-04	31-Dec-08								Max. benefit increased to \$1m.		L.2004, S.B. No. 1394
	01-Jan-08	-	35%	35%	30%					Cannot claim any compensation for employees earning > \$1m. Min. ≥ \$50k expenditure, or \$100k if > 30 minutes.		L.2007, 1st Ex. Sess., H.B. No. 1
MT	06-May-05	02-May-07	8%	12%	0%	N/A	Yes	No	4 Years¶	Only the first \$50k per resident qualifies. Max. benefit of \$1m	§15-31-907 et seq.	Laws 2005, ch. 593
	03-May-07	-	9%	14%						Max. benefit removed.		Laws 2007, ch. 367

\* This incentive is a tax credit for in-state non-labor expenditure, which has a carry forward, and a rebate for resident labor.

† The first \$1m of “base investment” (which does not include non-resident labor) receives the 20% rate. The next \$4m receives 25%, and any beyond \$5m receives the 30% rate.

‡ +5% for the payroll of honorably discharged veterans of the United States Armed Forces.

¶ Carry forward only for resident payroll.

	From	To	Expenditure Rates					Refundable	Transferable	Carry Forward	Rules & Restrictions	Statute	Act
			In-State Non-Labor	Resident Labor	Non-Resident Labor	Bonus Rates							
NE	Never*												
NV	01-Jan-14	-	15%	15%	12%	2-4%†	No	Yes	No	Compensation to producers must not exceed 5% of expenditure, or 10% if the producer is a NV resident to be claimable. Min. \$0.5m in expenditure required. Max. benefit of \$6m per production. ≥ 60% of expenditures must occur in NV.	§360.2?	Laws 2013, Ch. ?	
NH	Never												
NJ	01-Jul-05	10-Jan-08	20%	20%	0%	N/A	No	Yes	7 Years	Max. benefit of 50% of tax liability. ≥ 60% of expenditure must take place in NJ. Must be ≥ 15 minutes and aimed at a national audience	C.54:10A-5.39, CA54A:4-12	Laws 2005, Ch. 345	
	11-Jan-08	28-Jun-10								Min. \$2m expenditure required. A “significant” percentage of expenditure must be for compensation of full-time NJ residents.		Laws 2006, Ch. 257	
	28-Jun-10	-								Tax credits temporarily non-redeemable for FY 2011.	Added C.54:10A-5.39a, CA54A:4-12a	Laws 2010, Ch. 20	

\* On April 5, 2012, the governor approved LB 863, which amended the Local Option Municipal Economic Development Act to allow certain cities and villages to collect property taxes or local sales taxes, if approved by voters, to fund economic development projects, which could now include an MPPI. The three largest cities do not fall under this act.

† +2% if more than half the BTL personnel are NV residents. +2% if more than half the filming days occur in a county that has less than \$10m in direct production expenditures in the last two years.

		Expenditure Rates								Rules & Restrictions	Statute	Act
From	To	In-State Non-Labor	Resident Labor	Non-Resident Labor	Bonus Rates	Refundable	Transferable	Carry Forward				
NM	01-Jan-02	30-Jun-03	15%	15%	0%	N/A	Yes	No	No		§7-2F-1 et seq.	L. 2002, Ch. 36
	01-Jul-03	31-Dec-05			15%*							
	01-Jan-05	30-Jun-06	15/20%†	15/20%†							Added §7-2G-1‡	L. 2005, Ch. 337; L. 2006, Ch. 78
	01-Jan-06	30-Jun-07	25%¶	25%¶	0%							L. 2006, Ch. 78
	01-Jul-07	30-Jun-11								Max. claim of \$5m in expenditure for performing artists.		L. 2007, Ch. 172
	01-Jul-11	-	25/30%	25/30%◇						Cannot claim this incentive along with the sales tax exemption.		L. 2013, Ch. 160

\* Only performing artists.

† 20% rate only for a TV series with  $\geq 60\%$  of BTL crew payroll paid to NM residents.

‡ This bonus program was added as a separate statute, and then repealed effective July 1, 2006.

¶ Any expenditure that received the federal New Markets Tax Credit gets a 20% rate.

|| 30% rate for a TV series with  $\geq$  six episodes and a budget  $\geq$  \$50k per episode.

◇ 30% rate can be achieved for a TV series with  $\geq$  six episodes and a budget  $\geq$  \$50k per episode or for labor costs of non-performing artists for productions that shoot  $\geq 10$  days at a NM production facility, or  $\geq 15$  days if the budget  $>$  \$30m.

		Expenditure Rates										
From	To	In-State Non-Labor	Resident Labor	Non-Resident Labor	Bonus Rates	Refundable	Transferable	Carry Forward	Rules & Restrictions	Statute	Act	
NY	01-Jan-04	22-Apr-08	10%	10%	10%	N/A	Yes	No	No	≥ 75% of expenditure must be associated with a qualified production facility in NY.	N.Y Tax Code Ch. 60, Art. 1, sec. 24	L. 2004, c. 60
	23-Apr-08	10-Aug-10	30%	30%	30%							L. 2008, c. 57
	11-Aug-10	-				5%*				Post-production added in separate statute. ≥ 10% of principal photography days must be at a qualified facility (except if a qualified independent film company).	Added N.Y Tax Code Ch. 60, Art. 1, sec. 31	L. 2010, c. 57
NC	02-Aug-00	30-Oct-02	15%	15%	15%	N/A		Grant		Max. benefit of \$200k per production.	§143B-434.3	S.L. 2000-153
	31-Oct-02	30-Jun-03								≥ \$1m expenditure required.		S.L. 2002-172
	01-Jul-03	30-Jun-05									Relocated to §143B-434.4	
	01-Jul-05	31-Dec-09					Yes	No	No	≥ \$250k expenditure required. Max. benefit of \$7.5m if a feature film. Does not include individuals earning > \$1m.	§105-130.47, §105-151.29	S.L 2005-276
	01-Jan-10	22-Jul-10	25%†	25%†	25%†	N/A	Yes	No	No	≥ \$250k expenditure required. Benefit reduced by what would have been paid in sales or use tax.		S.L 2009-529
	22-Jul-10	-	25%	25%	25%					Max. benefit increased to \$20m.		S.L 2010-147

\* +5% for post-production expenditure in upstate NY.

† This 25% rate was an alternative credit briefly added to the statute. The criteria listed applies to this credit and not the original 15% credit is as listed above. This was removed effective



		Expenditure Rates										
From	To	In-State Non-Labor	Resident Labor	Non-Resident Labor	Bonus Rates	Refundable	Transferable	Carry Forward	Rules & Restrictions	Statute	Act	
ND	Never											
OH	01-Jul-09	-	25%	25/35%*	25%	N/A	Yes	No	No	≥ \$300k expenditure required. Max. benefit of \$5m.	§122.85, §131.02	2009 H 1
OK	01-Jul-01	30-Jun-02	15%	15%	0%	N/A		Rebate	Cannot take this incentive and the sales and use tax exemption at the same time.	§3621 tit. 68	Laws 2001, Ch. 259	
	01-Jul-02	05-Jun-05							Min. budget of \$1m.		Laws 2002, Ch. 203	
	06-Jun-05	30-Jun-06	5/10/15%†						Min. budget of \$2m, of which ≥ \$1.25m spent in OK.		Laws 2005. c. 259	
	01-Jul-06	30-Jun-07	5/10/15%‡						No more than 25% of expenditure can be for ATL salaries.		Laws 2006. c. 29	
	01-Jul-07	30-Jun-09	5/10/15%¶						Min. budget of \$0.5m, of which not less than \$300k spent in OK.		Laws 2007. c. 341	
	01-Jul-09	-	35%	35%		2%			≥ \$50k expenditure required, of which ≥ \$25k is spent in OK.		Laws 2009, c. 426	

\* 35% for cast and crew that are OH residents.

† 5% if < 25% of crew are OK residents, 10% for 25% to 49%, 15% for ≥ 50%.

‡ As above, but 15% rate also achievable by having a budget of ≥ \$30m.

¶ As above, but \$5m.

|| +2% if the company spends ≥ \$20k for music created by an OK resident or recorded in OK.

		Expenditure Rates										
	From	To	In-State Non-Labor	Resident Labor	Non-Resident Labor	Bonus Rates	Refundable	Transferable	Carry Forward	Rules & Restrictions	Statute	Act
OR	04-Nov-05	-	0%	6.2%	6.2%	N/A		Rebate		≥ \$1m expenditure required.	§316.131 et seq.	Laws 2005, Ch. 559
	01-Jul-05	26-Sep-07	Up to 10%	Up to 10%	Up to 10%	N/A	No	Yes	3 Years	≥ \$250k expenditure for a film, \$30k for a TV episode. Benefit capped at the larger of \$1m or tax liability.	§284.300 et seq.	Laws 2003, Ch. 736
	27-Sep-07	26-Sep-09	Up to 20%									Laws 2007, Ch. 815; Laws 2007, Ch. 843
	27-Sep-09	-								Local filmmakers eligible if expenditure ≥ \$75k and ≤ \$750k.		Laws 2009, Ch. 787
PA	01-Jul-04	06-Jul-05	20%	20%	20%	N/A	No	Yes	3 Years	≥ 60% of expenditure must be in PA.	§8701-C et seq.	Act 2004-95
	07-Jul-05	30-Jun-06	Up to 20%	Up to 20%	Up to 20%					Does not include compensation for those earning > \$1m.		Act 2005-40
	01-Jul-06	-	Up to 20%	Up to 20%	Up to 20%			Grant		Does not include compensation for those earning > \$1m. 60% of expenditure must be in PA.	§4101 et seq.	Act 2006-42
	25-Jul-07	01-Jul-12	Up to 25%	Up to 25%	Up to 25%	N/A	No	Yes	3 Years	Can only claim \$15m in compensation. ≥ 60% of expenditure must be in PA.	§8701-D et seq.	Act 2007-55
	02-Jul-12	-								60% requirement could be waived if ≥ \$1.5m (or \$5m if expenditure > \$30m) was spent at a qualified production facility.		Act 2012-85; Act 2013-52

	From	To	Expenditure Rates					Carry Forward	Rules & Restrictions	Statute	Act	
			In-State Non-Labor	Resident Labor	Non-Resident Labor	Bonus Rates	Refundable					Transferable
RI	13-Jul-00	27-Jun-02	25%	25%	25%	N/A	No	No	7 Years	Budget of \$300k to \$5m. The median annual wage paid to full time employees must be above the average annual wage paid by all taxpayers in the state which share the same two-digit SIC Code. Does not reduce the tax due for the year by more than 50% of the tax liability that would be payable, or less than the minimum tax as prescribed in §44-11-2(e) for corporations.	§44-31-1 et seq.	P.L. 2000, Ch. 224
	28-Jun-02	31-Dec-04								Removed wage restriction. Primary filming locations must be in RI.	§44-31.1-1 et seq.	P.L. 2002, Ch. 265
	01-Jan-05	13-Apr-06	15/25%*	15/25%	15/25%				3 Years	Base investment of $\geq$ \$300k. \$5m cap removed.	§44-31.2-1 et seq.	P.L. 2005, ch. 95
	14-Apr-06†	30-Jun-12	25%	25%	25%				Yes			P.L. 2005, ch. 19
	01-Jul-12	-								Budget of $\geq$ \$100k. Max. benefit of \$5m, but could be waived for some feature films and TV series. Either at least 51% of filming must occur in RI or 51% of the budget must be spent in RI and the production must employ at least five individuals in the state.		P.L. 2012, ch. 241

\* 25% if total base investment > \$5m.

† Changes in this act were retroactive to January 1, 2005.

		Expenditure Rates								
From	To	In-State Non-Labor	Resident Labor	Non-Resident Labor	Bonus Rates	Refundable	Carry Forward Transferable	Rules & Restrictions	Statute	Act
SC	01-Jul-04	30-Jun-08	0%	5%	5%	N/A	Rebate	≥ \$1m expenditure required. Cannot claim compensation of ≥ \$1m.	§12-62-10 et seq.	2004 Act No. 299
	01-Jul-08	07-May-13		15%	15%					2008 Act No. 313, 2008 Act No. 359
	08-May-13	-	Up to 30%	25%	20%					2012 Act No. 26
SD	Never									
TN	27-Jun-06	30-Jun-12	17%	17%	17%	N/A	Grant	≥ \$0.5m per production/episode required (\$150k if through a TN-based production company.)	§4-3-4902 et seq.	2006 Ch. 916
	27-Jun-06	30-Jun-12	15%	15%	15%	N/A	Grant	≥ \$1m expenditure required.	§67-4-2109	2006 Ch. 1019
	01-Jul-12	-	≈ 25%*	≈ 25%*	≈ 25%*	N/A		≥ \$200k per production/episode required	§4-3-4902 et seq.	
TX	01-Sep-05	07-Jun-07	0%	20%	0%	5%†	Grant	≥ \$0.5m in wages to TX residents required, or \$50k for commercials. Max. benefit of \$750k. Does not include wages or compensation that are "a major part of the production costs of the entertainment, as determined by the office" or negotiated or spent before production begins.	§485.001 et seq.	2005 Ch. 342

\* "The amount of each grant awarded pursuant to this section shall not exceed twenty-five percent (25%) of the total expenses incurred by a production company for a project; except, however, the department may award grants in excess of this amount if deemed appropriate by the department. It is the legislative intent that funding be appropriated each year in the general appropriations act for awarding grants. It is further the legislative intent that the department strive to award the maximum amount of incentive grants authorized by this section" (§4-3-4903(2))

† +5% if at least 25% of the filming days occur outside the metro areas of Austin, Houston, or Dallas-Fort Worth.

	From	To	Expenditure Rates				Carry Forward Transferable Refundable	Rules & Restrictions	Statute	Act
			In-State Non-Labor	Resident Labor	Non-Resident Labor	Bonus Rates				
TX	08-Jun-07	09-Mar-08	5%	5%	5%	1.25%*	≥ \$1m expenditure required, or \$100k for commercials. Max. benefit of \$2m for a film, \$2.5m for a TV program, \$200k for commercials, \$250k for a digital interactive media production. ≥ 80% of production days in TX, ≥ 70% of both paid crew and cast must be TX residents.		2007 Ch. 260	
	10-Mar-08	31-Oct-09					Only the first \$50k of compensation (\$200k for department heads) was claimable.	Added 13 TAC §§121.1-121.14	33 TexReg 2019	
	01-Nov-09	27-Aug-11	0%	8/17/25%	0%	4.25%†	Texas Wage Option. Must choose this or the Texas Spend Option. Commercials, reality TV, instructional or educational videos, and video games must choose the spend option. Above cast and crew restriction could be waived if the Texas Film Commission determined that qualified crew were not available. Only the first \$1m in compensation per employee could be claimed. ≥ \$250k in expenditure required, or \$100k for commercials, education or instructional videos, or video games.	33 TexReg 2019; 34 TexReg 6725; 2009 Ch. 2		
	01-Nov-09	27-Aug-11	5/10/15%‡ for all			2.5%¶	Texas Spend Option. Same restrictions as above apply		33 TexReg 2019; 34 TexReg 6725; 2009 Ch. 2	
	28-Aug-11	-	5/10/15%   for all						36 TexReg 5201; 37 TexReg 5737	

\* +1.25% if at least 25% of the filming days occur outside the metro areas of Austin or Dallas-Fort Worth.

‡ +4.25% if at least 25% of filming days occur in an underutilized or economically distressed area.

† 10% if expenditure > \$1m, 15% if > \$5m. Commercials, reality TV, instructional or educational videos, and video games only receive 5% rate.

¶ +2.5% if at least 25% of filming days occur in an underutilized or economically distressed area.

|| Commercials, reality TV, instructional or educational videos, and video games now eligible for 10% and 15% rates.

			Expenditure Rates							Rules & Restrictions	Statute	Act	
	From	To	In-State Non-Labor	Resident Labor	Non-Resident Labor	Bonus Rates	Refundable	Transferable	Carry Forward				
UT	01-Jan-09	09-May-11	Up to 20%	Up to 20%	Up to 20%	N/A				Rebate*	Max. benefit of \$0.5m if rebate chosen over tax credit.	§63M-1-1802 et seq.	Laws 2009, c. 135
	10-May-11	-				5%†					≥ \$200k expenditure required.		Laws 2011, c. 338
VT	01-Jul-06	26-May-11	Up to 10%	Up to 10%	Up to 10%	N/A				Grant	Can only claim the first \$1m in compensation per employee. ≥ \$1m expenditure required.	10 V.S.A. Ch. 26 §§651-651g	Laws 2006, No. 184, repealed by Laws 2011, No. 52
VA	01-Jan-11	-	15%	25/35%‡	15%	5-15%¶	Yes	No	No		Can only claim the first \$1m in compensation per employee. ≥ \$250k expenditure required.	§58.1-439.12.03	Acts 2010, c. 419; Acts 2010, c. 599
WA	01-Jul-06	19-Mar-08	20%	20%	20%	N/A				Rebate	≥ \$0.5m expenditure required for a feature film, \$300k for a TV episode, \$250k for an infomercial or commercial. Max. benefit of \$1m per project.	82.04 RCW	Laws 2006 Ch. 247
	20-Mar-08	28-Mar-12	Up to 20%	Up to 20%	Up to 20%						Min. expenditure for infomercials and commercials reduced to \$150k.		Laws 2007-2008, Ch. 85
	15-Apr-09	29-Mar-12	Up to 30%	Up to 30%	Up to 30%								Laws 2009, Ch. 100
	29-Mar-12	-	Up to 30%	Up to 30%	15%	5%◇							2012 Ch. 189

\* Can choose either a cash rebate or a tax credit. The tax credit is neither refundable, transferable, nor has a carry forward.

† +5% if a significant percentage of cast and crew are from UT and certain promotion obligations are met.

‡ 25% if expenditure < \$1m, 35% otherwise.

¶ +5% for filming in an economic distressed area. +10% for compensation for VA residents employed for the first time as actors or crew.

|| Only up to 15% for commercials, unless the production company is based in WA, then 25%. Documents summarizing this incentive mention that resident labor always gets the 30% rate.

◇ Up to 35% for a TV series that films at least six episodes.

	From	To	Expenditure Rates				Bonus Rates	Refundable	Transferable	Carry Forward	Rules & Restrictions	Statute	Act
			In-State Non-Labor	Resident Labor	Non-Resident Labor								
WV	01-Jan-08	07-Mar-08	27%	27%	0%	2-4%*	No	No	2 Years	≥ \$25k expenditure required.	§11-13X-1 et seq.	2007 Ch. 117	
	01-Jan-08	03-May-09				4%†		Yes				2008 Ch. 107	
	01-Jan-08‡	-			27%¶							2009 Ch. 102	
WI	01-Jan-08	31-Dec-08	25%	25%	0%	N/A	Yes	No	No	≥ \$100k for ≥ 30 min. production, otherwise \$50k. Can only claim up to \$25k per employee, and cannot claim to top two earners.	§71.07(5f)	2005 Wisconsin Act 483	
	01-Jan-09	-								≥ \$50k expenditure required. Top two earners rule replaced with rule stating that any employee with compensation > \$250k cannot be claimed. Can only claim up to \$20k per employee. Max. benefit of \$0.5m. ≥ 35% of the production budget must be spent in WI.		2009 Wisconsin Act 28; 2009 Veto Notes	
WY	01-Jul-07	26-Feb-09	12%	12%	0%	1-3%		Rebate		≥ \$0.5m expenditure required.	W.S. 9-12-402 et seq.	2007 Ch. 73	
	27-Feb-09	-								≥ \$200k expenditure required.		2009 Ch. 74	

\* +2% if ≥ ten WV residents were employed full-time. +2% if ≥ of full-time employees were WV residents.

† +4% if ≥ ten WV residents were employed full-time.

‡ These changes applied retroactively to January 1, 2008, but this legislation was not approved until May 4, 2009.

¶ Rule changed to include those who are subject to WV income tax, and not those who are residents.

|| Must use WY props and product placement to achieve 13% rate. Must providing behind the scene footage highlights to achieve 14%. Production must be set in WY to achieve 15%.

## Chapter 3

# Expanding Disability Discrimination Protections to Those With Less Severe Impairments: Evidence from California's Prudence Kay Poppink Act

### 3.1 Abstract

Effective 2001, California passed the Prudence Kay Poppink Act which broadened California's disability discrimination in employment laws to cover individuals with less severe impairments. This is an important legal change as individuals with less severe impairments are often ineligible for Social Security Disability Insurance and are often not covered by the Americans with Disabilities Act, but this group may still face barriers to employment through



discrimination or needing job accommodations. I estimate how the Prudence Kay Poppink Act affected the labor market outcomes for these newly-covered disabled workers using both difference-in-differences and difference-in-differences-in-differences regression analyses using data from the Current Population Survey's Annual Social and Economic Supplement (a.k.a. "the March CPS"). The results show strong evidence of increased employment and weak evidence of increased earnings.

## 3.2 Introduction

While Social Security Disability Insurance (SSDI) and Supplemental Security Income (SSI) are available to those with more severe impairments, those with less severe impairments are much less likely to be eligible for these programs and generally must work to achieve economic independence. However, individuals with less severe impairments still face many barriers to employment such as discrimination and possibly the need for accommodations on the job.

One strategy used to increase the employment of individuals with disabilities is employment discrimination laws. The most notable is Title I of the *Americans with Disabilities Act of 1990* (ADA), effective July 1992. In addition to forbidding discrimination in hiring, terminations, promotion, and wages on the basis of disability, the Title I of the ADA requires employers to reasonably accommodate employees with disabilities by providing physical aids or some job restructuring, so long as this accommodation is reasonable given the nature of the job and size of the firm. The ADA applies to firms with at least 15 employees. The ADA provides three routes for an individual to be considered disabled:

“The term ‘disability’ means, with respect to an individual-

(A) a physical or mental impairment that substantially limits one or more major

- life activities<sup>1</sup> of such individual;
- (B) a record of such an impairment; or
- (C) being regarded as having such an impairment. (42 U.S. Code §12102 (1))”

However, proving that an impairment “substantially limits” a major life activity has proved difficult. Defendants (firms) win the vast majority of cases because plaintiffs (workers) cannot establish that they qualify as disabled under the demanding “substantially limits” standard (Colker, 1999). Burgdorf (1997) (p. 536-538) cites numerous cases stemming from *Forrisi v. Bowen*, 794 F.2d 931, 934 (4th Cir. 1986) which interpreted the ADA to only cover the “truly disabled” and not those with more minor impairments. Adding to this, coverage of the ADA further narrowed after the “Sutton Trilogy” of United States Supreme Court Cases<sup>2</sup>, which excluded individuals with “mitigating measures” such as glasses, medication, or assistive devices, from being considered disabled if the mitigating measure(s) made their condition(s) no longer “substantially limit” a major life activity. Because the requirements to be considered disabled under the ADA was so demanding, a significant portion of individuals with less severe impairments were not covered by disability discrimination laws.

A major change in disability discrimination law in California provides an opportunity to study the implications of expanding legal protection to additional persons with less severe impairments. On September 30, 2000, California passed the Prudence Kay Poppink Act (PKP Act), which became effective January 1, 2001 (see Figure 3.1 for a timeline). This act made the following five changes to the definition of disability under the California’s Fair Employment and Housing Act (FEHA) (Cal Gov Code §12900 et seq.):

1. Changed the requirement that a condition “substantially limits” a major life activity,

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<sup>1</sup>Major life activities were not defined in the original ADA (they were explicitly added later with the *ADA Amendment Act of 2008 (ADAAA)*), but were defined by the Equal Employment Opportunity Commission (EEOC) in its regulations. See Taylor (2009) for a discussion of the evolution of major life activities over time.

<sup>2</sup>*Sutton v. United Airlines* (119 S. Ct. 2139 (1999)), *Murphy v. United Parcel Service, Inc.* (119 S. Ct. 2133 (1999)), and *Albertson’s, Inc. v. Kirkingburg* (119 S. Ct. 2162 (1999)).

- as in part (A) of the ADA definition of disability, to just “limits”;
2. Ignored the *Sutton* trilogy of U.S. Supreme Court cases, thus covering individuals who used mitigating measures even if these mitigating measures caused the individuals to no longer be “substantially limited”;
  3. Explicitly added “working” to the list of major life activities;
  4. Explicitly considered several conditions such HIV/AIDS, hepatitis, epilepsy, seizure disorder, diabetes, clinical depression, bipolar disorder, multiple sclerosis, and heart disease to be disabilities without any requirement that they limit a major life activity; and
  5. Made it a punishable offense for an employer to fail to adequately participate in a timely, and good faith, manner in the interactive process with an employee or applicant to determine effective reasonable accommodations.

The first four changes broadened who was considered disabled and the last change sought to expand reasonable accommodations. Lawyers, employers, disability advocacy groups, and case law<sup>3</sup> all saw the PKP Act as a significantly broadening of who was covered by disability discrimination law in California. The changes resulting from the PKP Act are discussed in more detail in Appendix 1.

The employment and earnings effects of such an expansion of disability discrimination law to those with less severe impairments is unclear. First, economic theory provides ambiguous predictions as to the effects of employment discrimination laws. Economic theory

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<sup>3</sup>For lawyers see <http://www.larryminsky.com/article1.aspx>, <http://kuehlforsupervisor.com/sheila-kuehls-10-bills-that-changed-california/>, <http://www.sohnenandkelly.com/2011/12/18/the-rights-of-applicants-under-fair-employment-law-caaaments/>. For employers see [http://www.puenteconsulting.com/PDFs/Business%20Law%20Practioner\\_Disability%20Article.pdf](http://www.puenteconsulting.com/PDFs/Business%20Law%20Practioner_Disability%20Article.pdf) (all accessed May 11, 2015). For case law see, most notably, *Colmenares v. Braemer Country Club, Inc.*, 63 P.3d 220, 223 (Cal. 2003), but also *Diffey v. Riverside County Sheriff’s Dept.*, 101 Cal. Rptr. 2d 353 (2000).

suggests that these laws should reduce job terminations as terminating an employee, whether there is discriminatory intent or not, opens firms to the risk of legal action (Acemoglu and Angrist, 2001). On the other hand, this increased cost to terminating a protected worker makes hiring a protected worker more costly (Bloch, 1994). Added to this are the costs of reasonable accommodation for disability non-discrimination laws (or the costs of possibly being sued for not providing it), which further increases hiring costs (Acemoglu and Angrist, 2001). While employment discrimination laws forbid discrimination in hiring, it is extremely difficult to prove discrimination in hiring and thus there is less enforcement. It is also more difficult to identify a class of affected workers, and economic damages are smaller than in termination cases, which means that the negative consequences to employers that wish to discriminate in hiring are lower, suggesting negative hiring effects (Neumark, Song and Button, 2015). Since the theoretical hiring and terminations effects move in opposite directions, it is unclear if employment of persons with disabilities would increase or decrease in net.

Similarly, the impact on wages (salary rates for those not paid at an hourly rate) is also ambiguous. Discrimination laws forbid paying workers with disabilities less than their counterparts, which suggests increased wages if this requirement is binding. The expansion of accommodations may also increase wages by increasing the productivity or tenure of the disabled. On the other hand, the increased hiring and firing costs for the disabled would decrease wages of the disabled if the equal pay requirement is non-binding, as some of these costs would be passed on as lower wages for the disabled (Acemoglu and Angrist, 2001).

Second, the empirical evidence on the impacts of disability discrimination laws is ambiguous. This evidence is summarized in Table 3.1. Previous studies found employment effects ranging from negative (DeLeire 2000; Acemoglu and Angrist 2001; Jolls and Prescott 2004) to null (Beegle and Stock 2003; Houtenville and Burkhauser (2004); Jolls and Prescott (2004)) to positive (Kruse and Schur, 2003). Impacts on wages and earnings are also ambiguous, but lean negative. However, only Thompkins (2013) speaks to the expansion (or

removal) of legal protections to those with less severe impairments<sup>4</sup>, a group for which the effects of legal protections likely differs. However, Thompkins (2013) only studies earnings and finds a positive effect.

I estimate the causal impacts of the PKP Act on labor market outcomes of individuals with disabilities (those who report work limitations) of age 25 to 61. I use data from the Current Population Survey Annual Social and Economic Supplement (CPS ASEC), colloquially referred to as the “March CPS”. The CPS ASEC provides information on the labor force status and earnings for the population in general and also for those who report work-limiting impairments. I use the sample period of 1994 to 2007, which covers the period after the ADA became effective, but before the Great Recession and the *ADA Amendment Act of 2008* (see the timeline in Figure 3.1). I use both difference-in-differences and difference-in-difference-in-differences regression methodologies. The difference-in-differences compares the disabled in California, before and after the PKP Act, to the disabled in other states over the same time period. The difference-in-difference-in-differences adds a third comparison: the disabled versus the non-disabled. This additional comparison helps control for state economic trends that may have differentially affected California relative to other states.

I find strong evidence that the PKP Act increased employment of the disabled. In my preferred specification, the employment-to-population ratio increased by 2.9 percentage points after the PKP Act. This effect is sizable relative to the employment-to-population ratio for the disabled in California from 1994 to 2000, before the PKP Act, which is 24.5%. The estimated effects on employment are robust to the methodology used (DD or DDD) and to the employment measure used (weeks worked or employment status) but are they

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<sup>4</sup>Thompkins (2013) studies a number of court decisions, such as *Sutton v. United Airlines* 119 S. Ct. 2139 (1999) and *Toyota Motor Mfg., KY, Inc. v. Williams*, 534 U.S. 184 (2002), that restricted the definition of disability by forcing consideration of mitigating measures in the determination of disability (*Sutton*) and requiring that individuals prove that they are substantially limited in their ability to work at a broad range of jobs, and not just the job in question (*Williams*). She also studies the *ADA Amendment Act of 2008* which expanded the definition of disability and mirrors aspects of the PKP Act, such as ignoring mitigating measures.

sensitive in magnitude, and somewhat sensitive to statistical significance, to the inclusion or exclusion of state-specific linear time trends. There is also evidence of increased earnings (conditional on working), although the results are sensitive to the earnings measure used and to state specific linear time trends. These positive estimated labor market effects suggest that expanding disability discrimination protections to a broader set of individuals with less severe impairments can help alleviate barriers to employment for these individuals and lead to better labor market outcomes.

### 3.3 Data

I use the Current Population Survey’s Annual Social and Economic Supplement (CPS ASEC), often referred to as the “March CPS” since this supplement occurs as an add-on to the CPS’s monthly survey in March. The primary benefit of this data is a large sample size: about 6,616 disabled individuals and 78,883 non-disabled individuals per year in my sample, which allows more accurate estimates of labor market outcomes at the state level, which is crucial to this analysis. The CPS ASEC, unlike the general monthly CPS, asks earnings questions and the question that establishes a work-limiting disability: “Does [insert name] have a physical, mental, or other health condition that limits the kind or amount of work [insert name] can do at a job or business?”. I deem individuals to be disabled if they answer “yes” to this question. ASEC also includes questions about income.

The CPS ASEC provides the following variables of interest: employment status (employed, not in labor force, unemployed), weeks of unemployment (if unemployed), weeks worked last calendar year, weekly earnings at the current job, total wage and salary income last calendar year, and income from various sources last calendar year (e.g., Supplemental Security Income (SSI), employer-sourced disability insurance (DI)). Employment status, unemployment duration, average weekly earnings, and disability status are asked in reference

to the current point in time<sup>5</sup>. Thus, when examining effects on these variables, employment for example, the sample includes individuals who indicate that they currently have a work-limiting impairment (or not) at the time of the interview and that they are currently employed (or not) at the time of the interview. Weeks worked, annual income, and income received from various sources are asked in reference to the entire previous calendar year.

In creating my sample, I restrict my time period to 1994 to 2007 (which is 1995 to 2008 for annual wage and salary income and weeks worked, since they refer to the previous calendar year). I start my sample in 1994 for two reasons. First, I want to avoid the immediate effects of the ADA that varied by state due to states that had different pre-existing discrimination laws (Jolls and Prescott, 2004). Second, I want to avoid changes in the CPS interview procedure, where the CPS moved to a computer-assisted (CATI) design from 1993 to 1994, which may have affected measurement of people with work-limiting disabilities (Kruse and Schur, 2003). The 2007 cut-off is for two reasons. First, I wish to exclude the Great Recession period (December 2007 to June 2009)<sup>6</sup> to avoid picking up its adverse impacts. Second, the *ADA Amendments Act of 2008* (ADAAA) became effective on January 1, 2009, and it is not the focus on this study. See Figure 3.1 for a timeline that presents my sample period and major events before, during, and after this time. My other sample restriction is to restrict age to be 25 to 61 to focus on individuals with more labor market attachment.

### 3.3.1 Implications of the Work-Limited Disability Measure

As discussed by Burkhauser et al. (2002), there is no explicit definition of disability as there is for race or sex. Different measures capture different (but overlapping) groups. The ideal would be to use a disability measure that closely matches the definition of disability

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<sup>5</sup>The question about average weekly earnings is not unique to the ASEC as it is asked monthly (from 1994 to 2013) in the monthly CPS, but only for about one quarter of the sample (the “Outgoing Rotation Group”).

<sup>6</sup>See <http://www.nber.org/cycles.html> (accessed March 4, 2015).

in FEHA after the PKP Act, but not before. Thus, those covered by the measure are not covered by FEHA before the PKP Act, but they are after. This would allow for the most accurate estimate of the effect of the PKP Act on the affected population.

The question is, then, how well does the work-limited measure of disability in the CPS ASEC correspond to the definition of disability in FEHA after the PKP Act? For one of the changes in the PKP Act, there is a good match. One of the changes in the PKP Act was to deem “working” a major life activity under California’s FEHA<sup>7</sup>, but this was deemed a major life activity in case law. For individuals who report being work-limited to prove that they are disabled under the ADA, they must prove that their reported work limitation substantially limits a major life activity or limits them from working in a class of jobs or a broader range of jobs (Long, 2008). It is not enough for them to argue that their condition limits them just in a particular job. Under FEHA after the PKP Act, individuals can use “working” as a major life activity and in doing so it is enough to show a limitation to working in a particular job (see Appendix 1 for additional discussion). Thus, those who report being work-limited are much more likely to be deemed disabled under FEHA after the PKP Act than under the ADA if the reported work-limitation is in reference to a particular job (a current or a recent job).

On the other hand, the work-limited measure does not explicitly align with other parts of the disability definition in FEHA after the PKP Act. The most notable change in the definition in FEHA after the PKP Act was the requirement that the impairment “limits” rather than “substantially limits” a major life activity. This work-limited measure does not explicitly include those who are only limited but not substantially limited. The work-limited measure also doesn’t capture those diagnosed with a condition that is explicitly covered under FEHA after the PKP Act. It also does not capture those who use mitigating measures, such

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<sup>7</sup>“...Further, under the law of this state, ‘working’ is a major life activity, regardless of whether the actual or perceived working limitation implicates a particular employment or a class or broad range of employments.” (Cal Gov Code §12926.1(c))



as mobility aids or medication, but that don't meet the "substantially limits" requirement when mitigating measures are used. Even if other survey data were used to construct an alternative measure of disability, it would be difficult to construct a disability measure that captured these.

However, numerous researchers have shown that the work-limited measure of disability is highly correlated with other more objective assessments of health and with clinical measures of disability (Burkhauser, Houtenville and Wittenburg, 2001). More importantly, Burkhauser et al. (2002) shows that while the work-limited measure does not capture everyone with impairments, trends in the prevalence of work-limitations and of employment for the work-limited match these same trends using limitations measures. Thus, Burkhauser et al. (2002) concludes that the work-limited measure is appropriate for monitoring trends in outcomes of those with disabilities, which provides support for its use in this study.

### **3.3.2 Sample Demographics**

Tables 3.2 and 3.3 present means of the demographic variables (which are also regression controls) in the sample. Presented are the means by disability status, region (California or the other states), and time period (before the PKP Act, 1994 to 2000, and after, 2001 to 2007). This is eight means to mirror the difference-in-differences (DD) and the difference-in-difference-in-difference (DDD) methodologies presented later. All means incorporate population weights to create population representative statistics. For the other states, this allows the means to reflect the national average without California. Table 3.2 presents age, gender, race, and Hispanic ancestry, while Table 3.3 presents marital status and highest educational attainment. Disabled individuals are on average older, about five years, reflecting the increasing onset of disability with age, are more likely to be Black or American Indian/Aleut/Eskimo, are less likely to be Hispanic, are less educated, and are less

likely to be married. California residents are slightly younger, are more likely to be Asian or Hawaiian/Pacific Islander or Hispanic, but less likely to be Black. Californians have a lower likelihood of reporting that they are work-limited: 7.5% in the pre-period versus 8.3% outside of California. This is presented over time in Figure 3.2.

### 3.3.3 Labor Market Outcomes

Labor force impacts can be measured through many variables, but employment is likely the most important. However, there are two different employment variables that are available: an indicator variable for being employed and the weeks worked last calendar year. These measures are highly correlated. The weeks worked measure has the advantage of including within-the-year variation in employment, such as only holding a job for part of the last calendar year, whereas the employment indicator variable only measures employment contemporaneously. On the other hand, the employment indicator variable is easier to interpret as an employment rate change and can be compared to indicator variables for being unemployed or not in the labor force. The employment indicator also has the advantage that it is asked in reference to the current period, as is the question about disability, while the weeks worked question refers to the previous calendar year. Thus it is possible that an individual who answers that they are disabled now, and reports their weeks worked last calendar year, may not have answered that they were disabled last year<sup>8</sup>. Because both variables have strengths and weaknesses, I use both.

Table 3.4 presents summary statistics of labor market outcomes and labor market controls (state unemployment rate, extra weeks of unemployment insurance available). Only about a quarter of the disabled are employed, relative to about four-fifths of the non-disabled.

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<sup>8</sup>Burkhauser, Houtenville and Wittenburg (2001) compares the work-limitations measure in the CPS to a two-period measure where individuals report work-limitations in two consecutive years. Burkhauser, Houtenville and Wittenburg (2001) finds that the incidence of disability in the CPS in 1996 using the one period (standard) disability measure is about 8% while for the two-period measure it is about 5% (see Exhibit 3). Thus it appears that this could be a problem for a significant portion of those who report being disabled.

This is also presented over time in Figure 3.3. Most of this employment gap is due to the disabled not being in the labor force. The proportion unemployment is slightly lower for the disabled than for the non-disabled, likely due to lower labor market attachment.

Also presented in Table 3.4 are two control variables: the extra available weeks of Unemployment Insurance (UI) and the state unemployment rate. The UI control is the mean number of extra weeks of UI that were available from 2002 to 2007 due to the federal Extended Benefits program and another temporary program that was available in the early 2000s (Farber and Valletta, 2015). This is used as a control variable in the regressions since the extra weeks available varied by state over time<sup>9</sup>. While the extra UI weeks differed by state, the average extra UI weeks were about the same in California relative to the rest of the country. However, the unemployment rate in California was consistently higher than the population-weighted average state unemployment rate in the rest of the country: 1.8 percentage points higher in the pre-period and 0.7 percentage points higher in the post-period. This suggests an improving economy over time in California relative to other states.

Table 3.4 presents mean annual wage and salary income, mean weekly wage and salary income, and mean weekly earnings all in 2013 dollars. Weekly earnings are derived from a question asking about weekly earnings at the current job, while the weekly wage and salary income is calculated by dividing the annual wage and salary income reported for the previous calendar year by the weeks worked in the previous calendar year for those that report both non-zero income and weeks worked. I calculate this alternative measure of weekly earnings because the weekly earnings question is only asked to the outgoing rotation group, which is about one quarter of the sample, while the annual wage and salary income and weeks worked questions are asked of everyone in the CPS ASEC. However, these measures are highly correlated. I include weekly earnings in my analysis even though it is based on less data since it was common in the literature (see, e.g., Acemoglu and Angrist (2001) and

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<sup>9</sup>Extra weeks of UI were available for all states for most of 2002, all of 2003, and about half of 2004, but only for two states in 2005 and one state in 2006.

Thompkins (2013)) and it avoids the issue discussed above for weeks worked last calendar year since the disability and weekly earnings questions are asked in reference to the current period and not separate periods. In comparing annual wage and salary earnings to the weekly measure, it is important to note that the weekly measures reflect wage or salary rates while annual wage and salary income reflects wage or salary rates but also incorporates some employment effects since individuals could have been employed for only part of the calendar year.

I use these earnings variables to gauge the impacts on wage rates (salary rates) rather than the hourly wage because the large majority of workers are not paid hourly, and also the hourly wage question is only asked to the CPS'S Outgoing Rotation Group, which is about one quarter of households. Thus this variable does not provide enough information. For example, my sample includes only 175 disabled Californians who were paid hourly. Also, the characteristics of those paid hourly are different (e.g., less skilled, less educated) than those not paid hourly, so any comparison of the effects on hourly wages could also reflect treatment effect heterogeneity.

Weekly wage and salary income is lower for the disabled: \$789 in California (\$696 outside California) versus \$1,069 in California (\$987 outside California) in the pre-period. Annual wage and salary income and weekly earnings follow a similar pattern. Income gradually rose over time, as shown in Figure 3.4.

From these summary statistics it is possible to calculate naïve difference-in-difference (DD) and difference-in-difference-in-difference (DDD) estimates, which mirror the estimates presented later except that they have no controls. For employment, the disabled in California had their employment drop from 24.5% to 20.4%, a drop of -4.1 percentage points, while the drop was larger, -4.9 percentage points, for the disabled outside California. Taking this difference-in-differences suggests a 0.8 percentage point increase in employment of the disabled in California after the PKP Act took effect. However, this estimate changes to -0.1

percentage points when taking a difference-in-difference-in-differences, since the non-disabled in California had a 0.3 percentage point increase in employment and the non-disabled outside of California had a 0.6 percentage point decrease. For weeks worked, the naïve DD estimate is an extra 0.2 weeks, but this is -0.1 for the DDD. For weekly wage and salary earnings, the naïve DD estimate is a \$22, which changes to -\$13 for the DDD. The effect is more optimistic for annual wage and salary earnings: the naïve DD estimate is a \$2,392 increase, which shrinks to a \$707 increase with the DDD. In general these naïve estimates lean slightly towards positive effects, particularly for the DD, but the effects are mixed. However, the regression estimates presented later provide more convincing evidence.

### 3.4 Methods

I use two related panel regression methodologies. The first is a difference-in-differences (DD) using a sample of only those who are disabled. This DD compares the disabled in California before and after the PKP Act passed in 2001 to the disabled in other states. The second is a difference-in-difference-in-differences (DDD), which adds a third difference: disabled versus non-disabled. Both methodologies control for time-invariant state characteristics and national-level shocks. The DDD has the added appeal of controlling for state by year shocks to the outcome variables under the assumption that the disabled and non-disabled experienced similar shocks independent of the effect of the PKP Act.

The DD, estimated only for the disabled, is as follows:

$$Y_{ist} = \alpha PKPAct + Controls_{st}\lambda + X_i\beta + \theta_s + \gamma_t + \epsilon_{ist} \quad (3.1)$$

In this equation,  $i$  refers to the individual,  $s$  to the state, and  $t$  to the year.  $PKPAct$  is an indicator variable equal to one for 2001 or later and living in California, zero otherwise. The coefficient of interest is  $\alpha$ , which captures the effect of the PKP Act on the outcome variable,

$Y_{ist}$ , for the disabled.  $\gamma_t$  are year fixed effects, which capture national-level shocks, such as declining employment of the disabled over time or changes in Social Security Disability Insurance.  $\theta_s$  are state fixed effects which capture time-invariant state characteristics, such as differences in state laws (Neumark, Song and Button, 2015).  $X_i$  is a set of individual covariates, more specifically indicator variables for each possible gender, race, Hispanic ancestry, marital status, age in years, and years of education.

$Controls_{st}$  contains four sets of controls for state-level policies or economic conditions:

1. a set of four indicator variables to control for minor changes in disability discrimination laws in Maine, Rhode Island, and Washington. See Appendix 2 for more details;
2. a set of five indicator variables to control for job creation hiring credits at the state level that specifically targeted the disabled (from Neumark and Grijalva 2013)<sup>10</sup>;
3. the number of additional weeks of unemployment insurance available by state and year (from Farber and Valletta 2015); and
4. the state unemployment rate, from the Bureau of Labor Statistics<sup>11</sup>.

The outcomes,  $Y_{ist}$ , that I investigate are employment, unemployment, and labor force non-participation (all of which are binary variables), unemployment duration in weeks, weeks worked last calendar year, log weekly earnings at the current job, log annual wage and salary income last calendar year, and log weekly wage and salary income last calendar year. For outcome variables that are binary, the above regression is run both as a linear probability model and as a logit model to test if results are robust to the model used.

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<sup>10</sup>Maryland’s “Disability Employment Tax Credit”, available from 1997 onward, New Jersey’s “Income Tax Credit for Employment of Certain Handicapped Persons” from 2006 onward, New York’s “Credit for Employment of Persons with Disabilities”, Tennessee’s “Credit for Hiring Disabled Persons” from 2006 onward, and Utah’s “Hiring Persons with Disabilities (Targeted Jobs Tax Credit)” from 1995 onward. See Neumark and Grijalva (2013) for more details.

<sup>11</sup>The specific series used was LAUSTxx0000000000003, where “xx” is replaced with each state’s Federal Information Processing Standard (FIPS) code. These estimates are provided at the monthly level and were not seasonally adjusted, but I averaged them to the annual level.

I estimate the model in Equation 3.1 both with and without state-specific linear time trends. State-specific linear time trends control for some changes in the outcome variable by state. For example, if employment of individuals with disabilities in California declines at a faster linear rate than in other states, then the estimate of  $\alpha$  will be negatively biased as these state-specific linear time trends are an omitted variable. With state-specific linear time trends included, estimated treatment effects are net of the estimated linear trend.

While the inclusions of these trends may reduce bias, they could also attenuate estimates (bias them toward zero). Meer and West (2013) shows that if the treatment effect occurs as a change in the growth rate of the independent variable (i.e. growth in the employment rate) instead of or in addition to a change in the level of an independent variable (i.e. employment rate increase), then including state-specific linear time trends will attenuate estimates by absorbing some of the growth effect. As the results show later, estimates generally move from positive to more positive when state-specific linear time trends are included, suggesting that attenuation likely isn't occurring (and if it is, the true estimate is actually larger). Thus, Meer and West (2013)'s criticism of state-specific linear time trends is much less of a concern here.

The DDD methodology augments the DD by adding the disabled versus non-disabled dimension:

$$Y_{idst} = \alpha PKPAct + Controls_{dst}\lambda + X_i\beta + \theta_{ds} + \delta_{st} + \gamma_{dt} + \epsilon_{idst} \quad (3.2)$$

The additional  $d$  subscript added here refers to disability status.  $PKPAct$  is an indicator variable equal to one for being 2001 or later, living in California, and being disabled, zero otherwise. While the DD includes state fixed effects and year fixed effects, the DDD includes instead disabled by state ( $\theta_{ds}$ ), state by year ( $\delta_{st}$ ), and disabled by year ( $\gamma_{dt}$ ) fixed effects. The same set of individual-level controls,  $X_i$ , are included.  $Controls_{dst}$  are included again, but there is an additional  $d$  subscript here denoting that the controls are interacted with a

disability indicator variable so that effects can differ for the disabled and the non-disabled. For the minor changes in disability discrimination laws and the hiring credits for the disabled, this interaction is required as these policies are specific to the disabled. The extensions to unemployment insurance are available to both the disabled and the non-disabled, and would be subsumed by the state by year fixed effects if included as-is. I include the extra weeks available interacted with the *Disabled* indicator variable to allow the effects of extra weeks of unemployment insurance to differ between the disabled and the non-disabled. Similarly, while the state unemployment rate is, by construction, the same for the disabled and the non-disabled, interacting it with the *Disabled* indicator allows the effects of state unemployment rate changes to differ by disability status.

The coefficient of interest is again  $\alpha$ , which captures the effect of the PKP Act on the outcome variable,  $Y_{ist}$  for the disabled relative to the non-disabled. “Relative” is key here, as the PKP Act could elicit substitutions between disabled and non-disabled workers. While non-disabled workers should be largely unaffected by the PKP Act<sup>12</sup>, the PKP Act could lead to increased or decreased demand for the disabled relative to the non-disabled. Differences between the  $\alpha$ 's in Equations 3.1 and 3.2 will reflect both possible substitutions and also the effect of controlling for state by year shocks using the non-disabled as a control group.

The DDD model is preferred to the DD model if the disabled in California were subject to different economic shocks than the disabled in other states, and these differential economic shocks were also experienced in the same way for the non-disabled. It appears to be the case that California faced different economic shocks than other states based on the unemployment rates presented in Table 3.4. From the pre-period to the post-period, the unemployment rate in California decreased by 0.8 percentage points, while outside California it increased by 0.3 percentage points. This suggests that California faced more favorable economic conditions after 2001 compared to before, relative to other states. If the disabled in California faced

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<sup>12</sup>Obviously non-disabled workers could become disabled, particularly if they are older as the probability of disability increases with age. See Neumark, Song and Button (2015) for a discussion of this.



these same trends, as is likely, then the DD model could generate artificially positive estimates of the effect of the PKP Act on labor market outcomes. In this case the DDD would be the preferred model by eliminating this bias through the additional control of the disabled versus non-disabled comparison.

I choose panel regression methodologies over an Abadie, Diamond and Hainmueller (2010) synthetic control case study because the synthetic control requires precise estimates of population parameters, which in this case would be precise estimates of labor market outcomes by disability status and state over time. The CPS ASEC is not intended to provide these parameter estimates by disability status and state. For example, the number of people with disabilities is variable in the CPS, and especially by state<sup>13</sup> Also, there is no analogue to Abadie, Diamond and Hainmueller (2010) for a DDD, making this methodology unrealistic for this study.

### **3.4.1 Inference using Conley-Taber Confidence Intervals**

As is common in studies of state-level policies, I present regression result where I cluster my standard errors at the level of policy variation, which here is the state level (Bertrand, Duflo and Mullainathan, 2004). However, Conley and Taber (2011) show using Monte Carlo experimental evidence that when a DD is used, confidence intervals constructed using clustered standard errors significantly over-reject the null hypothesis (increased Type I Error) when there are few treated groups. They propose a method to create confidence intervals (“Conley-Taber confidence intervals”) that provide much more accurate inference. Their method does not require the typical asymptotic assumption that both the number of treated and untreated groups is large but rather assumes only that the number of untreated groups is large, but the number of treated groups and the time span of the data are held fixed. To construct the Conley-Taber confidence intervals, information from the untreated groups

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<sup>13</sup>See [http://www.bls.gov/cps/cpsdisability\\_faq.htm](http://www.bls.gov/cps/cpsdisability_faq.htm) (accessed June 1, 2015) for a discussion.

is used to consistently estimate the distribution of the DD point estimator. Their method performs significantly better in Monte Carlo experiments, particularly when there is only one treated unit. See Conley and Taber (2011) for a more technical and thorough explanation.

In this paper I present confidence intervals estimated following Conley and Taber (2011) in addition to those based on state-clustered standard errors. Due to the nature of the procedure used to construct Conley-Taber confidence intervals, it is not possible in my application to test for statistical significance at the 99% level. This is because the procedure creates a step-wise estimate of the noise, where there is one step for each group, and a 99% confidence interval requires as at least 200 groups. I have only 51 groups in the DD (50 states plus DC), and 102 groups in the DDD (state by disabled, so  $51 \times 2$ ). Appendix 3 discusses further how I construct Conley-Taber confidence intervals.

## **3.5 Results**

### **3.5.1 Effects on Employment**

Table 3.5 presents estimates of  $\alpha$ , the effect of the PKP Act, for weeks worked last calendar year using both the DD (presented in Columns (1) and (2), using Equation 3.1) and the DDD (Columns (3) and (4), Equation 3.2). Columns (2) and (4) include state-specific linear time trends, while Columns (1) and (3) do not. Confidence intervals using state-clustered standard errors are shown in brackets under the coefficient estimate for  $\alpha$ . Below the state-clustered confidence intervals, in parentheses, are Conley-Taber confidence intervals.

The point estimates range from a 0.36 week increase to a 1.42 week increase, with point estimates being slightly larger for the DD than for the DDD. This suggests that without the

non-disabled control group in the DDD, the DD may be overstating the effect of the PKP Act by not controlling for more favorable labor market outcomes in California, but if this is occurring it is a mild effect.

What seems to matter significantly are the state-specific linear time trends. The point estimates are much larger and are only statistically significant when state-specific linear time trends are included. While the Conley-Taber confidence intervals are generally much wider, there is statistical significance for the estimates with state-specific linear time trends using both state-clustered and Conley-Taber confidence intervals.

The fact that the estimates are more positive when state-specific linear time trends are included suggests that there were more negative linear trends in weeks worked in California relative to in other states. But the fact that the results with state-specific linear time trends are larger in magnitude is somewhat surprising in light of Meer and West (2013)'s argument that these trends may attenuate estimates. If there is in fact attenuation, then the estimated effects would be even larger.

A back of the envelope calculation using the weeks worked results shows that the employment effects are moderate to large. Ignoring partial year employment, the estimates suggest a 0.69 percentage point (0.36/52) to a 2.73 percentage point (1.42/52) increase in the employment-to-population ratio, with the preferred estimate being that in Column (4): a 2.54 percentage point increase. This is a sizable impact given the employment rate of the disabled in California in the pre-period (24.5% or 13.3 weeks).

The employment results using the indicator variable are shown in Table 3.6. Panel (a) presents results using a linear probability model and Panel (b) presents results using a logit regression, with marginal effect estimates at the means presented just below. The use of both a linear probability model and a logit model is to ensure that results are not sensitive to the particular model used. Also presented are logit regression results but without the use of

population weights, which are used in all other regressions. The unweighted logit results are included because the Conley-Taber procedure does not work for logit regressions with population weights<sup>14</sup> Because the unweighted estimates are similar to the weighted estimates, the Conley-Taber confidence intervals for the unweighted estimates suggest that the confidence intervals for the weighted estimates would be similar if they could be calculated. Unfortunately, Conley-Taber confidence intervals could not be calculated after marginal effects calculations, although these confidence intervals should be the same in terms of inference as the Conley-Taber confidence intervals for the corresponding logit estimates.

These employment results are similar to the weeks worked results. The DD and DDD estimates are very similar, but the DDD estimates are again slightly smaller. Estimates with state-specific linear time trends are larger and more statistically significant. The estimates are larger, however, ranging from a 1.7 percent point increase to a 3.9 percent point increase in employment (using the linear probability model estimates in Panel (a)). The preferred estimate from the logit model DDD with state-specific linear time trends (Panel (b), Column (4)) suggest a 2.9 percentage point increase at the means.

The employment estimates without state-specific linear time trends are also more likely to be statistically significant than the weeks worked results, likely because they are larger than the corresponding weeks worked estimates. In Panel (a) these estimates are statistically significant at the 99% level with state-clustered standard errors but are not statistically significant with Conley-Taber confidence intervals. However, there is statistical significance at at least the 95% level using Conley-Taber confidence intervals for the unweighted logit (Panel (c)). Thus there are some statistically significant employment effects even in the regressions without state-specific linear time trends. Collectively, the employment estimates in both Table 3.5 and 3.6 point towards an employment increase, although the evidence is

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<sup>14</sup>To employ the Conley-Taber procedure, residuals need to be saved to be used in computations (see Appendix 3). However, the Stata “predict” command that would be used to do this is not allowed after logit regressions that use population weights (“pweights”). Alternative strategies, such as using the “glm” command did not work. More information is available upon request.

stronger when state-specific linear time trends are included. State-specific linear time trends are preferred because they control for heterogeneous state trends that cannot be accounted for by the DD or DDD methodology, or through the other controls. Thus the estimates with state-specific linear time trends should be given more weight.

### **3.5.2 Effects on Unemployment and Labor Force Non-Participation**

Table 3.7 presents the estimated effects on unemployment. These estimates are negative but smaller in magnitude than the employment results. Only the estimates with state-specific linear time trends are statistically significant, however estimates are similar for the logit and the linear probability models. The preferred estimate from the logit model DDD with state-specific linear time trends (Panel (b), Column (4)) is a one percentage point decrease in unemployment, calculated as the marginal effect at the means. Panel (d) presents effects on unemployment duration, in weeks, but there is no clear pattern and almost no statistical significance.

Table 3.8 presents the effects on labor force non-participation. The effects for labor force non-participation are negative and smaller in magnitude than the employment results, but larger in magnitude than the unemployment results. The preferred estimate is a 1.6 percentage point decrease in the probability of being not in the labor force (Panel (b), Column (4)). While there is consistent statistical significance using state-clustered confidence intervals, there is mixed statistical significance using Conley-Taber confidence intervals. The linear probability estimates with state-specific linear time trends (Panel (a), Columns (2) and (4)) are statistically significant under Conley-Taber for employment and unemployment, but they are no longer statistically significant under Conley-Taber for labor force non-participation. The unweighted logit results (Panel (c), Columns (2) and (4)) are however still statistically significant.

These results suggest that more of the employment increase comes from net movements from labor force non-participation to employment than from net movements from unemployment to employment. While the unemployed have more attachment to the labor force, and thus it is more likely that increased employment would occur for them, there are far more disabled individuals who are not in the labor force (71.1% in California in the pre-period) than unemployed (4.5%). This may explain why the estimated impacts for labor force non-participation are larger in magnitude than the estimated unemployment impacts. But the preferred estimate of the unemployment decrease (one percentage point) is large relative to the proportion of Californians with disabilities in the pre-period who are unemployed (4.5%). This is a large increase in the ability of those with disabilities with stronger labor force attachment to secure (or hold onto) employment.

### 3.5.3 Effects on Earnings

Table 3.9 presents the effects on log annual wage and salary income (Panel (a)), log weekly wage and salary income (Panel (b)), and log weekly earnings (Panel (c)). The results for log annual wage and salary income mirror the earlier results in the paper, where the DD and DDD methodologies provide similar results, but the DDD estimates are slightly smaller. Again similar to earlier, regressions with state-specific linear time trends show larger and statistically significant results, while the estimates without state-specific linear time trends are smaller and only weakly statistically significant. The estimate in Column (2) is statistically significant at the 95% level under Conley-Taber, but the estimate in Column (4) is only statistically significant at the 90% level under Conley-Taber. The preferred estimate in Column (4) suggests an increase in annual wage and salary income of 11.5%, a large effect.

The log weekly wage and salary income results (Panel (b)) are weaker and results

are more positive for regressions without state-specific linear time trends, contrary to the trend seen up until this point. While the estimates without these trends are statistically significant at the 99% level under state-clustering, no estimates are statistically significant under Conley-Taber.

The results for log weekly earnings (Panel (c)) are radically different from the log weekly wage and salary income results. These estimates follow the earlier pattern of the estimates with state-specific linear time trends being larger and more statistically significant. In this case, the estimates without state-specific linear time trends are negative and statistically significant, while the results with these trends are huge: the preferred estimate in Column (4) suggests an increase in weekly earnings of 40.2%. The corresponding estimate (DD with trends, Column (2)) has a larger estimate of 46.6%, and both these estimates are statistically significant at the 95% level under Conley-Taber. This confusing mix of results is hard to reconcile, but all results point to positive effects, but these results are sensitive to the earnings measure used and to the inclusion or exclusion of state-specific linear time trends.

## **3.6 Possible Endogeneity and Compositional Changes**

### **3.6.1 The Work-Limited Measure and Compositional Changes**

With any disability measure that is self-reported, there is a risk that self-reported disability status is endogenous to employment status or to the PKP Act. Being endogenous to employment is more likely to be the case with the work-limited measure, since it is based on reported limitations to work. Burkhauser et al. (2002) shows that for individuals with a reported impairment (e.g., blind in both eyes), those that report being work-limited are more likely to be employed than those who are not. This suggests that individuals who have an impairment but are sufficiently integrated into the workforce do not report a work

limitation.

Thus, compositional changes could occur in response to employment shocks. A positive employment shock to the disabled would increase hiring of the disabled, however some recently hired disabled individuals may no longer report that they are work-limited because they feel sufficiently integrated in their new workplace, thus understating the employment increase for the disabled (Kruse and Schur, 2003). If the positive employment shock reduces terminations, then there is less movement of individuals with impairments who are employed, but do not report being work-limited, to being not-employed, with some of these individuals then reporting that they are work-limited. Similarly, a negative employment shock to the disabled would decrease employment, and could cause some movement from employed, but not work-limited, to not employed and work-limited. Regardless of if the shock is positive or negative, there is a resulting possibility of negative bias in the estimated employment effect.

It is also possible that the work-limited measure of disability is endogenous to the PKP Act if the PKP Act affects employment. But there is possibly another route: workplace accommodations. If the PKP Act makes workplaces more accessible, and increases reasonable accommodations, then individuals may no longer report that they have a work-limiting disability (Kruse and Schur 2003; Kirchner 1996). This is possible as the PKP Act could increase accommodations both due to covering more individuals, but also due to the unique feature in the PKP Act that employers can be penalized for not adequately participating in the interactive process to identify reasonable accommodations. This could negatively bias estimates of employment effects by classifying some individuals as not having a work-limiting disability when they would have reported one had accommodations not expanded as a result of the PKP Act. This further increases the magnitude and possibility of negative bias. However, I find positive labor market outcomes for the work-limited after the PKP Act, even given the possibility of negative bias. This suggests that my estimates could be lower bounds as the true effects could be more positive.



As a first point of investigation into if this compositional change occurred, Figure 3.2 presents the proportion of the sample by year that reports being work-limited for California and other states. I present probabilities based on raw annual averages in addition to regression-adjusted annual average probabilities. The regression-adjusted probabilities come from the DD logit model below:

$$Disabled_{ist} = \alpha PKPAct_{st} + X_i\beta + \theta_s + \gamma_t\epsilon_{ist} \quad (3.3)$$

*Disabled* is an indicator variable if individual *i* in state *s* and year *t* reports being work-limited. This regression is similar to that in Equation 3.1 except that it does not include *Controls<sub>st</sub>*. In addition to using the predicted probabilities from the logit regression above to construct annual averages for Figure 3.2, I present the estimates of  $\alpha$  using both a linear probability model and a logit model and present results in Table 3.10.

While the series are noisy<sup>15</sup> for California in Figure 3.2, California and other states seem to track, and both states experienced a decrease in reporting being work-limited over time. The figure doesn't show any clear compositional change that occurred only in California after the PKP Act.

Table 3.10 presents the regression results. The linear probability model (Column (1)) estimates a reduced probability of reporting being work-limited of 0.3 percentage points. Given that a positive employment effect was found, this slight decrease in the probability of reporting being work-limited is consistent with the story that some individuals who become employed or receive on-the-job accommodations no longer report being work-limited. However, this estimate is only statistically significant at the 90% level with state clustering, and not at all with Conley-Taber. The marginal effect estimate under the logit regression (Column (2), Panel (b)) has the same estimate of 0.3 percentage points, but it is now statis-

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<sup>15</sup>Estimates of the population of those with disabilities is particularly variable. See [http://www.bls.gov/cps/cpsdisability\\_faq.htm](http://www.bls.gov/cps/cpsdisability_faq.htm) (accessed April 8, 2015) for a discussion.

tically significant at the 95% level under state-clustering. The unweighted logit (Column (3)) suggests that it's unlikely that there would be statistical significance under Conley-Taber for the weighted logit estimate. Thus there is insufficient evidence to conclude that there were compositional changes in reporting disability that coincided with the PKP Act.

### **3.6.2 Possible Endogeneity of the Prudence Kay Poppink Act**

One could be concerned that the adoption of the PKP Act is endogenous to my outcome variables. Endogeneity would cause my estimates to be biased. For example, perhaps there were negative shocks to the labor market outcomes of the disabled in California before the PKP Act, and these negative shocks moved legislators to pass the PKP Act. This would be a so-called “Ashenfelter’s Dip” (Ashenfelter, 1978) and would result in estimates being negatively biased. This is because in this example labor market outcomes were already deteriorating in California relative to other states, and if this trend continued into the PKP Act period then the negative effects of this trend could be falsely attributed to the PKP Act. I investigate the possibility of endogeneity two ways. First, I examine figures of labor market outcomes over time to look for any Ashenfelter’s Dips and second, I study the motivation behind the adoption of the PKP Act.

My DD and DDD methodology control for many aspects of possible endogeneity, but not all. The DD methodology, using the sample of the disabled, controls for any endogeneity related to time-invariant state factors affecting the disabled, such as the disabled in California generally having lower employment rates (e.g., 24.5% in California versus 35.2% elsewhere, see Table 3.4), and endogeneity related to national shocks to disabled individuals, such as the general decline in employment of the disabled (see Houtenville and Burkhauser (2004)). The DDD adds an additional level of control as it controls for shocks that affected California, but not other states, under the assumption that these shocks affected the disabled and non-

disabled the same.

Thus for there to be an endogeneity problem for the DDD, there must have been a shock that only affected the disabled in California, and not non-disabled Californians or other disabled individuals in other states, and the PKP Act was a reaction to this shock. On top of this, the regressions with state-specific linear time trends control for linear trends in labor market outcomes for the disabled in each state<sup>16</sup>, and the inclusion of state unemployment rates as a control also controls for state economic conditions over time, so shocks that could be reacted to would need to be net of these controls. The endogeneity that I cannot control for with the DD or DDD methodology is if there were different trends in California relative to other states, net of these controls. The example above of deteriorating conditions for the disabled in California, relative to non-disabled Californians and relative to the disabled in other states, prompting the PKP Act is a possibility, particularly if the negative trend is not entirely picked up by my controls.

Figure 3.3 presents the employment-to-population ratio over time for the disabled (Panel (a)) and the non-disabled (Panel (b)) for both California and the rest of the country. These figures show that employment rates decreased slightly for the disabled leading up to the PKP Act, with a big drop occurring from 1998 to 1999. Meanwhile, employment rates rose gradually for the non-disabled over this same pre-period. These trends were similar for those in California and for those in the rest of the country. Figure 3.4 presents the mean weekly wage and salary income. This income rose over time for the disabled, but at a somewhat slower than for the non-disabled. Again, trends were similar for California and the rest of the country. Since these figures show slightly deteriorating conditions for the disabled in California relative to the non-disabled, it could be possible that the PKP Act was a reaction to this. But if this were the case, this endogeneity would be controlled for since other states, which form the control group, experienced the same trends.

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<sup>16</sup>For the DDD, this is linear trends in the disabled versus non-disabled gap in employment outcomes.

The likely motivation behind the PKP Act was not slightly deteriorating labor market outcomes for the disabled, but rather court rulings that diverged from what legislators saw as the intent of California’s FEHA. FEHA always had codified in law that the requirement to be considered disabled was that a condition only “limit” a major life activity, and not substantially limit one. However, courts interpreted FEHA the same as the ADA, thus making the standard in FEHA to be “substantially limits” (Long, 2004). This history of alleged misinterpretation is demonstrated in *Colmenares v. Braemer Country Club, Inc.*, 63 P.3d 220, 223 (Cal. 2003), where the plaintiff was deemed not disabled because his case preceded the PKP Act, when FEHA’s “limits” was interpreted consistent with the ADA’s “substantially limits”, but he would have been considered disabled after the PKP Act. So while the misinterpretation of “limits” to be “substantially limits” was always looming, the breaking point that prompted the PKP Act was likely the “Sutton Trilogy” of United States Supreme Court decisions<sup>17</sup>. The “Sutton Trilogy” deemed individuals to not be disabled if mitigating measures, such as mobility aids or medication, made their impairment no longer “substantially limit” a major life activity. The PKP Act explicitly overturned the “Sutton Trilogy” in California law, mentioning these cases by name.

This rationale for adopting the PKP Act is listed in the minutes of California’s Assembly Committee on Judiciary hearing on bill A.B. 2222 on April 11, 2000, which was the first time the precursor to the PKP Act was discussed in the legislature<sup>18</sup>. The rationale section explicitly proposes the measures in the PKP Act in reaction to the court misinterpretations of “limits” and in reaction to the *Sutton* Trilogy. Given all this evidence it is highly unlikely that the PKP Act was enacted as an endogenous response to labor market conditions.

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<sup>17</sup>*Sutton v. United Airlines* (119 S. Ct. 2139 (1999)), *Murphy v. United Parcel Service, Inc.* (119 S. Ct. 2133 (1999)), and *Albertson’s, Inc. v. Kirkingburg* (119 S. Ct. 2162 (1999)).

<sup>18</sup>See [http://www.leginfo.ca.gov/pub/99-00/bill/asm/ab\\_2201-2250/ab\\_2222\\_vote\\_20000411\\_000002\\_asm\\_comm.html](http://www.leginfo.ca.gov/pub/99-00/bill/asm/ab_2201-2250/ab_2222_vote_20000411_000002_asm_comm.html) (accessed May 23, 2015).

## 3.7 Conclusion

In 2001, the Prudence Kay Poppink Act (PKP Act) came into effect in California, which significantly broadened who was considered disabled under California’s disability discrimination law. While there were five changes resulting from the PKP Act, the most important was California explicitly requiring only that individuals have an impairment that “limits” a major life activity, rather than “substantially limits” as in the federal *Americans with Disabilities Act*, to qualify as disabled under California’s Fair Employment and Housing Act (FEHA). This change reduced the legal burden for plaintiffs to prove that they were disabled and led to broader coverage of FEHA to individuals with less severe impairments.

I quantify the impacts of the PKP Act on individuals with disabilities (those who report work-limiting impairments) using a difference-in-difference (DD) regression analysis framework, comparing the disabled in California before and after the PKP Act to the disabled in other states over the same time period. I augment this to a difference-in-difference-in-differences (DDD) by adding a third comparison: disabled versus non-disabled, which controls for state by year economic shocks under the assumption that the disabled and non-disabled faced the same shocks independent of effect of the PKP Act.

The results of this analysis show that there is strong evidence that the PKP Act led to an increase in employment for the disabled. My preferred estimate is a 2.9 percentage point increase in employment, which is large relative to the employment rate of Californians with disabilities in the 1994-2000 period before the PKP Act (24.5%). This positive estimated employment effect is robust to the employment measure used (employment status or weeks worked last calendar year) and the methodology used (DD or DDD), but it is sensitive to the inclusion of state-specific linear time trends, except the logit results for employment status hold regardless of if these trends are used. There is also evidence for increased earnings. While these earnings estimates are also similar regardless of if a DD or a DDD is used, they

are sensitive to the earnings measures used and to the inclusion of state-specific linear time trends.

These results show that the PKP Act's broadening of FEHA's definition to cover more individuals with disabilities led to positive labor market outcomes for the disabled. This conclusion may apply more broadly to other groups protected by employment discrimination laws. The labor market impacts of disability discrimination laws are likely to be more negative, relative to for other protected groups (e.g., race, gender, age), due to the increased hiring costs from the reasonable accommodation requirement (Bloch, 1994). Hiring costs may be even higher under California's FEHA after the PKP Act, relative to the ADA, because the PKP Act's additional penalty for failing to discuss accommodations may push employers to provide additional accommodations (see Appendix 1 for a discussion). This further suggests that labor market effects would be more negative for disability discrimination laws than for laws protecting other groups. However, the positive estimated labor market outcomes found here suggest that discrimination protections for other groups are also likely to improve labor market outcomes.

## 3.8 References

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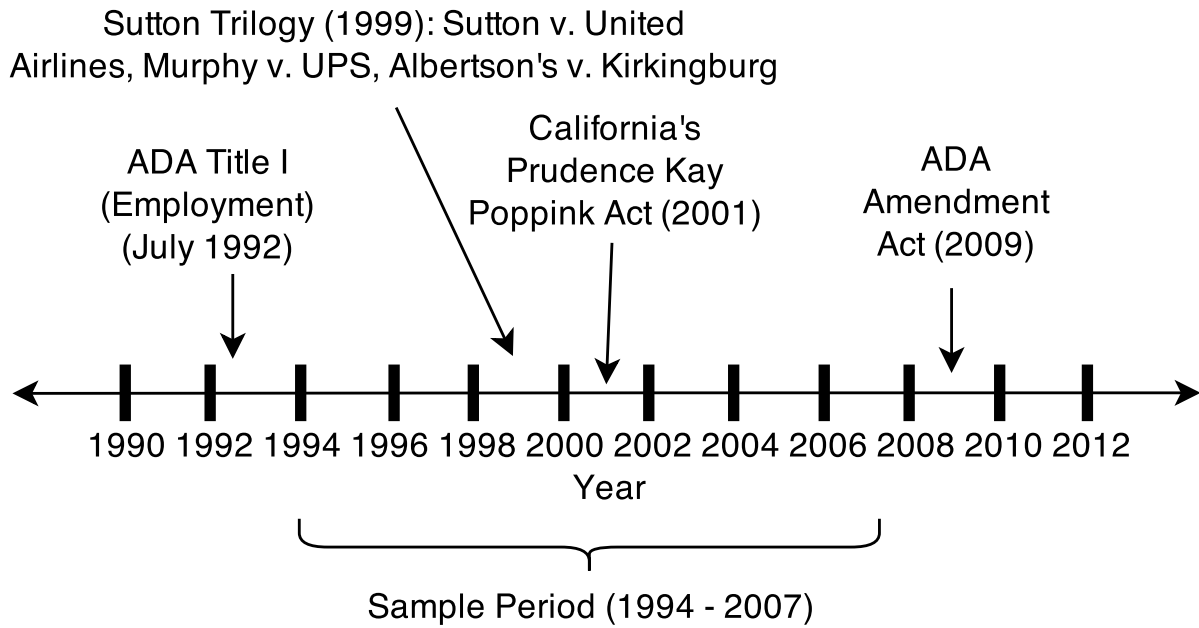
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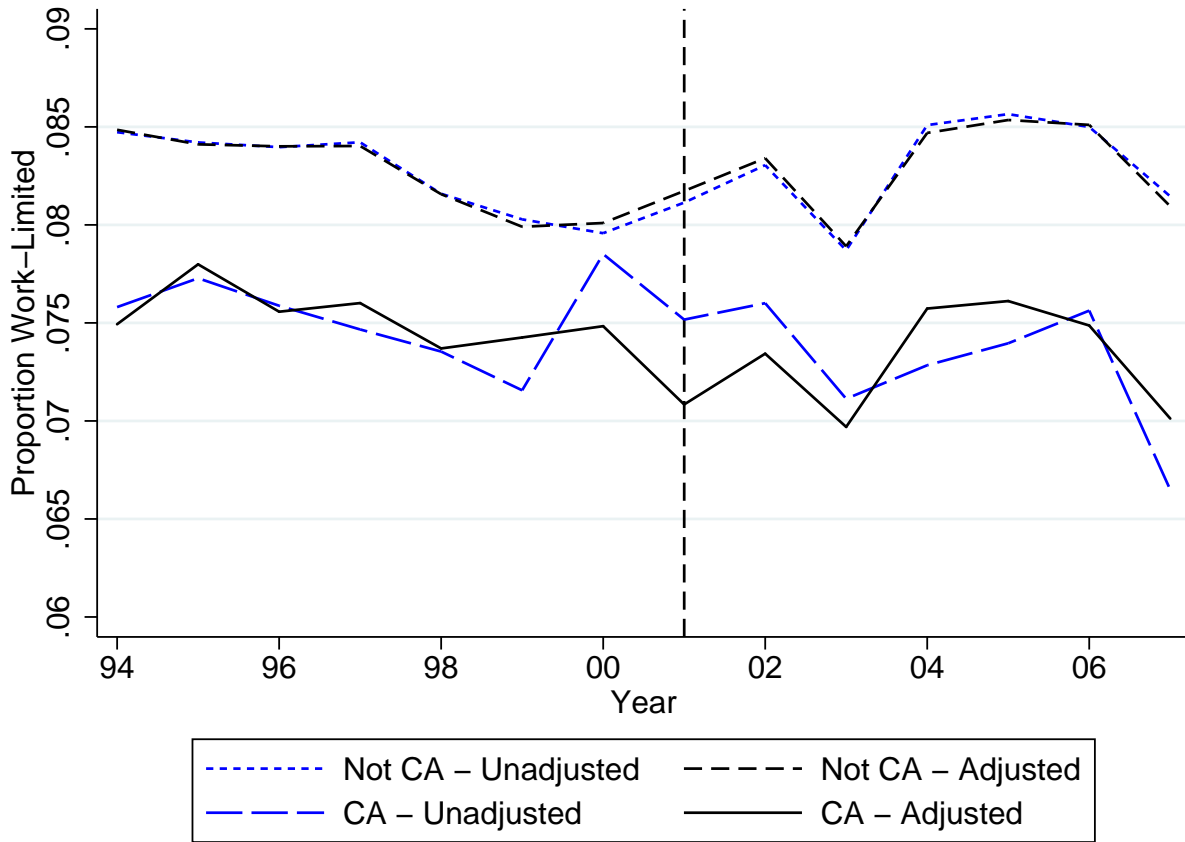
**Thompkins, Allison V.** 2013. "The Earnings Consequences of the Americans with Disabilities Act on People with Disabilities." Princeton, NJ: Mathematica Policy Research Working Paper, November 2013.

Figure 3.1: Timeline of Major Legal Events and my Sample Period



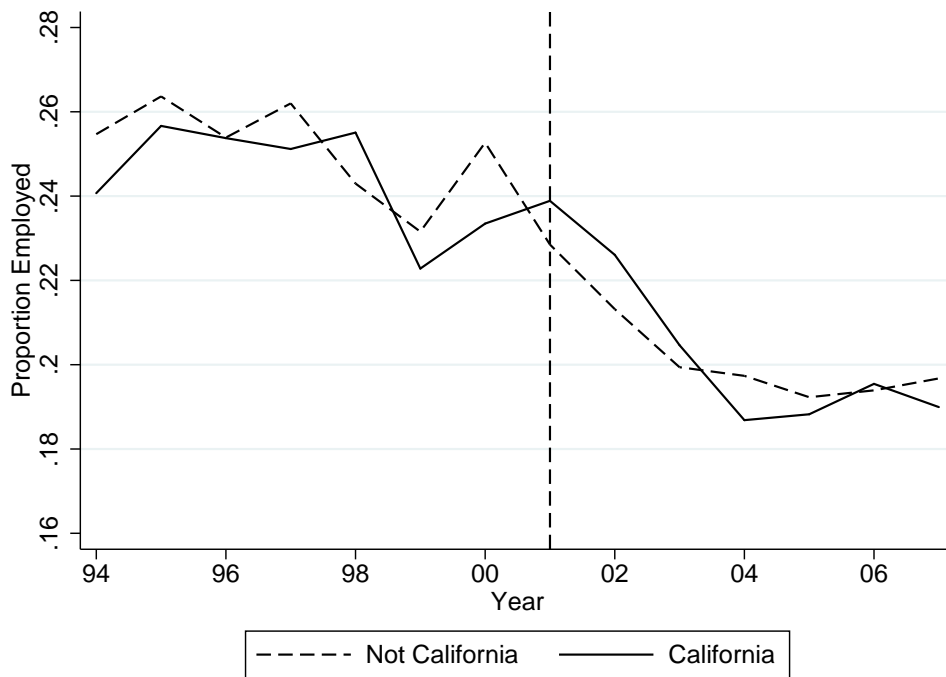
Dates of events are the dates for which the legal change became effective, not when it was passed.

Figure 3.2: Proportion of Sample with Work-Limiting Disabilities

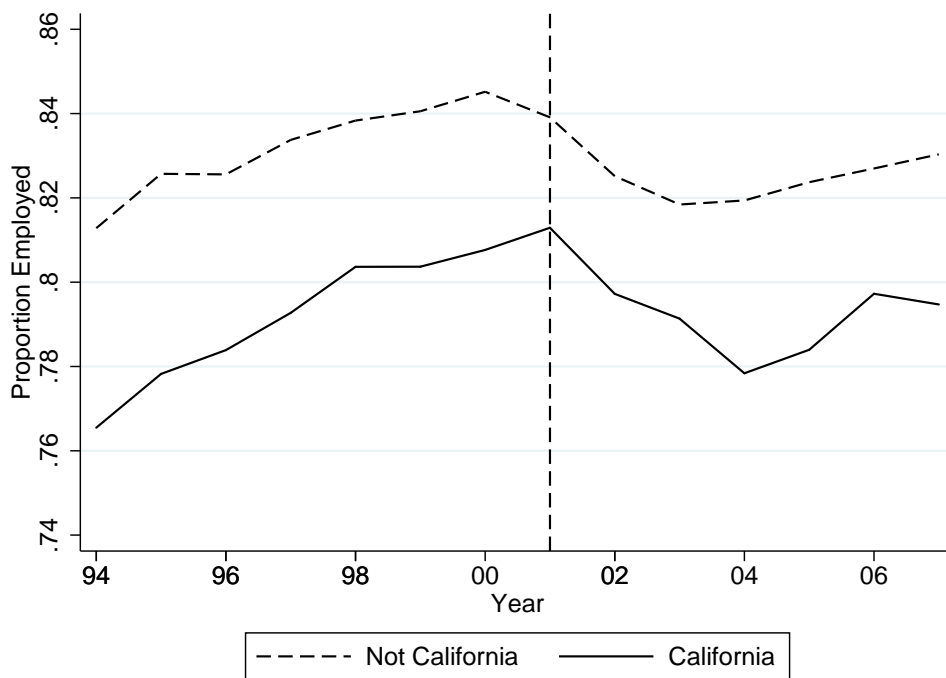


These estimates are generated from CPS ASEC for individuals 25 to 61. Individuals are deemed work-limited (disabled) if they answer “Yes” to the following question: “Does [insert name] have a physical, mental, or other health condition that limits the kind or amount of work [insert name] can do at a job or business?” Unadjusted estimates are raw averages by year while adjusted estimates are the average predicted probabilities by year from the regression in Equation 3.3. Population weights are used to create population-representative series.

Figure 3.3: Employment-to-Population Ratio by Year, Disabled and Non-Disabled



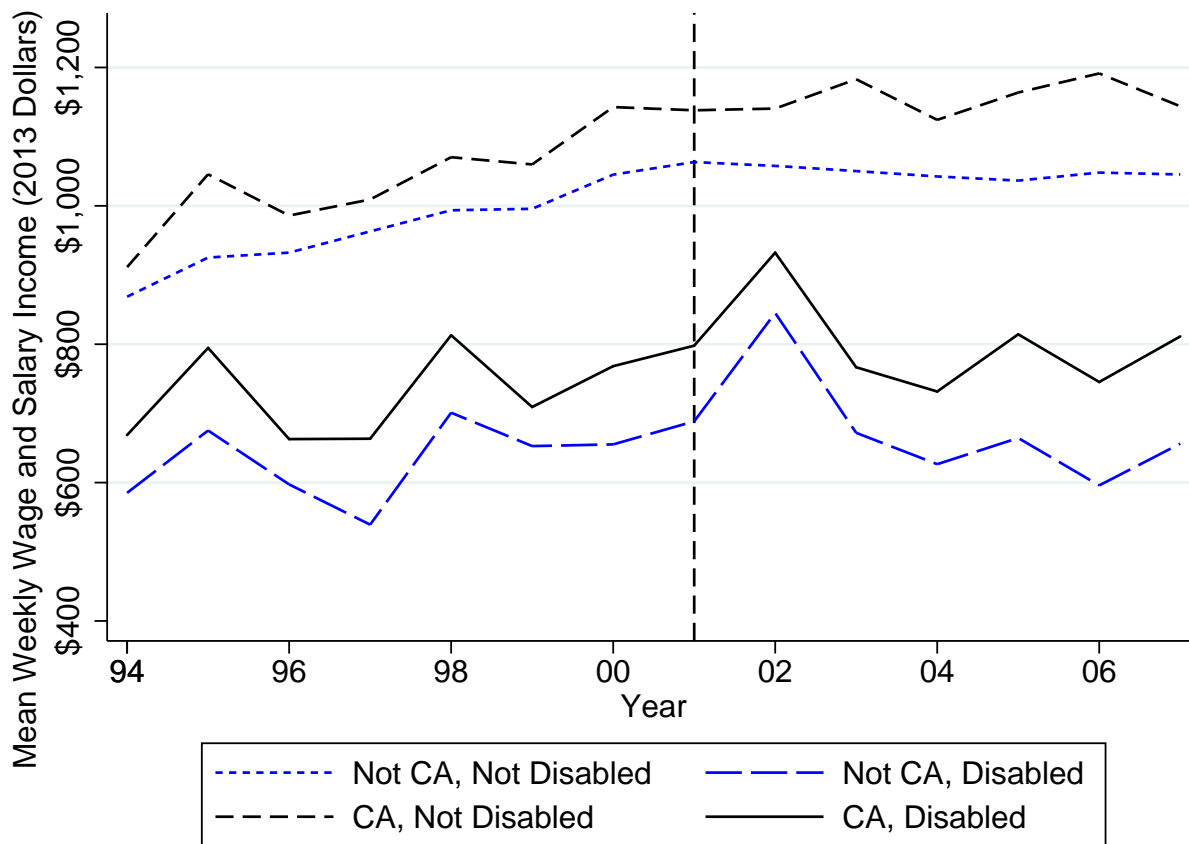
(a) Disabled



(b) Non-Disabled

See the notes to Figure 3.2. These estimates use average predicted probabilities by year from the regression in Equation 3.2.

Figure 3.4: Mean Weekly Wage and Salary Income Over Time



See the notes to Figures 3.2 and 3.3. Income is converted to 2013 dollars using the CPI variable provided by IPUMS CPS.

Table 3.1: Summary of the Empirical Literature on Disability Discrimination Laws

Study	Legal Change Studied	Survey	Measures of Disability	Outcomes Studied and Results
DeLeire (2000)	ADA	SIPP	Work-Limited (all), and by type (Physical, Mental, Other)	Hourly Wage (-) Employment (- for all, - for physical, null for mental and other)
Acemoglu and Angrist (2001)	ADA	CPS	Work-Limited	Weeks Worked (-) Weekly Earnings (-)
Beegle and Stock (2003)	Pre-ADA state laws	Census	Work-Limited	Employment (null), Annual Earnings (-) Labor force participation (-)
Kruse and Schur (2003)	ADA	SIPP	Work-Limited, Functional Limitations	Employment (- for work-limited, + for functional limitations)
Houtenville and Burkhauser (2004)	ADA	CPS	Work-Limited, Work-Limited (Two-Period)	Employment (- for work-limited, null for two-period)
Jolls and Prescott (2004)	ADA, controlling for pre-ADA state laws	CPS	Work-Limited	Employment (- in states without reasonable accommodation law, otherwise null)
Thompkins (2014)	ADAAA, ADA court cases	CPS	Work-Limited	Weekly Earnings (+)

ADA = Americans with Disabilities Act of 1990, ADAAA = ADA Amendment Act of 2008, CPS = Current Population Survey, SIPP = Survey of Income and Program Participation. Houtenville and Burkhauser (2004)'s "two-period" CPS disability measure deems individuals disabled if they reported being work-limited in two consecutive years.

Table 3.2: Summary Statistics - Demographics

		1994-2000		2001-2007	
		Non-disabled	Disabled	Non-disabled	Disabled
		(1)	(2)	(3)	(4)
Work Limited (Disabled)	CA	7.5%		7.3%	
	Not CA	8.3%		8.3%	
Age	CA	40.2	45.3	41.3	46.6
	Not CA	40.8	45.9	42.0	47.2
Female	CA	49.9%	50.7%	50.2%	51.5%
	Not CA	51.3%	50.8%	51.0%	51.9%
White	CA	80.5%	77.9%	77.9%	75.7%
	Not CA	84.2%	77.1%	82.4%	75.9%
Black/Negro	CA	6.2%	11.4%	5.9%	12.3%
	Not CA	12.1%	20.1%	12.0%	19.7%
Asian or Hawaiian/Pacific Islander	CA	11.9%	8.4%	13.9%	8.1%
	Not CA	2.7%	1.2%	4.0%	1.7%
American Indian/Aleut/Eskimo	CA	0.8%	1.9%	1.2%	1.8%
	Not CA	0.7%	1.4%	0.8%	1.4%
Other Single Race	CA	0.6%	0.4%	0%	0%
	Not CA	0.2%	0.3%	0%	0%
Two or More Races	CA	0%	0%	1.1%	2.2%
	Not CA	0%	0%	0.8%	1.5%
Hispanic	CA	28.5%	23.5%	32.9%	25.7%
	Not CA	8.6%	8.3%	11.3%	8.7%
N	CA	41,879	3,119	56,572	4,079
	Not CA	389,169	34,066	616,748	51,363

Presented here are means of summary statistics for those 25 to 61 in the CPS ASEC from 1994 to 2007. Means are weighted using population weights. The CPS revised its race and Hispanic origin questions in 2003 by removing the “Other” option and replacing it with options for two or more combinations. For simplicity, this table presents some aggregated categories: “Hawaiian/Pacific Islander only” is combined with “Asian only”, and all two or more race options are combined. Regressions include as controls indicator variables for each possible survey response rather than these aggregated groups.

Table 3.3: Summary Statistics - Marital Status and Highest Educational Attainment

		1994-2000		2001-2007	
		Non-disabled	Disabled	Non-disabled	Disabled
		(1)	(2)	(3)	(4)
Never Married/Single	CA	20.9%	24.0%	22.2%	29.4%
	Not CA	16.8%	23.7%	18.2%	25.9%
Married	CA	63.9%	47.3%	63.2%	41.7%
	Not CA	67.3%	46.9%	66.2%	43.6%
Separated	CA	3.4%	5.8%	2.7%	4.9%
	Not CA	2.8%	5.4%	2.5%	4.9%
Divorced	CA	10.4%	19.6%	10.3%	20.0%
	Not CA	11.5%	18.9%	11.7%	21.0%
Widowed	CA	1.5%	3.3%	1.5%	4.0%
	Not CA	1.6%	5.1%	1.5%	4.7%
Less than High School	CA	17.7%	26.3%	17.6%	25.7%
	Not CA	11.0%	31.1%	10.0%	25.4%
High School or GED	CA	23.8%	29.4%	21.9%	29.3%
	Not CA	34.4%	37.5%	31.4%	38.8%
Some College	CA	20.2%	21.8%	18.7%	21.3%
	Not CA	18.4%	15.8%	17.7%	17.4%
Bachelor's or Associate's Degree	CA	29.3%	18.7%	31.3%	19.7%
	Not CA	27.2%	12.6%	30.5%	15.2%
Graduate or Professional Degree	CA	9.0%	3.9%	10.4%	4.1%
	Not CA	9.0%	3.0%	10.5%	3.2%
N	CA	41,879	3,119	56,572	4,079
	Not CA	389,169	34,066	616,748	51,363

See the notes to Table 3.2. For simplicity, this table presents some aggregated categories: “Married, spouse present” is combined with “Married, spouse absent” and each possible year of incomplete education are aggregated up to “Less than High School” and “Some College”. Regressions include as controls indicator variables for each possible survey response rather than these aggregated groups.



Table 3.4: Summary Statistics - Labor Market Outcomes

		1994-2000		2001-2007	
		Non-disabled	Disabled	Non-disabled	Disabled
		(1)	(2)	(3)	(4)
Employed	CA	79.1%	24.5%	79.4%	20.4%
	Not CA	83.2%	25.2%	82.6%	20.3%
Not in Labor Force	CA	16.1%	71.1%	16.5%	76.0%
	Not CA	13.4%	71.5%	13.8%	76.5%
Unemployed	CA	4.8%	4.5%	4.2%	3.6%
	Not CA	3.4%	3.3%	3.6%	3.2%
Weeks Worked	CA	40.6	13.3	40.6	11.2
	Not CA	42.6	13.2	42.3	10.9
Received SSI/DI	CA	0.6%	35.0%	0.6%	37.4%
	Not CA	0.6%	29.7%	0.5%	29.3%
State Unemployment Rate	CA	6.6%		5.8%	
	Not CA	4.8%		5.1%	
Extra Available Weeks of Unemployment Insurance	CA	0		4.4	
	Not CA	0		4.4	
N	CA	41,879	3,119	56,572	4,079
	Not CA	389,169	34,066	616,748	51,363
Conditional on Unemployment:					
Weeks Unemployed	CA	21.1	24.7	20.1	28.4
	Not CA	18.0	23.1	18.7	23.7
N	CA	2,040	137	2,273	141
	Not CA	12,968	1,165	21,123	1,682
Conditional on Employment:					
Annual Wage & Salary Income	CA	\$51,423	\$29,019	\$57,533	\$32,976
	Not CA	\$48,231	\$24,825	\$52,656	\$26,390
Weekly Wage & Salary Income	CA	\$1,069	\$789	\$1,192	\$866
	Not CA	\$987	\$696	\$1,075	\$751
N	CA	33,716	1,076	44,274	1,110
	Not CA	347,417	11,935	503,106	14,161
Weekly Earnings	CA	\$982	\$812	\$1,094	\$814
	Not CA	\$886	\$583	\$962	\$657
N	CA	5,786	137	6,961	127
	Not CA	67,645	1,720	97,114	1,992

See the notes to Table 3.2. All income variables are converted to 2013 dollars using the provided CPI variable. Weekly wage and salary income is calculated by dividing the annual wage and salary income by weeks worked for those that report non-zero income and weeks worked. Mean weekly earnings is derived from an explicit question asked only to those in the outgoing rotation group, which is about one quarter of the households in the full sample.

Table 3.5: Effects on Weeks Worked

(1)	(2)	(3)	(4)
	Identification:		
DD	DD	DDD	DDD
	State-Specific Linear Time Trends:		
No	Yes	No	Yes
0.52	1.42	0.36	1.32
[-0.05, 1.09]*	[0.63, 2.21]***	[-0.16, 0.88]	[0.43, 2.22]***
(-1.73, 5.52)	(0.24, 3.35)**	(-1.22, 4.67)	(0.53, 2.11)**

See the notes to Table 3.4 . Regressions are weighted using population weights. All regressions include indicator variables for each possible gender, age in years, race, ethnicity, marital status, and highest educational attainment. The DD regressions use a sample of only the disabled, and include state and year fixed effects, the number of extra weeks of unemployment insurance available, via Farber and Valletta (2015), indicator variables for minor law changes in other states (see Appendix 2), indicator variables for four states programs that provided incentives to hire individuals with disabilities, via Neumark and Grijalva (2013), and state unemployment rates. The DDD regressions include state by year, disabled by year, and state by disabled fixed effects, and above listed state-level policy and economic controls, each interacted with a disabled indicator variable. Below the coefficient estimates are the 95% confidence intervals, in brackets, using state-clustered standard errors, and below these are 95% Conley-Taber confidence intervals, in parentheses. \* = Statistically significant at 90% level, \*\* = 95% level, and \*\*\* = 99% level. In this application it is not possible to test for 99% significance using Conley-Taber confidence intervals.

Table 3.6: Effects on Employment

(1)	(2)	(3)	(4)
	Identification:		
DD	DD	DDD	DDD
	State-Specific Linear Time Trends:		
No	Yes	No	Yes
(a) Linear Probability Model			
0.021	0.039	0.017	0.036
[0.009, 0.034]*** (-0.038, 0.116)	[0.023, 0.055]*** (0.013, 0.087)**	[0.005, 0.029]*** (-0.019, 0.104)	[0.019, 0.052]*** (0.016, 0.058)**
(b) Logit			
0.141	0.232	0.112	0.190
[0.066, 0.221]*** (N/A)	[0.131, 0.332]*** (N/A)	[0.040, 0.184]*** (N/A)	[0.086, 0.293]*** (N/A)
<i>Marginal Effect at Means</i>			
0.023	0.037	0.018	0.029
[0.011, 0.035]*** (N/A)	[0.021, 0.053]*** (N/A)	[0.009, 0.028]*** (N/A)	[0.013, 0.045]*** (N/A)
(c) Logit (Unweighted)			
0.143	0.264	0.138	0.225
[0.081, 0.206]*** (0.014, 0.358)**	[0.181, 0.348]*** (0.220, 0.373)**	[0.082, 0.195]*** (0.035, 0.277)**	[0.138, 0.313]*** (0.169, 0.288)**
<i>Marginal Effect at Means</i>			
0.023	0.043	0.020	0.033
[0.013, 0.034]*** (N/A)	[0.029, 0.056]*** (N/A)	[0.012, 0.029]*** (N/A)	[0.020, 0.046]*** (N/A)

See the notes to Table 3.4 and 3.5. Regressions are weighted using population weights except Panel (c). Conley-Taber confidence intervals cannot be calculated for the weighted logit regressions or for the marginal effects.

Table 3.7: Effects on Unemployment and Unemployment Duration

(1)	(2)	(3)	(4)
	Identification:		
DD	DD	DDD	DDD
	State-Specific Linear Time Trends:		
No	Yes	No	Yes
(a) Unemployed - Linear Probability Model			
-0.003	-0.016	-0.004	-0.013
[-0.008, 0.001]	[-0.021, -0.011]***	[-0.008, 0.001]	[-0.019, -0.008]***
(-0.017, 0.019)	(-0.026, -0.006)**	(-0.017, 0.011)	(-0.020, -0.005)**
(b) Unemployed - Logit			
-0.036	-0.536	-0.088	-0.342
[-0.201, 0.129]	[-0.726, -0.346]***	[-0.238, 0.061]	[-0.545, -0.139]***
(N/A)	(N/A)	(N/A)	(N/A)
<i>Marginal Effect at Means</i>			
-0.001	-0.014	-0.003	-0.010
[-0.005, 0.003]	[-0.019, -0.009]***	[-0.007, 0.0017]	[-0.015, -0.004]***
(N/A)	(N/A)	(N/A)	(N/A)
(c) Unemployed - Logit (Unweighted)			
-0.004	-0.444	-0.050	-0.282
[-0.141, 0.133]	[-0.633, -0.255]***	[-0.151, 0.052]	[-0.470, -0.094]***
(-0.116, 0.097)	(-0.523, -0.385)**	(-0.139, 0.049)	(-0.342, -0.229)**
<i>Marginal Effect at Means</i>			
-0.000	-0.012	-0.001	-0.008
[-0.004, 0.004]	[-0.017, -0.007]***	[-0.004, 0.001]	[-0.013, -0.003]***
(N/A)	(N/A)	(N/A)	(N/A)
(d) Unemployment Durations (Weeks)			
4.09	0.87	1.90	-3.81
[0.63, 7.55]**	[-5.66, 7.39]	[-1.05, 4.86]	[-9.33, 1.70]
(-14.11, 25.07)	(-7.23, 8.41)	(-9.84, 20.70)	(-8.33, 2.09)

See the notes to Tables 3.4, 3.5, and 3.6.

Table 3.8: Effects on Labor Force Non-Participation

(1)	(2)	(3)	(4)
	Identification:		
DD	DD	DDD	DDD
	State-Specific Linear Time Trends:		
No	Yes	No	Yes
<i>(a) Linear Probability Model</i>			
-0.018	-0.023	-0.014	-0.023
[-0.030, -0.006]*** (-0.101, 0.041)	[-0.040, -0.006]** (-0.051, 0.006)	[-0.024, -0.003]** (-0.086, 0.025)	[-0.040, -0.005]** (-0.042, 0.002)*
<i>(b) Logit</i>			
-0.121	-0.126	-0.096	-0.133
[-0.188, -0.053]*** (N/A)	[-0.227, -0.024]** (N/A)	[-0.155, -0.037]*** (N/A)	[-0.237, -0.028]** (N/A)
<i>Marginal Effect at Means</i>			
-0.022	-0.023	-0.012	-0.016
[-0.034, -0.010]*** (N/A)	[-0.041, -0.004]** (N/A)	[-0.019, -0.004]*** (N/A)	[-0.029, -0.003]** (N/A)
<i>(c) Logit (Unweighted)</i>			
-0.127	-0.174	-0.129	-0.180
[-0.183, -0.071]*** (-0.314, 0.002)	[-0.261, -0.088]*** (-0.275, -0.121)**	[-0.176, -0.082]*** (-0.246, -0.025)**	[-0.272, -0.088]*** (-0.245, -0.103)**
<i>Marginal Effect at Means</i>			
-0.023	-0.032	-0.015	-0.021
[-0.033, -0.013]*** (N/A)	[-0.048, -0.016]*** (N/A)	[-0.020, -0.010]*** (N/A)	[-0.032, -0.010]*** (N/A)

See the notes to Tables 3.4, 3.5, and 3.6.

Table 3.9: Effects on Earnings

(1)	(2)	(3)	(4)
	Identification:		
DD	DD	DDD	DDD
	State-Specific Linear Time Trends:		
No	Yes	No	Yes
(a) Log Annual Wage and Salary Income			
0.074	0.157	0.046	0.115
[0.015, 0.134]**	[0.042, 0.273]***	[-0.003, 0.094]*	[0.005, 0.224]**
(-0.218, 0.315)	(0.014, 0.279)**	(-0.168, 0.241)	(-0.000, 0.232)*
(b) Log Weekly Wage and Salary Income			
0.065	0.020	0.045	-0.009
[0.025, 0.105]***	[-0.067, 0.106]	[0.014, 0.076]***	[-0.090, 0.073]
(-0.147, 0.227)	(-0.075, 0.110)	(-0.098, 0.084)	(-0.98, 0.084)
(c) Log Weekly Earnings			
-0.057	0.466	-0.067	0.402
[-0.179, 0.064]	[0.240, 0.692]***	[-0.184, 0.049]	[0.230, 0.574]***
(-0.553, 0.347)	(0.163, 0.582)**	(-0.455, 0.368)	(0.133, 0.505)**

See the notes to Tables 3.4, 3.5, and 3.6.

Table 3.10: Estimated Change in Reporting Being Work-Limited in California after the Prudence Kay Poppink Act

(1)	(2)	(3)
	Model:	
Linear Probability	Logit	Logit
	Population Weights Used:	
Yes	Yes	No
(a) Coefficient Estimates:		
-0.003	-0.058	-0.020
[-0.006, 0.000]*	[-0.103, -0.013]**	[-0.058, 0.018]
(-0.019, 0.009)	(N/A)	(-0.080, 0.061)
(b) Marginal Effects at Means:		
	-0.003	-0.001
	[-0.005, -0.001]**	[-0.003, 0.001]
	(N/A)	(N/A)

See the notes to Tables 3.5 and 3.6. These estimates come from Equation 3.3.

## 3.10 Appendix 1: Additional Details on the Prudence Kay Poppink Act

The Prudence Kay Poppink Act (PKP Act) led to five changes in disability discrimination law in California. The first four broaden who can be considered disabled under California law, while the last seeks to expand reasonable accommodations by forcing employers to discuss reasonable accommodations more diligently with employees.

The first and arguably the most important of the changes in the PKP Act is that the PKP Act made it explicit that the statement in California's Fair Employment and Housing Act (FEHA) that an impairment need only "limit" a major life activity is weaker than the ADA's "substantially limits" requirement. Although FEHA always had a "limits" requirement written in law, even before the PKP Act, the FEHA's "limits" requirement was always interpreted consistently with the ADA's "substantially limits" requirement (Long, 2004). For example, in *Colmenares v. Braemer Country Club, Inc.*, 63 P.3d 220, 223 (Cal. 2003), the plaintiff was deemed not disabled because his case preceded the PKP Act, when FEHA's "limits" was interpreted the same as the ADA's "substantially limits", but he would be considered disabled under FEHA after the PKP Act.

Second, the PKP Act rejected key decisions in the *Sutton* trilogy of U.S. Supreme Court cases<sup>19</sup> which restricted who was considered disabled under the ADA and in many states with state laws tied to ADA case law, such as California. These cases deemed individuals to be not disabled if mitigating measures, such as glasses, medication, or assistive devices, made the individual no longer "substantially limited" in a major life activity. The PKP Act, however, explicitly states that: "'Limits' shall be determined without regard to mitigating measures, such as medications, assistive devices, prosthetics, or reasonable accommodations, unless the

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<sup>19</sup>*Sutton v. United Airlines* (119 S. Ct. 2139 (1999)), *Murphy v. United Parcel Service, Inc.* (119 S. Ct. 2133 (1999)), and *Albertson's, Inc. v. Kirkingburg* (119 S. Ct. 2162 (1999)).



mitigating measure itself limits a major life activity.” (Cal Gov Code §12926(l)(1)(B)(i)) This further expands who is considered disabled under California’s FEHA relative to the ADA.

Third, the PKP Act also added “working” to the list of major life activities, while “working” was not considered as a major life activity under the ADA case law. The inclusion of working as a major life activity avoids the so-called “single job rule”, where case law established that it is not enough for workers to argue that their impairment precludes them from a single job or narrow range of jobs, they must argue that it precludes them from a class of jobs or a broader range of jobs (Long, 2008). There have been several cases where plaintiffs could only establish that their condition limited the major life activity of working only in their particular job<sup>20</sup>. The PKP Act resolved this issue by adding to FEHA: “...Further, under the law of this state, “working” is a major life activity, regardless of whether the actual or perceived working limitation implicates a particular employment or a class or broad range of employments.” (Cal Gov Code §12926.1(c)) Thus, the PKP Act’s addition of “working” as a major life activity made it easier for individuals to be deemed disabled.

Fourth, the PKP Act added to FEHA that: “Physical and mental disabilities include, but are not limited to, chronic or episodic conditions such as HIV/AIDS, hepatitis, epilepsy, seizure disorder, diabetes, clinical depression, bipolar disorder, multiple sclerosis, and heart disease.” (Cal Gov Code §12926.1(c)) These conditions were explicitly considered as disabilities after the PKP Act, regardless of if the “limits” requirement was met,<sup>21</sup> but these conditions were not necessarily covered under the ADA or the FEHA before the PKP Act. Episodic conditions such as epilepsy, seizure disorder, episodic depression, bipolar disorder, and multiple sclerosis were not covered by the ADA or the FEHA before the PKP Act,

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<sup>20</sup>See e.g., *Sutton v. United Airlines* (119 S. Ct. 2139 (1999)); *Diffey v. Riverside County Sheriff’s Dept.*, 101 Cal. Rptr. 2d 353 (2000); *Toyota Mfg., Ky., inc. v. Williams* (00-1089) 534 U.S. 184 (2002) 224 F.3d 840

<sup>21</sup>For example, *Maureen K. v. Tuschka* (2013, 2d Dist) 2013 Cal App states that the jury should never have been consulted as to if the plaintiff’s HIV positive status made her legally disabled, as she was disabled as a matter of law.

since these episodic conditions were deemed not to be disabilities by court decisions<sup>22</sup>. For the less episodic conditions that became covered after the PKP Act: HIV/AIDS, hepatitis, diabetes, depression, and heart disease, these were not explicitly covered under the ADA or the pre-PKP Act FEHA. They were covered if they met the usual requirement of their condition substantially limiting one or more major life activities, but this was often hard to establish.<sup>23</sup>

Fifth, PKP Act made it a violation of FEHA: “For an employer or other entity covered by this part to fail to engage in a timely, good faith, interactive process with the employee or applicant to determine effective reasonable accommodations...” (Cal. Gov. Code §12940(n)). While the ADA also requires employers to interact in good faith with employees regarding reasonable accommodations, the requirement under FEHA after the PKP Act is different than the ADA. Byron (2012) states: “Under the FEHA, participation is mandatory. Under the ADA, the better practice is to engage in the interactive process because the employer bears the burden of persuasion as to whether participation should be excused by proving that no reasonable accommodation was available.<sup>24</sup>” Thus, it is possible for an employee to win a claim against an employer for failing to engage in the interactive process to determine reasonable accommodations and at the same time that employee could lose their case that reasonable accommodation was not given<sup>25</sup>. While the policies of which accommodations are reasonable under the ADA and FEHA are the same, this requirement places addition pressure

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<sup>22</sup>For bipolar disorder, see a discussion in Long (2008), who cites *Horwitz v. L & J.G. Stickley, Inc.*, 122 F. Supp. 2d 350 (N.D.N.Y. 2000). See *Menchaca v. Maricopa Cmty. Coll.*, 595 F. Supp. 2d 1063, 1077 (D. Ariz. 2009), which is discussed by Harned and McBride (2010), as an example of how episodic conditions were not covered under the ADA. See *Todd v. Academy Corp.*, 57 F. Supp. 2d 448, 453 (S.D. Tex. 1999) as an example for epilepsy and see *Pimental v. Dartmouth-Hitchcock Clinic*, 236 F. Supp. 2d 177, 182-83 (D.N.H. 2002) as an example for conditions in remission.

<sup>23</sup>For HIV, see a discussion in Long (2008), who cites *Blanks v. Sw. Bell Commc’ns, Inc.*, 310 F.3d 398 (5th Cir. 2002) (HIV). For heart disease, see *Wallace v. Mantych Metalworking*, 189 Ohio App.3d 25, 2010-Ohio-3765, which is discussed in <http://www.ohioemployerlawblog.com/2010/08/real-life-example-of-difference-between.html> (accessed March 1, 2015).

<sup>24</sup>See *Dark v. Curry County* (9th Cir. 2006) 451 F.3d 1078, 1088 as cited by Byron (2012).

<sup>25</sup>See *Wysinger v. Automobile Club of Southern California* (2007) 157 Cal.App.4th 413, 424-425, which is discussed in <http://www.mondaq.com/unitedstates/x/160950/Discrimination+Disability+Sexual+Harassment/Disability+Discrimination+And+The+Obligation+To+Provide+Reasonable+Accommodation+The+Interactive+Process> (accessed March 8, 2015)

on employers to discuss reasonable accommodations with employees, perhaps making it more likely that accommodation agreements are made.

### 3.11 Appendix 2: Details on Law Change Controls

There were three states other than California that changed their disability discrimination laws during the sample period of 1994 to 2007, with these changes being non-trivial but less important than California's PKP Act. These changes occurred in Maine in 2006 and 2007, Rhode Island in 2000, Washington in 2007. Because of these changes, I include four control variables: one each for Washington and Rhode Island, and two for Maine, with each variable equaling one when that state has the new law in effect.

#### Maine

Maine's disability discrimination laws followed those of the ADA until *Whitney v. Wal-Mart* 2006 ME 37<sup>26</sup> where the Maine Supreme Judicial Court ruled that Maine's definition of disability did not require the "substantially limits" requirement of the ADA. In response to *Whitney*, Maine's legislature passed a bill in 2007 (Laws 2007 c. 385, §3), effective June 21, 2007, which overturned *Whitney*, while also modifying the definition of disability slightly beyond that of the ADA at the time. These changes mirror a portion of the changes in the PKP Act.

One change in this act was to deem individuals with certain impairments disabled regardless of if their impairments "substantially limit" a major life activity. These conditions are:

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<sup>26</sup>See [https://scholar.google.com/scholar?case?case=12271024718339929818&hl=en&as\\_sdt=6&as\\_vis=1&oi=scholar](https://scholar.google.com/scholar?case?case=12271024718339929818&hl=en&as_sdt=6&as_vis=1&oi=scholar) (accessed Apr. 18, 2015)

“...absent, artificial or replacement limbs hands, feet or vital organs: alcoholism; amyotrophic lateral sclerosis; bipolar disorder blindness or abnormal vision loss; cancer; cerebral palsy; chronic obstructive pulmonary disease; Crohn’s disease: cystic fibrosis; deafness or abnormal hearing loss diabetes; substantial disfigurement; epilepsy; heart disease HIV or AIDS; kidney or renal diseases; lupus; major depressive disorder; mastectomy mental retardation; multiple sclerosis; muscular dystrophy; paralysis; Parkinson’s disease; pervasive developmental disorders; rheumatoid arthritis; schizophrenia and acquired brain injury;” (MRSA §4553-A (1)(B))

Another change was to ignore mitigating measures, contrary to the *Sutton* trilogy. To control for both *Whitney* and the subsequent act, I include an indicator variable for Maine in 2006 (the year of *Whitney*) and another indicator variable for Maine in 2007, when the act took effect.

## Rhode Island

Rhode Island amended their disability discrimination law (Laws 2000, c. 507, §2) effective July 22, 2000, to ensure that mitigating measures were not considered in the determination of disability. The act stated that: “...whether a person has a disability shall be determined without regard to the availability or use of mitigating measures, such as reasonable accommodations, prosthetic devices, medications or auxiliary aids.” (RI ST §42-87-1 (A)(1)(e)) This corresponds to one of the changes in the PKP Act.

## Washington

Washington’s definition of disability was vague before an amendment (Laws 2007, c. 317), effective May 4, 2007, changed Washington’s definition to follow a medical diagnosis definition like Connecticut, Illinois, New Jersey, and New York. These medical diagnosis definitions deem individuals to be disabled without requiring them to show that their condition “substantially limits” a major life activity (Neumark, Song and Button 2015; Long 2004). Prior to this amendment, Wash. Rev. Code §49.60.180 prohibited discrimination on the basis of physical disability, but the term was not well defined (Long, 2004). It appears that Washington’s lack of definition caused courts to rely on the federal definition of disability, which included the “substantially limits” requirement. After the 2007 amendment, Washington law stated that:

“Disability’ means the presence of a sensory, mental, or physical impairment that:

- (i) Is medically cognizable or diagnosable; or
- (ii) Exists as a record or history; or
- (iii) Is perceived to exist whether or not it exists in fact.” (Wash. Rev. Code 49.60.040 (7)(a))

### 3.12 Appendix 3: Constructing Conley-Taber Confidence Intervals

This appendix provides additional details on my construction of Conley-Taber confidence intervals. For the DD (Equation 3.1), I follow the procedure outlined by Conley and Taber (2011) exactly, using their code and only making minor modifications (e.g., adding

sampling weights). The general procedure, closely following the exposition in Conley and Taber (2011), is as follows. Consider the following DD regression:

$$Y_{jt} = \alpha d_{jt} + X'_{jt}\beta + \theta_j + \gamma_t + \epsilon_{jt} \quad (3.4)$$

In this equation, groups are denoted by  $j$ , where group  $j = 1$  is treated, and all others  $j = 2 \dots N$  are untreated, and time is denoted by  $t$ , ranging from  $t = 1 \dots T$ . The indicator variable  $d$  denotes treatment status.  $X_{jt}$  is a vector of regressors,  $\theta_j$  are group fixed effects and  $\gamma_t$  are time fixed effects.

Consider a generic variable  $Z_{jt}$ . Then define the following averages:

$$\begin{aligned} \bar{Z}_j &= \frac{1}{T} \sum_{t=1}^T Z_{jt} \\ \bar{Z}_t &= \frac{1}{N} \sum_{j=1}^N Z_{jt} \\ \bar{Z} &= \frac{1}{NT} \sum_{j=1}^N \sum_{t=1}^T Z_{jt} \end{aligned} \quad (3.5)$$

Then define variable  $\tilde{Z}_{jt}$  such that it equals the residual of the projection of  $Z_{jt}$  on the group and time indicators:

$$\tilde{Z}_{jt} = Z_{jt} - \bar{Z}_j - \bar{Z}_t + \bar{Z} \quad (3.6)$$

The DD regression can be re-written using this notation:

$$\tilde{Y}_{jt} = \alpha \tilde{d}_{jt} + \tilde{X}'_{jt}\beta + \tilde{\epsilon}_{jt} \quad (3.7)$$

After running the above regression, group the residuals,  $\hat{\epsilon}_{jt}$ , by each group  $j$ . Conley-Taber confidence intervals can be created by calculating the following for each group's residuals:

$$\hat{\Gamma} = \frac{\sum_{t=1}^T (d_{1t} - \bar{d}_1) \hat{\epsilon}_{jt}}{\sum_{t=1}^T (d_{1t} - \bar{d}_1)^2} \quad (3.8)$$

The distribution of  $\hat{\Gamma}$  can be used to construct Conley-Taber confidence intervals by taking percentiles of this distribution. In my DD application,  $j$  indexes states, of which there are 50, plus DC. Given that there are only 51 groups, it is not possible to calculate 99% confidence intervals, but 95% are possible.

The procedure to construct Conley-Taber confidence intervals for the DDD is only slightly different.  $\tilde{Y}_{jt}$  is instead the projection of  $Y_{jt}$  on disabled by state, state by year, and disabled by year indicators. Instead of the groups,  $j$ , being states, as in the DD, the groups in the DDD are defined by disabled by state, which is 102 groups. Thus this added disabled versus non-disabled dimension doubles the number of groups, although there are still not enough to calculate 99% confidence intervals (at least 200 would be required).