Evaluating the Unemployment Insurance Modernization Provisions of the American Recovery and Reinvestment Act

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ABSTRACT

Despite the high current unemployment rate and the corresponding importance of the American unemployment insurance (UI) system, scholarship on UI examines only a few aspects of UI policy—such as optimal benefit levels and extended duration—and has largely failed to address critical policy issues pertaining to UI eligibility and utilization. I measure the increase in UI utilization and total UI benefit receipts caused by the implementation of the Unemployment Insurance Modernization Provisions, which were incentivized by the American Recovery and Reinvestment Act of 2009 through categorical grants totaling $4.4 billion. I compile and analyze a large state-level panel dataset containing information on state implementation decisions and unemployment utilization rates. Because a non-random selection of states implements these provisions, I account for sample selection bias using a modified control function approach. I find that implementing the eligibility modernizations resulted in more than 1,500,000 people receiving UI benefits between 2009 and 2011. Moreover, I find that those people received approximately $8.0 billion in total UI benefits, which is nearly double the federal government's cost for incentivization. My findings suggest that the ARRA's modernizations were an effective tool for broadening UI eligibility and a substantial advancement in the U.S. unemployment insurance system.

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To my family, and to Julia.

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1. INTRODUCTION

Unemployment is one of the central concerns of contemporary labor economics, and today's high unemployment rate makes it one of the central concerns of any American citizen. As a consequence, the design of unemployment insurance (UI) is a key policy issue, receiving substantial attention in both policy debates and academic research. However, while there is a wealth of research on the effects of the level and the duration of unemployment benefits, questions related to the third aspect of UI, eligibility, have not been adequately addressed.[1] This paper helps to fills that gap in the literature by examining the effects of key recent changes in UI eligibility policy.

The American Recovery and Reinvestment Act of 2009 engineered a major UI eligibility reform with its Unemployment Compensation Modernization Incentive Payments provision, which I refer to as the MIP Act. The MIP Act offered states categorical grants of up to a total of $7 billion (divided proportionally by population) in return for those states implementing designated UI modernizations, each of which increases either UI eligibility or UI benefit generosity (targeting low-income job-losers).[2]

Evaluating the effects of the MIP Act is important for at least two reasons. First, the MIP Act was a large-scale policy which was intended to expand UI eligibility. A natural question, therefore, is whether implementing the modernization provisions of ARRA did in fact increase UI utilization. The answer to this question sheds light on which groups should be targeted, and what kinds of policies should be adopted, by state governments seeking to expand UI coverage. Second, by increasing UI eligibility, the

[1] Nicholson (1997) notes that "there has been comparatively little quantitative research on the effect of UI eligibility provisions…this seems like a very promising area for future research" (106). In the policy context, Kletzer and Rosen (2006) point out that state governments have not significantly altered their UI eligibility requirements since the policies' inception, despite substantial changes in the composition of the unemployment pool.
[2] See the American Recovery and Reinvestment Act of 2009 (§2003).

modernization provisions were thought to improve consumption smoothing possibilities for unemployed individuals, thereby stimulating their spending and stabilizing the American economy.[3] Understanding the result of these modernizations is thus an important step in comparing and evaluating the effectiveness of stimulus programs.

This paper studies the effects of three of the designated modernizations on UI utilization and total UI benefits. In particular, I study the Alternative Base Period (ABP), the Part-Time Work Provision (PTW), and the Compelling Family Reasons Provision (CFP), the three modernizations that directly affect UI eligibility. I answer two questions about each modernization: (1) how many people collected UI because of it, and (2) how much money in benefits those individuals received.

Estimation of these effects is complicated by two issues. The first is data availability. The lack of individual-level data prevents me from directly estimating the effect of the modernizations on affected individuals. Moreover, available data does not include all the relevant variables that determine UI utilization. Consequently, I must use proxy variables. The second issue is sample selection: a non-random selection of states implemented modernizations. My empirical strategy addresses these issues.

I employ a difference-in-differences framework using a state-level panel dataset to estimate the increase in UI utilization and total UI benefits caused by each of three of the modernizations. I propose a theoretical framework and provide a host of proxy variables to control for extraneous variation in both dependent variables of interest. I then present an econometric strategy that accounts for sample selection bias using a modified control function approach motivated by Heckman (1979) and Heckman and Navarro-Lozano (2004).

[3] For evidence of this individual consumption-smoothing behavior induced by UI, see Gruber (1997).

I find that the ABP increases UI utilization by 14% in implementing states, while PTW and CFP increase utilization by 10% and 5.4%, respectively. Moreover, I show that individuals collecting UI under these newly implemented modernizations received $8.0 billion in benefits between 2009 and 2011, nearly twice the $4.4 billion paid by the federal government to incentivize UI modernization. My findings imply that the modernizations were successful in considerably expanding UI eligibility.

Section 2 provides background on the MIP Act and related scholarship. Section 3 presents my data sources, and Section 4 states my theoretical framework. Section 5 discusses my empirical methods, focusing on identification and sample selection bias. Section 6 presents my results, and Section 7 discusses robustness. Section 8 concludes.

2. BACKGROUND

2.1 LEGAL HISTORY

Each US state maintains its own UI system funded by a combination of state and federal taxes. Individuals are typically eligible for up to 26 weeks of UI benefits (which are proportional to their former wage) upon losing their employment at no fault of their own, if they meet certain monetary and non-monetary eligibility criteria. US states are natural grounds for experimentation in all sorts of UI policy, with each state implementing its own policies.[1] The MIP Act incentivized states to implement five of those policies, described in Table 2.1. I study the policies that expand UI eligibility, which are the first three modernizations in that Table.[2] States received one-third of their designated MIP Act funding for implementing the Alternative Base Period, and the

[1] For instance, in January 2005, 17 states had already implemented the ABP, and 6 states had already implemented PTW.

[2] For an evaluation of a similar UI job-training provision, see LaLonde (1995), who argues the negative net benefits of such programs. I do not evaluate this modernization here.

Table 2.1: The MIP Act's Modernizations		
Modernization	**Abbr**	**Effect of Modernization**
Alternative Base Period	ABP	Allows workers to calculate their eligibility-determining earnings as the sum of the four most recent quarters of earnings, instead of skipping the most recent quarter and summing the four previous quarters
Part-Time Work Provision	PTW	Extends UI to job-losers who are only searching for part-time work
Compelling Family Reasons Provision	CFP	Extends UI to job-losers who lost their jobs because of (1) domestic violence, (2) an immediate family member's illness, or (3) relocation for a spouse's employment
Dependent Benefits Provision	DBP	Increases benefits for UI recipients with dependents
Training Program Benefits Provision	TPP	Allows UI recipients to collect an additional 26 weeks of UI in return for participating in an approved job-training program

remaining two-thirds of their funding for implementing any two of the four other modernizations, even if they had implemented the modernizations in the distant past.

These policies had three stated goals: to modernize UI systems by accounting for the changing composition of the unemployment pool; to expand UI eligibility for more job-losers who were victims of the Great Recession; and to stimulate the economy by providing substantial UI payments to individuals in need, who are likely to spend that money in the short-term.[3] This paper studies the effectiveness of the first three of these modernizations, the three policies that directly affect UI eligibility.[4]

2.2 LITERATURE REVIEW

Prior to the MIP Act, studies of non-monetary eligibility, or UI eligibility criteria other than the minimum wage workers must have received in their most recent jobs,

[3] Notice that these policies might also have increased publicity for the UI program in general, or might have otherwise increased utilization among individuals who were eligible for UI even before modernization. I include these individuals in my estimation of the effects of modernization, as an ancillary (but possibly significant) effect of each modernization.
[4] These policies widen initial eligibility, the determination whether someone can begin collecting UI, and not continuing eligibility, the determination whether someone can continue collecting UI. For an overview of the eligibility and benefit-level effects of a wide variety of continuing eligibility policies, see Grubb (2000).

largely focused on the ABP.[5] Vroman (1995) uses administrative data from the six states that had implemented the ABP at that time to show that between 6% and 8% of applicants collected UI under the ABP. Stettner, Boushey, and Wenger (2005) use SIPP (Survey of Income and Program Participation) survey data of the unemployed to predict that a nationwide ABP would increase eligibility by 7.2%. My finding of a 14% increase caused by the ABP is higher than these authors' estimates; the fact that I correct for sample selection bias, along with the evolving labor market and the labor activity leading up to and occurring during the Great Recession likely account for this difference.

O'Leary (2011) presents a case study of all of the modernizations. O'Leary uses administrative data to measure the cost of each of the ARRA modernizations to the state of Kentucky by examining rejected UI applicants, calculating what fraction would have been accepted had each modernization been in place. Using this methodology, O'Leary calculates that eligibility increases by 2.82% from ABP, 0.6% from PTW, and 0.6% from CFR. However, O'Leary's values are lower bounds on the effects of each of these laws, because he does not account for any increase in UI claims from newly eligible workers after the modernizations' implementation.

Lindner and Nichols (2012) use SIPP data from 1996-2008 to estimate the effect of each modernization on national UI eligibility. They find that ABP increases eligibility by 3.9% and CFP by 6.0%, but that PTW increases eligibility by 23.9%. The authors assume that all job-losers who lost part-time jobs can only collect UI under PTW; however, part-time workers can collect UI in most states (so long as they seek full-time work). The authors thus attribute many low-wage workers who would be eligible under

[5] For a summary of early research on non-monetary eligibility, see Nicholson (1997); however, the author shows that very little analysis on any UI initial eligibility policies had been completed.

ABP to the expected effect of PTW, leading to an over-estimation of the effect of PTW at the expense of ABP. If this were corrected, our findings would likely be very similar.

Scholarship on non-monetary UI eligibility, then, largely uses individual-level data. Administrative data is copious, but only includes UI applicants (and thus cannot predict increases in UI eligibility caused by new applications). Survey data has smaller sample sizes (especially at the state level) and may have misreporting and participation biases, but is representative of all of the unemployed. Neither indicates whether an individual could only collect UI due to the implementation of one of the modernizations. I take a macro approach to the evaluation of non-monetary UI eligibility by investigating it at the state level. Because so many states implemented the modernizations between 2009 and 2011, there is sufficient variation at the state level to estimate the real effect of policies on eligibility and benefit levels instead of their predicted or expected effects.

In addition to the particular difficulties of each of these studies, they estimate vastly different effects of the modernizations in question, and they do not measure the total benefits provided to UI recipients under each policy. My state-level panel approach is thus an important addition to the literature.

More broadly, this paper contributes to an already large literature studying the effects of UI. Most of this literature has focused on other aspects of UI policy, such as benefit levels and the expiration and extension of benefits (see Moffitt (1985) and Meyer (1990) for seminal studies, Krueger and Meyer (2002) for a survey, and Rothstein (2011), Valetta and Kuang (2010), and Fujita (2010) for studies on the Great Recession). As shown above, there is comparatively little research on the third key aspect of UI—eligibility. This paper fills that gap.

3. DATA

I combine several distinct datasets to form the two datasets used in my analysis. My first dataset, a long panel including monthly data for all 50 states from January 2005 to December 2011, is primarily built from state-level UI Administrative Data (UIAD), which measures the number of individuals who begin collecting UI in each month and the average benefits of those collectors. [1] I construct dummies for each of the UI modernizations in question using state legal codes provided by the Department of Labor. A number of national surveys conducted by the Departments of Labor and Interior supplement this data (for use as control variables): the Quarterly Workforce Indicators (QWI), the Quarterly Census of Employment and Wages (QCEW), the US Census, the Current Employment Statistics Program (CES), and the Local Area Unemployment Statistics (LAUS).[2] I discuss all data cleaning strategies in Appendix 1.

Although 4,200 observations (50 states by 7 years by 12 months) are expected, I leave 19 states out of my analysis, largely because I cannot evaluate my selection model (which I present in Section 5.2 below) for states that implemented the ABP before 2005.[3] I discuss the sample selection bias caused by these omissions in Section 5.2. Moreover, BLS does not report one of my control variables, the percent of workers employed in the Manufacturing super-sector, in Delaware from January 2005 to February 2006 (for an

[1] Burtless and Saks (1984) use similar panel data to estimate various effects on UI utilization (though they use a stock measure of utilization as their dependent variable); however, their theoretical framework is far simpler than my own.
[2] Modernization implementation data is available at http://www.doleta.gov/recovery/#PressReleases; UIAD data at http://workforcesecurity.doleta.gov/unemploy/finance.asp; QWI at http://lehd.did.census.gov/applications/qwi_online/; QCEW at http://www.bls.gov/bdm/, Census at http://www.census.gov/popest/data/historical/2000s/index.html; CES at http://bls.gov/sae/; and LAUS at http://www.bls.gov/lau/.
[3] The excluded states are Connecticut, Georgia, Hawaii, Maine, Massachusetts, Michigan, Nebraska, Nevada, New Hampshire, New Jersey, New Mexico, New York, North Carolina, Ohio, Rhode Island, Vermont, Virginia, Washington, and Wisconsin. Nebraska is excluded because it has a unicameral legislature, which also prohibits evaluation of the selection model.

Table 3.1: Summary Statistics

	2005	2006	2007	2008	2009	2010	2011
% States with ABP	0	0	0	3.23	9.68	45.2	58.1
% States with PTW	3.23	3.23	3.23	3.23	9.68	32.3	45.2
% States with CFP	0	0	0	0	6.45	25.8	32.3
Mean Utilization	5.94	5.84	6.40	7.70	14.6	10.2	10.0
Mean Ben. Generosity	35.9	35.0	35.0	35.6	38.6	35.6	34.0
Mean Min UI Ben.	6.27	6.19	6.05	5.95	6.13	6.08	5.95
Mean Max UI Ben.	50.5	49.7	49.1	49.7	51.5	51.3	49.5
Mean Unemploy. Rate	4.89	4.44	4.22	5.00	8.47	8.53	8.12

Note: Data across the 31 included states, as of June of the designated year.

unknown reason). I assume that this omission is uncorrelated with my independent and dependent variables. Thus, my dataset includes 2,592 data points.

My second dataset, a cross-section of the 50 states that I use to model sample selection between states that modernize and states that do not modernize, uses published state government figures collated by Klarner (2003) and the Center on Budget and Policy Priorities.[4]

Table 3.1 shows summary statistics for the variables of interest in these datasets. I discuss this data, including individual sources, in the identification sections below.

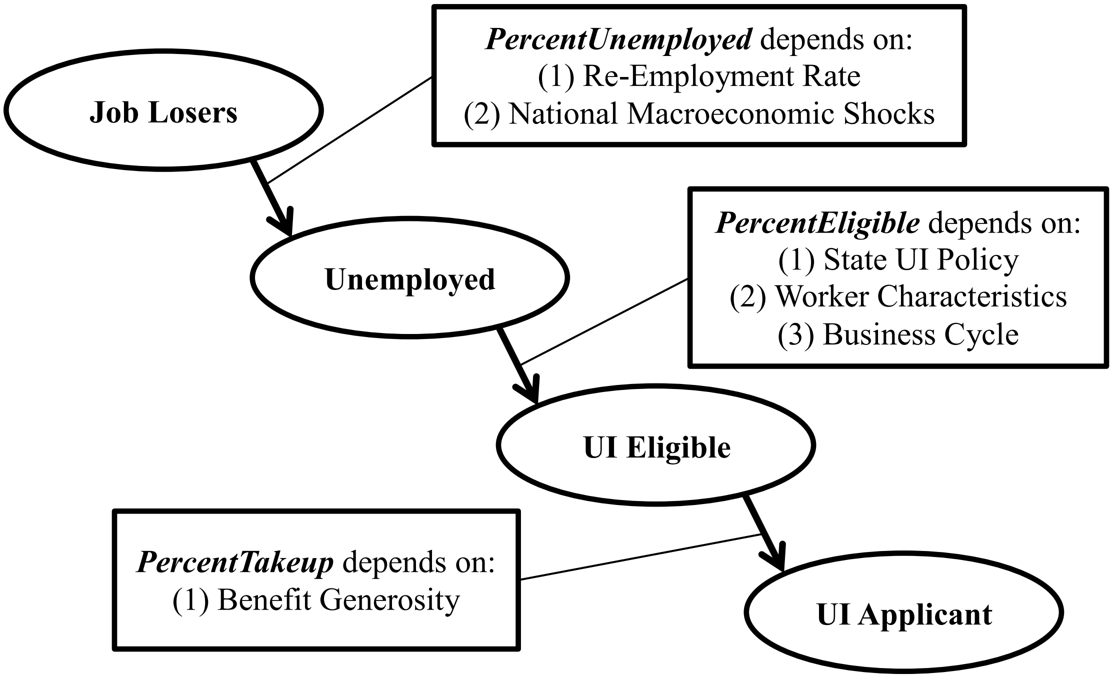
4. Theoretical Framework

4.1 UI Utilization

The primary statistic estimated in this paper is the effect of implementing each of the three eligibility-related modernizations on the number of people who commence collecting UI in a given month.

[4] I discuss the form and identification of the sample selection equation below. Klarner (2003) data is available at http://www.indstate.edu/polisci/klarnerpolitics.htm; CBPP data is available at http://www.cbpp.org/files/2-8-08sfp.pdf.

Figure 4.1: Flow of UI Utilization



Each month, some people separate from their jobs. Of these job-losers, some find immediate reemployment. Of those who remain unemployed, some are eligible for UI. Of those who are eligible, some actually apply for UI, and ultimately begin to collect UI. See Figure 4.1 for a depiction of this progression. Mathematically:

$$\frac{People\ Begin\ UI}{Job\ Losers} = \frac{Unemployed}{Job\ Losers} * \frac{People\ Eligible\ for\ UI}{Unemployed} * \frac{People\ Begin\ UI}{People\ Eligible\ for\ UI} \tag{1}$$

The dependent variable, $\frac{People\ Begin\ UI}{Job\ Losers}$, is UI utilization.[1] I name the other variables, in order, *PercentUnemployed*, *PercentEligible*, and *PercentTakeup*. Taking the natural log of both sides yields a linear equation:

[1] Note that I use a flow measure of UI utilization (counting the number of people *entering unemployment each month* who collect UI) as opposed to a stock measure of UI utilization like the more-common AIUR/TUR ratio (counting the percent of the unemployed who are collecting UI; see Vroman (1991)). A flow measurement allows me to more precisely measure individual eligibility, since I avoid any unwanted discrepancies caused by changing benefit duration or the exhaustion rate (implying insensitivity to the implementation of extended benefits). See Baker, Corak, and Heisz (1996) for an excellent discussion of the advantages of using a flow measure of UI utilization in empirical work.

$$\ln(UI\ utilization) = \ln(PercentUnemployed) + \ln(PercentEligible) + \ln(PercentTakeup) \quad (2)$$

Implementing one of the UI modernizations strictly broadens the set of people who are eligible for UI. Moreover, it might be the case that individuals who are eligible for UI under a modernization (largely low-income job-losers) have higher takeup rates (out of greater financial need), further increasing UI utilization.[2] Thus, there is good reason to expect that modernization implementation increases UI utilization.

4.2 Generosity Model

The secondary statistic estimated in this paper is the effect of implementing each of the three modernizations on the average benefit collected by UI recipients in general. I estimate this statistic in order to calculate the total benefits received by individuals collecting under each of the modernizations; I explain this calculation below.

Each month, some people in a state lose their jobs and begin collecting UI. The average former weekly wage of those people is some proportion of the average weekly wage in that state. The state offers average weekly UI benefits to the job-losers equivalent to some percent of their average former weekly wages. Mathematically:

$$\frac{Average\ Weekly\ UI\ Benefits}{Average\ Weekly\ Wage} = \frac{Average\ UI\ Recipient\ Wage}{Average\ Weekly\ Wage} * \frac{Average\ Weekly\ UI\ Benefits}{Average\ UI\ Recipient\ Wage} \quad (3)$$

The dependent variable, $\frac{Average\ Weekly\ UI\ Benefits}{Average\ Wage}$, is UI benefit generosity. The two independent variables are *WageRate* and *ReplacementRate*, respectively.[3] As in the case of UI Utilization, taking the natural log of both sides yields a linear equation:

$$\ln(UI\ benefit\ generosity) = \ln(WageRate) + \ln(ReplacementRate) \quad (4)$$

[2] I do not distinguish between these effects; indeed, the modernizations might increase UI utilization in other ways as well. Since I have no individual-level data, I calculate only the cumulative effect of the modernizations.

[3] Note that I use Replacement Rate to refer to the actual percentage of wages replaced by UI benefits, which differs from the standard usage replacement rate as the statutory average of that value.

I expect implementing one of the modernizations to decrease UI benefit generosity. All three modernizations increase UI recipience among low-earning workers. After all, the kind of worker who can begin collecting UI having earned a lower-than-usually-accepted salary in their most recent job, or who seeks only part-time work, or who is forced to leave their job because of domestic violence or household illness, is likely going to be eligible for lower weekly UI benefits than the average UI recipient. This implies that implementing one of the modernizations decreases the ratio of the average weekly benefits earned by all UI recipients to the wages earned by all workers in general.

5. EMPIRICAL METHODOLOGY

5.1 IDENTIFICATION

I use a fixed effects model to calculate the effect of each of the three UI modernizations on both UI utilization and UI benefit generosity, the two dependent variables of interest. Each modernization has a fixed effect for whether it is in implementation in that state-month. [1] This difference-in-differences analysis of the modernizations compares the effect of the modernizations on implementing states (before vs. after, the first difference) with the states that did not implement the modernizations (implementation state vs. non-implementation state, the second difference).

My benchmark equation is:

$$\ln(Y_{wit}) = \beta_{w0} + M_{it}'\beta_{w1} + \ln(X_{wit})'\beta_{w2} + \gamma_i'\beta_{w3} + \varphi_i'\beta_{w4} + \nu_{wit} \tag{5}$$

Y_{wit}, the dependent variable, is either UI utilization or UI benefit generosity for state i in time t. M_{it} is a vector of the three modernization fixed effects, and X_{it} is a vector

[1] The policy fixed effects equal 1 when their policy *is implemented or continues to be implemented* in the given month; it is not related to the month of original passage of the policies. For example, if a state's ABP goes into effect in March 2010, then the ABP dummy is zero in every month before March 2010, 1 in March 2010, and 1 in every month after 2010.

of variables proxying for *PercentUnemployed*, *PercentEligible*, and *PercentTakeup* in the first case and *WageRate* and *ReplacementRate* in the second. γ_i' are state fixed effects and φ_i' are time fixed effects (for each period). Since the control variables are proxied, I include a measurement error term ν_{it}.

5.1.1 UI UTILIZATION

UI utilization is the ratio of the number of people who receive first UI payments to the number of job-losers, both of which I observe. [2] I do not observe *PercentUnemployed*, *PercentEligible*, or *PercentTakeup*, each of which I proxy using the dependences listed in Figure 4.1. Consider each of these in turn.

PercentUnemployed is the percent of job-losers who actually enter unemployment (as opposed to immediately beginning a new job or leaving the labor market). *PercentUnemployed* has both state-level and national-level components: better state hiring conditions might lead to higher immediate reemployment, and better national macroeconomic conditions (like changes to the tax code) might lead to people moving to other states in order to find employment or better entrepreneurial opportunities. Thus, I proxy *PercentUnemployed* with both state-level hiring rates and national time dummies. I calculate the hiring rate by finding the ratio between the total hires in a state-month[3] and the population of the state in that month.[4] National time dummies capture the effect of any national macroeconomic shocks, like the Great Recession.

PercentEligible, the percent of unemployed people who are eligible for UI, varies in at least three dimensions. First, specific states' UI eligibility policies differ in myriad

[2] First payments data is from UAID; separations data is from QWI. I lag forward first payments by one month in order to account for the timing between job loss and UI first payment; see Appendix 1.
[3] Data from the Bureau of Labor Statistics' Quarterly Census of Employment and Wages. Hiring data is quarterly, which leads to some degree of measurement error. However, there is no reason to expect that the failure to include monthly wage information biases the regression, and instead only results in attenuation error in the β coefficient on *PercentUnemployment* (which is acceptable given that it is not the variable of interest).
[4] Hiring data is from QCEW; population data is from the Census.

ways, from differing monetary eligibility and hourly work requirements to special treatment for members of the armed forces or people with temporary disabilities.[5] These policies lead to great variation in which workers are eligible for UI. Second, states have workers with different demographic distributions, which correspond with differing distributions of job-loser demographics.[6] For example, some states have relatively more workers in the financial industry, which may imply that many workers in those states will have been long-employed and well paid before losing their jobs. Even if two states had identical UI eligibility policies, it may appear that one has more flexible eligibility policies than another merely because the former state has job-losers with different demographics than those of the latter state. Third, fluctuations in the business cycle might affect the kind of worker entering unemployment; during recessions, for instance, firms might have to lay off longer-term employees who are more likely to by insured by UI.[7]

I proxy for each of these dimensions. First, I include state dummy variables to control for differences in eligibility policy, assuming that the modernizations were the only substantial changes to UI eligibility during the Recession.[8] They also account for differences in administrative effectiveness and eligibility determination.[9] Second, I include two sets of demographic variables: industry control variables measuring the percent of individuals who work in each of the 11 CES super sectors and in the government, and age control variables measuring the percent of individuals *collecting UI*

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[5] See DOL ETA's Comparison of State Unemployment Laws (2012), under both Monetary Entitlement and Nonmonetary Eligibility, for an enumeration of the variety of differences among state eligibility laws.
[6] For example, McMurrer and Chasanov (1995) show a positive association between both larger unionized industries and a larger manufacturing industry and higher UI utilization.
[7] Gordon (2009) argues for the counter-cyclicality of UI, both in first claims and first payments.
[8] See Lancaster (2005-2011) for yearly evidence that the only significant changes to nonmonetary eligibility policy during the time covered in this dataset were the modernizations.
[9] See Corson, Hershey, and Kerachsky (1986) for a discussion of eligibility determination. They find, unsurprisingly, that well-defined administrative policy at the state level causes higher levels of correct eligibility determination.

who are within each of seven age brackets.[10] By including a set of age control variables instead of only an average, I allow for a piecewise-linear relationship between age and UI eligibility instead of a merely linear relationship. Third, I include the Total Unemployment Rate (TUR) to allow for cyclicality in eligibility.[11]

Finally, I proxy *PercentTakeup* with two UI statutory generosity variables. The implicit assumption is that the significant determinant of applying to UI is how valuable that insurance is; the more money available from UI, the more likely an eligible individual is to apply for UI.[12] In particular, I include the minimum and maximum weekly benefits available through UI.[13] Since wage levels differ across states, I normalize these UI policy generosity variables by dividing them by the average (median) weekly wage in the state, so that higher UI policy generosity implies not a higher cost of living, but the greater value of the UI benefits.[14]

Most control variables are included in logarithmic form. Of course, I do not take logarithms of state and time dummies. I also do not take the log of the age distribution variables, since they are percentages constructed to sum to one in order to determine a piecewise-linear relationship, and they would lose this distinctive quality in log form.

5.1.2 UI Benefit Generosity

UI benefit generosity is ratio of the average weekly UI benefit to the average wage, both of which I observe. I use this ratio to account for differences in salaries and

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[10] Industry data is from CES, which combines hundreds of jobs types into 11 super sectors: National Resources and Mining; Construction; Manufacturing; Trade, Transportation, and Utilities; Information; Financial Activities; Professional and Business Services; Education and Health Services; Leisure and Hospitality; Other Services; and Public Administration. For my industry control variables, I take the ratio of the number of individuals working in the private sector in each super sector to the total number of individuals working in the private sector. Age data is from UIAD. I modify the data by dividing by the percent of people who report their ages (almost exclusively over 90%), to correct for any bias in non-reporting (assuming the same distribution of reported and non-reported ages). The age brackets used are <22, 22-24, 25-34, 35-44, 45-54, 55-59, 60-64, and >65 years old.

[11] TUR data is from LAUS.

[12] For evidence of this strong positive relationship, see Anderson and Meyer (1997), who find an elasticity between the takeup rate and UI benefits of between 0.39 and 0.59.

[13] Data from Loryn Lancaster's yearly reports on the subject; see Lancaster (2005-2011)

[14] Data is from QCEW.

average costs of living between states. I do not observe *ReplacementRate* or *WageRate*, each of which I proxy. Consider each of these in turn.

WageRate is the ratio of the wage of the average new UI recipient to the average wage in the state. I proxy the Wage Rate using demographic composition and time dummies. I include the industry and age distribution variables (along with the percent of workers employed by the government) to account for demographic differences across states, since those differences likely lead to different distributions of UI recipients, which manifests itself in higher or lower benefits-to-wages ratios. I include time dummies for each state-month in order to capture two effects. First, there are seasonal effects of low- or high-wage workers regularly collecting UI with more frequency during certain months, across states (for instance, many symphony employees work nine months each year and collect low UI during the off-quarter). Second, Benefit Generosity is sensitive both to changes in average benefit levels and to average wage levels, and the latter might be sensitive to national macroeconomic shocks that discourage regular wage increases.[15]

ReplacementRate is the ratio of average UI benefits to the wage of an average UI recipient. I proxy *ReplacementRate* with state-level policy variables that determine the monetary generosity of each state's UI system. I use state dummies to distinguish states' UI eligibility policies, and include the same measures of UI statutory generosity as above (minimum and maximum available weekly benefits) to control for changes in benefit levels. In addition, although only the three modernizations studied in this paper directly affect UI Utilization, a fourth modernization (which increases benefit levels for job-losers

[15] A relative decrease in the average wage would likely manifest itself in average benefit levels, since benefits are a function of the wages of job-losers. However, there might be a delay in that relationship (because the workers whose wages fail to increase are not necessarily the same workers who lose their jobs in that month), and so long as that delay is similar across states (and there is no reason to think otherwise), time dummies capture its effects.

with children or other dependents) might increase the Replacement Rate in states that implement it, so I include a dummy for the Dependent Benefits modernization.

I include only the logarithm of any variable that is not either a dummy or one of the members of the age distribution.

5.2 Econometric Methodology

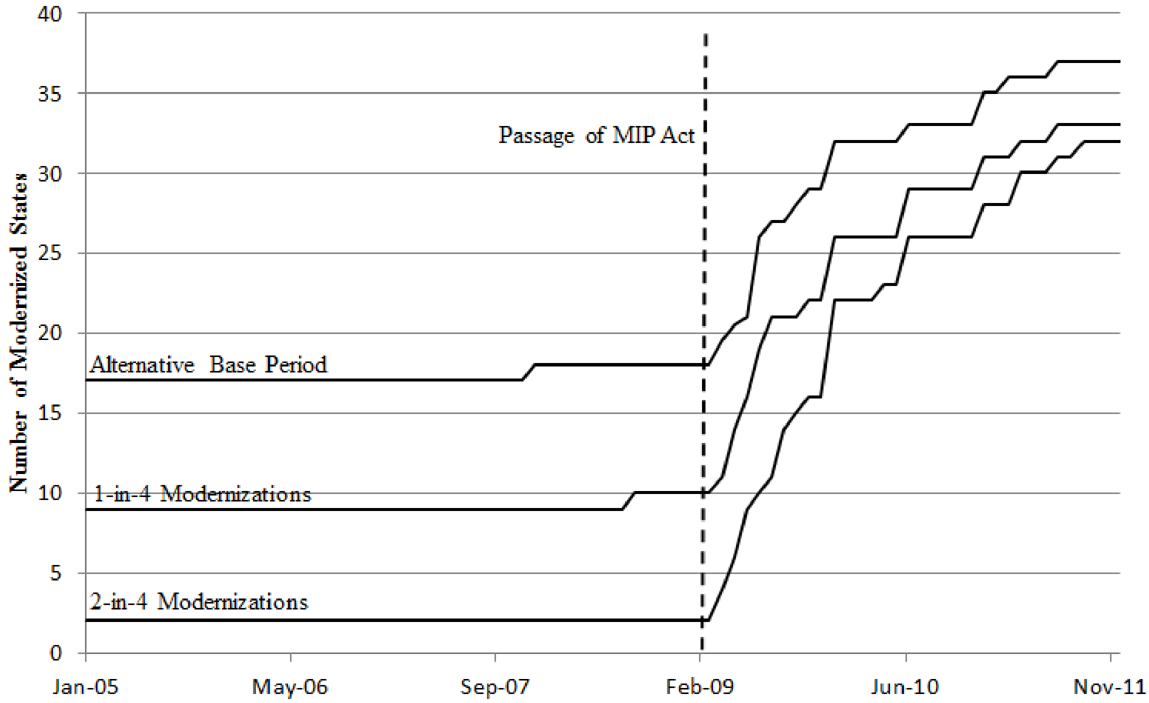
I state my benchmark equations above as Equations 5. However, I cannot estimate this equation directly because sample selection bias, caused by a non-random selection of states that implement the modernizations, violates the Gauss-Markov linearity condition. This section discusses my solution to sample selection bias in the stated substantial equations, which uses a control function framework with the propensity score approximated by the hazard rates of a duration model. Following sample selection bias, I also discuss the violation of spherical errors.

5.2.1 Sample Selection Bias

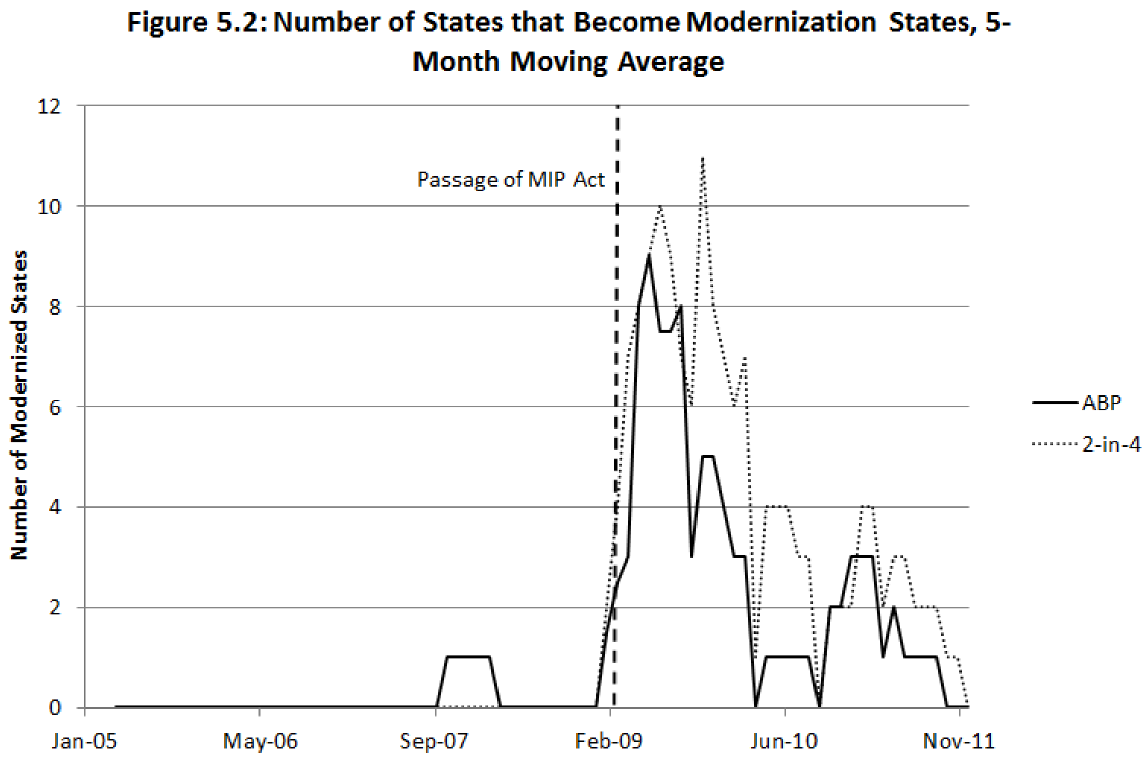
If a non-random selection of states implemented modernizations (e.g. if the states' selection mechanisms correlate with the effects of the modernizations), then β_{w1} would estimate the combination of two different effects: the effect of the implementation of the modernization, and the effect of *being the kind of state that* implements that modernization, the two of which might be correlated. Although part of this latter effect is absorbed by the control variables described above, those control variables cannot account for correlation between the implementation of modernizations and UI utilization. This study is interested in the actual effect of the modernizations' implementation, but sample selection bias confounds those results through omitted variable bias.

Analysis of sample selection bias usually makes use of a linear selection equation that would model which states are included in the sample using state-level characteristics.

Figure 5.1: Total Number of Modernized States



However, the ARRA asserts that the modernizations cannot include sunset provisions; the laws are without expiration dates. Of course, state legislatures could repeal the laws at any time, but as of the end of 2012, no state had repealed its modernizations (see Figure 5.1). In other words, any selection model that does not account the time dynamics of modernization would be ineffective, because once a state has implemented a modernization, it always has that modernization, even if its characteristics change. I use a duration, or survival, model to identify modernization implementation. Frequently used in biological drug tests and similar experimental settings, duration models predict the amount of time it takes a state to die, or, in this case, to modernize. Duration models use permanent state qualities and a specified distributional form to model how states become



modernization states over time.[16] Thus, this model form prohibits states to move from being modernized to being not modernized.

I use a parametric duration model, which specifies the time distribution down to parameters. In particular, I use a lognormal distribution of survival time, thus assuming that (1) it is appropriate to use a continuous time framework to model the likelihood of implementing modernizations over time, and (2) the likelihood of implementing modernizations follows a (continuous) lognormal distribution.[17]

The random variable of interest (the number of months it takes for a state to modernize) is, strictly speaking, discrete. However, the underlying random variable is continuous (a law can be passed any working hour), so because I include a large number

[16] A state is a modernization state if it implements the ABP and two of the four other modernizations.

[17] I measure the Akaike Information Criterion (See Akaike (1973)) for the selection equations using exponential, Weibull, loglogistic, and lognormal distributions without covariates, which determines the best-fit parametric form for the duration model in question. The lognormal distribution has the smallest AIC in both cases, implying that it is the superior fit of those four possibilities.

of periods (84 for each state), I approximate that discrete distribution with the continuous lognormal distribution. The greatest concern with the lognormal form is that the passage of ARRA appears to be a discontinuity in the time distribution; after all, many states implement modernizations just after the passage of ARRA. However, the lognormal form allows for this jump with its asymmetrical form, swiftly reaching a peak but flexible about how quickly the instantaneous probability of modernization returns to zero. In addition, state legislatures have been long aware of the ARRA modernizations, because an identical Unemployment Insurance Modernization Act had been introduced in both the House and the Senate more than two years earlier.[18] The assumption of rational expectations of state legislatures implies a continuous increase rather than a discontinuous spike in the likelihood of modernization implementation.[19] Flexible in multiple mean parameters as well as the curve's standard error, the lognormal distribution is a good parametric fit for a duration model of state modernization implementation. Figure 5.2 shows the distribution of modernization implementation across states, strongly suggesting a lognormal distribution.

Table 5.1 shows if and when states became ABP states and 2-in-4-modernization states. I use two kinds of covariates to model the transition probability of state *i* to modernize. First, I include variables measuring the median Democratic control of the state House of Representatives, the Senate, and the Governorship over every month from

[18] See S. 1871 and H.R. 3920, Section 402. The bill was passed in the House of Representatives, but died in Senate committee before it was placed into the ARRA.

[19] Strictly speaking, on this interpretation one would expect a discontinuous spike in the probability of implementing modernizations when the law was originally announced, two years before the passage of the ARRA. However, at that time the probability of the modernization incentivization funding actually being implemented by Congress was very small, and compounded with future-discounting would result in a very small discontinuity that I assume to be insignificant. Therefore, prior expectations of the passage of the incentivization contained in the ARRA are adequate to eliminate any significant discontinuities in the modernizaiton implementation distribution.

Table 5.1: Timing of Modernization Implementation

Alternative Base Period

Pre-2009		2009		2010	2011	Never	
Connecticut	New Mexico	Arkansas	Nevada	Alaska	California	Alabama	Mississippi
Georgia	New York	Colorado	Oklahoma	Delaware	Maryland	Arizona	Missouri
Hawaii	North Carolina	Idaho	Oregon	Kansas	Nebraska	Florida	North Dakota
Illinois	Ohio	Iowa	South Dakota	Tennessee	South Carolina	Indiana	Pennsylvania
Maine	Rhode Island	Minnesota	West Virginia		Utah	Kentucky	Texas
Massachusetts	Vermont	Montana				Louisiana	Wyoming
Michigan	Virginia						
New Hampshire	Washington						
New Jersey	Wisconsin						

2-in-4 Modernizations

Pre-2009	2009		2010		2011	Never	
Nevada	Arkansas	Montana	Alaska	Illinois	California	Alabama	North Dakota
New Mexico	Colorado	New Hampshire	Delaware	Kansas	Maryland	Arizona	Ohio
	Connecticut	New Jersey	Georgia	North Carolina	Nebraska	Florida	Pennsylvania
	Iowa	New York	Hawaii	South Dakota	Rhode Island	Indiana	Texas
	Maine	Oklahoma	Idaho	Tennessee	South Carolina	Kentucky	Utah
	Massachusetts	Oregon			Vermont	Louisiana	Virginia
	Minnesota	Wisconsin				Michigan	Washington
						Mississippi	West Virginia
						Missouri	Wyoming

2005-2011.[20] I include the medians of these variables in order to identify the general political sentiments in each state; I do not want this average to be sensitive to outlier political behavior like the influx of Tea Party presence in state legislatures in 2010. In general, states that are more Democratic are more likely to implement the modernizations, especially given the politically charged nature of responses to the ARRA in general.[21] Second, I include the mean yearly percent budget deficit (as a percentage of the state's total budget) from the years 2009, 2010, and 2011 in order to measure states' need for short-term funding during the Great Recession.[22] High deficits heighten the incentive for states to implement the modernizations (since they received short-term

[20] Data from Klarner (2003).

[21] See, for instance, Mitchell (2010), who notes that "Republican governors or lawmakers in eleven states have declined to reform their UI system and twelve other states have either made only some of the changes, have not applied for the funds, or have not taken legislative votes on the reforms" (68).

[22] Data from the Center on Budget and Policy Priorities. I do not use budget deficit data from before 2009 for two reasons: most states did not have deficits from 2005-2008 (with deficits only appearing with the Great Recession), and data for budget deficits before 2009 is unavailable.

funding in return for long-term liability). According to my model, then, these four variables identify the state decision to become ABP and 2-in-4-modernization states (states that implement two of the four additional modernizations)

I model the expected time in which state *i* becomes an ABP state (A_i) and becomes a 2-in-4-modernization state (T_i) using two distinct duration models, each of which separates states into two groups: modernized and unmodernized. Mathematically:

$$\ln(A_i) = Z_i'\omega_1 + \mu_{1i}, \qquad E[\mu_{1i}|Z_i'] = 0, \qquad z_{1it}^* = \ln(A_i) - \ln(t), \qquad z_{1it} = \begin{cases} 0, & z_{1it}^* > 0 \\ 1, & z_{1it}^* \leq 0 \end{cases} \tag{6}$$

$$\ln(T_i) = Z_i'\omega_2 + \mu_{2i}, \qquad E[\mu_{2i}|Z_i'] = 0, \qquad z_{2it}^* = \ln(T_i) - \ln(t), \qquad z_{2it} = \begin{cases} 0, & z_{2it}^* > 0 \\ 1, & z_{2it}^* \leq 0 \end{cases} \tag{7}$$

where Z_i' is a vector of the selection covariates described above, ω_τ are the estimated coefficients, and $\mu_{\tau i}$ is an error term caused by measurement error of Z_i'. Modernization states (at time t) are states for which $z_{it} = 1$; non-modernization states (at time t) are states for which $z_{it} = 0$.

I estimate these first-step equations using a duration maximum likelihood framework. States may be of two types: either they become modernization states during the time of my dataset (2005-2011), or they never become modernization states (in my timeframe). In the case of states that never become modernization states, I maximize their survival rate $S_\tau(t_i|Z_i')$, or the chance that the state has not modernized by *t*:

$$S_\tau(t|Z_i') = 1 - \Phi\left(\frac{\ln(t) - Z_i'\omega_\tau}{\sigma_\tau}\right) \tag{8}$$

In the case of states that modernize, on the other hand, I maximize their hazard rate $\lambda(t)$, or the chance that the state modernizes in time t_i conditional on their not having yet modernized:

$$\lambda_\tau(t|Z_i) = \frac{\frac{1}{t\sigma_\tau}\phi\left(\frac{\ln(t) - Z_i'\omega_\tau}{\sigma_\tau}\right)}{1 - \Phi\left(\frac{\ln(t) - Z_i'\omega_\tau}{\sigma_\tau}\right)} \tag{9}$$

The hazard rate is equal to the negative derivative of the survival rate (the instantaneous probability of death in time t_i) divided by the survival rate of time t_i, which conditions on the state not having yet modernized. This is a proportional hazard model, since its covariates are time-invariant. The likelihood function that I maximize in order to estimate the parameters β and σ is:[23]

$$L_\tau(\omega_\tau, \sigma_\tau) = \prod_{i=1}^{S} \lambda_\tau(T_i|Z_i')^{d_i} * S_\tau(84|Z_i')^{1-d_i} \tag{10}$$

The dummy variable d_i = 1 if the state modernizes at any time between 2005 and 2011.

This model, then, identifies which states implement modernizations and which states do not, and predicts when they are most likely to implement modernizations if they do. The question remains, however, how to incorporate this information about sample selection in the substantive equation.

Motivated by Heckman (1979) and Heckman and Navarro-Lozano (2004), I use a modified control function approach to correct sample selection bias in my substantial equation. I describe my full procedure in Appendix 2. My procedure is completed in two steps. The first-step regression predicts a state's modernization hazard rate, the probability that the state modernizes in that time conditional on its either having or not having modernized. The second-step regression includes a polynomial expansion of these hazard rates as control variables in the substantive equation to consistently estimate the coefficients in that equation. By adding a polynomial expansion of this additional term,

[23] See Wooldridge (2010), pp. 993-994, for a derivation of this likelihood function. I state this equation for 2-in-4-modernization states; the respective likelihood equation for ABP states replaces T_i with A_i.

the substantive equation separately calculates the effects of the modernizations and the effect of being the kind of state that implements modernizations, and therefore consistently estimates the effect of the modernizations alone. [24] As I describe in Appendix 2, because I assume that the error terms of the selection models are uncorrelated, I do not include cross-polynomials between the two hazard rates in my regressions, though I do present those results in the Robustness section below.

The intuition for my econometric strategy is as follows. I include a polynomial expansion of the likelihood that a state becomes a modernization state in my substantial equation. This likelihood is very low in all states when *t* is very low, and increases over time, jumping up in most states around the time that the MIP Act was implemented. Notice that this coefficient is different from the inverse Mills ratio used in Heckman (1979) because it captures, and controls for, the time dynamics of modernization implementation. This coefficient, with its polynomials, absorbs any variation in utilization caused by states being the kind of states that become modernization states, leaving variation caused by the modernizations themselves to the fixed effects.

I thus use the following hazard function polynomials:

$$H'_{0\tau}(i,t) = \left[\left(\frac{\frac{1}{t\sigma_\tau}\phi\left(\frac{\ln(t)-Z'_i\omega_\tau}{\sigma_\tau}\right)}{1-\Phi\left(\frac{\ln(t)-Z'_i\omega_\tau}{\sigma_\tau}\right)}\right), \left(\frac{\frac{1}{t\sigma_\tau}\phi\left(\frac{\ln(t)-Z'_i\omega_\tau}{\sigma_\tau}\right)}{1-\Phi\left(\frac{\ln(t)-Z'_i\omega_\tau}{\sigma_\tau}\right)}\right)^2, \left(\frac{\frac{1}{t\sigma_\tau}\phi\left(\frac{\ln(t)-Z'_i\omega_\tau}{\sigma_\tau}\right)}{1-\Phi\left(\frac{\ln(t)-Z'_i\omega_\tau}{\sigma_\tau}\right)}\right)^3\right] \quad (11)$$

$$H'_{1\tau}(i,t) = \left[\left(\frac{\frac{1}{t\sigma_\tau}\phi\left(\frac{\ln(t)-Z'_i\omega_\tau}{\sigma_\tau}\right)}{\Phi\left(\frac{\ln(t)-Z'_i\omega_\tau}{\sigma_\tau}\right)}\right), \left(\frac{\frac{1}{t\sigma_\tau}\phi\left(\frac{\ln(t)-Z'_i\omega_\tau}{\sigma_\tau}\right)}{\Phi\left(\frac{\ln(t)-Z'_i\omega_\tau}{\sigma_\tau}\right)}\right)^2, \left(\frac{\frac{1}{t\sigma_\tau}\phi\left(\frac{\ln(t)-Z'_i\omega_\tau}{\sigma_\tau}\right)}{\Phi\left(\frac{\ln(t)-Z'_i\omega_\tau}{\sigma_\tau}\right)}\right)^3\right] \quad (12)$$

[24] In the words of Berk, "By including the hazard rate as an additional variable, one is necessarily controlling for these nonzero expectations [caused by sample selection bias]. Alternatively stated, the deviations of the expected values from the regression line result from an omitted variable that has now been included," namely, a variable modeling sample selection. See Berk (1983), pp. 391.

The first of these polynomials of hazard functions is the sample selection correction for states that *have not* modernized, either by becoming an ABP state ($\tau = 1$) or becoming a 2-in-4-modernization state ($\tau = 2$). Following common practice, I include the third-order polynomial expansion; indeed, very few of the higher-order values are statistically significant.[25] The full substantial equation, then, with these adjusted Heckman terms, is:

$$\ln(Y_{wit}) = \begin{cases} \begin{cases} \beta_{w0} + M_{it}'\beta_{w1} + \ln(X_{wit})'\beta_{w2} + H_{11}'(i,t)\alpha_{w11} + H_{12}'(i,t)\alpha_{w12} + \varepsilon_{1wit}, & z_{1it} = 1 \\ \beta_{w0} + M_{it}'\beta_{w1} + \ln(X_{wit})'\beta_{w2} + H_{01}'(i,t)\gamma_{w01} + H_{12}'(i,t)\gamma_{w12} + \varepsilon_{2wit}, & z_{1it} = 0 \end{cases}, & z_{2it} = 1 \\ \begin{cases} \beta_{w0} + M_{it}'\beta_{w1} + \ln(X_{wit})'\beta_{w2} + H_{11}'(i,t)\gamma_{w11} + H_{02}'(i,t)\gamma_{w02} + \varepsilon_{3wit}, & z_{1it} = 1 \\ \beta_{w0} + M_{it}'\beta_{w1} + \ln(X_{wit})'\beta_{w2} + H_{01}'(i,t)\gamma_{w01} + H_{02}'(i,t)\gamma_{w02} + \varepsilon_{4wit}, & z_{1it} = 0 \end{cases}, & z_{2it} = 0 \end{cases} \quad (13)$$

As I described above, this selection model causes a sample selection bias of its own, since it requires me to leave 19 states out of my analysis (see Section 3). While these omissions do lead to a sample selection concern of their own, it is not as substantial as the bias discussed above, because the state dummies present in each model above proxy for a sample selection correction term (which, after all, exists only at the state level, not the state-time level). Thus, I cannot interpret the coefficients on the state fixed effects (since they combine multiple effects), and the only sample selection bias present is in the estimation of the selection equation itself, for which I use as many states as possible to estimate the hazard rates; I assume that that bias is small.

5.2.2 AUTOCORRELATION AND HETEROSKEDASTICITY

Equation 13 does not satisfy the Gauss-Markov assumption of spherical errors because of both autocorrelation and heteroskedasticity. First, autocorrelation in the form of serial correlation occurs because the model's error terms are correlated across time within the same state (e.g. shock in UI Utilization in that state can have prolonged consequences). Second, autocorrelation occurs in the error terms of states within the same

[25] See, for instance, Helpman, Melitz, and Rubenstein and Attanasio, Battistin, and Mesnard (2012), both of whom assume the sufficiency of cubic polynomial approximation for control functions.

time (e.g. some national shocks affect all (or most) states' UI Utilization similarly). Third, heteroskedasticity occurs in the error term; there is no reason to expect different times and states having the same variance levels in their error terms.

My dataset is a 'long' panel dataset in that it allows for a large-T asymptotic assumption (assume $T \to \infty$) in addition to large-dataset asymptotic assumption (assume $N \to \infty$). Thus, in order to account for all three variance concerns, I use the variance estimator developed by Driscoll and Kraay (1998).[26] Driscoll-Kraay error terms are asymptotically valid given a large-T asymptotic assumption, independent of the size of S (number of states).[27] Like the Newey-West estimator, the Driscoll-Kraay estimator requires the specification of an economically determined order of autocorrelation, which I set at three years (36 months), based on the assumption that the effects of shocks to UI Utilization might be sustained for up to that length of time. The estimator uses linearly decaying Bartlett weights such that the serial correlation between terms decreases as those terms grow further apart.[28] I compare these Driscoll-Kraay variances with other variance estimations in the Robustness section below.

5.3 POLICY EVALUATION

So far, I have discussed how to estimate the effect of each of the three modernizations on both UI Utilization and UI Benefit Generosity, where the first is

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[26] Robust standard errors (White (1980)) use a large-N assumption to consistently account for heteroskedasticity, but not autocorrelation. Both clustered standard errors and Newey-West (1987) standard errors use a large-N assumption to allow for serial correlation (complete in the former case and limited in the latter case), but neither allows for cross-sectional autocorrelation. Finally, panel-corrected standard errors (Beck and Katz (1995)) consistently account for heteroskedasticity and contemporaneous cross-sectional autocorrelation, but can only account for serial autocorrelation by assuming an AR(1) process in the error terms, which would invalidate the control function approach defined above. Moreover, panel-corrected standard errors require a small-S/T assumption, which is not the case in my dataset.

[27] Note also that Driscoll-Kraay standard errors, as a generalization of Newey-West standard errors, allay any concerns with serial correlation in the independent variables of interest, as shown through Monte Carlo simulation in Bertrand, Duflo, and Mullainathan (2004), pp. 271. The authors' two caveats are that this procedure is not effective if S, the number of states, is small (6-10 states) or the order of autocorrelation is small, but my cross-section is large and I specify a large order of autocorrelation.

[28] See Hoechle (2007), pp. 287-288, for the specific Driscoll-Kraay covariance matrix and estimation procedure.

$\frac{\#\ People\ Collecting\ UI}{\#\ Job\ Losers}$ and the second is $\frac{Average\ Weekly\ Benefits}{Average\ Weekly\ Wage}$. Since the models are both evaluated using the logarithm of these values, following standard practice (since both values are small) I interpret coefficients β_1 and α_1 as the percent change in UI Utilization and UI Benefit Generosity caused by the modernizations, respectively. I assume that a state's implementing a modernization has no effect on either the number of job losers in that state or the average wage in that state; my model includes no moral hazard on the part of employers, and there is no obvious reason that these laws affect anything except the kinds of people who collect UI. Therefore, I interpret β_1 as the percent change in the number of people who begin collecting UI benefits because of the modernizations, and α_1 as the percent change in the average level of benefits because of the modernizations (each the numerator of the respective variable).

Let M be one of the three modernizations, and let β_M and α_M be the respective fixed effect coefficients for M. Let t be an arbitrary time in state i such that i has implemented M on or before time t. Finally, let x_{it} be the total number of people who begin collecting UI, and let y_{it} be the average benefits of all UI recipients. I calculate the number of people who begin collecting benefits only because of the implementation of M (n_{Mit}), and the average weekly benefit collected by those people (b_{Mit}), using:[29]

$$n_{Mit} = \beta_M x_{it}, \qquad b_{Mit} = \left(\alpha_M \left(\frac{1}{\beta_M} - 1\right) + 1\right) y_{it} \tag{14}$$

Following these equations, the total benefits paid out to individuals who are only eligible for UI because the state has implemented M, in their first week of benefits (since eligibility measures UI first payments and the benefit level measures weekly benefits), is

[29] The first equation in Equation 14 is the definition of β_M discussed above. The second equation comes from the identity $\frac{((1-\beta_M)x_{it})((1-\alpha_M)y_{it})+(\beta_M x_{it})b_{it}}{x_{it}} = y_{it}$, which averages the benefit levels for each of the two groups (those who collect UI only because of M and those who can collect otherwise), and results in the current benefit level under M.

the product of these two values: $n_{Mit} * b_{Mit}$. Moreover, if we let d_{Mit} be the average duration for which those individuals collect UI, then $n_{Mit} * b_{Mit} * d_{Mit}$ is the total UI benefits committed in time t to be paid to individuals who can only collect benefits because M is implemented. In order to calculate the total benefits between January 2005 and December 2011 paid out to individuals, I add up that product for every state-time in which M was implemented. Let T_{iM} be the period in which state i implements M. Then:

$$Total\ UI\ Benefits\ (2005-2011)\ Paid\ under\ M = \sum_{i=1}^{50} \sum_{t=T_{iM}}^{84} n_{Mit} * b_{Mit} * d_{Mit} \qquad (15)$$

However, in order to evaluate the modernizations of the MIP Act, I am interested only in the benefits paid out in states that had not implemented M until after the passage of the ARRA (February 2009, t=50). This underestimates the effect of the MIP Act if states implemented the modernizations prior to February 2009 with the expectation of receiving incentivization funding after that date, and overestimates that effect if states would have implemented the modernizations after February even without being incentivized by the MIP Act. Nevertheless, it is the best available metric to measure the monetary effect of the MIP Act. Thus, the total amount of money paid to UI recipients between 2009 and 2011 resulting from one of the three eligibility-related modernizations, $\$_M$, is:

$$\$_M = \sum_{i=1}^{50} \sum_{t=T_{iM}}^{84} n_{Mit} * b_{Mit} * d_{Mit} * \delta_{iM}\,, \qquad \delta_{iM} = \begin{cases} 0, & T_{iM} < 50 \\ 1, & T_{iM} \geq 50 \end{cases} \qquad (16)$$

The variable $\$_M$ estimates the total funding that individuals collecting UI under newly implemented modernizations received through their state UI programs. Importantly, $\$_M$ estimates the actual benefits paid to UI recipients, disregarding ancillary costs to states (e.g. bureaucratic costs). Reported along with (A) the list of states that implemented modernizations and (B) the number of people who received UI benefits under the MIP Act, $\$_M$ is an important metric in evaluating the effectiveness of that Act.

I calculate a lower bound variance of $\$_M$. I do so by first repeatedly resampling my data and evaluating Equation 13 (for both utilization and benefit generosity) to bootstrap $V[\alpha_M + \beta_M - \alpha_M\beta_M]$. Since these coefficients are estimated separately, I cannot directly calculate this variance; however, bootstrapping provides an asymptotically valid variance estimation.[30] I then must assume that the covariances of $n_{Mit} * b_{Mit}$ across state and time are negligibly small, an assumption that, while necessary (as I cannot calculate those covariances), implies that there is no serial correlation or contemporary cross-sectional correlation in UI benefits (from M). Finally, since I do not estimate the average duration of UI collection by individuals collecting UI under M (which is a topic for future research), I assume that individuals collecting under M collect UI on average for the same duration as all UI recipients; because that data is available in UIAD, d_{Mit} is given (and thus has no variance). The somewhat tenuous nature of these assumptions implies that the reported error terms are lower bounds on the true error terms. Nevertheless, given these assumptions and well-known rules of variances:

$$V[\$_M] = \sum_{i=1}^{50} \sum_{t=T_{iM}}^{84} V[\alpha_M + \beta_M - \alpha_M\beta_M] * x_{it}^2 * y_{it}^2 * d_{Mit}^2 * \delta_{iM} \tag{17}$$

6. RESULTS

6.1 SELECTION EQUATION

Table 6.1 shows the regression results from the first-step selection equation. There are two modeled equations: Equation 6 (Column 1) and Equation 7 (Column 2). I

[30] For the efficiency, validity, and procedure of bootstrapping, see Wooldridge (2010), pp. 438-442. I draw 10,000 samples (with replacement) to calculate the variances.

Table 6.1: Selection Equation Results

	ABP	2-in-4
Selection Variables		
Med. Senate Dem %	-1.341**	-0.850*
	(0.674)	(0.479)
Med. House Dem %	1.077	0.407
	(0.754)	(0.523)
Med. Gov'nor Dem	-0.225*	-0.152*
	(0.121)	(0.082)
Mean Deficit %	-0.216	-0.414
	(0.540)	(0.456)
constant	4.568***	4.697***
	(0.228)	(0.168)
ln(Sigma) Value		
lnSigma	-1.223***	-1.372***
	(0.179)	(0.145)
Chi2	5236.793	10744.601
p-value	0.000	0.000
Observations	32	47

Note: Standard Errors are reported in parentheses.
Columns show the results for lognormal duration models.
Column 1 shows ABP; Column 2 shows 2-in-4-modernization.
Significance: *10% **5% ***1%

expect all four coefficients to be negative and for the constant term to be positive. For example, the coefficient on the median control of the Senate in Column 1 implies that a 10 percentage point increase in median Democratic control is associated with a 0.134 unit decrease in the mean of the ABP duration model. This is equivalent to the difference between states that implement the ABP at t = 70 and at t = 61 (3/4 of a year).[1]

The coefficient on the median Democratic control of the House is positive but insignificant; this probably arises because of similar political control of the two Congressional houses in most states, implying that, conditional on Democratic control of

[1] Thus, I expect the coefficients to be negative because they estimate the respective effect of the different selection variables on the mean of the duration model, such that a negative value implies that an increase in that variable is associated with a decrease in the expected amount of elapsed time before a state passes the respective modernizations.

the Senate, the effect of Democratic control of the House is statistically no different from zero. The coefficients on mean state budget deficits during the Great Recession are negative but insignificantly non-zero, which provides no evidence of a negative effect between budget deficits and duration until modernization. However, in both models Democratic control of the Senate and of the Governorship corresponds with significantly lower duration until modernization at the 10% level.

The natural log of the variances in both models ($\ln(\sigma_{\mu_1})$ and $\ln(\sigma_{\mu_2})$) are -1.223 and -1.372, with the 2-in-4-modernizations model having a slightly lower variance than the ABP model. A $\ln(\sigma)$ of -1.372 implies a variance in the duration model of 0.0643, which for a state expected to modernize at t=70 implies a 95% confidence interval of modernizing between t=42 and t=115.

The χ^2 test statistic evaluates the null hypothesis that all of the coefficients on the independent variables are equal to zero (i.e. there is no association between the independent variables and the mean of the duration model). The p-value in both regressions is 0.000, implying that the specified selection models are strongly predictive.

6.2 Substantive Equations

Table 6.2 shows the coefficient estimates for Equation 13, with columns 1-3 showing the estimates for UI utilization and 4-6 show the results for UI benefit generosity. Columns 1 and 4 show OLS results without any control variables; Columns 2 and 5 show results with controls but without correcting for sample selection; and columns 3 and 6 show results including the control function polynomials. Driskoll-Kraay error terms are displayed with all six models.

Table 6.2: Substantive Equation Results

	UI Uti'n OLS	OLS	SS OLS	UI Ben's OLS	OLS	SS OLS
Alt. Base Period	0.268**	0.016	0.140*	0.079***	0.000	-0.033
	(0.117)	(0.021)	(0.075)	(0.008)	(0.008)	(0.021)
Part-Time Benefits	0.056	0.016	0.100*	-0.003	0.011*	0.012***
	(0.095)	(0.026)	(0.059)	(0.027)	(0.006)	(0.005)
Comp. Family Reasons	0.146***	0.023*	0.054***	-0.088***	-0.011***	-0.010***
	(0.030)	(0.014)	(0.015)	(0.021)	(0.002)	(0.002)
Min Weekly Bens		-0.147***	-0.151***		0.017**	0.016**
		(0.043)	(0.042)		(0.008)	(0.008)
Max Weekly Bens		0.292	0.239		0.545***	0.535***
		(0.262)	(0.278)		(0.042)	(0.043)
% Financial Jobs		0.347	0.400		-0.155***	-0.176***
		(0.290)	(0.303)		(0.027)	(0.037)
% Resources Jobs		0.069	0.062		0.004	0.002
		(0.064)	(0.056)		(0.005)	(0.006)
% Construction Jobs		-0.069	-0.058		-0.030	-0.033
		(0.219)	(0.219)		(0.038)	(0.035)
% Manufacture Jobs		-0.003	0.008		-0.007	-0.012
		(0.056)	(0.059)		(0.015)	(0.014)
% Transport Jobs		2.241***	2.281***		0.466***	0.470***
		(0.588)	(0.651)		(0.131)	(0.125)
% Information Jobs		-0.136	-0.133		-0.003	-0.011
		(0.110)	(0.120)		(0.036)	(0.034)
% Business Jobs		-1.265***	-1.133***		0.043*	0.060**
		(0.235)	(0.221)		(0.026)	(0.024)
% Education Jobs		-0.961**	-0.953**		-0.151***	-0.170***
		(0.422)	(0.457)		(0.050)	(0.041)
% Leisure Jobs		-0.987***	-0.926***		-0.070*	-0.061*
		(0.177)	(0.206)		(0.037)	(0.032)
% Government Jobs		1.017***	0.952***		0.030	0.040
		(0.306)	(0.295)		(0.024)	(0.029)
% Age <22		-3.378***	-2.805***		-0.026	0.007
		(0.620)	(0.656)		(0.058)	(0.067)
% Age 22-24		0.555	0.579		0.632***	0.683***
		(0.510)	(0.582)		(0.180)	(0.188)
% Age 24-34		-3.554***	-3.320***		0.383**	0.383**
		(0.683)	(0.762)		(0.162)	(0.162)
% Age 34-44		-5.525***	-5.100***		-0.210***	-0.211***
		(1.824)	(1.818)		(0.072)	(0.064)
% Age 44-54		-5.234***	-5.006***		0.241**	0.285***
		(0.679)	(0.759)		(0.095)	(0.106)
% Age 55-59		-5.848***	-5.083***		-0.159	-0.126
		(0.703)	(0.645)		(0.187)	(0.192)
% Age 60-64		-5.021**	-4.480**		-0.343*	-0.331
		(2.001)	(2.019)		(0.186)	(0.206)
Unemployment Rate		0.228***	0.233***			
		(0.033)	(0.036)			
Hires Per Capita		-1.518***	-1.544***			
		(0.082)	(0.088)			
Dependent Benefits					-0.002	-0.006
					(0.010)	(0.011)
Time FEs	No	Yes	Yes	No	Yes	Yes
State FEs	No	Yes	Yes	No	Yes	Yes
Control Fns	No	No	Yes	No	No	Yes
R2	0.053	0.885	0.887	0.019	0.964	0.964
Observations	2592	2592	2592	2592	2592	2592

Driskoll-Kraay standard errors are reported in parentheses. Significance: *10% **5% ***1%

The shift from Column 2 to Column 3, which controls for sample selection bias using the modified control function approach discussed above, marks a substantial increase in the estimated effect of all three modernizations. Indeed, as I show in the Robustness section below, there is a strong negative correlation between being the kind of state that modernizes and UI utilization. Remember that my sample selection correction is dynamic; this result implies that states that are likely to modernize early according to my model, but fail to do so, have far lower UI utilization. This is an unsurprising result: if a state is highly Democratic but modernizes late, then it is likely that there are unobserved factors that cause the state to not modernize (e.g. a powerful Republican senator) that also cause an unobserved decrease in UI utilization.

The rest of this section only discusses the results in Columns 3 and 6, which display my main results. The first three rows show the fixed effects of the three modernizations. I find that all three modernizations studied in this paper had significant positive effects on Utilization at the 10% level. The largest effect, as expected, was from the ABP, which increased eligibility by about 14.0% (percent, not percentage points). PTW and CFP increased eligibility by about 10.0% and 5.4%, respectively. Moreover, I find a significant and negative change in Benefit Generosity from CFP at the 1% level, implying that UI recipients under that provision collected significantly lower average weekly benefits than those otherwise available for UI. Surprisingly, I also find a significantly positive (though small) coefficient in Benefit Generosity on PTW, which implies that job-losers collecting UI under that provision obtain *higher-than-average* benefits. I discuss the policy implications of these coefficients below.

The other control variables largely have the expected sign and have reasonable coefficient values. I find that states with higher maximum UI benefits have higher benefit generosity (e.g., a doubled maximum UI benefit is associated with a 54% increase in average weekly benefits). States with higher minimum UI benefits have slightly higher benefit generosity but lower utilization (implying that generous states actually have lower minimum UI benefits, as this increases the number of workers eligible for UI). I find that the unemployment rate is positively associated with utilization, with a coefficient that estimates that an increase in the TUR from 5% to 8% would imply an increase in utilization from 30.0% to 33.8%, because longer-term more-eligible workers are laid off during high-unemployment spells. I find that a higher hiring rate implies lower utilization, suggesting that job-losers are more likely to find a new job quickly (instead of applying for UI) in states with more hiring; moving from the 50^{th} to the 75^{th} percentile of hiring (7% to 8.5%) is associated with a decrease in utilization from 30.0% to 22.0%.

I find that large Transportation and Government sectors are positively associated with UI utilization, which might reflect a high unionization rate (which increases awareness and eligibility for UI) or high employment turnover rates.[2] One surprising result is that states with larger finance industries have significantly lower-than-average benefit generosity; this might be because states with larger finance industries are more urban, and urban areas in general have lower benefit generosity because of the availability and turnover of low-wage jobs. For the age control variables, Age 65+ is omitted out of multicolinearity, so the coefficients measure effects relative to those of Age 65+. I find that states with high percentages of young workers (below 24) and old

[2] Burtless and Saks (1984) and McMurrer and Chasanov (1995) show a positive association between larger unionized industries and higher UI utilization.

Table 6.3: Policy Evaluation Results

	Utilization	95%	Total Benefits (bill.)	95% (bill.)
ABP	517,227	± 6,205	$2.182	± $3.668
PTW	581,460	± 11,905	$3.464	± $6.997
CFP	475,370	± 14,556	$2.321	± $8.982

workers (65+) collecting UI have relatively higher utilization, while the rest of the distribution is flat. Moreover, states with high percentages workers below 34 or between 44 and 54 collecting UI have relatively higher benefit generosity. The age variables are included without logarithm, so while the industry coefficients are interpreted as percent-percent increases, the age coefficients are interpreted as percentage -percent increases.

6.3 Policy Evaluation

After the passage of ARRA, 19 states implemented the ABP, 18 implemented PTW, and 19 implemented CFP. Table 6.3 shows the Policy Evaluation results from the regression analysis above, as explained in Section 5.3, with bootstrapped 95% confidence intervals; however, as explained above, these intervals are lower bounds (I do not account for covariance between state-months). Note that the results in Table 6.4 include the increase in utilization and total benefits only in states that implemented modernizations after the passage of ARRA, which I attribute to the MIP Act's incentivization.

29,204,000 individuals collected UI between February 2009 and the end of 2011. My analysis shows that about 2,300,000 of those individuals collected UI strictly under the ABP, 520,000 of whom collected UI in states that did not have the ABP when the ARRA incentivized ABP implementation. Similarly, about 1,200,000 individuals collected UI under PTW, 580,000 of whom were in newly implementing states, and about 500,000 collected under CFP, nearly all of whom (480,000) collected in newly

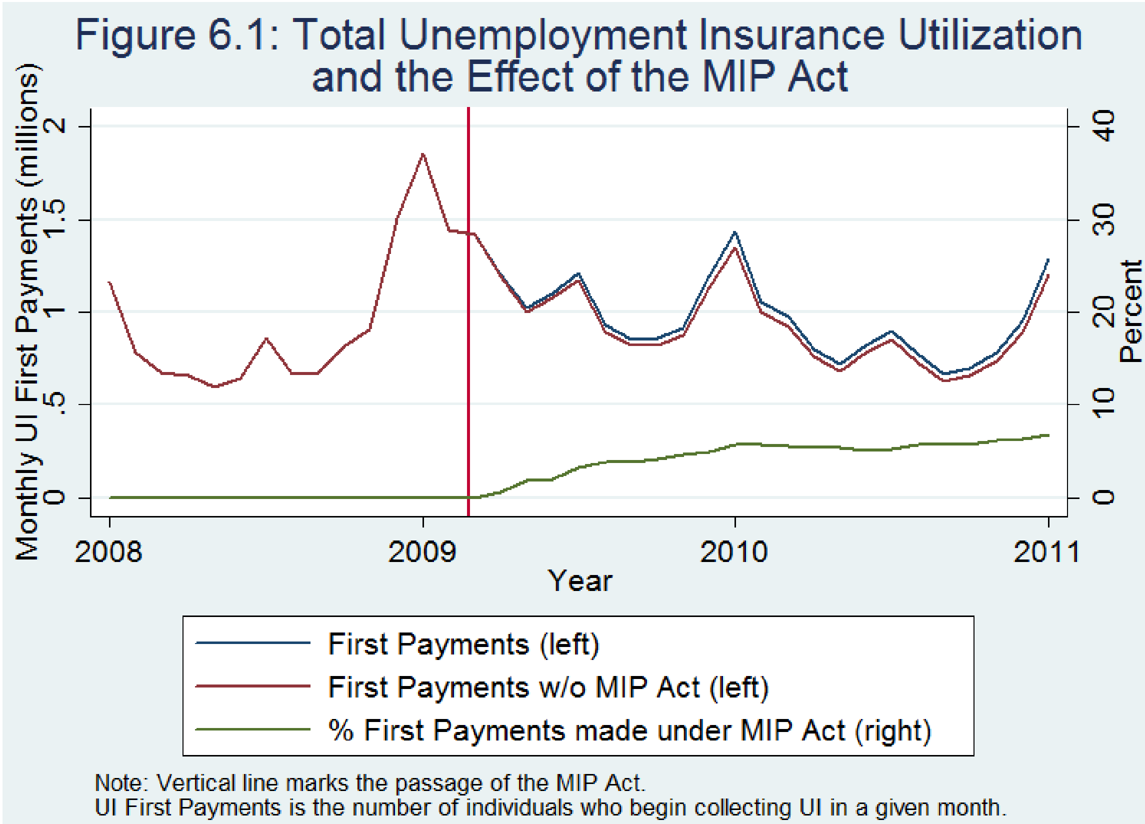

Figure 6.1: Total Unemployment Insurance Utilization and the Effect of the MIP Act

Note: Vertical line marks the passage of the MIP Act.
UI First Payments is the number of individuals who begin collecting UI in a given month.

implementing states. Figure 6.1 shows that by the end of 2011, about 8% of all new UI recipients nation-wide received UI under a modernization in a newly implementing state.

In newly implementing states, UI recipients received $2.2 billion under the ABP, $3.5 billion under PTW, and $2.3 billion under CFP, totaling about $8 billion from 2009 to 2011. This is approximately double the incentive payments made by the federal government (which totaled $4.4 billion).[3]

7. Robustness

I present three robustness checks below. First, I remove one control variable at a time from the Utilization model, showing that my results are not strictly dependent on

[3] See Modernizing Unemployment… (2012). Note that California, which has double the population of any other state that implemented any modernizations (which excludes Texas), already had PTW in effect and did not implement ABP until after 2011, but implemented CFP in 2011 and itself accounted for $239 million of the CFP payouts.

any one. Second, I vary the polynomial expansion of my control function to show the importance of selection bias and my exclusion of the cross-polynomial terms. Third, I present the standard errors derived from other variance estimation procedures, showing that my choice of Driscoll-Kraay errors is appropriate and unremarkable.

7.1 CONTROL VARIABLES

Table 7.1 shows the results of the UI Utilization model evaluated without each set of proxied control variables.[1] The only set that I do not remove is the set of state dummy variables, since they stand in as the sample selection correction for leaving some states out of my regression (as described above), and thus cannot be removed without incurring not only omitted variable bias but also (likely significant) sample selection bias.

Although there is variation in the magnitude and positive significance of all three coefficients of interest, in most cases all three coefficients are within about one standard error away from the fully identified coefficients, which are shown in Column 1. Two of the largest deviations occur in Column 7, which shows that omitting the time dummy variables increases the purported effect of the ABP to 21.6% while decreasing the purported effect of PTW to 4.7%. Remember that the time dummies have at least three roles in this model. First, they account for seasonality. Second, they account for national Utilization shocks (which may be the result of national macroeconomic conditions). Third, they nationally smooth the jumps in quarterly- and yearly-reported independent variables (for instance, controlling for the sudden national increase in population each January, when the yearly Census data updates). Moreover, there is good reason to expect

[1] I do not provide this robustness check for the Benefit Generosity model, both because that model is less well-developed (since because its interpretation is less informative) and because of the space required to discuss such a robustness check; I discuss control-omissions only from the Utilization model.

Table 7.1: Control Variable Robustness Check

	1	2	3	4	5	6	7
Alt. Base Period	0.140*	0.147**	0.148*	0.083	0.157***	0.123	0.216***
	(0.075)	(0.068)	(0.079)	(0.095)	(0.054)	(0.096)	(0.064)
Part-Time Benefits	0.100*	0.089*	0.100	0.143**	0.114**	0.082	0.047
	(0.059)	(0.050)	(0.063)	(0.065)	(0.055)	(0.051)	(0.045)
Comp. Family Reasons	0.054***	0.048***	0.031**	0.040**	0.017	0.052***	0.064*
	(0.015)	(0.015)	(0.013)	(0.017)	(0.013)	(0.018)	(0.034)
Min Weekly Bens	-0.151***		-0.159***	-0.077	-0.265***	-0.122**	-0.299***
	(0.042)		(0.047)	(0.055)	(0.037)	(0.049)	(0.058)
Max Weekly Bens	0.239		0.213	0.330	0.153	0.389*	-0.332*
	(0.278)		(0.296)	(0.348)	(0.280)	(0.233)	(0.178)
Unemployment Rate	0.233***	0.232***		0.358***	0.204***	0.228***	0.374***
	(0.036)	(0.041)		(0.091)	(0.051)	(0.035)	(0.046)
Hires Per Capita	-1.544***	-1.537***	-1.589***		-1.637***	-1.637***	-1.469***
	(0.088)	(0.102)	(0.067)		(0.139)	(0.079)	(0.112)
Industry Controls	Yes	Yes	Yes	Yes	No	Yes	Yes
Age Controls	Yes	Yes	Yes	Yes	Yes	No	Yes
Time FEs	Yes	Yes	Yes	Yes	Yes	Yes	No
State FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control Fn	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R2	0.887	0.886	0.886	0.870	0.882	0.881	0.826
Observations	2592	2592	2592	2592	2592	2592	2592

Note: Driskoll-Kraay standard errors are reported in parentheses. Significance: *10% **5% ***1%

that the time dummies are correlated with the two modernizations, because later periods are much more likely to have both modernizations. The time dummies, then, are an integral part of my model, and one should expect significant omitted variable bias if they are omitted. Assume, for the moment, that there is a linear relationship (effectively a linear probabilistic relationship) between the time dummies and each policy. Then the purported effect of the ABP, for instance, should be interpreted as the sum of all of the correlations between the time dummies and the ABP plus the actual coefficient of interest. Many more states have an ABP than have PTW, so one possible explanation for the former having a larger coefficient and the latter a smaller coefficient is that there is outsized correlation between all of the time dummies and the ABP, which bloats its coefficient, while there is net-negative correlation between PTW and the dummies, shrinking its coefficient. A second, more intuitive explanation for the difference may be that there was a positive Utilization shock before the Great Recession (perhaps resulting from national regulatory policy), and then during that recession, after controlling for cyclicality, there was a negative shock (perhaps national Tea Party dissuasion from accepting governmental payouts). Since many more states had the ABP than had PTW before the Great Recession, the ABP coefficient might capture that earlier shock, while the PTW coefficient might capture the later shock.

One other large deviation from the fully identified coefficients occurs in Column 5, which shows that omitting the industry control variables decreases the purported effect of CFP to 1.7%. Further examination shows that this purported decrease in the coefficient results from the omission of only two of the industry control variables: the Transportation industry and the Leisure industry (indeed, omitting all of the other industries, but maintaining these two, yields a coefficient of 5.3%, almost the same as the fully identified result). This implies a large negative correlation between CFP and the Transportation industry and a large positive correlation between CFP and the Leisure industry, and indeed those correlations are the case (both have magnitude above 0.2). Intuitively, the CFP coefficient in the omitted-variables model appears to be smaller than the coefficient in the fully identified model, but this is only because the coefficient is not only measuring the effect of the CFP, but is also proxying for states with large transportation and small leisure industries. Including these variables is appropriate to fix this omitted variables problem and obtain an unbiased estimate of the effect of CFP.

7.2 Control Function Polynomials

The first four columns of Table 7.2 show the results of the UI Utilization regression with the first three orders of control functions included, with the fourth column showing my

Table 7.2: Control Function Robustness Check

	Poly.				Cross-P.		
	1	2	3	4	5	6	7
Alt. Base Period	0.016	0.162***	0.203***	0.140*	0.274***	0.015	-0.150
	(0.021)	(0.044)	(0.057)	(0.075)	(0.055)	(0.178)	(0.129)
Part-Time Benefits	0.016	0.071	0.079	0.100*	0.085	0.129*	0.137*
	(0.026)	(0.046)	(0.053)	(0.059)	(0.054)	(0.065)	(0.070)
Comp. Family Reasons	0.023*	0.033**	0.038***	0.054***	0.038**	0.087***	0.094***
	(0.014)	(0.015)	(0.014)	(0.015)	(0.015)	(0.018)	(0.019)
SaSAltBase1Yes		-0.460	1.619	11.868***	0.298	0.000	0.000
		(0.351)	(1.154)	(3.686)	(0.685)	(.)	(.)
SaSAltBase1No		3.940***	9.608***	4.334	8.174***	-0.206	0.000
		(0.596)	(3.240)	(6.241)	(1.394)	(3.838)	(.)
SaSTotal1Yes		-1.681***	-5.442**	-15.446***	-5.777***	0.000	0.000
		(0.472)	(2.137)	(5.263)	(1.861)	(.)	(.)
SaSTotal1No		-0.963	-7.392***	-7.562	1.477	0.000	0.000
		(0.722)	(2.461)	(8.045)	(1.171)	(.)	(.)
SaSAltBase2Yes			-15.939**	-197.393***		10.176	0.000
			(7.853)	(60.265)		(15.521)	(.)
SaSAltBase2No			-82.254	280.001		997.437***	3599.871***
			(57.732)	(221.494)		(345.473)	(982.340)
SaSTotal2Yes			27.985*	181.762**		230.013	1822.671**
			(14.346)	(69.516)		(217.203)	(916.352)
SaSTotal2No			95.470**	-81.950		354.047***	4976.820***
			(46.774)	(282.643)		(102.798)	(1032.426)
SaSAltBase3Yes				773.992***			-183.130
				(237.815)			(353.532)
SaSAltBase3No				-4666.396*			-110299.769***
				(2492.681)			(27298.844)
SaSTotal3Yes				-616.678**			-29979.568*
				(274.283)			(15709.171)
SaSTotal3No				2454.733			-53846.321***
				(2702.143)			(11310.418)
R2	0.885	0.886	0.886	0.887	0.886	0.888	0.889
Observations	2592	2592	2592	2592	2592	2592	2592

Note: Driskoll-Kraay standard errors are reported in parentheses. Models include all control variables. Columns 5-7 include cross-polynomial expansion terms. Significance: *10% **5% ***1%

primary results. Columns 5-7 include the cross-polynomials between the two selection equations' hazard rates, which I exclude above (see Appendix 2). Notice that most of the polynomial coefficients are statistically significant, indicating significant selection bias. Also, notice that the cross-polynomial equations could not be fully estimated because of colinearity in the control function; the number of cross-polynomial terms is very high (50), and the model cannot be estimated with their inclusion (for instance, Column 7 reports the ABP *decreases* utilization by about 15%, though the result is insignificant). I defend my choice of control function polynomial in Appendix 2, and provide these results to show that both (1) lower-order or cross-polynomial control functions provide

Table 7.3: Variance Matrix Robustness Check

	Robust		Clustered		Newey		PCSE	
	1	2	3	4	5	6	7	8
Alt. Base Period	0.140*	-0.033**	0.140	-0.033	0.140	-0.033	0.140*	-0.033**
	(0.083)	(0.016)	(0.138)	(0.029)	(0.108)	(0.024)	(0.072)	(0.013)
Part-Time Benefits	0.100***	0.012**	0.100**	0.012	0.100**	0.012	0.100***	0.012**
	(0.030)	(0.006)	(0.037)	(0.013)	(0.045)	(0.012)	(0.031)	(0.005)
Comp. Family Reasons	0.054**	-0.010**	0.054	-0.010	0.054*	-0.010	0.054**	-0.010**
	(0.026)	(0.004)	(0.037)	(0.008)	(0.032)	(0.007)	(0.027)	(0.005)
R2	0.887	0.964	0.887	0.964			0.887	0.964
Observations	2592	2592	2592	2592	2592	2592	2592	2592

Standard errors are reported in parentheses. Models include all control variables.
Odd columns report UI utilization model; Even columns report UI benefit generosity model.
Significance: *10% **5% ***1%

insubstantial or inestimable sample selection bias corrections, and (2) my control function approach is the somewhat tenuous, with wide variation given different specifications.

7.3 Standard Errors

Table 7.3 shows four different kinds of standard errors estimated for the same regression results provided in Table 6.2. Columns 1 and 5 present robust standard errors, Columns 2 and 6 present clustered standard errors (by state), Columns 3 and 7 present Newey-West standard errors, and Columns 4 and 8 present Panel-Corrected Standard Errors. As discussed in Section 3.1.2 above, each of these error estimators is problematic given the structure of my data. Robust and Panel-Corrected standard errors do not account for serial autocorrelation in the error term. Robust, clustered, and Newey-West standard errors do not account for contemporaneous cross-sectional autocorrelation in the error term. Moreover, Bertrand, Duflo, and Mullainathan (2004) show using Monte Carlo simulations that, in the case of serially-correlated variables of interest (like policy fixed effects, which, in my model, are equal to 1 in every period after they are first equal to 1), robust, clustered, and Panel-Corrected standard errors all underestimate the true variance. Nevertheless, notice that the error terms presented here are largely in line with those estimated above using the Driskoll-Kraay procedure, and that most of the coefficients of interest are similarly statistically significant (different from 0).

8. CONCLUSION

The American Recovery and Reinvestment Act of 2009 designated $7 billion to incentivize states to modernize their UI systems, through a provision that I call the MIP Act. The stated intention of the ARRA was to stimulate the American economy, with the MIP Act providing money to largely low-income job-losers who were likely to spend the money quickly (through consumption smoothing). The MIP Act was also intended to incentivize states to permanently modernize their UI systems, largely by expanding UI eligibility to workers who, for one reason or another, have been excluded from their state's UI eligibility provisions. I provide answers to two questions: (1) how many people ultimately collected UI because of states' modernizing their UI programs under the MIP Act, and (2) how much money in benefits those individuals received.

I present a state-level difference-in-difference approach to answer these questions, developing an identification strategy and using a modified control function approach to correct for sample selection bias. I find that the Alternative Base Period, the Part-Time Work Provision, and the Compelling Family Reasons Provision (the three eligibility-related MIP Act modernizations) increased eligibility by 14%, 10%, and 5.4%, respectively, each of which is statistically different from zero at the 10% level. In total, my results show that an additional 1,580,000 job-losers collected unemployment insurance during the Great Recession in states that had not implemented the respective modernization until after the passage of the MIP Act, a 5.4% increase in UI utilization nationwide.

In addition to these eligibility values, I also estimate total UI benefits paid out to individuals collecting UI under each modernization, which the literature not previously estimated. I find that benefits received under the ABP, PTW, and CFP total $2.2 billion, $3.5 billion, and $2.3 billion respectively, totaling about $8.0 billion (between February 2009 and the end of 2011). These total benefits are far higher than the $4.4 billion distributed by the federal government to modernization states.[1] This estimate suggests that the MIP Act was an effective use of federal stimulus dollars, providing significant funding to low-income job-losers who were likely spend that money in the short term.

One shortcoming of my state-level two-step approach is low efficiency, which results in large errors. Efficiency could be improved in two ways. First, future research could benefit greatly from strong individual-level data, which is currently unavailable because no data source captures whether people collect UI under any specific eligibility policy. Second, future research could implement a one-step maximum likelihood strategy instead of my two-step strategy, which, though time-consuming, would provide a more efficient estimation, especially in correcting for sample selection bias.

My research suggests a number of possible topics for further study. First, I have assumed that the modernizations' effects are independent of modernizing states' demographics; using interaction terms might result in a better understanding of these effects. Second, although the above analysis corrects for sample selection bias between states that are or are not ABP states and those that are or are not 2-in-4-modernization states, I do not correct for potential sample selection bias between the four secondary modernizations, which would require modeling how states choose which policy to adopt.

[1] My results thus confirm O'Leary's (2011) worry that the federal incentive payments would cover less than three years of the increased UI benefits caused by the modernizations (the remaining costs come from state treasuries).

Third, my results show a number of strong relationships between UI Utilization and several industry and age variables that researchers have not investigated. Investigating these relationships further appears to be a fruitful avenue for future research.

9. Appendices

9.1 Appendix 1: Data Structure

I define UI utilization as the ratio of the number of people who begin collecting UI in the next state-month to the number of job-losers the current state-month. The numerator is lagged forward by one month to account for three factors: any delay in the job-loser's application for UI, any processing delay at state UI administrative offices, and any statutory waiting period (found in most states) that require the job-loser to wait approximately two weeks before beginning to collect UI (which commences on receiving a first payment). Although the numerator (UI First Payments, from UIAD) is reported for every state-month, the total number of job-losers (Separations[1], from QWI) is only reported every quarter. In order to calculate a monthly UI Utilization rate, then, I multiply quarterly separations by the proportion of that quarter's UI First Claims (which counts the number of people who apply for UI for the first time after losing their employment) that occur in that month.

I claim that the distribution of separations across months in a quarter is the same as that of first claims, which follows from two assumptions: (1) the proportion of job-losers who apply for UI stays constant in the three months of each quarter, and (2) little (or no) time elapses between job-loss and UI application. Notice that the first assumption does *not* imply a constant *PercentTakeup* over the quarter (which would assume away the need to control for that value), since *PercentTakeup* measures the percent of people *eligible for UI* who apply for UI, not the percent of job-losers in general. If the error in the ratio of first claims to job-losers is mean-zero and independent of the independent variables in the regression, then the violation of the first assumption merely suggests measurement error in the dependent variable, which does not result in biased β coefficients. Many people who apply for UI are *not* eligible, as evidenced by first claims figures that are significantly higher than first payments figures, and I assume that, when restricted to looking within a single quarter, the specific state or time is independent of the relationship between separations and claims.[2] The second assumption is more

[1] The separations data has two flaws, each working in opposite directions. On the one hand, if an individual loses more than one job within a quarter, Separations data counts their situation as a single job loss, leading to an underestimation of Separations. On the other hand, if an individual leaves one job in order to take another job, or is fired from their job, or leaves in order to commence self-employment, or leaves the labor force altogether, then these situations are counted as Separations, although none of these individuals are eligible for UI. This leads to an over-estimation of Separations. Indeed, summary statistics show that median UI Utilization stands at about 8% in my data, lower than expected. However, I assume that the over-estimation of Separations leads only to measurement error in the dependent variable, which does not imply bias of any kind. In addition, this value of separations is significantly better than either a stock measure of total Unemployed or any other available measure of job loss.

[2] Notice, then, that I assume that the first of the three delays described above to explain the lagged dependent variable is insignificant; I assume that there is very little time for most UI-collecting job-losers between job loss and first claim.

problematic, since its negation implies that the ratio of claims to separations might be artificially high or low just because of the time that people wait between the separation and the first claim. Imagine if all workers wait exactly one month after losing their jobs to apply for UI, and that separations are high in the second month of a quarter but low in the third month; in this case, my procedure assigns too many separations to the third month in the quarter, which might bias the regression results. I know of no researcher who has examined the timing of UI claims after separations, and I leave that as a question for future research. However, I think that assuming almost no lag between claims and separations is the best alternative, and assume that any lag ends up merely contributing to measurement error in the dependent variable.

Several independent variables in both substantial equations are reported on an irregular basis: total hires (quarterly), state population (yearly), statutory generosity (yearly), and average wage (quarterly).[3] However, there is no reason to expect that the failure to include monthly wage information biases the regression, and instead results only in attenuation error in the respective coefficients (which is acceptable given that they are not the variables of interest). In addition, since I include national time dummies, I do not expect any significant time-discontinuities caused by the variables' jumps every three months or every year, as those jumps are likely at the national level.

9.2 APPENDIX 2: CONTROL FUNCTIONS

Notice that, in Equation 5:

$$\mathrm{E}[\ln(U_{it})|M_{it}, X_{it}, z_{it}=0] = \beta_0 + \beta_1 M_{it} + \beta_2 \ln(X_{it})' + E[\nu_{it}|M_{it}', X_{it}', z_{it}=0] \tag{18}$$

Pioneering work on sample selection bias, like Heckman (1979), directly estimates this expected value of a truncated error term by assuming bivariate normality between the two error terms. However, μ_i is not normally distributed in my model, which prohibits using Heckman's procedure. Instead, I use the control function approach discussed in Heckman and Navarro-Lozano (2004). Those authors show that, assuming that the two error terms are independent of M_{it}' and X_{it}:

$$\mathrm{E}[\nu_{it}|M_{it}', X_{it}', z_{it}=0] = \mathrm{E}[\nu_{it}|\ln(t) - Z_i'\omega > \mu_i] = \mathrm{K}_0\big(P(X_{it}', Z_i')\big) \tag{19}$$

where P is defined as the propensity score, or the probability that a state takes treatment conditional on the available covariates, and K_0 is some function that can be estimated by a Taylor expansion of P.

Heckman and Navarro-Lozano's (2004) propensity score is determined independent of the state of the subject in previous periods; however, I cannot estimate such a propensity score using a duration model, which necessarily controls on having not modernized in previous periods. Thus, although Heckman and Navarro-Lozano use a single propensity score term to absorb all selection bias, I use two such terms, conditioning on whether or not the state has modernized before that time. I include these terms separately, estimated with different coefficients, in order to provide more flexibility to my approach. Thus, I only approximate Heckman and Navarro-Lozano's method, and do not instance it. The intuition behind both methods is the same.

This question is, to my knowledge, unresearched. However, the lag in Separations is still justified by administrative delays and state-mandated waiting periods.

[3] Note that average wage is also used as the denominator of the dependent variable in the benefit generosity model; however, measurement error in the dependent variable does not bias the coefficients of interest.

According to the assumptions of a duration model, it is only possible to move from the non-treated group to the treated group, and not vice-versa. However, selection bias clearly exists in both cases; I observe the UI Utilization of non-modernization states only in certain cases, and similarly I observe the UI Utilization of modernization states only in certain cases. Notice that Heckman and Navarro's definition of propensity score does not imply $P_{1\tau} = 0$. To the contrary, the probability that a state takes treatment conditional on the available covariates has merely changed from the hazard rate stated in Equation 9 to that same hazard rate without the "1-" term in the denominator, which conditions the hazard rate on having been selected instead of conditioning it on having not been selected.[4] Consider the intuition of this approximation: the modernization-state control function polynomials control for any variance in utilization in states that are more the kind of state that would be a modernization state, conditional on their being such a state. Despite my selection model being such that treated states cannot choose to become untreated states, the two-step estimation procedure remains essentially the same.

Vella (1998) shows that in the multivariate-normal case, if μ_{1i} and μ_{2i} are uncorrelated, then their resulting control function is additively separable.[5] Consider that assumption given my model. I assume that certain states become ABP states, and other people become 2-of-4-modernization states *for the same reasons* (i.e. because of underlying politics and fiscal need), but that knowledge about a state's passage of ABP (for instance), conditional on state politics and fiscal need, offers no insight into when (or if) the state will become a 2-in-4-modernization state. In other words, the only knowledge you get about a state's likelihood to become a 2-in-4-modernization state given when it becomes an ABP state is knowledge about its politics and fiscal need, which only indirectly relates to becoming a 2-in-4-modernization state. In general, states become 2-in-4-modernization states *after* they become ABP states, but I assume this is because the distribution of states becoming 2-in-4-modernization states peaks at a later time than that of ABP states, and is explained fully by politics and need.[6] Thus, motivated by Vella, I make this assumption that follows from the relationship between the three error terms:

$$\mathrm{E}[\nu_{it} |\, z_{1it} = 0, z_{2it} = 0] = \mathrm{K}^*\big(P^*(X'_{it}, Z'_i)\big) = \mathrm{K}_{01}\big(P_{01}(X'_{it}, Z'_i)\big) + \mathrm{K}_{02}\big(P_{02}(X'_{it}, Z'_i)\big) \tag{20}$$

where $P_{\theta\tau}$ is the propensity score of state i in time t if $z_{\tau it} = \theta$ and $\mathrm{K}_{\theta\tau}$ can be estimated by a polynomial expansion of $P_{\theta\tau}$. Thus, I do not include cross-polynomial terms in my regression.

10. Bibliography

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[4] See Lee (1978), who develops this idea in the bivariate-normal case.

[5] Notice that Vella's model is one instance of Amemiya's Type 4 Tobit model; See Amemiya (1984).

[6] This assumption implies that some states might become 2-in-4-modernizations states *before* becoming ABP states. Indeed, *four states* became 2-in-4-modernization states before becoming ABP states: Arkansas (by 3 months), California (by 12 months), Nevada (by 51 months), and Oregon (by 1 month).

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