Title
Essays on the Labor Market Effects of the Minimum Wage and Earned Income Tax Credit

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Essays on the Labor Market Effects of the Minimum Wage and Earned Income Tax Credit

Dissertation

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Doctor of Philosophy

in Economics

by

Peter Paul Shirley

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Dedication

I dedicate this dissertation to my wife, Kathryn, who encouraged and supported me in this pursuit wholeheartedly from day one.

Additionally, I would like to dedicate this dissertation to my mother, Cynthia, and my grandparents Paul and Janice Shirley. I would never have been able to pursue this dream without their love, care, and dedication.

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Abstract of the Dissertation

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Two of the most important economic policies in the United States, especially for low-income individuals and families, are the minimum wage and Earned Income Tax Credit. This dissertation explores various dimensions of these policies and how individuals respond to them.

The first chapter explores the response of commuting patterns across state borders to cross-border differentials in the minimum wage and EITC. The main results show a $1 increase in the minimum wage differential is associated with a 0.5 to 1 percentage point increase in the probability of commuting for minimum wage-eligible groups, approximately a 15-25 percent increase. Results for the EITC are less precise and show that a 0.1 increase in the phase-in rate differential is associated with an increase in commuting probability of 0.9 percentage points, a result that is not statistically different from zero.

Chapter 2 utilizes a Hierarchical Bayesian framework to estimate the effects of the minimum wage on employment in the United States using 400 elasticity estimates from 18 studies. The Bayesian Hierarchical framework allows probability statements to be made about any number of elasticity estimate ranges. Estimates from the preferred clustering method show that the probability the minimum wage has negative effects on employment is 0.731. Additionally, the probability that the elasticity between the minimum wage and employment is more negative than -0.100 equals 0.440.

As Chapter 3 discusses, one of the EITC's primary goals is to encourage labor force participation, especially for single mothers, but analysis of these effects is complicated by endogeneity issues as fertility, marriage, and educational decisions, which are typically used to define eligible and ineligible groups, are all theoretically endogenous to the EITC. By utilizing birth timing around the end of the calendar year, this paper identifies a source of arguably exogenous variation in EITC payments, albeit for mothers with newborn children. Using the Study of Income and Program Participation, results show positive employment effects for unmarried women, especially those with a high school degree or less. Additionally, I find evidence of a negative employment response by low-ed unmarried women, but this result is less robust. I argue that the mechanism through which these effects occur is the acquisition of knowledge about the EITC and its benefits.

Chapter 1: Introduction and Motivation

Two of the most studied policies in applied microeconomics are the minimum wage and the Earned Income Tax Credit. Researchers have explored a variety of outcomes which may be affected by these policies. Some questions researchers have asked are straightforward, including the effects of the minimum wage on employment (Card and Krueger (1994) and Neumark and Wascher (2000)) and the effect of the EITC on labor force participation (Eissa and Liebman (1996)). Other responses to these policies are more nuanced, such as the effect of the minimum wage on high school dropout rates (Neumark and Wascher (1995)) and the effect of the EITC on marriage (Dickert-Conlin and Hauser (2002)) and fertility decisions (Baughman and Dickert-Conlin (2003)). One outcome which has not been explored in either literature is the response of workers when changes in these policies occur across state lines. This paper seeks to uncover how worker commuting patterns across state lines respond to changes in policy differentials when the policy benefit in question is received based on place of work.

The most important implication of these commuting responses involves estimating policy effects across contiguous geographic areas. Under a scenario where, for example, effects of the minimum wage on employment are estimated using payroll data (e.g. Dube, Lester, and Reich (2010, 2016)) it is assumed that geographically contiguous areas which lie across state borders (Dube, Lester, and Reich (2010, 2016) specifically use counties) make proper treatments and controls for each other. However, if workers respond across state lines by searching more intensively for work in the state which has relatively increased its minimum wage, it is no longer clear what such an estimate is measuring.[1]

Such a response could bias estimates of the employment effect of the minimum wage positively or negatively. If, for example, employers are monopsonists in the market for low-skilled labor and an increase in a state's minimum wage attracts low-skilled labor from individuals previously working in a neighboring state, then a simple differences-in-differences estimate of low-skilled employment in the adjacent geographies using payroll data will positively bias the effect of the minimum wage on employment. Alternatively, if the market for low-skilled labor is competitive, an increase in a state's minimum wage (assuming the minimum wage binds) will decrease low-skilled employment in that state. In response to this, workers may commute across

[1] Other studies, including Thompson (2009) and Clemens and Wither (2016) use county-level data, but because their identification strategy does not rely on cross-border county-level comparisons, this bias is not as threatening to their approach.

state lines to find employment in the border state. If low-skilled employment increases in this border state then the DiD estimates across this state border will be negatively biased.[2] Also important, of course, is the degree to which such a response occurs. A detectable but small change in commuting rates likely would not invalidate results using such a strategy.

With these two simple scenarios it can be argues that commuting patterns across states for work may bias estimates of the effects of these policies when receipt of the policy (or its level of generosity) is based on place of work. The immediate question that follows from this concerns the degree to which this response can be measured and quantified. This paper explores this question in the context of the minimum wage and Earned Income Tax Credit using the American Community Survey. The main results of this paper suggest that minimum wage responses are robust across subgroups; a $1 increase in the minimum wage differential is associated with a 0.5 to 1 percentage point increase in the cross-border commuting rate. This is a 15-25 percent increase over the mean commuting rate for these groups. Results for the EITC are generally of a similar magnitude, but are less precisely estimated. Overall, the evidence suggests that individuals do indeed respond to cross-border policy differentials via their commuting patterns. However, given that these are large relative effects, but small in absolute terms, any bias introduced to estimates using a cross-border identification strategy are likely fairly small.

The actual employment effects, if any, of the minimum wage threaten to confound our estimates for a commuting response across state lines. For example, if low-skilled workers wish to respond to a cross-border increase in the minimum wage but cannot find work then no commuting response will occur. Similarly, if the minimum wage increases the number of available job opportunities then my estimation strategy will capture both of these effects. The Earned Income Tax Credit increases the relative effective wage on one side of the border without, at least in a partial equilibrium, impacting the availability of low-skilled work. Although no identification strategies have estimated the effects of the EITC in a sub-state contiguous geography manner, because there is overlap in the populations eligible for these policies the estimates of the EITC's effects on commuting reflect a reasonable approximation to those of the minimum wage net of the relevant employment effects.

[2] Alternative scenarios incorporating labor search models and heterogeneous skills could be introduced. However, for the purposes of this paper the only need is to demonstrate that these biases can exist and that their impact on the employment estimates is ambiguous.

McKinnish (2017) is most closely related paper to this research. McKinnish explores the effects of the federal minimum wage increase from \$5.15 per hour in 2006 to \$7.25 per hour in 2009 on cross-state commuting using the American Community Survey from 2005 to 2011.[3] McKinnish finds that the compression of cross-state minimum wage differentials decreases the likelihood that individuals commute out-of-state for work, which is opposite of the results found in this paper. However, McKinnish ignores all state level variation in minimum wages, which by fiat excludes a wealth of potentially useful identifying variation. McKinnish argues that this is done to avoid endogeneity of timing minimum wage laws to macroeconomic conditions. Other approaches, such as including higher order state-specific trends (Neumark and Wascher (2014)), could be used as a robustness check to explore whether such a relationship exists. This important distinction between McKinnish (2017) and this paper combined with commuting effects being driven by the largest minimum wage differentials could explain the difference in findings across the two papers. The results here, however, *are* consistent with Kuehn (2016) who poses that spillover effects can indeed bias estimates of the minimum wage's employment effects when using small geographically contiguous areas and that individuals are induced to commute across state lines towards higher minimum wages.

Furthermore, because the minimum wage has both labor demand and supply effects, it is clear that higher minimum wages should increase incentives for low-skilled workers to commute towards the now higher minimum wage, but whether or not that actually occurs net of any employment effects is ambiguous. To explore this relationship further, this paper employs the Earned Income Tax Credit as a policy that effects a roughly similar group of people (generally low-skilled), is also based on place of work, but does not have the same labor demand effects that the minimum wage does. This is a key distinction because it allows estimates of the labor supply incentives of more generous policies across state lines while not being contaminated by a labor demand response by firms from higher labor costs. This distinction allows us, to at least a degree, tease the two potentially competing effects out from one another.

Other than McKinnish (2017) and Kuehn (2016), the most related paper examining commuting across state lines in response to policy differentials is Agrawal and Hoyt (2016), who investigate how income tax differentials across state lines impact commuting times. Specifically, they model Metropolitan Statistical

[3] This paper also uses the ACS from 2005 to 2011 for its analysis.

Areas which lie on state borders and show that tax differentials distort commuting times. The robustness of their results to whether two bordering states have a reciprocity agreement demonstrates that residents of a geographical area may respond via employment location towards jurisdictions with favorable policy environments.

Of course, employment location is not the only dimension on which individuals may respond to policy differentials. Perhaps the most related context in the applied microeconomics literature is the welfare magnet hypothesis, which says that areas with more generous welfare systems should attract disproportionately higher numbers of welfare recipients. Because causal treatment effect estimation of various welfare policies would be difficult in the presence of such migration, a large volume of research exists attempting to quantify these effects, with rather mixed results.[4] Blank (1988) and Enchautegui (1997) both examine the response of women to welfare differentials, finding strong positive effects on inter-state migration from welfare generosity. Particularly, Enchautegui estimates that these effects are strongest for single mothers and for women without recent labor market experience. Similarly, Gelbach (2004) finds that groups of women with higher proportions of welfare recipients, such as never-married high school dropouts, are more likely to move to high-welfare states. These effects are strongest for women with young children, who possess a longer horizon over which benefits could be collected.

McKinnish (2005) estimates to what degree poor families are induced to cross state lines for welfare by comparing Aid to Families with Dependent Children (AFDC) participation rates in border areas to interior areas. The findings are consistent with the theory that individuals who live near a border are more likely to migrate across borders to take advantage of a favorable policy. While the policies in the welfare magnet literature operate on the basis of an individual's state of residence, other policies require only that an individual work in a state to receive its benefits.[5]

The two policies to be studied in this paper, the minimum wage and Earned Income Tax Credit state subsidies, fall in the second group. Although the welfare magnet literature does not reach a consensus about whether residents migrate across state lines in response to policy, a commuting response is more likely for several reasons. Perhaps the most obvious reason individuals would be relatively more willing to

[4]See Moffitt (1992) for a review of the classic welfare migration literature.

[5]I estimate migration effects of the minimum wage and Earned Income Tax Credit but find little evidence of a response. As such, these results are not included in this paper.

commute than migrate across state lines is that it allows them to avoid the direct costs associated with moving. Factoring in security deposits, first month's rent, utility turn-on fees, and the costs associated with physically moving possessions (e.g. renting a moving truck), a migration across state lines could cost several thousands of dollars, potentially a prohibitively large expense for low-income individuals likely to be affected by welfare, the minimum wage, or EITC policy changes. Indirect costs, such as moving away from family or friends, would also factor into an individual's decision to migrate, but not necessarily into a change in employment location.

Meanwhile, the only costs associated with commuting are transportation costs (gas, vehicle maintenance, bus fare) and value of time costs. Additionally, note that a decision to change commuting patterns occurs at the margin of current commuting times. A migration response, conversely, does not have such a marginal change; an individual either bears the full cost of the move or they do not. These differences in cost structures may make commuting responses more feasible than migration responses, especially for low-income individuals.[6] One disadvantage of studying commuting responses to policy is that because commuting costs increase with distance, both directly and in terms of opportunity costs, commuting responses are likely to only be present near state borders, whereas migration responses could occur both near state lines and in the interior of states. For this reason, the analysis in this paper will focus on geographies which lie on state borders.

Specifically, the data used in this paper come from the American Community Survey (ACS). The finest geography publicly available in this data set is the Public Use Microdata Area (PUMA). PUMAs are geographically contiguous regions with approximately 100,000 people and do not cross state borders. The implication of this geographical limitation is that any results in this paper likely translate to county-level studies, although not perfectly. In some instances a single county may be divided into multiple PUMAs while in other locations several counties may be aggregated together to create a single PUMA.

Additionally, a decision to commute to a bordering state allows an individual to change their mind more easily compared to a migration. An individual commuting across state lines who decides they no longer wish to do so can more easily adjust their behavior than an individual who migrates across state lines and decides

[6]See Greenwood (1993) for a discussion of how migration rates evolve across different observable dimensions. Note specifically that education and the probability of migration are positively correlated.

they wish to move back. If the minimum wage and EITC are "working as intended" (i.e. not as a permanent wage or permanent income supplement, but as policies which induce labor force participation, experience accumulation, and wage growth) then a worker who wishes to cross a state border to take advantage of a policy differential could be more likely to choose the option that allows them to more easily change their mind.

Given that policymakers treat the migration of individuals across state lines as a concern regardless of whether or not such movements occur (Brueckner (2000)), would a policymaker be similarly concerned about individuals commuting across state lines to take advantage of policies based on employment? Typically, a policymaker would worry about attracting large numbers of low-income residents and the associated budget concerns of a relatively generous welfare programs (i.e. becoming a welfare magnet), but attracting out-of-state workers has a more nuanced effect.

If an individual responds to a cross-border policy by commuting across the border for work, the new state of employment receives that worker's income taxes if the two states do not have a reciprocity agreement. At the same time, because they do not live in the cross-border state, the cross-border state would not pay for any welfare programs this individual may receive, which would be provided by the individual's state of residence. On their own, receiving taxes but not paying for social welfare benefits for these workers imply that states would want to "import" workers from bordering areas to work, but not live, in their state. Although, because low-income individuals pay relatively little in taxes, this effect, if it exists at all, is likely quite small. If the two states do have a reciprocity agreement, then the state of work would neither receive the individual's taxes nor pay for any social programs the individual may be eligible to receive.

However, regardless of the status of tax reciprocity across the border, pursuing such a strategy may crowd out local in-state workers who are competing for the same jobs, which could place an increased burden on the state. In the context of a higher minimum wage, for example, slightly more skilled workers may commute from outside areas and crowd out slightly less-skilled "domestic" workers. Because these domestic workers have lost employment opportunities to outsiders, they may be unable to find work and in turn fall back on the social safety net. As a result, a policymaker attempting to decrease reliance on the social safety net by the least-skilled workers in their state may inadvertently increase reliance with such a policy.

Presumably, the policymaker's intention when raising the minimum wage is to increase the earnings of workers *who reside in their state* at the bottom of the income distribution. Even ignoring the impacts such a policy could have from the demand side, increasing the minimum wage could decrease the earnings of the lowest-skilled workers in the state where the minimum wage was raised. Interestingly, this implies that even if disemployment effects of the minimum wage are modest or even zero, it is still possible for an increased minimum wage to have harmful impacts on the lowest-skilled workers who are, presumably, the group such a policy is targeting. This highlights one reason why the commuting response to cross-border policy differentials needs to be understood and measured.

While the response of commuting patterns across state lines to policy changes is a concern for policy makers, it is also a concern for researchers. When using real-world data to estimate treatment effects, a suitable control group needs to be identified in order for any estimated effects to be considered causal. One method used in the applied microeconomics literature for identifying a suitable control group is to use county-level data, exploiting differences in policy across state boundaries to assign treatment and control status to bordering counties. Researchers utilizing this approach argue that because these counties border each other, they are likely to be similar in terms of demographics and industry breakdown. Any macroeconomic shocks are likely to impact these two locations in a similar way, satisfying the parallel trends assumption of the differences-in-differences estimator.

However, careful attention must be paid to the other differences-in-differences assumption which assumes no compositional changes as a result of the treatment. If workers commute across state lines in response to changes in the minimum wage or Earned Income Tax Credit, this assumption is violated. However, before discussing the two policies in this paper in further detail, it is important to provide a broad overview of borders throughout the United States and how many provide this study with identifying variation. In total, there are 109 state borders throughout the United States, including those which meet at a single point (e.g. Arizona and Colorado). Of these 109 borders, 90 have at least one minimum wage differential over the sample period (2005-2011) and 65 have at least one EITC differential over the sample period.[7] More than half of all U.S. state borders (58) have differentials in both the minimum wage and EITC over the sample

[7] The fifteen borders marked with an asterisk in Figure 1 will be excluded from the EITC analysis because of tax reciprocity agreements. This caveat is discussed in further detail in the EITC section.

period while only about 10 percent (12 borders) have neither. These 12 borders are primarily located in two regions: the South and the northern Great Plains. Figure 1 lists all of the borders in the United States and identifies which have identifying variation for each of the two policies of interest.

The Minimum Wage

On January 1st, 1981 the federal minimum wage was increased to $3.35 per hour for all covered, nonexempt workers.[8] Over the course of the 1980s, the real value of the federal minimum wage in the United States steadily declined, as the next increase in the federal minimum wage would not come until 1990, when it was increased to $3.85 per hour. As a result, the real value of the minimum wage in 2012 dollars declined from $8.29 per hour in 1981 to $6.18 per hour in 1989, a 25 percent decrease.[9] In response to the eroding federal minimum wage, many states passed their own state-level minimum wage laws above the federal level.

Before the 1990s, studies of the minimum wage were often time-series in nature as the source of identifying variation was across-time, but the cross-state variation created by these state-level minimum wages allowed new estimation strategies, including differences-in-differences, to emerge. This research has focused primarily on differences in methodology, in particular disagreements in this literature concern the selection of a proper counter-factual group, which this paper hopes to help inform, albeit only slightly. This "new minimum wage research" is reviewed extensively by Neumark and Wascher (2007). Their conclusion from reviewing the literature is that the vast majority of studies support the traditional view that higher minimum wages have an adverse effect on employment, especially for the least-skilled. However, this is still a hotly-contested issue. More recent papers, including Allegretto; Dube; and Reich (2011) and Dube; Lester; and Reich (2010) argue that the most credible identification strategies show no discernible negative employment effects of the minimum wage. This intense debate over what, if any, effects the minimum wage has on employment demonstrates the need to uncover all possible avenues through which traditional estimates of these effects may be biased.

Debate over the merits of the minimum wage, and what an appropriate wage should be, has not been limited to the realm of academic researchers. Indeed, discussion of the minimum wage and its impacts

[8] Exempt workers include, but are not limited to, farm workers, casual babysitters, disabled workers, and newspaper deliverers.
[9] Source: https://www.dol.gov/featured/minimum-wage/chart1

Figure 1.1: Identifying Variation Across State Borders in the United States, 2005 to 2011

State 1	State 2	Minimum Wage Variation	EITC Variation
Alabama	Florida	x	
Alabama	Georgia		
Alabama	Mississippi		
Alabama	Tennessee		
Arkansas	Louisiana	x	x
Arkansas	Missouri	x	
Arkansas	Mississippi	x	
Arkansas	Oklahoma	x	x
Arkansas	Tennessee	x	
Arkansas	Texas	x	
Arizona	California	x	
Arizona	Colorado	x	
Arizona	New Mexico	x	x
Arizona	Nevada		
Arizona	Utah	x	
California	Nevada	x	
California	Oregon	x	x
Colorado	Kansas	x	x
Colorado	Nebraska	x	x
Colorado	New Mexico	x	x
Colorado	Oklahoma	x	x
Colorado	Utah	x	
Colorado	Wyoming	x	
Connecticut	Massachusetts	x	x
Connecticut	New York	x	x
Connecticut	Rhode Island	x	x
District of Columbia	Maryland	x	x*
District of Columbia	Virginia	x	x*
Delaware	Maryland	x	x
Delaware	New Jersey	x	x
Delaware	Pennsylvania	x	
Florida	Georgia	x	
Georgia	North Carolina	x	x
Georgia	South		
Georgia	Tennessee		
Iowa	Minnesota	x	x
Iowa	Missouri	x	x
Iowa	Nebraska	x	x
Iowa	South Dakota	x	x
Iowa	Wisconsin	x	x
Idaho	Montana	x	
Idaho	Nevada	x	
Idaho	Oregon	x	x
Idaho	Utah	x	
Idaho	Washington	x	
Idaho	Wyoming		
Illinois	Iowa	x	x*
Illinois	Indiana	x	x
Illinois	Kentucky	x	x*

Figure 1.1 Contd.: Identifying Variation Across State Borders in the United States, 2005 to 2011

State 1	State 2	Minimum Wage Variation	EITC Variation
Illinois	Missouri	x	x
Illinois	Wisconsin	x	x*
Indiana	Kentucky	x	x*
Indiana	Michigan	x	x*
Indiana	Ohio	x	x*
Kansas	Missouri	x	x
Kansas	Nebraska		x
Kansas	Oklahoma		x
Kentucky	Missouri	x	
Kentucky	Ohio	x	
Kentucky	Tennessee	x	
Kentucky	Virginia	x	
Kentucky	West Virginia	x	
Louisiana	Mississippi		x
Louisiana	Texas		x
Massachusetts	New Hampshire	x	x
Massachusetts	New York	x	x
Massachusetts	Rhode Island	x	x
Massachusetts	Vermont	x	x
Maryland	Pennsylvania	x	x*
Maryland	Virginia	x	x*
Maryland	West Virginia	x	x*
Maine	New Hampshire	x	x
Michigan	Ohio	x	x*
Michigan	Wisconsin	x	x*
Minnesota	North Dakota	x	x*
Minnesota	South Dakota	x	x
Minnesota	Wisconsin	x	x
Missouri	Nebraska	x	x
Missouri	Oklahoma	x	x
Missouri	Tennessee	x	
Mississippi	Tennessee		
Montana	North Dakota	x	
Montana	South Dakota	x	
Montana	Wyoming	x	
North Carolina	South Carolina	x	x
North Carolina	Tennessee	x	x
North Carolina	Virginia	x	x
North Dakota	South Dakota		
Nebraska	South Dakota		x
Nebraska	Wyoming		x
New Hampshire	Vermont	x	x
New Jersey	New York	x	
New Jersey	Pennsylvania	x	x*
New Mexico	Oklahoma	x	x
New Mexico	Texas	x	x
New Mexico	Utah	x	x
Nevada	Oregon	x	x

Figure 1.1 Contd.: Identifying Variation Across State Borders in the United States, 2005 to 2011

State 1	State 2	Minimum Wage Variation	EITC Variation
Nevada	Utah	x	
New York	Pennsylvania	x	x
New York	Vermont	x	x
Ohio	Pennsylvania	x	
Ohio	West Virginia	x	
Oklahoma	Texas		x
Oregon	Washington	x	x
Pennsylvania	West Virginia	x	
South Dakota	Wyoming		
Tennessee	Virginia		
Utah	Wyoming		
Virginia	West Virginia	x	

on labor market outcomes has occurred in a variety of media and political settings, from 24-hour news networks to presidential debates and more recently local and city council meetings. The minimum wage receives considerable discussion and it is clear ordinary Americans must be aware of both the minimum wage's existence and its variance across time and place. As a result, workers who are working at or near the minimum wage are likely to be aware of both their state's minimum wage and the minimum wage of any nearby states. Since workers are likely aware of these minimum wage differentials, it is reasonable that they may respond to cross-border variations in minimum wage policies by commuting across state lines in an attempt to reap the benefits of a higher wage.

Over the 2005 to 2011 time period, which is the sample period for this paper, there is considerable variation in minimum wage differentials across state lines. Figures 2A and 2B show how the minimum wage differential across state borders evolves over the sample period. Note that these figures are close to, but not quite, symmetrical. This reflects that state borders need not have equal numbers of PUMAs on either side. Variation in the minimum wage differential can come from either the federal minimum wage changing which occurs three times over the sample period[10] and is binding for many states (especially in the southern United States) or from individual states passing minimum wage laws. Note that the federal minimum wage is a lower bound on the effective minimum wage; states may pass laws that implement a minimum wage above the federal level, but are bound from below by the federal minimum wage.

[10]The federal minimum wage was $5.15 in 2005 and was subsequently increased to $5.85 on July 24th 2007, $6.55 on July 24th 2008, and $7.25 on July 24th 2009.

Figure 1.2A: Minimum Wage Differential Distributions Across PUMAs, 2005 to 2008

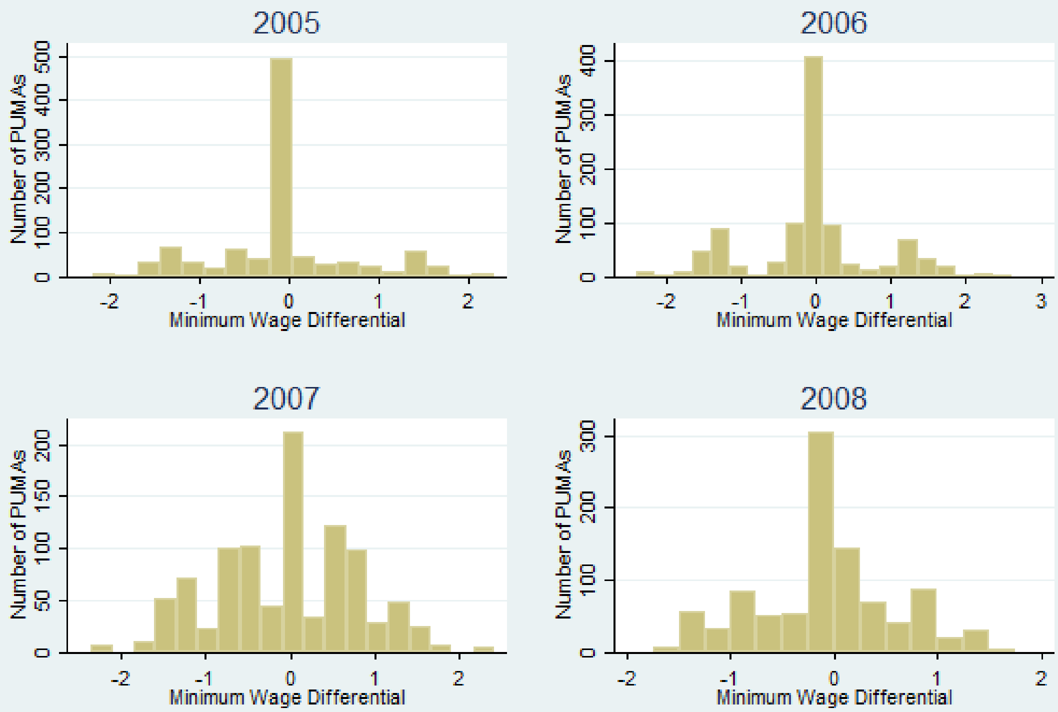


Figure 1.2B: Minimum Wage Differential Distributions Across PUMAs, 2009 to 2011

The Earned Income Tax Credit

The Earned Income Tax Credit is one of the United States's largest anti-poverty programs. This refundable tax credit was received by 28 million people for tax year 2013, and directly lifted an estimated 6.2 million people out of poverty.[11] The federal EITC schedule, which uses a recipient's (or family's) income and number of children to determine the amount of credit received, has three ranges. The first is the phase-in range, where for each dollar of earned income a worker receives, they receive a percentage[12] of that dollar in credit. This phase-in rate and its differential across states is the source of identifying variation for this paper's analysis. Once the phase-in range ends, the next range is the plateau region, wherein a claimant receives the same maximum credit over the entire region.[13] Finally, over the phase-out range, the claimant receives fewer EITC dollars as income increases until the credit phases out completely.[14][15] See Figure 3 for the federal EITC schedule for tax year 2017. The phase-in rate differential is chosen as the source of identifying variation in this paper to accurately capture the extensive margin labor market effects found by the relevant EITC literature (e.g. Eissa and Liebman (1996), Meyer and Rosenbaum (2001)).

[11]Source: Policy Basics: Earned Income Tax Credit, Center on Budget and Policy Priorities, www.cbpp.org, August 6th, 2015.

[12]The percentage at which the EITC phases-in depends on the number of children. It does not depend on the single/married filing status of the taxpayer.

[13]The maximum credit depends on the number of children. It does not depend on the single/married filing status of the taxpayer

[14]The credit phases-out at a slower rate than it is phased-in.

[15]The amounts at which the phase-in and phase-out areas of the EITC schedule begin and end are based both on the number of children and on the marital status of the taxpayer.

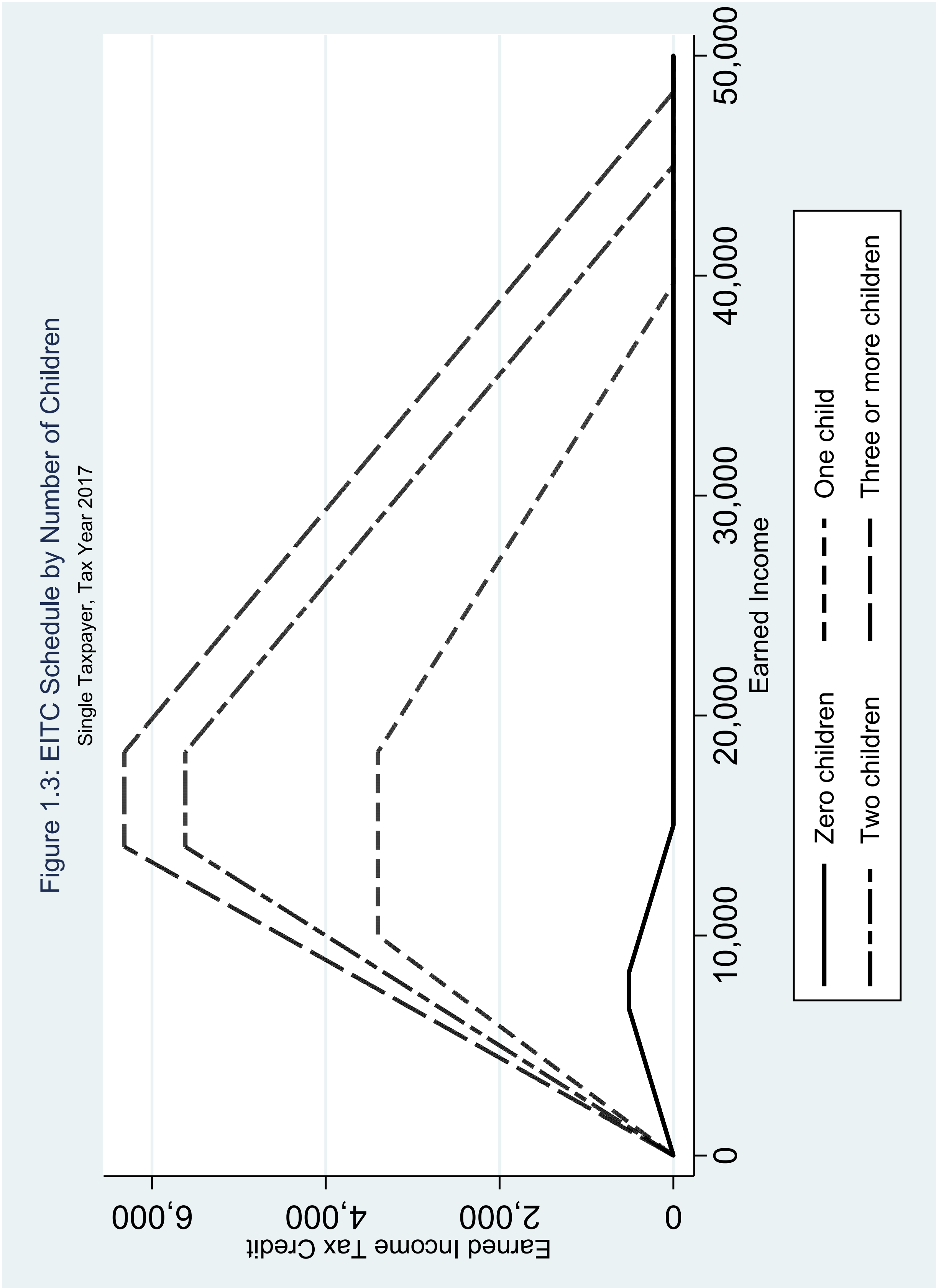


Figure 1.3: EITC Schedule by Number of Children

In addition to the federal EITC, 25 states (including the District of Columbia) as of 2014 offered a supplement to the EITC, with most of these credits calculated as a flat percentage of the federal credit. With the exception of Delaware, Maine, Rhode Island, and Virginia the state credits are fully refundable. The distinction between refundable and non-refundable credits is significant because most low-income families who are eligible for the EITC face no income tax liabilities and as a result would typically not be able to take advantage of a non-refundable credit. For the analysis performed in this paper, only refundable state credits are used.

There are three main reasons why state supplements to the EITC are a concrete choice for studying commuting patterns in response to cross-border policy differentials. First, note that take-up of the EITC is quite high. Approximately four out of five eligible individuals receive the credit, meaning the EITC can be described as salient (Currie (2006)). In order to argue that individuals respond to a policy, it is necessary to ensure that the policy is understood by those individuals, at least to a degree.

Second, the average federal EITC received in 2014 was $2,407,[16] which translates into several hundred to over a thousand dollars in state EITC supplement, depending on the state which an individual worked in. This amount is plausibly large enough to induce workers to change their place of work in order to take advantage of these differences in EITC state supplements across state lines. Note that although the federal credit is static across the entire United States for a given tax year, the state supplements can induce commuting across state lines because the supplement is tied to a worker's state income tax return which is filed with the state where a person worked, with one caveat.

The caveat to using state EITC supplements as a identification for commuting responses is the previously mentioned issue of tax reciprocity status across borders. If two adjacent states have a tax reciprocity agreement, then a worker who lives in one state but works in the other state can file a non-resident exemption from paying state taxes in the state of work. Such an individual would file a state return only with their state of residence. Alternatively, if a worker lives and works in different states and the two do not have a tax reciprocity agreement, the individual would have to file a state tax return in both the state of residence and the state of work. However, the individual is allowed to deduct any tax paid to the state of work

[16]Source: EITC Calendar Year Report - July 2014.

from their taxes owed to their state of residence. As a result, EITC differentials will only be assigned and studied in locations where they are relevant; that is, EITC differentials will be studied across borders without reciprocity agreements.

Third, the EITC state supplement has the advantage of targeting individuals and families across a variety of income levels. Indeed, both the minimum wage and EITC are available to low-income individuals, but, for example, the EITC does not completely phase-out until income reaches $47,747 for a single individual with three or more children in 2015. This means that although most minimum wage workers are likely eligible for some EITC credit (excluding, for example, workers under the age of 25 without children), there are a large portion of workers who do not work at or near the minimum wage, but are eligible for the EITC. This allows for analysis of slightly more well-off, and therefore perhaps more mobile, individuals.

EITC phase-in rate differentials are calculated using the difference between the EITC phase-in rate an individual would face in the cross-border state and the EITC phase-in rate that same individual would face in their home state.[17][18] To see the evolution of EITC phase-in rate differentials for individuals with three or more children over the 2005 to 2011 sample period, refer to Figures 4A and 4B. These differentials range from 0 to 0.172, demonstrating variation in these supplements exists across both different state borders and time. As with the minimum wage differentials in Figures 2A and 2B, note that these figures are close to, but not perfectly, symmetric. Again, this stems from differences in the number of PUMAs on either side of the border.

[17]The total phase-in rate an individual faces in a given number of children and state combination can be calculated as (1+State Supplement Percent)*Federal Phase-in Rate.

[18]Only one state does not use a flat percent of the federal EITC as their state supplement, Wisconsin, which has varying credit percentages based on number of children. In that case we use those varying percentages.

Figure 1.4A: State EITC Supplement Differential Distributions Across PUMAs, 2005 to 2008

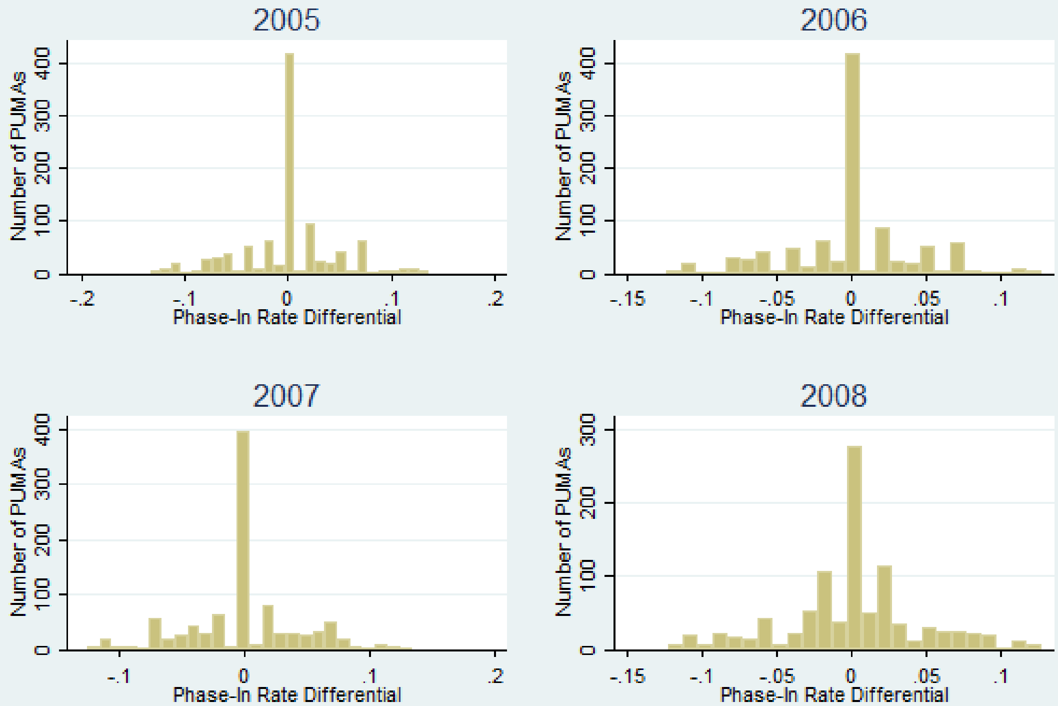
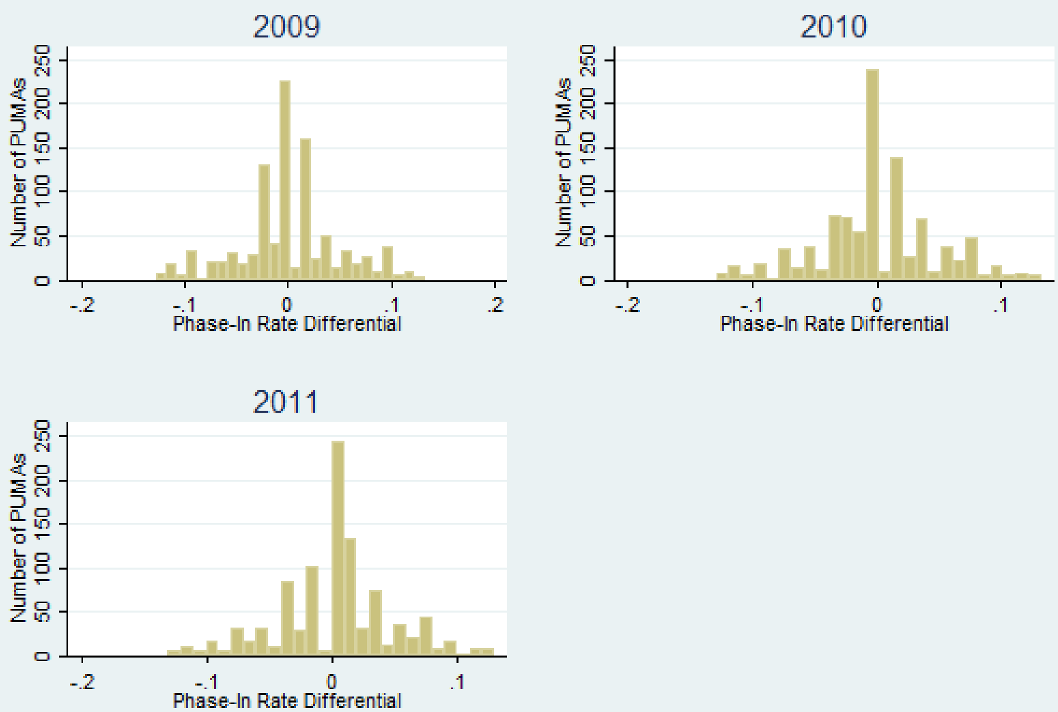


Figure 1.4B: State EITC Supplement Differential Distributions Across PUMAs, 2009 to 2011



Data

The data set for this paper comes from the American Community Survey (ACS). The Census Bureau began collecting data for the ACS in a limited fashion in 2000. Between 2000 and 2004, the scope and quantity of data collected increased. Starting in 2005, the ACS was fully implemented as a yearly one percent sample of the United States population, allowing the ACS to replace the long-form decennial census. For the analysis that will be done here, the Public Use Microdata Sample (PUMS) from the ACS will be used. The PUMS data provide individual-level responses aggregated at the Public Use Microdata Area (PUMA). PUMAs are geographic entities that are created by combining Census tracts and counties to obtain distinct contiguous areas within states that have a population of at least 100,000. In sparsely populated areas, such as Wyoming, multiple counties are combined into a single PUMA, whereas in densely populated areas a single county (e.g. Los Angeles County) can be broken up into multiple PUMAs, each made up of contiguous Census tracts. Because PUMAs are the finest geographic area available using publicly-available ACS data, the PUMA will be the sub-state geographic area of interest.

The ACS PUMS data allow a sample to be constructed that contains respondents living in PUMAs incorporating a state border. Constructing the sample in this manner includes all counties which lie on a state border. Because commuting costs increase with distance, this methodology focuses on geographies where this response is most likely to occur. In total, there are 109 state borders in the United States,[19] and of these borders 89 experience at least one change in the minimum wage differential over the sample period, 2005 to 2011.[20] There are 64 such state borders with changes in EITC differentials over the same time period. The final step before the empirical analysis is performed requires the identification of respondents who are or are likely to receive the minimum wage or EITC payments.

Using the available ACS PUMS data, there is a multitude of potential methods to define minimum wage workers. For the purposes of this paper, three definitions will be used. The first definition restricts the sample to all restaurant workers.[21] Restaurant workers are one of the most logical choices for defining minimum

[19] This includes states which only meet at a single point (e.g. Arizona and Utah).

[20] This is the time period of interest because the Census Bureau changed the PUMA boundaries between the 2000 Census and the 2010 Census. The 2000 Census PUMA definitions are used for the ACS 2005 through the ACS 2011. The 2010 Census PUMA definitions are used for the ACS 2012 and ACS 2013. As a result of these boundary changes, it is not possible to perform the analysis over the two time periods jointly. Also, the ACS 2000 to ACS 2004 report state as the finest level of geography available, so those records are not usable for this analysis either as this paper requires a geography below the state level.

[21] Restaurant workers are defined as having 722Z "Restaurants and other food services" recorded as their NAICS code.

wage receivers for two reasons. First, they are frequently studied in the minimum wage literature (e.g. Card and Krueger (1992) and Dube et. al (2010)). Second, 21.2 percent of restaurant workers work at or below the prevailing federal minimum wage, as opposed to 3.9 percent of all hourly paid workers, meaning restaurant workers have a high concentration of minimum wage workers.[22] An important caveat to this measure is that it includes tipped workers, which complicates the analysis as such workers typically earn an hourly wage below the minimum. In the analysis performed in this paper, restaurant workers whose occupation is either waiter or waitress (22.7 percent of all restaurant workers) are excluded from the analysis.

The second method restricts the sample to seemingly low-skilled workers. Specifically, this is achieved by restricting the full PUMA border sample to only individuals without a high school degree. The advantage of this method is capturing minimum wage workers across a variety of occupations and industries.

The third method for identifying minimum wage workers is to use all teenagers in the ACS PUMS. Around 15 percent of all teenagers in 2014 worked at or below the prevailing federal minimum wage compared to around 3 percent of workers over the age of 25. As with restaurant workers, teenagers are much more likely to be minimum wage workers than other segments of the population.

It is also necessary to define a sample of individuals who are likely to receive the EITC. The sample chosen for the analysis here will be single female heads of household with children. This group of individuals is a natural choice for studying these effects because single women with children are an oft-studied group in both the welfare migration (Blank (1988), Enchautegui (1997), Meyer (1998), etc.) and EITC (Meyer and Rosenbaum (2001), Eissa and Hoynes (2006), Eissa, Kleven, and Kreiener (2008), etc.) literatures. One of the main determinants for EITC eligibility is having children,[23] so restricting the sample to a group with children focuses on a population for whom the EITC is a potential source of substantial income.[24]

To measure commuting responses to cross-border policy differentials, information provided by respondents about their place of work will be used. Specifically, the ACS records both the state and PUMA where an individual works. If a worker lives on one side of a border and commutes across that border for work, they are classified as a commuter. If they work and live on the same side of the border, they are classified as a

[22] Source: BLS Report April 2015: Characteristics of Minimum Wage Workers, 2014.

[23] In Tax Year 2015, for example, the maximum federal EITC credits were $503, $3,359, $5,548, and $6,242 for individuals with 0, 1, 2, and 3 or more children, respectively.

[24] This assumes fertility is exogenous. Baughman and Dickert-Conlin (2009) find little impact of the EITC on fertility decisions.

non-commuter.

Collection of data for the ACS occurs throughout the year, but the month of the response is not recorded, creating an issue for establishing the appropriate value for either the minimum wage or EITC for a given individual. For the minimum wage, a weighted average is assigned to each individual. To create this weighted average, first it is assumed that ACS responses occur uniformly throughout the year. Second, a 12-month average is created for each type of individual. There are twelve total types, each representing one of the twelve months during the year a respondent could have completed a survey. For example, a January respondent's 12 month average would be January to December of the previous year and a February respondent's would be February of the previous year through January of the current year. From these twelve types, a single average is created from these to create the minimum wage measure used in this paper.[25] For the EITC, the process is much more straightforward as any changes in policy occur with the Tax Year, which coincides with the calendar year. As a result, there are only two EITC values (previous and current year) to be assigned based on state, year, and number of own children.

Estimation Strategy

For estimating the effects of the minimum wage and Earned Income Tax Credit state subsidies on commuting patterns, linear probabilities of the following forms will be used:

$$Commute_{ipS_Ht} = \alpha + \beta_1 MW_{S_Ht} + \beta_2 MW_{S_Bt} + X_i'\gamma + \tau_t + \rho_p + \varepsilon_{ipS_Ht}$$

$$Commute_{ipS_Ht} = \alpha + \beta(MW_{S_Bt} - MW_{S_Ht}) + X_i'\gamma + \tau_t + \rho_p + \varepsilon_{ipS_Ht}$$

$$Commute_{ipS_Ht} = \alpha + \beta_1 EITC_{S_Ht} + \beta_2 EITC_{S_Bt} + X_i'\gamma + \tau_t + \rho_p + \varepsilon_{ipS_Ht}$$

$$Commute_{ipS_Ht} = \alpha + \beta(EITC_{S_Bt} - EITC_{S_Ht}) + X_i'\gamma + \tau_t + \rho_p + \varepsilon_{ipS_Ht}$$

[25]Note: For the available years of data, only San Francisco has a local-level minimum wage. In this paper, San Francisco is treated as if it is a separate state from California.

For an individual i living in Public Use Microdata Area (PUMA) p and state S_H, $MW_{S_H t}$ and $MW_{S_B t}$ represent the average minimum wage (in 2005 dollars) in an individual respondent's home state and cross-border state for the year of their response.[26] In the second minimum wage regression, the policy variable is the differential between the weighted minimum wages of the cross-border state and the home state. The terms $X_i'\gamma$, τ_t, and ρ_p are a vector of individual-level controls, year fixed effects, and PUMA level fixed effects.[27] Recall that PUMAs are the finest level of geography published in the American Community Survey Public Use Microdata Sample and are (very roughly) analogous to counties. For the EITC linear probability model, the subscripts on the variables and the specification itself are identical to the minimum wage model with the obvious exception of the policy variables. In the first EITC model, the two policy variables included in the regression are the EITC phase-in rates for an individual in their home and cross-border states assigned based on the relevant year and number of children. In the second EITC model, the policy variable is the differential between the cross-border state and home state EITC phase-in rates.

A natural concern of the previous models is the correlation between the minimum wage and EITC state supplements. If states with higher minimum wages also tend to have more generous EITC state supplements and if workers are plausibly induced to change state of work on the basis of both policies simultaneously, then a model will need to be estimated which captures this effect. To this end, models will be estimated which include both the minimum wage and EITC effects simultaneously. This estimation strategy will allow the impacts of both policies to be estimated independently of each other. This is not necessarily a concern for some minimum wage groups which should not be eligible for the EITC (i.e. teenagers) but other low-income workers who work at the minimum wage are almost certainly eligible for some amount of EITC credit.

The choice of which fixed effects to include in the model requires some discussion. Given the structure of the ACS PUMS data and the nature of the question being asked, three possible specifications include border segment, border segment side, and PUMA fixed effects.

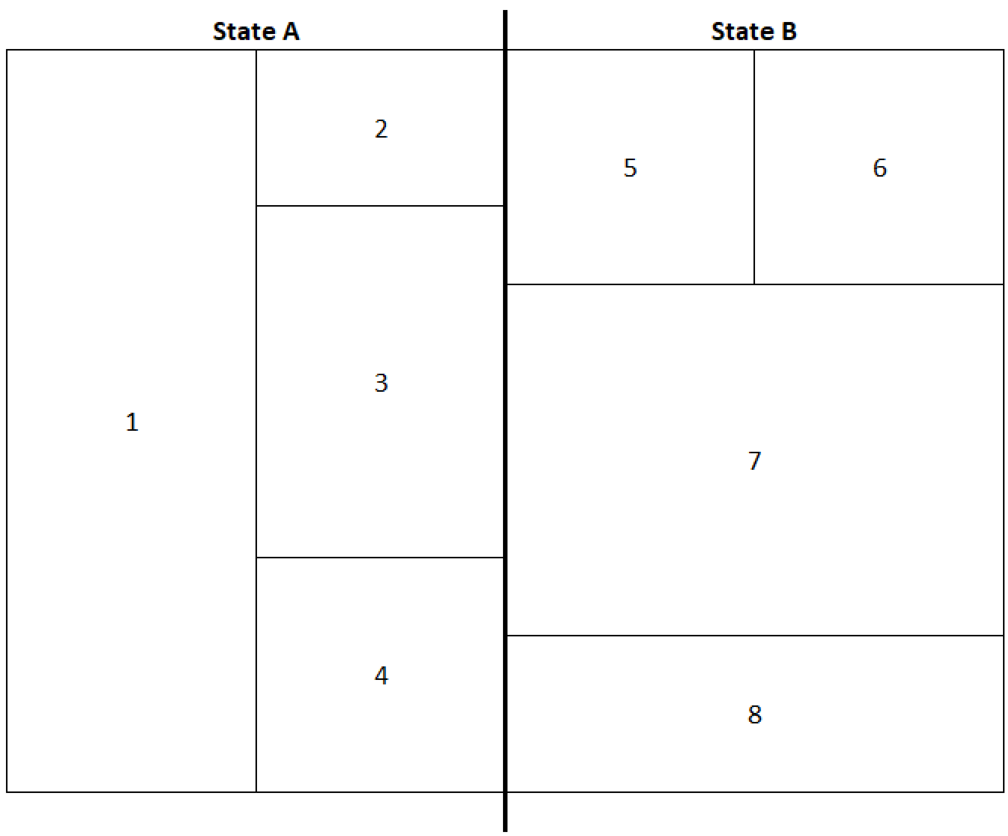
A border segment consists of all PUMAs which lie on a particular state border (e.g. New Jersey-New York or California-Nevada). A border segment side consists of all PUMAs which lie on a particular state

[26]In cases where a single PUMA borders two states (e.g. the District of Columbia bordering both Maryland and Virginia), the individuals in that PUMA appear twice, once with each bordering state's policy values.

[27]Individual-level controls include white only, black only, and Hispanic ethnicity dummies as well as a sex dummy, age, age squared, and education level dummies. Some controls are excluded from certain results (e.g. education level dummies are excluded from the high school dropout analysis).

border and are members of the same state. This means that each border segment has exactly two border segment sides. For example, the New Jersey-New York border segment has a New Jersey border segment side and a New York border segment side. Figure 5 shows an example of what a typical border segment may look like. It is important to recognize that PUMAs are perfectly collinear with border segment sides and border segment sides are perfectly collinear with border segments. As a result, only one of these three sets of fixed effects can be included at a time in any regression results. Because of this, and because PUMA-level fixed effects are the most appropriate choice, all of the results reported here will come from specifications where PUMA-level fixed effects are used.

Figure 5: Example Border Segment



In this example, the thick line in the middle represents the boundary between State A and State B, and individual PUMAs are numbered from 1 to 8. PUMAs 1 and 6 would not be included in the sample because they do not lie on the state border. PUMAs 2, 3, and 4 would represent the State A border segment side, while PUMAs 5, 7, and 8 would represent the State B border segment side. These six PUMAs combined (2, 3, 4, 5, 7, and 8) represent the State A-State B border segment.

Border segment fixed effects control for differences in commuting rates across entire border segments, allowing border segments to be relatively higher or lower traffic. This is potentially true for border segments which are entirely rural, such as South Dakota-Wyoming or Idaho-Washington, or entirely urban such as Maryland-Virginia or New Jersey-New York. Using this specification does not paint the full picture, however.

If the model is saturated further by instead using border segment side fixed effects, the model now allows for high and low commuting flows in different directions (i.e. State A to State B can have a different flow than State B to State A). Types of border segments where border segment side fixed effects are more sensible than border segment fixed effects would include areas with a large city near the border on one side, and a relatively rural area across the border. Commuting flows in these areas likely move from the rural area to the urban area, but not the other way around. Examples of such border segments include Florida-Georgia (Jacksonville, FL), New Mexico-Texas (El Paso, TX), and Kentucky-Ohio (Cincinnati, OH).

Still, even border segment by side fixed effects are not the ideal choice. The most sensible choice of specification includes PUMA fixed effects. PUMA fixed effects allow PUMAs of the same border segment, and even border segment side, to have relatively higher or lower commuting flows. This can be especially important for borders which cover long distances (e.g. Indiana-Illinois, New York-Pennsylvania, Oregon-Washington), where some parts have major cities and commuting levels are high in one or both directions, while other areas are rural with low levels of commuting. The PUMA fixed effects allow for variations in commuting rates even within a border segment side.[28]

Results

Main Results

In order to put the rest of this section into context, refer to Table 1 which shows mean commuting rates for a variety of subgroups over the full sample period, 2005 to 2011. First, note that approximately 3 to 5 percent of workers across these groups commute across state borders for work. Also note that while restaurant workers (3.18%) and single mothers with children (4.24%) commute at relatively low rates, high school dropouts (4.65%) are closer to the average rate of all workers in the PUMA border sample (5.04%). Finally, note that across all of these groups the commuting rate increases with age.

[28]Further robustness checks include the estimation of models with state by year time trends. These results are quantitatively and qualitatively similar to what is presented in this paper and are available by request.

Table 1.1: Commuting Rates for Selected Subgroups, ACS 2005 to ACS 2011

	Commuting Rate
Full Border PUMA Sample	5.04%
16 to 19 Year Olds	2.34%
16 to 17 Year Olds	1.76%
18 to 19 Year Olds	2.68%
20 to 24 Year Olds	4.03%
25 Years and Older	5.26%
Restaurant Workers*	3.18%
16 to 19 Year Olds	1.87%
16 to 17 Year Olds	1.63%
18 to 19 Year Olds	2.11%
20 to 24 Year Olds	2.59%
25 Years and Older	3.90%
High School Dropouts	4.65%
18 to 19 Year Olds	2.51%
20 to 24 Year Olds	3.95%
25 Years and Older	4.82%
Single Female Heads of Household with Children	4.24%

The commuting rate is defined as the fraction of workers who work in the cross-border state.
Restaurant Workers include all individuals with NAICS code 722Z except waiters and waitresses.

The first impacts of cross-border policy differentials on commuting patterns discussed here are for two of the three sub-populations which are likely to have minimum wage workers: restaurant workers and high school dropouts. Table 2 shows the results of the linear probability models for these two sub-populations. Note that all results throughout the proceeding sections have individual-level controls and year fixed effects with clustered standard errors at the border segment side level.

For restaurant workers, the home state minimum wage appears to have little effect on the probability of commuting. Looking at the cross-border state minimum wage, a $1 increase in the cross-border state minimum wage is associated with a 1.0 percentage point increase in the probability of commuting, although this result is only marginally statistically significant. Given that the mean commuting rate for restaurant workers across the entire border PUMA sample is 3.18% (as shown in Table 1), this is a substantial effect. To put this result into slightly more context, this effect would imply that an increase in the cross-border minimum wage from $7 per hour to $8 per hour (a 14 percent increase in gross pay) would be associated with one in every 100 workers who live at or near the border changing their state of work.

High school dropouts show similarly significant effects, with a $1 increase in the cross-border state minimum wage associated with a large increase (1.3 percentage points) in commuting probability, an effect that is statistically significant at the 5 percent level. The mean commuting rate for high school dropouts over the sample is 4.65%, so this effect is similar in magnitude to the effect found for restaurant workers. The effects are similar when analyzing the minimum wage differential, with a $1 increase in the differential associated with a 0.9 percentage point increase in the probability of commuting, a statistically significant result at the 10 percent level.

Table 1.2: Impact of Home State and Cross-Border State Minimum Wages on Commuting Probability, Results for Restaurant Workers and High School Dropouts

	Restaurant Workers		High School Dropouts	
Home State Minimum Wage	-0.004		0.0004	
	(0.002)		(0.004)	
Cross-Border State Minimum Wage	0.010*		0.013**	
	(0.006)		(0.006)	
Minimum Wage Differential		0.007*		0.009*
		(0.005)		(0.005)
Controls	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
PUMA FEs	Yes	Yes	Yes	Yes
Observations	161,757	161,757	955,480	955,480

Each column represents the results of a separate regression.
Using data from the 2005 to 2011 American Community Survey Public Use Microdata Sample, the full sample is first restricted to all individuals who live in a Public Use Microdata Area (PUMA) encompassing a state border. Then, a commuting dummy is created which is equal to one for if an individual works in the cross-border state. Using a linear probability model, this dummy is then regressed on either the home state and cross-border state minimum wages or the minimum wage differential (defined as the cross-border state minimum wage minus the home state minimum wage), a series of control variables, and selected combinations of fixed effects.
All regressions have standard errors clustered at the border segment side level.
Clustered standard errors are in parentheses.
Home state and cross-border state minimum wages are in 2005 dollars.
Restaurant workers are defined as all workers with NAICS code "722Z- Restaurants and other food services" except those whose occupation is listed as waiter or waitress.
Control variables include white only, black only, and hispanic race dummies as well as a sex dummy, age, and age squared.
Education level dummies are included for restaurant workers.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Tables 3 and 4 repeat the analysis done in Table 2 with workers in the two groups now broken out into subsets by age. Table 3 breaks restaurant workers out by age group and Table 4 does the same for high school dropouts. Both groups exhibit a similar pattern; namely, that commuting responses to cross-border state minimum wages appear to increase with age. Because many teenagers are still in high school, they have fewer hours in the day for work or for leisure. As a result, a longer commute time would represent a larger fraction of their available time for work than it would for an adult who is not attending school. Alternatively, teenagers may have more restricted access to a vehicle or other modes of transportation than adults which poses another potential barrier to them commuting. The effects of the cross-border state minimum wage are significant here from Table 3, including the response of 18-19 year old restaurant workers (1.0 percentage points, 5 percent level) drives the result for all teenage restaurant workers (0.7 percentage points, 5 percent level). This evidence suggests that older workers are more likely to respond to cross-border policy differentials, although the point estimate for 25 and older restaurant workers is larger but is not significant, even though it is the coefficient with the largest magnitude.

The story is relatively similar for high school dropouts, with point estimates for the cross-border minimum wage increasing as workers get older. Indeed, the estimate for 25 and older high school dropouts (1.4 percentage points, 5 percent level) is larger and more precisely estimated than for either 20-24 year old workers (0.7 percentage points, not significant) or 18-19 year old workers (0.3 percentage points, not statistically significant). For high school dropouts at least 25 years old, a $1 change in the minimum wage differential is associated with a 1.0 percentage point change in the probability of commuting.

The third sub-population of workers that is a natural choice through which to study effects of the minimum wage on commuting patterns is the population all teenage workers. The previously discussed Tables 3 and 4 show some of these results, but are limited to teenagers that are restaurant workers or lack a high school degree. In Table 5, the analysis expands to all teenage workers, regardless of industry. These results tell a similar story to Tables 3 and 4.

For teenagers, there is an overall small effect (0.4 percentage points) on the probability of commuting associated with a $1 increase in the cross-border state minimum wage. Breaking teenagers out into those 16-17 years old (likely still in high school) and those 18-19 years old (less likely), the results are consistent with the earlier findings. For 16-17 year olds, the point estimate on the cross-border minimum wage is quite small (0.3 percentage points) and not statistically significant. The effect is marginally significant for teenagers 18-19 years old and is slightly larger than for the 16-17 year olds (a 0.9 percentage point point estimate on a mean commuting rate of 2.68 percent).

Across all of the minimum wage specifications two main patterns emerge: the minimum wage has eco-

Table 1.3: Impact of Home State and Cross-Border State Minimum Wages on Commuting Probability

Results for Restaurant Workers By Age Group

					Restaurant Workers					
	16-19 Year Olds		16-17 Year Olds		18-19 Year Olds		20-24 Year Olds		25 Years and Older	
Home State Minimum Wage	0.002		-0.003		0.005		-0.002		-0.004	
	(0.003)		(0.003)		(0.005)		(0.004)		(0.003)	
Cross-Border State Minimum Wage	0.007**		0.005		0.010**		0.008		0.012	
	(0.002)		(0.003)		(0.003)		(0.006)		(0.008)	
Minimum Wage Differential		0.004*		0.004*		0.004		0.006		0.009
		(0.002)		(0.002)		(0.003)		(0.005)		(0.005)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
PUMA FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	39,747	39,747	19,889	19,889	19,858	19,858	27,207	27,207	94,803	94,803

Each column represents the results of a separate regression.
Using data from the 2005 to 2011 American Community Survey Public Use Microdata Sample, the full sample is first restricted to all individuals who live in a Public Use Microdata Area (PUMA) encompassing a state border. Then, a commuting dummy is created which is equal to one for if an individual works in the cross-border state. Using a linear probability model, this dummy is then regressed on the home state and cross-border state minimum wages, a series of control variables, and selected combinations of fixed effects.
All regressions have standard errors clustered at the border segment side level. Clustered standard errors are in parentheses.
Home state and cross-border state minimum wages are in 2005 dollars.
Restaurant workers are defined as all workers with NAICS code "722Z- Restaurants and other food services."
Control variables include white only, black only, and hispanic race dummies as well as a sex dummy and education level dummies.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.4: Impact of Home State and Cross-Border State Minimum Wages on Commuting Probability, Results for High School Dropouts By Age Group

	High School Dropouts					
	18-19 Year Olds		20-24 Year Olds		25 Years and Older	
Home State Minimum Wage	-0.004		0.003		0.001	
	(0.003)		(0.004)		(0.004)	
Cross-Border State Minimum Wage	0.003		0.007		0.014^{**}	
	(0.003)		(0.005)		(0.006)	
Minimum Wage Differential		0.003		0.004		0.010^{**}
		(0.002)		(0.004)		(0.005)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
PUMA FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	39,895	39,895	75,283	75,283	840,302	840,302

Each column represents the results of a separate regression.
Using data from the 2005 to 2011 American Community Survey Public Use Microdata Sample, the full sample is first restricted to all individuals who live in a Public Use Microdata Area (PUMA) encompassing a state border. Then, a commuting dummy is created which is equal to one for if an individual works in the cross-border state. Using a linear probability model, this dummy is then regressed on the home state and cross-border state minimum wages, a series of control variables, and selected combinations of fixed effects.
All regressions have standard errors clustered at the border segment side level.
Clustered standard errors are in parentheses.
Home state and cross-border state minimum wages are in 2005 dollars.
Control variables include white only, black only, and hispanic race dummies as well as a sex dummy.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

nomically and statistically significant effects on the probability of commuting across all of the identified subgroups. Additionally, we see that the effects increase with age, which is expected given that the mean rate of commuting across borders for work increases with age.

Table 1.5: Impact of Home State and Cross-Border State Minimum Wages on Commuting Probability, Results for All Teenage Workers

	All Teenage Workers					
	16-19 Year Olds		16-17 Year Olds		18-19 Year Olds	
Home State Minimum Wage	-0.002		-0.004**		-0.002	
	(0.001)		(0.002)		(0.002)	
Cross-Border State Minimum Wage	0.004*		0.003		0.005*	
	(0.002)		(0.002)		(0.002)	
Minimum Wage Differential		0.003**		0.003**		0.004*
		(0.002)		(0.002)		(0.002)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
PUMA FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	187,553	187,553	69,866	69,866	117,687	117,687

Each column represents the results of a separate regression.
Using data from the 2005 to 2011 American Community Survey Public Use Microdata Sample, the full sample is first restricted to all individuals who live in a Public Use Microdata Area (PUMA) encompassing a state border. Then, a commuting dummy is created which is equal to one for if an individual works in the cross-border state. Using a linear probability model, this dummy is then regressed on the home state and cross-border state minimum wages, a series of control variables, and selected combinations of fixed effects.
All regressions have standard errors clustered at the border segment side level. Clustered standard errors are in parentheses.
Home state and cross-border state minimum wages are in 2005 dollars.
Control variables include white only, black only, and hispanic race dummies as well as a sex dummy and education level dummies.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The main results of the EITC on commuting probabilities, estimated for all single female heads of household with children, are shown in Table 6. The estimated effects for the EITC specifications will be discussed in the context of a 0.1 change in the relevant state's phase-in rate or the differential. Note that a 0.1 increase in a state's EITC phase-in rate is a large policy shift. For example, with the current two-child phase-in rate of 40 percent, a state EITC supplement rate of 25 percent being implemented in a state that previously had no such state supplement would raise the effective phase-in rate for an individual with two children from 0.4 to 0.5, the same magnitude of shift discussed in these results. We find a large effect of the home state EITC on the probability of commuting, with a 0.1 increase in the home state EITC phase-in rate associated with a 0.5 percentage point decline in the probability of commuting for single female heads of household with children, although the standard error on this estimate is quite large and the effect is not statistically different from zero.

Table 1.6: Impact of Home State and Cross-Border State EITC Payments on Commuting Probability, Results for Single Female Heads of Household with Children

	Single Female Heads of Household With Children	
Home State EITC Supplement	-0.085	
	(0.057)	
Cross-Border State EITC Supplement	0.087	
	(0.054)	
EITC State Supplement Differential		0.087
		(0.055)
Controls	Yes	Yes
Year FEs	Yes	Yes
PUMA FEs	Yes	Yes
Observations	163,742	163,742

Each column represents the results of a separate regression.
Using data from the 2005 to 2011 American Community Survey Public Use Microdata Sample, the full sample is first restricted to all individuals who live in a Public Use Microdata Area (PUMA) encompassing a state border. Then, a commuting dummy is created which is equal to one if an individual works in the cross-border state. Using a linear probability model, this dummy is then regressed on the home state and cross-border state EITC phase-in rates, a series of control variables, and selected combinations of fixed effects.
All regressions have standard errors clustered at the border segment side level. Clustered standard errors are in parentheses.
Home state and cross-border state EITC phase-in rates are on a 0-1 scale.
Control variables include white only, black only, and Hispanic race dummies as well as a sex dummy, age, age squared and education level dummies.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

In keeping with the minimum wage results, the cross-border state EITC is associated with an increase in the probability of commuting for women in the sample with children; a 0.1 increase in cross-border state EITC phase-in rate is associated with a 0.9 percentage point increase in the probability of commuting. Note that single female heads of household with children commute at a 4.24% rate, meaning a increase of that magnitude is approximately a 25 percent increase over the mean commuting rate. It is important to note that neither the cross-border state and EITC differential results (0.9 percentage points) are statistically significant, with quite large standard errors.

Robustness

One threat to identification with the strategy employed here is that states with high minimum wages may be more likely to offer generous state EITC supplements. If this is the case, estimates of the minimum wage or EITC separately can be subject to omitted variable bias. Table 7 pools the groups analyzed in Tables 2 through 6 (restaurant workers, high school dropouts, teenagers, and single female heads of household with children) and the policies into a single regression and the results are mostly consistent with the previous tables.[29] Looking at the first two columns of Table 7, which pool all of these groups into a single regression, a $1 increase in the cross-border state minimum wage is associated with a 1.3 percentage point increase in the probability of commuting, a result which is statistically significant at the 5 percent level. Alternatively, an increase by 0.1 in the cross-border state EITC phase-in rate is associated with a 1.0 percentage point increase in the probability of commuting, an estimate that is also statistically significant. The minimum wage differential (0.9 percentage points, 5 percent significance) and EITC differential (1.0 percentage points, 5 percent level) also show that an increase in the policy differential is associated with a change in the probability of commuting, even when the policies are estimated together in a single regression.

Across all these groups we find statistically significant effects of both the minimum wage and EITC on commuting patterns. However, it would also be interesting to separate the minimum wage groups and the EITC groups out. The rest of Table 7 does exactly this. Columns 3 and 4 of Table 7 examine the minimum wage sample (restaurant workers, high school dropouts, and teenagers) and columns 5 and 6 examine the single female heads of household with children. The effects of the minimum wage and EITC are both highly statistically significant for the minimum wage sample, but not for the EITC sample. This should not be surprising. Minimum wage workers are likely eligible for the EITC, but not the converse.

To further analyze the validity of the minimum wage results, the same analysis that was performed for restaurant workers, high school dropouts, and teenagers is repeated for manufacturing, construction, and retail workers. These three groups of workers are a logical choice for a placebo test because they are low-skilled workers similar to those in minimum wage jobs, but the pay in these industries is usually above the minimum wage. As a result, should not react to changes in the minimum wage across state lines. As Table 8 shows, this appears to be the case. The home state and differential coefficients for manufacturing workers are significant, but none of the results are statistically significant for construction workers. Similarly for

[29] Specifically, every individual in these subgroups are assigned the home-state and cross-border state minimum wages and EITC supplements are assigned to every restaurant worker or high school dropout if they are the head of the household and have children. Teenagers do not receive the EITC and are assigned zero values for the EITC variables.

Table 1.7: Impact of Home State and Cross-Border State Minimum Wages and EITC Payments on Commuting Probability, Pooled Results

	All Previously Estimated Groups*		Minimum Wage Samples*		Single Female HH With Children*	
Home State Minimum Wage	-0.0001		-0.0001		-0.003	
	(0.003)		(0.003)		(0.003)	
Cross-Border State Minimum Wage	0.013**		0.012***		0.018*	
	(0.005)		(0.005)		(0.010)	
Minimum Wage Differential		0.009**		0.008**		0.013*
		(0.004)		(0.004)		(0.007)
Home State EITC Supplement	-0.097**		-0.119**		-0.077	
	(0.041)		(0.050)		(0.056)	
Cross-Border State EITC Supplement	0.097**		0.120**		0.075	
	(0.042)		(0.050)		(0.054)	
EITC State Supplement Differential		0.099**		0.120**		0.079
		(0.041)		(0.050)		(0.054)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
PUMA FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,306,670	1,306,670	1,184,113	1,184,113	163,742	163,742

*All previously estimated groups are single female heads of household with children, restaurant workers, high school dropouts, and teenagers. The minimum wage samples are all of the previously estimated groups minus single female heads of household with children.
Each column represents the results of a separate regression.
Using data from the 2005 to 2011 American Community Survey Public Use Microdata Sample, the full sample is first restricted to all individuals who live in a Public Use Microdata Area (PUMA) encompassing a state border. Then, a commuting dummy is created which is equal to one if an individual works in the cross-border state. Using a linear probability model, this dummy is then regressed on the home state and cross-border state EITC phase-in rates, a series of control variables, and selected combinations of fixed effects.
All regressions have standard errors clustered at the border segment side level. Clustered standard errors are in parentheses.
Home state and cross-border state EITC phase-in rates are on a 0-1 scale.
Control variables include white only, black only, and Hispanic race dummies as well as a sex dummy, age, age squared and education level dummies.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

retail workers, the effects of the minimum wage on commuting probabilities are small in magnitude and not statistically different from zero. Additionally, all of the magnitudes for these coefficients are smaller in absolute terms than most of the results we have found for our minimum wage workers, with perhaps the exception of teenagers. This evidence supports the notion that Tables 2 through 5 are, at least to a degree, capturing the actual effects of the minimum wage on commuting patterns.

Table 1.8: Impact of Home State and Cross-Border State Minimum Wages on Commuting Probability, Robustness Results for Manufacturing and Construction Workers

	Manufacturing Workers		Construction Workers		Retail Workers	
Home State Minimum Wage	-0.003**		-0.001		0.0001	
	(0.001)		(0.002)		(0.001)	
Cross-Border State Minimum Wage	0.004		0.002		0.003	
	(0.003)		(0.003)		(0.002)	
Minimum Wage Differential		0.004*		0.001		0.002
		(0.002)		(0.002)		(0.001)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
PUMA FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	525,742	525,742	308,188	308,188		

Each column represents the results of a separate regression. Using data from the 2005 to 2011 American Community Survey Public Use Microdata Sample, the full sample is first restricted to all individuals who live in a Public Use Microdata Area (PUMA) encompassing a state border. Then, a commuting dummy is created which is equal to one if an individual works in the cross-border state. Using a linear probability model, this dummy is then regressed on the home state and cross-border state minimum wages, a series of control variables, and selected combinations of fixed effects. All regressions have standard errors clustered at the border segment side level.
Clustered standard errors are in parentheses.
Home state and cross-border state minimum wages are in 2005 dollars.
Manufacturing workers are defined as all workers whose NAICS code begins with “3.”
Construction workers are defined as all workers whose NAICS code is “23.”
Retail workers are defined as all workers whose NAICS code begins with “44” or “45.”
Control variables include white only, black only, and hispanic race dummies as well as a sex dummy, age, age squared and education level dummies.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The EITC has a number of rules to decide eligibility, with the presence of claimable children being the most prominent. If an individual does not have claimable children, they still may be eligible for the EITC if they meet other requirements (e.g. they are between 25 and 65 years old), but EITC credits for individuals without children are quite small relative to those with children. Using these rules, a set of individuals who are ineligible for the EITC but are similar in other respects to the main EITC sample can be identified. Namely, these individuals are single female heads of household under the age of 25 who do not have children.[30] EITC

[30] There are some exceptions to this rule, such as an individual who is a full-time student and is claimed as a dependent by

credits for individuals with three or more children are assigned to these individuals and the corresponding results from this placebo test are shown in Table 9. Because these individuals are ineligible for the EITC, they should not respond to changes in these credits across state lines. The results of Table 9 lend evidence to this, as all of the coefficients are opposite the hypothesized sign and statistically insignificant. The home-state EITC coefficient is the largest in terms of absolute magnitude at 0.5 percentage point but the standard error is quite large. The EITC differential coefficient is negative and relatively small (a 0.1 increase in the phase-in rate differential is associated with a 0.3 percentage point decline in commuting), lending credence to the results previously discussed for the effects of the EITC on commuting probabilities in Table 6.

TABLE 1.9: IMPACT OF HOME STATE AND CROSS-BORDER STATE EITC PAYMENTS ON COMMUTING PROBABILITY, ROBUSTNESS RESULTS FOR A GROUP OF INELIGIBLE WORKERS

	Single Female HHs W/out Children	
Home State EITC Supplement	0.052	
	(0.185)	
Cross-Border State EITC Supplement	-0.036	
	(0.169)	
EITC State Supplement Differential		-0.033
		(0.166)
Controls	Yes	Yes
Year FEs	Yes	Yes
PUMA FEs	Yes	Yes
Observations	2,760	2,760

Each column represents the results of a separate regression.
Using data from the 2005 to 2011 American Community Survey Public Use Microdata Sample, the full sample is first restricted to all individuals who live in a Public Use Microdata Area (PUMA) encompassing a state border. Then, a commuting dummy is created which is equal to one if an individual works in the cross-border state. Using a linear probability model, this dummy is then regressed on the home state and cross-border state EITC phase-in rates, a series of control variables, and selected combinations of fixed effects.
All regressions have standard errors clustered at the border segment side level. Clustered standard errors are in parentheses.
Home state and cross-border state EITC phase-in rates are on a 0-1 scale.
Control variables include white only, black only, and Hispanic race dummies as well as a sex dummy, age, age squared and education level dummies.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The next set of results examines whether commuting responses to the minimum wage and EITC vary between MSA/non-MSA borders. As Table 10 shows, effects of the minimum wage on commuting across state lines are essentially non-existent across all groups. Meanwhile, the EITC results are remarkably consistent. Indeed, the point estimates for the EITC coefficients are quite similar between Tables 7 and 10, but here the

their parent, but this rule covers the vast majority of cases.

standard errors are quite a bit larger. This is not surprising given the decrease in sample size between the two tables. Overall, the minimum wage results are not robust across all borders, while the EITC results are quite consistent.

Conclusion

This paper has explored commuting patterns across state borders and how cross-border policy differentials impact these patterns. Utilizing American Community Survey Public Use Micro Sample data, the response of commuting patterns to the minimum wage is tested using restaurant workers, high school dropouts, and teenage workers. To test whether individuals' commuting patterns respond to the Earned Income Tax Credit, single female heads of household with children are used. The results for all of these groups indicate that commuting patterns across state lines do indeed respond to cross-border policy differentials.

The commuting response was similar in magnitude for both restaurant workers and high school dropouts using the minimum wage, with estimates of a $1 increase in the cross-border state's minimum wage associated with a 0.5 to 1 percentage point increase in the probability of commuting. The magnitude of the commuting effect was consistently largest (and usually statistically significant) for workers over the age of 25, and smallest for teenage workers. This evidence is consistent with the notion that adults are more willing and able to commute, both because they have more time available for commuting and because adults are more likely to have access to transportation. The minimum wage results are concentrated in non-MSA borders and are stronger for larger minimum wage differentials. Although large in relative terms, these effects are generally small in absolute terms and likely do not threaten estimates of the minimum wage's employment effects using county-level cross-border identification strategies.

Effects for single mothers with children responding to changes in EITC credits are similar in magnitude to the minimum wage results and are more consistent across different specifications, but the main results are suggestive at best. When estimated jointly with the minimum wage on a larger group of potentially eligible workers, 0.1 increase in the EITC phase-in rate differential across state borders is associated with a 1.0 percentage point increase in the probability of commuting, a 25 percent increase over the mean commuting rate for this sample of interest.

Table 1.10: Impact of Home State and Cross-Border State Minimum Wages and EITC Payments on Commuting Probability in only PUMAs which are part of a Metropolitan Statistical Area, Pooled Results

	All Previously Estimated Groups*		Minimum Wage Samples*		Single Female HH With Children*	
Home State Minimum Wage	0.002		0.001		0.002	
	(0.002)		(0.002)		(0.002)	
Cross-Border State Minimum Wage	0.003		0.004		-0.001	
	(0.003)		(0.003)		(0.003)	
Minimum Wage Differential		0.001		0.002		-0.002
		(0.002)		(0.002)		(0.002)
Home State EITC Supplement	-0.084		-0.115		-0.033	
	(0.071)		(0.100)		(0.030)	
Cross-Border State EITC Supplement	0.090		0.125		0.061*	
	(0.073)		(0.102)		(0.033)	
EITC State Supplement Differential		0.098		0.136		0.077*
		(0.077)		(0.108)		(0.041)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
PUMA FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	555,780	555,780	505,818	505,818	67,402	67,402

*All previously estimated groups are single female heads of household with children, restaurant workers, high school dropouts, and teenagers. The minimum wage samples are restaurant workers, high school dropouts, and teenagers.
Each column represents the results of a separate regression.
Using data from the 2005 to 2011 American Community Survey Public Use Microdata Sample, the full sample is first restricted to all individuals who live in a Public Use Microdata Area (PUMA) encompassing a state border and an MSA. Then, a commuting dummy is created which is equal to one if an individual works in the. cross-border state.
Using a linear probability model, this dummy is then regressed on the home state and cross-border state EITC phase-in rates, a series of control variables, and selected combinations of fixed effects.
All regressions have standard errors clustered at the border segment side level. Clustered standard errors are in parentheses.
Home state and cross-border state EITC phase-in rates are on a 0-1 scale.
Control variables include white only, black only, and Hispanic race dummies as well as a sex dummy, age, age squared and education level dummies.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Chapter 2: Introduction and Motivation

A cornerstone of applied microeconomics research is estimating the effects of economic policy and it is not uncommon for multiple studies to exist examining the effects of a single policy. Whether estimating for different affected groups using new or improved data sources, exploiting a different source of identifying variation, or estimating disparate outcomes, each study provides an important piece of information for categorizing and describing the overall impacts of the policy in question. However, different studies do not always reach similar conclusions; in the case of the minimum wage debate over its employment effects has persisted for several decades despite a wealth of research on the topic.

In light of this phenomenon, how can a researcher or policymaker make conclusions about the minimum wage's (or any other policy's) effects when presented with conflicting studies? One method of reconciling the issue is to compile the different studies in a qualitative literature review.[1] A more systematic approach could involve the use of meta-analysis techniques, the most recent example of which in the minimum wage literature is Belman and Wolfson (2016). This paper seeks to answer the same fundamental question as other minimum wage meta-analyses (i.e. What are the minimum wage's effects on employment?) and reaches a relatively parsimonious, if not entirely satisfying, conclusion. A significant contribution of this paper lies in its use of a Hierarchical Bayesian framework which allows me to explicitly model differences across studies of the minimum wage and generate probability statements about its effects on employment. Specifically, I model differences across author groups, dependent variable, and population of interest and, in my preferred specification, find that the probability of the minimum wage having disemployment effects to be 0.731. Additionally, I find that the likelihood these effects are "very" negative, meaning the elasticity between employment and the minimum wage to be more negative than -0.100 to be 0.440. Overall, the minimum wage is likely to have disemployment effects, with a posterior distribution centered around -0.08, and effects are more likely to be very negative than positive.

The use of a Bayesian Hierarchical framework to explore the relationship between the minimum wage and employment represents a significant advance in both the minimum wage literature as well as in the broader economics meta-analysis literature. Although there have been a number of meta-analyses exploring

[1] For a thorough and detailed analysis of this sort in the minimum wage literature see Neumark and Wascher (2007).

the effects of the minimum wage as far back as Card and Krueger (1995), but this is a marked improvement in methodology as much of the minimum wage research focuses on whether the minimum wage negatively impacts employment measures for groups likely to work at or near the minimum wage. However, regressing an employment measure on aa minimum wage measure and testing the resulting coefficient for statistical significance is only one part of the story. It is reasonable to infer that a policymaker would treat a proposed minimum wage increase differently if the elasticity between some relevant employment measure (e.g. hours for a typical minimum wage worker or number of fast-food workers) and the minimum wage were -0.010 versus -0.100. Additionally, a policymaker might respond differently to two estimates of the minimum wage's employment effects with the same point estimate but different standard errors even if neither is statistically different from zero. Previous meta-analyses of the minimum wage have primarily concentrated on whether there are detectable disemployment effects, but this approach neglects to categorize the likelihood that the minimum wage's employment effects are strongly negative (or strongly positive). A policymaker considering a minimum wage increase would be interested in the full probability distribution of employment effects, not simply the mean and whether that mean was statistically different from zero.

Regardless of eventual methodology, all meta-analysis begins with the researcher selecting which studies will to include in the analysis. In the context of minimum wage meta-analyses, a researcher may be interested in the effects of the minimum wage on employment in the United States (Belman and Wolfson (2014)), other developed countries (e.g the United Kingdom [Linde-Leonard (2014)]), or in low-income countries (Nataraj et. al (2014)). Some researchers may choose to include all available studies, while others may restrict their meta-analysis to peer-reviewed publications.[2] With the policy of interest and relevant studies identified, researchers create data sets which can include, but are not limited to, point estimates with standard errors, authors, model specification, year, journal, and other information pertinent to the how the estimation was obtained. For the minimum wage, the most commonly used unit of observation is a regression estimate of the elasticity between some measure of employment and the minimum wage. With these elasticities in hand, different visual (e.g. funnel plot) or statistical (e.g. meta-regression) techniques can be applied to explore the relationship between the outcome and policy of interest.

[2]Both approaches have pitfalls, which will be discussed in the next section.

Meta-analysis has gained popularity across a variety of disciplines including applied microeconomics, but its roots lie in medicine, where a single clinical trial may be individually unable to determine that a treatment or drug's efficacy is statistically different from zero, but combining several trials which study the same drug allows its effects to be distinguished or categorized more broadly. The fact that these techniques were popularized in medical trials which often rely on either single-blind or double-blind randomization, is an important distinction. This randomization implies that the effect of the drug can be considered causal and the estimate of that drug's effectiveness can be viewed as a draw from the true underlying treatment effect distribution. As a result, the easiest and most direct method of aggregating these studies would be to simply stack the data together and create a single estimate of the drug's effectiveness. Of course, in real medical trials small differences in protocol can preclude these types of analyses and random-effects models are typically employed in practice (DerSimonian and Laird (1986)). Studying the minimum wage (and other economic policies more generally) poses a number of issues which challenge the view of each study, or each estimate from a study, as an independent and unbiased draw from the underlying sampling distribution. These issues include publication bias (and the related file-drawer problem), sample of interest, and model specification.

The Basic Meta-Analysis Framework and Potential Issues

The first issue, publication bias, can appear in several forms but only one will be discussed at length in this paper. The particular form of publication bias often discussed around the minimum wage literature arises when journal editors or referees show favor in publication decisions towards research submissions whose estimates show statistical significance.[3] In a literature muddled by this form of publication bias, a researcher performing a meta-anlaysis can no longer treat estimates of the parameter of interest as independent draws from the underlying sampling distribution. If the true parameter of interest's sampling distribution were centered around zero, now one (or both) tails of the distribution are over-represented with a "hollowing-out" of the distribution's midsection. Ideally, a researcher performing a meta-analysis would like to have every available estimate of a policy's treatment effects, even those rejected for publication. However, if journals

[3] DeLong and Lang (1992) extensively discuss this issue and the evidence for publication bias in the general economics literature.

give preference in publishing to papers whose results show statistical significance, traditional meta-analysis techniques which fail to account for this issue will be biased in the direction of the publication bias, a symptom of what is more commonly known as the "file-drawer problem" (Rosenthal (1975)).

A simple test for detecting publication bias in a given literature is the funnel plot, a scatter plot with the horizontal axis as the estimated coefficient of interest from a regression, and the vertical axis as the precision of that estimate (i.e., the inverse of the standard error). In the case of the minimum wage, the coefficient of interest from a regression is typically an elasticity between a measure of employment and the minimum wage. Assuming that the estimates are unbiased and the only differences in estimated coefficients are due to sampling variation, the graph of these estimates should create a symmetrical inverted funnel shape around the true value of the parameter of interest. If the true effect of the minimum wage was zero (or very close to it), and referees or journal editors were biased towards papers with negative and statistically significant effects, then a simple perusal of the literature may lead readers to believe either that the minimum wage has an adverse effect on employment when it does not, or to overstate the actual adverse effects on employment. Evidence in favor of specification searching for results of a particular sign can be seen in a funnel plot if the distribution is heavily asymmetrical, and evidence in favor of preference for statistically significant results can be seen if the tails of the plot have much greater mass relative to the mode of the distribution.

Publishing work in a peer-reviewed journal involves authors as well, and journal editors or referees potentially favoring statistically significant results does not paint the complete picture. Researchers have incentives to publish peer-reviewed research and if they know or believe either editors or referees prefer statistically significant results, they may engage in specification searching, where they run various versions of their model until statistically significant results are found. The model specification(s) eventually shown in the paper may not be the most appropriate choice(s),[4] and the meta-researcher must decide how to handle the specifications shown given there may be specifications the original researcher estimated but ultimately did not include in the study for a variety of reasons, including legitimate ones.

In order to accomplish this, the meta-researcher must first take steps to identify and detect the presence of publication bias in a particular literature. While the aforementioned funnel plot is a relatively easy method

[4]What makes an appropriate choice of model specification is an extraordinarily interesting but difficult question that I leave open-ended for future work. I tackle this issue indirectly with author group clusters, but I do not take a stand on whether some authors are "right" and others are "wrong."

of visually examining a policy's effects and may be useful for detecting obvious publication biases, it can be prone to subjective interpretation. Meta-regression, conversely, intends to answer the same fundamental questions, but is more rigorous and not open to subjective interpretation. At its most basic level, a meta-regression may take the form:

$$Est_i = \beta_0 + \varepsilon_i \tag{1}$$

where Est_i are the estimated coefficients of interest (e.g. an elasticity between and employment measure and the minimum wage), β_0 is intended to capture the true effect. Note that in this example β_0 would be a simple average of the estimated effects. However, the goal is to incorporate publication bias, so Equation 1 could be modified slightly to:

$$Est_i = \beta_0 + \beta_1 se_i + \varepsilon_i \tag{2}$$

where Est_i and β_0 are the same as in Equation 1 but now $\beta_1 se_i$ is intended to measure the degree of publication bias. As previously mentioned, if the only difference in estimated effects were due to sampling variation, then $E(\beta_1) = 0$. However, Begg and Berlin (1988) show that publication bias is proportional to the standard error when only statistically significant results are published. Also, as noted by Stanley (2005), the ε_i in this equation are clearly heteroskedastic, but, $1/se_i$ is an estimate of that equation's heteroskedasticity and dividing each side by this measure allows Equation 2 to be re-expressed as:

$$t_i = \beta_1 + \beta_0 \left(1/se_i\right) + \nu_i \tag{3}$$

Note that increases in sample size will increase the precision of estimates. As a result, the dependent variable is now the simple t-statistic t_i, which is now a function of the estimate's precision $(1/se_i)$ and β_0 now acts as a test of the asymmetry of the funnel plot. This Funnel Asymmetry Test gives an indication of the size and direction of any publication bias based on $\hat{\beta}_1$, derived from an OLS estimation of Equation 3. These two techniques form the basis for the meta-analyses performed in the minimum wage literature.

To this point, the discussion of meta-analysis techniques has focused on a literature that featured relatively homogeneous studies. That is, each estimate could be seen as a draw from the underlying sampling distribution of our parameter of interest, the elasticity between employment and the minimum wage. But to characterize the minimum wage literature in such a manner would be incorrect. Indeed, there are potentially many different outcomes (earnings, employment, employment growth, hours to name only a few) and many different populations of interest (teenagers, restaurant workers, high school dropouts). It is not necessarily the case that all of these outcomes or groups must be affected by the minimum wage or, even if they were, affected to the same magnitude or direction. Accounting for these cross-group and cross-outcome differences is an important next step in building upon the simple meta-regression framework.

Sample of interest, particularly in the minimum wage literature, is another potential source of bias in meta-analysis estimates. A plethora of groups working or likely to be working at or near the minimum wage have been studied with respect to changes in minimum wage policies including restaurant workers (e.g. Card and Krueger (1994); Neumark and Wascher (2000)), high school dropouts (Deere, Murphy, and Welch (1995)), and teenagers (Card (1992)). In fact, the minimum wage literature explicitly discusses to what extent increases in the minimum wage may have different employment effects across these groups (Neumark and Wascher (2007)). To the extent that disparate effects of the minimum wage across these populations exist, meta-analyses attempting to summarize the minimum wage's effects must successfully account for these differences; otherwise its estimates will be biased towards whichever population is the most frequently studied.

A priori it is not clear which affected group is the most important or relevant[5] for a study of the minimum wage, nor have researchers honed in on any particular group. In part, this leads back to the idea of there not being one true effect of the minimum wage on employment and there can be differential effects across groups. Nevertheless, this is a testable hypothesis as researchers have created a body of research that captures minimum wage effects across a number of eligible groups. Restaurant or fast-food wokrers could be argued as the most relevant or interesting group, in part because the food and beverage sector employs more minimum wage workers than any other. However, many teenagers work minimum wage jobs in the food and beverage sector, creating a large degree of overlap between the two groups. If a policymaker's primary concern when adjusting the minimum wage is the well-being of adult primary-earners, then analyzing the employment effects for restaurant workers may not be ideal. High school dropouts are another potential group

[5] Assuming such a group even exists.

of interest because they are generally older workers and hence are more likely to be a head of household or to be supporting a family. At the same time, many high school dropouts do not work at or near the minimum wage introducing a substantial amount of noise to any analysis. Analyzing employment outcomes across subgroups and explicitly accounting for potential differences between these groups is an important component to minimum wage meta-analysis, both because of potential differential effects across groups and certain groups may be of greater concern or relevance for policymakers.

The final major issue this paper accounts for highlights a major focus of the past 25 years of minimum wage research, specifically dealing with model specification and the identification of a valid counter-factual group. A great deal of the research looking at the effects of the minimum wage on employment has focused on the fundamental issues of identification, and it is possible many of the estimates of the minimum wage's effects on employment are biased. As previously discussed, meta-analysis relies on the assumption that it is aggregating unbiased estimates of a particular treatment effect and the discourse in the minimum wage literature highlights that this is likely not the case in simple meta-analyses. Without the ability to distinguish a "good" study from a "bad" study, it would be quite difficult for a reader of the minimum wage literature to make a solid conclusion.

The approach taken here to account for this issue is the inclusion of author-group specific clusters. This allows for estimated effects to have different distributions across authors. If certain authors routinely find estimates more negative or positive this can be explicitly modeled and quantified. Note that such effects could be found if certain authors are specification searching for results that confirm their beliefs or they think has the best chance of being published. Alternatively, authors could use a particular methodology that they find the most defensible and it is the methodology that pulls their estimates in a particular direction. This paper seeks to demonstrate if and how author groups differ from one another in a quantitative manner and leaves the question of determining "good" estimates from "bad" as an avenue for future research.

These are three of the largest issues facing a researcher when attempting to perform a meta-analysis with studies using economic policies as treatments and real-world data. The Bayesian Hierarchical framework implemented in this paper allows all three of these issues to be accounted for, modeled, and assigned posterior distributions. This allows for a test of statistical significance, as is the standard benchmark in applied microeconomics, but also for probability statements to be made about any range of values for the elasticity between employment and the minimum wage. Beyond being of interest for its own sake, the minimum wage is perhaps the most interesting case study for showcasing the value of meta-analysis in a broader applied microeconomic context.

The Minimum Wage as a Case-Study

Before the 1990s, studies of the minimum wage often relied on cross-time variation in the federal minimum wage (e.g. Brown, Gilroy, and Kohen (1982); Meyer and Wise (1983)). However, throughout the course of the 1980s, the minimum wage policy environment in the United States changed. On January 1st, 1981 the federal minimum wage was increased to $3.35 per hour for all covered, nonexempt workers.[6] Over the next decade, the real value of the federal minimum wage steadily declined, as the next increase in the federal minimum wage would not come until 1990, when it was increased to $3.85 per hour. As a result, the real value of the minimum wage (in 2012 dollars) declined from $8.29 per hour in 1981 to $6.18 per hour in 1989, a 25 percent decrease.[7] In response to the eroding federal minimum wage, many states passed their own state-level minimum wage laws above the federal level.

The cross-state variation at any given period of time created by these state-level minimum wages allowed new estimation strategies, specifically differences-in-differences, to emerge. From this, the "new minimum wage research" emerged, focusing on appropriate methodologies and how to select a counter-factual group. This literature is reviewed extensively by Neumark and Wascher (2007), who conclude the vast majority of studies support the traditional view that higher minimum wages have an adverse effect on employment, especially for the least-skilled. However, this is still a hotly-contested issue. More recent papers, including Allegretto; Dube; and Reich (2011) and Dube; Lester; and Reich (2010) argue that the most credible identification strategies show no discernible negative employment effects of the minimum wage. This abundance of literature in the past 25 years is the first of three reasons why the minimum wage is the most interesting case study for examining the more general question of how a policymaker or researcher should think about the effects of a particular economic policy. Meta-analyses by their definition aggregate results from multiple

[6] Exempt workers include, but are not limited to, farm workers, casual babysitters, disabled workers, and newspaper deliverers.
[7] Source: https://www.dol.gov/featured/minimum-wage/chart1

studies to qualitatively or quantitatively (or both) summarize the relevant literature. Because of this, literatures with large and diverse identification strategies and data sources, such as the minimum wage, stand to gain the most from such an analysis.

Second is the extent of disagreement among labor economists on the effects of the minimum wage on employment. This can most readily be seen in the exchange of Card and Krueger (2000) and Neumark and Wascher (2000), who discuss potential issues with Card and Krueger (1994), the famous New Jersey-Pennsylvania fast-food restaurant study. A policy with a large volume of research where a general consensus exists on the policy's effects is an uninteresting topic for a meta-analysis; there is little to meaningfully contribute to the literature. Conversely, a meta-analysis of a policy without a relatively mature literature is unlikely to yield anything informative. The minimum wage exists in the position of having an active and extensive literature while still the subject of intense debate over the policy's effects.

Third is the minimum wage's relevance to the contemporary political climate in the United States. Two notable examples are California's recent passing of a $15 per hour minimum wage by 2021 and New York's plan to raise minimum wages to $15 per hour in New York City by the end of 2019[8] and at least $12.50 by the end of 2020 statewide. These policy decisions, along with similar discussions by the District of Columbia and other locations including Seattle (Jardin et. al (2017)), mean the minimum wage will reach unprecedented levels, in real terms, over the next five years. Discussions about the minimum wage also featured in the 2016 U.S. Presidential election, particularly during the Democratic primaries. Aggressive increases in the minimum wage across a number of locations throughout the United States, combined with a lack of formal consensus on its employment effects underscores the need for additional analyses of the policy.

State of the Minimum Wage Meta-Analysis Literature

Card and Krueger (1995) represents the first attempt to use meta-analsyis techniques to aggregate multiple studies of the minimum wage's effects on employment. Using 15 time-series studies of the minimum wage, their assessment is that the selection of results published may have been subject to specification searching and publication bias. They argue this could result either if editors are predisposed towards negative statis-

[8]For large businesses (with 11 or more employees) the minimum wage will be $15 per hour by December 31, 2018 while for small businesses (10 or fewer employees) the minimum wage will reach $15 per hour by December 31, 2019.

tically significant results (if, for example, they line up with the editor's economic intuition) or researchers are biased towards and search for specifications with negative and statistically significant results (if theys align with the authors' economic intuition or if they believe such results give their paper a greater chance of publication). The issue with this meta-analysis stems from their use of time-series studies which rely on cross-time minimum wage variation. These studies can conflate effects of the minimum wage with variation in other unobserved factors which are common across the country. The "new minimum wage research" other meta-analyses rely on uses considerably more reliable identifying variation, relying on both cross-time as well as cross-state variation.[9]

Belman and Wolfson (2014) are able to obtain 439 estimated minimum wage elasticities from 23 studies of the minimum wage's effects on employment, hours, or both. Utilizing a funnel plot, they find a fairly symmetrical distribution around an elasticity of -0.05. They conclude that if the minimum wage does indeed have negative effects on employment these effects are too small to be economically meaningful. A follow-up to this work, Wolfson and Belman (2016), reaches a similar conclusion with an expanded list of studies around a slightly more negative elasticity range of -0.12 to -0.05.

Doucouliagos and Stanley (2009) use 64 studies of the minimum wage in the United States (1,474 estimates) to explore the evidence for publication bias in the minimum wage literature. The authors criticize Card and Krueger (1995) for mistaking publication bias with the absence of a genuine effect. They find that a funnel plot of the estimates of the minimum wage's effect on employment to be rather asymmetrical, which they argue is evidence for the existence of publication bias. They conclude that the minimum wage literature is contaminated by publication bias, with the magnitude of publication bias to be slightly larger than the average reported effect of the minimum wage. Thus, Doucouliagos and Stanley conclude that correcting for publication bias leaves little to no evidence for negative impacts on employment from the minimum wage.

Similarly, Chlestos and Giotis (2015) find evidence for publication bias and no effects of the minimum wage on employment using 77 international studies published since Card (1992) and Katz and Krueger (1992). Chlestos and Giotis use a meta-regression framework with 27 explanatory variables to control for model specifications and population of interest and show the degree of the effects of the minimum wage on

[9] Some studies also look at local minimum wages, relying on cross-geography variation within a state.

employment do indeed differ along these dimensions.

Linde-Leonard et. al (2014) also use a meta-regression to analyze 16 studies from the United Kingdom using an ordinal measure with four categories (negative or positive and statistically significant or not statistically significant) finding "no overall practically significant adverse employment effect" and no evidence of publication bias. Boockmann (2010) looks at 55 empirical studies across major industrial countries and finds that the effects of the minimum wage are heterogenous across countries. Boockmann identifies three sources of heterogeneity: levels of benefit payments, employment protection, and the collective bargaining system. Boockmann also finds no evidence of publication bias. Nataraj et. al (2014) use 11 studies of current or recently low-income countries and find that after controlling for publication bias, higher minimum wages are associated with lower levels of formal employment and an increase in the share of workers who are employed informally. They conclude that the overall employment effects are ambiguous.

Perhaps unsurprisingly, as with the literature on the effects of the minimum wage on employment, the meta-analysis literature also disagrees on the minimum wage's effects. Although different studies reach varying conclusions about these effects, a common theme is that studies concerned with evaluating the role of publication bias find evidence that it indeed contaminates the minimum wage literature.

Methodology

The main estimation strategy in this paper is a Hierarchical Bayes framework. In a typical (non-hierarchical) Bayesian model, the parameters θ determine a probability distribution over the outcomes and y represents the observations collected as a series of draws from this distribution. The posterior distribution under these conditions can be written as:

$$\pi(\theta|y) \propto f(y|\theta)\pi(\theta)$$

That is, the posterior distribution is proportional to the product of the likelihood $f(y|\theta)$ and the prior distribution $\pi(\theta)$.

In a hierarchical model, however, observations belong to clusters. In this specific context, clusters will be all elasticity estimates of employment with respect to the minimum wage which come from the same group of authors, share the same dependent variable category, or come from the same population of interest. Any authors which have published a paper together on the effects of the minimum wage are included as a single cluster. Intuitively, this captures correlations in estimated elasticities among author groups. For example, these clusters would capture authors who selectively choose only estimates of a particular sign or magnitude which align with their own views, or which give them the highest probability of publishing the study. It also captures, at least to an extent, differing methodologies across author groups which could act as the mechanism through which they gear estimates towards a particular result.[10] The second clustering level used in this paper captures differential effects for teenagers, retail workers, restaurant workers, and all other populations of interest. Again, to the degree effects are different across these groups, the model is able to capture this via clustering.

To handle different outcomes I choose not to cluster, but instead estimate across different categories of results, specifically employment and hours. Earnings also make a natural choice to include in this group but all earnings estimates in the data come from a single author group. because it is rather unclear what μ would represent if employment and hours clusters are estimated together and if such a parameter is meaningful, I have chosen to produce separate estimates for each group individually and present them as such.

The notable econometric difference when clustering observations in a Bayesian framework is that the distribution over outcomes is now determined by both θ, the parameters which are shared by all clusters and by another set of parameters which are shared within a cluster, but not necessarily across clusters. Note that there is now also a probability distribution over the cluster-specific parameters, denoted as σ_b.

For the purposes of estimation, it will be assumed that each cluster will have a different mean μ_{jk}, but a shared variance, σ_y^2. Further, we will assume that the individual means μ_{jk} are distributed normally with a variance σ_μ. Our model can then be written succinctly as:

$$\mu_{jk} \sim \mathcal{N}(\mu, \sigma_\mu^2)$$

[10] I explicitly do not take a stand on whether any particular methodology is correct or incorrect.

$$y_{ijk} \sim \mathcal{N}(\mu_{jk}, \sigma_y^2)$$

where μ_{jk} is a cluster-specific mean and y_{ijk} is the elasticity estimate i from cluster jk. It is perhaps more intuitive to think of the cluster specific means as deviations from the overall mean μ. Here, μ becomes a θ parameter (as it is shared across all of the clusters) and the mean of deviations would be zero. Our model in this case can be written as:

$$b_j \sim \mathcal{N}(0, \sigma_{b_j}^2)$$

$$b_k \sim \mathcal{N}(0, \sigma_{b_k}^2)$$

$$\mu_{jk} = \mu + b_j + b_k$$

$$y_{ijk} \sim \mathcal{N}(\mu_{jk}, \sigma_y^2)$$

This parameterization lends itself to being written quite nicely as a linear model of the form:

$$y_{ijk} = \mu + b_j + b_k + \varepsilon_{ijk}$$

where

$$b_j \sim \mathcal{N}(0, \sigma_{b_j}^2)$$

$$b_k \sim \mathcal{N}(0, \sigma_{b_k}^2)$$

$$\varepsilon_{ijk} \sim \mathcal{N}(0, \sigma_y^2)$$

Data

The data used for this analysis was provided by Dale Belman and Paul Wolfson, and is the data set used for Belman and Wolfson (2013). Their full data set contains information on authors, paper titles, and estimated elasticities with standard errors for 23 papers and 448 total elasticities, but I exclude studies performed on data outside the United States, resulting in 18 papers and 408 elasticities. The decision to exclude studies performed on data from countries outside the U.S. is driven by two reasons: first, the research question of interest in this paper focuses solely on the effects of the minimum wage on employment in the United States. Second, estimates of the effects of the minimum wage on employment in other countries confounds labor

Table 2.1: Studies Included in Analyses

Author	Journal	Year	Author Group
Addison, Blackburn, and Cotti	Labour Economics	2009	1
Addison, Blackburn, and Cotti	British Journal of Industrial Relations	2012	1
Addison, Blackburn, and Cotti	Labour Economics	2013	1
Allegretto, Dube, and Reich	IRLE Working Paper No. 181-09	2009	2
Allegretto, Dube, and Reich	Industrial Relations	2011	2
Bazen and Marimoutou	Oxford Bulletin of Economics and Statistics	2002	3
Belman and Wolfson	Labour	2010	4
Dodson	Journal of Labor Research	2002	5
Dube, Naidu, and Reich	Industrial & Labor Relations Review	2007	2
Dube, Lester, and Reich	The Review of Economics and Statistics	2010	2
Even and Macpherson	Southern Economic Journal	2014	6
Keil, Robertson, and Symons	Robert Day School Working Paper No. 2009-03	2009	7
Neumark, and Wascher	The American Economic Review	2000	8
Orazem, and Mattila	Journal of Labor Research	2002	9
Orrenius, and Zavodny	Industrial & Labor Relations Review	2008	10
Potter	Bureau of Bus. Econ. Rsrch/UNM	2006	11
Sabia	Journal of Labor Research	2009	12
Singell and Terborg	Economic Inquiry	2007	13
Zavodny	Labour Economics	2000	10

market, societal, and economic factors which are not pertinent to the discussion at hand.[11] In order to avoid these measures entering as a confounding issue here, other countries' estimates are discarded. The remaining studies are displayed in Table 1.

Results

The parameters of this model will be estimated using a Gibbs sampler with the Metropolis-Hastings algorithm. This process will be done by choosing prior distributions for μ and Σ_b and marginalizing over b. The resulting posterior distribution is then:

$$\pi(\Sigma_b, \mu|y) \propto \int_b f(y|\mu, b)P(b|\Sigma_b, jk)\pi(\mu|\Sigma_b)\pi(\Sigma_b)db$$

The specific prior distributions used in Table 1 are as follows:

$$\mu \sim \mathcal{N}(0, 1)$$

[11] For a discussion of the differential effects of the minimum wage across countries, see Neumark and Wascher (2004).

$$\sigma_{b_j} \sim \mathcal{IG}(3,1)$$

$$\sigma_{b_k} \sim \mathcal{IG}(3,1)$$

It is important note that the model is quite robust to informative priors over μ. Specifically, informative priors centered over elasticities of -0.100, 0, and 0.100 yield almost identical results to the prior used here. Estimates from models with different hyperpriors over σ_{b_j} and σ_{b_k} are also relatively robust. Furthermore, recall that our model can be written as:

$$b_j \sim \mathcal{N}(0, \sigma^2_{b_j})$$

$$b_k \sim \mathcal{N}(0, \sigma^2_{b_k})$$

$$\mu_{jk} = \mu + b_j + b_k$$

$$y_{ijk} \sim \mathcal{N}(\mu_{jk}, \sigma^2_y)$$

Our observed elasticities and their associated variances enter this model as μ_{jk} and σ^2_y, respectively.

For my first analysis, I present results of my MCMC estimation for employment outcomes with author group and population of interest clusters.[12] Table 2 shows the results from this estimation and Figures 1A and 1B show the kernel density and full diagnostics of our primary parameter of interest, μ. Note from Figure 1B that the parameters of this model still have high degrees of autocorrelation going out many lags and, as such, should be taken with a great deal of caution. Nevertheless, because this paper discusses these three important pieces to consider when estimating the effects of the minimum wage (author groups, dependent variable, population of interest) it is important to demonstrate my attempt to model all of these together, even if the attempt does not yield particularly informative results.

[12] All of the proceeding results are from a Gibbs sampler using 100,000 draws after a 10,000 draw burn-in. Tables 3 and 5 are quite robust to changes in the number of draws while Tables 2 and 4 are not. This is discussed in more detail in the proceeding section.

Figure 2.1A: Kernel Density of Elasticity of Employment with Respect to the Minimum Wage, Author Group and Population of Interest Clusters

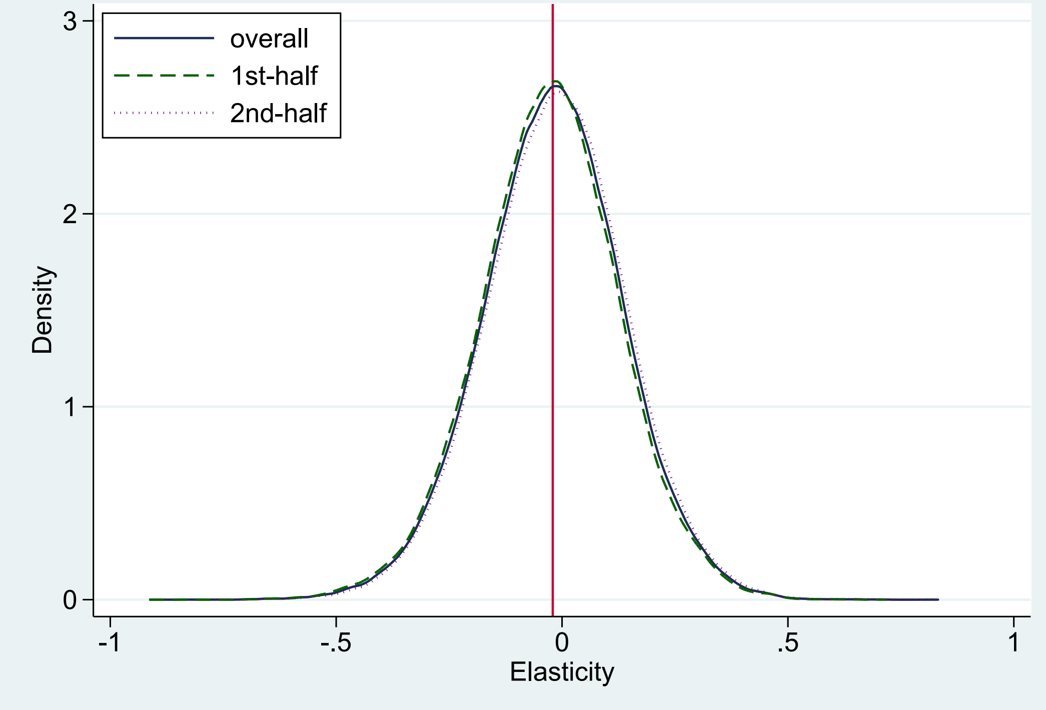


Figure 2.1B: Diagnostics of Elasticity of Employment with Respect to the Minimum Wage, Author Group and Population of Interest Clusters

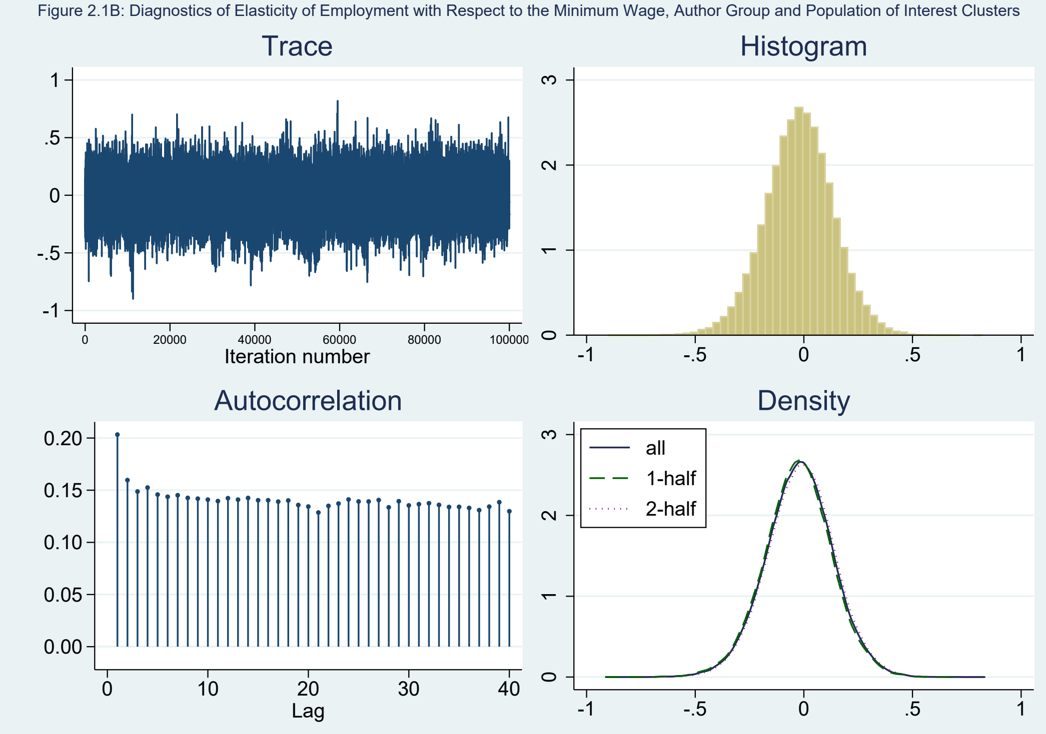


Table 3 does not suffer from such issues, as Figures 2A and 2B show. Table 3 performs an analysis over the same estimates as Table 2, but excludes the population of interest clusters. After doing so, we

Table 2.2: Bayesian Hierarchical Model, Only Employment Outcomes with Author Group and Population of Interest Clusters

Cluster	Mean	Std. Deviation	95% Cred. Int.
Addison, Blackburn, & Cotti	-0.040	0.174	[-0.419, 0.265]
Allegretto, Dube, Lester, Naidu, & Reich	-0.006	0.174	[-0.384, 0.300]
Bazen & Marimoutou	-0.045	0.176	[-0.425, 0.264]
Belman & Wolfson	0.017	0.272	[-0.528, 0.556]
Dodson	-0.090	0.273	[-0.635, 0.449]
Even & Macpherson	-0.059	0.174	[-0.439, 0.245]
Orazem & Mattila	-0.019	0.489	[-0.987, 0.951]
Potter	-0.017	0.488	[-0.982, 0.953]
Singell & Terborg	-0.027	0.175	[-0.406, 0.279]
Teenagers	-0.063	0.175	[-0.368, 0.316]
Retail	0.123	0.175	[-0.183, 0.502]
Restaurant	0.018	0.174	[-0.287, 0.398]
All Other	-0.023	0.272	[-0.562, 0.522]
Population Average	**-0.021**	**0.156**	**[-0.333, 0.285]**

see that there is a great deal of difference between results found by different author groups. For example, Addison, Blackburn, and Cotti find elasticities between employment and the minimum wage that are much more positive than any other group with their cluster centered around -0.004. With a standard deviation of 0.006, the lower tail of their 95 percent credibility interval lies above the corresponding upper tail of four other author groups.

Figure 2.2A: Kernel Density of Elasticity of Employment with Respect to the Minimum Wage, Author Group Clusters

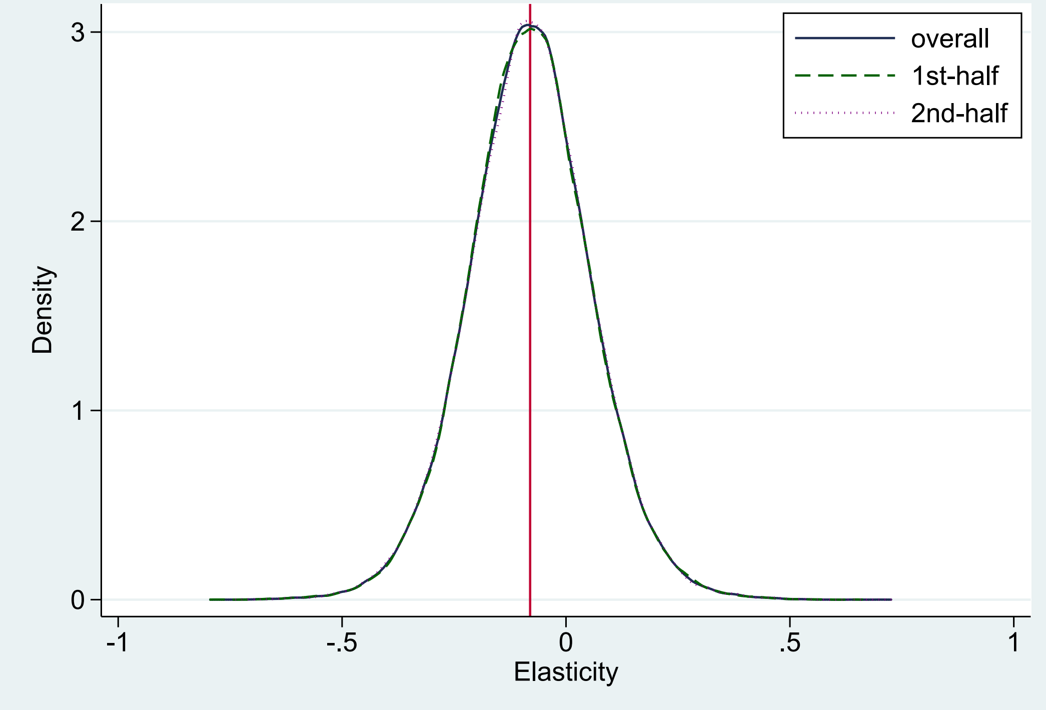
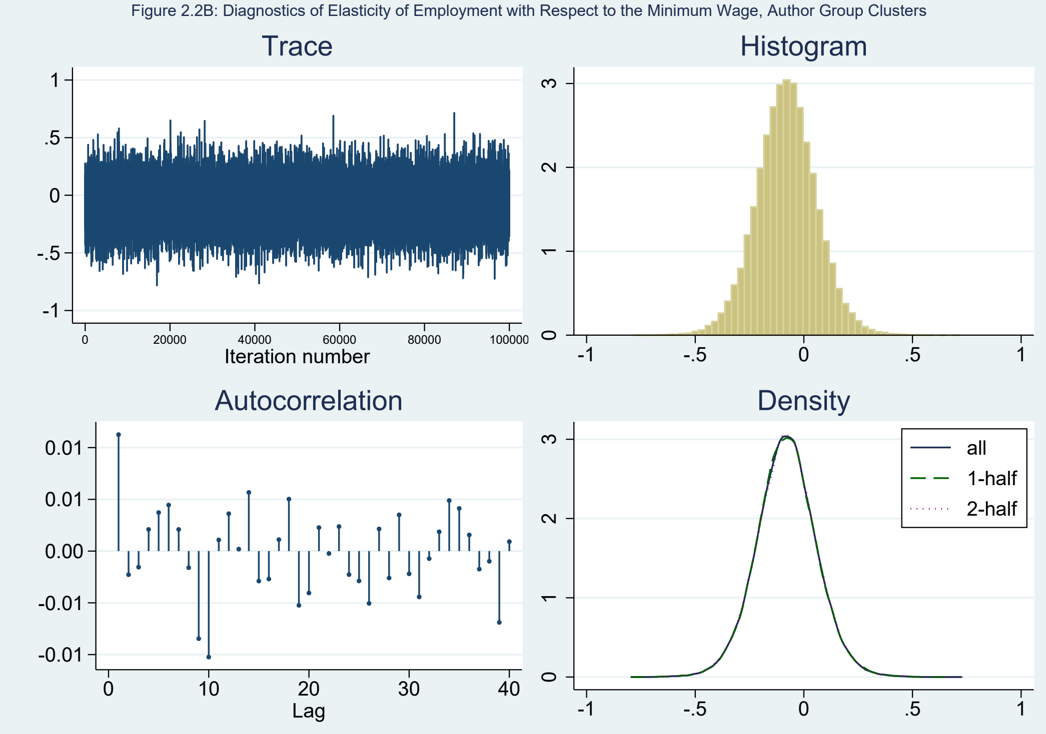


Figure 2.2B: Diagnostics of Elasticity of Employment with Respect to the Minimum Wage, Author Group Clusters



Clearly there is a marked degree of disparity in estimated results across these groups. Authors performing meta-analyses in the past have often pointed to such disparities as evidence of publication bias or specification

Table 2.3: Bayesian Hierarchical Model, Only Employment Outcomes with Author Group Clusters

Cluster	Mean	Std. Deviation	95% Cred. Int.
Addison, Blackburn, & Cotti	-0.004	0.006	[-0.016, 0.007]
Allegretto, Dube, Lester, Naidu, & Reich	-0.028	0.009	[-0.045, -0.011]
Bazen & Marimoutou	-0.109	0.021	[-0.150, -0.068]
Belman & Wolfson	-0.006	0.016	[-0.037, 0.025]
Dodson	-0.114	0.030	[-0.174, -0.055]
Even & Macpherson	-0.041	0.007	[-0.055, -0.028]
Orazem & Mattila	-0.330	0.065	[-0.456, -0.202]
Potter	-0.097	0.072	[-0.237, 0.043]
Singell & Terborg	-0.010	0.014	[-0.038, 0.018]
Population Average	**-0.082**	**0.137**	**[-0.357, 0.192]**

searching, contending that either editors and/or referees have a preference for negative and statistically significant results or authors are searching for such effects (or both). However, there are alternative possible explanations. It could be the case that negative statistically significant effects represent the true effect of the minimum wage on employment and the publication bias actually works in the opposite direction.

One way of visually examining this is a funnel plot, as discussed earlier in this paper. Yet such an analysis, or the quantitatively similar funnel asymmetry test, is flawed under the circumstances surrounding the minimum wage literature, specifically the use of control variables. Generally, a regression with only the minimum wage as an independent variable (and a constant term) would yield the most precise estimates of the minimum wage's effects, defined as the smallest standard error. As such, we might expect these regressions to be at the peak of the funnel plot which should be centered around the true elasticity of employment with respect to the minimum wage. At the same time, it is unlikely any researcher would contend such a model is well-specified. Controls including state or year fixed effects, education dummies, and race or age controls all have important reasons for being included in such estimates. However, the inclusion of these controls (and others) would certainly increase the standard errors and affect the point estimate of the minimum wage without these controls. Consequently, a funnel plot being asymmetrical or results being more positive or negative in and of itself does not demonstrate publication bias or specification searching. In a similar manner, my author group clusters showing more or less positive kernel densities from one another do demonstrate there are differences across author groups, but any differences could be the results of different empirical approaches measuring disparate outcomes across various populations of interest and nothing more.

To reiterate, I do not take a stand on these results but instead ask if we allow each author group to have a different distribution of results, driven by various methodologies or identification strategies, is there something useful to learn about the minimum wage generally? Indeed there is, although the answer certainly leaves the door open for further research. The overall elasticity between employment and the minimum wage has a density centered around -0.08, slightly more positive than the much discussed -0.10 benchmark, but negative nonetheless. As previously mentioned, one of the advantages of Bayesian methods is the ability to make probability statements that our elasticity of interest, μ, takes any range of values. Table 6 shows a number of these values for the analyses in Tables 2 and 3. Focusing on Table 3, my preferred specification, Table 6 shows that the probability the minimum wage has negative employment effects equals 0.731. This number is probably in line with a reasonable reading of the minimum wage literature: that is, the effects are most likely negative but the issue is not cut and dry. In a sense, this result is a microcosm of the minimum wage literature itself.

Beyond simply knowing the sign of the minimum wage's effects it is perhaps more important to know the magnitude of its effects, or in this case assign probabilities to a range of effect sizes. As the reflection of what was previously stated, the probability the minimum wage has positive employment effects equals 0.269. However, it is more likely that the minimum wage has large adverse employment effects. As Table 6 shows, the likelihood the minimum wage has "very negative" employment effects, defined as an elasticity more negative than -0.10 is 0.440. Thus, it is important to be cautious with large proposed minimum wage increases as can be seen across many parts of the United States as the effects are most likely negative and are more likely to be very negative than positive.

Moving on to estimations of the minimum wage's effect on hours, the focus turns towards Table 4 and Figures 3A and 3B. Note a similar autocorrelation issue when both author group and population of interest are included. Consequently, I put little stock into these results and do not discuss them beyond this brief mention.

Figure 2.3A: Kernel Density of Elasticity of Hours with Respect to the Minimum Wage, Author Group and Population of Interest Clusters

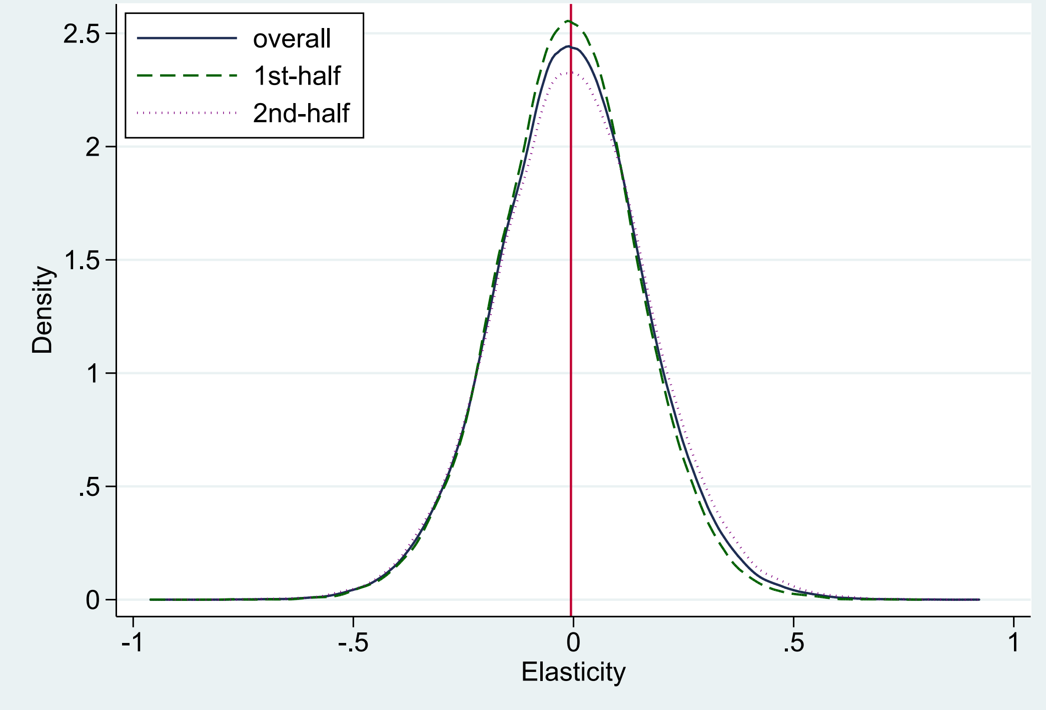
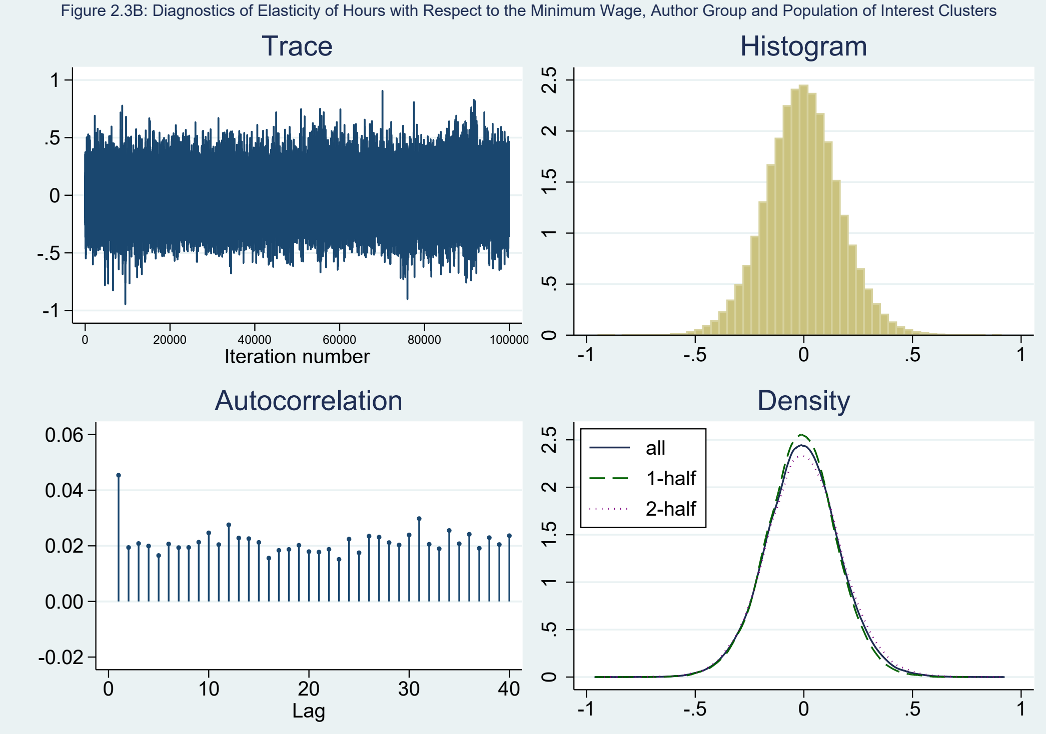


Figure 2.3B: Diagnostics of Elasticity of Hours with Respect to the Minimum Wage, Author Group and Population of Interest Clusters



Excluding population of interest clusters in Table 5 and Figures 4A and 4B solve the autocorrelation issue and allow a much more fulfilling analysis of the hours estimates. Here, the posterior distribution of

Table 2.4: Bayesian Hierarchical Model, Only Hours Outcomes with Author Group and Population of Interest Clusters

Cluster	Mean	Std. Deviation	95% Cred. Int.
Allegretto, Dube, Lester, Naidu, & Reich	0.038	0.160	[-0.254, 0.400]
Belman & Wolfson	0.114	0.162	[-0.186, 0.473]
Keil, Robertson, & Symons	-0.212	0.182	[-0.556, 0.166]
Orrenius & Zavodny	0.070	0.160	[-0.224, 0.431]
Sabia	0.085	0.160	[-0.207, 0.448]
Teenagers	-0.098	0.160	[-0.461, 0.194]
Retail	-0.072	0.160	[-0.434, 0.221]
Restaurant	0.091	0.180	[-0.285, 0.431]
All Other	-0.079	0.160	[-0.440, 0.214]
Population Average	**-0.006**	**0.169**	**[-0.341, 0.333]**

the elasticity between hours and the minimum wage is centered around -0.032 with a very large 95 percent confidence interval. Similarly to the employment results, there are notable differences across author groups with respect to their findings. Again, it is not necessarily the case that these differences imply publication bias or specification searching. With these differences across author groups accounted for, the probability the minimum wage has negative effects on hours is 0.564 and, therefore, there is a 0.436 probability the effects are positive. These hours results paint a much less clear picture of the minimum wage's effects but given the minimum wage could affect the two outcomes in very different ways this result is not surprising.

Figure 2.4A: Kernel Density of Elasticity of Hours with Respect to the Minimum Wage, Author Group Clusters

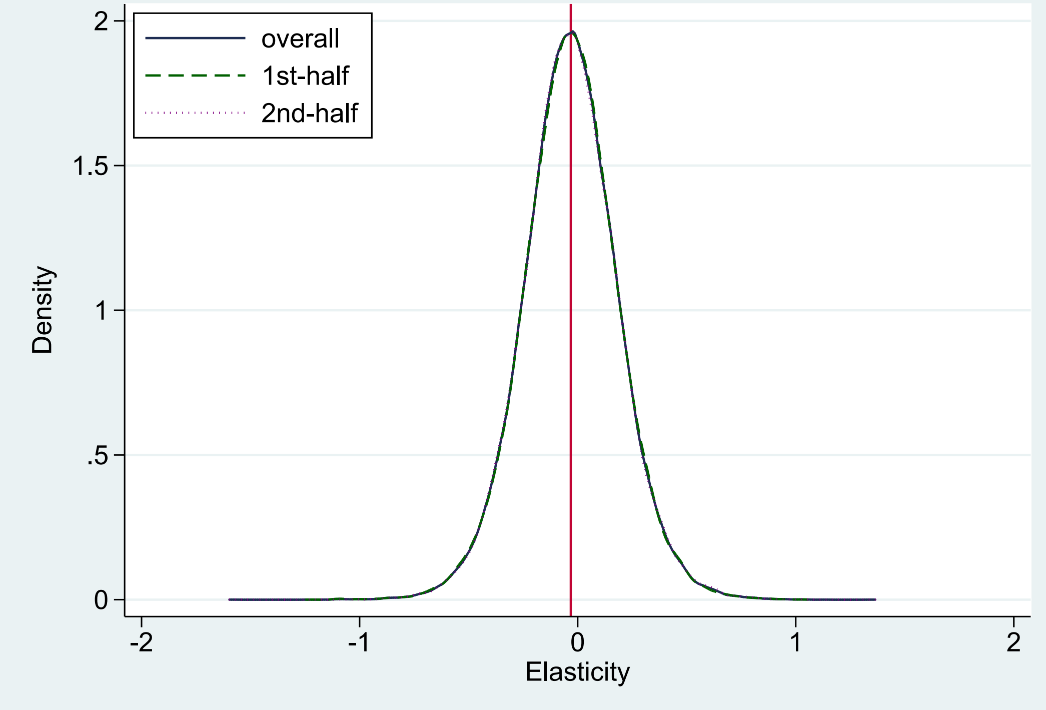


Figure 2.4B: Diagnostics of Elasticity of Hours with Respect to the Minimum Wage, Author Group Clusters

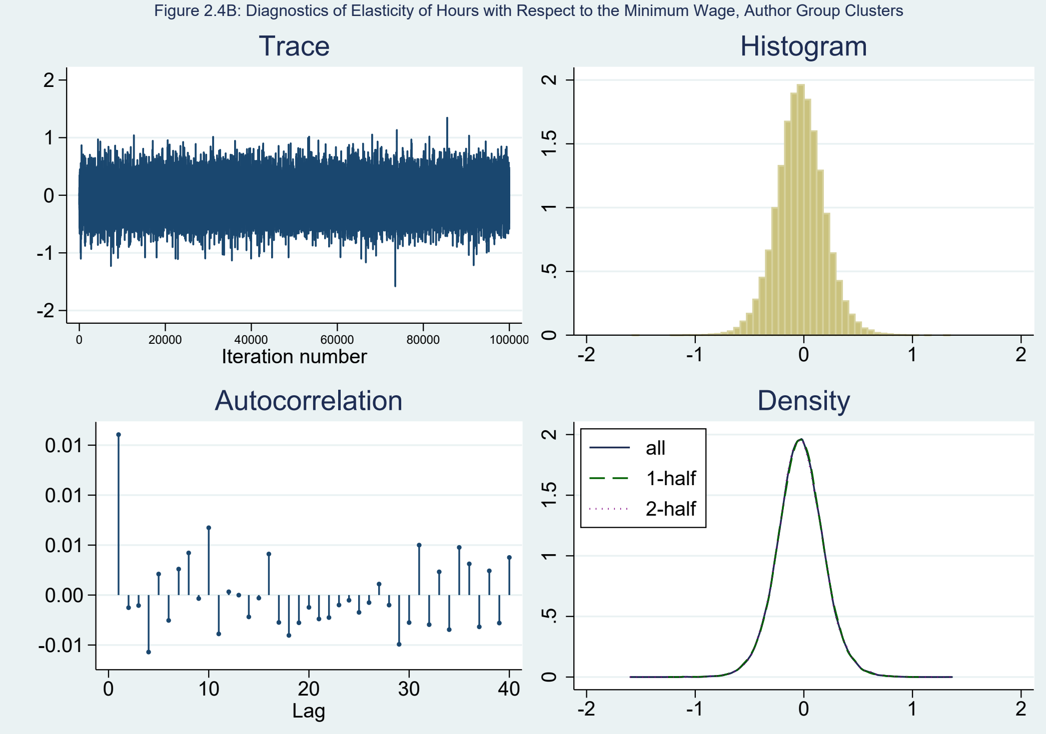


Table 2.5: Bayesian Hierarchical Model, Only Hours Outcomes with Author Group Clusters

Cluster	Mean	Std. Deviation	95% Cred. Int.
Allegretto, Dube, Lester, Naidu, & Reich	-0.056	0.007	[-0.069, -0.043]
Belman & Wolfson	0.035	0.030	[-0.024, 0.094]
Keil, Robertson, & Symons	-0.121	0.027	[-0.173, -0.069]
Orrenius & Zavodny	-0.014	0.010	[-0.033, 0.005]
Sabia	-0.012	0.003	[-0.017, -0.006]
Population Average	**-0.032**	**0.217**	**[-0.462, 0.404]**

Table 2.6: Bayesian Hierarchical Model, Employment Elasticity Range Probabilities

Interval	Spec. 1	Spec. 2
$\mu < -0.100$	0.295	0.440
$\mu < -0.050$	0.417	0.592
$\mu < -0.025$	0.583	0.665
$\mu < 0$	0.550	0.731

Table 2.7: Bayesian Hierarchical Model, Hours Elasticity Range Probabilities

Interval	Spec. 1	Spec. 2
$\mu < -0.100$	0.282	0.369
$\mu < -0.050$	0.394	0.465
$\mu < -0.025$	0.455	0.515
$\mu < 0$	0.516	0.564

Conclusion

The minimum wage is one of the most frequently studied and hotly debated policies in applied microeconomics, particularly with regards to its effects on the labor market outcomes of restaurant workers, teenagers, and other affected groups. In the new minimum wage research born out of the debate between Neumark and Wascher (2000) and Card and Krueger (2000) many studies have focused on the selection of a proper counterfactual group and other issues across methodologies. This paper uses 18 studies and 408 estimates of the minimum wage's effects in an empirical Bayesian Hierarchical meta-analysis to try quantifying and categorizing the minimum wage's effects on employment and hours.

Overall, it is most likely that the minimum wage does indeed have adverse employment effects, with the posterior from the preferred specification centered around an elasticity of -0.08. This is slightly less negative but qualitatively similar to the conclusion of Neumark and Wascher (2007) who perform a more qualitative analysis of the literature, although that study is a bit too old to contain some of the more recent studies used here. One of the advantages when using Bayesian techniques is that it allows probability distributions to be assigned any range of values. Of note here is the likelihood that the minimum wage's effects are negative (probability=0.731) and the likelihood that the elasticity between employment and the minimum wage is more negative than -0.10 (probability=0.440). Notice that this implies it is more likely the effects of the minimum wage are very negative than that they are positive.

Further avenues of research would include new and forthcoming results on the minimum wage's employment effects, both to increase the scope of this analysis but also to allow more levels of clustering. Additionally, I leave as an open-ended question how (if even possible) a researcher might distinguish between or give greater weight to different methodologies or identification strategies in estimating the minimum wage's effects on labor market outcomes.

Chapter 3: Introduction and Motivation

The Earned Income Tax Credit is one of the largest anti-poverty programs in the United States,[1] allocating over $67 billion to more than 27 million families for tax year 2016.[2] The EITC has clear positive extensive margin labor market incentives for single women with children who are not working, and many traditional studies of the EITC have focused primarily on single mothers (e.g. Eissa and Liebman (1996), Meyer and Rosenbaum (2001)). However, research on the EITC is not limited to single mothers or extensive margin labor market impacts. Indeed, the EITC's theoretical incentives are quite broad, including negative intensive[3] margin labor market effects for women in dual-earner households (Eissa and Hoynes (2004)), fertility incentives (Baughman and Dickert-Conlin (2009)), and impacts on marriage decisions (Eissa and Hoynes (1999)).

I exploit a discontinuity in EITC payments for women who give birth to their first child around the end of the calendar year to estimate its labor market effects in the 12 months following the child's birth. My results show strong positive employment effects for low-ed unmarried women. These results are rather robust across a number of different specifications and definitions of employment. Additionally, I find some moderately less robust evidence of negative employment effects for married women. Such declines in employment for these women are theoretically possible, but no strong evidence for such a response has been presented in the EITC evidence to date. I argue that this may be due to these previously mentioned endogeneity issues, but as my sample is for a rather specific group of women I am hesitant to make broader generalizations.

Single mothers are frequently studied by researchers for two primary reasons. First, single mothers with children are the largest group benefiting from the EITC, accounting for 31 percent of recipients and 41 percent of payments (Meyer (2008)). Second, estimating labor market outcomes for single mothers is a straightforward and parsimonious way of selecting a group of individuals relatively more likely to be affected by the positive extensive margin labor market effects of the EITC. Married mothers, in contrast, are more likely to be in a dual-earner household and to be the secondary earner. Such women likely face the negative intensive margin effects of the EITC which result from large effective marginal tax rates for earners in the

[1]Only Medicaid, Food Stamps, and Social Security spend larger amounts.
[2]Source: eitc.irs.gov/Partner-Toolkit/basicmaterials/ff
[3]Depending on the model of labor supply negative extensive margin labor market effects are also possible.

phase-out region;[4] in some cases, these effective marginal tax rates can be close to 50 percent. In order to avoid muddling these effects, estimates are often performed separately for married and unmarried women. Additionally, single mothers have lower education levels and decreased labor force participation compared to married mothers, again increasing their relative likelihood of being affected by the extensive margin effects of the EITC.

Marital status is a key component in traditional analyses of the EITC for these reasons. Also important are whether a woman has children and her level of education. Because number of children is a primary determinant of EITC eligibility and the credit available for individuals without children is very small, a simple way of dividing a sample of women into recipients and non-recipients is to assign mothers as recipients and women without children as non-recipients. Earned income is the other primary determinant of EITC eligibility, but directly using earnings to assign EITC eligibility is problematic, as a mother may choose her earnings endogenously with respect to the EITC. To avoid this issue, education level is often used as a proxy for eligibility. Mothers with low levels of education are assumed eligible for the EITC and women with higher levels of education are assumed ineligible or less likely to be eligible.

Such estimation strategies assume, at least implicitly, that the categories used to determine EITC eligibility (having children, marital status, education) are not endogenous to the policy itself. In a differences-in-differences or generalized DiD identification strategy, this refers to the assumption that there are no compositional changes as a result of the treatment. If women respond through these channels in response to the EITC, estimates of its labor market effects could be biased.[5] The approach in this paper avoids these endogeneity issues by using a fundamentally different source of identifying variation: an information shock induced by birth timing around the end of the calendar year. EITC eligibility (and other Internal Revenue Service tax rules) state that children born during a calendar year are eligible to be claimed during that year's tax return. Consequently, a woman who gives birth on December 31st can claim her child on that year's tax return, but a mother who gives birth a single day later on the 1st of January must wait a full calendar year before receiving the EITC and other tax-related benefits.

There are a few notable differences between the approach in this paper and in traditional EITC studies.

[4]For context, two spouses working at the U.S. federal minimum wage of $7.25 per hour would reach the phase-out region if they each work approximately 25 hours per week.

[5]I discuss the evidence for such responses in detail in the EITC Literature section.

First, the Local Average Treatment Effect (LATE) identified by the regression discontinuity design employed here is a causal estimate of the EITC's average effect on compliers. Here, I assume perfect compliance and utilize a sharp regression discontinuity design. This is due to the fact that all individuals who had children in the latter half of the calendar year and filed a tax return are eligible for the EITC, provided their income lies under the end of the phase-out region ($39,617 for one child in 2017). However, take-up of the EITC is not guaranteed; a taxpayer must claim the EITC on their tax return. Although tax softwares and tax professionals will claim the credit on behalf of an individual or family if they are eligible, a taxpayer filing on their own behalf may not know to claim the credit. This is typically not the case, however, as historical take-up of the EITC conditional on eligibility is quite high, with Scholz (1994) and the IRS (2002) estimating that 80-87 percent of eligible households receive the credit. Furthermore, some individuals or families will earn too much to be eligible for the EITC, but I do not condition on earnings as it is endogenous to the EITC.

Additionally, the sample used here is restricted to mothers whose labor market outcomes are observed in the twelve months following the birth of their first child. Other EITC studies do not make such restrictions; at most they may control for having children above or below schooling age. Consequently, this paper contains a caveat: although I obtain arguably causal estimates of the EITC's labor market effects, the population for which I do is not representative of all mothers. Even so, this is a sample all mothers belong to at some point during their childbearing history.

There are two potential mechanisms to which a mother with a newborn child may respond under this identification strategy. The first, which I argue is the primary mechanism driving my results, is an information acquisition after receiving the EITC for the first time.[6] Essentially, I argue that women, and particularly single mothers with low levels of education, are unaware of the EITC before initially receiving it.[7] As a result, women who give birth in the latter half of the year receive the EITC as part of their tax return in the beginning of the following year, learn about the policy, and respond to its incentives. Women who give birth in the first half of the following calendar year are not eligible for the credit on the previous year's taxes. As

[6]Technically, from 1994 onwards there is a small childless EITC. However, due to its small size both in absolute terms and relative to the one-child EITC, as well as the much smaller income range over which it can be claimed, I ignore this zero-child credit.

[7]I detail the evidence for this assumption in the Identification and Methodology Section.

such, this identification strategy captures both knowledge effects of receiving the EITC for the first time and the income effect of the credit.

The knowledge effects of the EITC impact married and unmarried women in generally opposite directions. Unmarried women who receive the EITC for the first time discover that working entitles them to the credit; this discovery therefore encourages labor force participation following the birth of their child. Married women, conversely, are more likely to be in a dual-earner household, so upon learning that the EITC is awarded at the household level, they are discouraged from returning to work or encouraged to reduce their hours. These incentives are similar to those acting on mothers in traditional studies of the EITC but there is one notable exception. Here, both treatment and control groups are eligible for the EITC in the year following the birth of their children. However, the treatment group responds (or is more likely to respond) to the EITC's incentives because they have already received it once.

The income effect of the EITC on a mother with a newborn child is more complicated, and the hypothesized direction of this effect is not immediately obvious. On one hand, a mother may view the EITC as a substitute for maternity leave, allowing her to forego returning to work, at least for a period of time. On the other hand, a mother may use the credit as a subsidy to child care, enabling her to return to work when other child care methods (e.g. leaving the infant with a grandparent or another friend or family member) are unavailable. As a result, the income effect of the EITC is theoretically ambiguous with respect to the mother's labor supply.

My assertion that informational gain and not income effects are driving my estimated labor market effects is a testable hypothesis using the SIPP. By examining women who have their second child, I can examine a group of mothers exposed to the income effect but not the information shock. In doing so, I demonstrate that my results are indeed primarily driven by this information acquisition encouraging low-ed unmarried women to work. Additionally, there is some evidence low-ed married women are may be discouraged from working upon initial EITC receipt. This is not inconsistent with Chetty and Saez (2013) who use a field experiment where tax preparers at H&R Block were randomly assigned whether to go over the EITC schedule in detail with clients. The hypothesized mechanism in both their paper and mine is the same in that individuals may change their behavior in response to learning about the EITC. While Chetty and Saez do not find statistically significant changes in earnings as a result of this information intervention on the panel who came in two years in a row for tax preparation, this finding is not at odds with mine. If these households and single mothers learned about the EITC upon initial receipt, the information gain from their experiment was minimal.

EITC History

Over the course of the 1960s and 70s, a plethora of government programs were implemented targeting low-income Americans. Some were born out of President Lyndon B. Johnson's War on Poverty including the Food Stamp Act of 1964 and the Social Security Act of 1965, which created Medicare and Medicaid. This trend continued into the late 1960s with President Richard Nixon advocated for a program called the Family Assistance Plan (FAP) which was, at its core, a modest Negative Income Tax. The primary advantage of a NIT comes from the reduction in bureaucracy; by eliminating other anti-poverty programs, each with its own set of eligibility criteria, paperwork, and overhead, a NIT would allow for a simplified and streamlined process of providing assistance to needy families. In practice, a NIT would look quite similar to the contemporary tax system in the U.S. with one major exception: low-income individuals would receive subsidies from the government in lieu of paying any taxes. At no income, an individual or household would receive some maximum amount. As income increases, the benefit declines at some rate slower than the income gain until the individual or household breaks even. As income further increases, they now pay tax on each dollar earned.

Critics of the Negative Income Tax will quickly point out that such a plan creates strong work disincentives for low-income individuals. Individuals (or families) who don't work at all receive the largest subsidy, while others may choose to work but reduce their hours due to a (potentially) large effective marginal tax rate. Low phase-out rates would dampen these work disincentives, but would increase the fraction of the population receiving subsidies in lieu of paying taxes, driving up the cost of the program. The specific program proposed by President Nixon would have provided a family of four without any income \$1,600 (~\$10,500 in 2017 dollars). This amount would decline as the family's income increased until it completely phased-out at \$3,920 (~\$26,000 in 2017 dollars). Additionally, the plan required families to have children and featured work requirements for "employable" householders. Although this plan passed the House of Representatives, it failed in the Senate.

In opposition to the FAP, Russel Long, Chair of the Senate Finance Committee, proposed what would become the Earned Income Tax Credit. The EITC was temporarily enacted under the Ford Administration as a part of the Tax Reduction Act of 1975. It was made permanent in 1978 and has since received several major expansions, most notably from the Tax Reform Act of 1986 and the much larger expansions passed as part of the Omnibus Budget Reconciliation Act of 1993. This later expansion came following President Clinton's first State of the Union[8] where he declared, "..the new direction I propose will make this solemn simple commitment. By expanding the refundable Earned Income Tax Credit we will make history, we will reward the work of millions of working poor Americans by realizing the principle that if you work 40 hours a week and you've got a child in the house, you will no longer be in poverty." Although a modest supplement to low-income families with children at first, as a result of this expansion the EITC grew into one of the United States' largest anti-poverty programs.

Although the specific parameters of the EITC have evolved over time, the basic formula has remained constant. The EITC is designed to encourage work for the lowest earners so, in stark contrast to a NIT, an individual or household with no income does not receive the EITC. As income increases from zero, for every dollar an individual or household earns, they receive an additional fraction of a dollar in credit. The specific amount they receive is commonly referred to as the phase-in rate. At a certain income level, the maximum credit is reached and a household enters the plateau region where the EITC is flat with respect to income. Finally, the credit declines over the phase-out region where a household loses a fraction of a dollar in credit for each additional dollar of income they earn. These phase-in and phase-out rates as well as the maximum credit vary with the number of children in the household.[9] The full EITC schedule for for a single taxpayer for tax year 2017 is shown in Figure 1.

As previously mentioned, the OBRA 1993 was the single largest expansion of the EITC, increasing the phase-in rate for one child (two children) from 14 (14) percent in 1990 to 34 (40) percent in 1996. Other

[8] Technically, it was only an address before a joint session as President Clinton had not yet been in office a full year when delivering the address.

[9] Additionally, the income level at which the EITC begins to phase-out depends on whether the tax unit is an individual of a married couple. Specifically, married filers see the EITC begin to phase-out at a higher income level.

notable EITC changes include the introduction of a modest zero-child credit with a phase in rate of 7.65 percent in 1994 (also part of the OBRA 1993) and the addition of a three or more children credit in 2009 which phases-in at 45 percent. Starting in 1986, the maximum available credit was also indexed to inflation. Combined with the earlier EITC expansions the maximum credit for two children rose from $1,235 in 1991 to $5,616 in 2017. Given that the minimum income to receive the maximum credit was $14,040 in 2017, the EITC represents a massive subsidy to low-income families. In addition to expansions by the federal government, 25 states (including the District of Columbia) have state supplements to the EITC as of 2017. In almost every case,[10] the state supplements offer tax filers a flat percentage of the household's federal EITC.[11]

EITC Literature

Two of the most oft-cited papers in the EITC literature are Eissa and Liebmann (1996) and Meyer and Rosenbaum (2001), which both utilize the Current Population Survey to estimate extensive margin employment effects for women across different fertility, marital, and education categories. Eissa and Liebmann use variation from the first major expansion of the EITC, included in the previously mentioned TRA 86, in a simple differences-in-differences framework to estimate employment effects for three groups of women: all women with children, women with less than a high school degree and children, and women with a high school degree and children.[12] They find generally strong positive extensive margin effects on employment for these women, especially the least-educated. These responses are rather robust across their different choices for potential control groups. Meyer and Rosenbaum (2001) also examine the extensive margin employment effects of the EITC, extending the analysis to cover 1984 to 1996 which includes both the TRA86 EITC expansion and the larger expansions of the early 1990s.

The underlying source of identifying variation in these and other[13] traditional EITC studies comes from

[10]Wisconsin, the lone exception, awards a different percent of the federal credit based on number of children. As of 2017, these percentages were 4, 11, and 34 for one, two, and three or more children, respectively.

[11]A typical state supplement in 2017 gives a recipient 15 to 20 percent of their federal credit.

[12]For all women with children, Eissa and Liebmann use all women without children as a control group. For women with children without a high school degree, the control groups are women with less than a high school degree and no children as well as women with beyond a high school degree and children. Finally, women with a high school degree and children are compared to women with a high school degree and no children and women with beyond a high school degree and children.

[13]See Meyer (2008) for a review of the EITC literature.

comparison across groups that are relatively more or less likely to be eligible for the EITC. Of the variety of potential ways to determine eligibility, having children, marital status, and education level are the most common.[14] Using these criteria to define treatment and control groups to implement a differences-in-differences estimation strategy relies upon two assumptions: parallel trends and no compositional changes due to treatment. The parallel trends assumptions requires that, in the absence of the EITC, the comparison groups would experience parallel trends in employment (or another labor market outcome). Unfortunately, it is often impossible to test his assumption directly due to the lack of counterfactual observations for the treated group. Instead, if sufficient pre-period data is available, researchers may compare outcomes for both the treatment and control groups before treatment occurs to test whether the trends are statistically different. Of course, this cannot prove that the parallel trends assumption holds, but it can identify if it is violated. I explicitly test this assumption using earnings and employment data available from 1967 through 1985 through the Panel Study of Income Dynamics. This replication work follows and extends directly from Neumark and Shirley (2018).

I consider the same treatment and control groups as defined by Eissa and Liebman (1996). Figures 2A to 2F show earnings and employment trends for these groups over the 1967 to 1985 time period.[15] Of the trends shown in these figures, only one set are statistically different from each other, specifically, the earnings trends for all women with and without children. This treatment and control group combination is, of course, the least likely group among those chosen by Eissa and Liebman to satisfy this assumption. The only other set of trends that may be considered worrisome (p-value=0.164) compare women with a high school degree who do or do not have children. The treated group has a clear downward trend from 1967 to 1985, while the childless women have an upward, albeit much noisier, trend. Overall, the evidence is relatively convincing that the pre-trends in earnings and employment across the selected groups are quite similar, especially for the employment outcomes which Eissa and Liebman actually study. With that being said, a natural continuation of this discussion asks whether the PSID reliably replicates the findings of Eissa and Liebman (1996) and Meyer and Rosenbaum (2001).

The short answer is that yes, by and large, the PSID does a reasonable job replicating these traditional

[14]Marital status is also used because of the different extensive and intensive margin effects that are likely to be different for unmarried and married women.

[15]Employment is defined as earnings of at least $2,500 in the previous year.

EITC studies. The primary downside of the PSID, its small sample sizes, is unfortunately noticeable in the standard errors of the replication estimates. However, this is a necessary concession in order to obtain nearly two decades' worth of pre-treatment outcome data, which was necessary for the pre-trends analysis.[16] Tables 1 and 2 show replications of Table 1 from Eissa and Liebman (1996) and Table III from Meyer and Rosenbaum (2001), respectively, using the PSID. The most notable difference between the two sets of estimates is in Table 1 from the comparison of women without a high school degree who do or do not have children. Eissa and Liebman find a moderately large DiD employment effect across these two groups, while the replication finds a large negative employment effect, albeit with an almost equally large standard error. The replication results in Table 2 are in many ways stronger than the original Meyer and Rosenbaum results but, again, the ability to precisely estimate these effects is hampered by sample size issues. Overall, the pattern noted by Meyer and Rosenbaum-that the timing of EITC expansions is associated with an increase in employment rates of women with children relative to those without children-is visible in the replication using the PSID.

As previously mentioned, the two fundamental DiD assumptions require parallel trends for the treatment and control groups in the absence of the treatment and that there is no compositional change in the two groups as a result of the treatment. This paper has demonstrated that there is little evidence that the parallel trends assumption does not hold in most of the outcome and treatment/control group combinations frequently studied using data from the PSID, and that this same data replicates the results of earlier studies with a reasonable degree of precision. However, for the DiD estimation strategy to provide unbiased estimates, it is also necessary that no compositional changes in the treatment and control groups occur as a result of the treatment. In the context of the EITC, this assumption requires that women's fertility, marital, and educational decisions are not influenced by the EITC.

If, for example, the EITC discourages some women from marrying, the only scenario under which EITC payments are strictly decreasing requires the mother to already be working. Consequently, the estimated labor market impacts of the EITC for unmarried women will be biased upwards as working women who otherwise be married in the absence of the EITC are now still unmarried. In a similar fashion, if the EITC

[16] This pre-treatment outcome data is also why I cannot use the SIPP for this analysis.

encourages childbearing, it can only do so for single women who are already working (as you must work to receive the credit), again biasing the EITC's labor market impacts for single unmarried women upwards. The proceeding paragraphs discuss the empirical evidence for these responses.

Generally, the amount of EITC an individual or family is eligible for increases with the number of children they have, with the largest jump from zero children to one child. Given the pro-fertility incentives from subsidizing childbearing, the question is whether women respond to the EITC through increased childbearing and, if so, to what degree. Baughman and Dickert-Conlin (2003, 2009) directly explore the relationship between the EITC and fertility, finding mixed evidence. In their baseline specification, Baughman and Dickert-Conlin find small reductions in higher-order fertility for white women as well as consistently large and positive fertility effects of state child tax (or child care) credits. Duchovny (2001) also finds some evidence of increases in fertility for married white women and unmarried nonwhite women. Overall, there is some degree of suggestive evidence that the EITC influences fertility decisions. Alone, this may not be of great concern when estimating the effects of the EITC. However, Baughman and Dickert-Conlin note that the results they find when estimating their model separately for married and unmarried women may be driven by endogenous marriage decisions in response to the EITC.

Depending on the earnings of two individuals considering marriage, the EITC may act as either a subsidy or penalty. Under a scenario where an individual with children and is not working considers marrying a childless working individual, the EITC acts as a marriage subsidy if the working individual's income would qualify for the EITC and is marriage-neutral if that income lies beyond the phase-out region. Alternatively, if both individuals work, the EITC will, in most cases, act as a marriage penalty. If the two individuals' combined incomes reach the phase-out region ($18,340 for one or more children in 2017[17]), then the individual with children receives less from the EITC than what they would filing their taxes separately. If both individuals have children the incentives are slightly more complicated, but again the EITC will generally act as a subsidy if one parent is working and a penalty if both are working.

In addition to subsidizing and penalizing marriage itself, married and unmarried women, on average, face fundamentally different incentives in response to the EITC. A primary objective of the EITC is to encourage

[17] This income could be reached by two individuals each working about 25 hours per week at the federal minimum wage.

work and subsidize earnings for low-income workers with children, and single mothers drive this response. A single mother who is not working would view the EITC as a large wage supplement, greatly increasing their effective wage and encouraging entry into the labor force. On the other hand, married women with a working spouse experience very large effective marginal tax rates if the combined dual income reaches the phase-out region, in some cases as high as 50 percent. Taking marital decisions as exogenous to the EITC allows for exploration of differential EITC effects across this dimension, which is particularly interesting as theory would predict married and unmarried women respond quite differently. The general consensus that the EITC increased labor force participation for single women with children (Eissa and Liebman (1996); Meyer and Rosenbaum (2000, 2001); Neumark and Wascher (2011)) and had negative intensive margin effects for married women (Eissa and Hoynes (2004), Meyer (2010)) displays that these theoretical responses are borne out empirically.

The intersection of these two forces-that the EITC may reward or penalize marriage and the differences in labor market responses for married and unmarried women-is a potential issue for identifying the labor market effects of the EITC. Both Eissa and Hoynes (1999) and Herbst (2011) investigate this relationship and find that although such endogenous response does occur, the effects are relatively modest and not of an economically meaningful magnitude. Ellwood (2000) and Dickert-Conlin and Houser (2002) also explore the EITC's marriage incentives, but find little effect of a response. More recently, however, Michelmore (2016) uses the Survey of Income and Program Participation and finds that the average EITC-eligible woman would lose approximately $1,300 in EITC benefits in the year following their marriage. Additionally, Michelmore finds that, relative to single mothers who expect no change or to gain EITC benefits from marriage, single mothers who expect to lose EITC benefits from marriage are less likely to marry and more likely to cohabitate. As with fertility responses, there is some evidence of endogenous marriage responses to the EITC. In light of this, care should be exercised in identifying and estimating labor market effects of the EITC.

Finally, the potential relationship between the EITC and the educational attainment of its recipients is interesting to note as it seemingly influences mothers in both directions. For example, if mothers are credit constrained by the costs of going back to school through tuition and fees, foregone earnings, increases in child care costs, or any combination of these, then the EITC could help to offset those costs and encourage these women to enroll in further education. Women may be able to reduce their hours worked, freeing up time for classes, but simultaneously not decrease their total income. On the other hand, the EITC acting as a wage subsidy decreases the returns to schooling. Depending on the relative size of these competing incentives, the EITC's effects on mothers' education could be positive, negative, or zero. In contrast to the fertility and marriage incentives, no work to my knowledge exists exploring this relationship.[18] Because the theoretical effects are unclear, and because there is a lack of empirical evidence, I leave this as an open-ended question and possibly the subject of future research.

The literature on the EITC to date examines its effects on numerous labor market outcomes across a variety of groups most likely to receive its benefits. Almost all of these studies use observable characteristics including having children, marital status, and education level to identify treatment and control groups. However, these categories are all theoretically endogenous to the EITC and there is some evidence to suggest that women do respond to the EITC along these dimensions. In order to best estimate the effects of the EITC, I use a different source of identifying variation and find results broadly consistent with the rest of the literature.

Identification and Methodology

Over 99 percent of EITC recipients receive the credit as part of an individual's (or family's) tax return (Holt (2009)), and eligibility rules state any child born during the calendar year is able to be claimed on that year's tax return. For example, a child born on December 31st, 2017 could be claimed on their parents' tax return for 2017 (filed early in 2018), but a child born a single day later on January 1st, 2018 is ineligible to be claimed on the parents' 2017 tax return; such a child could not be claimed until the parents file taxes the following year (in early 2019). Because the difference in maximum zero child and one child federal credits was $2,890 in 2017,[19] this single day difference in birth timing represents a tremendous potential income shock. In fact, the lowest eligible income for the maximum one-child credit in 2017 was $10,000; at that income, the difference between the zero-child and one-child credits was $3,017, or over 30 percent of the

[18]There is work estimating the educational effects for children of EITC recipients. See Bastian and Michelmore (2015) and Manoli and Turner (2014).

[19]Depending on the mother or household's earned income, this difference could be as large as the maximum one child credit ($3,400 in 2017).

household's yearly earned income.

This is the first paper to exploit arguably exogenous variation in EITC payments using end-of-year births, but this is not the only attempt at obtaining causal estimates of the EITC's effects. Jones (2013) attempts to exploit another discontinuity in the EITC, specifically the kinks in the EITC schedule from phase-in to plateau and plateau to phase-out. Using a regression kink design, Jones estimates that there is a statistically significant but economically small decrease in hours for women with more than one child who face a high effective marginal tax rate due to being on the phase-out region of the EITC schedule. She finds no effects at the kink from the phase-in to plateau region. Although Jones estimates causal impacts of the EITC, the groups for which these effects are identified are the mothers whose incomes lie around the two kinks in the EITC schedule and, as such, only intensive margin effects can be identified. Unfortunately, the extensive margin employment effects for single mothers cannot be estimated using such an RKD strategy, but the regression discontinuity design employed here does allow such an analysis, albeit only for new mothers.

That is not to say this paper is free from its own identification threats. The single greatest threat to this paper's identification strategy relates to whether EITC-eligible women manipulate their births to take advantage of these policies. If women are aware of the tax benefits associated with having a child before the end of the calendar year, including the EITC, and they can (at least to some extent) manipulate when they have their child, then the validity of the regression discontinuity assumptions are called into question. Without some form of medical intervention it would be extremely difficult, and most likely impossible, to manipulate a birth with any degree of precision A typical gestation period is approximately 280 days (40 weeks), with 80 percent of births occur between 37 and 41 weeks (Hoffman et. al (2008)). In addition to greatly varying gestation periods, women's ovulation cycles pose an additional challenge for a woman attempting to manipulate her birth. Specifically, women are typically only fertile for approximately 24 to 48 hours during each menstrual cycle. Issues including variation in menstrual cycle length and ovulation timing differences across cycles can further complicate precise timing of a pregnancy (Leridon (1977)). As a result, the only reliable way for a woman to precisely manipulate when she has her child requires medical intervention, primarily through a cesarean section or labor induction.

This birth manipulation issue has been explored explicitly, most notably by Dickert-Conlin and Chandra

(1999) and LaLumia et. al (2015). Both papers seek to answer whether women purposefully shift their birth from early January to late December to take advantage of tax incentives and, if so, to uncover the degree to which this occurs. Dickert-Conlin and Chandra use the NLSY and find substantial tax incentive effects on the probability of a December birth, but LaLumia et. al reach a more nuanced conclusion utilizing the universe of tax returns from 2001 to 2010. LaLumia et. al do indeed find evidence of movements in birth timing, but these effects are quite small for low-income women who are recipients of the EITC. They argue two primary reasons for this: first, many of these women are largely unaware of the benefits involved in timing their births before the end of the calendar year. Second, many of these women are likely insured through Medicaid and, as a result, have less agency over manipulating their birth.

In addition to the evidence provided by LaLumia et al. (2015), I use natality data from the National Vital Statistics System to analyze birth data over a number of different dimensions allowing a categorization of the degree to which women seemingly manipulate the timing of their births. The natural starting point for this analysis is births by day of the year, shown in Figure 3A. Note that the date range for this figure covers 1969 to 1988, which will not overlap with my eventual SIPP sample. In 1989 the NVSS switched from providing day of the year for births to providing month of year and day of the week. This hinders my ability to measure how births by day of the year evolved over the time period with the largest EITC reforms. Nonetheless, even before these large EITC reforms, there is evidence of manipulation around the end of the year.

Figure 3B zooms in on the 20-day period around the end of the calendar year, allowing a closer look at the time frame of greatest interest. Unsurprisingly, the two holidays in this period, Christmas Day and New Year's Day, have the lowest average birth counts. Births fall from December 22nd through Christmas, rise from the 26th to the 30th, fall through the new year, and then normalize soon thereafter.[20] If instead of day of the year, the birth data is averaged to week of year, as shown in Figure 4A, things appear slightly less noisy. Figure 4A shows that although the first week of January has the lowest average number of births,[21] the last week of December is comparable to all of the other surrounding weeks. On the whole, it does not seem that births in the first week of January are being pushed into the last week of December. Finally, if

[20]This effect is not driven by day of the week effects. Weekends, defined as either Fridays, Saturdays, and Sundays or Saturdays and Sundays are slightly under-represented from 1969 to 1988.

[21]The other outlier in Figure 4A shows the week which includes February 29th.

we average birth data (now using NVSS data from 1969 through 2016) all the way up to the month level, which is the level at which estimation is performed, births appear relatively uniform throughout the year; Figure 4B shows this distribution.

Figure 4B was able to cover all years of NVSS natality data (1969 to 2016) because month of birth is provided consistently over the entire data set. However, from 1989 onwards, the day of the week for each birth is provided in lieu of the day of the year. Day of the week is, of course, a less useful piece of information to have than day of the year when analyzing the manipulation of birth timing. Nevertheless, I explore this issue as best the data will allow, starting with Figure 5A which shows the share of births from 1989 to 2016 by day of the week. There is a clear drop-off in births on the weekend, clearly indicating that some manipulation in birth timing occurs. However, this pattern is much less dramatic when births are broken out between method of payment. Indeed, Figure 5B shows the same births broken into two categories: those paid for by Medicaid and by private insurance. This breakdown shows that, while not uniform, Medicaid deliveries are more evenly spaced throughout the week. This could be driven by a number of factors, including that women on Medicaid have less agency over decisions related to their birth or because they tend to be younger and thus more likely to deliver naturally and more uniformly across the week. Because Medicaid recipients are low-income they will, conditional on working, be eligible for the EITC. This pattern is reassuring for the identification strategy employed in this paper.

Additionally, Figure 5C shows that the decline in deliveries throughout the week is driven in large part by Cesarean deliveries. Vaginal deliveries drop off somewhat on the weekend, but not nearly as precipitously as Cesareans. Furthermore, if we restrict to only first births as in Figure 5D, this pattern is further flattened. Overall, birth timing manipulation may exist, but the degree to which it occurs is seemingly much lower for low-income first-time mothers. Cesarean births have increased as a total share of births relative to vaginal births since 2000, as shown in Figure 6A. However, when broken out by payment method (see Figure 6B) we again see that women with private insurance are relatively more likely to deliver via Cesarean.

A mother's desire to shift birth from early January to late December in order to reap tax benefits is not the only force at work determining the timing of a child's birth. Doctors themselves, through a desire to have weekends or holidays off (or to reduce workloads on these days) may choose to not schedule deliveries(either by

Cesarean or labor induction) on those days. In effect, it is possible that all of the variation in birth timing examined thus far is driven solely by doctors' preferences, independent of any considerations for benefits that may accrue to the mothers. In reality the observed patterns are almost certainly a combination of both mothers' and doctors' preferences, with the latter not threatening this paper's identification. Subsequently, any of the visual evidence presented here examining birth manipulation will represent an upper bound on the true extent of this issue.

Another quirk of this approach worth revisiting is that outcome variables are current, based on labor market outcomes in the 12 months following the birth of a woman's child, but EITC treatment comes from a previous tax year, which differs from traditional EITC studies. Traditionally, EITC studies investigate contemporaneous labor market responses to contemporaneous EITC generosity. Although not the traditional identification strategy, my approach is arguably capturing a more salient effect. Beyond saliency, the mechanism at work here differs from other studies. When analyzing the relevant labor market effects here there are two mechanisms. The first is a more traditional EITC labor market response, driven by an increase in knowledge or awareness between the groups which did and did not receive the EITC from the previous year based on the timing of their child's birth. Note that such an identification strategy doe not require all women to learn about the EITC when they receive it for the first time; in fact, such a scenario is highly unlikely. This identification strategy works as long as some women learn about the EITC in this manner; fewer women acquiring knowledge through this mechanism would only attenuate the estimates.

At the same time, the women who receive the EITC are also affected by the receipt of the credit itself. Given that low-income individuals heavily discount (Green et. al 1994; 1996), these mothers may respond by using the EITC as a substitution for maternity leave and thereby decrease their hours or leave the labor market entirely. To the extent which mothers respond this way, estimates of any labor market effects would be biased downwards. Alternatively, women who strongly desire to return to work may use the credit as a de facto child care subsidy, enabling mothers to return to work who otherwise may not have had available child care opportunities (e.g. leaving the child with a grandparent or other friend or family member). This would bias estimates of the EITC's labor market effects upwards. In an attempt to disentangle these effects, I will examine women having their second child, who are only exposed to the income effects of the EITC and not the information shock.

Data

The data set utilized for the empirical analysis in this paper comes from the Survey of Income and Program Participation. The SIPP is a nationally representative survey designed to collect detailed data about incomes, labor force participation, and take-up and eligibility for various social programs. To achieve these objectives, the SIPP uses a continuous series of national panels, with durations ranging from 2.5 to 4 years. The SIPP is an ideal data set for this analysis because it provides data about birth timing and high-frequency labor market outcomes in the subsequent year. Other data sets may only provide annual hours or earnings which would pose a significant issue given my interest in labor market outcomes at a month-by-month level. The SIPP's provision of such outcomes and its information on children's birth months make it an ideal public data set for this analysis.

To identify women with newborn children, I first identify all children within each family unit. Next, I calculate the age of the oldest child in each family unity and restrict to female heads of household and spouses whose oldest child during a reference month was less than a year old.[22] From there, I assign a woman's labor market outcomes by annualizing over the observed reference months in which the woman's child has been born but is under one year old. Additionally, I assign treatment status to all women whose children were born from July to December and control status to those with births from January to June. Taking note of birth year and state, the appropriate EITC parameters and other relevant tax policies are merged on to complete the data set.

After restricting the sample to only women observed in the year proceeding the birth of their first/only child, 15,956 observations remain. Table 3, which contains descriptive statistics for the sample of women used in the primary analysis, shows that the sample is primarily married women. This fact is a reflection of the sampling frame of the SIPP. Additionally, we see that the married women generally have higher earnings ($22,330 versus $13,102) and are more likely to be highly educated. Employment rates and birth timings across the two groups are fairly comparable. Figure 7 shows the comparison of birth counts by month across

[22] I also drop twins at this stage.

the four groups of women in Table 3. Although not detailed enough for a thorough analysis, births appear relatively uniform across months for all groups of women during our sample period. This fact is crucial for evaluating the validity of our regression discontinuity estimation strategy.

Estimation Strategy

The estimation strategy for this paper is a sharp regression discontinuity design, with the running variable being the distance between a child's month of birth and the end of the calendar year. The full estimation equation follows the form:

$$Y_{ist} = \alpha + \beta PhaseinRate_{st} D_i + \gamma ChildTaxCredit_t D_i + \delta AddDeduct_t D_i + \zeta DeductDiff_t D_i Unmarried_i + f(X_i) + \eta Age_i + \theta Age_i^2 + \iota Race_i + S_s + \tau_t + \varepsilon_{ist}$$

$$D_i = \begin{cases} 1 & \text{if } X_i \leq -0.5 \\ 0 & \text{if } X_i \geq 0.5 \end{cases}$$

In this equation, Y_{ist} represents a relevant labor market outcome, β is the parameter of interest, $f(X_i)$ is the control function of X_i (birth month) on either side of the cut-off. For each set of results, I estimate versions with linear and quadratic $f(X_i)$ whenever possible.[23] Following Dong (2015) birth month is adjusted to run from -5.5 in July to 5.5 in June the subsequent year. The two months around my cut-off, December and January, take values of -0.5 and 0.5, respectively. This is done because my running variable is discrete and a standard regression discontinuity estimation (e.g. December births as $X_i = -1$ and January births as $X_i = 0$) would lead to inconsistent treatment estimates even if my $f(X_i)$ function is properly specified. Additionally, I control for age, age squared, race, and state and cohort fixed effects.[24]

The three outcomes of interest are employment, log earnings, and log hourly wages of mothers in the 12

[23]For example, when estimating my November-February births only specification I cannot include a quadratic $f(X_i)$ as there are only two possible X_i values on each side of the cut-off.

[24]The sample is further restricted such that individuals who change states, races, marital status, or education during their reference periods are excluded from the analysis.

months following the birth of her first/only child. Note that the Local Average Treatment Effect (LATE) identified in this design is for a different subpopulation of EITC recipients than estimates from other studies. Here, the LATE is identified for mothers with newborn children, and examines past EITC exposure's effects on contemporaneous outcomes. As previously discussed, the EITC has two channels here to affect a mother's labor supply. One is the information gain from receiving the EITC for the first time while the other is the effect of the credit itself. The information gain should have positive extensive margin labor market effects for unmarried women and negative effects for married women, while the income effect is ambiguous for both groups.

The Identification and Methodology section covered in detail the first identifying assumption of the regression discontinuity design, namely that EITC-eligible women are unable to precisely time their birth in order to shift their treatment status. I will now address the second identifying assumption regarding whether the jump in treatment status at the end-of-year cut-off is the only discontinuity. To accomplish this, I examine whether there are any discontinuities in observable characteristics through that cut-off. Figures 8A through 8E present plots of the observables I use to divide the sample (marital status, education level) and use as control variables in my regressions (race and age) by month of birth. Figures 8A to 8D show fractions black, Native American, Asian, and white[25] for all unmarried, low-ed unmarried, all married, and low-ed married women, respectively. The last figure in this set, Figure 8E, shows full sample balance tests of marital status and education level.[26]

For all unmarried women (Figure 8A) there is little evidence of discontinuities in covariates through the end-of-year cut-off. There is a large jump in fraction Native American (3.5 percentage points), but given there are a total of 36 unmarried individuals identifying as Native American it is hard to draw any firm conclusions. Also note that unmarried women with December births are slightly younger than those with January births. If women were manipulating birth timing we would expect older women, who are presumably more knowledgeable about potential EITC or other tax benefits and more likely to deliver via cesarean section to be the ones to do so.

The covariate balance tests for low-ed unmarried women (Figure 8B) paint a similar story; none of

[25]Here, white is defined as non-black, non-Native American, and non-Asian.

[26]I also run these balance tests with a linear functional form, although I do not show them here. There are no qualitative differences across that dimension.

the estimated discontinuities are statistically different from zero. Again, women with December births are younger than those with January births, but the point estimate of the jump (-0.7 years) is fairly small with noticeably larger standard errors relative to the estimates for all unmarried women. This is unsurprising given the reduction in sample size.

Married women represent a much larger fraction of the sample than unmarried women, so it follows that Figure 8C shows more precise estimates than those in Figures 8A and 8B. The first panel of Figure 8C shows a statistically significant increase in the fraction of the sample identifying as black (4.8 percentage point increase, 1 percent significance). This is reflected in a near one-to-one decline in the fraction white, as shown in the fourth panel. This could imply that black mothers are more likely to anticipate the benefits of a December birth and manipulate their birth timing to take advantage of these benefits.

However, low-ed married mothers have the most to gain from such a movement, both in absolute and relative terms compared to high-ed married women, and the analysis of their covariates through the end-of-year cut-off is the subject of Figure 8D. The increase in fraction black through the threshold is still visible here, but it is of a smaller magnitude and no longer statistically significant. The other panels reinforce that there is little observable difference in covariates at the discontinuity.

Results

The first estimates of the EITC's labor market effects are presented visually in Figures 9A to 9D, 10A to 10D, and 11A to 11D. Each set contains one particular outcome; Figures 9A to 9D show employment estimates, 10A to 10D have log earnings, and 11A to 11D are for log hourly wages. Inside each set are separate estimates for all unmarried, low-ed unmarried, all married, and low-ed married, respectively. These figures visually show the estimates of the following equation:

$$Y_i = \alpha + \beta D_i + \gamma X_i + \delta X_i D_i + \zeta X_i^2 D_i + \varepsilon_i$$

The point estimates and standard errors for these regression discontinuities are in the quadratic functional form rows of Table 4. There is a large positive effect of the EITC on employment for low-ed unmarried women, with receipt of the EITC increasing employment rates in the 12 months following the birth of a woman's first child by 13.9 percentage points. Across the full low-ed unmarried sample, 44.1 percent of mothers work in the year following their first child's birth, implying the EITC has quite large positive extensive margin employment effects for these women. For low-ed married women the employment effects are small and statistically insignificant, but there is some evidence that low-ed married women earn less and have lower wages. These effects, especially earnings, are sensitive to the specification of $f(X_i)$.

Building from the simplest specification in Table 4, I now account for variation in EITC generosity across states and time by interacting D_i with $PhaseinRate_{st}$. The equation estimated in Table 5 is thus:

$$Y_{ist} = \alpha + \beta PhaseinRate_{st} D_i + f(X_i) + \varepsilon_i$$

Results here reveal the same pattern as before, but the interpretation is slightly different. Now, as Table 5 shows in Panel A, a 10 point increase in the phase-in rate increases employment for low-ed unmarried women in the year following the birth of their first child by 4.7 percentage points, a result that is marginally significant. There is also some evidence the EITC causes small reductions in the employment of low-ed unmarried women, but the statistical significance of this result is sensitive to the functional form chosen for the running variable, month of birth. Furthermore, there is evidence that the earnings and wages of low-ed married women decline in response to EITC generosity but, again, the results are not robust across linear and quadratic $f(X_i)$ specifications.

With the unconditional relationship between labor market outcomes and the EITC for new mothers examined in detail, Table 6 adds the other tax policies that are received around the end-of-year birth cut-off as well as the race dummies, age, age squared, and the state and year fixed effects. This is the full estimation equation shown at the beginning of the Estimation Strategy section. Including these controls does not change the direction of the EITC's effects on employment across the four groups of women, but it does increase the magnitude of the effects. A 10 point increase in the phase-in rate now increases employment for low-ed unmarried women by 29.4 percentage points (significant at the 5 percent level) while decreasing employment for low-ed married women by 15.5 percentage points (not statistically significant). There is also some evidence that receiving the EITC decreases the earnings of low-ed married women, but this result is only marginally significant with the quadratic $f(X_i)$ specification.

With the baseline specification estimated, the following eight tables examine various robustness and placebo tests in order to discover how well the results can be replicated or falsified under a series of reasonable alternative approaches. The first table in this sequence, Table 7, restricts the sample to only December and January births. One potential issue with using births across the entire year is that as the analysis includes mothers farther from the end-of-year cut-off, the women on either side grow less alike. Under ideal conditions, one would compare all women in the United States who give birth during a short bandwidth around the end of the year, say several weeks on either side. However, due to both sample size and birth timing information limitations, the analysis presented in Tables 4 through 6 is the most parsimonious attempt at uncovering the EITC's effects. Nevertheless, the closest replication I can achieve to that ideal scenario is the two-month window in Table 7. Note that both point estimates as well as standard errors are much larger here than in past tables, in some cases by an order of magnitude. Even so, although the effects are much less precisely estimated, a familiar pattern emerges; namely, I observe positive employment (and now earnings and wage) effects for low-ed unmarried women and negative effects across the same three outcomes for low-ed married women.

As a compromise between the full year sample in Table 6 and the December to January only sample of Table 7, Table 8 adds one month onto either side of the cut-off so that the sample now covers November to February.[27] This specification, as one might expect, falls between the previous two tables, both in effect sizes and standard errors. There is only one marginally significant effect in Table 8, a negative earnings effect for low-ed married women, but the remaining results provide suggestive evidence in line with previous results. Table 8 shows a positive employment effect (37.0 percentage points) for low-ed unmarried women from a 10 point increase in the phase-in rate, and a negative effect (-20.6 percentage points) for low-ed unmarried women.

Table 9 seeks to address any concerns that may linger about birth timing manipulation. Women may be able to exert some degree of control over the timing of their birth, but they cannot do so on a scale that spans multiple months. Hence, Table 9 runs the baseline specifications from Table 6 but excludes December

[27] Tables 15, 16, and 17 show results using three, four, and five months on either side of the cut-off, respectively.

and January, the two months directly on either side of the end-of-year cut-off. This, of course, comes with a trade-off as women with November births may not be as similar to February births as the December and January births are to each other, but this is a necessary concession. These results are quite similar to the baseline, with large positive employment effects (36.0 percentage points, 5 percent significance) for the low-ed unmarried women. One difference is that now the negative employment effects for low-ed married women (-31.2 percentage points) are also statistically significant at the 5 percent level. Earnings effects are additionally negative for low-ed unmarried women but are, as usual, statistically insignificant.

Table 10, whose results have been alluded to at points throughout the paper, shows my primary placebo test. This table assigns the same baseline estimation strategy as Table 6, but it does so with the 1 to 2 child EITC phase-in rate differential and women who give birth to their second child. The primary mechanism that I argue I am capturing in my main results-that women respond to the EITC after receiving it for the first time due to an information gain-should not affect these women as they have already received this information shock when they had their first child. The only effects this regression should capture are the income effects of the credit which, as I have previously argued, are theoretically ambiguous.

The evidence from Table 10 lends supporting evidence that the previous results have been driven, at least in large part, by the information shock. Here, employment effects for low-ed unmarried women are less than half the magnitude as their counterparts in the first birth sample and statistically insignificant. Rather interestingly, employment effects for the low-ed married women are now both positive and a larger magnitude than the effects for their low-ed unmarried counterparts, albeit not statistically different from zero. One might expect more negative income effects for married women relative to unmarried women. Married women, if they desire to return to work after giving birth, have a greater ability to pay for child care in the absence of the EITC due to spousal earnings.

Even within the first year of their child's life, there are potentially differential responses to the EITC based on the child's age. For example, the EITC could induce a new mother back to work when the child is 11 months old but not in the weeks immediately proceeding the child's birth. To the extent that this relationship can be explored, Table 11 examines labor market responses during months where a woman's child is 0 to 5 months old while Table 12 does the same for women when their infant is 6 to 11 months old.

Contrary to the story proposed here, labor market response as generally stronger during the months where the infant is younger. One notable exception is the log hourly wage results for low-ed married women; there is a statistically significant strong negative effect for these women while the child is 0 to 5 months old and a statistically significant strong positive effect from 6 to 11 months old. Still, the results consistently show that the EITC encourages low-ed unmarried women to work and discourages low-ed married women.

Because the SIPP heavily oversamples families, the majority of my sample is married. One way to account for this is to use the sample weights provided in the data set to perform a weighted analysis; Table 13 shows this analysis. Across the board these results are quite similar to their unweighted counterparts, both quantitatively and qualitatively. I still find positive employment effects (25.5 percentage points, 10 percent significance) for low-ed unmarried women but the low-ed married results are now statistically significant as well. Indeed, Table 13 shows that a 10 point increase in the EITC phase-in rate decreases employment for low-ed married women by 22.2 percentage points in the 12 months following the birth of a first child, a result that is significant at the 5 percent level.

Finally, Table 14 asks whether my results are sensitive to the specific employment definition I have chosen where a woman is considered employed if her annualized earnings are at least $2,500. Additionally, Table 14 considers whether outliers in the top of the earnings distribution are driving the log earnings and log hourly wage results. Specifically, Panel A of Table 14 considers any earnings to constitute employment and Panel B raises the threshold to $5,000. Panel C excludes the top 10 percent of earners in each of the four samples. The results are not sensitive to these alternative specifications.

Conclusion

The Earned Income Tax Credit is one of the largest anti-poverty programs in the United States. The EITC has been widely praised for its anti-poverty effects and its pro-work incentives. A number of studies have analyzed these effects, particularly for single mothers who are a primary target and recipient of the EITC. Most of these studies separate women into treatment and control groups based on having children, marital status, and education level. However, as all of these categories are potentially endogenous to the EITC, it is possible the estimated labor market effects of the EITC are biased upwards. This paper has identified these biases, categorized the degree to which they occur, and implemented a different identification strategy using another source of EITC variation to avoid them.

Namely, this source of variation is the timing of a woman's first birth around the end of the calendar year. Utilizing data from the Survey of Income and Program Participation and a regression discontinuity design around this end-of-year eligibility cut-off, I find robust positive employment effects of the EITC and moderately less robust negative employment effects for married women. The positive effects for unmarried women are consistent with the rest of the EITC literature, but little evidence has been found showing negative effects for married women.

However, as I've argued throughout this paper, the bias introduced from comparing across having children, marital status, and education level is upward. Hence, my identification strategy may identify a true negative effect for married women that other studies have been unable to previously capture. The caveat to my approach is that effects are only estimated for women in the 12 months following the birth of their first child, so care must be exercised when attempting to draw conclusions about all EITC recipients from my specific sample. Nevertheless, all mothers (except those whose first birth is to more than one child) are represented by my sample at some point during their childbearing history.

Further work should seek additional ways to estimate the labor market impacts of the EITC using estimation strategies which avoid these endogeneity issues, particularly for married mothers. Another potential avenue of research may examine the degree to which the EITC encourages or discourages women from obtaining additional education.

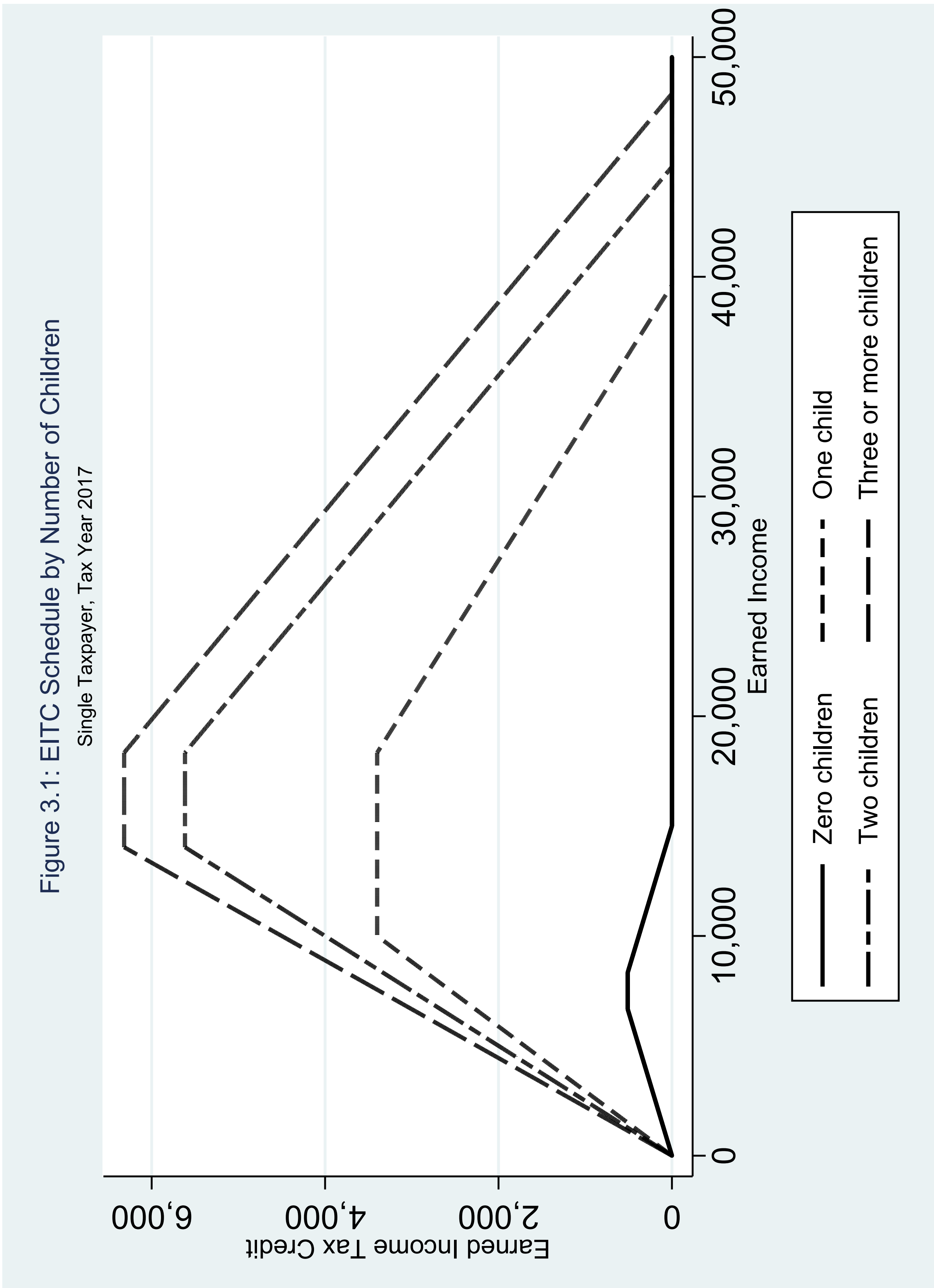
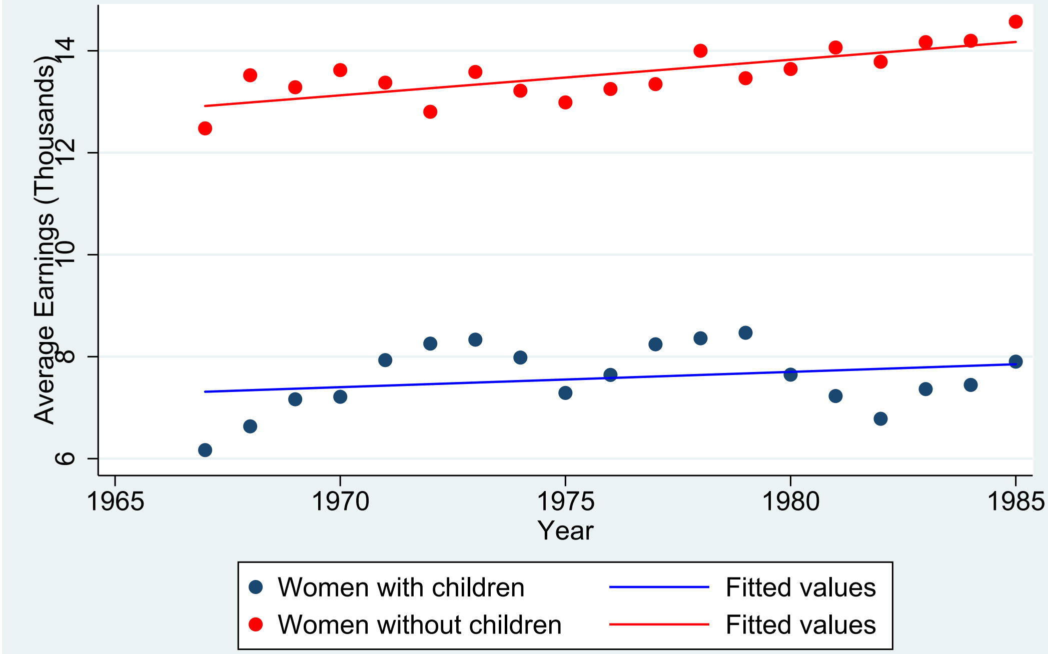


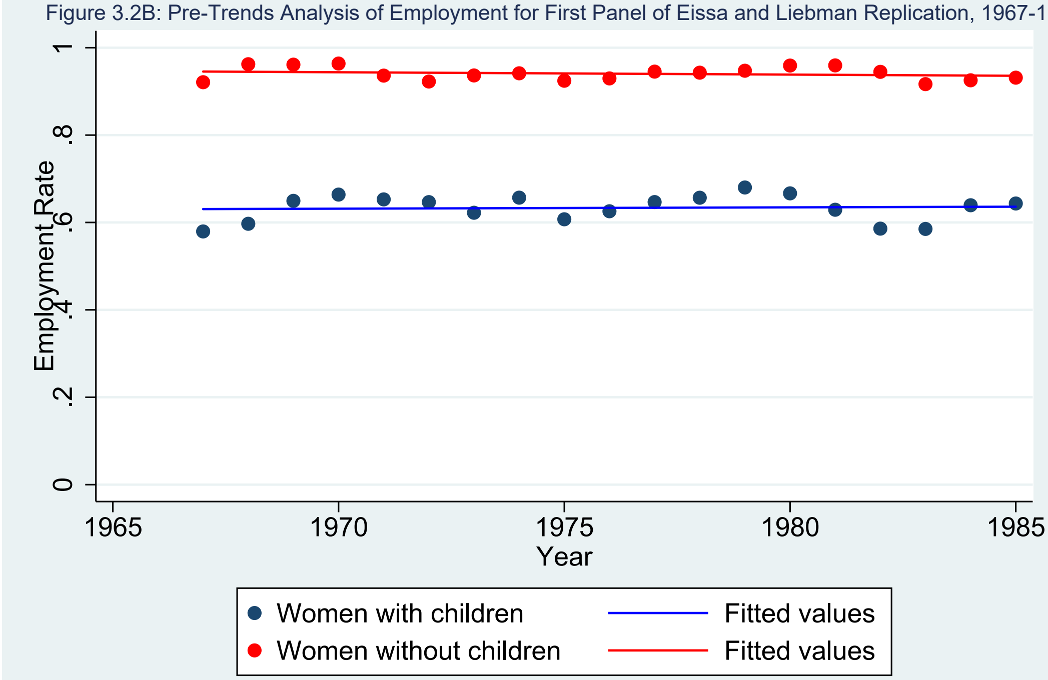
Figure 3.1: EITC Schedule by Number of Children

Figure 3.2A: Pre-Trends Analysis of Earnings for First Panel of Eissa and Liebman Replication, 1967-198



Data Source: Panel Study of Income Dynamics

Figure 3.2B: Pre-Trends Analysis of Employment for First Panel of Eissa and Liebman Replication, 1967-19



Data Source: Panel Study of Income Dynamics

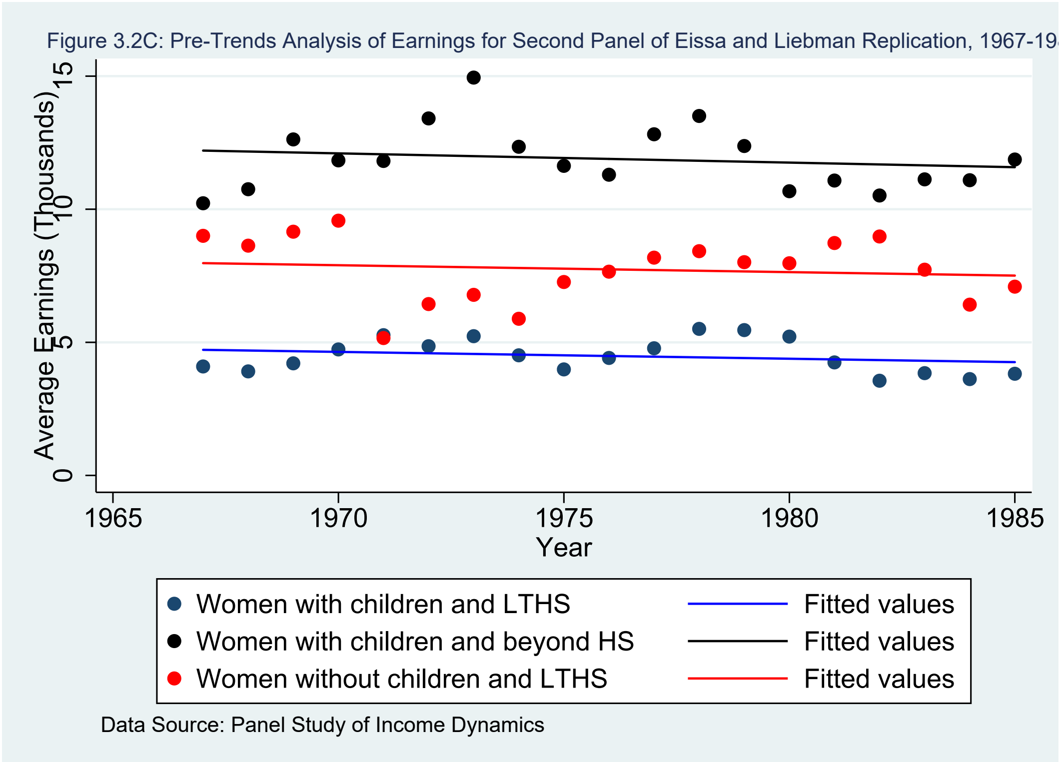


Figure 3.2C: Pre-Trends Analysis of Earnings for Second Panel of Eissa and Liebman Replication, 1967-19
Average Earnings (Thousands)
0
5
10
15
1965
1970
1975
1980
1985
Year
Women with children and LTHS
Fitted values
Women with children and beyond HS
Fitted values
Women without children and LTHS
Fitted values
Data Source: Panel Study of Income Dynamics

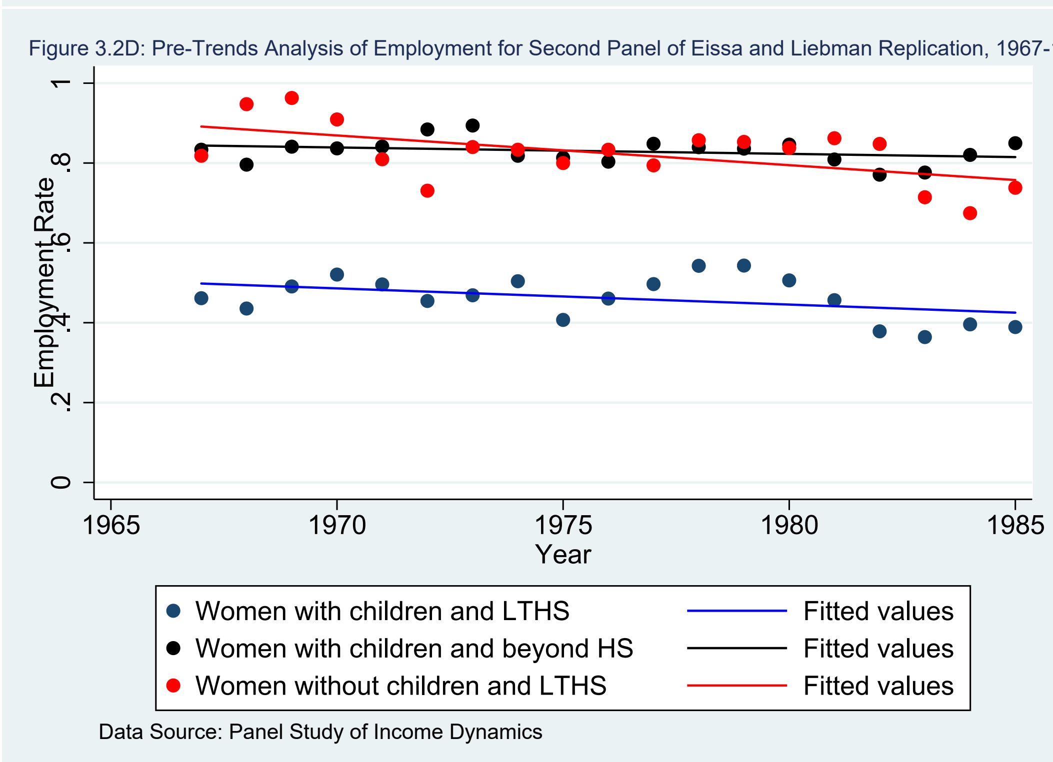
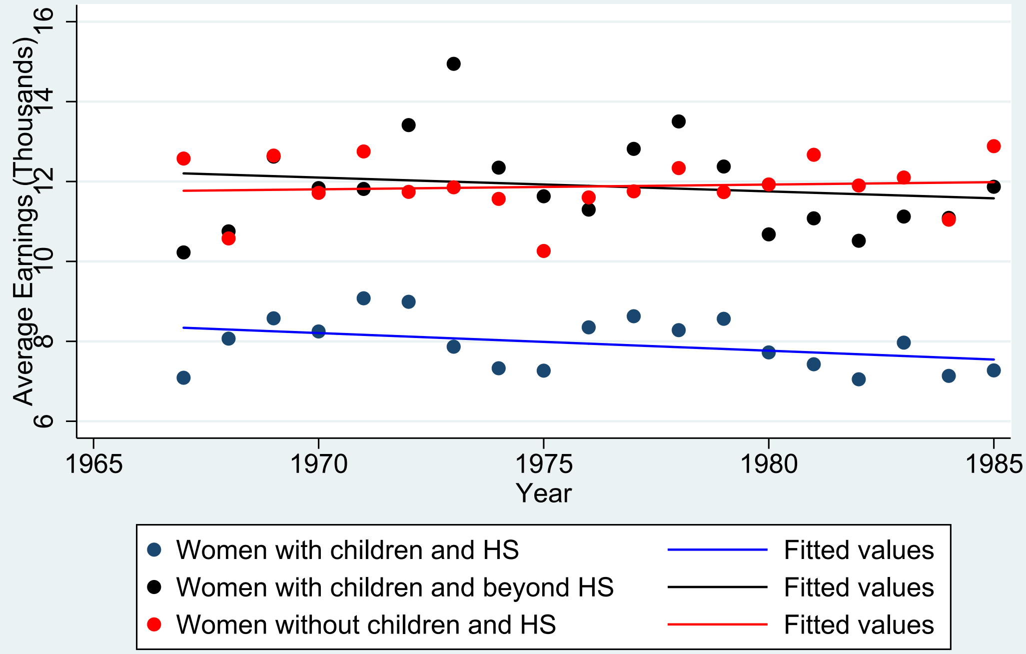


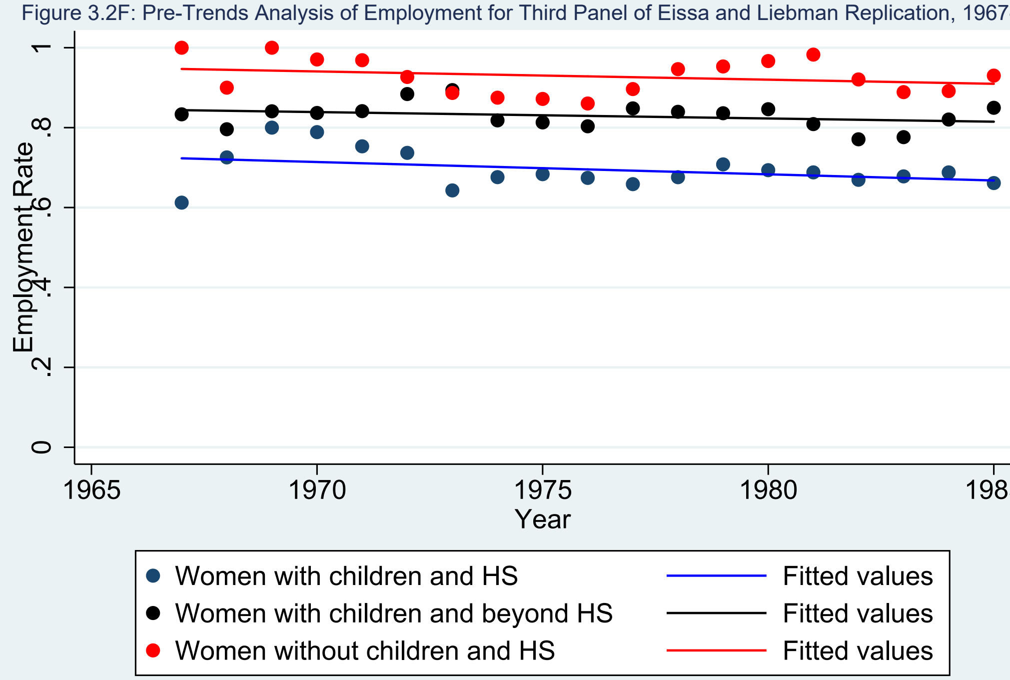
Figure 3.2D: Pre-Trends Analysis of Employment for Second Panel of Eissa and Liebman Replication, 1967-1
Employment Rate
0
.2
.4
.6
.8
1
1965
1970
1975
1980
1985
Year
Women with children and LTHS
Fitted values
Women with children and beyond HS
Fitted values
Women without children and LTHS
Fitted values
Data Source: Panel Study of Income Dynamics

Figure 3.2E: Pre-Trends Analysis of Earnings for Third Panel of Eissa and Liebman Replication, 1967-198



Data Source: Panel Study of Income Dynamics

Figure 3.2F: Pre-Trends Analysis of Employment for Third Panel of Eissa and Liebman Replication, 1967-19



Data Source: Panel Study of Income Dynamics

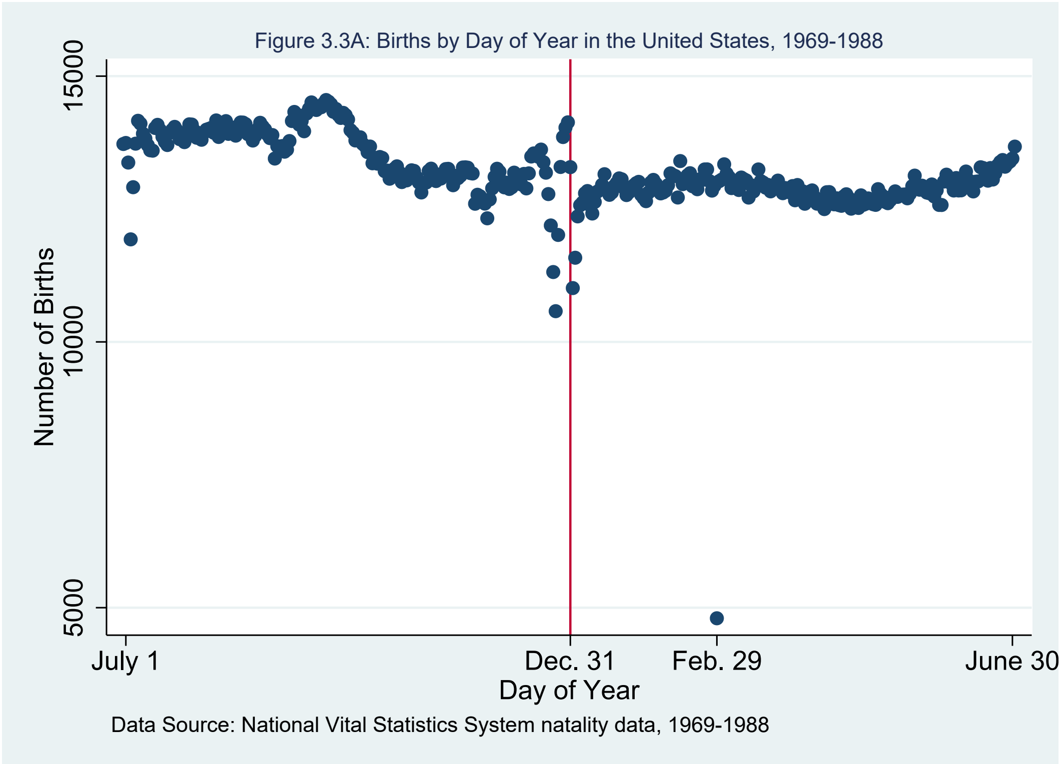


Figure 3.3A: Births by Day of Year in the United States, 1969-1988
15000
10000
5000
Number of Births
July 1
Dec. 31
Feb. 29
June 30
Day of Year
Data Source: National Vital Statistics System natality data, 1969-1988

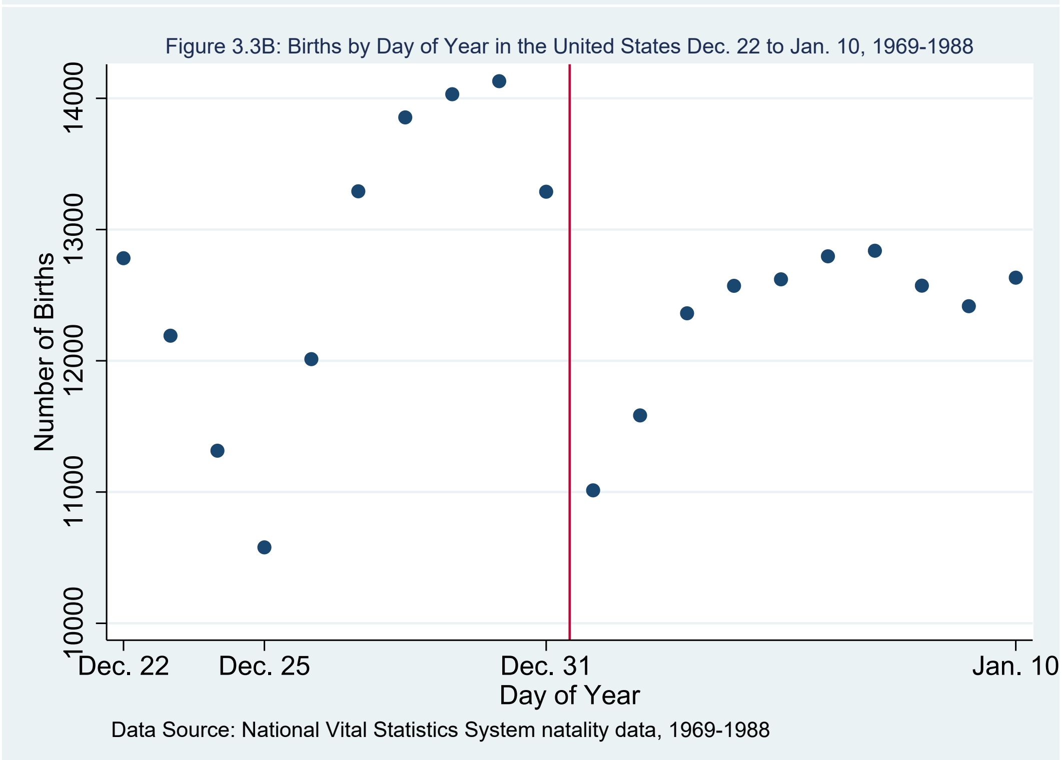
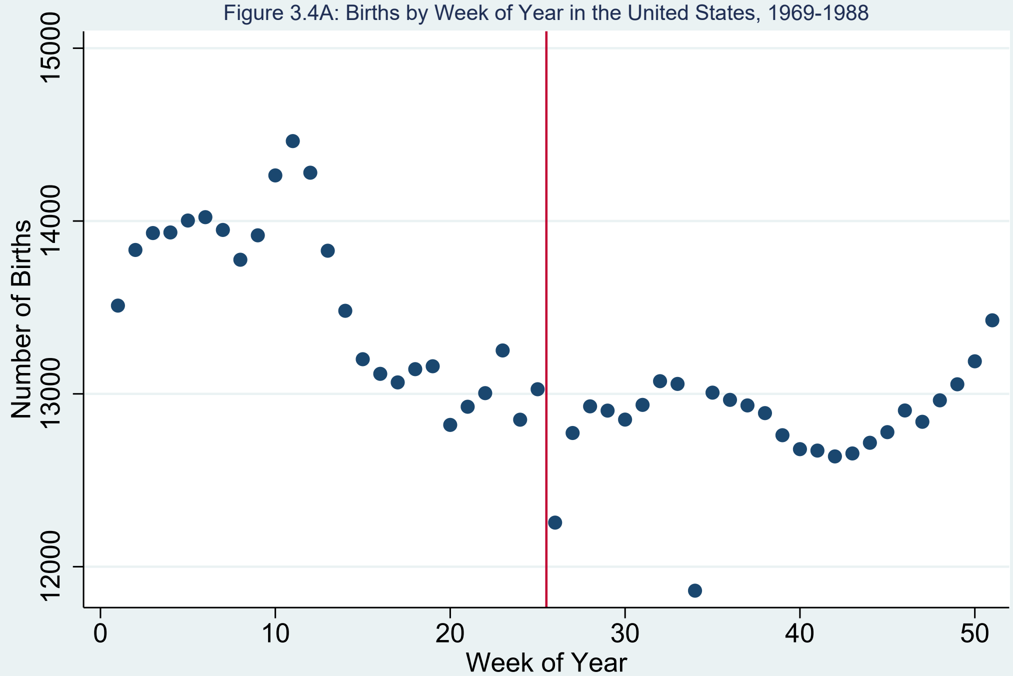


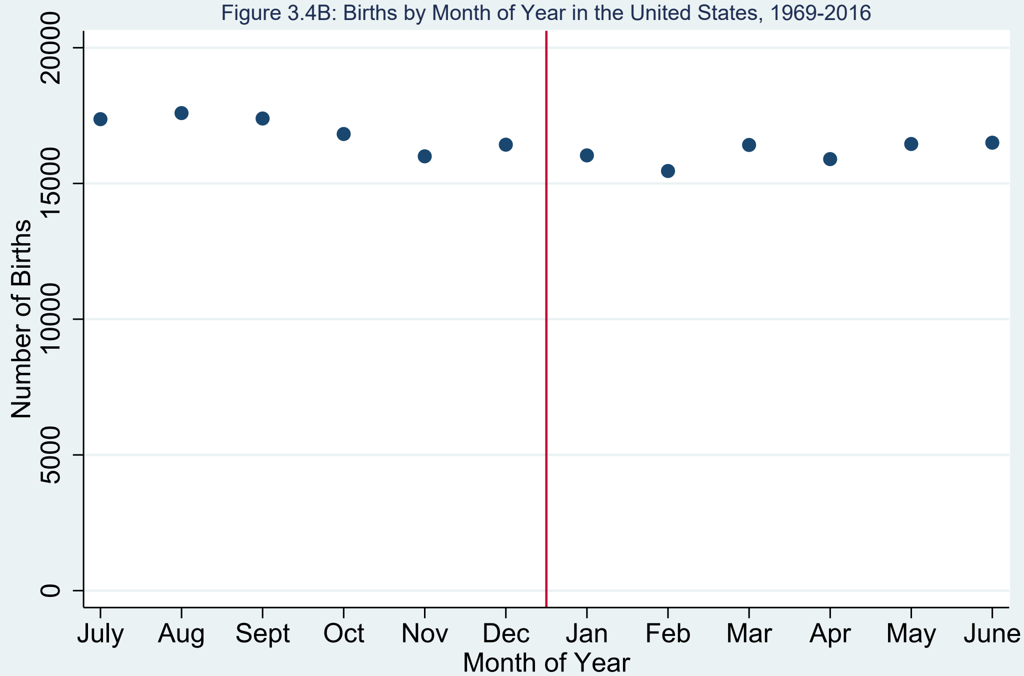
Figure 3.3B: Births by Day of Year in the United States Dec. 22 to Jan. 10, 1969-1988
14000
13000
12000
11000
10000
Number of Births
Dec. 22
Dec. 25
Dec. 31
Jan. 10
Day of Year
Data Source: National Vital Statistics System natality data, 1969-1988

Figure 3.4A: Births by Week of Year in the United States, 1969-1988



Data Source: National Vital Statistics System natality data, 1969-1988

Figure 3.4B: Births by Month of Year in the United States, 1969-2016



Data Source: National Vital Statistics System natality data, 1969-2016

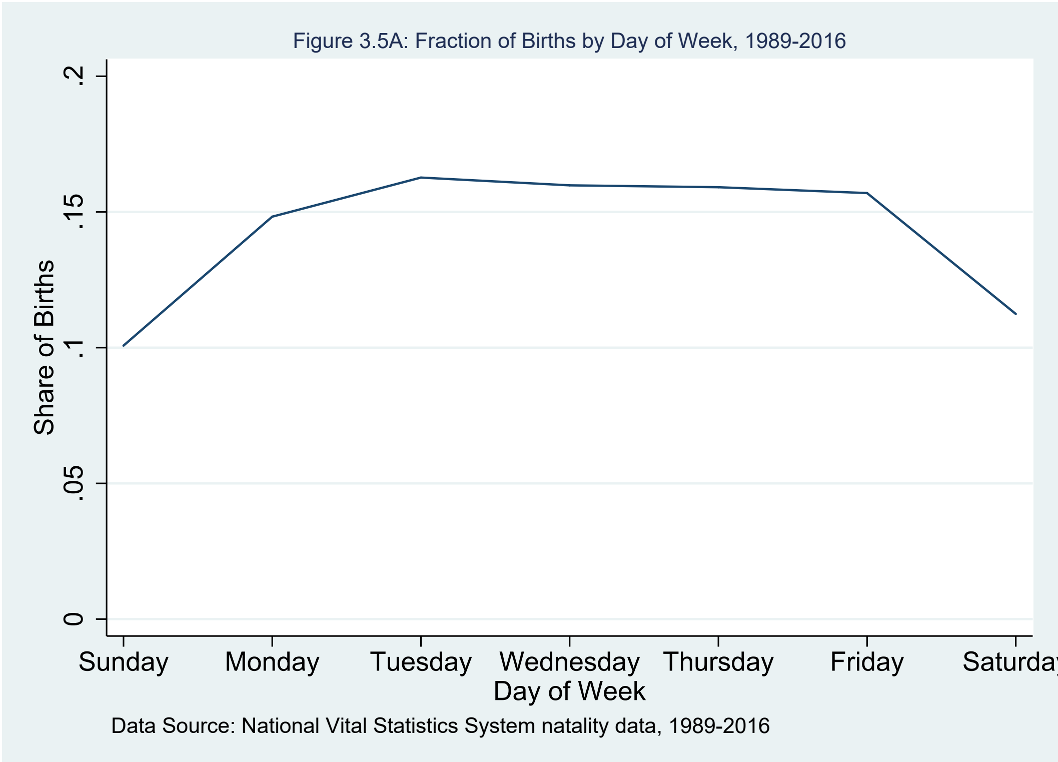


Figure 3.5A: Fraction of Births by Day of Week, 1989-2016
Share of Births
0
.05
.1
.15
.2
Sunday
Monday
Tuesday
Wednesday
Thursday
Friday
Saturday
Day of Week
Data Source: National Vital Statistics System natality data, 1989-2016

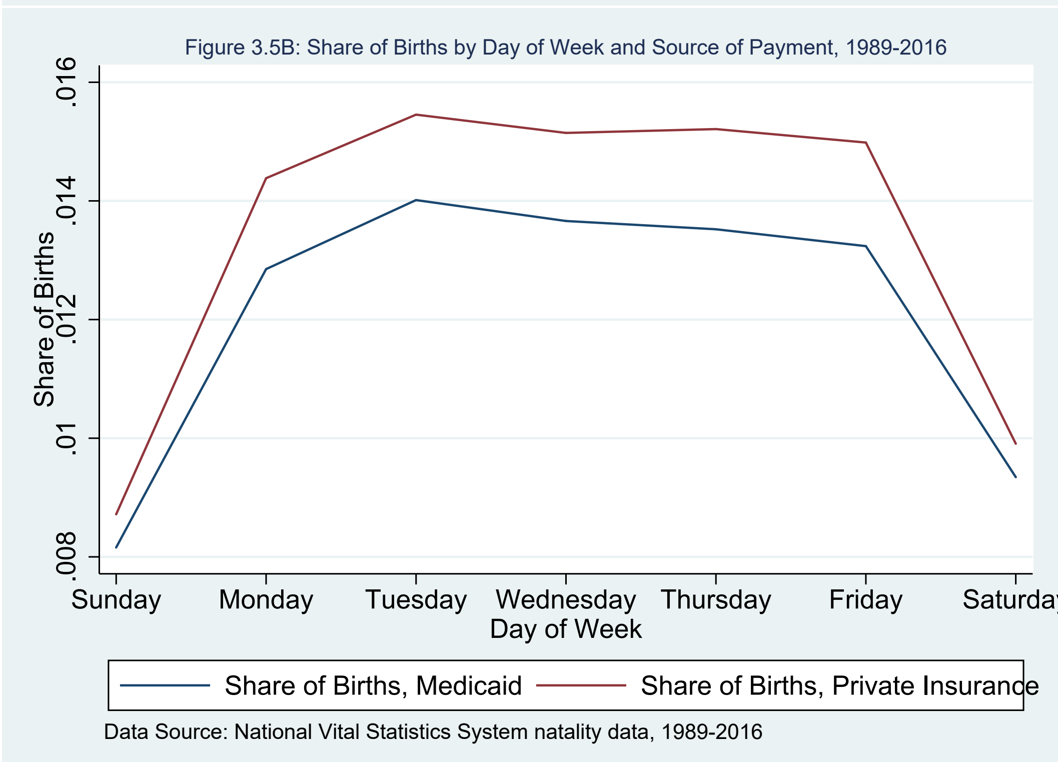


Figure 3.5B: Share of Births by Day of Week and Source of Payment, 1989-2016
Share of Births
.008
.01
.012
.014
.016
Sunday
Monday
Tuesday
Wednesday
Thursday
Friday
Saturday
Day of Week
Share of Births, Medicaid
Share of Births, Private Insurance
Data Source: National Vital Statistics System natality data, 1989-2016

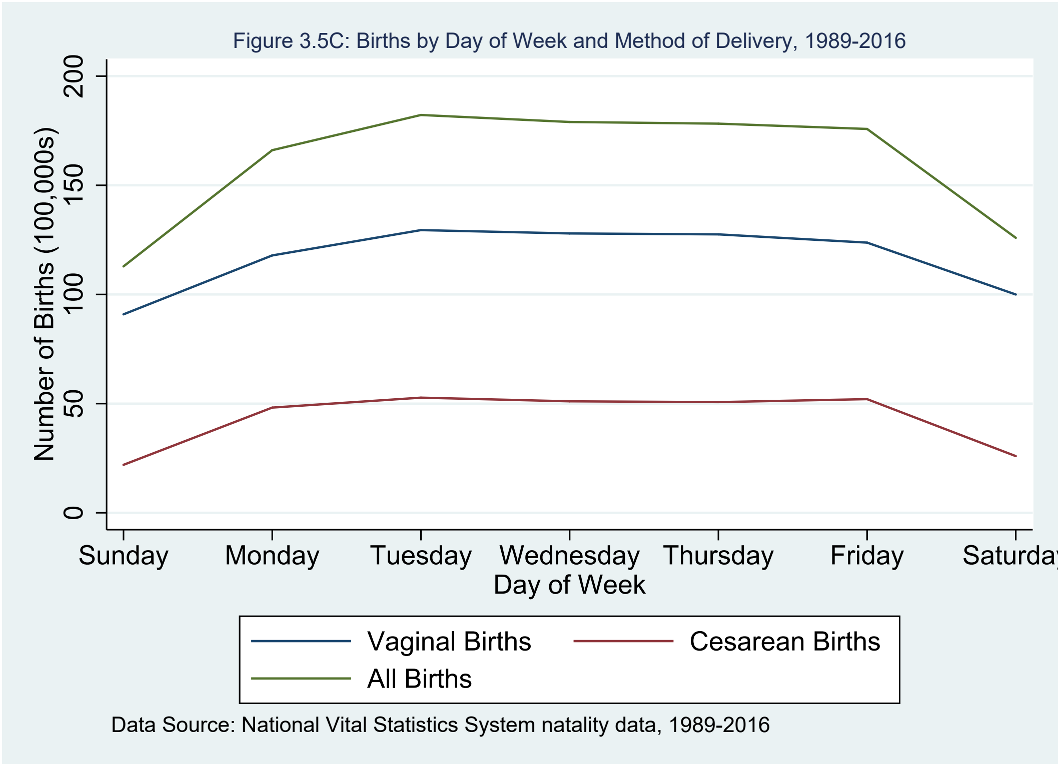


Figure 3.5C: Births by Day of Week and Method of Delivery, 1989-2016
Number of Births (100,000s)
0
50
100
150
200
Sunday
Monday
Tuesday
Wednesday
Thursday
Friday
Saturday
Day of Week
Vaginal Births
Cesarean Births
All Births
Data Source: National Vital Statistics System natality data, 1989-2016

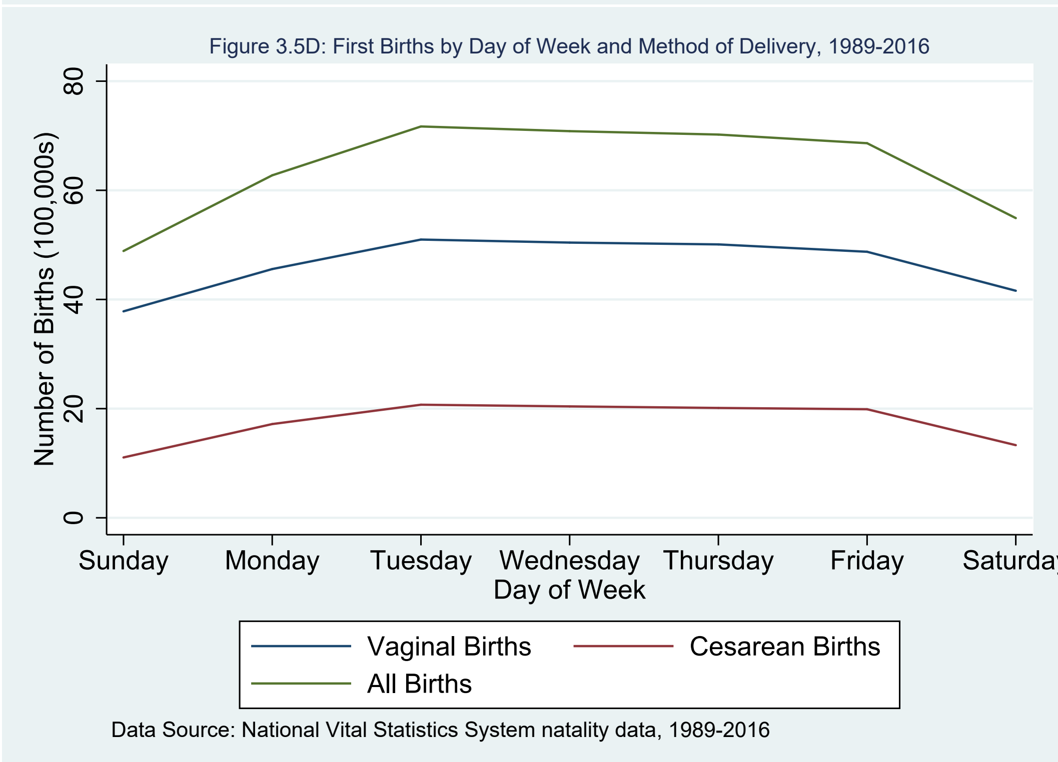


Figure 3.5D: First Births by Day of Week and Method of Delivery, 1989-2016
Number of Births (100,000s)
0
20
40
60
80
Sunday
Monday
Tuesday
Wednesday
Thursday
Friday
Saturday
Day of Week
Vaginal Births
Cesarean Births
All Births
Data Source: National Vital Statistics System natality data, 1989-2016

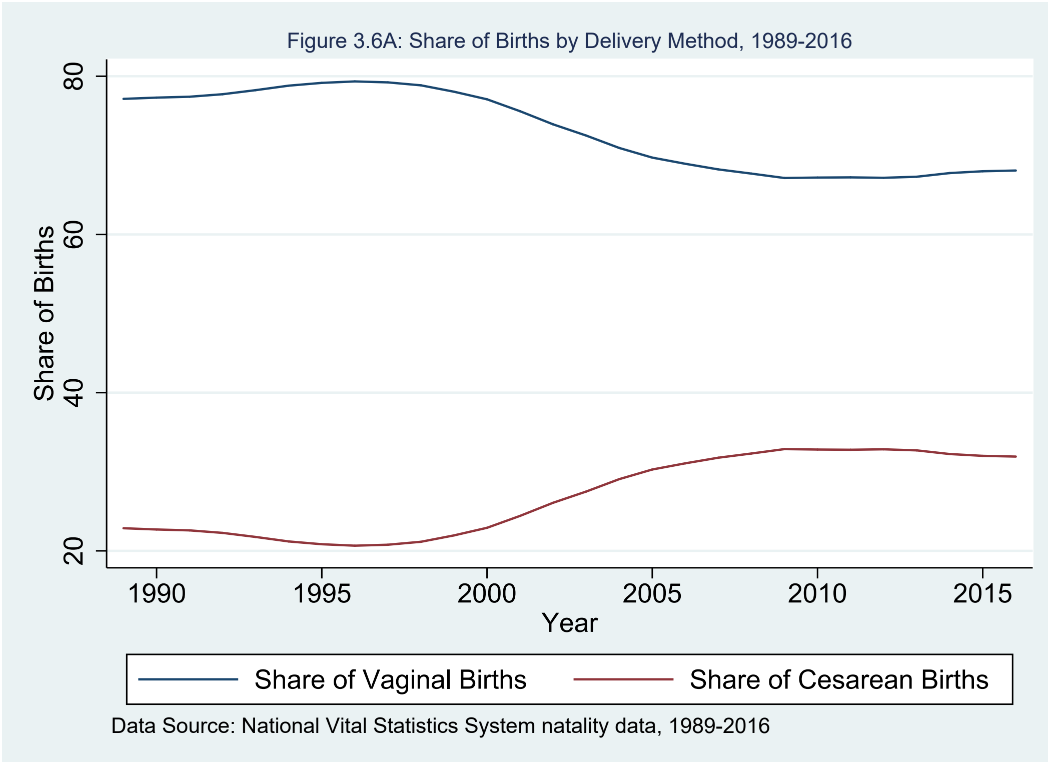


Figure 3.6A: Share of Births by Delivery Method, 1989-2016
Share of Births
80
60
40
20
1990
1995
2000
2005
2010
2015
Year
Share of Vaginal Births
Share of Cesarean Births
Data Source: National Vital Statistics System natality data, 1989-2016

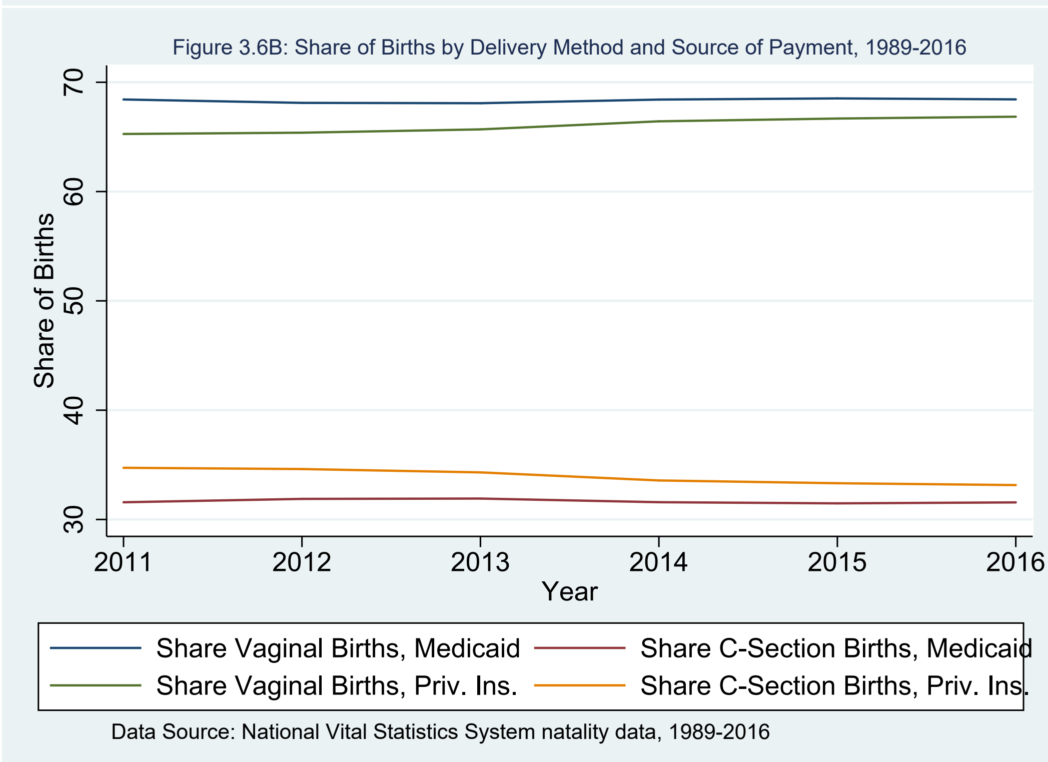


Figure 3.6B: Share of Births by Delivery Method and Source of Payment, 1989-2016
Share of Births
70
60
50
40
30
2011
2012
2013
2014
2015
2016
Year
Share Vaginal Births, Medicaid
Share C-Section Births, Medicaid
Share Vaginal Births, Priv. Ins.
Share C-Section Births, Priv. Ins.
Data Source: National Vital Statistics System natality data, 1989-2016

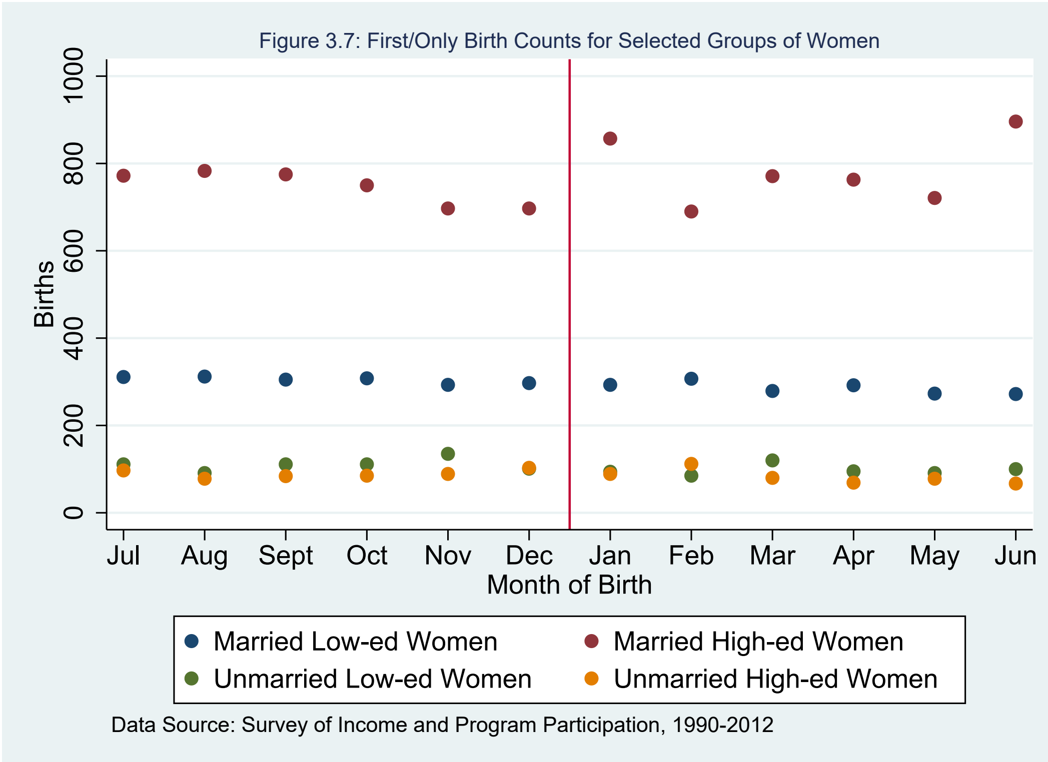
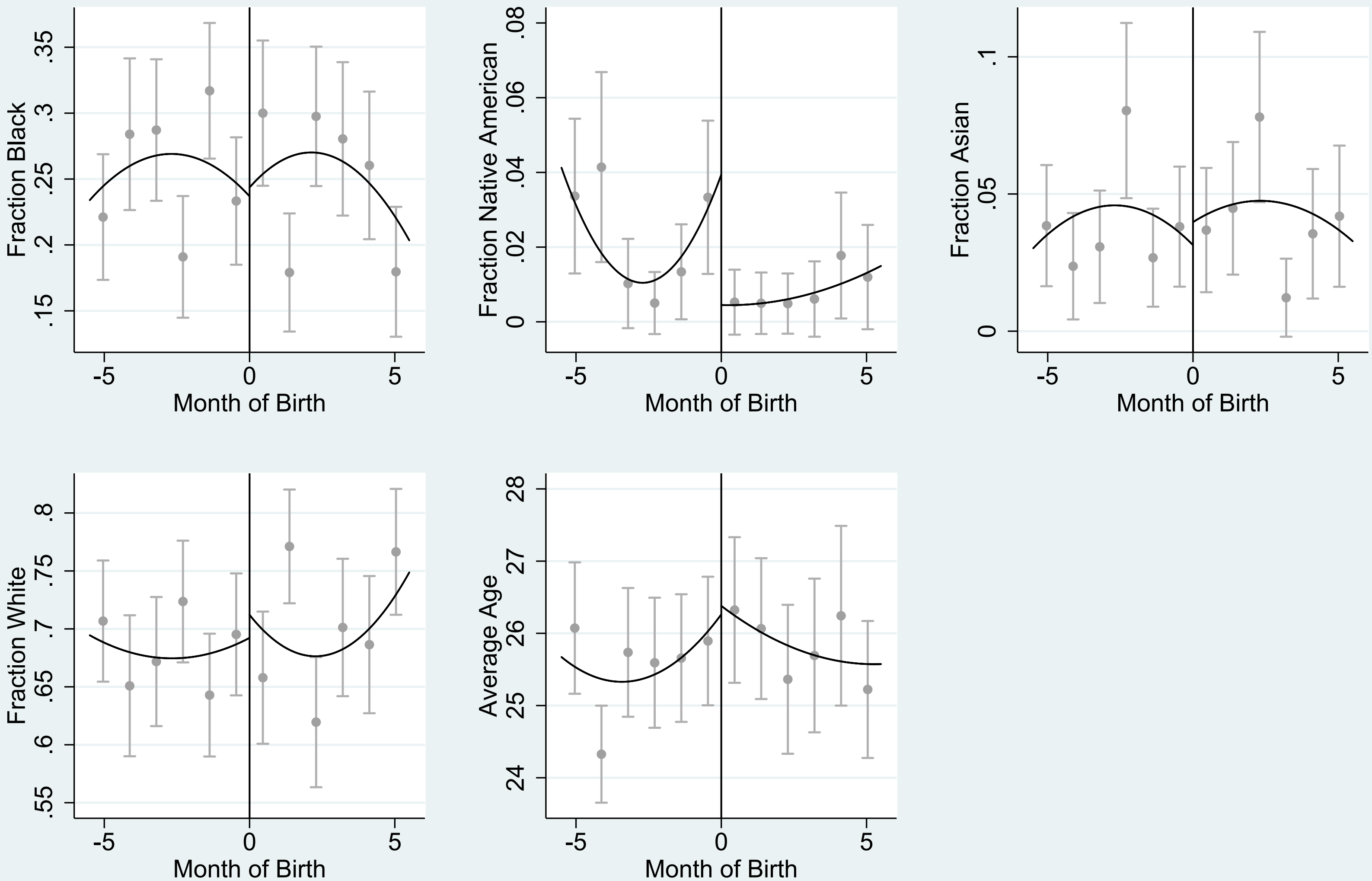


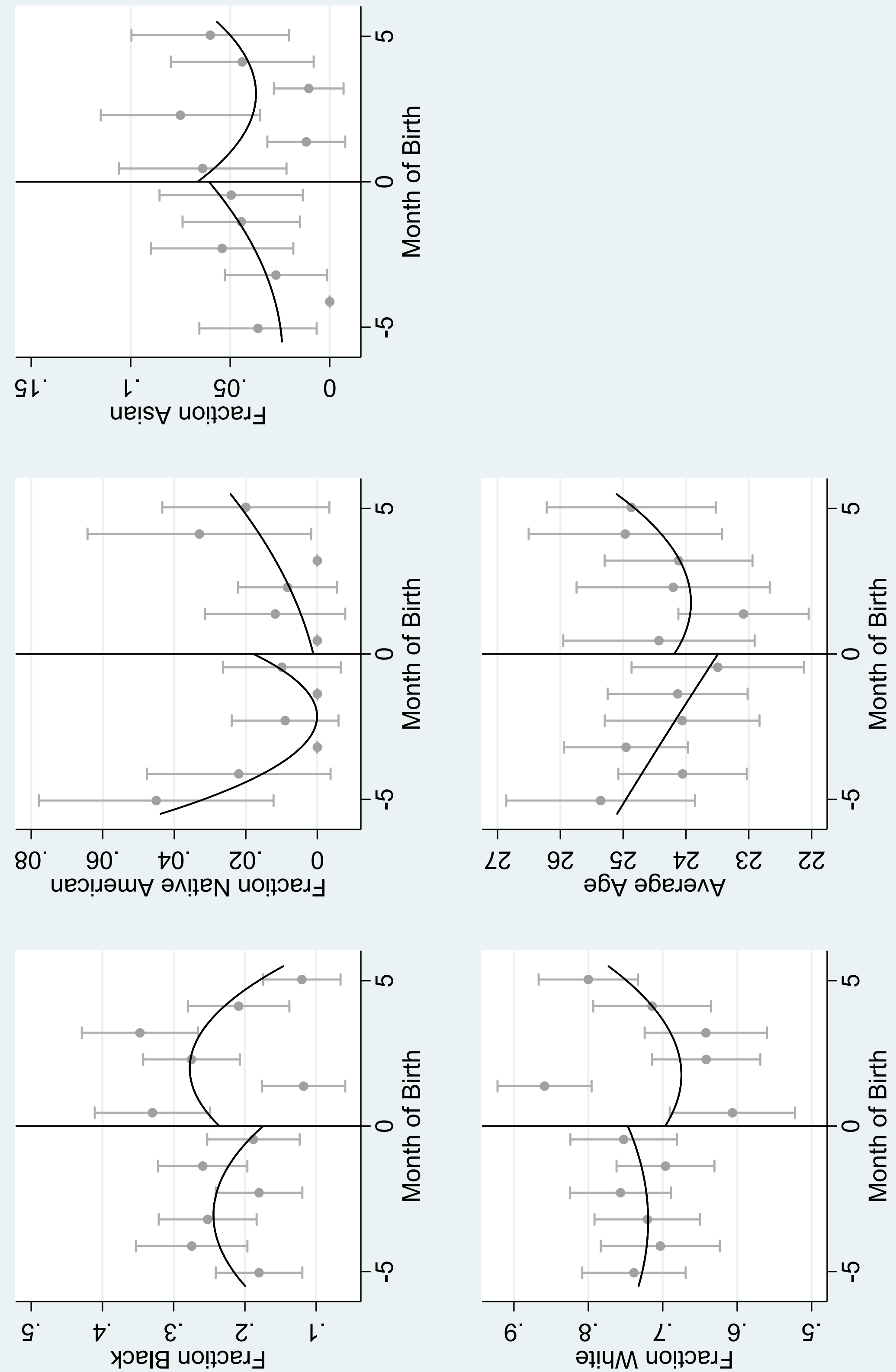
Figure 3.7: First/Only Birth Counts for Selected Groups of Women
Births
0
200
400
600
800
1000
Jul
Aug
Sept
Oct
Nov
Dec
Jan
Feb
Mar
Apr
May
Jun
Month of Birth
Married Low-ed Women
Married High-ed Women
Unmarried Low-ed Women
Unmarried High-ed Women
Data Source: Survey of Income and Program Participation, 1990-2012

Figure 3.8A: Covariate Balance Tests for All Unmarried Women by Month of First Birth



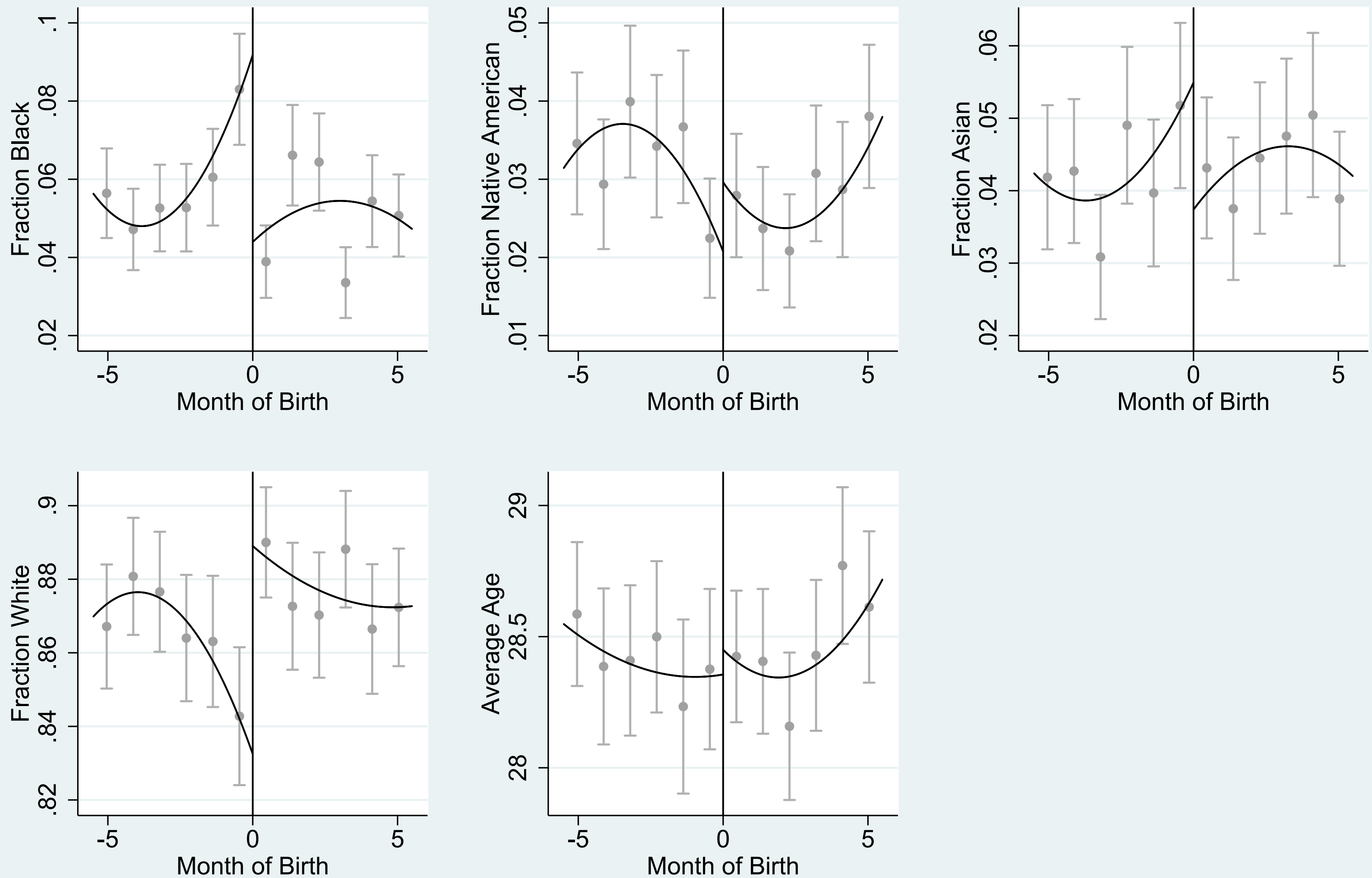
Data Source: Survey of Income and Program Participation, 1990-2012

Figure 3.8B: Covariate Balance Tests for Low-ed Unmarried Women by Month of First Birth



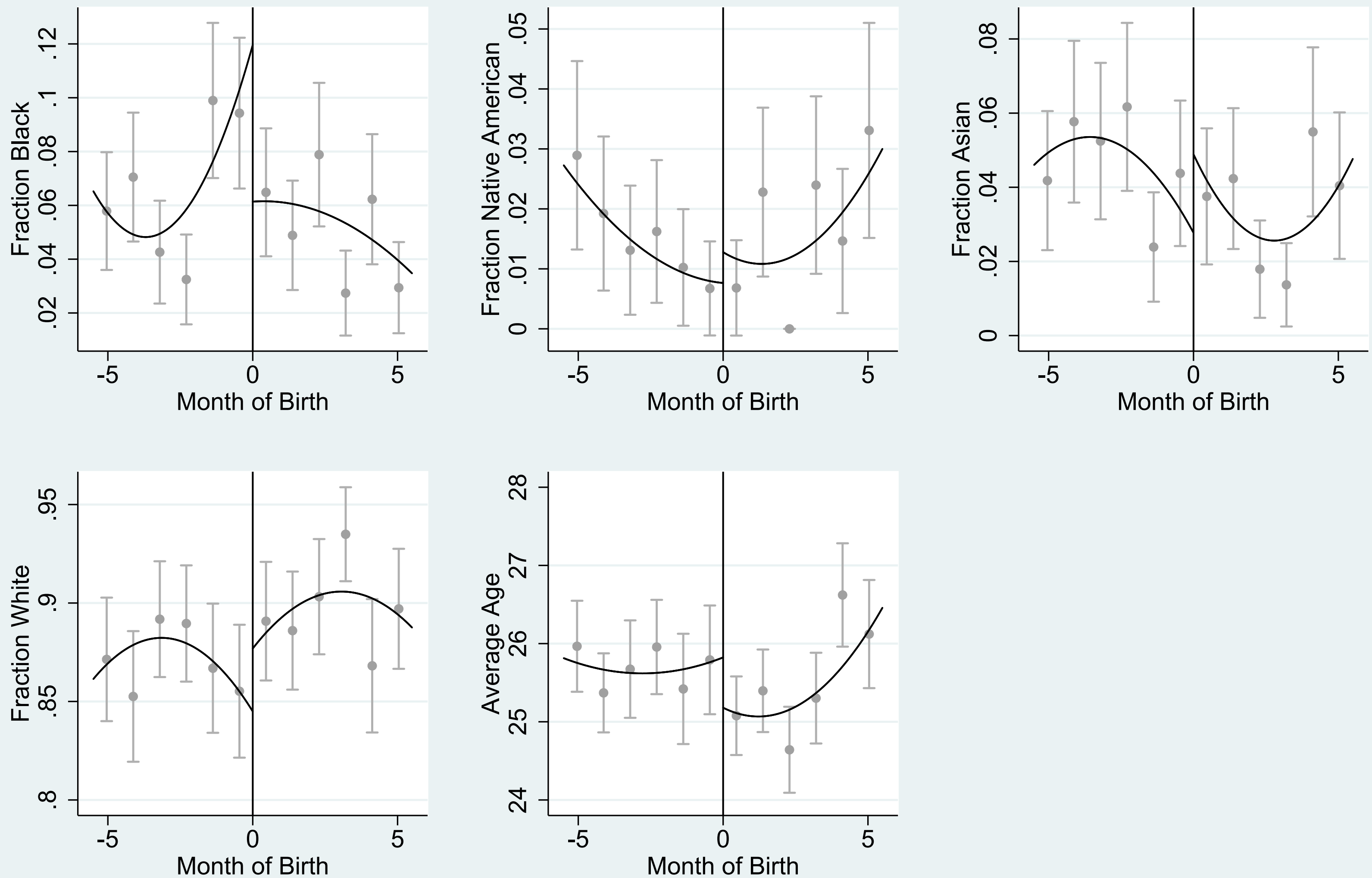
Data Source: Survey of Income and Program Participation, 1990-2012

Figure 3.8C: Covariate Balance Tests for All Married Women by Month of First Birth



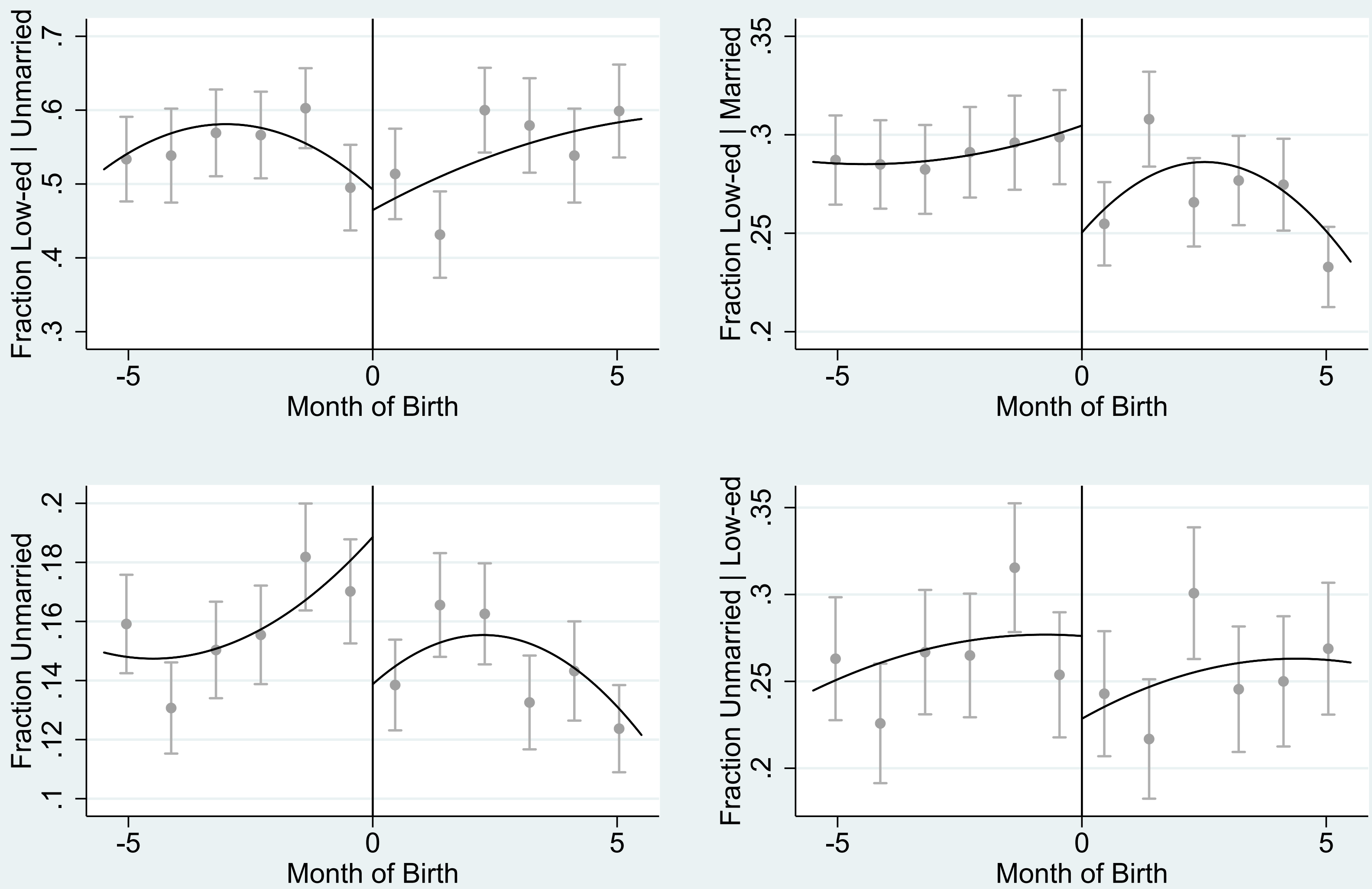
Data Source: Survey of Income and Program Participation, 1990-2012

Figure 3.8D: Covariate Balance Tests for Low-ed Married Women by Month of First Birth



Data Source: Survey of Income and Program Participation, 1990-2012

Figure 3.8E: Covariate Balance Tests for Low-ed and Marital Status by Month of First Birth



Data Source: Survey of Income and Program Participation, 1990-2012

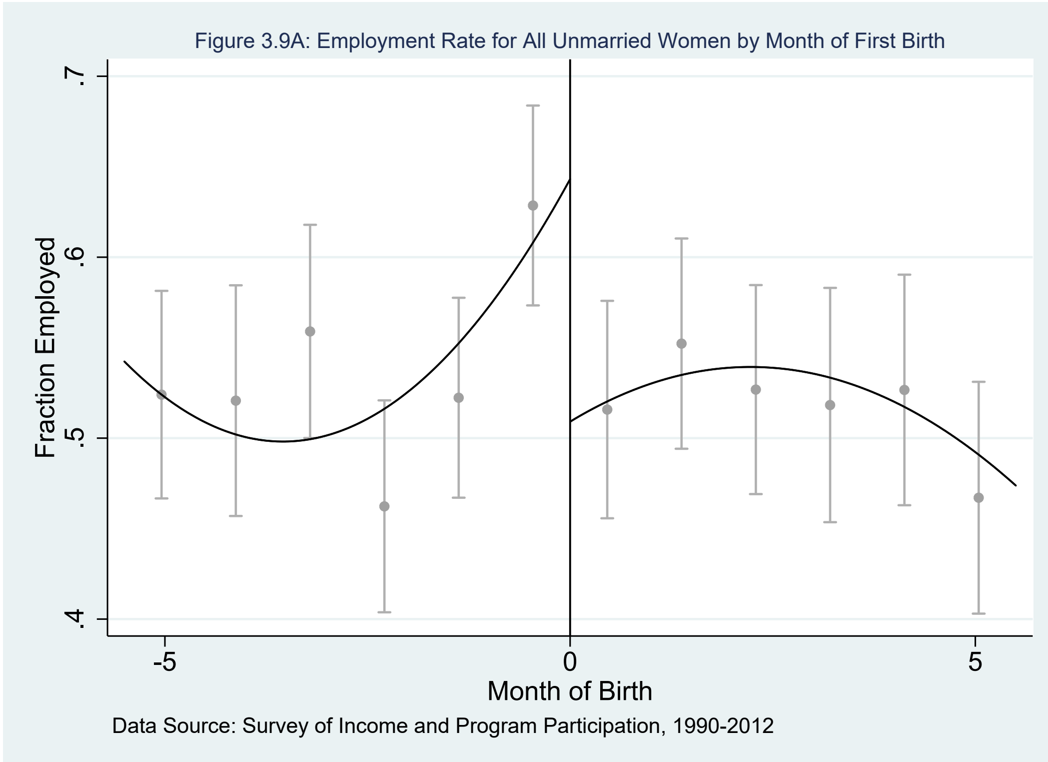


Figure 3.9A: Employment Rate for All Unmarried Women by Month of First Birth
Fraction Employed
.4
.5
.6
.7
-5
0
5
Month of Birth
Data Source: Survey of Income and Program Participation, 1990-2012

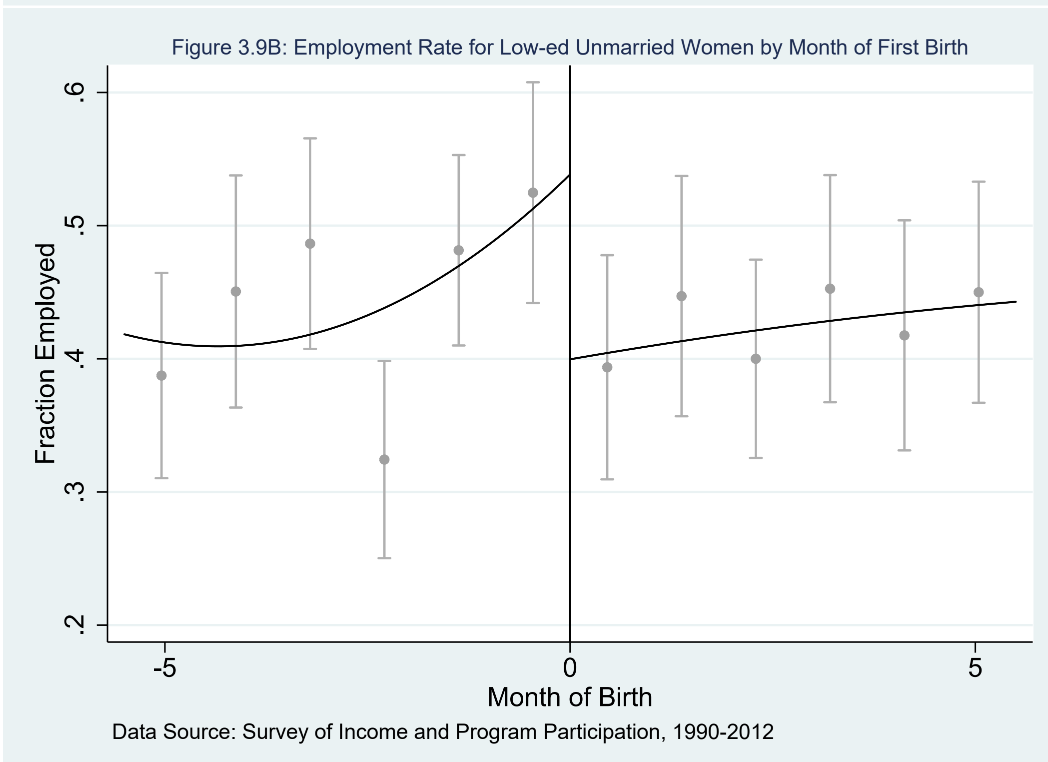


Figure 3.9B: Employment Rate for Low-ed Unmarried Women by Month of First Birth
Fraction Employed
.2
.3
.4
.5
.6
-5
0
5
Month of Birth
Data Source: Survey of Income and Program Participation, 1990-2012

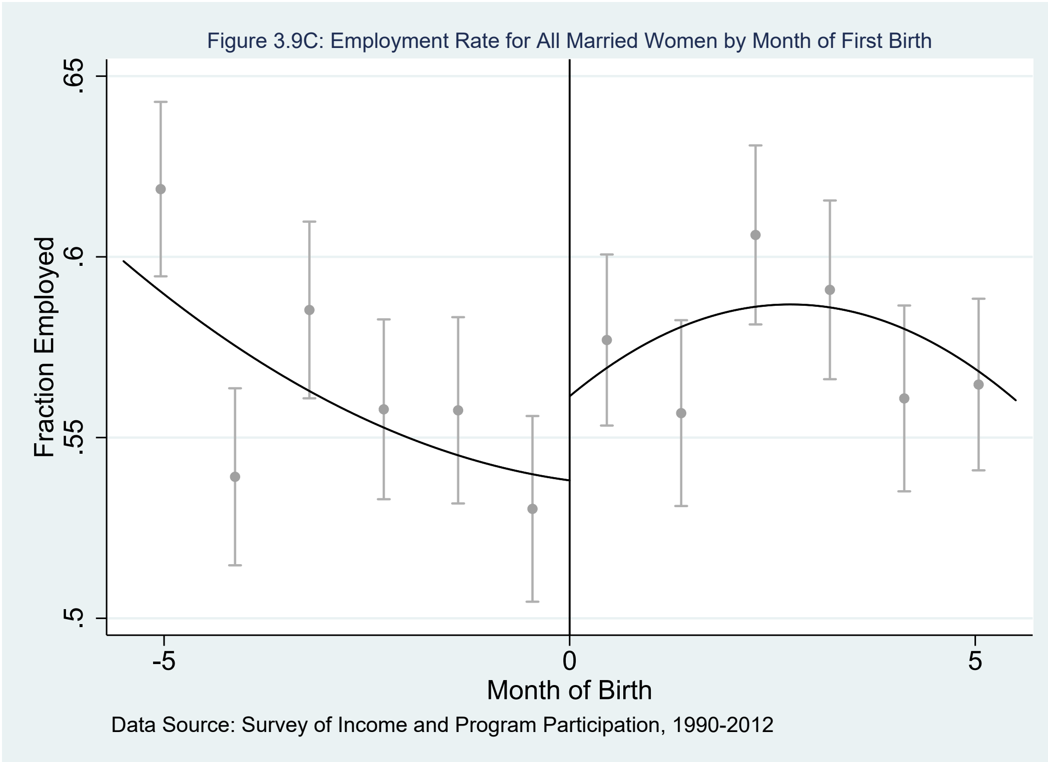


Figure 3.9C: Employment Rate for All Married Women by Month of First Birth
Fraction Employed
.5
.55
.6
.65
-5
0
5
Month of Birth
Data Source: Survey of Income and Program Participation, 1990-2012

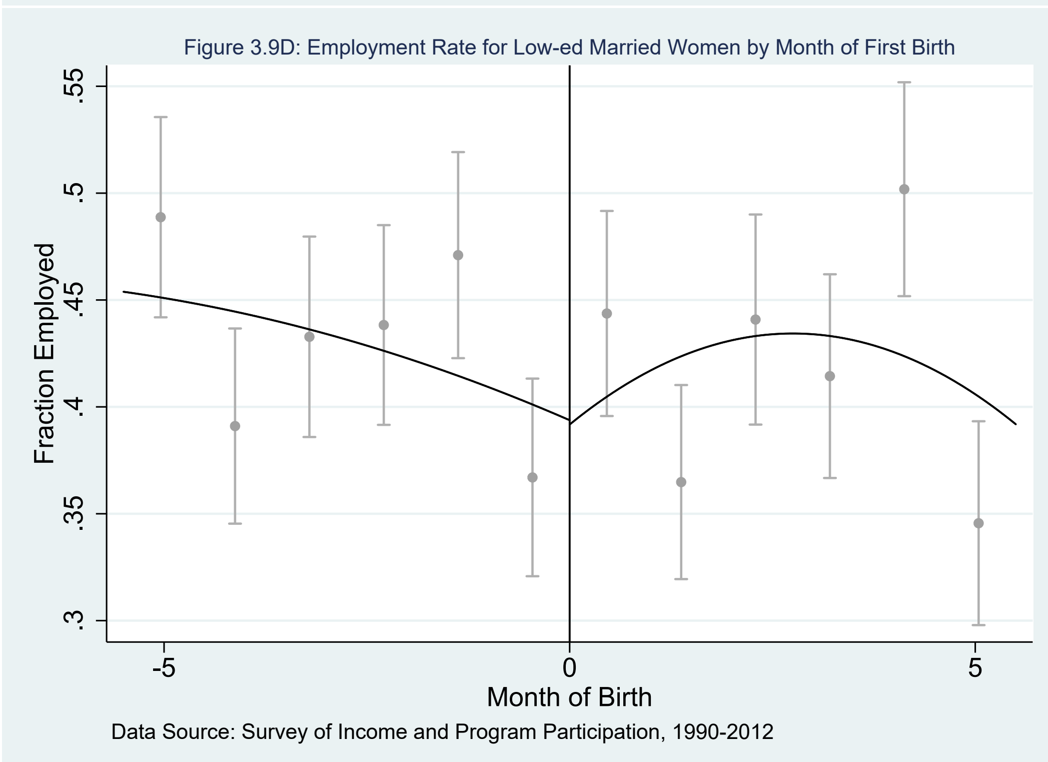
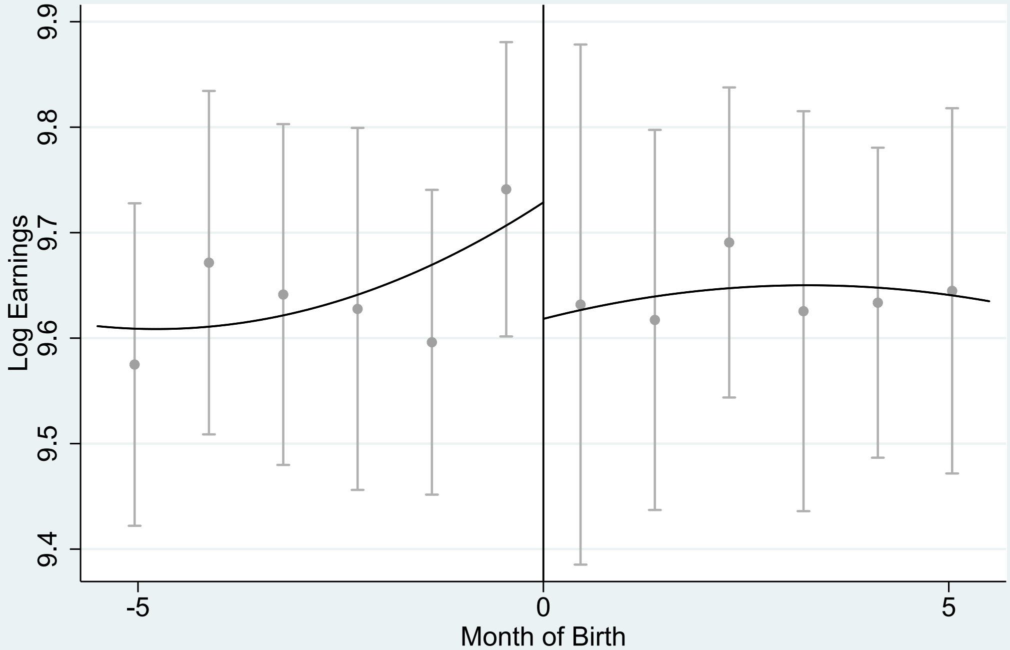


Figure 3.9D: Employment Rate for Low-ed Married Women by Month of First Birth
Fraction Employed
.3
.35
.4
.45
.5
.55
-5
0
5
Month of Birth
Data Source: Survey of Income and Program Participation, 1990-2012

Figure 3.10A: Log Earnings for All Unmarried Women by Month of First Birth



Data Source: Survey of Income and Program Participation, 1990-2012

Figure 3.10B: Log Earnings for Low-ed Unmarried Women by Month of First Birth

Data Source: Survey of Income and Program Participation, 1990-2012

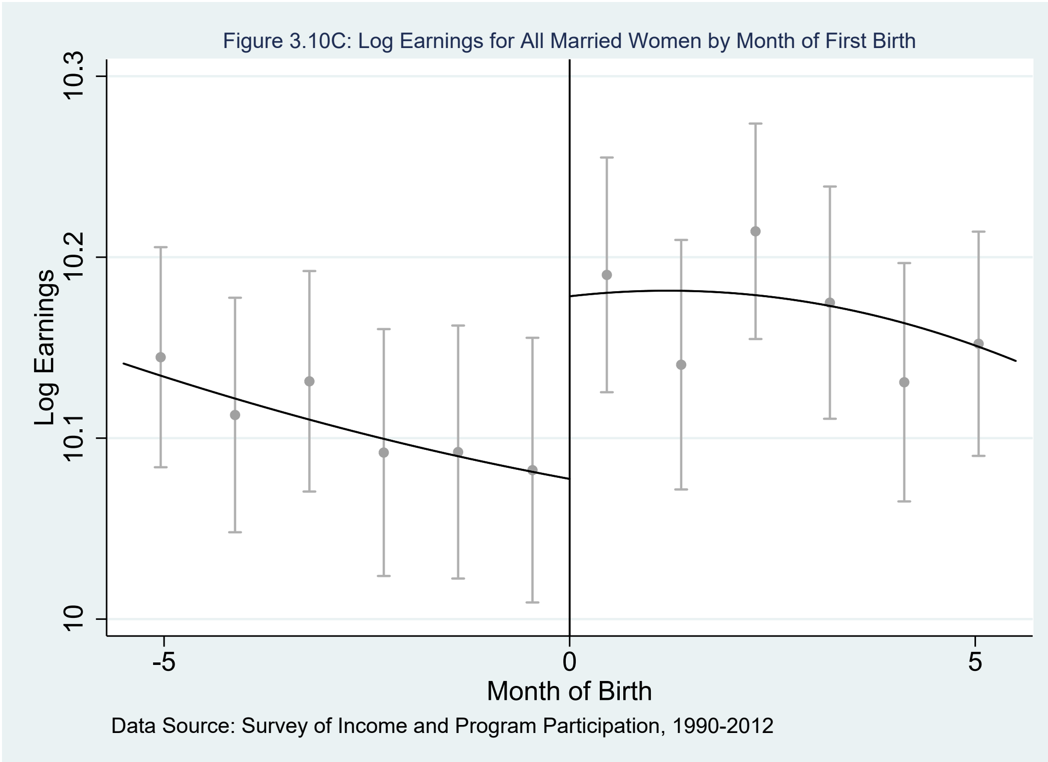


Figure 3.10C: Log Earnings for All Married Women by Month of First Birth
Log Earnings
10
10.1
10.2
10.3
-5
0
5
Month of Birth
Data Source: Survey of Income and Program Participation, 1990-2012

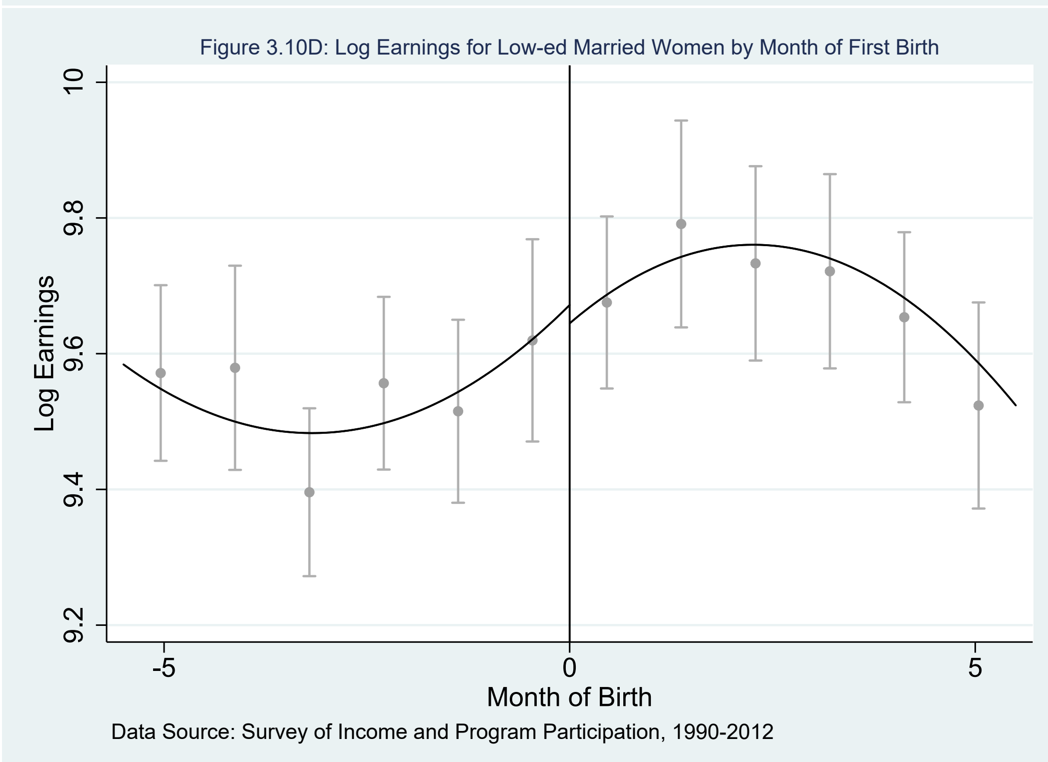
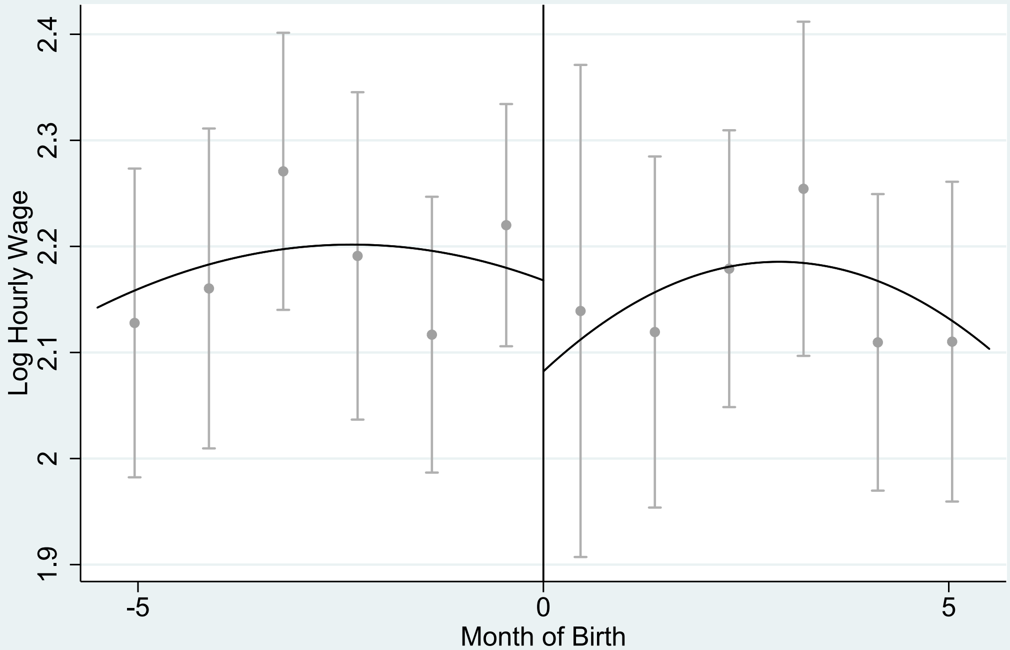


Figure 3.10D: Log Earnings for Low-ed Married Women by Month of First Birth
Log Earnings
9.2
9.4
9.6
9.8
10.0
-5
0
5
Month of Birth
Data Source: Survey of Income and Program Participation, 1990-2012

Figure 3.11A: Log Hourly Wage for All Unmarried Women by Month of First Birth



Data Source: Survey of Income and Program Participation, 1990-2012

Figure 3.11B: Log Hourly Wage for Low-ed Unmarried Women by Month of First Birth

Data Source: Survey of Income and Program Participation, 1990-2012

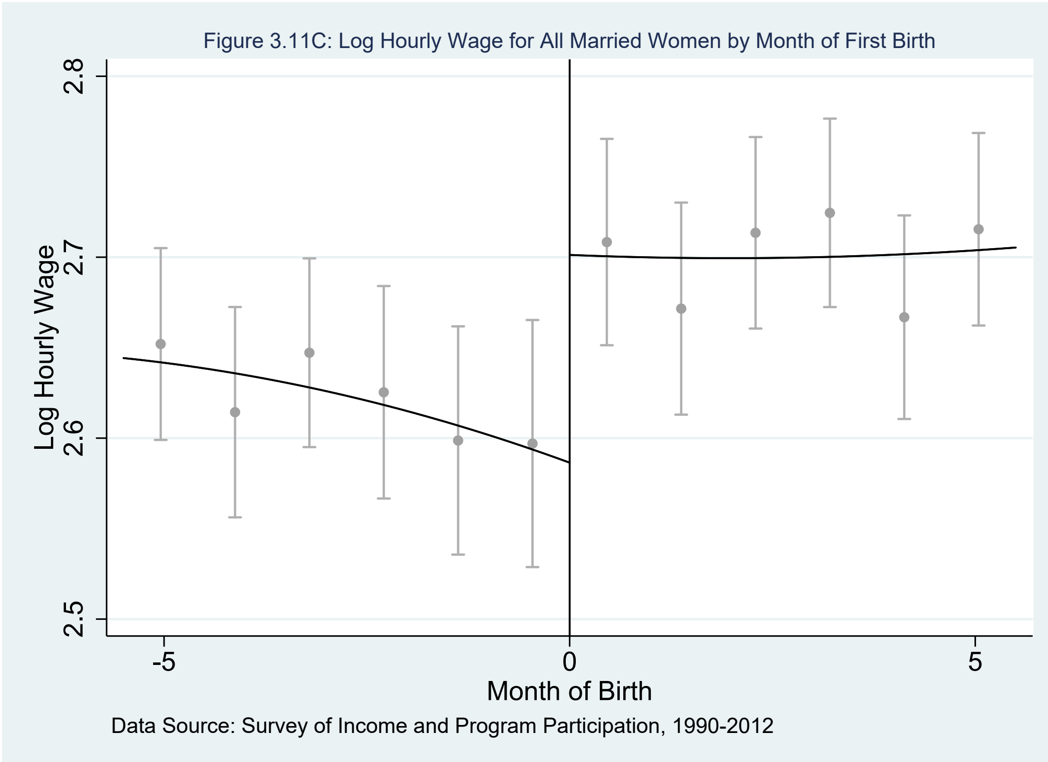


Figure 3.11C: Log Hourly Wage for All Married Women by Month of First Birth
Log Hourly Wage
2.5
2.6
2.7
2.8
-5
0
5
Month of Birth
Data Source: Survey of Income and Program Participation, 1990-2012

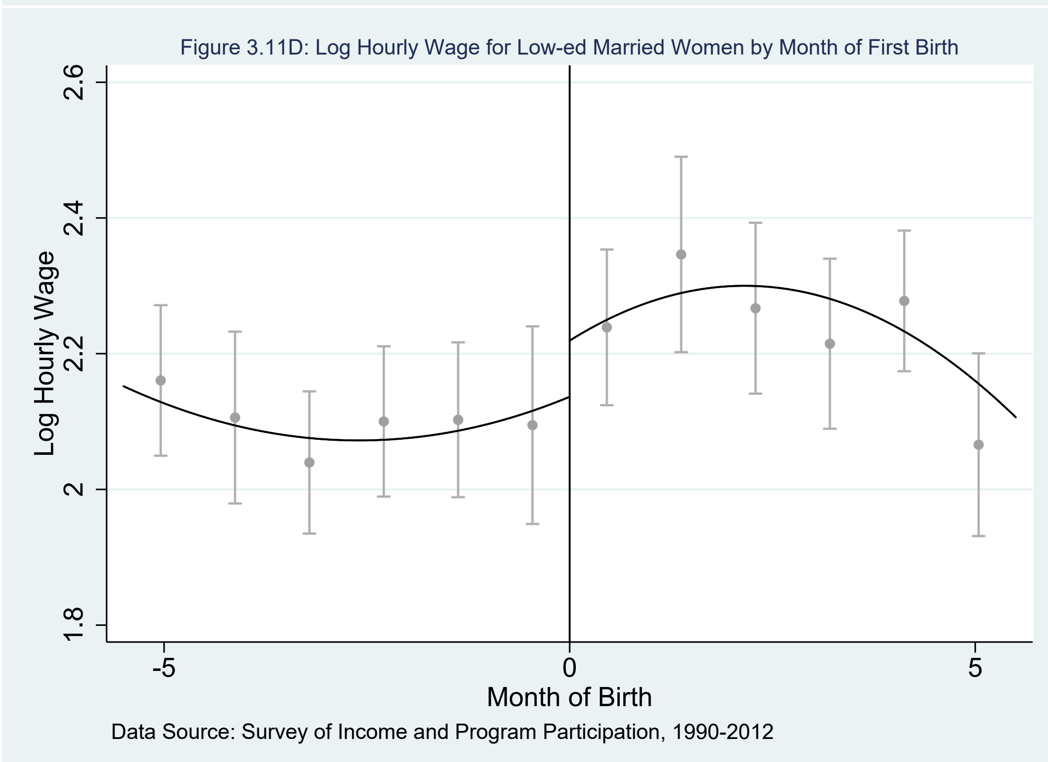


Figure 3.11D: Log Hourly Wage for Low-ed Married Women by Month of First Birth
Log Hourly Wage
1.8
2
2.2
2.4
2.6
-5
0
5
Month of Birth
Data Source: Survey of Income and Program Participation, 1990-2012

Table 3.1: Replication of Table 1 from Eissa and Liebman (1996) using the Panel Study of Income Dynamics

	Pre-TRA 86		Post-TRA 86		Difference		DiD	
	E & L	Replication	E & L	Replication	E & L	Replication	E & L	Replication
T: With children	0.729	0.768	0.753	0.782	0.024	0.015		
	(0.004)	(0.015)	(0.004)	(0.014)	(0.006)	(0.021)		
C: Without children	0.952	0.969	0.952	0.970	0.000	0.001	**0.024**	**0.014**
	(0.001)	(0.005)	(0.001)	(0.006)	(0.002)	(0.008)	**(0.006)**	**(0.022)**
T: With children and LTHS	0.479	0.571	0.497	0.615	0.018	0.044		
	(0.010)	(0.033)	(0.010)	(0.034)	(0.014)	(0.048)		
C1: Without kids and LTHS	0.784	0.648	0.761	0.819	-0.023	0.171	**0.041**	**-0.127**
	(0.010)	(0.076)	(0.009)	(0.055)	(0.013)	(0.094)	**(0.019)**	**(0.105)**
C2: With children and beyond HS	0.911	0.898	0.920	0.860	0.009	-0.038	**0.009**	**0.082**
	(0.005)	(0.020)	(0.005)	(0.025)	(0.007)	(0.032)	**(0.015)**	**(0.057)**
T: With children and HS	0.764	0.805	0.787	0.828	0.023	0.023		
	(0.006)	(0.021)	(0.006)	(0.019)	(0.008)	(0.029)		
C1: Without children and HS	0.945	0.963	0.943	0.958	-0.002	-0.006	**0.025**	**0.028**
	(0.002)	(0.009)	(0.003)	(0.011)	(0.004)	(0.015)	**(0.009)**	**(0.032)**
C2: With children and beyond HS	0.911	0.898	0.920	0.860	0.009	-0.038	**0.014**	**0.060**
	(0.005)	(0.020)	(0.005)	(0.025)	(0.007)	(0.032)	**(0.011)**	**(0.043)**

Sample sizes for each group are as follows for Eissa and Liebman (1996) and PSID replication, respectively, and include both pre- and post- observations. All women with children:20810, 3231. All women without children: 46287, 2265. Women with children and less than a high school degree: 5396, 928. Women without children and less than a high school degree: 3958, 175. Women with children and beyond a high school degree: 5712, 839. Women with children and a high school degree: 9702, 1409. Women without children and a high school degree: 16527, 894.

Table 3.2: Replication of Table III from Meyer and Rosenbaum (2001) using the Panel Study of Income Dynamics

	Meyer and Rosenbaum		Replication	
Explanatory Variable	Marginal Eff.	Sd. Error	Marginal Eff.	Std. Error
Any Children*1984	-0.1087	0.0160	-0.0762	0.1192
Any Children*1985	-0.0120	0.0156	-0.1021	0.1053
Any Children*1986	-0.1144	0.0153	-0.1736	0.1262
Any Children*1987	-0.1056	0.0144	-0.0949	0.1176
Any Children*1988	-0.0918	0.0140	-0.1283	0.0934
Any Children*1989	-0.0745	0.0131	-0.1739	0.0882
Any Children*1990	-0.0832	0.0136	-0.0932	0.0978
Any Children*1991	-0.0916	0.0151	-0.0645	0.0907
Any Children*1992	-0.0706	0.0159	-0.0160	0.0866
Any Children*1993	-0.0830	0.0153	0.0017	0.0776
Any Children*1994	-0.0388	0.0145	0.0054	0.0661
Any Children*1995	-0.0154	0.0143	0.0420	0.0528
Any Children*1996	0.0042	0.0140	-0.0180	0.0789
Nonwhite	-0.0727	0.0033	N/A	N/A
Hispanic	-0.0608	0.0033	N/A	N/A
Black	N/A	N/A	-0.0402	0.0176
Age 19-24	-0.0077	0.0055	0.0092	0.0122
Age 25-29	-0.0107	0.0095	-0.0002	0.0117
Age 35-39	0.0008	0.0052	0.0003	0.0125
Age 40-44	0.0107	0.0116	-0.0268	0.0148
High School Dropout	-0.1512	0.0032	-0.1594	0.0246
Some College	0.0989	0.0055	0.0829	0.0128
Bachelors	0.1755	0.0055	0.1281	0.0091
Masters	0.1927	0.0095	0.1374	0.0066
Divorced	0.0620	0.0052	0.0163	0.0293
Widowed	-0.1218	0.0116	-0.1165	0.0663
Any Children*Divorced	0.0720	0.0063	0.0601	0.0206
Any Children*Widowed	0.1148	0.0137	0.0806	0.0299
Number of Children Under 18	-0.0325	0.0020	-0.0462	0.0060
Number of Children Under 6	-0.0699	0.0027	-0.0569	0.0165
State Unemployment Rate	-0.0101	0.0015	-0.0145	0.0068
Any Children*State Unemployment Rate	0.0032	0.0017	0.0023	0.0089
Number of Observations	119,019		13,679	

This sample includes 19-44 year-old single women (divorced, widowed, or never married) who are not in school. Explanatory variables included in the regression but not reported in the Table are year and state fixed effects. "Employment Rate" is defined in this Table as having worked in the past year (i.e. annual hours greater than zero).

Table 3.3: Descriptive Statistics

Variable	All Unmarried Women	Low-ed Unmarried Women	All Married Women	Low-ed Married Women
Earnings	12,852	7,874	21,532	9,426
Employed	0.53	0.43	0.57	0.42
Hourly Wage	10.96	8.15	17.93	10.42
Fraction with Jan-Jun Birth	0.48	0.47	0.50	0.48
Fraction with Jul-Dec Birth	0.52	0.53	0.50	0.52
Phase-in Rate | Treatment	30.7	29.8	29.6	27.5
Fraction Low-ed	0.55	1	0.28	1
Month of Birth	6.63	6.72	6.45	6.55
Number of Observations	2,301	1,245	12,956	3,542

Data Source: Survey of Income and Program Participation, 1990-2012

Table 3.4: Effects of the Earned Income Tax Credit on Labor Market Outcomes, Baseline Regressions with a 0-1 Discontinuity and Without Controls

Functional Form	All Unmarried Women	Low-ed Unmarried Women	All Married Women	Low-ed Married Women
Panel A: Employment				
Linear	0.027	0.092	-0.053**	-0.026
	(0.056)	(0.076)	(0.021)	(0.050)
Quadratic	0.134	0.139	-0.023	0.002
	(0.098)	(0.125)	(0.028)	(0.065)
Panel B: Log Earnings				
Linear	0.063	0.114	-0.117*	-0.226*
	(0.125)	(0.172)	(0.064)	(0.116)
Quadratic	0.110	0.159	-0.101	0.027
	(0.251)	(0.342)	(0.096)	(0.187)
Panel C: Log Hourly Wage				
Linear	0.047	0.052	-0.105*	-0.245**
	(0.115)	(0.146)	(0.055)	(0.100)
Quadratic	0.086	0.082	-0.115	-0.083
	(0.225)	(0.281)	(0.081)	(0.149)

Data Source: Survey of Income and Program Participation, 1990-2012

Each cells shows the primary coefficient of interest from a separate regression.

Each cell shows the parameter of interest from a separate regression. All regressions also include time (month of birth) and time*second-half of the year birth (July-December). Quadratic functional form regressions also include time squared and time squared*second-half birth.

Employment is defined as having annualized earnings of at least $2,500.

Earnings are annualized and in 2013 dollars.

Log hourly wage is calculated from the quotient of annualized earnings (in 2013 dollars) and annualized hours.

Table 3.5: Effects of the Earned Income Tax Credit on Labor Market Outcomes without Controls

Functional Form	All Unmarried Women	Low-ed Unmarried Women	All Married Women	Low-ed Married Women
		Panel A: Employment		
Linear	0.028**	0.037	-0.012**	-0.031**
	(0.014)	(0.024)	(0.005)	(0.014)
Quadratic	0.037*	0.047*	-0.010	-0.022
	(0.020)	(0.025)	(0.007)	(0.021)
		Panel B: Log Earnings		
Linear	0.036	0.025	0.027	-0.056**
	(0.029)	(0.040)	(0.020)	(0.024)
Quadratic	0.032	0.083	0.005	-0.036
	(0.040)	(0.057)	(0.034)	(0.038)
		Panel C: Log Hourly Wage		
Linear	0.019	0.006	0.014	-0.065***
	(0.024)	(0.034)	(0.019)	(0.024)
Quadratic	0.004	0.034	-0.006	-0.059
	(0.037)	(0.052)	(0.030)	(0.035)

Data Source: Survey of Income and Program Participation, 1990-2012

Each cells shows the primary coefficient of interest from a separate regression.

Phase-in rates are coded on a 0 to 10 scale, so coefficients can be interpreted as the change in the outcome variable from a 10 percentage point increase in the phase-in rate.

Employment is defined as having annualized earnings of at least $2,500.

Earnings are annualized and in 2013 dollars.

Log hourly wage is calculated from the quotient of annualized earnings (in 2013 dollars) and annualized hours.

Table 3.6: Effects of the Earned Income Tax Credit on Labor Market Outcomes, Baseline Regressions

Functional Form	All Unmarried Women	Low-ed Unmarried Women	All Married Women	Low-ed Married Women
		Panel A: Employment		
Linear	0.153**	0.320***	-0.017	-0.094
	(0.067)	(0.084)	(0.031)	(0.084)
Quadratic	0.078	0.294**	-0.025	-0.155
	(0.083)	(0.111)	(0.036)	(0.097)
		Panel B: Log Earnings		
Linear	-0.288	-0.104	-0.053	-0.296
	(0.211)	(0.283)	(0.057)	(0.226)
Quadratic	-0.264	-0.091	-0.113	-0.607*
	(0.241)	(0.331)	(0.072)	(0.311)
		Panel C: Log Hourly Wage		
Linear	-0.229	0.005	-0.042	-0.100
	(0.235)	(0.221)	(0.058)	(0.177)
Quadratic	-0.253	0.032	-0.083	-0.341
	(0.255)	(0.248)	(0.074)	(0.246)

Data Source: Survey of Income and Program Participation, 1990-2012

Each cells shows the primary coefficient of interest from a separate regression.

Each cell shows the parameter of interest from a separate regression. Phase-in rates are coded on a 0 to 10 scale, so coefficients can be interpreted as the change in the outcome variable from a 10 percentage point increase in the phase-in rate. All regressions include time (month of birth), time*Phase-in rate, race dummies, personal exemption control, difference between head of household and single filer standard deduction control, and state and cohort dummies. Quadratic functional form regressions also include time squared and time squared*Phase-in rate.

Employment is defined as having annualized earnings of at least $2,500.

Earnings are annualized and in 2013 dollars.

Log hourly wage is calculated from the quotient of annualized earnings (in 2013 dollars) and annualized hours.

Table 3.7: Effects of the Earned Income Tax Credit on Labor Market Outcomes, December and January Births Only

Functional Form	All Unmarried Women	Low-ed Unmarried Women	All Married Women	Low-ed Married Women
		Panel A: Employment		
N/A	-0.016	0.493	0.010	-0.377
	(0.225)	(0.365)	(0.115)	(0.272)
		Panel B: Log Earnings		
N/A	0.554	8.666	0.487	-1.397
	(1.166)	(7.827)	(0.431)	(1.021)
		Panel C: Log Hourly Wage		
N/A	0.315	11.737	0.311	-1.758*
	(1.089)	(10.288)	(0.354)	(1.038)

Data Source: Survey of Income and Program Participation, 1990-2012

Each cells shows the primary coefficient of interest from a separate regression.

Each cell shows the parameter of interest from a separate regression. Phase-in rates are coded on a 0 to 10 scale, so coefficients can be interpreted as the change in the outcome variable from a 10 percentage point increase in the phase-in rate. All regressions include race dummies, personal exemption control, difference between head of household and single filer standard deduction control, and state and cohort dummies.

Employment is defined as having annualized earnings of at least $2,500.

Earnings are annualized and in 2013 dollars.

Log hourly wage is calculated from the quotient of annualized earnings (in 2013 dollars) and annualized hours.

Table 3.8: Effects of the Earned Income Tax Credit on Labor Market Outcomes, November-February Births Only

Functional Form	All Unmarried Women	Low-ed Unmarried Women	All Married Women	Low-ed Married Women
		Panel A: Employment		
Linear	-0.066	0.370	0.002	-0.206
	(0.119)	(0.223)	(0.047)	(0.239)
		Panel B: Log Earnings		
Linear	0.013	-0.268	-0.091	-1.533*
	(0.304)	(0.623)	(0.145)	(0.874)
		Panel C: Log Hourly Wage		
Linear	0.156	-0.166	-0.058	-1.256
	(0.276)	(0.417)	(0.127)	(0.769)

Data Source: Survey of Income and Program Participation, 1990-2012

Each cells shows the primary coefficient of interest from a separate regression.

Each cell shows the parameter of interest from a separate regression. Phase-in rates are coded on a 0 to 10 scale, so coefficients can be interpreted as the change in the outcome variable from a 10 percentage point increase in the phase-in rate. All regressions include time (month of birth), time*Phase-in rate, race dummies, personal exemption control, difference between head of household and single filer standard deduction control, and state and cohort dummies.

Employment is defined as having annualized earnings of at least $2,500.

Earnings are annualized and in 2013 dollars.

Log hourly wage is calculated from the quotient of annualized earnings (in 2013 dollars) and annualized hours.

Table 3.9: Effects of the Earned Income Tax Credit on Labor Market Outcomes, Excluding December and January Births

Functional Form	All Unmarried Women	Low-ed Unmarried Women	All Married Women	Low-ed Married Women
		Panel A: Employment		
Linear	0.231*	0.334**	-0.035	-0.132
	(0.118)	(0.139)	(0.032)	(0.084)
Quadratic	0.198	0.360**	-0.032	-0.312**
	(0.185)	(0.174)	(0.057)	(0.123)
		Panel B: Log Earnings		
Linear	-0.275	-0.333	-0.081	-0.285
	(0.193)	(0.389)	(0.087)	(0.304)
Quadratic	-0.329	-0.281	-0.316**	-0.651
	(0.251)	(0.632)	(0.155)	(0.417)
		Panel C: Log Hourly Wage		
Linear	-0.130	-0.085	-0.075	0.078
	(0.214)	(0.316)	(0.090)	(0.225)
Quadratic	-0.214	0.023	-0.262*	-0.090
	(0.248)	(0.510)	(0.152)	(0.385)

Data Source: Survey of Income and Program Participation, 1990-2012

Each cells shows the primary coefficient of interest from a separate regression.

Each cell shows the parameter of interest from a separate regression. Phase-in rates are coded on a 0 to 10 scale, so coefficients can be interpreted as the change in the outcome variable from a 10 percentage point increase in the phase-in rate. All regressions include time (month of birth), time*Phase-in rate, race dummies, personal exemption control, difference between head of household and single filer standard deduction control, and state and cohort dummies. Quadratic functional form regressions also include time squared and time squared*Phase-in rate.

Employment is defined as having annualized earnings of at least $2,500.

Earnings are annualized and in 2013 dollars.

Log hourly wage is calculated from the quotient of annualized earnings (in 2013 dollars) and annualized hours.

Table 3.10: Effects of the Earned Income Tax Credit on Labor Market Outcomes, 1 to 2 Child Placebo Test

Functional Form	All Unmarried Women	Low-ed Unmarried Women	All Married Women	Low-ed Married Women
		Panel A: Employment		
Linear	0.159	0.102	0.117**	0.180
	(0.119)	(0.161)	(0.037)	(0.133)
Quadratic	0.201	0.150	0.166***	0.152
	(0.201)	(0.254)	(0.046)	(0.199)
		Panel B: Log Earnings		
Linear	0.192	-0.148	0.058	0.191
	(0.275)	(0.354)	(0.132)	(0.206)
Quadratic	0.322	-0.245	0.164	0.051
	(0.340)	(0.455)	(0.216)	(0.455)
		Panel C: Log Hourly Wage		
Linear	0.146	0.125	0.057	0.136
	(0.246)	(0.364)	(0.105)	(0.159)
Quadratic	0.396	0.348	0.135	0.167
	(0.279)	(0.491)	(0.185)	(0.348)

Data Source: Survey of Income and Program Participation, 1990-2012

Each cells shows the primary coefficient of interest from a separate regression.

Each cell shows the parameter of interest from a separate regression. Phase-in rates are coded on a 0 to 10 scale, so coefficients can be interpreted as the change in the outcome variable from a 10 percentage point increase in the phase-in rate. All regressions include time (month of birth), time*Phase-in rate, race dummies, personal exemption control, difference between head of household and single filer standard deduction control, and state and cohort dummies. Quadratic functional form regressions also include time squared and time squared*Phase-in rate.

Employment is defined as having annualized earnings of at least $2,500.

Earnings are annualized and in 2013 dollars.

Log hourly wage is calculated from the quotient of annualized earnings (in 2013 dollars) and annualized hours.

Table 3.11: Effects of the Earned Income Tax Credit on Labor Market Outcomes, Outcomes when Infants Less Than 6 Mos.

Functional Form	All Unmarried Women	Low-ed Unmarried Women	All Married Women	Low-ed Married Women
		Panel A: Employment		
Linear	0.200**	0.343***	0.003	-0.150
	(0.082)	(0.095)	(0.039)	(0.090)
Quadratic	0.107	0.399***	0.023	-0.178
	(0.093)	(0.115)	(0.051)	(0.107)
		Panel B: Log Earnings		
Linear	-0.504**	-0.476	-0.130*	-0.571*
	(0.242)	(0.345)	(0.074)	(0.309)
Quadratic	-0.617*	-0.496	-0.192**	-0.976**
	(0.365)	(0.355)	(0.092)	(0.405)
		Panel C: Log Hourly Wage		
Linear	-0.349	-0.084	-0.120	-0.602**
	(0.216)	(0.343)	(0.080)	(0.292)
Quadratic	-0.469	-0.198	-0.149	-0.867**
	(0.346)	(0.365)	(0.100)	(0.367)

Data Source: Survey of Income and Program Participation, 1990-2012

Each cells shows the primary coefficient of interest from a separate regression.

Each cell shows the parameter of interest from a separate regression. Phase-in rates are coded on a 0 to 10 scale, so coefficients can be interpreted as the change in the outcome variable from a 10 percentage point increase in the phase-in rate. All regressions include time (month of birth), time*Phase-in rate, race dummies, personal exemption control, difference between head of household and single filer standard deduction control, and state and cohort dummies. Quadratic functional form regressions also include time squared and time squared*Phase-in rate.

Employment is defined as having annualized earnings of at least $2,500.

Earnings are annualized and in 2013 dollars.

Log hourly wage is calculated from the quotient of annualized earnings (in 2013 dollars) and annualized hours.

Table 3.12: Effects of the Earned Income Tax Credit on Labor Market Outcomes, Outcomes when Infants 6 to 11 Mos. Old

Functional Form	All Unmarried Women	Low-ed Unmarried Women	All Married Women	Low-ed Married Women
		Panel A: Employment		
Linear	0.048	0.244**	-0.035	-0.031
	(0.068)	(0.103)	(0.039)	(0.116)
Quadratic	-0.013	0.123	-0.041	-0.076
	(0.078)	(0.140)	(0.039)	(0.112)
		Panel B: Log Earnings		
Linear	-0.460***	-0.495**	0.010	0.139
	(0.146)	(0.244)	(0.071)	(0.311)
Quadratic	-0.376*	-0.522	-0.067	-0.199
	(0.217)	(0.377)	(0.090)	(0.354)
		Panel C: Log Hourly Wage		
Linear	-0.306*	-0.307	0.051	0.407**
	(0.157)	(0.206)	(0.048)	(0.186)
Quadratic	-0.260	-0.173	0.005	0.113
	(0.242)	(0.309)	(0.074)	(0.223)

Data Source: Survey of Income and Program Participation, 1990-2012

Each cells shows the primary coefficient of interest from a separate regression.

Each cell shows the parameter of interest from a separate regression. Phase-in rates are coded on a 0 to 10 scale, so coefficients can be interpreted as the change in the outcome variable from a 10 percentage point increase in the phase-in rate. All regressions include time (month of birth), time*Phase-in rate, race dummies, personal exemption control, difference between head of household and single filer standard deduction control, and state and cohort dummies. Quadratic functional form regressions also include time squared and time squared*Phase-in rate.

Employment is defined as having annualized earnings of at least $2,500.

Earnings are annualized and in 2013 dollars.

Log hourly wage is calculated from the quotient of annualized earnings (in 2013 dollars) and annualized hours.

Table 3.13: Effects of the Earned Income Tax Credit on Labor Market Outcomes, Baseline Regressions Weighted

Functional Form	All Unmarried Women	Low-ed Unmarried Women	All Married Women	Low-ed Married Women
		Panel A: Employment		
Linear	0.205**	0.280***	-0023	-0.167*
	(0.090)	(0.099)	(0.030)	(0.099)
Quadratic	0.160	0.255*	-0.014	-0.222**
	(0.099)	(0.138)	(0.034)	(0.106)
		Panel B: Log Earnings		
Linear	-0.275	-0.248	-0.027	-0.241
	(0.243)	(0.257)	(0.076)	(0.300)
Quadratic	-0.296	-0.283	-0.065	-0.546
	(0.259)	(0.270)	(0.088)	(0.360)
		Panel C: Log Hourly Wage		
Linear	-0.193	-0.030	-0.007	-0.0004
	(0.248)	(0.215)	(0.069)	(0.188)
Quadratic	-0.238	-0.042	-0.025	-0.243
	(0.268)	(0.213)	(0.083)	(0.235)

Data Source: Survey of Income and Program Participation, 1990-2012

Each cells shows the primary coefficient of interest from a separate regression.

Each cell shows the parameter of interest from a separate regression. Phase-in rates are coded on a 0 to 10 scale, so coefficients can be interpreted as the change in the outcome variable from a 10 percentage point increase in the phase-in rate. All regressions include time (month of birth), time*Phase-in rate, race dummies, personal exemption control, difference between head of household and single filer standard deduction control, and state and cohort dummies. Quadratic functional form regressions also include time squared and time squared*Phase-in rate

Employment is defined as having annualized earnings of at least $2,500.

Earnings are annualized and in 2013 dollars.

Log hourly wage is calculated from the quotient of annualized earnings (in 2013 dollars) and annualized hours.

Table 3.14: Effects of the Earned Income Tax Credit on Labor Market Outcomes, Alternate Outcome Variables

Functional Form	All Unmarried Women	Low-ed Unmarried Women	All Married Women	Low-ed Married Women
		Panel A: Employment (Earnings≥\$0)		
Linear	0.176**	0.292***	-0.014	-0.103
	(0.071)	(0.082)	(0.032)	(0.080)
Quadratic	0.097	0.257**	-0.022	-0.166*
	(0.083)	(0.109)	(0.036)	(0.093)
		Panel B: Employment (Earnings≥\$5,000)		
Linear	0.162**	0.250***	-0.033	-0.119
	(0.076)	(0.089)	(0.030)	(0.087)
Quadratic	0.084	0.221*	-0.046	-0.186*
	(0.085)	(0.118)	(0.033)	(0.101)
		Panel C: Trimmed Log Earnings		
Linear	-0.251	-0.216	-0.077	-0.416
	(0.286)	(0.297)	(0.049)	(0.292)
Quadratic	-0.283	-0.189	-0.174**	-0.623*
	(0.303)	(0.353)	(0.066)	(0.342)

Data Source: Survey of Income and Program Participation, 1990-2012

Each cells shows the primary coefficient of interest from a separate regression.

Each cell shows the parameter of interest from a separate regression. Phase-in rates are coded on a 0 to 10 scale, so coefficients can be interpreted as the change in the outcome variable from a 10 percentage point increase in the phase-in rate. All regressions include time (month of birth), time*Phase-in rate, race dummies, personal exemption control, difference between head of household and single filer standard deduction control, and state and cohort dummies. Quadratic functional form regressions also include time squared and time squared*Phase-in rate

Employment is defined as having annualized earnings greater than zero in Panel A and $5,000 in Panel B.

Trimmed Log Earnings Regressions exclude the top decile of earners in each subsample.

Earnings are annualized and in 2013 dollars.

Log hourly wage is calculated from the quotient of annualized earnings (in 2013 dollars) and annualized hours.

Table 3.15: Effects of the Earned Income Tax Credit on Labor Market Outcomes, October to March Births Only

Functional Form	All Unmarried Women	Low-ed Unmarried Women	All Married Women	Low-ed Married Women
		Panel A: Employment		
Linear	0.038	0.247	0.013	-0.227
	(0.099)	(0.150)	(0.040)	(0.162)
Quadratic	-0.010	0.157	0.058	-0.112
	(0.108)	(0.167)	(0.049)	(0.198)
		Panel B: Log Earnings		
Linear	-0.212	-0.046	-0.105	-1.661***
	(0.273)	(0.572)	(0.121)	(0.524)
Quadratic	-0.093	-0.061	0.125	-1.749***
	(0.350)	(0.571)	(0.140)	(0.620)
		Panel C: Log Hourly Wage		
Linear	-0.172	0.016	-0.074	-1.387**
	(0.287)	(0.413)	(0.124)	(0.552)
Quadratic	-0.068	-0.053	0.090	-1.481**
	(0.370)	(0.456)	(0.142)	(0.651)

Data Source: Survey of Income and Program Participation, 1990-2012

Each cells shows the primary coefficient of interest from a separate regression.

Each cell shows the parameter of interest from a separate regression. Phase-in rates are coded on a 0 to 10 scale, so coefficients can be interpreted as the change in the outcome variable from a 10 percentage point increase in the phase-in rate. All regressions include time (month of birth), time*Phase-in rate, race dummies, personal exemption control, difference between head of household and single filer standard deduction control, and state and cohort dummies. Quadratic functional form regressions also include time squared and time squared*Phase-in rate.

Employment is defined as having annualized earnings of at least $2,500.

Earnings are annualized and in 2013 dollars.

Log hourly wage is calculated from the quotient of annualized earnings (in 2013 dollars) and annualized hours.

Table 3.16: Effects of the Earned Income Tax Credit on Labor Market Outcomes, September to April Births Only

Functional Form	All Unmarried Women	Low-ed Unmarried Women	All Married Women	Low-ed Married Women
		Panel A: Employment		
Linear	0.121	0.243**	-0.052*	-0.192
	(0.078)	(0.109)	(0.030)	(0.118)
Quadratic	0.029	0.208	-0.050	-0.199
	(0.086)	(0.133)	(0.035)	(0.138)
		Panel B: Log Earnings		
Linear	-0.272	-0.218	-0.151	-1.175**
	(0.236)	(0.355)	(0.108)	(0.448)
Quadratic	-0.410	-0.344	-0.076	-1.323**
	(0.313)	(0.383)	(0.102)	(0.496)
		Panel C: Log Hourly Wage		
Linear	-0.189	-0.062	-0.140	-0.731*
	(0.260)	(0.264)	(0.102)	(0.394)
Quadratic	-0.304	-0.212	-0.042	-0.861*
	(0.346)	(0.293)	(0.103)	(0.430)

Data Source: Survey of Income and Program Participation, 1990-2012

Each cells shows the primary coefficient of interest from a separate regression.

Each cell shows the parameter of interest from a separate regression. Phase-in rates are coded on a 0 to 10 scale, so coefficients can be interpreted as the change in the outcome variable from a 10 percentage point increase in the phase-in rate. All regressions include time (month of birth), time*Phase-in rate, race dummies, personal exemption control, difference between head of household and single filer standard deduction control, and state and cohort dummies. Quadratic functional form regressions also include time squared and time squared*Phase-in rate.

Employment is defined as having annualized earnings of at least $2,500.

Earnings are annualized and in 2013 dollars.

Log hourly wage is calculated from the quotient of annualized earnings (in 2013 dollars) and annualized hours.

TABLE 3.17: EFFECTS OF THE EARNED INCOME TAX CREDIT ON LABOR MARKET OUTCOMES, AUGUST TO MAY BIRTHS ONLY

Functional Form	All Unmarried Women	Low-ed Unmarried Women	All Married Women	Low-ed Married Women
		Panel A: Employment		
Linear	0.106	0.301***	-0.033	-0.250**
	(0.084)	(0.103)	(0.028)	(0.103)
Quadratic	0.058	0.223**	-0.065*	-0.189
	(0.090)	(0.109)	(0.037)	(0.116)
		Panel B: Log Earnings		
Linear	-0.228	-0.032	-0.058	-0.615**
	(0.202)	(0.308)	(0.074)	(0.290)
Quadratic	-0.292	-0.130	-0.178	-0.867**
	(0.269)	(0.367)	(0.088)	(0.345)
		Panel C: Log Hourly Wage		
Linear	-0.194	0.064	-0.055	-0.324
	(0.219)	(0.244)	(0.077)	(0.281)
Quadratic	-0.266	0.048	-0.131	-0.522
	(0.272)	(0.279)	(0.093)	(0.317)

Data Source: Survey of Income and Program Participation, 1990-2012

Each cells shows the primary coefficient of interest from a separate regression.

Each cell shows the parameter of interest from a separate regression. Phase-in rates are coded on a 0 to 10 scale, so coefficients can be interpreted as the change in the outcome variable from a 10 percentage point increase in the phase-in rate. All regressions include time (month of birth), time*Phase-in rate, race dummies, personal exemption control, difference between head of household and single filer standard deduction control, and state and cohort dummies. Quadratic functional form regressions also include time squared and time squared*Phase-in rate.

Employment is defined as having annualized earnings of at least $2,500.

Earnings are annualized and in 2013 dollars.

Log hourly wage is calculated from the quotient of annualized earnings (in 2013 dollars) and annualized hours.

Table 3.18: Effects of the Earned Income Tax Credit on Labor Market Outcomes, Baseline with a 0-1 Discontinuity

Functional Form	All Unmarried Women	Low-ed Unmarried Women	All Married Women	Low-ed Married Women
Panel A: Employment				
Linear	0.018	0.061	-0.043*	-0.026
	(0.054)	(0.077)	(0.023)	(0.048)
Quadratic	0.121	0.172	-0.018	0.015
	(0.097)	(0.136)	(0.032)	(0.064)
Panel B: Log Earnings				
Linear	0.049	-0.168	-0.058	-0.110
	(0.097)	(0.180)	(0.059)	(0.126)
Quadratic	0.066	-0.159	-0.067	0.251
	(0.212)	(0.345)	(0.093)	(0.184)
Panel C: Log Hourly Wage				
Linear	0.066	-0.133	-0.046	-0.124
	(0.085)	(0.146)	(0.052)	(0.102)
Quadratic	0.065	-0.211	-0.076	0.085
	(0.175)	(0.271)	(0.082)	(0.163)

Data Source: Survey of Income and Program Participation, 1990-2012

Each cells shows the primary coefficient of interest from a separate regression.

Each cell shows the parameter of interest from a separate regression. All regressions include time (month of birth), time*second-half of the year birth (July-December), race dummies, and state and cohort dummies. Quadratic functional form regressions also include time squared and time squared*second-half birth.

Employment is defined as having annualized earnings of at least $2,500.

Earnings are annualized and in 2013 dollars.

Log hourly wage is calculated from the quotient of annualized earnings (in 2013 dollars) and annualized hours.

Table 3.19: Effects of the Earned Income Tax Credit on Labor Market Outcomes, Baseline without Other Tax Policies

Functional Form	All Unmarried Women	Low-ed Unmarried Women	All Married Women	Low-ed Married Women
		Panel A: Employment		
Linear	0.015	0.030	-0.011*	-0.010
	(0.014)	(0.024)	(0.006)	(0.016)
Quadratic	0.026	0.050*	-0.010	-0.007
	(0.019)	(0.028)	(0.009)	(0.022)
		Panel B: Log Earnings		
Linear	0.035	-0.003	-0.005	-0.016
	(0.033)	(0.055)	(0.016)	(0.039)
Quadratic	0.017	0.049	-0.021	0.022
	(0.040)	(0.061)	(0.029)	(0.046)
		Panel C: Log Hourly Wage		
Linear	0.025	-0.002	-0.006	-0.014
	(0.030)	(0.044)	(0.015)	(0.035)
Quadratic	-0.005	0.020	-0.021	0.003
	(0.037)	(0.048)	(0.026)	(0.044)

Data Source: Survey of Income and Program Participation, 1990-2012

Each cells shows the primary coefficient of interest from a separate regression.

Each cell shows the parameter of interest from a separate regression. All regressions include time (month of birth), time*second-half of the year birth (July-December), race dummies, and state and cohort dummies. Quadratic functional form regressions also include time squared and time squared*second-half birth.

Employment is defined as having annualized earnings of at least $2,500.

Earnings are annualized and in 2013 dollars.

Log hourly wage is calculated from the quotient of annualized earnings (in 2013 dollars) and annualized hours.

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