Recent Minimum Wage Policy and Its Impacts on Birth Outcomes*

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Abstract:

After a pause in statutory minimum wage increases during the Great Recession, several states increased minimum wages since 2010, including increases as large as 60% over just a few years. Using a triple-differences framework and data on the universe of births from 2010 to 2019, this study investigates how recent minimum wage hikes affected birth outcomes. We first document sizeable negative impacts of minimum wage policy on the employment and economic well-being of females ages 16-44. We then find mixed effects on birth outcomes, estimating that minimum wage increases led to lower birth weights, fewer normal birthweight births, and more small-for-gestational age births, but longer gestations. These findings generally contrast with an earlier literature relying on typically much smaller minimum wage increases which showed more evidence of positive impacts on birth outcomes.

Keywords: *minimum wages, infant health, birthweight, gestational age*

JEL Codes*: I18, J31, J38*

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1. Introduction

The minimum wage remains one of the most contentious and actively debated policies in the United States. Initiatives to increase state and local minimum wages are typically motivated by the goal of improving the lives of low-wage workers and their families. A large literature has studied whether minimum wage policy induces disemployment effects among groups that the policy is intended to help (Card & Krueger, 1994; Cengiz et al., 2022; Cengiz et al., 2019; Dube et al., 2010; Neumark & Shirley, 2022; Neumark & Yen, 2022). While employment rates are the most common outcome studied, the literature also examines the relationship between the minimum wage and broader socioeconomic indicators, such as household poverty rates (Burkhauser et al., 2025; Burkhauser & Sabia, 2007; Neumark & Wascher, 2002) and the distribution of income (Dube, 2019; Neumark et al., 2004). The economic consequences of the minimum wage may impact population health, and there is considerable policy and academic interest in the multitude of downstream health outcomes affected by minimum wage policy.

A prior literature has considered whether minimum wage policy affects health behaviors and health outcomes as summarized in Leigh (2016), Leigh et al. (2019), and Neumark (2024). A handful of studies have specifically examined minimum wage impacts on infant health and have consistently found modest salubrious effects (Komro et al., 2016; Wehby et al., 2020). Analyses of infant health outcomes are uniquely critical for understanding the impact of social and economic policy due to their well-recognized importance for instigating life course and intergenerational feedback in stratified economic mobility and health status (Aizer & Currie, 2014).

The Great Recession prompted a pause in minimum wage legislation across most of the country. However, in the second decade of the 21st century, a series of initiatives were passed to update existing thresholds and bring them more in-line with rising costs of living. These policies typically increased the minimum wage in each year until a new maximum was reached, after which many jurisdictions indexed further increases to inflation. Clemens and Strain (2021) document striking cross-state variation in minimum wage policy from 2011 to 2019. Over this span, 20 states increased their statutory minimum wage by more than 25%, including six states that exceeded 50%. Notably, Maine, New York, and the District of Columbia each raised their

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minimum wage by at least 60%. At the same time, several states saw no change at all (see Table A1).

The economic effects of these new and large minimum wage increases are only now being understood (Burkhauser et al., 2025; Clemens & Strain, 2021), while studies about the population health consequences lag even further behind. It is an open question how these recent increases impacted infant health. It is plausible that they generated larger positive effects on birth outcomes than those documented in prior studies typically exploiting much smaller—on average—minimum wage hikes. Alternatively, it is possible that large recent increases had unintended impacts that altered population health in different ways than the existing literature would predict. Given the political movement to replicate these policies in other states and even federally, it is important to understand whether and how these specific policy changes are meaningfully impacting population health. These new minimum wage increases reach far beyond the existing evidence base, demanding new research on their population health impacts.

This paper examines the consequences of the large variation in minimum wage policy across the United States since the Great Recession on birth outcomes using rich administrative data on births in the US Vital Statistics System. Infant health predicts a wide range of later-life socioeconomic outcomes such as educational attainment (Black et al., 2007; Royer, 2009) and lifetime income (Behrman & Rosenzweig, 2004; Bharadwaj et al., 2018). In addition, birth outcomes are responsive to economic conditions (De Cao et al., 2022; Kaplan et al., 2017; Margerison-Zilko et al., 2017) and the availability and generosity of social assistance (Bitler & Currie, 2005; Dettling & Kearney, 2023; Hoynes et al., 2015; Hoynes & Schanzenbach, 2015; Strully et al., 2010), suggesting that they may be especially influenced by large minimum wage hikes. Notably, these impacts are unlikely to be uniform across the population since minimum wage legislation targets lower-income workers; these workers disproportionately represent social groups with high relative risks of poor infant health; and thus, major changes to the minimum wage may meaningfully alter social disparities in birth outcomes.

We study the effects of recent minimum wage policy in a triple-differences framework, permitting us to both richly account for unobserved state-time factors that could concurrently influence labor outcomes and infant health and to also rigorously evaluate whether social group differences in policy effects conform with known disparities in infant health. We study changes

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in outcomes over time in adopting relative to non-adopting and not-yet-adopting states. Because we are concerned that states might endogenously implement minimum wage policies for reasons that could independently affect state birth outcomes, we further exploit variation in exposure to the minimum wage across mother's educational attainment in a triple-differences framework, using mothers with a college degree as an additional comparison group. Similar triple-difference designs are common in the minimum wage literature (Clark et al., 2020; Clemens & Wither, 2019; Du & Leigh, 2018; Horn et al., 2017), motivated by the goal of accounting for state-specific time-varying endogeneity.

We operationalize treatment as each states' first minimum wage increase since the Great Recession, similar to approaches found in Clemens and Strain (2021) and Burkhauser et al. (2025) and aided by the scarcity of minimum wage policy changes in the immediate aftermath of the Great Recession (i.e., our pre-period).[1] We implement an imputation approach (Borusyak et al., 2024; Gardner, 2022), using untreated observations to construct counterfactuals for the treated observations, avoiding biases resulting from staggered adoption and possible heterogeneous treatment effects. This method permits us to analyze pre-treatment placebo estimates as a suggestive test of the underlying "parallel trends" assumption. It also allows us to examine the timing of effects, recognizing that births occurring immediately after a minimum wage increase have limited exposure to the policy and may therefore exhibit delayed responses. We will also discuss why we might expect dynamic treatment effects in this setting. Our event study approach remains agnostic about the timing of the outcome responses.

We find evidence that minimum wage increases, on average, economically harmed women ages 16-44. We estimate that employment rates, labor force participation rates, and hours worked drop after minimum wage increases. Labor market outcomes are rarely studied in conjunction with health outcomes, suggesting a notable gap in the literature on the mechanisms by which minimum wage policy affects health. Our main estimates imply that statutory minimum wage hikes reduce mean birthweight and increase gestational age at birth, suggesting a possible reduction in intrauterine growth rates. We study small-for-gestational-age (SGA), which refers to infants in the bottom 10% of birth weights for their gestational age and may

[1] Clemens and Strain (2021) use 2015+ as the post-period for all states, though they also provide results designating the state's first statutory minimum wage increase as "treatment" (see Figure A4 of the paper).

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reflect intrauterine growth restriction, as a primary outcome. We estimate that minimum wage increases lead to a 0.3 percentage point increase in the rate of SGA, equivalent to a 2.3% increase. We observe little evidence that the results are driven by pre-existing trends or differential exposure to and recovery from the Great Recession. We find that the responsiveness of birth outcomes is generally larger for subgroups more exposed to the minimum wage.

The results contrast sharply with those found in the literature which had previously found that the minimum wage increased birthweight (Komro et al., 2016; Wehby et al., 2020). These studies relied on decades of minimum wage variation predominantly before the Great Recession. As further evidence that would have substantiated these earlier salubrious findings continuing into the period of our study, Rokicki et al. (2023) associated the minimum wage increases between 2004 and 2015 with declines in women's reporting of "stressful life events" during pregnancy, which a great body of literature has linked with better infant health (Beydoun & Saftlas, 2008).

This paper makes two primary contributions. First, we are the first to comprehensively evaluate the impacts of the large post-Great Recession minimum wage increases on labor market and birth outcomes. These increases are of special interest to researchers and policymakers given their size—relative to historical minimum wage growth—and the growing policy interest in equivalent minimum wage increases. Second, on methodological innovation, the minimum wage literature typically relies on two-way fixed effects models while providing little evidence of pre-existing trends. We adopt a two-stage triple-differences framework that jointly circumvents bias in treatment effect estimates that may arise from staggered policy adoption or from including covariates additively in a two-way fixed effects model (Caetano et al., 2022; Powell, 2023), while also controlling flexibly for state-specific time-varying endogeneity. Recent work in the minimum wage literature has implemented new difference-in-differences methods (Burkhauser et al., 2025; Cengiz et al., 2019; Clemens & Strain, 2021), but not in the context of birth outcomes, nor in conjunction with a more flexible triple-differences approach.

We provide additional background in the next section. Section 3 discusses the data. Section 4 explains the empirical strategy. Results are provided in Section 5, and we conclude in Section 6.

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2. Background

2.1 Birth Outcomes

Birthweight is often considered a leading, though imperfect, indicator of health at birth (Wilcox, 2001) and a strong predictor of later life outcomes (Behrman & Rosenzweig, 2004; Bharadwaj et al., 2018; Currie & Cole, 1993),[2] including in developing countries (Currie & Vogl, 2013). More broadly, infant health metrics are commonly used to assess population health (Gonzalez & Gilleskie, 2017; Reidpath & Allotey, 2003). Both birthweight and gestational age are often used as measures of infant health (Camerota & Bollen, 2016; Currie, 2009) as gestational age also predicts later life health outcomes (Boyle et al., 2012; Crump et al., 2011; Sansavini et al., 2011). Preterm births (infant born before 37 weeks of pregnancy), in particular, are predictive of poor outcomes later in life (Ahmed et al., 2024; O'Nions et al., 2021). Birthweight typically reflects the combination of time in utero (gestation) and rate of growth while in utero. Notably, the relationship between birthweight and long-term outcomes is partially attributable to the correlation between gestational age and birthweight (Maruyama & Heinesen, 2020; Wilcox & Skjaerven, 1992).

To disentangle gestational age and birthweight, a large literature considers the predictive power of birthweight given gestational age, finding evidence that babies born small or large for their gestational age can suffer worse later life outcomes (Abel et al., 2017; Chiavaroli et al., 2016; Helgertz & Vågerö, 2014; Hong & Chung, 2018; Hong & Lee, 2021; Nam & Lee, 2018; Yang et al., 2023), including long-term survival propensities (Ludvigsson et al., 2018; Wennerström et al., 2015). For this reason, our study relies on size-for-gestational-age measures, though we also present estimates related to birthweight and gestational age in isolation.

Birth outcomes respond to economic conditions and social policy more broadly (Almond et al., 2011; Reynolds & Homan, 2023), suggesting that there is scope for the minimum wage to impact them and a need to better understand the existence and importance of such a relationship.

[2] Other work has argued that birthweight does not causally improve later life outcomes (Maruyama and Heinsen, 2020).

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2.2. Minimum Wage Policy and Labor Outcomes

The disemployment effects of the minimum wage have been well-studied (Azar et al., 2023; Cengiz et al., 2019; Dube et al., 2010; Manning, 2021; Meer & West, 2016; Neumark et al., 2014; Powell, 2022; Totty, 2017). However, disemployment itself does not capture the full individual and household economic effects of the minimum wage. Research has considered the extent to which the minimum wage affects poverty (Burkhauser et al., 2025; Burkhauser & Sabia, 2007), wages, and income (Clemens & Wither, 2019; Dube, 2019; Giupponi et al., 2024). These mechanisms suggest that minimum wage policy likely helps some people through wage increases while potentially adversely impacting others through a range of firm-level responses. Neumark (2024) highlights that the theoretical link between the minimum wage and health measures is generally ambiguous, motivating the need for empirical evidence. For the more recent minimum wage increases studied in this paper, the literature has provided clearer evidence that economic harms occur (Clemens & Strain, 2021) with less evidence of benefits such as poverty reduction (Burkhauser et al., 2025), though the literature on these post-Great Recession minimum wage increases is sparse and may evolve in the upcoming years.

Firms may also respond on margins other than employment levels (Clemens, 2021), such as the provision of health insurance (Clemens et al., 2018; Dworsky et al., 2022; Meiselbach & Abraham, 2023). There is evidence that firms increase the prevalence of wage theft (Clemens & Strain, 2022) and respond through "skill-upgrading" such as requiring a high school diploma (Clemens et al., 2021), implying disproportionate harms for the lowest educated workers. Case studies, such as those that evaluate the large minimum wage increase in Seattle to $13, have found reductions in hours worked with less evidence of employment losses (Jardim et al., 2022).

2.3.Minimum Wage Policy and Non-Labor Outcomes

A large literature has studied a breadth of consequences associated with minimum wage policy beyond labor-related outcomes, such as consumption patterns of nondurable goods (Alonso, 2022), crime (Fone et al., 2023), eviction risk (Agarwal et al., 2022), voting turnout (Markovich & White, 2022), teen childbearing (Bullinger, 2017; Lenhart, 2021; Matsuzawa et al., 2025), "deaths of despair" due to alcohol, drugs and suicide (Dow et al., 2020), as well as a variety of other population health measures previously reviewed (Leigh, 2016, 2021; Leigh et al., 2019).

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In our survey of the literature on minimum wage and health, we find that the relationship is strongly patterned according to the years of data studied. Studies including data from the 1980s and 1990s report exclusively salubrious effects (Du & Leigh, 2018; Horn et al., 2017; Kaufman et al., 2020; Komro et al., 2016; Strully et al., 2010; Van Dyke et al., 2018; Wehby et al., 2020). Studies that begin in the mid-1990s find either exclusively null or null and salubrious effects across sociodemographic strata (Averett et al., 2017, 2018). Like these findings, data beginning in the 2000s also predominantly find a combination of null and salubrious effects (Andreyeva & Ukert, 2018; Gertner et al., 2019; Wehby et al., 2022): mixed findings are by sociodemographics and sometimes across outcomes. The few studies using data from the 2000s depart further. They find null and non-salubrious effects by sociodemographics or exclusively non-salubrious effects (Buszkiewicz et al., 2021; Huang et al., 2021; Pearlman & Robinson, 2022). A few find all three effects—salubrious, non-salubrious, and null—with differences by sociodemographics and across measures (Dave et al., 2021; Horn et al., 2017; Sigaud et al., 2022)

Despite the wealth of outcomes explored in this literature, there is relatively little work on the role of minimum wage policy in shifting birth outcomes. Studies of the minimum wage and infant health have consistently found modest salubrious effects using data for which the majority of observations occur in the 1980s and 1990s (Komro et al., 2016; Wehby et al., 2020; Wolf et al., 2021).[3,4] Thus, the limited work on this topic focuses primarily on the pre-Great Recession minimum wage increases, which we explained above are predominately smaller than more recent initiatives. These newer policies also tend to set a future target minimum wage that is implemented incrementally over time and may also be indexed to inflation. Firms may thus shift their employment practices due to an expectation that wages will increase in the future.

2.3 Conceptual Framework

The minimum wage can impact population health through a variety of mechanisms. By increasing income, maternal stress may diminish, and consumption patterns might change. Minimum wage hikes have been shown to increase smoking rates (Huang et al., 2021), which

[3] Strully et al. (2010) studies the relationship between state EITC expansions and birth outcomes while accounting for minimum wage policies. The estimated coefficients on the state minimum wage variable suggest positive effects on health.

[4] Wolf et al. (2021) studies state preemption laws, which prevent sub-state minimum wage increases. The results of that paper also imply that increases in the minimum wage tend to improve birth outcomes.

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can shift birth outcomes. Firm-level responses may decrease employment propensities or health insurance availability for those close to the bottom of the wage distribution. The broader effects of the minimum wage, such as changes in crime rates, could also impact population health. Moreover, Ruffini (2024) finds that statutory minimum wage increases improve health outcomes in nursing homes due to staff productivity improvements, suggesting that the minimum wage could impact the health production function. As such, the minimum wage impacts on infant health are theoretically ambiguous, and our empirical design estimates the net effect of the various mechanisms at play.

3. Data

We describe the construction of our outcomes and explanatory variables in this section. A detailed description of each of these measures is included in Table A2.

A. Measures of Infant Health and Sample

For our infant health measures, we use the Restricted-Use Natality Detail Files, the universe of live births in the United States, from 2010 to 2019 (National Center for Health Statistics, 2022). These files include a wealth of information about each birth, including infant birthweight, gestational age, and maternal age, race, ethnicity, and education attainment. We condition on indicators related to mother's age (16-24, 25-29, 30-34, 35-44; categorized according to lower and higher risk obstetric age bands), mother's race and ethnicity, mother's educational attainment, live birth order, and infant sex. Maternal education is underreported or missing in several state-year records during our study period because some states delayed adoption of the 2003 revision of the U.S. Standard Certificate of Live Birth, which introduced a standardized question on maternal education. To maintain uniformity and comparability, the Center for Disease Control and Prevention recoded education data to "excluded" if a state had not adopted the revised 2003 version (Centers for Disease Control and Prevention, 2025).[5] Due to the importance of mother's education in our empirical specification, we omit these state-years in the sample.[6] A state is considered "treated" starting from the first year in which a statutory

[5] These state-years are Alaska (2010-2012), Arkansas (2010-2013), Alabama (2010-2013), Arizona (2010-2013), Connecticut (2010-2015), Hawaii (2010-2015), Louisiana (2010), Massachusetts (2010-2011), Maine (2010-2013), Minnesota (2010-2011), Mississippi (2010-2012), North Carolina (2010), New Jersey (2010-2015), Rhode Island (2010-2014), Virginia (2010-2012), Wisconsin (2010), and West Virginia (2010-2013).
[6] Results are generally similar if we exclude these states entirely.

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minimum wage hike begins to take effect.[7] To permit examination of parallel trends in the pre-period, we drop states with fewer than 3 years of pre-treatment years.[8] The final sample includes 42 states (including Washington D.C.), 17 of which are treated.

The infant health measures that we examine are average birthweight in grams and gestational age in weeks as collected in the delivery room by medical personnel at birth and recorded in vital records. Focusing on means can obscure important distributional shifts so we also study low birthweight (LBW), normal birthweight (NBW), and high birthweight (HBW), corresponding with standard clinical and public health thresholds of risk. Low birthweight is defined as less than 2500 grams, normal birthweight is 2500-4000 grams, and high birthweight is more than 4000g. Changes in gestational age can affect the birthweight distribution. To address this issue, we examine small for gestational age (SGA), or infants below the 10^{th} percentile of size for gestational age, appropriate for gestational age (AGA), or infants between the 10^{th} and 90^{th} percentiles of size for gestational age, and large for gestational age (LGA), or infants above the 90^{th} percentile of size for gestational age, as defined in Oken et al. (2003). To understand important movements in the distribution of gestational age, we study preterm births, defined as births before 37 completed weeks of gestation.

B. Labor Market Outcomes

We study labor market outcomes to test whether specific groups were disproportionately impacted by minimum wage increases. Our empirical strategy relies on some groups being more exposed to these policy changes than others. While some of these labor outcomes have been studied in the recent minimum wage literature, we study them for women ages 16-44. This analysis operates as an important step in our understanding of birth outcome impacts since we would be less likely to expect birth outcomes to change if economic conditions were unaffected by minimum wage policy.

For labor market outcomes, we use individual-level data from the American Community Survey (ACS) for 2010-2019, available from IPUMS (Ruggles et al., 2023). To permit

[7] After dropping state-years with missing mother's information, Connecticut, New Jersey, and Rhode Island are "always treated" due to the timing of their first statutory minimum wage hike post-pause and are thus omitted from the analytic sample.
[8] This criterion requires us to drop Alaska, Arkansas, Hawaii, Minnesota, Nevada, and West Virginia.

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comparability with the infant health sample, we drop the state-years that were omitted in the natality files and restrict our sample to the female population aged 16-44. The labor market measures that we examine are any employment, labor force participation, usual hours worked in a week, full-time work status, and official household poverty status.[9] For household poverty, we apply household weights provided by the ACS. For all other measures, we apply person weights.

Only a small portion of the ACS sample of women of childbearing age will give birth in that same year, which limits comparability of the ACS and natality samples. We thus bolster the analysis of poverty status from the ACS with additional analyses of participation in the Special Supplemental Nutrition Program for Women, Infants, and Children (WIC) as recorded in the Natality Detail Files. WIC serves pregnant women (and other populations), and eligibility is dependent on low-income status. We study WIC participation as a proxy for financial adversity and food insecurity among the population of interest.

C. Explanatory Variables

Our policy variable is an indicator variable equal to 1 when a state first begins to increase their minimum wage following the "minimum wage freeze" described extensively in Clemens and Strain (2021), and 0 otherwise. After the Great Recession, state and federal efforts to increase minimum wages were generally paused. This established a baseline period that could be leveraged in empirical research. The timing of states' first statutory minimum wage hike during our study period was reported above in Table A1. We consider the passage of any new minimum wage law with a specific target wage to be a statutory increase: the year a minimum wage statute started taking effect—including those statutes coming from ballot measures, legislation, or state constitutional amendments—is considered the beginning of a statutory increase, even if the target wage is not met yet.[10] The average *first* statutory increase during our study period is a 90 cent minimum wage increase, ranging from 25 cents to $1.95, with possible increases in subsequent years (discussed more in Section 5A). Our comparison group consists of states that never or have not-yet enacted a statutory increase in their minimum wage. States that indexed their minimum wages to inflation during the entire study period, but without any statutory

[9] We also examined the Supplemental Poverty Measure (SPM). These results (not shown) were consistent with the poverty results presented below.

[10] For instance, Vermont passed a law in June 2014, which set a target minimum wage of $10.50 by 2018. The law started to increase the minimum wage from January 2015, so Vermont is considered treated beginning in 2015.

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increase, are considered untreated (we revisit this decision in Section 5E).[11] To the extent that these nominal minimum wage increases also affect infant health outcomes, our estimates will be attenuated.[12]

We control for a range of state and individual-level factors. Our state-level covariates (applicable only for the difference-in-differences analyses) include Medicaid expansion status under the Affordable Care Act, state EITC as a share of the federal EITC, TANF generosity for a family of four, medical marijuana laws, and the housing price index. Person-level controls (applicable in both the difference-in-differences analyses and the triple-differences analyses) include race and ethnicity indicators, age group dummies, and educational attainment indicators.[13] For birth outcomes, we also control for birth order and an indicator for infant sex.

Summary statistics of these dependent and explanatory variables are reported in Table A3 in the Appendix. Figures A1 and A2 present time series plots of birth outcomes. Figure A1 shows that both birthweight and gestational length increased in the early portion of our sample, then declined from 2015 onward. We report these trends for both our analysis sample and for the full national sample (which includes state-years missing educational information), and the similarity of their patterns suggests our findings are likely to generalize despite data exclusions. Figure A2 displays analogous trends, separately comparing states that adopt versus those that do not, and distinguishing between more and less exposed groups based on education and age.

4. Methods

We rely on difference-in-differences and triple-differences to study changes in birth outcomes in adopting relative to non-adopting states. We present event study estimates for all outcomes to assess pre-treatment trends and to examine the timing of the effects. The timing is especially important in our context because we would not assume that births immediately after a minimum wage increase would be impacted. Instead, we expect a partial effect in the first year

[11] We assign states that are always indexed and never statutorily increase minimum wages to the control group.
[12] The treated states also operate as controls prior to their first statutory minimum wage increase.
[13] We control for granular education levels using the following categories: 8th grade or less, 9th-12th grade with no diploma, high school graduate or GED competed, some college credit (but not a degree), associate degree, bachelor's degree, master's degree, doctorate or professional.

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and implement models which permit flexibility by time-relative-to-treatment. We discuss our models in detail in this section.

4.1 Difference-in-Differences

We leverage the staggered increases in minimum wages across states and time to identify their effects on infant health outcomes. Our difference-in-differences analyses study infant health outcomes for mothers with a high school degree or less, though we will also provide results for several different sets of subgroups. To account for potential contamination of estimates due to treatment effect heterogeneity across states and time, we implement a two-stage difference-in-differences (2sDID) approach (Gardner, 2022; Gardner et al., 2025).[14] This approach addresses concerns about staggered adoption and treatment heterogeneity which can bias estimation results in a conventional two-way fixed effects model (Goodman-Bacon, 2021; Sun & Abraham, 2021). In addition, including covariates additively in a two-way fixed effects model can also bias the treatment effect estimates (Caetano et al., 2022; Powell, 2023), but an imputation approach circumvents these problems as well. In the first step, the method only uses untreated observations to estimate unit and time effects plus the relationship between the covariates and the outcome, evading concerns related to treatment heterogeneity since none of the observations are treated. Once these parameters are estimated, it is straightforward to generate counterfactuals for the treated outcomes. The average differences between the observed outcomes and the counterfactual predictions are the estimates of the treatment effects.[15]

The minimum wage is a continuous variable and all states are impacted by their own minimum wage policies or the federal minimum wage. We designate the first statutory increase in the minimum wage since the Great Recession as our "treatment" and denote $MW_{st} = 1$ (0 otherwise) for time periods in states after this first increase. Our sample period begins during a "pause" in federal and state minimum wage policy, followed by especially disparate state policies regarding the minimum wage. We will show empirically how the minimum wage evolves over time in treated and non-treated states. In our first stage, we estimate:

[14] For estimation of average effects (overall or by time-relative-to-adoption), the point estimates are identical to those provided by the imputation estimator discussed in Borusyak et al. (2024).

[15] While there are many recent difference-in-differences estimators developed to address concerns about staggered adoption and treatment heterogeneity, most are difficult to apply to more granular data given state-level treatment.

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$$Y_{ist}(0) = \mu_s + \tau_t + X'_{ist}\Omega + \epsilon_{ist} \quad (1)$$

using the subsample of untreated and not-yet-treated states (i.e., $MW_{st} = 0$). $Y_{ist}(0)$ is the untreated outcome for individual i in state s at time t, μ_s are the state fixed effects, τ_t are the year fixed effects, X_{ist} includes a set of time-varying controls at the individual and state-level, and ϵ_{ist} is the idiosyncratic error.

Residualizing the outcomes gives $\tilde{Y}_{ist} = Y_{ist} - \hat{Y}_{ist}(0) = Y_{ist} - \hat{\mu}_s - \hat{\tau}_t - X'_{ist}\hat{\Omega}$. Using the residualized dependent variable, we estimate the second stage on the full sample:

$$\tilde{Y}_{ist} = \beta \times MW_{st} + \nu_{ist} \quad (2)$$

where MW_{st} is the treatment indicator equal to 1 if a state has previously increased their minimum wage for the first time during the study period, at year t. We extend (2) to include leads and lags of the treatment indicator in the following specification:

$$\tilde{Y}_{ist} = \sum_{k=-4}^{5} \beta^k MW_{s,t-k} + \nu_{ist} \quad (3)$$

This specification permits us to assess the parallel trends assumptions by examining how the outcomes differentially evolve prior to treatment.[16] We will also consider the dynamic effects of the policy since we expect that the impacts of the policy may vary over time.[17] We might assume that a minimum wage increase would not affect infant health for births occurring immediately after the policy was enacted, since these infants had little in utero exposure to the policy. Our event study analyses are agnostic about the timing of the impact. When reporting event study estimates, we show estimates related to 4 years before adoption to 5 years after adoption.[18] Two-stage difference-in-differences implicitly normalizes the pre-treatment average to zero (i.e., no specific estimate needs to be normalized).

[16] In an imputation framework, the pre-treatment estimates are not directly comparable to the post-treatment estimates, but both sets of estimates provide important information. We rely on the pre-period estimates as "placebos" to provide evidence about the validity of the underlying experiment. The post-treatment estimates provide evidence of policy responses and dynamic effects.

[17] Meer and West (2016) find that minimum wage increases have delayed employment effects, which would suggest that health impacts might also be observed with some delay.

[18] We do not "bin" the endpoints.

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4.2 Triple-Differences

To account for potential endogenous shocks to infant health, we exploit variation in *exposure* to the minimum wage using mother's age and educational attainment as proxies. Specifically, we use mothers who have a bachelor's degree or more, aged 25-44,[19] as a comparison group in a triple-differences framework, given that this group is less likely to be exposed by minimum wages compared to other demographic groups (Wehby et al., 2020). Supporting this point, we find that females with at least a college degree aged 25-44 have the smallest share of individuals earning a wage at or below the prevailing minimum wage as shown in Figure A3, based on an ACS sample of women of childbearing age.[20,21] Those without a high school diploma have the highest share of minimum wage earners in their age group. A triple-differences approach permits us to account non-parametrically for any state*time-specific shocks to birth outcomes that are correlated with minimum wage increases. To the extent that mothers with a bachelor's degree or more, aged 25-44, are potentially influenced by the minimum wage to some degree, the resulting estimates will be biased towards zero. The treated group for these analyses remains mothers aged 16-44 with a high school diploma or less.

The triple-differences specification can be easily incorporated into the two-stage difference-in-differences estimator. Let *i* denote groups as before. A_g represents an exposure indicator variable equal to 1 for mothers with a high school diploma or less, and 0 for mothers aged 25-44 with at least a bachelor's degree.[22] Following a standard triple differences specification, our first-stage equation becomes:

$$Y_{igst}(0) = \gamma_{st} + \lambda_{gt} + \theta_{gs} + X'_{igst}\Omega + \epsilon_{igst}, \quad (4)$$

where $Y_{igst}(0)$ is an untreated infant health outcome for individual i, in group g, in state s at year t. "Untreated" here is defined as a birth in a state that has not yet increased its minimum

[19] This age group was chosen to provide adequate time to complete educational attainment.
[20] This analysis uses the ACS data – the sample is not the same of the mothers in the natality data.
[21] When calculating exposure to minimum wages, we followed the Economic Policy Institute's State of Working America methodology and excluded outliers defined as those earning below $0.85 or above $169.88 per hour in 2010 dollars. Source: https://www.epi.org/data/methodology/.
[22] The "some college" group is excluded from this analysis since they are not considered part of the treated group or the comparison group.

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wage or any birth to a mother aged 25-44 with at least a bachelor's degree. γ_{st} are state-by-year fixed effects, λ_{gt} are group-by-year fixed effects, θ_{gs} are group-by-state fixed effects, and X_{igst} is a vector of individual-specific controls. State-level policy variables are collinear with the state-by-year fixed effects and are not included. Taking the residualized dependent variable, the second stage becomes:

$$\tilde{Y}_{igst} = \beta \times A_g MW_{st} + \nu_{igst} \tag{5}$$

We also show the event study estimates of the two-stage triple-differences estimator:

$$\tilde{Y}_{igst} = \sum_{k=-4}^{5} \beta^k A_g MW_{s,t-k} + \nu_{igst} \tag{6}$$

For transparency, we provide both difference-in-differences and triple-differences estimates when reporting the main outcomes of interest. Given the possible endogenous nature of minimum wage hikes, we rely primarily on the triple-differences results. In our subgroup analyses, we redefine exposure equal to 1 for different education and age group strata, but our comparison group of college degree earners aged 25-44 remains the same throughout our triple differences analyses.

When presenting estimates for difference-in-difference and triple differences, we consider coefficient stability by providing estimates in which no covariates are included and estimates where additional controls are included. We present standard errors and 95% confidence intervals which are adjusted for state-level dependence and for the two-stage process of the estimator (Butts, 2023; Gardner et al., 2025; Hansen, 1982).[23] We additionally present counterfactual means, which are the outcome means of treated observations minus the estimated coefficients (i.e., the predicted counterfactuals), so we can discuss the implied proportional effects of the estimates.

[23] Gardner et al. (2025) find that this inference procedure generally has good properties.

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5. Results

A. Minimum Wages

We begin by examining how minimum wages changed over the study period after the post-Great Recession minimum wage freeze. Figure 1, Panel A presents trends in the average prevailing minimum wage in states that increased their statutory minimum wage post-freeze and states that did not increase their minimum wage post-freeze. Panel B reports the centered event study estimates with a full set of time and state fixed effects and state-level controls, accounting for the staggered timing of states' minimum wage hikes post-freeze (estimating equations 1 and 3). While pre-period event study estimates unsurprisingly hover close to zero, post-period event study estimates indicate an increase in average statutory minimum wages by almost one dollar in the first year. By the fifth year, minimum wages increased, on average, by over three dollars more than the untreated states, which translates into a 30% increase relative to base years. Our research design exploits this post-Great Recession freeze and permits us to capture the effects of these large minimum wage hikes. Our results show that while we define treatment as the initial minimum wage increase, the treated states continue, on average, to increase their minimum wages in subsequent years. While our empirical approach has many conceptual advantages, it is difficult for us to determine whether any dynamic effects are due to the subsequent growth in the minimum wage differences or because the outcomes take time to respond to the initial policy shock.

B. Employment and Income Measures

We next examine how these statutory minimum wage increases affect labor market outcomes for women aged 16-44. Some of these outcomes have been studied in a similar empirical framework for the post-Great Recession period (e.g., Burkhauser et al. (2025) studies poverty using more recent difference-in-differences methods that are robust to dynamic treatment effects). We study a broader set of outcomes for women in our specific age and education groups – and using triple-differences – to examine how they specifically were impacted by these policy changes. These results provide context to the birth outcomes results discussed below.

Figure 2 presents event study estimates from our triple-differences specification for the employment-to-population ratio, labor force participation rate, usual hours worked in a week

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(among workers), full-time work (working 35 hours or more per week), household poverty, and WIC participation. For these outcomes, we generally do not find visual evidence of systematic pre-trends in the years leading up to states' initial minimum wage hikes. After the initial minimum wage hikes, we observe large changes in the outcomes, all consistent with worsening economic outcomes after the policy. In general, these effects grow in magnitude over time. We observe decreases in the employment-to-population ratio, labor force participation, hours worked, and full-time work. We find corresponding increases in WIC receipt among mothers.

We summarize these results in Table 1. Panel A provides the triple-differences results without additional controls; Panel B adds demographic controls to the model. Focusing on Panel B, we estimate that minimum wage increases lead to decreases in employment for this population by 0.9 percentage point, implying a 1.9% decline, and labor force participation by 0.6 percentage points, equivalent to a 1.1% decrease. Usual hours worked decrease by 0.38 hours and full-time work decreased by 1.7 percentage points. Employment and labor force participation were statistically significant at the 10% level, hours worked at the 5% level, and full-time work at the 1% level. These estimates use ACS data to construct outcomes for the full female population aged 16-44. To better isolate the relevant population and as a measure of food insecurity, we study WIC receipt during pregnancy among our births and estimate a 1.6 percentage point increase (statistically significant at the 1% level), implying a 2.4% increase from baseline (see Figure 2 Panel F for the event study estimates). In general, the estimates are similar for all outcomes whether we include additional controls in the model or exclude them.

We include the corresponding difference-in-differences estimates in Table A4. The Panel B estimates (which include additional state- and person-level covariates) are generally the same sign as the triple-differences estimates, though the magnitudes tend to be smaller. However, the results still suggest large and statistically significant increases in WIC receipt and decreases in labor supply on the intensive margin. In general, the results of this section suggest that minimum wage increases engender worsening economic outcomes for women with a high school diploma or less aged 16-44.

We later show results for infant health outcomes across different age and education groups to consider heterogeneity. We hypothesize that each of these groups is more impacted by minimum wage policy than the comparison group – ages 25-44 with a college degree. However, we expect

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some of the subgroups to be more affected than others. Table A5 includes the triple-differences labor market estimates for these groups. In general, we observe relative worsening economic conditions for each group. Women without a HS diploma (regardless of age group) experience large declines in labor force participation, employment, and hours worked. We also estimate increases in WIC receipt for these groups. Younger women with a high school degree also suffer large employment reductions and see rises in WIC participation. Notably and predictably, higher-educated women appear to be less impacted by minimum wage increases, with the exception of women with some college but no degree aged 16-24, who experience a 5.2% increase in household poverty. Women aged 16-24 appear to be more impacted by minimum wage increases than the 25-44 groups.

C. Infant Health Measures

We next examine how these minimum wage hikes affect infant health outcomes, focusing on our main comparison: women ages 16-44 with a high school degree or less to women ages 25-44 with a college degree in a triple-differences framework. Figure 3 presents event study estimates for mean birthweight (Panel A), low birthweight (Panel B), normal birthweight (Panel C), and high birthweight (Panel D). Average effects are summarized in Table 2 for all outcomes discussed throughout this section.

We observe little evidence of differential pre-existing trends, suggestive that the implicit parallel trends assumption of our triple-differences design is valid. After the first minimum wage increase, there is evidence of a delayed and noisy reduction in birthweight. The average effect over the entire post-period suggests that an increase in the statutory minimum wage is associated with lower birthweight by 1.96 grams (not statistically different from zero), equivalent to a 0.06% decrease.

Mean birthweight is generally uninformative of changes in critical parts of the distribution. Panels B-D of Figure 3 report the event study estimates for the impacts of minimum wages on birthweight at different margins. The minimum wage decreases the prevalence of NBW births while increasing LBW and HBW births. On average, we estimate a decline in NBW of 0.003, statistically significant at the 1% level and implying a 0.37% reduction from baseline (see Table 2 Panel B). There is a corresponding increase in LBW of 0.001 (not statistically significant) and HBW of 0.002 (statistically significant at the 1% level). This latter increase represents a 3.2%

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rise in HBW births. These findings are generally robust to the exclusion or inclusion of additional controls (comparing Panel A to Panel B in Table 2).

Figure 4 presents event study estimates for mean gestational age in weeks (Panel A), and preterm births (Panel B). Estimates suggest that the minimum wage is associated with older gestational age at birth. We estimate statistically significant increases in the year of adoption and the subsequent three years. The average effect (Table 2 Panel B) implies an increase in gestational age of 0.027 weeks, statistically significant from zero at the 1% level and equivalent to a 0.07% increase. We also observe a decrease in preterm births by 0.4 percentage points, or a 4% decline and statistically significant from zero at the 1% level. Taken together, while we observed some evidence that birthweight declines, this decline is not driven by a reduction in gestational age. Instead, gestational age increases and the prevalence of preterm births decreases. The rise in HBW births could potentially be driven by the increase in gestational age.

We next study SGA, AGA, and LGA propensities to account for this possibility and due to the independent interest in these outcomes. The event study estimates are presented in Figure 5. We observe post-treatment increases in the probability of SGA and decreases in AGA and LGA propensities. The latter result contrasts with the increases in HBW observed in Figure 3, suggesting that the rise in gestational age accounts for the increase in HBW births. On average (see Table 2 Panel B), we estimate that statutory minimum wage hikes reduce the probability of LGA births by 0.1 percentage points (not statistically significant). We observe a corresponding increase in SGA births by 0.3 percentage points, or a 2.36 percent increase (statistically significant at the 5% level).

The corresponding difference-in-differences estimates are provided in Table A6. We generally find similar – typically stronger – results. We estimate statistically significant reductions in birthweight, including increases in the incidence of LBW and decreases in the incidence of HBW. The latter contrasts with the triple-differences results. The difference-in-differences estimates suggest a 0.3 percentage point increase in SGA prevalence (statistically significant at the 5% level), similar to the triple-difference estimates. We also estimate increases in gestational age when using our triple-differences strategy, but we find decreases using difference-in-differences. However, the main conclusions drawn from the triple-differences results are generally robust across models.

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D. Subgroup Analyses

Next, we explore heterogeneous effects of minimum wage policies on infant health by studying alternative and more granular education and age groups. We report subgroup analyses for infant health outcomes in Table 3 based on age group (16-24, 25-44) and educational attainment (no HS diploma, high school diploma only, some college but no bachelor's degree). We generally observe the strongest evidence for the groups which experienced the biggest labor and poverty responses (discussed above for Table A5). For example, women ages 16-24 – both without a HS degree and those with a HS degree – suffered large decreases in employment after minimum wage increases. These same groups had the largest (in magnitude) birthweight responses. Women ages 25-44 with some college are estimated to have substantially smaller changes in birth outcomes, consistent with less exposure to the minimum wage among this group (and consistent with the Table A5 results).

E. Indexed Minimum Wages and Large Minimum Wage Hikes

Several states in our comparison group experienced regular minimum wage increases throughout our sample period because they indexed their minimum wage to a cost-of-living measure. In Table A7 Panel A, we replicate our results for labor outcomes while excluding the indexed states. We observe similar, though typically larger, magnitudes across all outcomes. In Table 4, we provide the equivalent for infant health outcomes. We generally find results comparable to our main findings, though we note that after removing indexed states, the increase in low birthweight that was previously statistically insignificant becomes statistically significant at the 1% level.

Six states increased their minimum wage by at least 50% over our study period. We expect magnitudes to be amplified when restricting our sample to larger statutory minimum wage hikes. We consider an analysis that focuses on these large minimum wage hikes, which consist of California, District of Columbia, Maine, Maryland, Massachusetts, and New York as well as the never-adopters (while still excluding the indexers). We find that the labor market impacts were larger in the states, as shown in Table A7 Panel B. In particular, we report a 2.8% decrease in employment from large minimum wage hikes, compared to a 1.9% decrease on the entire sample. Similarly, we report a 7.3% increase in WIC participation in large minimum wage states compared to a 2.4% increase in the entire sample.

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The birth outcome results from this analysis are summarized in Table 4 Panel B, which report triple-differences estimates on those with a high school degree or less aged 16-44 as the treatment group. For large minimum wage increases, we report increases in SGA propensities, statistically significant at the 5% level; however, the magnitude for gestational age remains minuscule, equivalent to a 0.1% change. On the other hand, large minimum wage hikes are associated with increases in low birthweight births of 2.3%, a decrease in normal birth weight births by 0.4%, and an increase in high birthweight births by 3.0%, all statistically significant at the 1% level. We estimate that these large minimum wage increases lead to a 3.1% rise in SGA births, compared to 2.4% for the full sample. In proportional terms, the estimated effects are generally larger in magnitude for all outcomes when compared to the full sample effect (shown in Table 2).

F. Sensitivity Analysis

In this section, we further consider the robustness of our main findings and the appropriateness and necessity of our triple-differences approach. First, we note that there is little evidence that our results are driven by pre-existing trends. Our sample period begins during a time of large economic and social changes given its proximity to the Great Recession. However, we do not observe that differential recovery from the Great Recession and other secular factors are impacting our results given that we rarely observe large magnitudes in the pre-period.

Our findings are also generally similar whether we rely on difference-in-differences or triple-differences, and they are robust to the inclusion of additional controls. These results suggest coefficient stability and are consistent with unobserved confounders not driving our findings. We also generally find that groups more exposed to the adverse economic impacts of the minimum wage were more affected in terms of infant health, again bolstering our confidence that minimum wage hikes are causally driving these results.

Our triple-differences empirical strategy was motivated by the possibility of endogenous minimum wage policy adoption. While generally similar, we observe some disparate results between our difference-in-differences and triple-differences estimates. The labor responses are mostly estimated to be stronger in the triple-differences models, and the gestational age results are sensitive to the model. To consider the endogeneity of the minimum wage increases, we estimate our difference-in-differences model while studying possible confounders/mediators as

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the outcome variable (Pei et al., 2019), including GDP per capita, the housing price index, Medicaid expansion status, EITC generosity, maximum TANF amounts for a 4-person household, and any medical marijuana law, and report these event studies in Figure A4. We observe large increases in GDP per capita, noisy increases in housing price index, and increased likelihood of Medicaid expansion status at around the same timing as minimum wage hikes. These results suggest that a naïve difference-in-differences approach might underestimate potential adverse effects on labor market outcomes because minimum wage hikes are more likely to occur during good economic times and be accompanied by additional financial support. When we estimate our difference-in-differences model on outcomes among women with a college degree aged 25-44, we observe little systematic changes in labor market outcomes, but generally worsening infant health measures, as reported in Figures A5 and A6, respectively. Taken together, this provides a justification for our triple-differences specification, which non-parametrically controls for potential unobservable state-level time-varying confounders. However, notably, the main findings of this paper are generally similar whether we use triple-differences or difference-in-differences.

Notably, Medicaid expansions would likely differentially impact pre-pregnancy care for our "most exposed" groups, potentially biasing our results. However, prior work did not find any relationship between Medicaid expansions and birthweight or gestational age (Brown et al., 2019) or birth outcomes in general (Margerison et al., 2021). The relative (but noisy and lagged) increases in EITC benefits would potentially attenuate our results.

As an additional sensitivity test, we consider whether our results for labor market outcomes are robust to the inclusion of state-years in the ACS that were excluded due to missing mother's education in the birth records data. As reported in Table A8, we find similar patterns of decreases in labor supply at the intensive and extensive margins, especially for large minimum wage hikes.

6. Conclusion and Discussion

We find that minimum wage hikes since the Great Recession are associated with decreases in labor supply among the female population aged 16-44 and increases in WIC receipt among mothers giving birth. These results echo findings in the literature that the large statutory minimum wage increases implemented by states since the Great Recession created some

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economic harms (Burkhauser et al., 2025; Clemens & Strain, 2021). Our findings suggest that these economic harms lead to worse infant health. The methodological strength of our research design and consistency of our findings across economic and infant health outcomes, taken in context with our review of prior research (as showing a shift from salubrious to non-salubrious effects in the post-Great Recession period), advances the literature by providing a more rigorous benchmark estimate of the health impacts of minimum wages (Leigh et al., 2019; Neumark, 2024).

For birth outcomes, we estimate decreases in the prevalence of NBW due to increases in HBW. Interestingly, we also observed increases in gestational age and decreases in the frequency of preterm births. This rise in gestational age appears to explain the rise in HBW births. We estimate increases in the rate of SGA births, which the literature has found to be especially predictive of poorer later-life outcomes. While the decrease in hours worked we observe could, theoretically, decrease maternal stress and thereby increase gestational length, these decreases in labor supply may also increase maternal food insecurity through decreases in earned income, leading to increases in the prevalence of small for gestational age, or intrauterine growth restriction.[24]

Notably, while our findings are often statistically different from zero, the implied impacts on population health are small. We estimate that a minimum wage increase leads to a 2.3% increase in the likelihood of an SGA birth. This does not mean that the impacts are not meaningful for more focused populations. It may be that we cannot accurately target the most exposed populations well enough relying only on age and education information. To compare our estimate to the policy effects of other programs, Bitler and Currie (2005) estimate that WIC participation decreases the likelihood of SGA births by 2%.[25] Hoynes et al. (2011) use the staggered implementation of WIC to estimate that WIC reduces the share of LBW births by 1.4%, similar to our (statistically insignificant) LBW point estimates.

The findings of this study are contrary to a related line of research that examines the relationship between minimum wages and infant health in the U.S. (Komro et al., 2016; Strully

[24] We observe a large decrease in earned income using the ACS, but we do not show this outcome because of concerns of data quality and top coding.

[25] This paper uses a different definition of SGA. They study infants below the 25th percentile of weight given gestational age.

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et al., 2010; Wehby et al., 2020) . Prior literature identifies improvements in infant health, as measured primarily by birthweight and infant mortality. In contrast, we report some evidence of adverse impacts on infant health. There may be a few reasons behind this discrepancy. First, the study period used in the prior literature are primarily prior to 2010. Labor market conditions may have evolved since then. Similarly, the recent minimum wage increases are much larger than the typical minimum wage increase prior to the Great Recession. By 5 years after the initial statutory minimum wage increase, our treated states had experienced an additional 3 dollar increase in their minimum wage relative to the comparison group. It is possible that such large increases generate disproportionate economic and population health harms when compared to earlier minimum wage policy. In support of this hypothesis, a small literature has found that recent minimum wage policy does appear to be associated with poor labor and economic outcomes (Burkhauser et al., 2025; Clemens & Strain, 2021).

Second, prior studies estimate standard two-way fixed effects models. Aside from potential bias stemming from heterogeneous treatment effects (Callaway & Sant'Anna, 2021; de Chaisemartin & d'Haultfoeuille, 2020; Goodman-Bacon, 2021; Sun & Abraham, 2021), a standard difference-in-differences specification may have an upward bias particularly in the context of minimum wages, as we have shown that statutory increases occur during favorable economic times. Prior literature reviews have highlighted the importance that future research employ stronger methodological specifications, such as triple-differences or estimators robust to dynamic treatment effects employed here (Leigh et al., 2019; Neumark, 2024).

We note a few important limitations of the present study. First, we cannot state with certainty that the sample of women analyzed in the ACS data are directly comparable to the mothers in the birth records data. The results from the ACS data can help to explain the observed changes in infant health only to the extent that these two samples are comparable. Second, we are unable to explain the precisely estimated, yet small decrease in gestational age among the highly educated group of mothers aged 25-44 from a naïve difference-in-difference specification. However, this necessitates our use of a triple-differences strategy to account for such sources of unknown state-time idiosyncrasies. Finally, we are unable to fully untangle the mechanisms behind our findings. While the increase in WIC participation suggests greater food insecurity and

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financial adversity among mothers, there could simultaneously be intrahousehold labor substitution that discourages female labor force participation while household income increases.

Despite these limitations, this study has important implications for policy. As our findings add to a growing body of evidence indicating that the health impacts of minimum wage policies are mixed, it challenges the prevailing assumption that such policies are uniformly beneficial to health. When interpreted alongside earlier evidence from the pre-Great Recession period, the results further highlight the potential importance of the magnitude of increase and pace at which minimum wage policy is implemented in shaping health outcomes.

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Figures

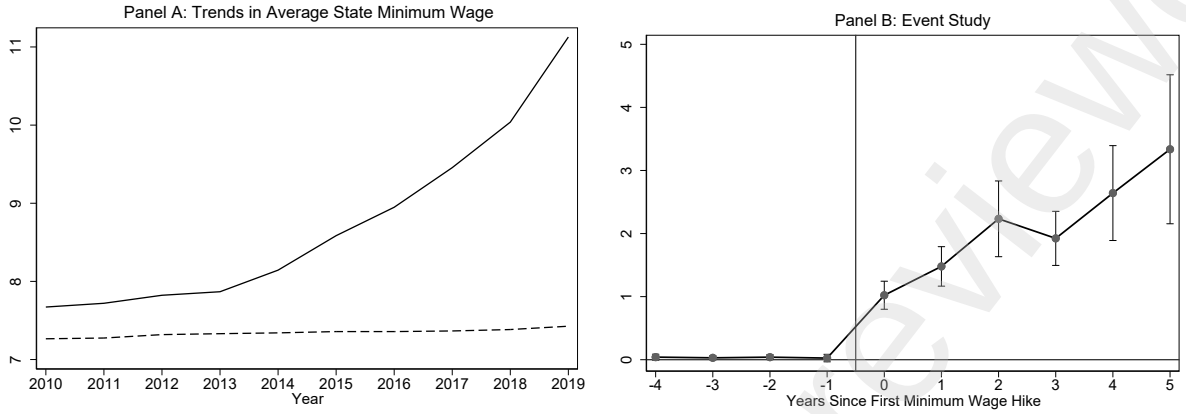
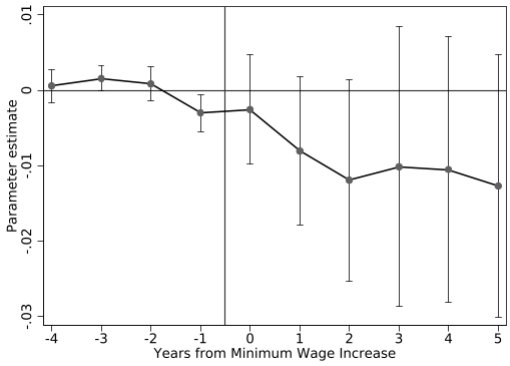


Figure 1. Changes in minimum wages over the study period

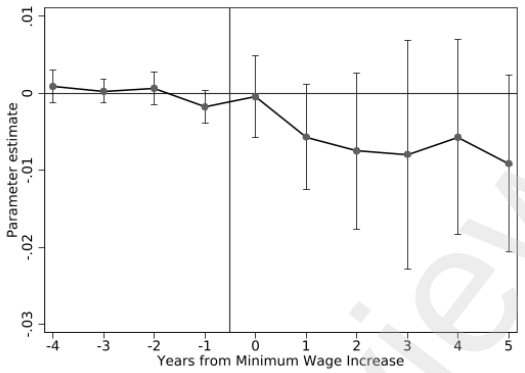
Notes: Panel A reports the trends in average state minimum wage in states that did and did not increase their statutory minimum wage during the study period. Panel B provides event study estimates with 95% confidence intervals showing the relative growth in the minimum wage. We use two-stage difference-in-differences in which the first stage estimates state fixed effects, year fixed effects, and parameters associated with the covariates using only untreated observations. See text for a list of covariates. The estimates are used to predict counterfactuals for the treated observations. The second stage takes averages of the differences between the observed and counterfactual outcomes for the treated observations.

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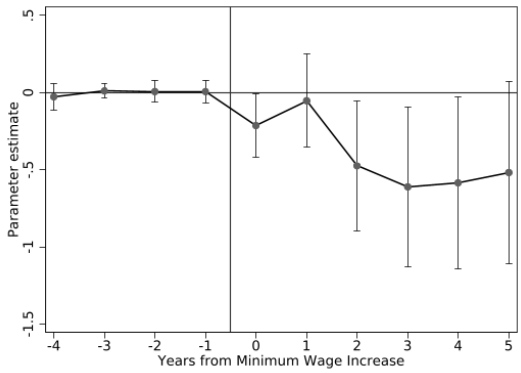
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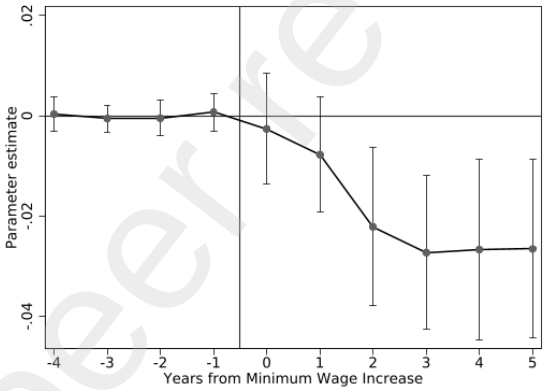
A. Employment-to-Population



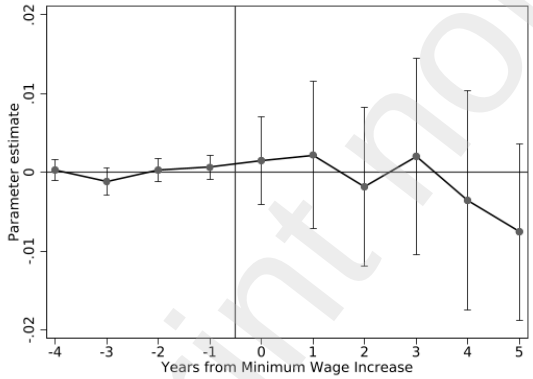
B. Labor Force Participation



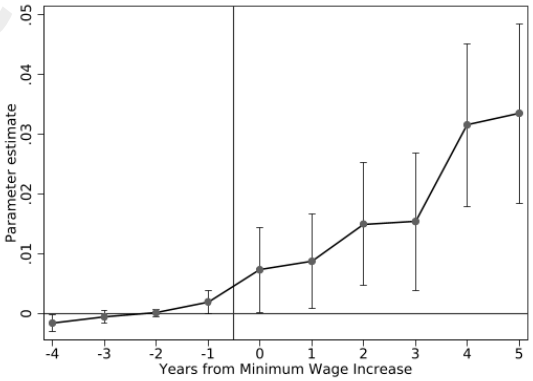
C. Hours Worked



D. Full-Time Work



E. Household Poverty



F. WIC

Figure 2: Triple-Difference-in-Differences Event Studies for Labor Market Outcomes (HS Diploma or less, ages 16-44)

Notes: Panels A-E use outcomes generated from ACS for women ages 16-44. Panel F uses information on WIC receipt from the Natality Detail Files for the sample used when studying birth outcomes. See Figure 3 notes for estimation details.

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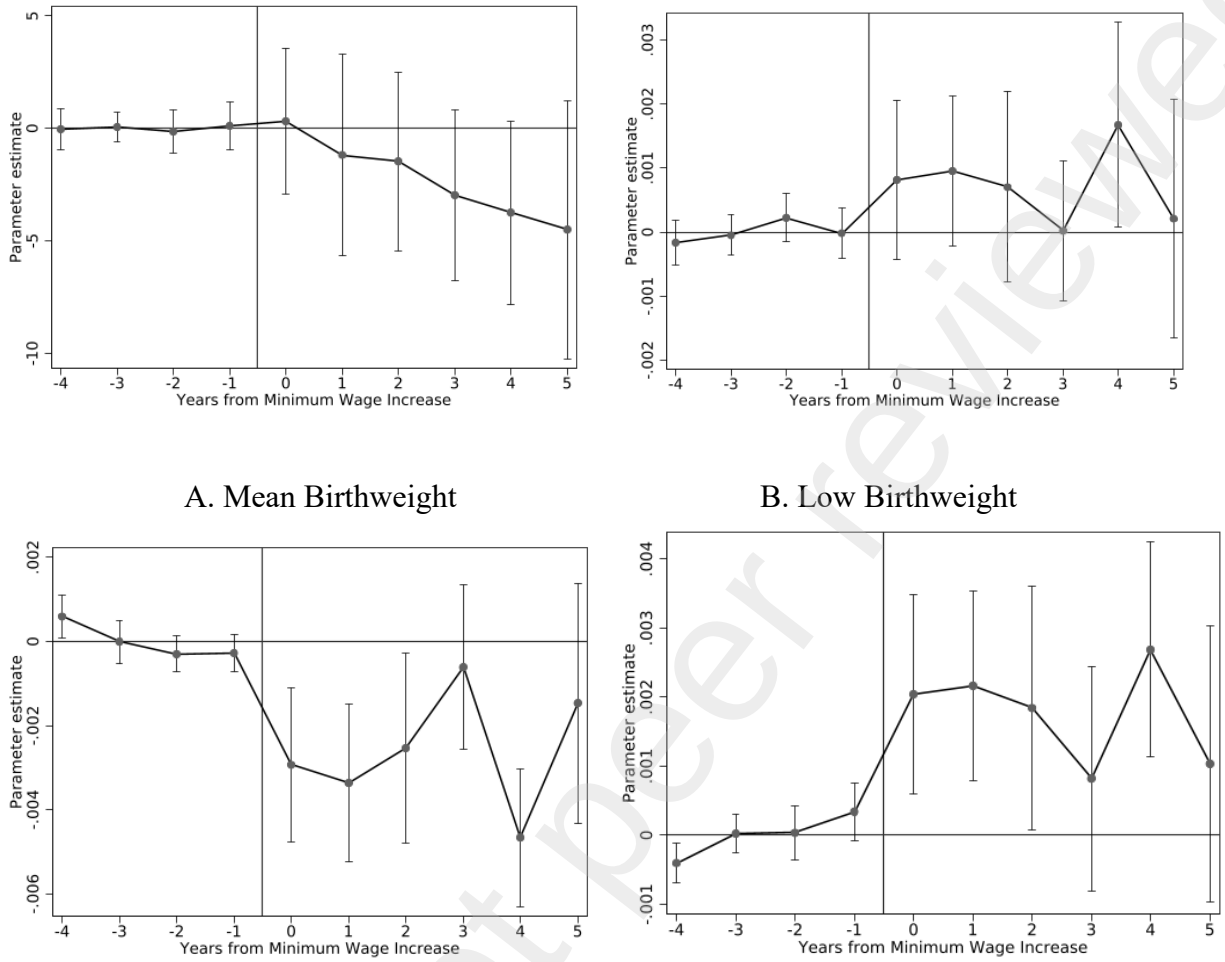
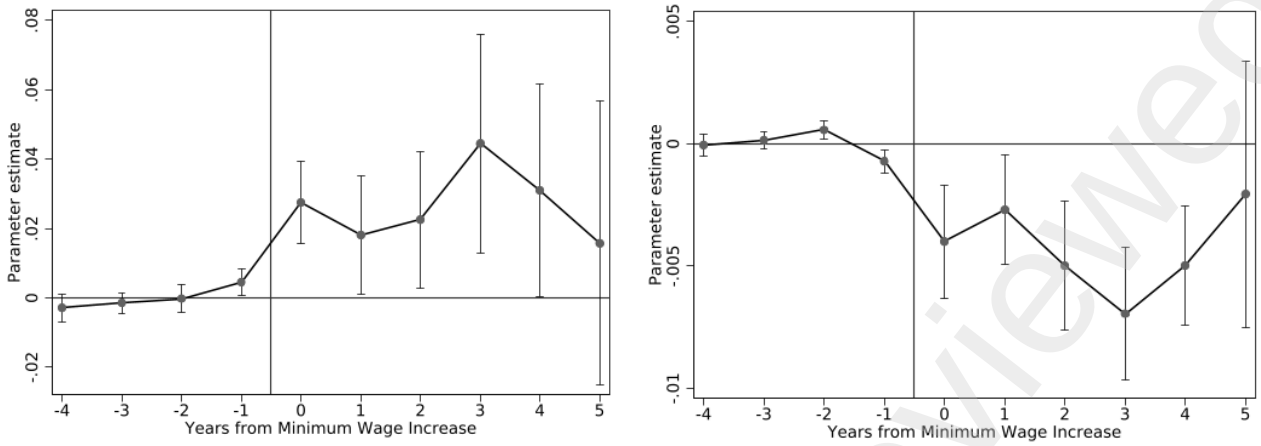


Figure 3: Triple-Differences Event Study Estimates of Relationship Between Minimum Wage Statutory Increases and Mean (Panel A), Low (Panel B), Normal (Panel C), and High (Panel D) Birthweight

Notes: Estimates and 95% confidence intervals (adjusted for state-level clustering) presented. We used two-stage difference-in-differences. In the first step, we estimate the state-group fixed effects, state-year fixed effects, group-year fixed effects, and parameters associated with the individual covariates using only untreated observations. The estimates are used to predict counterfactuals for the treated observations. See text for list of mother-specific covariates included in the model. "Group" is defined by age group and educational attainment. The treated group is mothers aged 16-44 with a high school diploma or less. The second step involves taking averages of the difference between the observed and predicted outcomes among the treated observations.

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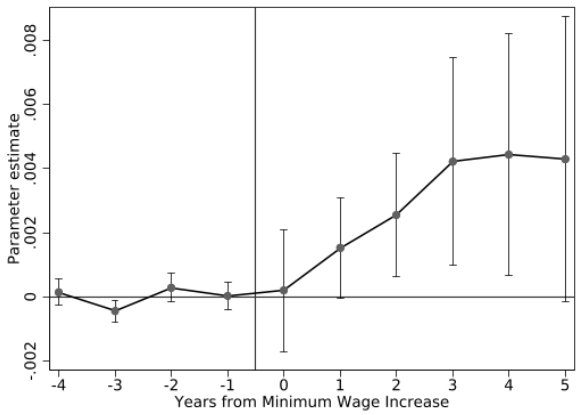
A. Gestational Age (Weeks) **B. Preterm Births**

Figure 4: Triple-Differences Event Study Estimates of Relationship Between Minimum Wage Statutory Increases and Mean Gestational Age (Weeks) and Preterm Birth

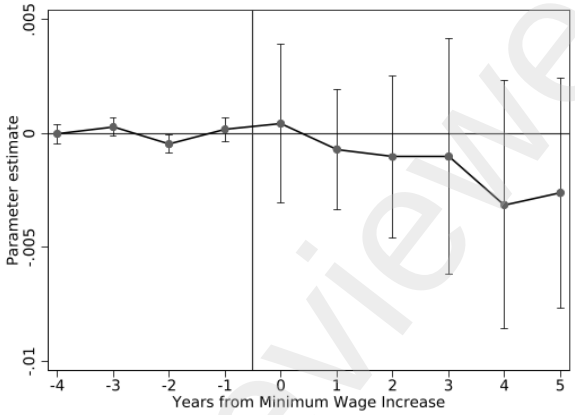
Notes: Estimates and 95% confidence intervals (adjusted for state-level clustering) presented. We used two-stage difference-in-differences. In the first step, we estimate the state-group fixed effects, state-year fixed effects, group-year fixed effects, and parameters associated with the covariates using only untreated observations. The estimates are used to predict counterfactuals for the treated observations. See text for list of mother-specific covariates included in the model. "Group" is defined by age group and educational attainment. The treated group is mothers aged 16-44 with a high school diploma or less. The second step involves taking averages of the difference between the observed and predicted outcomes among the treated observations.

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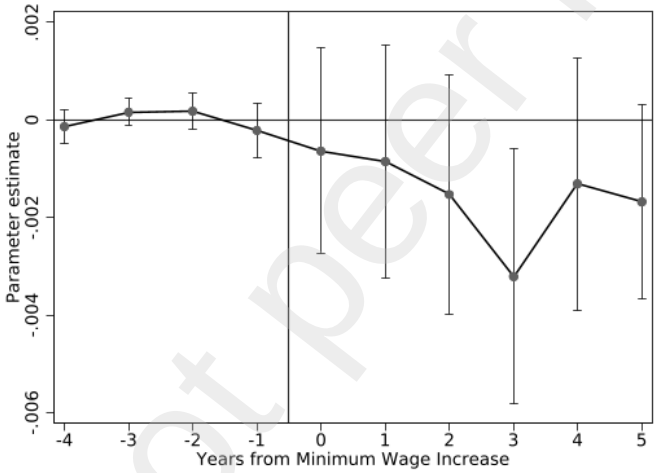
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A. Small for Gestational Age



B. Appropriate for Gestational Age



C. Large for Gestational Age

Figure 5: Triple-Differences Event Study Estimates of Relationship Between Minimum Wage Statutory Increases and Small (Panel A), Appropriate (Panel B), and Large (Panel C) for Gestational Age

Notes: Estimates and 95% confidence intervals (adjusted for state-level clustering) presented. We used two-stage difference-in-differences. In the first step, we estimate the state-group fixed effects, state-year fixed effects, group-year fixed effects, and parameters associated with the covariates using only untreated observations. The estimates are used to predict counterfactuals for the treated observations. See text for list of mother-specific covariates included in the model. "Group" is defined by age group and educational attainment. The treated group is mothers aged 16-44 with a high school diploma or less. The second step involves taking averages of the difference between the observed and predicted outcomes among the treated observations. Outcomes use the definitions in Oken et al. (2003).

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Tables

Table 1: Triple-Differences Estimates for Labor Outcomes

	Employment-to-Population Ratio	Labor Force Participation	Hours Worked	Working Full Time	Household Poverty	WIC
		Panel A: Triple Differences (without additional covariates)				
Minimum Wage Increase	-0.009*	-0.006*	-0.380**	-0.017***	-0.000	0.015***
	(0.005)	(0.003)	(0.181)	(0.006)	(0.004)	(0.005)
Counterfactual Mean	0.50	0.56	32.17	0.57	0.25	0.65
Implied Percent Change	-1.90	-1.10	-1.18	-3.00	-0.19	2.34
		Panel B: Triple Differences (with additional covariates)				
Minimum Wage Increase	-0.009*	-0.006*	-0.381**	-0.017***	-0.001	0.016***
	(0.005)	(0.003)	(0.179)	(0.006)	(0.004)	(0.005)
Counterfactual Mean	0.50	0.56	32.17	0.57	0.25	0.65
Implied Percent Change	-1.88	-1.08	-1.18	-3.01	-0.27	2.43

*Notes: ***1%, **5%, *10% Significance. Triple-difference estimates condition on state*year, state*group, and group*year interactions where "groups" are defined by age and education. Women aged 16-44 with a high school degree or less are the treated group. Estimates generated from imputation approach. Standard errors in parentheses account for the two-stage process and within-state dependence. Covariates used in Panel B include controls for race/ethnicity, age (4 age groups), and educational attainment. "Counterfactual mean" is the prediction generated from the imputation model. All outcomes are constructed using the ACS stratified by women ages 16-44, except for the final column. WIC is constructed using the Natality Detail Files for mothers aged 16-44 and additionally controls for live birth order and infant sex (in Panel B).*

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Table 2: Triple-Differences Estimates for Infant Health Outcomes

	Birthweight	Gestational Age		Birthweight			For Gestational Age		
	(in grams)	Average (weeks)	Preterm	Low	Normal	High	Small	Appropriate	Large
Panel A: Triple Differences (without additional covarites)									
Miimum Wage Increases	-0.428	0.028**	-0.004***	0.001	-0.003***	0.003***	0.002**	-0.001	-0.001
	(1.637)	(0.011)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
Counterfactual Mean	3272.99	38.68	0.11	0.06	0.86	0.07	0.12	0.81	0.08
Implied Percent Change	-0.01	0.07	-4.12	0.97	-0.39	3.81	1.87	-0.17	-1.12
Panel B: Triple Differences (with additional covariates)									
Minimum Wage Increases	-1.956	0.027***	-0.004***	0.001	-0.003***	0.002***	0.003**	-0.002	-0.001
	(1.631)	(0.010)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
Counterfactual Mean	3274.52	38.68	0.11	0.06	0.86	0.07	0.12	0.81	0.08
Implied Percent Change	-0.06	0.07	-4.02	1.35	-0.37	3.20	2.36	-0.19	-1.64

*Notes: ***1%, **5%, *10% Significance. Triple-difference estimates condition on state*year, state*group, and group*year interactions where "groups" are defined by mother's age and education. Mothers aged 16-44 with a high school degree or less are the treated group. Estimates generated from imputation approach. Standard errors in parentheses account for the two-stage process and within-state dependence. Covariates used in Panel B include controls for infant sex, live birth order, mother's race/ethnicity, mother's age (4 age groups), and mother's educational attainment. "Counterfactual mean" is the prediction generated from the imputation model. All outcomes are constructed using the Natality Detail Files for mothers aged 16-44.*

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Table 3: Triple-Differences Estimates for Infant Health Outcomes by Subgroup

	Birthweight	Gestation	Preterm	LBW	NBW	HBW	SGA	AGA	LGA
No HS Diploma, Aged 16-24	-4.950	0.037***	-0.006***	0.001	-0.002*	0.001	0.002	0.001	-0.003*
	(3.609)	(0.011)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)
Counterfactual Mean	3215.23	38.72	0.11	0.06	0.88	0.05	0.14	0.80	0.06
Implied Percent Change	-0.15	0.10	-5.31	1.26	-0.23	2.73	1.66	0.08	-5.35
No HS Diploma, Aged 25-44	-2.609	0.024**	-0.003**	0.002**	-0.004***	0.002*	0.002	-0.000	-0.002
	(2.351)	(0.010)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.004)	(0.002)
Counterfactual Mean	3288.81	38.54	0.12	0.06	0.85	0.08	0.11	0.80	0.09
Implied Percent Change	-0.08	0.06	-2.53	3.05	-0.47	2.79	2.29	-0.04	-2.38
HS Diploma (= 12), Aged 16-24	-5.437**	0.022*	-0.004***	0.001*	-0.003***	0.002**	0.004***	-0.003**	-0.001
	(2.134)	(0.013)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Counterfactual Mean	3256.63	38.85	0.09	0.06	0.87	0.06	0.13	0.81	0.06
Implied Percent Change	-0.17	0.06	-3.95	2.35	-0.38	2.91	3.21	-0.35	-2.09
HS Diploma (= 12), Aged 25-44	0.991	0.029**	-0.005***	0.000	-0.003***	0.003***	0.003**	-0.002	-0.000
	(1.529)	(0.013)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
Counterfactual Mean	3298.89	38.64	0.10	0.06	0.85	0.08	0.11	0.81	0.08
Implied Percent Change	0.03	0.07	-4.50	0.08	-0.36	3.45	2.36	-0.27	-0.37
Some college (no BA), Aged 16-24	-3.798**	0.009	-0.002**	0.001	-0.002***	0.002***	0.002*	-0.000	-0.002**
	(1.639)	(0.012)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Counterfactual Mean	3288.74	38.93	0.08	0.05	0.87	0.07	0.12	0.82	0.06
Implied Percent Change	-0.12	0.02	-2.87	1.68	-0.25	2.24	1.79	-0.06	-2.52
Some college (no BA), Age 25-44	-1.219	0.011*	-0.002***	-0.000	-0.001	0.001	0.001	-0.001	-0.001
	(0.998)	(0.006)	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
Counterfactual Mean	3331.83	38.74	0.09	0.05	0.85	0.09	0.10	0.81	0.09
Implied Percent Change	-0.04	0.03	-2.25	-0.34	-0.10	1.14	1.36	-0.06	-0.90

*Notes: ***1%, **5%, *10% Significance. Triple-difference estimates condition on state*year, state*group, and group*year interactions where "groups" are defined by mother's age and education. Estimates generated from imputation approach. Standard errors in parentheses account for the two-stage process and within-state dependence. Estimates control for infant sex, live birth order, mother's race/ethnicity, mother's age (4 age groups), and mother's educational attainment. "Counterfactual mean" is the prediction generated from the imputation model. All outcomes are constructed using the Natality Detail Files for mothers aged 16-44.*

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Table 4: Triple-Differences Estimates of Infant Health Consequences, Excluding Indexed States and Small Minimum Wage Increases (<50%)

	Birthweight	Gestational Age		Birthweight			For Gestational Age		
	(in grams)	Average (weeks)	Preterm	Low	Normal	High	Small	Appropriate	Large
				Panel A: Excluding Always Indexed States					
Miimum Wage Increases	-2.903	0.031***	-0.004***	0.001***	-0.004***	0.002***	0.003**	-0.001	-0.002
	(1.845)	(0.011)	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
Counterfactual Mean	3275.67	38.68	0.10	0.06	0.86	0.07	0.12	0.81	0.08
Implied Percent Change	-0.09	0.08	-4.10	2.45	-0.41	2.85	2.58	-0.16	-2.24
				Panel B: Excluding Always Indexed States and Small MW Hikes (<50% increases)					
Minimum Wage Increases	-2.683	0.037***	-0.005***	0.001***	-0.004***	0.002***	0.004**	-0.002	-0.002
	(1.959)	(0.010)	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
Counterfactual Mean	3283.69	38.69	0.10	0.05	0.86	0.07	0.11	0.81	0.08
Implied Percent Change	-0.08	0.09	-4.66	2.31	-0.41	2.98	3.10	-0.21	-2.40

*Notes: ***1%, **5%, *10% Significance. Triple-difference estimates condition on state*year, state*group, and group*year interactions where "groups" are defined by mother's age and education. Mothers aged 16-44 with a high school degree or less are the treated group. Estimates generated from imputation approach. Standard errors in parentheses account for the two-stage process and within-state dependence. All estimates control for infant sex, mother's race/ethnicity, mother's age (4 age groups), mother's educational attainment, and live birth order. "Counterfactual mean" is the prediction generated from the imputation model. All outcomes are constructed using the Natality Detail Files for mothers aged 16-44.*

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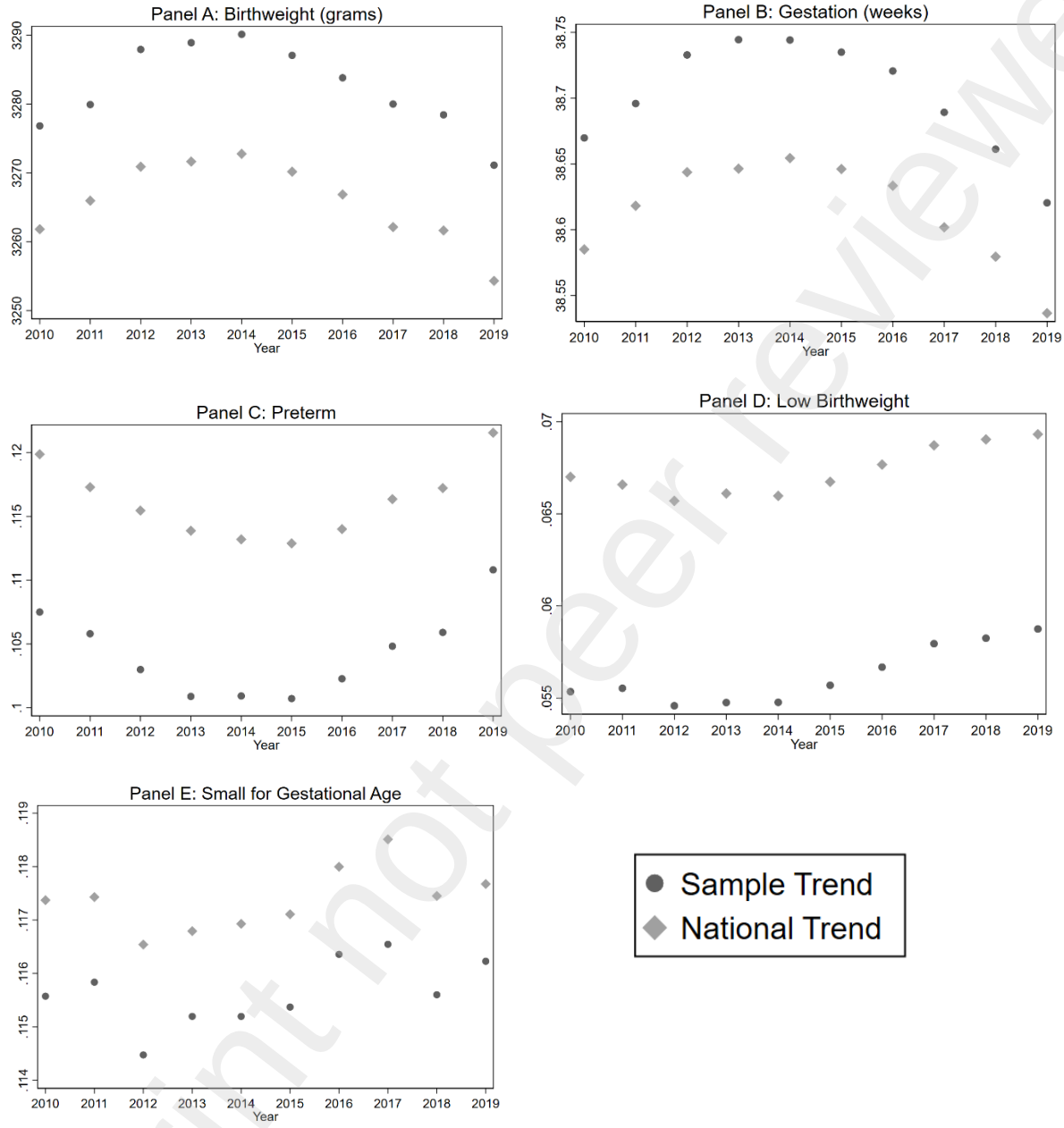
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Online Appendix

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Figure A1: Unadjusted Sample and National Trends in Birth Outcomes

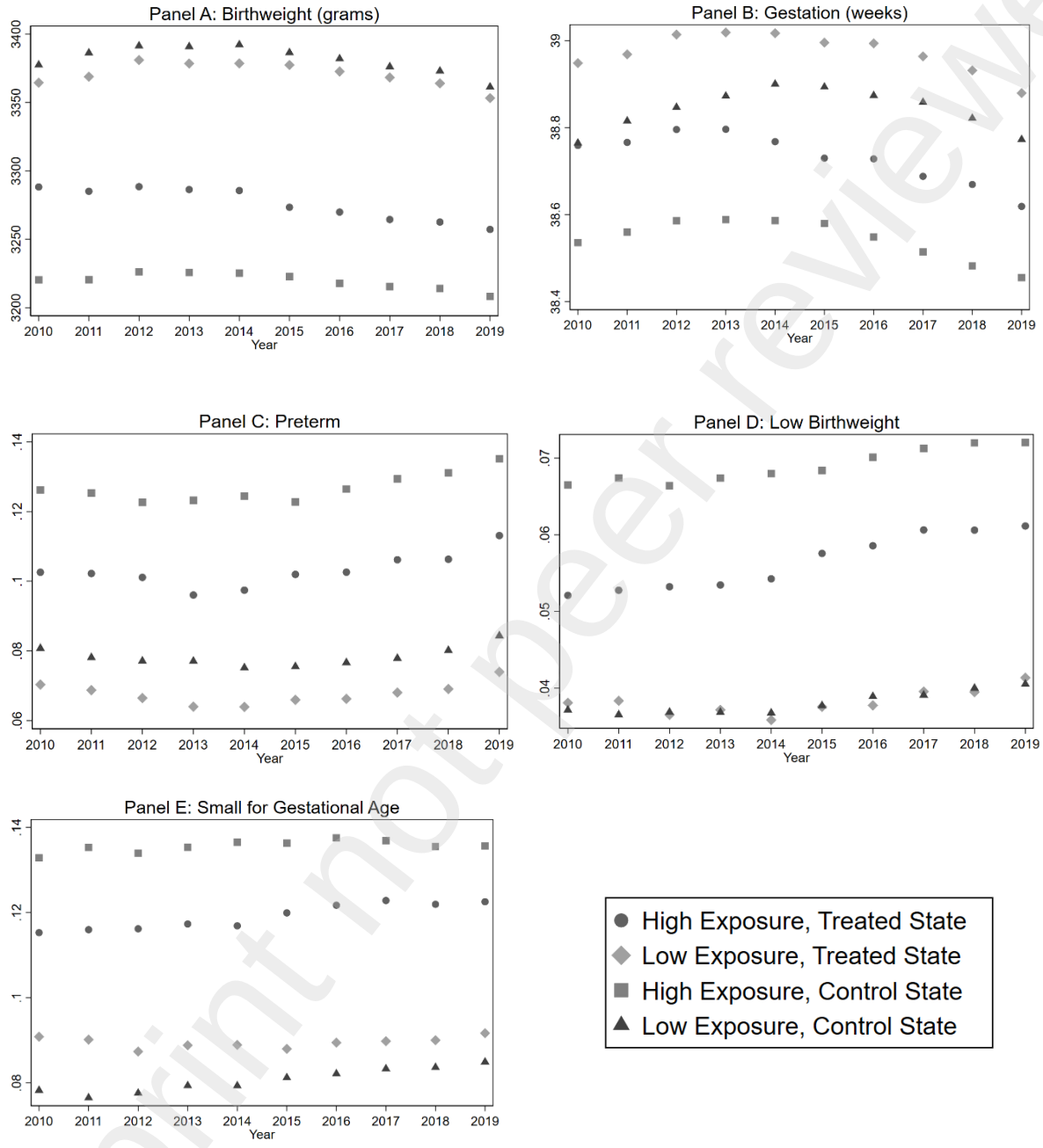


Notes: The sample trend consists of state-year-groups that were included in the main regression analysis reported in Table 2. The national trend includes the universe of births during the study period, including state-years with missing mother's education.

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Figure A2: Unadjusted Trends in Birth Outcomes by Mother's Age and Educational Attainment and State Treatment Group



Notes: High exposure group indicates that the mother has a high school diploma or less. Low exposure group indicates that the mother has a college diploma or higher and is aged 25+. Treated state indicates states that were ever treated, while control state indicates states that were never treated. State-years with missing mother's education were dropped from trends.

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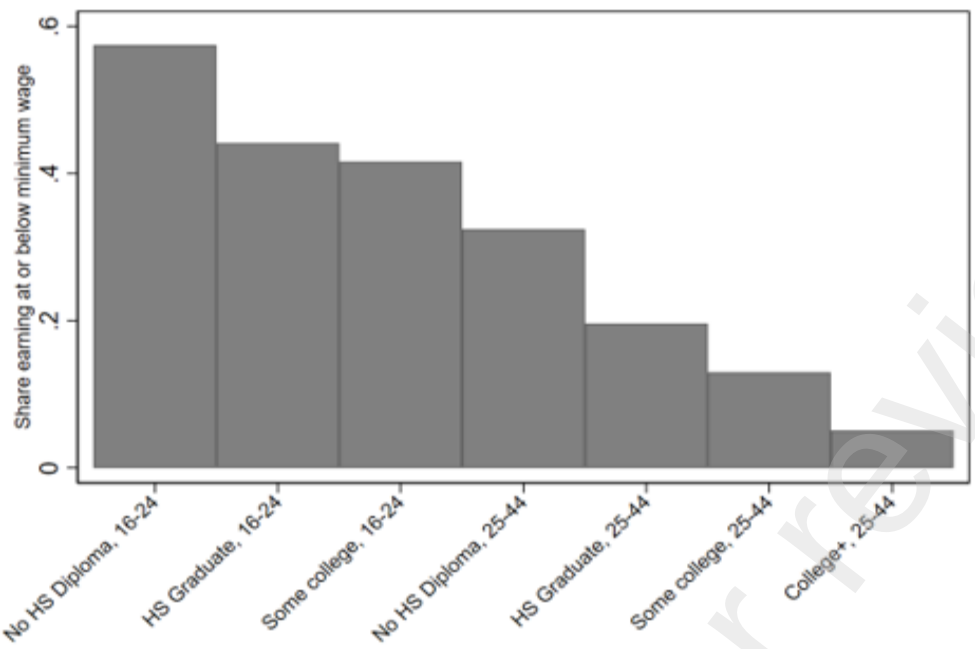


Figure A3. Share earning at or below the prevailing minimum wage, by education and age groups

Notes: The definition of shares affected by minimum wages is derived from Card (1992). Estimates are derived from our ACS sample of women of childbearing age.

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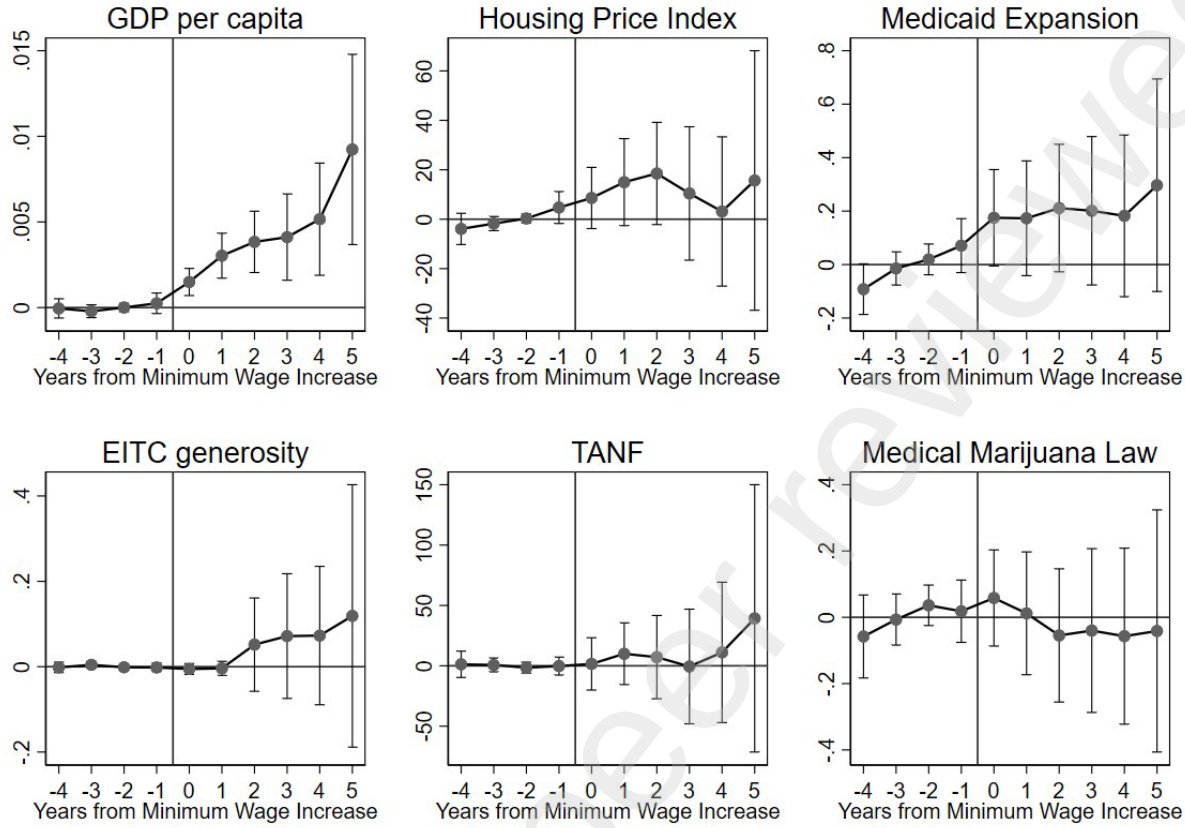


Figure A4. Difference-in-Differences event studies for concurrent policy adoption

Notes: Difference-in-differences event study estimates include state and year fixed effects. Estimates generated from imputation approach. Standard errors in parentheses account for the two-stage process and within-state dependence.

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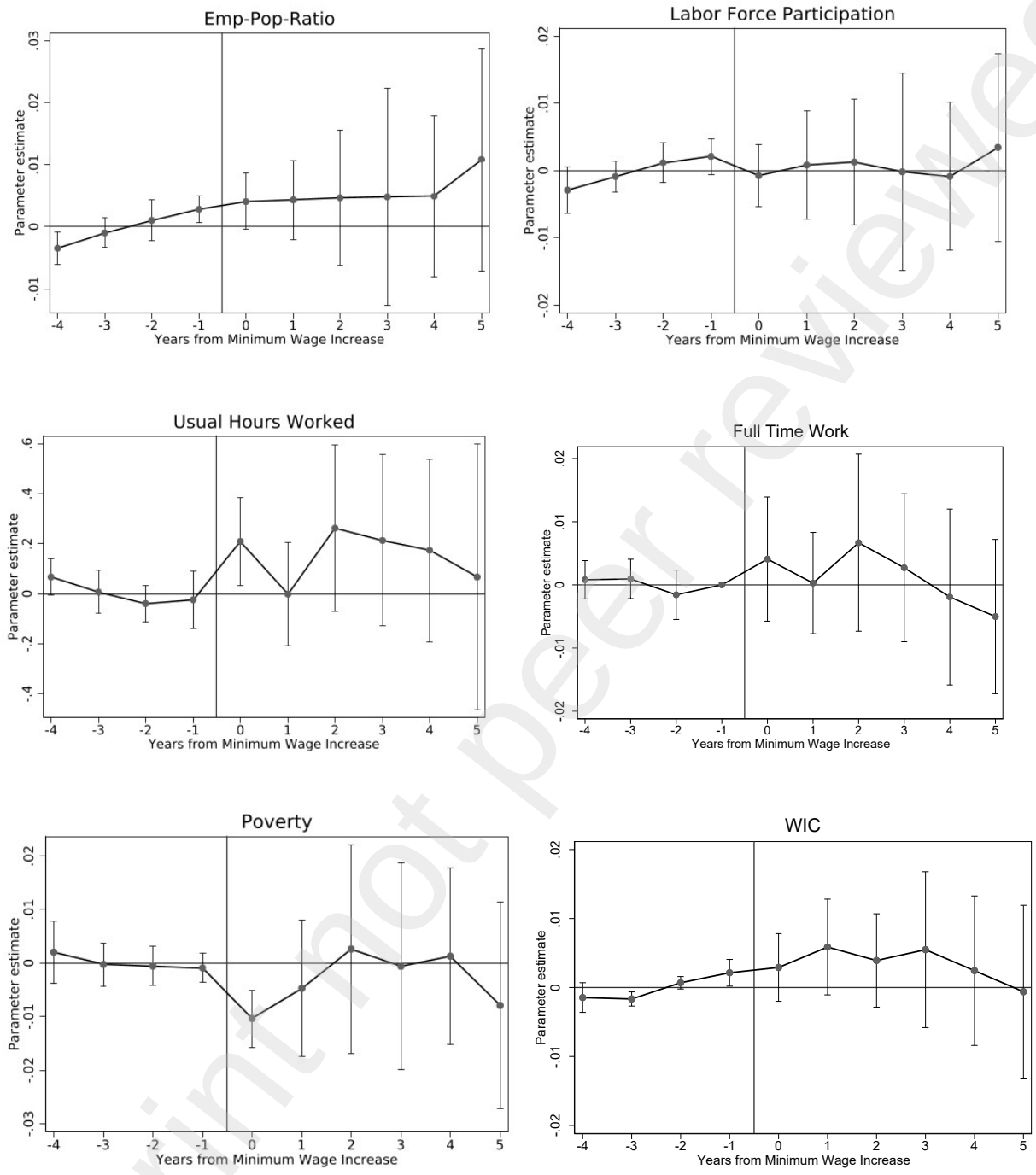


Figure A5: Difference-in-Differences estimates for labor market outcomes among women with a college degree aged 25-44

Notes: Difference-in-differences event study estimates condition on state and year fixed effects. Estimates generated from imputation approach. Standard errors account for the two-stage process and within-state dependence. Covariates include race/ethnicity, age (4 age groups), educational attainment, state EITC as a percentage of federal EITC, AFDC/TANF maximum monthly benefits, housing price index, and medical marijuana laws. WIC is constructed using the Natality Detail Files for mothers aged 16-44 and additionally controls for live birth order and infant sex.

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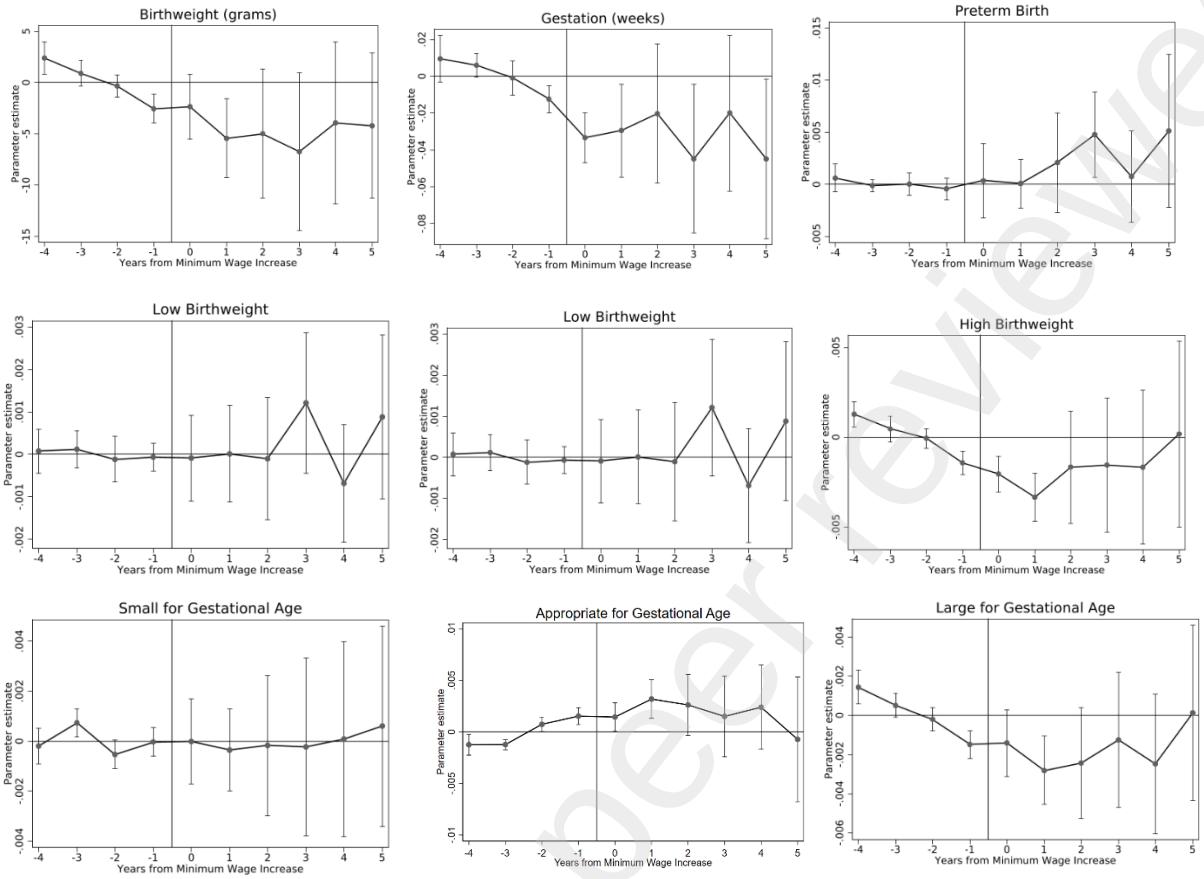


Figure A6: Difference-in-differences event studies for natality outcomes for mothers with a college degree aged 25-44

Notes: Difference-in-differences event study estimates condition on state and year fixed effects. Estimates generated from imputation approach. Standard errors account for the two-stage process and within-state dependence. Covariates include infant sex, race/ethnicity, age (4 age groups), educational attainment, live birth order, state EITC as a percentage of federal EITC, AFDC/TANF maximum monthly benefits, housing price index, and medical marijuana laws.

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Table A1. Treated States and Timing of Minimum Wage Hikes

State	Year of first statutory MW hike post-pause	MW in year prior to first hike	MW in 2019	Cumulative Increase in MW	Implied Percent Change
Alaska	2015	7.75	10.19	2.44	31%
Arizona	2017	8.05	12	3.95	49%
Arkansas	2015	7.25	9.25	2	28%
California	2014	8	12	4	50%
Colorado	2017	8.31	12	3.69	44%
Connecticut	2014	8.25	11	2.75	33%
Delaware	2014	7.25	9.25	2	28%
District of Columbia	2014	8.25	14	5.75	70%
Hawaii	2015	7.25	10.1	2.85	39%
Illinois	2019	8.25	9.25	1	12%
Maine	2017	7.5	12	4.5	60%
Maryland	2015	7.25	11	3.75	52%
Massachusetts	2015	8	12.75	4.75	59%
Michigan	2014	7.4	9.65	2.25	30%
Minnesota	2014	7.25	10	2.75	38%
Missouri	2019	7.85	9.45	1.6	20%
Nebraska	2015	7.25	9	1.75	24%
Nevada	2011	7.55	8.25	0.70	9%
New Jersery	2014	7.25	11	3.75	52%
New York	2014	7.25	11.8	4.55	63%
Oregon	2016	9.25	11.25	2	22%
Rhode Island	2013	7.4	10.5	3.1	42%
South Dakota	2015	7.25	9.3	2.05	28%
Vermont	2015	8.73	10.96	2.23	26%
Washington	2017	9.47	13.5	4.03	43%
West Virginia	2015	7.25	8.75	1.5	21%

Notes: States highlighted in gray are those that are excluded from the sample due to being always treated or because that state does not have at least 3 pre-treatment years, after omitting state-years with missing mother's education data. After these exclusions, we have 17 treated, and 25 never treated states.

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Table A2: Summary of Infant Health and Labor Market Outcome Variables

Outcome	Description
Panel A: Infant Health Measures	
Small for gestational age	=1 if below 10th percentile of size for gestational age
Low birthweight	=1 if birthweight < 2500 grams, 0 otherwise
Appropriate for gestational age	=1 if between the 10th and 90th percentile of size for gestational age
Normal birthweight	=1 if birthweight between 2500-4000 grams, 0 otherwise
Large for gestational age	=1 if above the 90th percentile of size for gestational age
High birthweight	=1 if birthweight greater than 4000 grams, 0 otherwise
Birthweight	Birthweight in grams
Gestational Age	Gestational age in weeks
Preterm	=1 if gestational age < 37 weeks, 0 otherwise
Panel B: Labor Outcomes	
Employment rate	=1 if an individual is employed, 0 otherwise.
Labor force participation	=1 if an individual is in the labor force, 0 otherwise
Hours worked	The number of hours worked in a week conditional on any work
Working Full Time	=1 if an individual is working at least 35 hours per week, 0 if working 1-34 hours
WIC Receipt	=1 if mother receives WIC, 0 otherwise
Poverty	=1 if a household is below the official poverty threshold, 0 otherwise

Notes: SGA/AGA/LGA are constructed using supplementary files from Oken et al. (2003).

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Table A3. Means of Covariates in Study Sample

	(1) Untreated States	(2) Treated States	(1)-(2) T-test P-value
Demographic and Policy Controls			
Race = Non-Hispanic White	0.565	0.483	P<0.01
Race = Non-Hispanic Black	0.175	0.115	P<0.01
Race = Non-Hispanic Other	0.047	0.104	P<0.01
Race = Hispanic	0.208	0.284	P<0.01
First Birthchild	0.385	0.399	P<0.01
Aged 16-24	0.312	0.246	P<0.01
Aged 25-44	0.688	0.754	P<0.01
Female Infant	0.488	0.488	0.797
Mothers with No HS Diploma	0.153	0.151	0.268
Mothers with a HS Diploma	0.273	0.236	P<0.01
Mothers with Some College but no Bachelor's Degree	0.296	0.281	P<0.01
Mothers with Bachelor's Degree+	0.278	0.331	P<0.01
ACA expansion state	0.232	0.519	P<0.01
State EITC Rate as Percentage of Federal Credit	0.037	0.152	P<0.01
AFDC/TANF Maximum Monthly Benefit for 4-person family	442.293	615.742	P<0.01
Housing Price Index	227.226	246.467	P<0.01
Medical Marijuana Law	0.215	0.725	P<0.01
Infant Health Outcomes			
Small for Gestational Age	0.113	0.105	P<0.01
Appropriate for Gestational Age	0.806	0.813	0.036
Large for Gestational Age	0.081	0.082	P<0.01
Birthweight (Grams)	3283.516	3318.790	P<0.01
Low Birthweight	0.057	0.049	P<0.01
Normal Birthweight	0.855	0.856	P<0.01
High Birthweight	0.077	0.086	P<0.01
Preterm Birth (<37 Weeks)	0.107	0.088	P<0.01
Gestation (Weeks)	38.61	38.815	P<0.01
Labor Market Outcomes			
Employment-to-population ratio	0.650	0.651	0.038
Labor Force Participation	0.709	0.713	P<0.01
Hours Worked in a Week	35.020	34.692	P<0.01
Working Full-Time	0.666	0.651	P<0.01
WIC Receipt	0.427	0.421	P<0.01
Household Poverty Status	0.458	0.437	P<0.01

Notes: Means and proportions reported for analytic sample, 2010-2019. P-values from a t-test for difference in means reported in third column.

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Table A4: Difference-in-Differences Estimates for Labor Outcomes (Women Aged 16-44 with a High School Degree or less)

	Employment-to-population ratio	Labor Force Participation	Hours Worked	Working Full Time	Household Poverty	WIC
			Panel A: Difference-in-Differences (without additional covariates)			
Minimum Wage Increase	0.003	0.002	-0.109	-0.005	-0.005	0.020***
	(0.005)	(0.003)	(0.181)	(0.007)	(0.006)	(0.006)
Counterfactual Mean	0.49	0.55	31.90	0.56	0.25	0.65
Implied Percent Change	0.67	0.39	-0.34	-0.98	-1.88	3.12
			Panel B: Differences-in-Differences (with additional covariates)			
Minimum Wage Increase	-0.002	-0.007	-0.457*	-0.020**	0.004	0.045**
	(0.009)	(0.005)	(0.238)	(0.010)	(0.006)	(0.022)
Counterfactual Mean	0.49	0.56	32.25	0.57	0.25	0.62
Implied Percent Change	-0.32	-1.24	-1.42	-3.57	1.66	7.29

*Notes: ***1%, **5%, *10% Significance. Difference-in-differences estimates condition on state and year fixed effects. Estimates generated from imputation approach. Standard errors in parentheses account for the two-stage process and within-state dependence. Covariates in Panel B include race/ethnicity, age (4 age groups), educational attainment, state EITC as a percentage of federal EITC, AFDC/TANF maximum monthly benefits, housing price index, and medical marijuana laws. "Counterfactual mean" is the prediction generated from the imputation model. All outcomes are constructed using the ACS stratified by women ages 16-44 with a high school degree or less, except for the final column. WIC is constructed using the Natality Detail Files for mothers aged 16-44 and additionally controls for live birth order and infant sex.*

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Table A5: Triple-Differences Estimates for Labor Outcomes by Subgroup

	Employment -to-population ratio	Labor Force Participation	Hours Worked	Working Full Time	Poverty	WIC
No HS Diploma, 16-24	-0.016*	-0.011	-0.567***	-0.019***	-0.001	0.014***
	(0.009)	(0.007)	(0.120)	(0.005)	(0.005)	(0.005)
Counterfactual Mean	0.24	0.30	20.93	0.20	0.20	0.74
Implied Percent Change	-6.69	-3.75	-2.71	-9.68	-0.69	1.93
No HS Diploma, 25-44	-0.014**	-0.012**	-0.568	-0.026	0.003	0.022***
	(0.005)	(0.006)	(0.457)	(0.018)	(0.005)	(0.006)
Counterfactual Mean	0.48	0.55	35.15	0.67	0.38	0.74
Implied Percent Change	-2.82	-2.17	-1.62	-3.89	0.67	2.94
HS Diploma (= 12), Aged 16-24	-0.016***	-0.007*	-0.316	-0.016	-0.005	0.013**
	(0.006)	(0.003)	(0.251)	(0.010)	(0.004)	(0.006)
Counterfactual Mean	0.57	0.66	29.09	0.43	0.23	0.68
Implied Percent Change	-2.79	-1.00	-1.09	-3.72	-2.05	1.89
HS Diploma (= 12), age 25-44	0.001	0.001	-0.269**	-0.013***	-0.007	0.016**
	(0.005)	(0.004)	(0.129)	(0.005)	(0.005)	(0.006)
Counterfactual Mean	0.64	0.70	35.91	0.71	0.23	0.56
Implied Percent Change	0.12	0.09	-0.75	-1.80	-3.01	2.81
Some college (no BA), Aged 16-24	-0.006	-0.007	-0.461***	-0.022***	0.012**	0.013*
	(0.005)	(0.005)	(0.158)	(0.006)	(0.005)	(0.008)
Counterfactual Mean	0.61	0.66	27.17	0.36	0.22	0.58
Implied Percent Change	-1.04	-1.10	-1.70	-6.20	5.28	2.27
Some college (no BA), Age 25-44	-0.000	-0.002	-0.225**	-0.008*	-0.003	0.009***
	(0.004)	(0.003)	(0.105)	(0.005)	(0.003)	(0.002)
Counterfactual Mean	0.74	0.79	36.44	0.72	0.15	0.37
Implied Percent Change	-0.01	-0.21	-0.62	-1.10	-1.82	2.38

*Notes: ***1%, **5%, *10% Significance. Triple-difference estimates condition on state*year, state*group, and group*year interactions where "groups" are defined by age and education. The first column defines the treated group. Estimates generated from imputation approach. Standard errors in parentheses account for the two-stage process and within-state dependence. Covariates used in Panel B include controls for race/ethnicity, age (4 age groups), and educational attainment. "Counterfactual mean" is the prediction generated from the imputation model. All outcomes are constructed using the ACS stratified by women ages 16-44, except for the final column. WIC is constructed using the Natality Detail Files for mothers aged 16-44 and additionally controls for live birth order and infant sex.*

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Table A6: Difference-in-Differences Estimates for Infant Health Outcomes (Mothers Aged 16-44 with a high school degree or less)

	Birthweight	Gestational Age		Birthweight			For Gestational Age		
	(in grams)	Average (weeks)	Preterm	Low	Normal	High	Small	Appropriate	Large
				Panel A: Difference-in-Differences (without additional covarites)					
Miimum Wage Increases	-6.671***	-0.020	-0.004***	0.001	0.001	-0.001***	0.002**	0.002*	-0.004***
	(2.173)	(0.016)	(0.001)	(0.000)	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)
Counterfactual Mean	3279.24	38.73	0.11	0.06	0.86	0.08	0.12	0.80	0.08
Implied Percent Change	-0.20	-0.05	-4.10	1.27	0.08	-1.88	1.57	0.26	-5.07
				Panel B: Difference-in-Differences (with additional covariates)					
Minimum Wage Increases	-13.737***	-0.043	0.002	0.002*	-0.000	-0.002**	0.003**	0.001	-0.004***
	(4.807)	(0.040)	(0.005)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)
Counterfactual Mean	3286.30	38.75	0.10	0.06	0.86	0.08	0.12	0.81	0.08
Implied Percent Change	-0.42	-0.11	2.42	3.86	-0.01	-3.10	2.72	0.09	-5.01

*Notes: ***1%, **5%, *10% Significance. Difference-in-differences estimates condition on state and year fixed effects. Estimates generated from imputation approach. Standard errors in parentheses account for the two-stage process and within-state dependence. Covariates in Panel B include infant sex, race/ethnicity, age (4 age groups), educational attainment, birth order, state EITC as a percentage of federal EITC, AFDC/TANF maximum monthly benefits, housing price index, and medical marijuana laws. "Counterfactual mean" is the prediction generated from the imputation model. All outcomes are constructed using the Natality Detail Files for women ages 16-44 with a high school degree or less.*

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Table A7. Triple-Differences Estimates for Labor Outcomes Excluding Always Indexed States and Small MW Increases

	Employment-to-Population Ratio	Labor Force Participation	Hours Worked	Working Full Time	Household Poverty	WIC
			Panel A: Excluding Always Indexed States			
Minimum Wage Increase	-0.010*	-0.007*	-0.354*	-0.017**	-0.002	0.022***
	(0.005)	(0.004)	(0.207)	(0.007)	(0.005)	(0.005)
Counterfactual Mean	0.48	0.55	31.75	0.55	0.26	0.69
Implied Percentage Change	-1.97	-1.29	-1.10	-3.06	-0.62	3.28
			Panel B: Excluding Always Indexed States and Small MW increasers (<50% increase)			
Minimum Wage Increase	-0.014***	-0.008*	-0.415*	-0.020**	-0.002	0.022***
	(0.005)	(0.005)	(0.243)	(0.009)	(0.005)	(0.005)
Counterfactual Mean	0.48	0.54	31.96	0.56	0.25	0.69
Implied Percentage Change	-2.77	-1.50	-1.28	-3.44	-0.88	3.28

*Notes: ***1%, **5%, *10% Significance. Triple difference estimates condition on state*year, state*group, and group*year interactions where "groups" are defined by age and education. Women aged 16-44 with a high school degree or less are the treated group. Estimates generated from imputation approach. Standard errors in parentheses account for the two-stage process and within-state dependence. All estimates control for race/ethnicity, age (4 age groups), and educational attainment. "Counterfactual mean" is the prediction generated from the imputation model. All outcomes are constructed using the ACS stratified by women ages 16-44, except for the final column. WIC is constructed using the Natality Detail Files for mothers aged 16-44 and additionally controls for live birth order and infant sex.*

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Table A8. Triple-Differences Estimates for Labor Outcomes using All States in the ACS

	Employment-to-Population Ratio	Labor Force Participation	Hours Worked	Working Full Time	Household Poverty
Panel A: Full Sample of States					
Minimum Wage Increase	-0.007	-0.005*	-0.391***	-0.017***	0.000
	(0.005)	(0.003)	(0.146)	(0.005)	(0.004)
Counterfactual Mean	0.50	0.57	32.10	0.57	0.25
Implied Percentage Change	-1.47	-0.96	-1.22	-2.91	0.08
Panel B: Excluding Always Indexed States					
Minimum Wage Increase	-0.008	-0.006*	-0.366**	-0.016***	-0.001
	(0.005)	(0.004)	(0.162)	(0.006)	(0.004)
Counterfactual Mean	0.50	0.57	32.03	0.56	0.25
Implied Percentage Change	-1.55	-1.08	-1.14	-2.92	-0.26
Panel C: Excluding Always Indexed States and Small MW increasers (<50% increase)					
Minimum Wage Increase	-0.014***	-0.009*	-0.397*	-0.019**	-0.001
	(0.005)	(0.005)	(0.220)	(0.008)	(0.005)
Counterfactual Mean	0.49	0.55	32.31	0.58	0.25
Implied Percentage Change	-2.88	-1.55	-1.23	-3.32	-0.35

*Notes: ***1%, **5%, *10% Significance. Sample does not exclude state-years that were missing in birth records data. Triple-difference estimates condition on state*year, state*group, and group*year interactions where "groups" are defined by age and education. Women aged 16-44 with a high school degree or less are the treated group. Estimates generated from imputation approach. Standard errors in parentheses account for the two-stage process and within-state dependence. All estimates control for race/ethnicity, age (4 age groups), and educational attainment. "Counterfactual mean" is the prediction generated from the imputation model. All outcomes are constructed using the ACS stratified by women ages 16-44.*

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