Inducing Labor: the Impact of Health Insurance on Post-Natal Labor Supply

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ABSTRACT

We analyze the role that access to health insurance plays in the widely documented and sharp fall in a mother's labor supply following childbirth. Our analysis uses administrative tax data and exploits variation created by the Affordable Care Act (ACA), which substantially expanded access to health insurance within the U.S. We find that mothers' relative post-childbirth employment increases by 10% for births that occur after the expansion and the employment response is pervasive across mothers' pre-birth characteristics. Our analysis suggests improved maternal health care utilization is a contributing factor to the response.

Keywords: child penalty, taxes, labor supply, health insurance

JEL Classification: H1, I13, J13

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1. Introduction

Childbirth gives rise to a notable divergence in labor supply between mothers and fathers, and contributes to the well-documented gender-wage gap (Kleven, Landais and Søgaard, 2019). Although this "child penalty" has been observed in several settings (Angelov, Johansson and Lindahl, 2016; Kleven et al., 2019), the underlying mechanisms are not well understood. One potential mechanism is the health effect of the pregnancy and childbirth, which is uniquely experienced by mothers and may directly reduce mothers' employment. We examine the role of health insurance and health care as a contributor to the U.S. child penalty in the context of the Affordable Care Act (ACA), which substantially increased access to insurance and especially to prenatal care.

We estimate the effect of access to health insurance on differences in post-childbirth employment by comparing mothers and fathers of children born just before and just after the implementation of the ACA.[1] The ACA sharply expanded health insurance access in the U.S through two primary channels: (1) by providing a marketplace for individuals to purchase affordable, comprehensive health insurance outside of employment, and (2) by incentivizing states to expand access to public insurance for low-income residents. Our analysis implements a differences-in-differences-in-differences empirical model (DDD) that compares (1) mothers' and fathers' employment, (2) before and after childbirth, (3) for those births that occur just before and just after the 2014 implementation of the ACA. This analysis relies on nearly 3 million first-births drawn from a combination of Social Security birth records and administrative tax data. We find that the ACA health insurance expansion reduced post-birth gendered employment gaps by 10 percent (1.2 percentage points).

[1]Note that the difference in employment between mothers and fathers after childbirth relative to the pre-childbirth gap is the numerator of the child penalty measure from previous literature (Kleven, Landais and Søgaard (2019); Kleven et al. (2019); Kleven, Landais and Søgaard (2021)

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Increasing access to health insurance in the U.S. has an ambiguous impact on gendered post-birth employment differences, and is therefore left as an empirical question. On the one hand, the ACA weakened the incentive for employment by increasing access to health insurance outside of an employer-employee relationship.[2] All else equal, this should reduce employment among employees who work to gain access to employer-sponsored health insurance (ESI). To the extent that this pressure disproportionately affects mothers during pregnancy and childbirth, the ACA will tend to reduce mothers' post-birth employment compared to fathers' post-birth employment. On the other hand, increased access to health insurance should increase health insurance coverage and health care utilization. If the increased access to health insurance improves mothers' health during pregnancy and childbirth, the ACA will tend to increase mothers' relative post-birth employment.

In our analysis, we study the population of mothers who give birth to their first child in 2013 or in 2015, i.e., just before and just after the 2014 implementation of the ACA.[3] We restrict these data to mothers aged 26–44 years both to ensure that we capture a meaningful pre-birth labor supply measure among parents and to avoid confounding factors from an early provision of the ACA that affected young adults. We obtain information on individual earnings and filing characteristics, such as marital status (joint filer vs. non-joint filer) and address for mothers and fathers from tax data.[4] We then determine whether or not a parent worked in a given year based on the presence of any individual earnings and create a panel data set that tracks parental employment from three years before birth to four years after birth.

[2]More than half of individuals in the U.S. are covered by employer-sponsored health insurance, which requires wage employment(Lurie and Pearce, 2021).

[3]We omit 2014 births from our analysis because they are partially treated by the implementation of the ACA — health plans rolled into the requirements of the ACA throughout 2014 based on the start date of the new plan year.

[4]Our dataset consists of the population of first-births in 2013 and 2015 based on Social Security application information. Identifying information for fathers is missing for about 12% of first-births in these years.

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We find that the ACA reduced post-birth gendered gaps in employment by 10 percent (1.2 percentage points). We document stability in post-birth gendered employment gap in the five years prior to the ACA, supporting our hypothesis that the change we observe is due to the ACA health insurance expansion. We estimate that this 10 percent reduction is driven by a combination of more entrants into and fewer exits from post-birth employment among mothers giving birth after the ACA.

Next, we investigate several dimensions of heterogeneity that are likely to be correlated with exposure to the ACA's expansion of insurance access. These include mothers' pre-birth income, which is correlated with increased eligibility for public insurance (Medicaid) and tax subsidies to purchase private insurance, mothers' pre-birth marital status (as proxied by filing status), which is related to the availability of outside insurance options through a spouse, and geographic variation directly tied to the ACA based on whether or not states elected to expand access to Medicaid. In all cases we find variation in the magnitude of the effect of the ACA on post-birth employment differences that is consistent with the expected variation in exposure to the ACA. Moreover, these tests provide robust evidence that the ACA reduced post-birth employment gaps for all mothers, regardless of income, martial status, and geography, which is consistent with the broad scope of the ACA to affect health insurance coverage.

Finally, we investigate the role that ACA-driven maternal and infant health improvements plays in increasing mothers' post-birth relative employment. We draw on external data sources for coverage (Medical Expenditures Panel Survey, MEPS), which is unobserved in the tax data prior to the ACA, and prenatal health care utilization (Centers for Disease Control), which is unobservable in the tax data. We use these external data to demonstrate two facts: mothers giving birth after the ACA are (1) more likely to be covered by health insurance during pregnancy, childbirth, and the postnatal period, and (2) more likely to use health care (early prenatal visits) during pregnancy. In addition, the medical literature has found that early prenatal care

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improves maternal and infant health (Evans and Lien, 2005; Corman, Dave and Reichman, 2018; Yan, 2017), and that the ACA has improved access to pregnancy related health care for women and has improved perinatal outcomes for infants (Lee et al., 2020). Taken together, the descriptive evidence that we provide supports the hypothesis that expanded access to health care under the ACA increased mothers' coverage and utilization. If these increases in coverage and in care allowed mothers to better manage the health effects of their pregnancy, then this could improve mothers' ability to work.

Our work contributes to two primary strands of literature. The first looks at the effect of the ACA on labor supply. Past work has exploited both geographic variation in the Medicaid expansion and the provision of the ACA that allowed young adults to remain on parents' policies until age 26; these studies find that the ACA did not induce significant changes in labor supply. (e.g. Heim, Lurie and Simon, 2015; Gooptu et al., 2016; Leung and Mas, 2018; Kaestner et al., 2017). By contrast, we find an unambiguously positive impact of the ACA on mothers' post-birth labor supply based on an analysis that combines both a health shock (i.e. pregnancy) and expanded access to health insurance.

In addition, our study contributes to prior work that documents a long-run, persistent child penalty for mothers in Scandinavian countries (Angelov, Johansson and Lindahl, 2016; Kleven, Landais and Søgaard, 2019) and for mothers in a broader set of developed countries (Kleven et al., 2019). Our estimates, when scaled appropriately, measure the child penalty in the U.S. based on high-quality administrative data for the population of first-birth mothers in 2013 and 2015. Specifically, we follow methodology from Kleven, Landais and Søgaard (2019), scaling our estimate of the gendered post-birth employment gaps—which we estimated to decline by 10 percent post-ACA— by a predicted counterfactual employment level in the absence of childbirth. We estimate that U.S. mothers who gave birth just prior to the ACA in 2013 faced a 14 percent employment penalty and a 28 percent earnings penalty for

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up to four years post-birth. Our estimate of the earnings child penalty, measured by wages, is similar in magnitude to existing estimates of the U.S. child penalty that were based on an earlier time period for a younger cohort of mothers (Kleven et al., 2019; Bailey et al., 2019).[5]

Although the child penalty has been observed in several contexts, evidence on its underlying mechanisms has been drawn exclusively from the Scandinavian context (Angelov, Johansson and Lindahl, 2016; Kleven, Landais and Søgaard, 2019; Kleven et al., 2019). In a study most related to ours, Kleven, Landais and Søgaard (2021) examines the role that biology plays in driving the child penalty for Danish mothers, finding evidence that biology may contribute to the child penalty during the first few years post-birth, but that these differences dissipate in the long run. We estimate that the ACA reduced the employment and earnings child penalty by 7.8 percent (1.1 percentage points) and 4.2 percent (1.1 percentage points), respectively. Our analysis supports an additional mechanism for the child penalty—access to health insurance—that is uniquely important in U.S. labor markets; mothers in most other developed countries with universal coverage are simply not subject to the same tension between employment and access to insurance coverage.

2. Data and Institutional Background

2.1. Administrative Social Security and Tax Data

We start by identifying the population of mothers giving birth for the first time in either 2013 or 2015 based on Social Security Number (SSN) applications that are filed at birth; these

[5]Our estimate of the employment child penalty is half as large as the only available estimate of the U.S. employment child penalty (Kleven et al., 2019). We attribute the difference in magnitude to differences in data, sample of mothers studied, and time period observed. We discuss these differences in more detail in Section 2.2.

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data identify the child's birth mother and, when available, the child's birth father.[6] Recall that large changes in access to and coverage for maternity services took effect with the 2014 implementation of the ACA. We exclude 2014 births from our analysis because 2014 births are partially treated by the implementation of the ACA—health plans rolled into the requirements of the ACA throughout 2014 based on the start date of the new plan year.

As we will show, parents whose first child was born in 2013 or in 2015 were comparable in both the policy and macroeconomic environments they faced, but the main provisions of the ACA only affected access to health care for maternity services for the 2015 births. We restrict our analysis to mothers aged 26–44 at the time of their first birth to avoid any confounding influence on mothers in our control group for two reasons.[7] First, this age restriction ensures that we can observe sufficient and meaningful pre-birth employment and earnings in the tax data that are not confounded by, for example, post-high school educational programs. Second, this restriction allows us to avoid the confounding influence of a separate provision of the ACA that extended coverage to dependent children under age 26 through their parents' private insurance plan beginning in 2011.

We collect earnings and employment information from 2010–2019 to span the three years before birth through the four years after birth, $t = -3, ..., 4$ for mothers and fathers using administrative tax data. Our measure of employment is derived from the IRS Form W2, which is issued by employers and reports total wages earned by an employee in a given year.[8] We define employment as having received any wages that year.

[6]These data are distinct from the data that underlie the Centers for Disease Control and Prevention Natality database, which collects information for all live births based on birth certificate data.

[7]Mothers' age is determined based on the date of birth reported in the Death Master file.

[8]We use box 3 of the W-2, which reports Medicare wages. Medicare wages include all wages subject to payroll taxes. The most noticeable exclusion to Medicare wages is the cost for the ESI premium (in most cases, these include the employer and the employees shares).

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We supplement these employment data with geographic information and self-employment earnings that are reported by the individual on their annual tax return. State of residence is based on the address field reported on Form 1040, while self-employment income is on Schedule SE of the Form 1040.[9] A practical consequence of these data is that we limit our analysis to mothers who file a tax return two years prior to the birth of their child.[10] The resulting data consists of 1,478,477 mothers and 1,306,021 fathers, who, together, form the primary unit of observation for our empirical analysis.[11]

Insert Table 1 about here

Table 1 presents summary statistics on pre-birth earnings and employment for our sample. Columns (1) and (2) report summary statistics for mothers and fathers, respectively, and column (3) reports the differences in means between mothers and fathers. Columns (4)–(9) report statistics for mothers and fathers separately based on whether they gave birth before (2013) or after (2015) the implementation of the ACA. Mothers in our sample are, on average, slightly younger: 31.3 years old compared to 33.4 years old for fathers. Mothers and fathers share similar labor force participation rates, where 89 percent of mothers and 88 percent of fathers were employed two years before birth. However, fathers are more likely to earn self-employment income as 7.5 percent of mothers compared with 10.2 percent of fathers report self-employment income. Although mothers and fathers are employed at similar rates, there is

[9] Schedule SE is filed by individuals who earned more than $400 in net income from a pass-through business (sole proprietors, partnerships, and S-corporations). Note that this measure will exclude negative business income.

[10] Filing requirements are based on annual income, and very low-income tax payers are generally not required to file Form 1040. The filing threshold in 2020 was $12,400 in gross income for single filers and $24,800 for joint filers. In practice, however, many low-income households still file an annual return as it is a necessary step for receiving most refundable credits for which they may be eligible.

[11] Counts of mothers will differ from birth counts reported by the CDC for several reasons including multiple births, mothers who do not file an application for a social security number, and mothers who do not file taxes two years before birth.

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significant heterogeneity in earnings by gender. For example, mothers earn 25 percent less in wages ($11,749) and 61 percent less in self-employment income ($555) compared to fathers. Importantly, columns (6) and (9) of Table 1 show that these parental differences are similar for pre- and post-ACA births.

2.2. Labor Supply And Childbirth

We begin our study by characterizing the general differences in labor supply outcomes that exist between mothers and fathers in the U.S. in 2013, prior to the ACA. Past work has extensively documented these differences in the European context, but few estimates exist for the U.S. labor market. Following Kleven, Landais and Søgaard (2019), we measure the child penalty based on the following difference-in-differences (DD) specification for 2013 births:

$$y_i = \alpha_0 + \alpha_1 \mathbb{I}[\text{Mom}] + \alpha_2 \mathbb{I}[t >= 0] + \alpha_3 \mathbb{I}[\text{Mom}] \times \mathbb{I}[t >= 0] + \varepsilon_i, \tag{1}$$

where $t = -3, ..., 4$ measures time relative to childbirth.[12] Here, y_i is our outcome of interest, and α_3 captures the relative change for a given outcome (e.g., employment, earnings, etc...) between mothers and fathers after childbirth compared to before childbirth. All changes include age-at-birth–fixed effects to control for differences in lifetime earnings and month-of-birth–fixed effects to control for seasonality in fertility (Buckles and Hungerman, 2013).[13] The

[12] We limit our pre-period analysis to three years prior to birth to ensure that younger parents can be observed with sufficient earnings period in our sample to provide meaningful comparisons.

[13] For this empirical strategy to be valid, the error term should evolve smoothly in the years surrounding childbirth. This form of exogeneity relates to the *timing* of childbirth rather than the childbirth itself. A violation of this assumption could occur if, for example, childbirth is the result of an expected future promotion for fathers, but not for mothers. Importantly, this expectation would need to change sharply and simultaneously with the childbirth itself, as any adjustments to a given outcome in the pre-birth period are observable and controlled for. Finally, identification requires that mothers' and fathers' earnings and employment would have followed a similar trajectory in the absence of the childbirth.

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child-penalty is calculated as α_3, the relative post-birth labor supply gap, scaled by gender-specific predicted labor supply trajectories that exclude the impact of childbirth.

Include Figure 1 about here.

Figure 1 depicts child penalties for employment and wage earnings (panels a and b, respectively) and reports the average penalty calculated over the post-birth years $t+1$ through $t+4$, relative to employment and wage earnings in the year before birth. The figure shows clearly that (1) mothers and fathers are on similar growth trajectories in the years before childbirth, and (2) that a sharp, persistent divergence in outcomes emerges by gender immediately following childbirth. Overall, we estimate that first-time mothers in the U.S. face a 14 percent employment penalty and a 28 percent wage earnings penalty.

Our results are in line with previous estimates of the child penalty, which have focused on the earnings penalty and range from 20 percent in Scandinavian countries to as high as 61 percent in German-speaking countries (Kleven et al., 2019). Two related studies provide estimates of U.S. child penalties. First, Bailey et al. (2019) report a wage earnings child penalty of 30 percent for mothers giving birth between 2004 and 2006 using U.S. administrative tax data. Second, Kleven et al. (2019) estimate a 28 percent gross earnings penalty and a 43 percent employment penalty for mothers giving birth between 1976 and 2006 using first births observed in the Panel Study of Income Dynamics (PSID). Estimates of earnings child penalties are very similar across these studies and our own analysis, lending support to the hypothesis that prior to the ACA the child penalty was relatively stable. At the same time, our estimated employment penalty of 14 percent in the U.S. is smaller than the 43 percent estimated in Kleven et al. (2019) — the only other employment penalty estimate that exists in for the U.S. labor market. This disparity likely owes to the fact that the latter estimate is derived from earlier, small-scale

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survey data on younger mothers and drawn from period when lower overall female labor force participation.[14]

The U.S. stands out as an outlier among the countries comprising these past studies in its failure to provide universal health insurance, leading to the questions of how and to what extent does access to health care influence women's postnatal labor supply and, likewise, the estimated child penalty. We investigate these matters directly by looking at how changes in health care access due to the Affordable Care Act altered labor supply decisions around childbirth.

2.3. Health Insurance and Childbirth

Pre-ACA Health Insurance Landscape Since 1989, Medicaid has guaranteed coverage for maternity services for low-income pregnant women, including prenatal care, delivery, and 60 days of postpartum care (Gifford et al., 2017).[15] Prior to the enactment of the ACA, the extent of maternity benefits for those with private insurance largely depended on whether coverage was purchased through the group market (i.e., employer-sponsored plans) or through the non-group market. States provided oversight and regulation of the non-group market, including whether to permit underwriting. A practical consequence of underwritten health insurance plans was price discrimination based on gender—women could pay as much as 30 percent

[14]Our data differs from Kleven et al. (2019) in several ways. First, Kleven et al. (2019) study first births in the Panel Study of Income Dynamics (PSID) for mothers aged 20–45 who gave birth between 1976 and 2006. This sample of mothers is younger than our population of interest — roughly 30 percent of births in 2013 were to mothers younger than 26. Second, our measure of earnings, wage earnings, is more targeted than gross labor earnings, which includes, for example, self-employment earnings. Third, our analysis is limited to mothers who file taxes two years prior to birth — this restriction removes very-low income mothers from our analysis. Finally, our child earnings penalty specific to 2013 births, whereas Kleven et al. (2019) study all first births observed in the PSID between 1976 and 2006.

[15]The federal law set the lower bound to cover women earning up to 133 percent FPL; however, some states offered expanded Medicaid coverage to higher FPLs. These states were required to maintain the higher eligibility thresholds. As a result, just 7 states set eligibility thresholds at 133 percent FPL in 2013 (AL, ID, NV, ND, SD, UT, and WY).

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more than men for health insurance premiums. In addition, women were commonly denied coverage for maternity-related medical expenses; fewer than 50 percent of non-group enrollees were covered for maternity services (ASPE, 2011). Finally, while maternity services were typically covered by ESI, there was still substantial variation in out-of-pocket costs across plans.

Post-ACA Health Insurance Landscape Following the 2014 implementation of the ACA, access to coverage for pregnancy related care increased. First, mothers living in states that expanded access to Medicaid were more likely to be covered beyond the 60 day postnatal coverage period that had been in place prior to the ACA. Furthermore, eligible mothers in Medicaid expansion states had access to medical services beyond pregnancy related care during the prenatal period. Second, the ACA mandated that all private insurance plans provide coverage for maternity care as part of a set of essential health benefits.[16] Finally, the ACA mandated insurance coverage for contraceptives and reduced its associated out-of-pocket cost to zero.

Together these fertility-related changes suggest the ACA may have been especially impactful for women relative to men in the years surrounding childbirth. One important consequence may be an effect of the ACA on fertility, albeit the sign of this effect is ambiguous. On the one hand, increased access to no-cost family planning and contraceptives, as mandated by the ACA, should reduce unintended pregnancies. On the other hand, reducing the cost of pregnancy might increase the likelihood of riskier pregnancies. Recent empirical work has studied the effect of the young adult provision of the ACA, which expanded health insurance to dependents aged 18–26 to their parents' plan beginning in 2011, and has generally found a reduction in overall childbearing, and an increase in contraceptive use (Abramowitz, 2018;

[16]Plans offered by self-insured employers were not required to provide this set of otherwise essential health benefits.

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Heim, Lurie and Simon, 2018). This is consistent with earlier work studying the effect of the Massachusetts health insurance expansion (Apostolova-Mihaylova and Yelowitz, 2018) and state-based changes to Medicaid (Kearney and Levine, 2009).

3. Empirical Framework and Results

We've shown that 2013 mothers in the U.S. faced a substantial penalty in employment and earnings relative to fathers following childbirth. Although studies have documented this phenomenon, their baseline environment is generally one in which individuals have universal access to health care (that is, they live in Europe). The U.S. setting affords an opportunity to measure the effect of heterogeneous access to health insurance on the gendered differences in post-birth labor supply within a novel setting, and the 2014 implementation of the ACA provides variation in access to insurance. We focus specifically on effect of the ACA on wage employment throughout the remainder of this analysis since it is directly related to the provision of health insurance. Additional estimates of the effect of the ACA on alternative measures of labor supply, such as self-employment and earnings are provided in Appendix Table A.2 and Appendix Section B. We will use the terms wage employment and employment interchangeably throughout the remainder of this analysis.

3.1. Empirical Framework

We estimate the effect of the ACA on relative employment based on the following difference-in-difference-in-differences (DDD) equation:

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$$
\begin{aligned}
y_i \;=\;& \beta_0 + \beta_1 \mathbb{I}[\text{Mom}] + \beta_2 \mathbb{I}[t >= 0] + \beta_3 \mathbb{I}[\text{Post ACA}] + \\
& \beta_4 \mathbb{I}[\text{Mom}] \times \mathbb{I}[t >= 0] + \beta_5 \mathbb{I}[\text{Mom}] \times \mathbb{I}[\text{Post ACA}] + \beta_6 \mathbb{I}[t >= 0] \times \mathbb{I}[\text{Post ACA}] + \\
& \beta_7 \mathbb{I}[\text{Mom}] \times \mathbb{I}[t >= 0] \times \mathbb{I}[\text{Post ACA}] + \varepsilon_i. \qquad (2)
\end{aligned}
$$

Equation 2 compares relative employment for parents having children before and after the ACA. The impact of the ACA on the post-birth employment gap is captured by β_7. Note that β_7 also captures the change in the α_3's derived from estimating Equation 1 separately for 2013 and 2015 births; in other words, the DDD estimate is a DD estimate of the change in post-birth employment gap for pre and post-ACA births. We include individual and year fixed effects in our primary specification to control for macroeconomic shocks and underlying trends in the data.[17]

3.2. Effect of the ACA on Relative Employment Gaps

Baseline Result Our baseline results are reported in column (1) of Table 2. The first two rows report the effects of childbirth on the relative employment differences between mothers and fathers for pre- and post-ACA births based on Equation (1), respectively, while the third row reports the change in these relative employment gaps based on Equation (2). The dynamic trends and the average difference in the post-birth effects between the treatment and control

[17] Recently, several methodological advancements have been made for difference-in-difference models that include individual and year fixed effects, sometimes called "Two Way Fixed Effects" models (Callaway and Sant'Anna, 2021; De Chaisemartin and d'Haultfoeuille, 2020; Goodman-Bacon, 2021; Sun and Abraham, 2021). These strategies are designed to accommodate staggered difference-in-difference experiments where treatment varies over time across different treated groups. In our analysis, we implement a classic "2x2" difference-in-difference model with one treated group (2015 births), one control group (2013 births), and one treatment (the 2014 implementation of the ACA). Because we do not have staggered treatment, our analysis is not subject to the identification concerns and solutions that are raised by these new methods.

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groups are also shown in panel (a) of Figure 2. We find that the relative post-birth employment gap was 11.7 percentage points before the implementation of the ACA. The comparable estimate after the implementation of the ACA is 10.5 percentage points. The effect of the ACA health insurance expansion is then measured as the change in these post-birth relative employment gaps between treated and control parents. Taken together, we find that the provisions of the ACA increased mothers' post-birth employment by 1.2 percentage points compared to fathers—a 10 percent (or 0.0120/-0.117) reduction inthe pre-ACA employment gap.[18]

Include Figure 2 about here.

Identifying Assumptions and Placebo Tests In order to attribute this reduction to the ACA, it must be true that the difference in mothers' and fathers' post-birth employment would have evolved similarly if not for the ACA. Figure 2 provides suggestive evidence that such parallel trends existed in the relative employment gaps.[19] In particular, panel (a) plots the difference in these relative employment gaps for 2013 and 2015 births and shows similar patterns prior to childbirth ($t = 0$) across the two sets of parents.

We provide additional evidence that coincidental secular trends—a common confounding factor for analyses that relies on secular variation—are unlikely to explain our estimate. Panel (b) of Figure 2 plots the effect of childbirth on the relative employment gap for first-births in

[18] Appendix Table A.2 reports the effect of the ACA on alternative employment outcomes, finding an increase in employment and earnings that mirrors this main result. We determine that the 10 percent increase in wage employment was accompanied by an 11 percent increase in wage employment among jobs that confer tax-preferred retirement savings. In other words, wage employment increased among the types of jobs most likely to include access to ESI. In addition, we find a small increase in self-employment, and a 4 percent increase in wage earnings. We discuss the effect of the ACA on wage earnings in Appendix Section B.

[19] Given the annual frequency of our data, the year before birth is partiall treated by pregnancy for mothers who give birth early in the calendar year. For this reason, we measure changes relative to two years prior to birth. This marks a departure from previous literature, including Kleven, Landais and Søgaard (2019), who measure changes one year before birth.

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each year from 2009–2013, when pregnancies for mothers aged 26–44 are untreated by the ACA, in addition to 2015, when pregnancies are fully treated by the ACA.[20] We find that the effect of childbirth on relative employment is stable for the five years preceding the ACA: mothers are 12 percentage points (s.e. 0.000704) less likely to be employed than fathers after childbirth for births in each of the five previous years.[21]

Finally, we formally test whether the impact of childbirth on relative employment is stable during the pre-ACA period through a series of placebo tests based on our main estimation strategy. The first test (Placebo 1) estimates Equations 1 and 2 but uses 2009–2010 births as a control group for 2011–2012 births. The second test (Placebo 2) imposes the two-year difference exploited in our main analysis by comparing relative employment for births that occurred in 2010 to those that occurred in 2012. Columns (2) and (3) of Appendix Table A.1 report estimates from these placebo analyses. In both cases, we fail to reject the null hypothesis that there was no change in relative employment across the placebo treatment and control groups. Moreover, these placebo estimates are economically insignificant: the child penalty grew by just 0.0508–0.129 percentage points from 2009–2012 (cols 2 and 3, panel c).

The stability in relative employment across all placebo years, shown in Figure 2, stands in contrast to the sharp decrease in the relative employment gap due to childbirth after the 2014 implementation of the ACA: mothers were 10.5 percentage points less likely to be working than fathers in 2015, whereas mothers were 12.0 percentage points less likely to be working

[20] For this analysis we rely on a 10 percent sample of first births for mothers aged 26–44 in light of computing constraints. Our primary analysis relies on the full population of 2013 and 2015 mothers, as described in section 2.1

[21] We omit 2014 from panel (b) because 2014 is partially treated by the implementation of the ACA — health plans rolled into the requirements of the ACA throughout 2014 based on the start date of the new plan year. Consistent with this partial treatment, the effect of childbirth on relative employment in 2014 shrinks by roughly 1 pp to 11 pp, and this is both statistically significant at the 1 percent level and statistically different from the same effect in 2013 at the 1 percent level. This estimate is also itself smaller than the effect of childbirth on relative employment in 2015, consistent with the hypothesis that 2014 is a partially treated year and that the 2014 estimate reflects the effect of the partial treatment.

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than fathers for all pre-ACA births analyzed. While this evidence cannot rule out the influence of all other confounding factors, these comparisons mitigate concerns that unrelated secular trends are driving the result.

Decomposition by Pre-Birth Employment We decompose our estimate based on mothers' pre-birth employment to determine whether labor market flows were differentially impacted by the policy. We categorize mothers' pre-birth employment into one of four categories: not employed; self-employed only; wage-employed only; and wage- and self-employed. These results are reported in columns (2) though (5) of Table 2 and panel (a) of Figure 3. We find that mothers in the not-employed group are 13 percent (1.8 pp / 13.6 pp, col. 2) more likely to be working following the ACA's implementation. In addition, the ACA increased employment for mothers who were wage-employed before birth by 9 percent (1.27 pp / 14.5 pp, col. 4). Both of these findings are significant at the 1 percent level. Finally, the relative wage employment of mothers who had income from wage and self-employment two years before birth increased by 4 percent (0.77 pp / 18.7 pp, col. 5). In contrast, we find no meaningful change in the relative post-birth wage employment of mothers who were self-employed before birth.[22] Taken together, this decomposition suggests our estimated increase in mothers' wage employment was driven by both an increase in labor supply among previously non-working mothers and a reduction in exits among previously wage-employed mothers.

Summary Our results thus far have highlighted two important findings. First, there is a sizeable employment penalty in the United States for mothers that coincides with the birth of their first child. We find that mothers are 11–12 percentage points less likely than fathers to work after childbirth despite similar pre-birth employment propensities and trajectories. This

[22] This result is consistent with the small effect effect we find on relative self-employment, shown in Appendix Table A.2.

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estimate translates to a 14 percent child penalty, which persists four years after birth. Second, mothers' employment *increases* by 10 percent compared to fathers for births that occur after the ACA expanded health insurance, implying that the ACA reduced the employment child penalty by 7.8 percent.

3.3. Heterogeneity Based on Variation in the Effect of the ACA

Next, we consider heterogeneity in the effect of the ACA on post-birth relative employment gaps based on expected variation in exposure to the effects of the ACA. We analyze differences based on (1) pre-birth income, which is correlated with an individual's likely source of coverage; (2) marital status (joint and non-joint filers), which is correlated with outside options to access health insurance; (3) mothers' age at birth, which is correlated with labor supply through experience and tenure; and (4) variation driven by the Medicaid expansion, which is correlated with differential access to post-birth insurance. If we find that variation in the effect of the ACA on post-birth employment gaps is consistent with expected variation in effect of the ACA, this provides additional support that the reduction that we estimate in 2015 relative employment gaps is causally related to the ACA.

Insert Table 2 about here.

Income First, we group mothers based on their earnings, as a share of the Federal Poverty Level (FPL), two years before birth.[23] This measure of earnings strongly correlates with one's source of coverage under the ACA: individuals under 138 percent FPL qualify for coverage for

[23] We approximate each parent's income relative to the FPL using their income and number of dependents, as reported on Form 1040 two years prior to birth (adding one additional child to account for the birth).

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maternity services through Medicaid in expansion, individuals with earnings 138–400 percent FPL are eligible for subsidized exchange coverage through Premium Tax Credits (PTC), and individuals with earnings over 400 percent FPL are very likely to be covered by ESI.[24] Individuals with expanded access to Medicaid and PTC-eligible individuals were more strongly affected by the ACA than individuals earning more than 400 percent FPL. As such, we would expect that the effect of the ACA expansion decreases with income based on variation in source of coverage.

Insert Table 3 about here.

Columns (2) through (4) of Table 3 report the impact of the ACA on relative employment for mothers with pre-birth household income below 138 percent of the FPL, between 138–400 percent FPL, and greater than 400 percent FPL, respectively. We find that Medicaid-eligible mothers' employment increased by 11 percent (1.8 pp / 15.8 pp, col. 2) after the implementation of the ACA, by 12 percent (1.75 pp / 12.3 pp, col. 3) for PTC-eligible mothers, and by 10 percent (0.953 pp / 9.63 pp, col. 4) for the ESI-likely population. Each result is statistically significant at the 1 percent level. Moreover, the effect for low-income mothers is statistically different (larger) than high-income mothers at the 1 percent level. This difference is consistent with our hypothesis that the effect of the ACA on relative employment is strongest for low-income mothers, who experienced the largest expansion in access to health insurance due to the ACA.

Marital Status Second, we compare mothers based on whether or not they are joint-filers (that is, married) two years before birth in columns (5) and (6) of Table 3. Joint filers may

[24] By using pre-birth measures, we avoid the mechanical endogeneity between our sample cuts and the outcome measure itself; however, this means that our sample cuts are only proxies for likely Medicaid and subsidy eligibility at birth.

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have access to health insurance through a spouse, reducing the intensity of the ACA treatment compared to non–joint filers. Consistent with this, we find that the impact of the ACA is larger for non–joint filing mothers. In particular, joint–filing mothers are 10 percent (1.12 pp / 11.1 pp, col. 5) more likely to work after the ACA, and non–joint filing mothers are 14 percent (1.39 pp / 9.8 pp, col. 6) more likely to work after the ACA; each effect is precisely estimated at the 1 percent level. These results provide suggestive evidence that the impact of the ACA was larger for non-joint filing mothers, however, we cannot reject a null hypothesis that these estimates are statistically indistinguishable from each other.

Age Third, we split our sample based on median age (31 years at the time of first-birth). Columns (7) and (8) of Table 3 report results for mothers aged 26–31 years and mothers aged 32–44 years, respectively. Older mothers may be more attached to the labor force given their greater experience and tenure. At the same time, older mothers may be in a better financial position to detach from the labor force. For this reason, the expected variation in the impact of the ACA based on age is ambiguous. We do not find meaningful differences in the post-ACA relative labor supply of mothers. Specifically, younger mothers are 10 percent (1.23 pp / 12.8 pp, col. 7) more likely to be working after the ACA, and older mothers are 11 percent (1.08 pp / 10.1 pp. col. 8) more likely to be working after childbirth. Each of these effects is statistically significant at the 1 percent level, but these results are not statistically distinguishable from each other.

Medicaid Expansion Finally, we exploit variation directly generated by the ACA by separately estimating, in Table 3, results for mothers who lived in states that did and did not expand Medicaid coverage by 2015 (cols. 9 and 10, respectively). This distinction is important as low-income mothers in expansion states have both expanded access to prenatal coverage for all health services and expanded post-natal coverage beyond 60 days. We find that the impact

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of the ACA is 7 percent (1.38 pp / 1.29 pp) larger for mothers in expansion states compared to non-expansion states. At the same time, we are unable to reject the null hypothesis that the change in relative employment is statistically different across these two groups.

Insert Figure 3 about here.

Summary Panel (b) of Figure 3 summarizes our sub-sample analysis based mothers' pre-birth characteristics. These results suggest that the effect of the ACA on relative post-birth employment was not driven by one particular group of mothers; instead, we find the result is pervasive for a broad set of mothers. Moreover, we find suggestive variation in mothers' post-ACA responses that aligns with ex-ante expectations for how the ACA might impact different subgroups of mothers. The comprehensive response that we find is consistent with the sweeping nature of the ACA, which reduced the uninsurance rate in the U.S. by 5.9 percentage points, or 35 percent, between 2013 and 2015.[25]

4. Implications for the Child Penalty

Our results show that mothers who give birth with better access to health insurance are relatively *more likely* to work after childbirth, compared to fathers. If better access increases health care utilization in a way that improves mother's productivity, this would explain our finding. However, this hypothesis is difficult to test using the tax data, which does not capture

[25] Statistics taken from the 2013 and 2015 American Community Survey for non-elderly individuals aged 0 to 64.

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changes in health care utilization or health outcomes.[26] Instead, we use the Medical Expenditures Panel Survey (MEPS), to provide indirect, descriptive evidence that health insurance coverage increased in the months immediately surrounding birth. In addition, using monthly birth characteristics reported in public-use natality data from the Center for Disease Control (CDC), we find direct evidence that prenatal health care utilization increased sharply with the 2014 implementation of the ACA. Figure 4 graphs prenatal coverage and utilization around the timing of the ACA health insurance expansion.

Include Figure 4 about here.

In panel (a), we show prenatal insurance coverage for up to 12 months before birth through six months after birth for mothers aged 26–44 years and giving birth between 2009–2017. Solid lines report pre-ACA monthly mean coverage for mothers covered by private (triangle symbol) and public insurance (square symbol), and dashed lines report post-ACA monthly mean coverage. These data reveal that the ACA health insurance expansion increased Medicaid coverage primarily during the post-natal months, as is expected given the expanded Medicaid thresholds available to low-income expecting mothers that predate the ACA. Additionally, the post-ACA private insurance rate increased by 2–3 percentage points throughout the prenatal and postnatal period. In other words, the implementation of the ACA coincides with an increase in mothers' pre and coverage surrounding birth, both for public and private plans.

Next, we plot the monthly share of births surrounding which mothers had a prenatal doctor visit within the first trimester of pregnancy — often described as "timely and adequate"

[26] One exception to this is the allowable deduction for certain medical and dental expenses that exceed 7.5% of a taxpayer's Adjusted Gross Income (AGI). However, just 30% of taxpayers are estimated to forego the standard deduction in favor of itemized deductions (Lu, 2017), and fewer than 20% of itemizers claim a deduction for medical and dental expenses. Moreover, itemizers are disproportionately high-income individuals: more than 90% of households with an AGI of more than $500,000 itemize compared to just 7% of households with less than $30,000.

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prenatal care — for first-birth mothers aged 26–44 years who gave birth between 2009 and 2017 in panel (b).[27] The grey dots reflect monthly births that occurred during 2014, a period of gradual transition as plans rolled into the ACA requirements. These data reveal a sharp 1.5 percentage point increase in the share of births for which a mother received early-pregnancy health care monitoring that coincided with the implementation of the ACA. Timely and adequate prenatal health care has been shown to improve maternal and infant health outcomes (Evans and Lien, 2005; Corman, Dave and Reichman, 2018; Yan, 2017). Moreover, the 2012 Oregon Medicaid expansion and the ACA Medicaid expansion have been shown to increase first-trimester prenatal visits for eligible mothers (Palmer, 2020; Harvey et al., 2021).

Although this evidence is descriptive, it illustrates a connection between prenatal health care utilization and the ACA. Combined with the broad labor supply response we find for mothers who give birth after the ACA, it is plausible that better health outcomes from the observed increase in health care utilization led to improved productivity. Moreover, recent work by Kleven, Landais and Søgaard (2021) considers the influence of health in driving the child penalty by comparing biological and adoptive parents in Denmark. If negative health effects were the primary mechanism underlying the post-birth child penalty, then biological mothers should experience a larger child penalty than adoptive mothers experience. Though the authors find similar long-run child-penalties among these two groups of parents, their analysis also finds that the child penalty is 20 percent larger for biological parents than for adoptive parents for up-to five years after birth. This short-run difference in the child penalty is consistent in both sign and magnitude with our estimated effect of the ACA on the US child penalty. Although differences exist between the U.S. and Danish healthcare and safety net

[27]These data are drawn from 2003 U.S. Standard Certificate of Live Birth. Between 2009 and 2016, some states continued to use the 1989 U.S. Standard Certificate of Live Birth, which did not capture this information. We report share of births with a first-trimester visit only for states that use the 2003 U.S. Standard Certificate of Live Birth. Those births reported on the 1989 U.S. Standard Certificate of Live Birth are excluded from this analysis. Reassuringly, our estimates are not sensitive to including births from states in the total number of births.

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systems, the similarity of these results provides external support for the role that health plays in contributing to the U.S. child penalty.[28]

5. Conclusion

Following childbirth, mothers, on average, experience a substantial and persistent decline in labor supply relative to fathers. We estimate the effect of access to health insurance on this gap by exploiting the expansion in health insurance access driven by the 2014 implementation of the Affordable Care Act. Overall, we find that mothers' post-childbirth labor force participation relative to fathers increased by 10% due to the ACA. This increase in employment is pervasive, existing even when mothers are grouped by pre-birth employment, income, marital status, age at birth, and geography. This result reflects both an increase in previously non-working mothers workers and a reduction in the number of mothers exiting employment after childbirth. We find that the 10% increase in relative employment for mothers is most consistent with a reduction in the physical toll of childbirth, which has the potential to reduce post-birth productivity. Overall, our results suggest that further expanding access to health insurance in the U.S. may provide a pathway for reducing disparities in labor supply between mothers in fathers arising from childbirth.

[28] Kleven, Landais and Søgaard (2021) are able to follow parents for up to ten years after birth. They find that the long-run child penalty for biological and adoptive parents converge 6–10 years after birth, and they conclude that the convergence of the child penalties is evidence of a limited role for biology in driving the child penalty. Given the recent implementation of the ACA, our study cannot speak to whether the child penalties for pre- and post-ACA parents converge in the long run.

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Table 1
Summary Statistics: Pre and Post ACA Births

Notes: This table summarizes earnings and employment two years before birth for parents with a first birth in 2013 and 2015. Earnings are deflated based on the CPI-U. Parents that are not working are included in employment statistics. Columns 1–3 report statistics for all mothers and fathers and differences in means, respectively. Columns 4–9 report statistics for mothers and fathers separately based on those that gave birth before the ACA enacted (2013 births) and those that gave birth after the ACA was enacted (2015 births). Differences in means account for month-of-birth fixed effects.

	All Births			Pre-ACA Births			Post-ACA Births		
Pre-Birth Outcome	Moms (1)	Dads (2)	Diff (3)	Moms (4)	Dads (5)	Diff (6)	Moms (7)	Dads (8)	Diff (9)
Wages	35,885	47,635	-11,749 (44.06)	35,972	47,325	-11,352 (62.87)	35,802	47,931	-12,128 (61.76)
Self-Employment Income	312	866	-554.9 (3.734)	304	857	-553.2 (5.296)	319	875	-556.5 (5.263)
Employment	0.892	0.881	0.0113 (0.000381)	0.891	0.879	0.0124 (0.000549)	0.893	0.883	0.0102 (0.000530)
Self-Employment	0.074	0.103	-0.0286 (0.000342)	0.073	0.102	-0.0297 (0.000487)	0.076	0.103	-0.0276 (0.000481)
Age At Birth	31.3	33.4	-2.103 (0.00580)	31.3	33.3	-2.100 (0.00829)	31.3	33.4	-2.107 (0.00810)
Births	1,478,477	1,306,021		722,898	638,701		755,579	667,320	

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Table 2
Mother's Wage Employment: Mother's Pre-Birth Employment

Notes: This table reports variation in the impact of the ACA on post-birth employment based on variation in mother's employment two years before birth. Panel A is restricted to 2013 (pre-ACA) births and Panel B is restricted to 2015 (post-ACA) births. Panel C reports the Differences-in-Differences-in-Differences estimate comparing pre- and post-ACA births. Column 1 reports the baseline estimate for the full sample of mothers. Cols. 2–5 split the sample of mothers into four groups based on their employment two years before birth. All specifications include individual and calendar-year fixed effects. Standard errors, reported in parentheses, are clustered at the parent level.

		Pre-Birth Employment			
	Baseline	No Wage or Self Employment	Self Employment	Wage Employment	Wage and Self Employment
	(1)	(2)	(3)	(4)	(5)
Panel A: Control Births					
Mom × *Post Birth*	-0.117	0.136	0.119	-0.145	-0.187
	(0.000478)	(0.00188)	(0.00347)	(0.000491)	(0.00237)
Panel B: Treated Births					
Mom × *Post Birth*	-0.105	0.154	0.125	-0.132	-0.179
	(0.000462)	(0.00191)	(0.00331)	(0.000472)	(0.00226)
Panel C: Δ *Relative Outcomes*					
Mom × *Post Birth* × *Post ACA*	0.0120	0.0181	0.00651	0.0127	0.00772
	(0.000665)	(0.00268)	(0.00480)	(0.000681)	(0.00327)
Individual FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
Person-Year Observations	22,275,984	1,788,712	576,528	18,851,600	1,059,144

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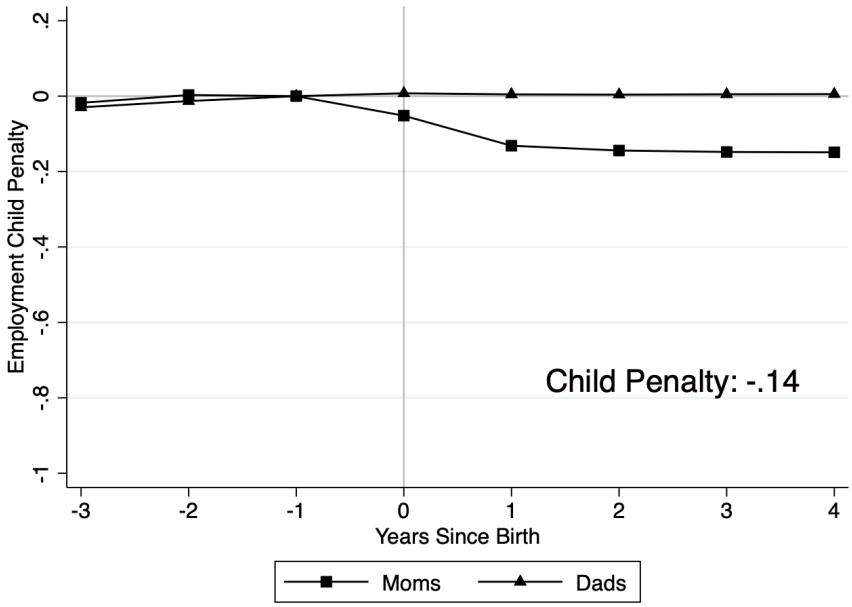
Table 3
Mother's Wage Employment: Mother's Pre-Birth Characteristics

Notes: This table reports variation in the impact of the ACA on post-birth wage employment based on variation in characteristics of mothers two years before birth. Panel A is restricted to 2013 (pre-ACA) births and Panel B is restricted to 2015 (post-ACA) births. Panel C reports the Differences-in-Differences-in-Differences estimate comparing pre- and post-ACA births. Column 1 repeats the baseline estimate from Col. 1 of Table A.1 for comparison purposes. Cols. 2–4 split the sample of mothers who file a 1040 based on their income as a share of the Federal Poverty Level (FPL). . All specifications include individual and calendar-year fixed effects. Standard errors, reported in parentheses, are clustered at the parent level.

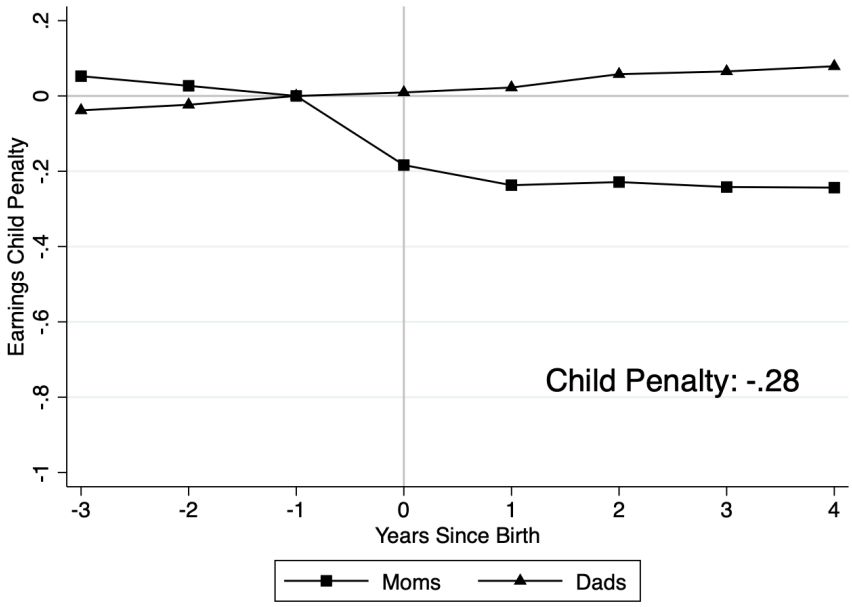
		Pre-Birth Income (% FPL)			Filing Structure		Age		2015 Medicaid Expansion	
	Baseline	<138	138 – 400	>400	Non-Joint	Joint	26 - 31	32 - 44	No	Yes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Control Births										
Mom × *Post Birth*	-0.117	-0.158	-0.123	-0.0963	-0.0980	-0.111	-0.128	-0.101	-0.123	-0.113
	(0.000478)	(0.00148)	(0.000756)	(0.000646)	(0.000762)	(0.000609)	(0.000629)	(0.000734)	(0.000785)	(0.000605)
Panel B: Treated Births										
Mom × *Post Birth*	-0.105	-0.139	-0.108	-0.0868	-0.0841	-0.0995	-0.115	-0.0903	-0.110	-0.0987
	(0.000462)	(0.00136)	(0.000710)	(0.000645)	(0.000720)	(0.000601)	(0.000614)	(0.000701)	(0.000765)	(0.000596)
Panel C: Δ *Relative Outcomes*										
Mom × *Post Birth* × *Post ACA*	0.0120	0.0181	0.0145	0.00953	0.0139	0.0112	0.0123	0.0108	0.0129	0.0138
	(0.000665)	(0.00202)	(0.00104)	(0.000913)	(0.00105)	(0.000856)	(0.000879)	(0.00101)	(0.00110)	(0.000850)
Individual FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Person-Year Observations	22,275,984	3,723,296	9,305,848	8,909,000	11,226,272	13,320,608	12,894,864	9,381,120	9,623,421	15,437,061

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Figure 1. Child Penalty: 2013 Births



(a) Employment Child Penalty

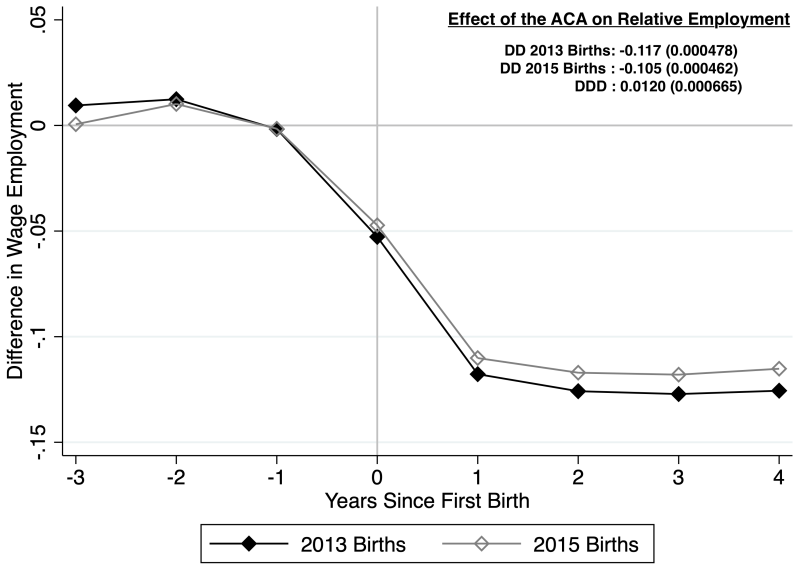


(b) Wage Child Penalty

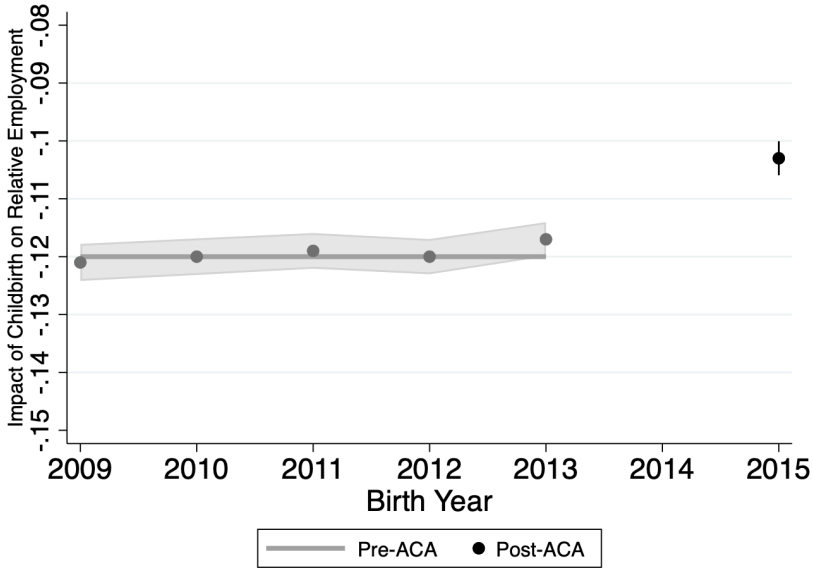
Notes: These figures plot mean earnings and employment gaps for 2013 Births. Employment is measured by an indicator function for any wages earned. Earnings are measured by wage earnings and include 0s. The child penalty is measured as in Kleven, Landais and Søgaard (2019).

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Figure 2. Mean Relative Employment: Pre- and Post-ACA



(a) Mean Relative Employment, 2013 vs 2015

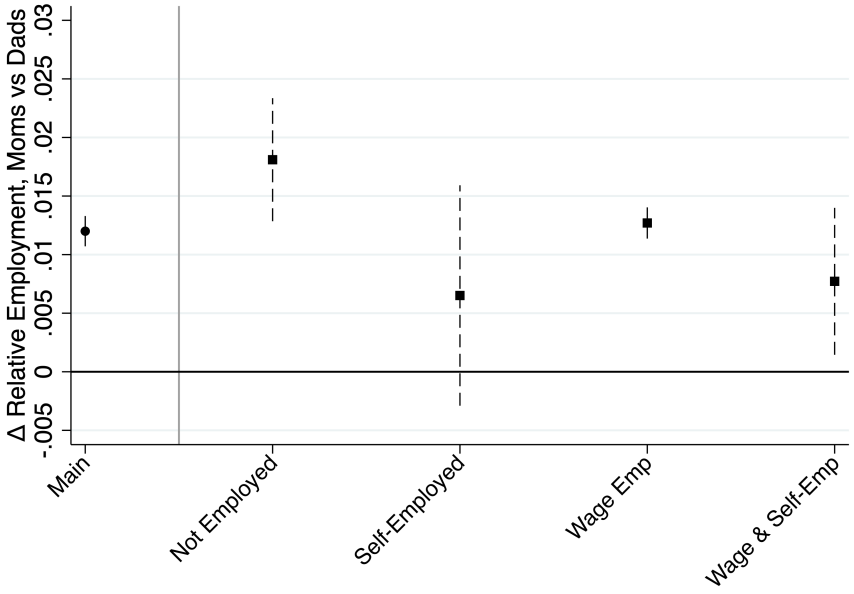


(b) Pre-ACA Trends in Relative Employment: Identifying Assumption

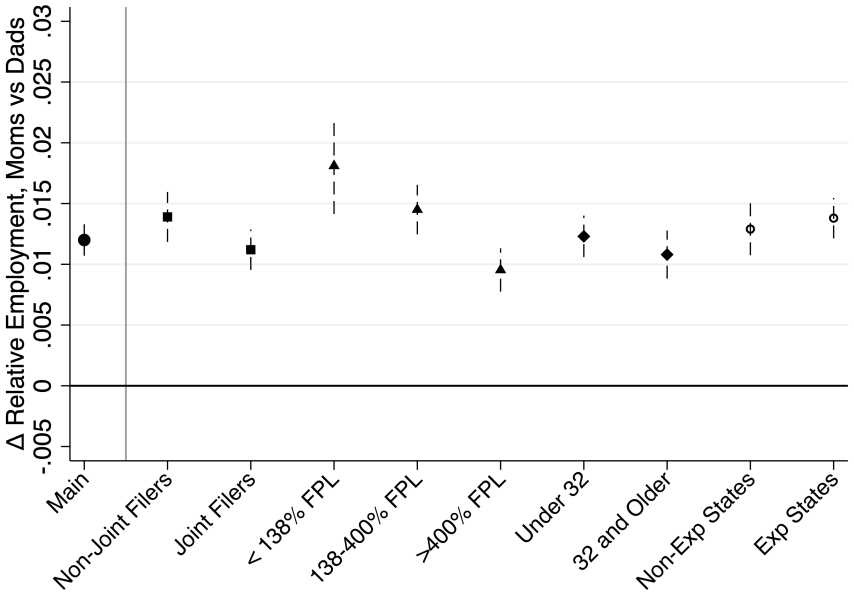
Notes: Panel (a) plots mean differences in employment for mothers and fathers giving birth in 2013 and 2015 based on the population administrative data. Estimates in the upper right corner are based on Eqns 1 and 2, and are reported in Appendix Table A.1. Panel (b) plots the estimated effect of childbirth on the mean difference in employment between mothers and fathers based on Equation 1 for 10% random sample of first-births occurring in 2009 – 2015. The solid grey line depicts the average impact of childbirth on relative employment for the years 2009–2013.

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Figure 3. The Impact of the ACA on Relative Employment



(a) Mother's Pre-Birth Employment

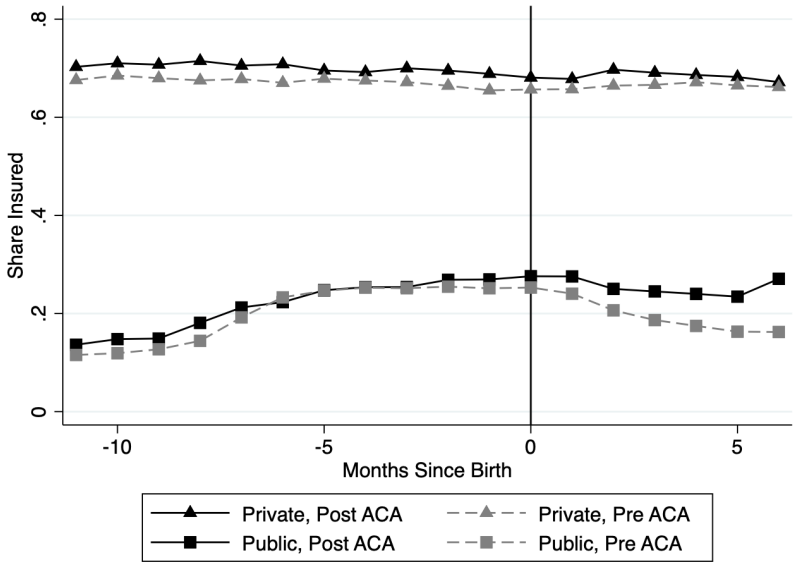


(b) Mother's Pre-Birth Characteristics

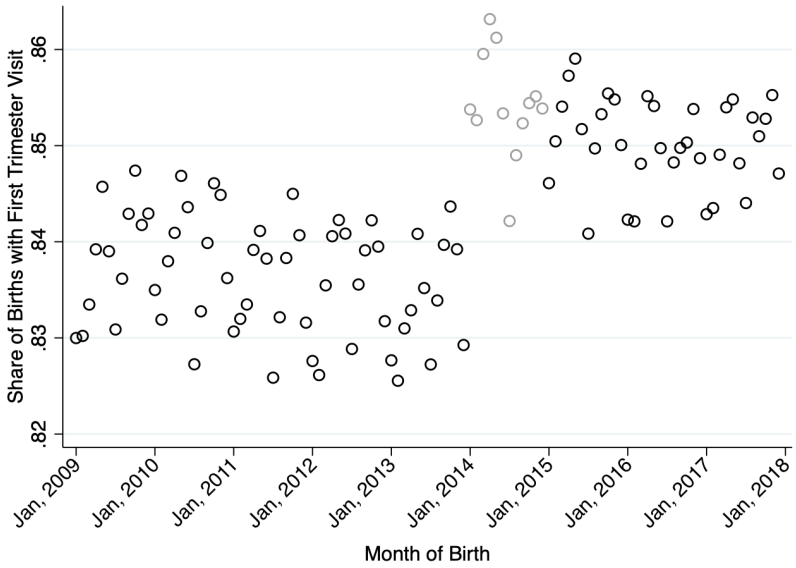
Notes: Panels (a) and (b) depict DDD estimates of the impact of the ACA on mothers' relative employment among various subsamples. Specifically, panel (a) corresponds to Table 2, and panel (b) corresponds to Table 3.

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Figure 4. Descriptive Evidence: ACA Implementation: Coverage and Utilization



(a) Insurance Rates, by Source of Coverage



(b) Share of Births with 1st Trimester Prenatal Visit

Notes: Panel A plots the share of the 26-44 population that are insured in the 18 months surrounding pregnancy by public and private insurance as identified in the 2009–2017 Medical Expenditure Panel Survey (MEPS). Panel B plots the monthly share of births for whom the mother had a first-trimester prenatal care visit based from monthly natality statistics reported by the the Centers for Disease Control.

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A. Appendix

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Table A.1
The Effect of the ACA on Relative Employment: Baseline and Placebo estimates

Notes: This table reports the results of a Differences-in-Differences estimate comparing mothers and father employment before and after birth. Column 1 reports our baseline estimate comparing relative employment for 2013 (control) moms to 2015 (treated) moms. Columns 2 and 3 report placebo estimates for a 10% sample of births that are untreated by the 2014 implementation of the ACA. Panel A reports the difference-in-difference estimate of mothers' post-birth employment compared to fathers for control births. Panel B reports the difference-in-difference estimate of mothers' post-birth employment compared to fathers for treated births. Panel C reports the Differences-in-Differences-in-Differences estimate comparing control and treated births. Standard errors, reported in parentheses, are clustered at the parent level.

Employment	Main (1)	Placebo 1 (2)	Placebo 2 (3)
Panel A: Control Births			
Mom × *Post Birth*	-0.117	-0.121	-0.120
	(0.000478)	(0.00115)	(0.00161)
Panel B: Treated Births			
Mom × *Post Birth*	-0.105	-0.120	-0.120
	(0.000462)	(0.00110)	(0.00155)
Panel C: Δ *Relative Outcomes*			
Mom × *Post Birth* × *Treated*	0.0120	0.00129	0.000508
	(0.000665)	(0.00159)	(0.00223)
Individual FE	✓	✓	✓
Year FE	✓	✓	✓
Control Births	2013	2009–2010	2010
Treated Births	2015	2011–2012	2012
Person-Year Observations	22,275,984	4,493,196	2,269,944

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Table A.2
The Effect of the ACA on Relative Self-Employment and Earnings

Notes: This table reports the results of a Differences-in-Differences estimate comparing mothers' and fathers' relative employment with benefits, sefl-employment, and wage earnings before and after birth (columns 1, 2, and 3, respectively). Panel A reports the difference-in-difference estimate of mothers' post-birth employment compared to fathers for 2013 births. Panel B reports the difference-in-difference estimate of mothers' post-birth employment compared to fathers for 2015 births. Panel C reports the Differences-in-Differences-in-Differences estimate comparing 2013 and 2015 births. Standard errors, reported in parentheses, are clustered at the parent level.

	Employment w/ Benefits (1)	Self-Employment (2)	Wage Earnings (3)
Panel A: Control Births (2013)			
Mom × *Post Birth*	-0.0823	-0.00141	-14,526
	(0.000609)	(0.000386)	(40.82)
Panel B: Treated Births (2015)			
Mom × *Post Birth*	-0.0731	0.000257	-13,970
	(0.000600)	(0.000384)	(40.61)
Panel C: Δ *Relative Outcomes*			
Mom × *Post Birth* × *Treated*	0.00923	0.00167	556.5
	(0.000855)	(0.000545)	(57.58)
Individual FE	✓	✓	✓
Year FE	✓	✓	✓
Person-Year Observations	22,275,984	22,275,984	21,796,328

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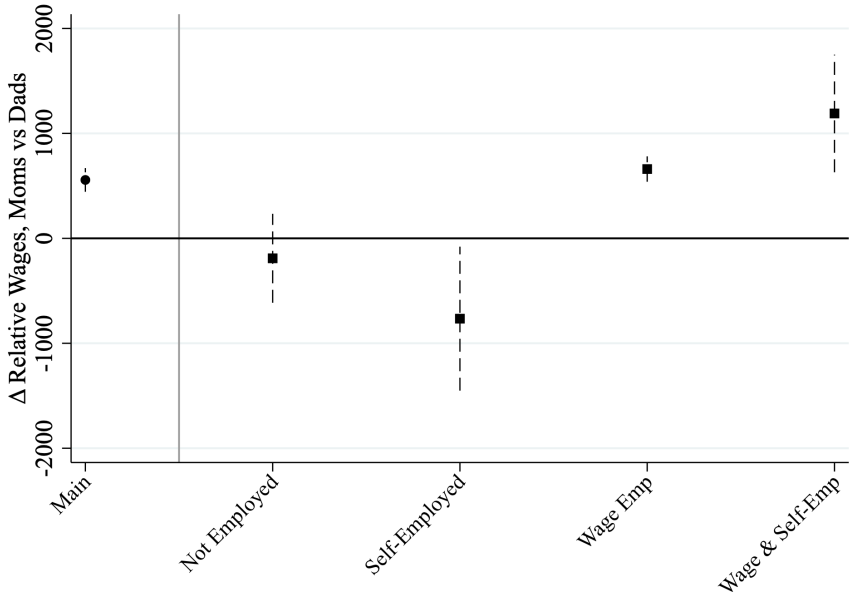
B. Effect of the ACA on Post-Birth Differences in Wage Earnings

We estimate the impact of the ACA on post-birth differences in wage earnings based on Equation 2. Our baseline estimate of this effect is reported in column (3) in Table A.2. We find that mothers who give birth before the ACA earn $14,526 less than fathers, while mothers who give birth after the ACA earn $13,907 less than fathers. The DDD estimate implies that the ACA reduced post-birth gendered differences in wage earnings by 3.8 percent ($556/$14,526).

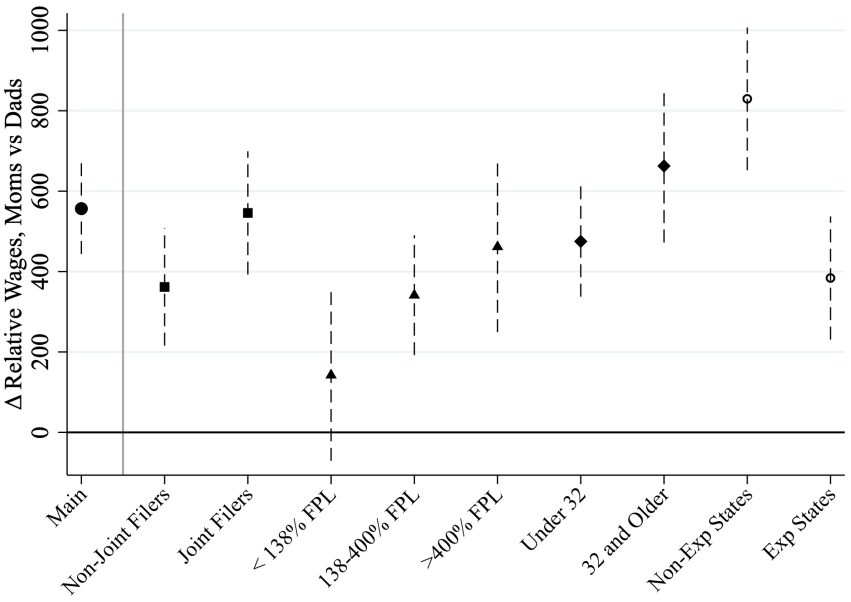
Panel (a) of Figure A.1 decomposes this result according to mothers' pre-birth employment. This analysis suggests that the positive effect on mothers' relative wage earnings is largely driven by mothers who were also wage-employed before birth. Panel (b) of Figure A.1 provides results when grouping mothers with different exposures to the ACA including by pre-birth income, marital status, age, and geography. Unlike with employment, expectations on the relative magnitude of these coefficients on wage earnings are less obvious as they contain both extensive margin and intensive margin responses, the latter of which is driven by both changes to wage rate and hours. Overall, we find that the effect of the ACA differs in magnitude across the different subgroups, though the estimated coefficients are always positive.

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Figure A.1. The Impact of the ACA on Relative Wages



(a) Mother's Pre-Birth Employment



(b) Mother's Pre-Birth Characteristics

Notes: Panels (a) and (b) depict DDD estimates of the impact of the ACA on mothers' relative earnings among various subsamples.

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