EARLY OR LATE? THE TIMING OF UNIVERSAL CASH TRANSFERS AND INFANT HEALTH*

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Abstract

We examine how the timing of universal and permanent cash transfers during pregnancy affects birth outcomes. Using US Vital Statistics natality data and exploiting variation in the payment schedule of the Alaska Permanent Fund Dividend, we find that earlier payment—during the quarter pre-conception and during the first trimester—leads to considerably greater health benefits compared to later payment. While the effects on birth weight decrease monotonically with payment timing, pre-conception transfers generate nearly twice as much reduction in low birth weight incidence compared to later trimesters. Earlier transfers also translate into faster fetal growth and higher prenatal care utilization compared to later payment. Our results suggest that the returns to cash transfer programs may be substantively underestimated when timing is ignored.

JEL Codes: H24, H51, I10, I12, J13, J18

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1 Introduction

When are cash transfers most effective during pregnancy? Poor birth outcomes have lifelong consequences, weakening health, reducing educational attainment and lowering earnings (Alloush and Riaz, 2025; Almond and Currie, 2011; Almond et al., 2010; Bharadwaj et al., 2018; Behrman and Rosenzweig, 2004; Black et al., 2007; East et al., 2023; Figlio et al., 2014; Persson and Rossin-Slater, 2018; Royer, 2009). A growing body of research shows that targeted financial assistance improves birth outcomes, particularly for low-income families. Food stamps and supplemental nutrition programs (Almond et al., 2011; Bitler et al., 2025; Hoynes et al., 2011), tax credits (Hoynes et al., 2015), cash transfers (Amarante et al., 2016), pandemic stimulus checks (Ruffini, 2024), and child benefits (González and Trommlerová, 2022; Reader, 2023) have been linked to better infant health. However, while some studies examine specific pregnancy periods—often the third trimester—systematic comparison of timing effects across pregnancy stages remains largely unexplored. Whereas the permanent income hypothesis predicts that the timing of anticipated cash transfers should not matter, timing may still be crucial as individuals do not necessarily smooth consumption (Kueng, 2018). Binding liquidity constraints and behavioral biases can prevent optimal maternal investment in prenatal health. Income received earlier in pregnancy—or even before conception—could thus enable mothers to adopt healthier behaviors, thereby improving fetal development and birth outcomes.

This paper examines how the timing of universal and permanent cash transfers affects birth outcomes, leveraging the Alaska Permanent Fund Dividend (APFD) as a natural experiment. The APFD is a large, universal, and unconditional annual cash transfer paid to all Alaskan residents since 1982 (Figure A1). Unlike targeted financial assistance programs, which face administrative barriers and incomplete take-up (Aizer et al., 2024), the APFD's universal structure mitigates selection concerns related to program participation. Moreover, the dividend is paid on a predetermined schedule each year and all Alaskans receive the same amount in a given year. Therefore, differences in birth outcomes across pregnancy stages within a year primarily reflect timing effects. However, since the dividend applies universally within Alaska, there is no natural control group

within the state, requiring us to construct one from other US states.

Our main specification employs a synthetic difference-in-differences (SDID) approach to construct a suitable control group. SDID combines elements of the synthetic control method (SCM) and difference-in-differences (DID) by assigning unit weights to control states that closely replicate Alaska's pre-reform trends and time weights that emphasize pre-treatment periods most similar to post-treatment periods for the control states (Arkhangelsky et al., 2021). This dual weighting approach ensures that the synthetic Alaska reflects pre-reform dynamics, providing a credible counterfactual for analyzing post-reform outcomes. Our findings remain robust to alternative identification strategies and extensive sensitivity analyses, including standard SCM and standard DID using Washington state as a natural control state (Kueng, 2018).

Our main analyses are based on the US Vital Statistics Natality Data. We focus on 1976-1988, six years before and six years after the introduction of the dividend in 1982, but note that our findings and inferences are robust to longer panels. With these data, we analyze birth weight in grams and low birth weight ($< 2,500$ grams), standard measures of infant health that are highly predictive of longer term health and economic outcomes (Almond and Currie, 2011; East et al., 2023). We also examine complementary gestational outcomes including prematurity (born before 37 weeks) and fetal growth in grams per week, as well as pregnancy behaviors such as the month prenatal care started and the number of prenatal care visits. To shed further light on potential mechanisms, we use Consumer Expenditure Survey (CEX) data to examine food and health care spending. Finally, we make use of the Integrated Public Use Microdata Series (IPUMS) Current Population Survey (CPS) data to examine potential stressors such as employment.

We find that the APFD increased birth weight by 27g and reduced low birth weight incidence by 11%. These effects are economically significant and among the largest documented for transfer programs in the US, including targeted programs such as food stamps and supplemental nutrition programs, tax credits, and pandemic stimulus checks (Almond et al., 2011; Hoynes et al., 2011, 2015; Ruffini, 2024). We further find evidence of improvements in gestational outcomes. In particular, the APFD reduced premature births by 11%, accelerated intrauterine growth by 0.7 grams

per week, and increased prenatal care visits by 0.6 on average. The effects are most pronounced among mothers with less than a high school education, suggesting that the cash transfer alleviates financial constraints and enables greater prenatal investments, thus improving fetal health.

Importantly, we find that earlier payments generate larger improvements in birth outcomes. Transfers received before conception and in the first trimester lead to the largest gains in birth weight (50g and 42g, respectively), while later payments have smaller effects (24g in the second trimester and 22g in the third). Earlier payments also lead to greater reductions in low birth weight incidence: transfers received before conception reduce low birth weight incidence by 20%, compared to 13% for first trimester exposure, and 11% for second and third trimester exposure. These early transfers also lead to stronger fetal growth and increased prenatal care utilization, suggesting that better nutrition and healthier pregnancy behaviors may be key mechanisms at play.

To explore these potential mechanisms, we examine consumption and employment patterns. We find suggestive evidence that households spend more on food away from home, personal care, and healthcare in the quarter in which the dividend is received. We also find that APFD receipt is associated with lower incidence of pregnancy-related complications such as hypertension and anemia, consistent with improved maternal health during pregnancy. Moreover, we find evidence of labor supply adjustments: mothers increase part-time work but not full-time work, suggesting that they may use the dividend to shift toward more flexible work arrangements that may better accommodate prenatal care and healthier pregnancy behaviors. These findings highlight timing as a critical policy lever: receiving income before or early in pregnancy enables mothers to adopt healthier behaviors when they matter most for fetal development.

The remainder of the paper proceeds as follows. Section 2 provides institutional background and discusses why payment timing may matter. Section 3 describes the data and empirical strategy. Section 4 presents the findings while Section 5 explores potential mechanisms. Section 6 concludes.

2 Background

2.1 The Alaska Permanent Fund Dividend

In 1969, a major oil discovery was made on Alaska's North Slope region. The subsequent Prudhoe Bay lease sale generated $900 million for the state, but residents feared the windfall would be quickly squandered. In response, the populace supported the establishment of the Alaska Permanent Fund (APF) to diversify investments from part of the oil royalties across a range of public and private assets, generate long-term risk-adjusted returns, and limit state legislators' ability to spend the revenue (O'Brien and Olson, 1990). The APF is managed by the Alaska Permanent Fund Corporation (APFC) and since 1982, a portion of the returns from the fund's investment has been disbursed to Alaskan residents in the form of a dividend as the APFD.

All Alaskan residents are eligible to receive the dividend except for those who are incarcerated or convicted of a misdemeanor or felony. The amount of the dividend in any given year is independent of individual Alaskans' behavior and the local economy (Jones and Marinescu, 2022). Instead, it is determined using a public formula based on the statutory net income generated by the fund over the previous four years (Kueng, 2018). Figure 1, plots the nominal and real (in 2024 USD) dividend amounts from 1982 to 1988. The dividend ranged from $331 to $1,000 ($1000 to $3,250) in nominal (real) terms. According to a representative survey conducted in March-April 2017 by Harstad (2017), the dividends are considered a significant financial help to Alaskans. Approximately 63% of respondents stated that the dividend made "quite a bit" or "a fair amount" of difference in their lives, and 85% agreed that many people use a large part of their APFD on basic needs. Additionally, 30% of respondents reported using the funds to pay off credit card debt or other obligations.

2.2 Investment at Different Pregnancy Stages and Birth Outcomes

The introduction of the APFD provides an exogenous and substantial increase in income for Alaskans (Harstad, 2017; Kueng, 2018). As infant health is considered a normal good, this in-

come boost is expected to enhance health inputs and improve birth outcomes (Currie, 2009). A growing literature has established that financial transfers improve infant health outcomes. However, most studies do not systematically examine how the timing of these transfers across different pregnancy stages affects birth outcomes. If transfers in a particular trimester generate larger improvements, policymakers could achieve greater impact at no additional cost simply by adjusting the schedule of payments. The extent to which timing matters may depend on the mechanisms through which financial transfers affect infant health.

First, an extensive body of work emphasizes the importance of nutrition programs during the third trimester of pregnancy. Almond et al. (2011) show that Food Stamp Program (FSP) exposure in the third trimester increased birth weight, particularly among Black mothers and those in high-poverty areas, while earlier exposure had weaker or insignificant effects. They argue this reflects medical and epidemiological evidence that nutrition is most critical for fetal growth in late pregnancy, citing both prior findings that the FSP increased overall food expenditures (Hoynes and Schanzenbach, 2009) and their own finding that earlier exposure adds little once third-trimester exposure is accounted for. Parallel evidence comes from Hoynes et al. (2011), who find that the supplemental nutrition program for Women, Infants, and Children (WIC) made available by the third trimester, improved birth outcomes.

Second, evidence on the importance of timing from targeted (means-tested and/or for families with children) transfer programs is mixed. Ruffini (2024) finds that pandemic stimulus checks improved infant health outcomes—with larger effects for payments received in the third trimester—while increasing the number of prenatal care visits and decreasing smoking. Conversely, Lyu et al. (2025) find that pandemic cash payments had virtually no significant effects on infant health outcomes. Reader (2023) finds that the Health in Pregnancy Grant—paid in the third trimester in England and Wales—increased birth weight and reduced prematurity, with effects most pronounced among younger mothers in deprived areas despite no changes in prenatal care use. The author concluded these effects operated through stress reduction rather than the third-trimester timing itself, noting that critics argued the third trimester was "too late in pregnancy for significant

nutritional gains." Examining Earned Income Tax Credit (EITC) expansions, Hoynes et al. (2015) find reduced low birth weight, increased prenatal care, and decreased smoking among single low-educated mothers, but could not statistically distinguish which trimester was most important in a horse race specification.

Third, prenatal stress at any pregnancy stage can harm birth outcomes (Bozzoli and Quintana-Domeque, 2014; De Cao et al., 2022; De Oliveira et al., 2023; Persson and Rossin-Slater, 2018). Several studies exploiting the exogenous timing of acute stressors find that first-trimester exposure has particularly pronounced effects. Torche (2011) found that first-trimester exposure to earthquake-induced maternal stress significantly reduced birth weight primarily by shortening gestation, while Brown (2020) showed that first-trimester exposure to the 9/11 attacks restricted fetal growth while mid-pregnancy exposure lowered birth weight. However, other studies find stronger effects for later-pregnancy stress exposure. Carlson (2015) show that distressing economic news during pregnancy—especially late in gestation—reduced birth weight, while Rosales-Rueda (2018) find that flood exposure—particularly in the third trimester—reduced birth weight. Importantly, Evans and Garthwaite (2014) provide evidence that income can buffer maternal stress, finding that EITC expansions improved maternal health with gains in self-reported health and reductions in risky biomarkers, suggesting stress reduction as an important mechanism through which cash transfers may improve birth outcomes.

Fourth, early prenatal care initiation enables more frequent care utilization and earlier detection of maternal health complications, which in turn can improve infant health. Inadequate prenatal care is defined as initiating care after the fourth month of pregnancy or receiving fewer than half of the recommended number of visits (Osterman, 2018). Although the existing literature offers mixed evidence regarding the impact of prenatal care on birth outcomes, several studies have provided support for its role in enhancing infant health (Conway and Deb, 2005; Corman et al., 2019; Currie and Gruber, 1996). Cygan-Rehm and Karbownik (2022) find that initiating prenatal care by the 10^{th} week of gestation led to a 9+ gram increase in birth weight and a 6.8% reduction in low birth weight, with suggestive survey evidence indicating these gains operated primarily through

reduced maternal smoking and drinking. Sonchak (2015) shows that an additional prenatal visit increases birth weight by about 20–24 grams for white disadvantaged mothers. Yan (2016) further shows that inadequate prenatal care—whether due to delayed initiation, too few visits, or both—significantly raises the likelihood of insufficient maternal weight gain during pregnancy, prenatal smoking, premature membrane rupture, and rapid (precipitous) labor. Thus, cash transfers received early in pregnancy could enable mothers to access prenatal care when it matters most for establishing healthy behaviors and detecting complications.

Fifth, the most striking evidence for pre-conception effects comes from González and Trommlerová (2022), who found that a cash transfer to new mothers in Spain reduced the likelihood of very low birth weight in their subsequent children through faster intrauterine growth. The effects were largest among poor, unmarried, and low-educated women. The mechanism appears to operate through improved maternal health, nutrition, and behaviors.

In sum, existing evidence suggests that different mechanisms may operate at different pregnancy stages: nutrition interventions appear most effective in the third trimester, while prenatal care benefits from both early initiation and frequent visits. Evidence on stress timing is mixed, suggesting that acute stress at any point during pregnancy may be detrimental to infant health. Existing studies examine means-tested programs or transfers targeted at families with children, leaving open how the timing of universal cash transfers affects birth outcomes across pregnancy stages.

2.3 The Saliency of APFD Payment Timing and Infant Health

To the best of our knowledge, this is the first study to systematically examine how the timing of a universal cash transfer across the pre-conception period and all three trimesters affects birth and related pregnancy outcomes. We contribute to the literature by examining a universal, permanent cash transfer independent of household income and family structure, allowing us to estimate pure income effects without concerns about endogenous program take-up or targeting.

Two prior studies examine the APFD's effects on infant health. Chung et al. (2016) find positive

effects using state-level variation, while Wyndham-Douds and Cowan (2024) find null effects using within-Alaska household-level amount variation.[1] Crucially, neither examines timing of receipt across pregnancy stages. We contribute by systematically comparing effects of pre-conception versus first, second, and third-trimester transfers, demonstrating that earlier payments generate substantially larger returns.

Despite the APFD being predictable, behavioral and financial frictions suggest that when during pregnancy families receive cash may substantially affect birth outcomes. Kueng (2018) documents a striking absence of anticipation effects for the APFD: household non-durable consumption responds disproportionately in the quarter of receipt despite the dividend's predictability. This excess sensitivity reflects behavioral biases such as mental accounting (Boehm et al., 2025; Thaler, 1999) combined with binding liquidity constraints that prevent consumption smoothing. When households cannot easily borrow against future income or maintain savings buffers, the specific timing of receipt becomes crucial for pregnancy-related investments.

As a result, receiving the dividend during earlier stages of pregnancy may promote healthier gestation and yield enhanced birth outcomes. First, receiving income earlier provides relief from financial constraints, relieving financial stress and allowing mothers to improve their nutrition and health investments. These in turn benefit the baby, leading to improved gestation outcomes. Second, earlier receipt enables better planning of prenatal care visits: mothers can access guidance on healthy pregnancy behaviors sooner, which reduces risky behaviors such as drinking and smoking, and allows for earlier and more frequent monitoring, lowering the risk of pregnancy complications. We investigate these mechanisms directly using natality microdata—fetal growth, prenatal care utilization, maternal drinking and smoking, and pregnancy complications. We also examine household spending on food and healthcare using CEX data, and employment patterns using CPS data, to assess whether cash receipt enables immediate investments in nutrition, health, and personal care while potentially reducing maternal work-related stress. While cash transfers may

[1]The divergent findings likely reflect different identification strategies. Wyndham-Douds and Cowan (2024) exploit within-Alaska variation in household dividend amounts, which is endogenous to household size. Chung et al. (2016) estimate overall average effects but do not study timing variation. Our approach exploits within-year variation across different pregnancy stages, enabling us to isolate timing effects.

improve birth outcomes at all stages of pregnancy, we conjecture that earlier payments generate the largest effects by providing timely financial relief and facilitating better planning, particularly for mothers with lower levels of education.

3 Empirical Strategy

3.1 Data and Measures

We use data from the US Vital Statistics Natality Data, which consists of micro data extracted from birth certificates covering the entire universe of births from the National Center for Health Statistics since 1985, and 50-100% of births prior to 1985.[2] We focus our main analyses on singleton births from 1976 to 1988 to mothers aged 15-44, which provides a balanced six-year window before and after the 1982 reform.[3] Figure A2 illustrates the main sample construction.

Our main outcomes of interest are birth weight in grams and an indicator for low birth weight ($< 2,500$ g). To explore key mechanisms underlying infant health outcomes, we analyze the impact of the APFD on premature births (< 37 weeks), and fetal growth, measured as the ratio of birth weight to gestation weeks. Additionally, we also examine the gestation month prenatal care began and the number of prenatal care visits.

Our main predictor takes two forms, each analyzed separately: (i) an interaction term between a unit indicator (1 for Alaska and 0 otherwise) and a time indicator (1 for periods after the 1982-policy implementation and 0 otherwise) and (ii) the real dividend amount (per thousand) adjusted to 2024 dollars (see Figure 1). The main covariates include infant demographics, namely, sex (male =

[2]Prior to 1985, the National Center for Health Statistics (NCHS) processed samples of birth certificates rather than the full universe: 50% of certificates until 1972, with states gradually transitioning to sending 100% of records through the Cooperative Health Statistics System from 1972-1984. All states sent 100% of records starting in 1985. This sampling does not affect our main estimates, which collapse births to state-year-month cells.

[3]The first dividend payments were distributed in June 1982. Our main sample uses births from 1976-1981 and 1983-1988 to maintain a symmetric six-year window around the reform, although results are robust to including 1982. We exclude six states—Arkansas, California, Idaho, New Mexico, Texas, and Washington—that did not report maternal education, a key determinant of infant health (Currie and Moretti, 2003). In sensitivity analyses, we extend our sample to 1968-2004, the full period for which state identifiers are publicly available, and estimate specifications without maternal education controls to include all states.

1, female = 0) and parity indicators (first, second, third, or greater (reference category)). Maternal demographics include ethnicity indicators (white, black, other (reference category)), educational attainment categorized as less than high school ($<$ Grade 12), high school graduate (Grade 12), some college (Grade 13-15), and four-year college or above ($\geq$ Grade 16 (reference category)), as well as age groups (15–19, 20–24, 25–34, and 35–44 (reference category)). State characteristics include lagged unemployment rates obtained from the Bureau of Labor Statistics.[4]

To conduct the main analyses, we collapse the data at different levels. First, by state-year to estimate the average effects of the APFD on key outcomes. Second, by state-year-month to evaluate the impact of payment timing across the pre-conception quarter, first, second, and third trimesters. Third, by state-year-education or state-year-month-education to examine heterogeneity by maternal education. For additional heterogeneity and sensitivity analyses, we collapse by other relevant dimensions. While the main analyses use collapsed data, individual-level analyses confirm the robustness of our results.

3.2 The SDID Method

To evaluate the impact of the APFD on birth outcomes and pregnancy behaviors, we compare trends in Alaska to those in a set of control states, which approximate the counterfactual outcomes that would have occurred in the absence of the dividend payments. To achieve this, we adopt the SDID method of Arkhangelsky et al. (2021), which provides a data-driven approach for constructing a weighted average of potential control states (synthetic Alaska) to serve as a comparison for the treated unit (Alaska). The SDID method combines the strengths of DID and SCM. Like DID, it allows for differences in outcome levels between treated and synthetic control units prior to the reform of interest. Like SCM, it relaxes the parallel trends assumption by generating a synthetic control with optimally chosen weights. SDID further extends SCM by incorporating time-specific weights, emphasizing pre-treatment periods that closely resemble post-treatment periods for con-

[4]In sensitivity analyses, we include additional controls such as maternal marital status and paternal education (including missing indicators), state population and temperature. Results remain robust to the inclusion of these variables (available upon request).

trol states.

For the first set of analyses on the impact of the APFD on birth outcomes, we collapse the data to the state-year level to obtain a balanced panel with $N = 45$ states and $T = 12$ periods. Since Alaska began disbursing the dividend in 1982, the panel has six pre-intervention periods, 1976-1981 ($T_{\text{pre}} = 6$), and six post-intervention periods, 1983-1988 ($T_{\text{post}} = 6$). Alaska is designated as the first unit, $i = 1$, while the remaining $N_{\text{co}} = 44$ control states are indexed by $i = 2, \ldots, N$.

Pre-processing. Let the outcome for state i in period t be denoted by Y_{it}, and the time-varying exogenous covariates by X_{it}. To account for the influence of these covariates, we follow Clarke et al. (2024) and first regress Y_{it} on X_{it} on the control states using ordinary least squares (OLS). Using the estimated coefficients $\widehat{\theta}$, we then obtain the residuals for the full sample:

$$Y_{it}^{res} = Y_{it} - X_{it}\widehat{\theta}. \tag{1}$$

This step is viewed as a pre-processing step that removes the impact of changes in the covariates on the outcomes. We next apply the SDID method to these residuals to get the unit weights ω^{sdid} and time weights λ^{sdid}.

Unit and time weights. Following Arkhangelsky et al. (2021), the optimal unit weights ω^{sdid} are found by minimizing the difference in trends between the treated unit and control units during the pre-treatment period:

$$(\hat{\omega}_0, \hat{\omega}^{\text{sdid}}) = \underset{\omega_0 \in \mathbb{R}, \omega \in \Omega}{\arg\min} \sum_{t=1}^{T_{\text{pre}}} \left(\omega_0 + \sum_{i=2}^{N} \omega_i Y_{it}^{\text{res}} - Y_{1t}^{\text{res}} \right)^2 + \zeta_\omega^2 T_{\text{pre}} ||\omega||_2^2, \tag{2}$$

where Ω defines a set of constraints ensuring that the $N_{\text{co}} = 44$ weights are non-negative and sum to 1. The term ω_0 allows for a constant difference between treated and control units, capturing baseline differences in outcome levels (analogous to a group indicator in standard DID) while focusing on matching pre-trends. $||\omega||_2^2 = \sum_{i=2}^{N} \omega_i^2$ represents the Euclidean norm, and ζ_ω is a

regularization parameter as described in Arkhangelsky et al. (2021), which ensures that the weights are well-distributed across the control states and facilitates a unique solution.

The time weights λ^{sdid} are computed minimizing the difference in trends between the pre- and post-treatment periods for control states:

$$(\hat{\lambda_0}, \hat{\lambda}^{\text{sdid}}) = \underset{\lambda_0 \in \mathbb{R}, \lambda \in \Lambda}{\arg\min} \sum_{i=2}^{N} \left(\lambda_0 + \sum_{t=1}^{T_{\text{pre}}} \lambda_t Y_{it}^{\text{res}} - \frac{1}{T_{\text{post}}} \sum_{t=T_{\text{pre}}+1}^{T} Y_{it}^{\text{res}} \right)^2 + \zeta_\lambda^2 N_{\text{co}} ||\lambda||_2^2, \tag{3}$$

where the set Λ ensures that the $T_{\text{pre}} = 6$ time weights are non-negative and sum to 1. λ_0 captures a constant difference between pre- and post-treatment periods for the unexposed units, analogous to a post-reform indicator in standard DID. The main objective is to assign greater weight to pre-treatment periods that exhibit trends most similar to the post-treatment period outcomes for the unexposed units. $||\lambda||_2^2 = \sum_{t=1}^{T_{\text{pre}}} \lambda_t^2$ represents the Euclidean norm, and ζ_λ is a regularization parameter as described in Clarke et al. (2024), which ensures that the weights are well-distributed across the pre-treatment periods and that there is a unique solution. Post-reform years are equally weighted so that the time weight for each post-reform year is $\frac{1}{T_{post}}$.

The optimal unit $\hat{\omega}^{\text{sdid}}$ and time weights $\hat{\lambda}^{\text{sdid}}$ are presented in Tables A1 and A2. These weights determine the relative importance of each control state and pre-treatment period in constructing our synthetic counterfactual for Alaska.

Weighted DID. Following Arkhangelsky et al. (2021) and Clarke et al. (2024), we estimate a DID model weighted by the product of the unit weights and time weights, $\hat{\omega}^{\text{sdid}} \cdot \hat{\lambda}^{\text{sdid}}$:

$$Y_{it}^{\text{res}} = \alpha_0 + \alpha_1 \text{APFD}_{it} + \gamma_i + \phi_t + \varepsilon_{it}. \tag{4}$$

Here, Y_{it}^{res} denotes the residuals from a regression of the relevant outcome variable (birth weight, low birth weight, premature birth, fetal growth, month prenatal care began, number of prenatal care visits) on the covariates X_{it}, as described in Eq. (1).

The predictor $\text{APFD}_{it} = \{\text{Treated}_{it}, \text{Amount}_{it}\}$ represents the main variable of interest. Specif-

ically, Treated_{it} is a binary indicator equal to 1 for post-reform periods in Alaska and 0 otherwise, and Amount_{it} denotes the real dividend amount (per thousand) adjusted to 2024 dollars. The SDID model identifies the average treatment effect on the treated (ATT) when Treated_{it} is the main predictor, or the average causal response (ACR) when Amount_{it} is the predictor. γ_i and ϕ_t denote state and year fixed effects, respectively.[5]

Inference. To assess the significance of our treatment effects, we adopt the permutation-based procedure of Arkhangelsky et al. (2021) and Clarke et al. (2024). This non-parametric approach generates a distribution of placebo treatment effects under the null hypothesis that the APFD had no impact on the outcomes. For each iteration, an ATT or ACR is estimated by assigning the treatment to a randomly selected control state, assuming treatment begins in 1982. For the binary treatment indicator Treated_{it}, the placebo treatment takes a value of 1 for the post-treatment period and 0 otherwise. For the continuous treatment Amount_{it}, the dividend amounts are assigned to the placebo-treated state, preserving the temporal structure of the payments.

This process is repeated $B = 1,000$ times to generate a distribution of placebo treatment effects, ensuring a robust null distribution. The p-value is calculated as the proportion of placebo effects that are as extreme or more extreme than the observed treatment effect:

$$p = \frac{\sum_{b=1}^{B} \mathbb{I}\left(|\hat{\alpha}_1^{\text{placebo},b}| \geq |\hat{\alpha}_1|\right)}{B}. \tag{5}$$

$\hat{\alpha}_1^{\text{placebo},b}$ denotes the placebo treatment effect in iteration b, $\hat{\alpha}_1$ is the observed treatment effect from eq. (4), and $\mathbb{I}(\cdot)$ is an indicator taking unity when the condition is satisfied and 0 otherwise.

To construct a 95% confidence interval, we use the empirical distribution of the placebo treatment effects. Let $\{\hat{\alpha}_1^{\text{placebo},b}\}_{b=1}^{B}$ denote the placebo effects from B permutations, and let $\bar{\alpha}_1^{\text{placebo}}$ represent their mean. To ensure the interval reflects the observed treatment effect $\hat{\alpha}_1$, we recenter

[5]Note that we do not control for X_{it} in the regression model specified in Eq. (4) because the pre-processing step removes the impact of changes in the covariates on the outcomes. Our findings are robust to computing unit and time weights directly on the raw outcome, Y_{it}, and to controlling for X_{it} in Eq. (4).

the placebo distribution as:

$$\hat{\alpha}_1^{r,b} = \hat{\alpha}_1 + \left(\hat{\alpha}_1^{\text{placebo},b} - \bar{\alpha}_1^{\text{placebo}} \right). \tag{6}$$

The 95% confidence interval is given by the 2.5^{th} and 97.5^{th} percentiles of the recentered distribution:

$$\text{CI}_{95\%} = \left[\hat{\alpha}_1^{r,0.025}, \hat{\alpha}_1^{r,0.975} \right], \tag{7}$$

where $\hat{\alpha}_1^{r,0.025}$ and $\hat{\alpha}_1^{r,0.975}$ are the corresponding percentiles.

3.3 Timing Effects Across Pregnancy Stages

To examine how the timing of dividend receipt during pregnancy affects birth outcomes and prenatal behaviors, we estimate models at the state-year-month level. The timing of dividend disbursement varied across years: in 1982, payments were distributed over ten months from June 1982 through April 1983, while from 1983 onward, payments were concentrated in the latter months of each calendar year (Table A3). The variation in aggregate payment timing across years, combined with variation in conception dates, generates differential exposure to the dividend across pregnancy stages, which we exploit to identify trimester-specific effects.

Let i denote state and t denote year-month. We define four pregnancy stages during which payment could have occurred: the pre-conception quarter, first trimester, second trimester, and third trimester (reference category). We assume a nine-month pregnancy duration such that these four stages span one full year.[6]

The weighted DID model is given by:

$$\begin{aligned} Y_{it}^{\text{res}} = \beta_0 &+ \beta_{\text{pre}} \text{APFD}_{it} \times \text{Pre-conception}_t + \beta_{1\text{st}} \text{APFD}_{it} \times \text{First-trimester}_t \\ &+ \beta_{2\text{nd}} \text{APFD}_{it} \times \text{Second-trimester}_t + \beta_{3\text{rd}} \text{APFD}_{it} + \gamma_i + \phi_t + \varepsilon_{it}, \end{aligned} \tag{8}$$

[6] We assume 9-month pregnancies for full-term births. For premature births, we calculate conception dates using actual gestation weeks from birth certificates, meaning their final pregnancy stage is appropriately shorter. Our findings remain robust when we restrict the sample to full-term births.

where as before, Y_{it}^{res} is the residualized outcome of interest, $\text{APFD}_{it} = \{\text{Treated}_{it}, \text{Amount}_{it}\}$ represents the treatment variable, and the model is weighted using unit and time weights. We also control for state fixed effects, γ_i, and year-month fixed effects, ϕ_t.

The pregnancy stage variables—Pre-conception$_t$, First-trimester$_t$, and Second-trimester$_t$—capture the likelihood of receiving the dividend during each stage based on the observed monthly distribution of payments. Specifically, we observe the proportion of dividends distributed each month (Table A3). For instance, 41.3%, 51.2%, and 7.5% of the 1984 dividend was distributed in October, November, and December, respectively. We assign trimester exposure based on these probabilities: mothers of babies born in February, March, and April 1985 have 41.3%, 92.5%, and 100% likelihood of second-trimester exposure, respectively (see Table A4 for details). This probabilistic assignment identifies the ATT or ACR for each pregnancy stage.[7]

We normalize the pro-rated pregnancy stage indicators to sum to one across all trimesters to facilitate interpretation of relative effects. The marginal effects for the pre-conception period, and first, second, and third trimesters are, respectively, given by $\left(\beta_{\text{pre}} + \beta_{\text{3rd}}\right)$, $(\beta_{\text{1st}} + \beta_{\text{3rd}})$, $(\beta_{\text{2nd}} + \beta_{\text{3rd}})$, and β_{3rd}. Inference on these marginal effects follows the permutation-based procedure described earlier.

3.4 Heterogeneity by Maternal Education

As discussed in Section 2, the effects of cash transfers should be larger among households facing tighter liquidity constraints, as these families are less able to smooth consumption across periods. We use maternal education as a proxy for socioeconomic status, examining whether less-educated mothers—who typically face greater financial constraints—show stronger responses to the APFD.

The empirical specification follows the structure of Eq. (8), except that analyses are conducted at the state-year-education level. We interact APFD_{it} with education categories: less than high school ($<$ Grade 12), high school graduate ($=$ Grade 12), and some college (Grade 13–15), using

[7] This approach appropriately captures the expected exposure to dividend receipt during each pregnancy stage. Any within-month variation in individual receipt dates would attenuate our estimates, making our findings conservative.

four-year college or above ($\geq$ Grade 16) as the reference category. We also examine how timing effects vary by education by collapsing data at the state-year-month-education level and estimating Eq. (8) for each education group.

4 Results

We find that the APFD significantly improves birth outcomes, increasing birth weight by 27 grams and reducing low birth weight incidence by 11%. Crucially, we document substantial heterogeneity in these effects based on payment timing: transfers received before conception or during the first trimester generate effects nearly twice as large as those received later in pregnancy. These timing effects are most pronounced among less-educated mothers, suggesting that financial constraints amplify the importance of early income support. We investigate several mechanisms—including prematurity, fetal growth, and prenatal care utilization as well as maternal health and consumption expenditures—that explain when transfers are particularly effective.

4.1 Parallel Pre-Reform Trends

Figure 2 demonstrates that Alaska and synthetic Alaska exhibit close parallel trends during the pre-reform period (1976–1981) across all main outcomes: birth outcomes (Panel A), gestation measures (Panel B), and prenatal care utilization (Panel C). Following the 1982 implementation of the APFD, Alaska's outcomes diverge from this counterfactual. Birth weight increase, low birth weight incidence declines, premature births fall, fetal growth increases, and prenatal care utilization improves.[8]

[8]We present raw outcome trends for ease of interpretation. Parallel pre-trends also hold for the residualized outcomes, Y^{res}, that SDID uses to construct synthetic Alaska.

4.2 Overall Effects of the APFD

We begin by establishing average treatment effects across all pregnancy stages. Table 1 Panel A presents these baseline results. The APFD increased birth weight by 27 grams ($p < .05$) and reduced low birth weight incidence by 0.5 percentage points (11%, $p < .05$). These effects are economically significant and comparable to or at the upper range of effects documented for targeted US transfer programs (Almond et al., 2011; Hoynes et al., 2011, 2015; Ruffini, 2024).

The APFD also improved gestational outcomes. The program reduced premature births by 0.8 percentage points (11%, $p < .05$) and increased fetal growth by 0.72 grams per week ($p < .01$). We find no significant effects on average gestational length, suggesting the program improves birth outcomes primarily by preventing very early deliveries and accelerating fetal growth rather than extending pregnancy duration.[9] Prenatal care utilization increased substantially: the APFD raised the number of prenatal visits by 0.6 (6%, $p < .01$) but had no significant effect on the timing of care initiation.

Panel B examines how outcomes respond to variation in the dividend amounts. An additional $1,000 (in 2024 USD) increases birth weight by 13 grams ($p < .05$) and reduces low birth weight incidence by 0.3 percentage points (7%, $p < .05$). The program also reduces premature births by 0.4 percentage points (6%, $p < .05$), increases fetal growth by 0.37 grams per week ($p < .05$), and raises prenatal care visits by 0.3 (3%, $p < .01$).

4.3 Effects of Payment Timing Across Pregnancy Stages

We now examine how the timing of cash receipt during pregnancy affects the main outcomes of interest: birth outcomes, gestation outcomes, and prenatal care utilization.

Birth weight. Figure 3 Panel A (left graph) reveals substantial heterogeneity in effects across pregnancy stages. Transfers received before conception increase birth weight by 50 grams ($p <$

[9] We also find no significant effects on very low birth weight ($< 1,500$ grams), APGAR scores, or low APGAR. Results available upon request.

.01), while first-trimester transfers increase it by 42 grams ($p < .05$). In contrast, second- and third-trimester transfers generate smaller effects (24 grams and 22 grams, respectively). Table A5 Panel A Column (1) formalizes these differences: relative to third-trimester receipt, pre-conception transfers generate an additional 28-gram gain ($p < .01$), while first-trimester transfers yield an additional 19 grams ($p < .05$). Second-trimester effects are economically and statistically indistinguishable from third-trimester effects.

Low birth weight. A similar pattern emerges for low birth weight in Figure 3 Panel A (right graph). Pre-conception transfers reduce low birth weight by 0.9 percentage points (20%, $p < .05$), compared to 0.5 percentage points (11%, $p < .05$) for first-trimester, 0.6 percentage points (13%, $p < .10$) for second-trimester, and 0.5 percentage points (11%, $p < .05$) for third-trimester transfers (Table A5 Column 2). Relative to third-trimester receipt, pre-conception transfers generate an additional 0.4 percentage point reduction (9%, $p < .10$), while first- and second-trimester effects are statistically indistinguishable from third-trimester effects (Table A5 Column 2).

Gestational outcomes. Early transfers also improve fetal growth in Figure 3 Panel B (right graph). Pre-conception and first-trimester transfers increase fetal growth by 0.9 and 0.7 grams per week, respectively ($p < .05$), compared to statistically insignificant effects for second- and third-trimester transfers (Table A5 Column 4). This pattern closely tracks birth weight improvements, with both outcomes showing strongest effects when cash transfers occur before conception or during early pregnancy. For premature births, we find a similar but statistically weaker pattern, with earlier transfers associated with lower likelihoods of premature delivery (Figure 3 Panel B, left graph).

Prenatal care. Prenatal care utilization responds more strongly to early transfers in Figure 3 Panel C (right graph). Pre-pregnancy transfers increase prenatal visits by 0.6 visits ($p < .01$), compared to 0.4 visits ($p < .10$) for first- and second-trimester transfers, and 0.55 visits ($p < .05$) for third-trimester transfers (Table A5 Column 6). When mothers receive cash transfers earlier,

they have more opportunities to accumulate visits throughout pregnancy, leading to more comprehensive care utilization. The third-trimester effect likely reflects catch-up behavior as mothers approach delivery. The timing of care initiation shows no significant effects across pregnancy stages (Figure 3 Panel C, left graph).

These patterns are robust to alternative specifications. Defining pregnancy stages as discrete quarters yields consistent results (Appendix Figure A3), as does examining dose-response relationships using dividend amounts (Appendix Figure A4).

These findings demonstrate that the timing of income support matters substantially for infant health. Earlier transfers generate consistently larger effects across outcomes, with pre-conception and first-trimester transfers producing birth weight gains nearly twice as large as third-trimester transfers. These effects are accompanied by corresponding improvements in fetal growth and prenatal care utilization, both of which show the strongest responses to early transfers.

4.4 Heterogeneity by Maternal Education

The benefits of unconditional and permanent cash transfers concentrate sharply among disadvantaged families. Figure 4 Panel A (left graph) reveals a clear educational gradient: infants born to less-educated mothers experience substantially larger health improvements, with effects declining monotonically as maternal education increases. Transfers increase birth weight by 34 grams ($p < .05$) among mothers with less than Grade 12 education, 21 grams ($p < .05$) for Grade 12, and 18 grams ($p < .10$) for Grade 13-15, with negligible effects for college-educated mothers (Table A6 Panel B Column 1). Relative to college-educated mothers, those without high school degrees see 35-gram larger gains ($p < .05$), while high school graduates see 22-gram larger gains and those with some college see 20-gram larger gains ($p < .10$) (Table A6 Column 1).

The educational gradient is similarly pronounced for low birth weight in Figure 4 Panel A (right graph). Transfers reduce low birth weight incidence by 0.8 percentage points (18%, $p < .10$) among mothers with less than Grade 12 education and 0.7 percentage points (16%, $p < .05$) among Grade 12 mothers, with economically and statistically insignificant effects for more ed-

ucated groups (Table A6 Column 2). Relative to college-educated mothers, those with high school or less experience 0.7 percentage point (16%, $p < .05$) larger reductions (Table A6 Column 2).

The concentration of birth outcome effects among less-educated mothers reflects corresponding heterogeneity in gestational outcomes. Figure 4 Panel B (left graph) shows that cash transfers reduce premature births by 1.2 percentage points (17%, $p < .10$) among mothers with less than high school education, while effects for other education groups are not statistically significant (Table A6 Column 3). For fetal growth in 4 Panel B (right graph), transfers increase growth rates by 0.8 grams per week ($p < .05$) among mothers with less than high school education and approximately 0.6 grams per week for both high school and some college educated mothers, with insignificant effects among college graduates (Table A6 Column 4). This educational gradient in fetal growth suggests that cash transfers are most effective for mothers facing the greatest financial constraints, likely reflecting differential access to adequate nutrition across education levels.

Prenatal care utilization shows parallel heterogeneity (Figure 4 Panel C, right graph). Less-educated mothers increase prenatal visits by 0.9 visits ($p < .001$), while the effect diminishes to 0.6 visits ($p < .05$) for high school graduates and becomes small and statistically insignificant for those with higher education (Table A6 Column 6). The timing of care initiation shows a similar educational gradient, though with less precision (Table A6 Column 5). These consistent patterns across education groups in both birth outcomes and prenatal care utilization (Figure 4 Panels A–C) provide additional evidence that prenatal care serves as a key mechanism through which cash transfers improve infant health.[10]

Importantly, the timing effects documented above vary systematically by maternal education, suggesting that financial constraints play a key role in determining when cash transfers matter most. Figure 5 reveals striking patterns: mothers with high school or less show substantially larger birth weight gains from pre-conception and first-trimester transfers compared to later-pregnancy transfers, while college-educated mothers show minimal responses regardless of timing. This dif-

[10]These patterns are robust to using continuous dividend amounts (Figure A5). Further heterogeneity analyses also reveal that the effects are also concentrated among younger mothers, particularly those aged aged 15-19 and 20-24 (Appendix Figure A6), consistent with greater financial constraints among this demographic.

ferential sensitivity extends across outcomes: less-educated mothers exhibit greater reductions in low birth weight, larger decreases in premature births, larger increases in fetal growth, and stronger prenatal care responses when transfers arrive early in pregnancy, while more-educated mothers show stable, modest effects across all pregnancy stages.

These findings demonstrate that APFD benefits are concentrated among disadvantaged families. The gradient suggests that families facing tighter financial constraints respond more strongly to unconditional and permanent cash transfers. Less-educated mothers likely have greater difficulty smoothing consumption across pregnancy stages and fewer savings buffers, making both the availability and timing of cash particularly consequential for prenatal investments.[11]

4.5 Sensitivity and Additional Results

We assess the robustness of our main findings—particularly those concerning payment timing—to alternative specifications, sample definitions, and potential policy confounds. Across all exercises, we find consistent evidence that the APFD improves infant health, with early-pregnancy transfers remaining the most effective. We also examine additional outcomes including fertility, neonatal mortality, and maternal composition.

Alternative Specifications and Samples. Our findings are robust to alternative empirical specifications and sample definitions. Results using standard DID with Washington State as control and standard SCM yield similar or larger estimates than our main SDID approach (Figures A7 and A8). Effects persist when extending the sample to 1968–2004 with all states or analyzing individual-level data (Figures A9–A10).

Concurrent Policy Changes. Alaska repealed its state income tax in 1980, which could have induced additional income effects. We address this in two ways. First, the SDID estimator assigns unit weights to control states with similar pre-treatment trajectories in birth outcomes, accounting

[11] Heterogeneity analyses by paternal education confirm larger effects for fathers with lower socioeconomic status, including those with missing paternal education who are more likely to be single mothers.

for any infant health trends in Alaska that may have arisen from the 1980 tax repeal. Second, event study estimates show no discernible effects in 1980–1981 (Figure 6), ruling out confounding from the tax repeal. A related concern is the Medicaid expansion under the Omnibus Budget Reconciliation Acts of 1986–1987. Alaska's expansion did not take effect until January 1989 (General Accounting Office, 1989), outside our main study period. Moreover, SDID assigns time weights to pre-treatment periods most comparable to post-treatment periods, mitigating concerns about policy shocks in control states.[12]

Fertility, Mortality, and Composition. The APFD increased fertility by 11.4 births per 1,000 women and had no effect on neonatal mortality (Table A7), consistent with Yonzan et al. (2024). A potential concern is that the fertility increase reflects compositional changes—for instance, if the dividend induces births among less educated mothers who would not otherwise have children. We find no evidence of such changes: the APFD has no statistically significant effects on maternal education, age, race, marital status, or birth parity (Table 2). The absence of compositional changes indicates the fertility response represents additional births to mothers with similar characteristics as those already giving birth, validating our identification strategy. Under quantity–quality tradeoffs (Becker and Lewis, 1973), this fertility increase should reduce average birth outcomes if household resources are spread across more children, making our observed health improvements lower-bound estimates. Because eligibility requires residency by December of the prior year, mothers could potentially time births for their child to qualify, but we observe no December spike during the post-implementation period and similar patterns between Alaska and synthetic Alaska (Figure A11).[13]

[12] Since Medicaid expansions improve infant health (Currie and Gruber, 1996), any expansion in control states not fully accounted for by the time weights would bias our estimates downward, making our findings a lower bound.

[13] Alaska and synthetic Alaska also exhibit nearly identical seasonal patterns in birth outcomes (Figure A12), ruling out seasonality as a confounding factor.

5 Mechanisms

Section 4 establishes that the APFD improves birth outcomes primarily through accelerated fetal growth and increased prenatal care, with substantially larger effects when transfers arrive early in pregnancy. Why does timing matter despite the dividend's predictability? And why do effects concentrate among disadvantaged mothers? This section examines the economic constraints and behavioral responses that make payment timing consequential for prenatal investment.

5.1 Consumption and Excess Sensitivity

If households could smooth consumption, the timing of a predictable cash transfer should not matter. We test this using CEX data (1996-2004, years with publicly available state identifiers) to examine spending responses in health-relevant categories: food consumption, healthcare, and personal care. Extending Kueng (2018)'s analysis of aggregate non-durable consumption to pregnancy-relevant spending components, Figure 7 reveals excess sensitivity: spending on food away from home, healthcare, and personal care increases precisely when dividends arrive, with no anticipation in prior quarters. Healthcare expenditure responds particularly strongly, indicating that prenatal investments depend on cash-on-hand rather than being smoothed over anticipated income. Thus liquidity constraints possibly contribute to why payment timing matters for birth outcomes. Transfers arriving early enable investments during critical developmental windows, while later transfers arrive after key decisions have been made under constraint.

5.2 Maternal Health and Behaviors

Maternal medical risks. Using available natality data from 1989-2004, Table A8 shows the APFD reduced pregnancy-associated hypertension by 0.6 percentage points per thousand dollars (15% decline, $p < .05$) and anemia by 1.1 percentage points (20% decline, $p < .01$). A composite measure of other risks declined by 3.4 percentage points ($p < .10$). Remaining conditions show small, insignificant coefficients. We do not find any effects by timing of payment either.

Health behaviors. Figures A13 display effects on smoking and drinking during pregnancy. We find no significant effects on smoking. Conversely, cash transfers significantly reduced maternal drinking, with somewhat larger effects for early-pregnancy transfers albeit not statistically significantly different across trimesters. The concentrated drinking reductions are also among disadvantaged mothers suggesting that financial stress possibly drives unhealthy coping behaviors and income relief enables healthier choices that benefit maternal health (Evans and Garthwaite, 2014; González and Trommlerová, 2022).

Earlier transfers generate substantially larger improvements in birth and gestation outcomes as well as the number of prenatal care visits (Figure 3). This strong gradient contrasts with limited timing effects for health behaviors (and on the timing of the first prenatal care visit). The difference may be due to pregnancy awareness: most women do not learn of pregnancy until week six. Behavioral responses require awareness, but pre-conception and early transfers may improve birth outcomes through mechanisms operating before awareness—improved maternal nutrition and reduced stress—and subsequently through mechanisms operating after awareness through better planning—number of prenatal care visits and healthier pregnancy behaviors.

5.3 Labor Supply and Work Flexibility

Using CPS data (1977-1988), Table 3 shows APFD receipt increased part-time work by 1.8 percentage points ($p < .10$)—consistent with findings from Jones and Marinescu (2022). In our context, this is driven by women with existing children. Among mothers with children, receipt increased part-time work by 3.2 percentage points ($p < .05$) while reducing full-time work by 3.3 percentage points, albeit insignificant. The results suggest that mothers may be switching from full-time to part-time work, which may also contribute to stress reduction. We do not find differential effects by payment quarters.

5.4 Discussion

In Section 2, we conjectured that earlier cash transfers would generate larger health benefits through two channels. First, by relieving financial stress and enabling better maternal nutrition and health investments, which in turn benefit the baby. Second, by facilitating better planning for prenatal care, which enables healthier pregnancy behaviors, and more frequent monitoring. We further hypothesized that effects would concentrate among less-educated mothers who are more likely to face liquidity constraints. Our findings strongly support these conjectures.

The monotonic decline in effectiveness—from 50 grams for pre-conception transfers to 22 grams for third-trimester transfers—confirms that earlier payments generate larger returns through both channels. First, earlier transfers improve maternal health: we find reductions in pregnancy-associated hypertension and anemia, consistent with stress relief and improved maternal nutrition. Our consumption analysis further supports this: food and healthcare spending spike when dividends arrive. These maternal health improvements benefit the baby—earlier transfers accelerate fetal growth more than later ones. Second, earlier transfers enable better planning for prenatal care: they increase prenatal care visits more, allowing mothers to access guidance on healthy pregnancy behaviors and enabling more frequent monitoring throughout pregnancy. We find drinking reductions and declines in pregnancy complications, consistent with better planning and guidance.

The concentration of effects among less-educated mothers validates our conjecture that financial constraints drive timing effects. These mothers show 34 gram birth weight gains compared to negligible effects for college-educated mothers, with corresponding gradients in fetal growth and prenatal care. Mothers who can self-finance early prenatal investments respond minimally to transfer timing while mothers who cannot depend critically on when income support arrives.

These findings demonstrate that cash transfers have critical period effects on infant health, with earlier support generating substantially larger returns through enabling investments—in maternal health and in infant health—when they matter most for fetal development. Simply retiming transfer payments—at no additional fiscal cost—could substantially improve infant health outcomes.

6 Conclusion

Beginning in 1982, the APFD has provided a unique opportunity to study the impact of universal and unconditional cash transfers on infant health outcomes. Our comprehensive analysis reveals several important findings. First, we document that the APFD significantly improved birth outcomes. Second, the effects are larger when the dividend was paid earlier, particularly in the quarter right before pregnancy and during the first trimester. Third, the effects were particularly pronounced among mothers with lower educational attainment, suggesting that unconditional cash transfers may help relax financial constraints. Fourth, we identify multiple mechanisms underlying these improvements, including enhanced intrauterine growth and increased prenatal care utilization, with health-related and economical behavioral responses also contributing.

Our findings contribute to the literature on cash transfers and infant health by providing systematic evidence on payment timing effects. We find that cash transfers received before conception or during the first trimester yield substantially larger benefits compared to later-pregnancy transfers. Our results suggest that early unconditional cash transfers may be particularly effective at improving fetal growth and enhancing prenatal care utilization, leading to improved birth outcomes. Additionally, we find that the APFD reduces maternal medical risks, including pregnancy-associated hypertension, anemia, and other unspecified medical risk irrespective of payment timing. We find suggestive evidence that early transfers enable healthier maternal behaviors, with early pregnancy income support being particularly effective in promoting alcohol abstinence, and allowing mothers to adjust their work schedules during the payment period. The consumption response patterns we document—showing excess sensitivity and no anticipation effects—further confirm that the timing of payment has real consequences for household behavior.

Our findings have important implications for the design of social policies aimed at improving infant health outcomes. While previous research has focused primarily on targeted or conditional transfers, our results suggest that universal and unconditional cash transfers can generate substantial improvements in birth outcomes. Moreover, the timing effects we document suggest that policymakers could consider front-loading payments to expectant mothers or providing supplemental

support during early pregnancy stages.

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Figures and Tables

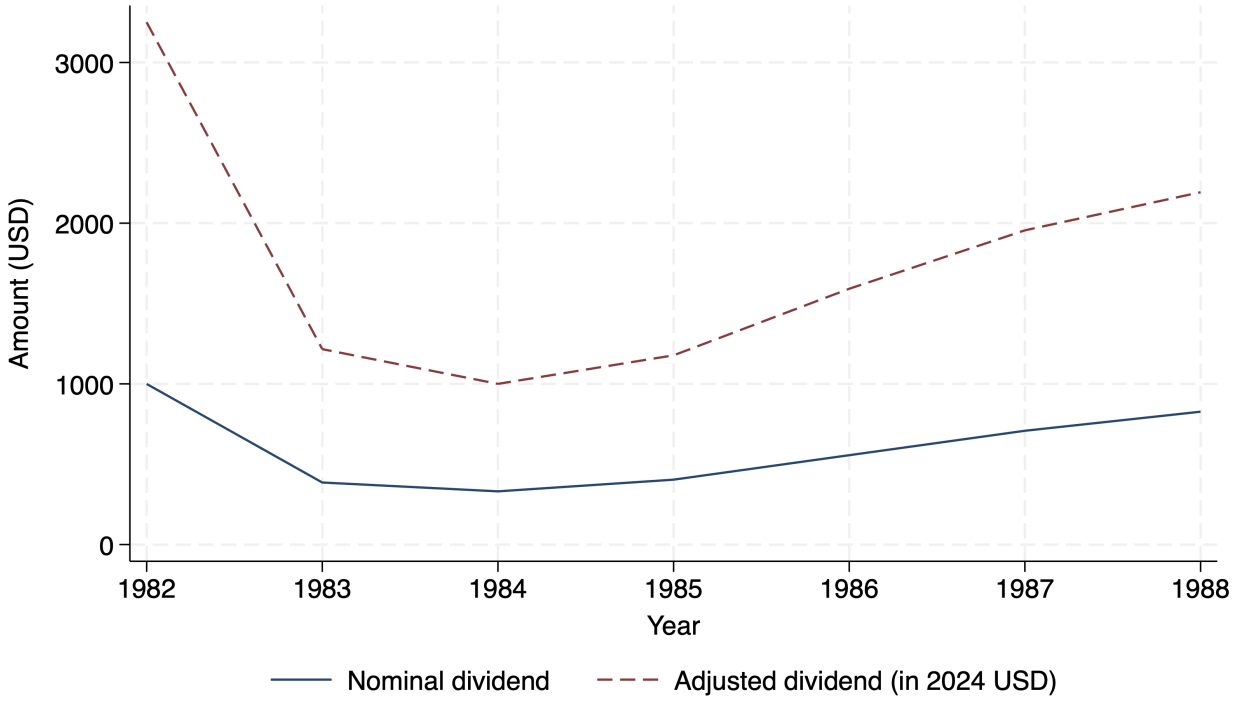


Figure 1: Alaska Permanent Fund Dividend

Notes: The Figure plots dividend amount in 1982-1988. The solid line plots nominal dollars and dashed line plots real amount (in 2024 USD).

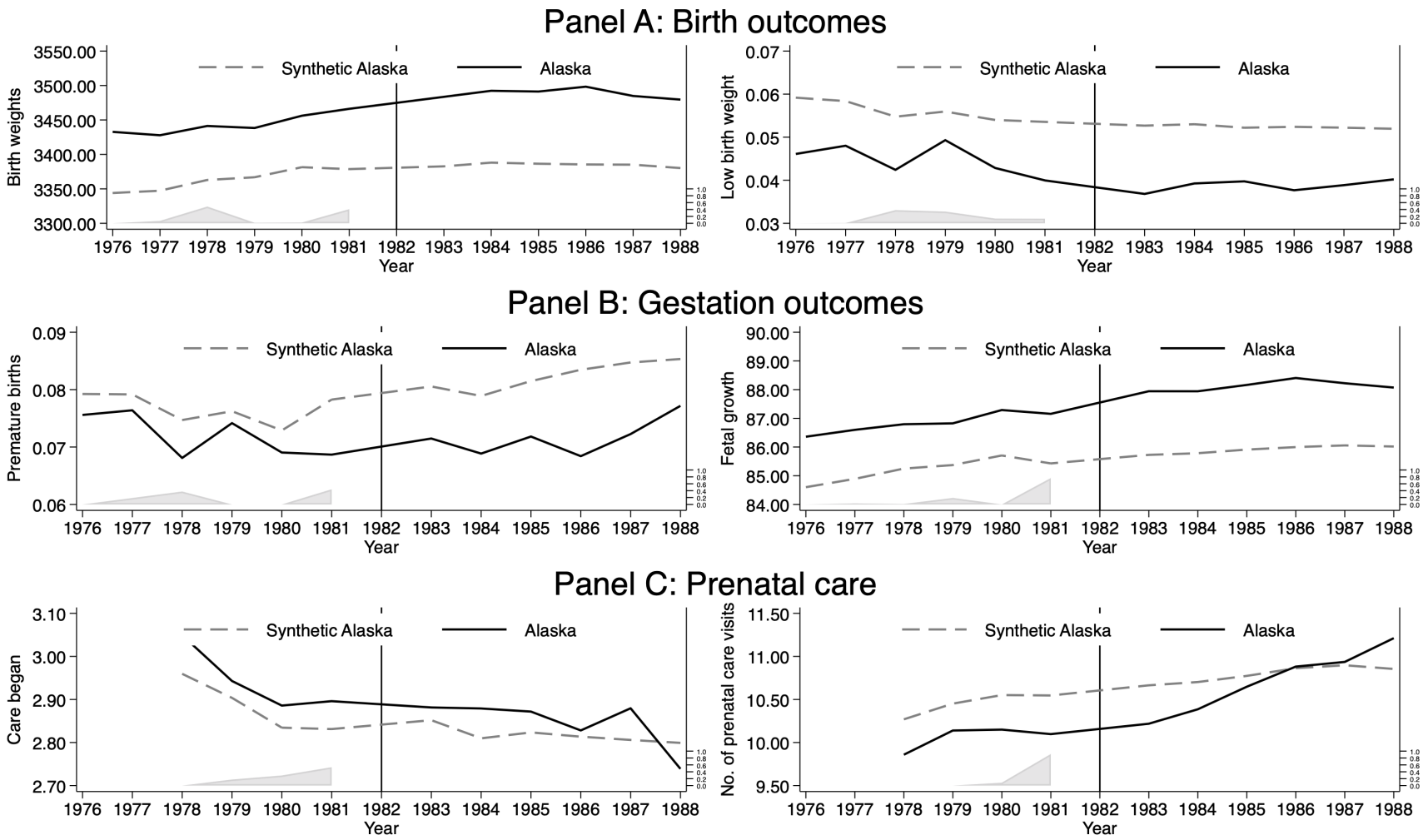


Figure 2: Raw Trend of Birth Outcomes and Mechanism for Alaska and Synthetic Alaska

Notes: This figure presents birth outcomes and mechanisms for Alaska (solid line) and synthetic Alaska (dashed line) constructed using SDID. The sample period is 1976–1988 for birth outcomes and fetal growth, and 1978–1988 for prenatal care due to data availability. The vertical solid line indicates 1982, the year APFD was implemented. Shaded gray areas represent the optimal pre-treatment time weights.

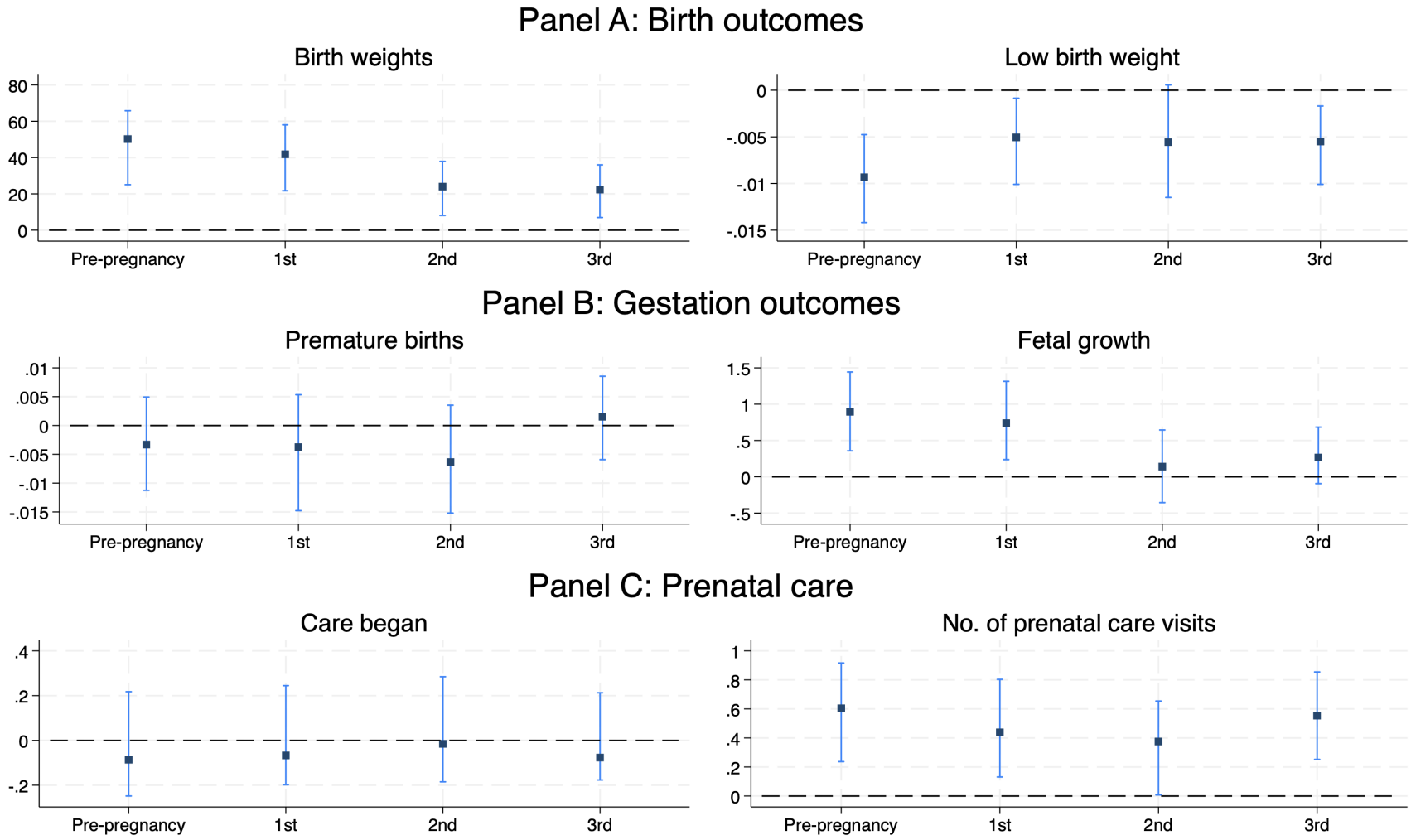


Figure 3: The Timing of APFD on Birth Outcomes and Mechanisms

Notes: The figure shows the marginal effects of the APFD on birth outcomes and mechanisms by trimester of receipt, with 95% confidence intervals. Estimates correspond to Table A5.

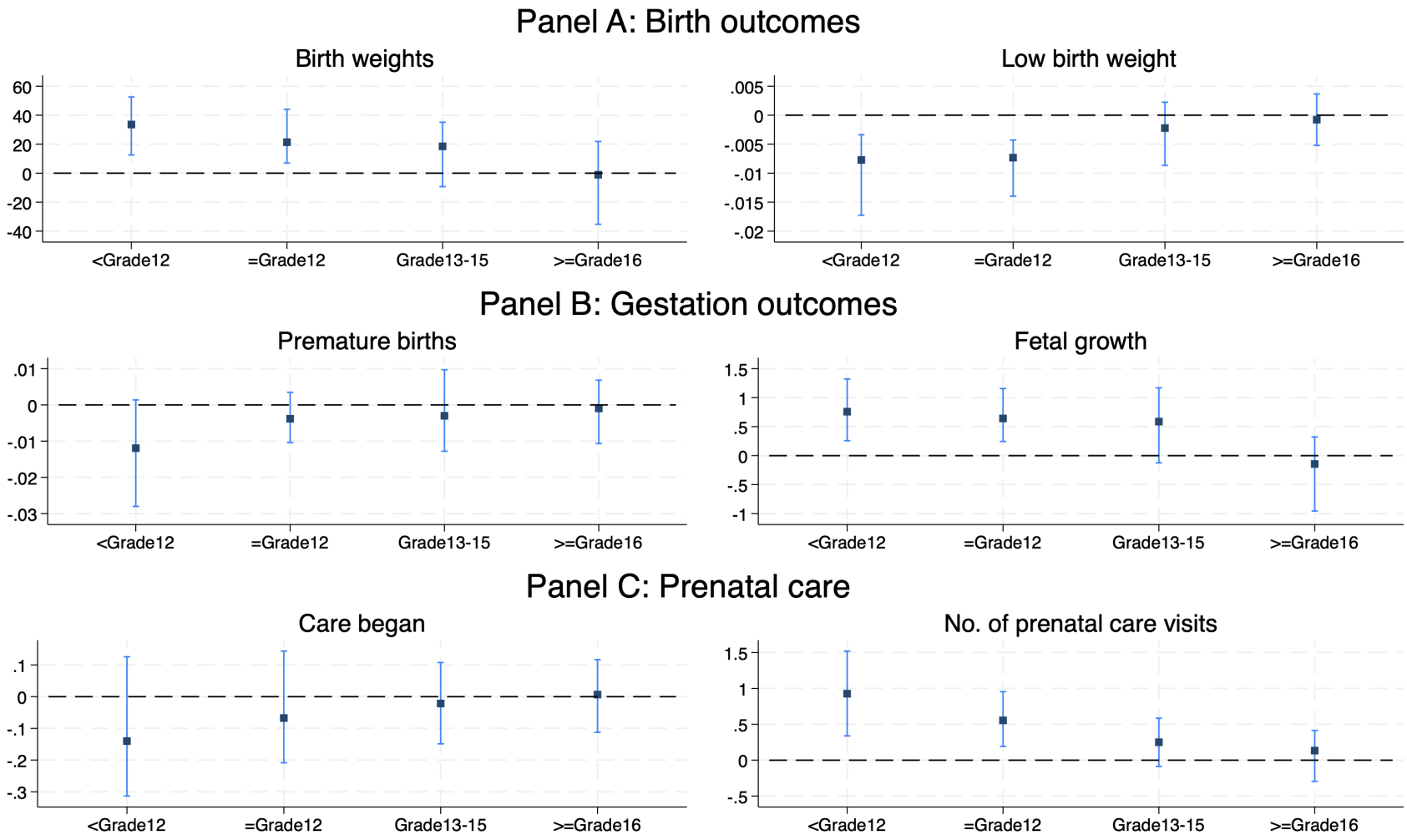


Figure 4: The Effects of APFD on Birth Outcomes and Mechanisms by Maternal Education

Notes: This figure shows the marginal effects of APFD on birth outcomes and mechanisms by maternal education, with 95% confidence intervals. Estimates correspond to Table A6.

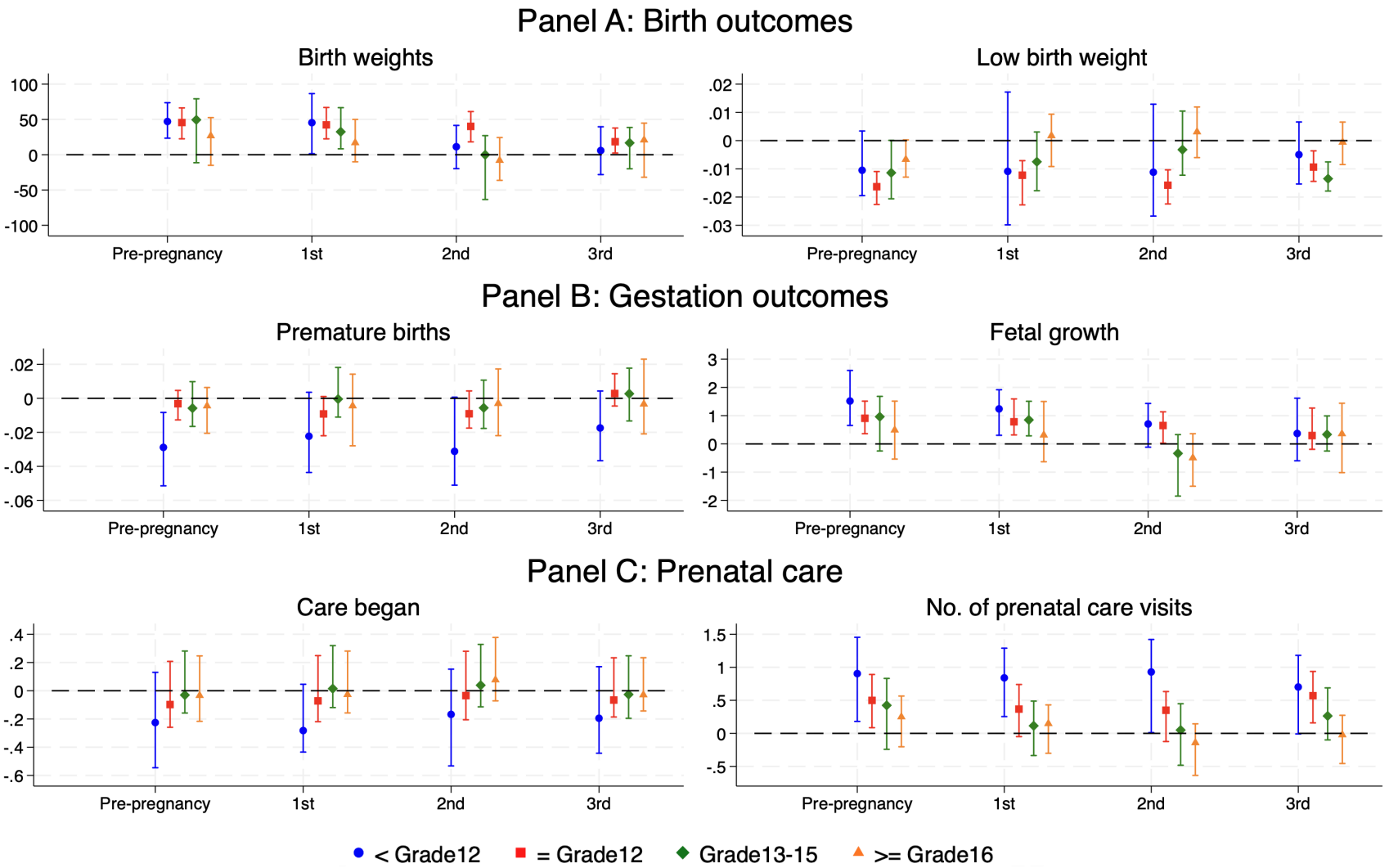


Figure 5: The Timing of APFD on Birth Outcomes and Mechanisms by Maternal Education

Notes: This graph plots estimates and 95% confidence intervals for the estimate of the trimester impact of APFD on the birth outcomes and mechanism by different maternal education groups.

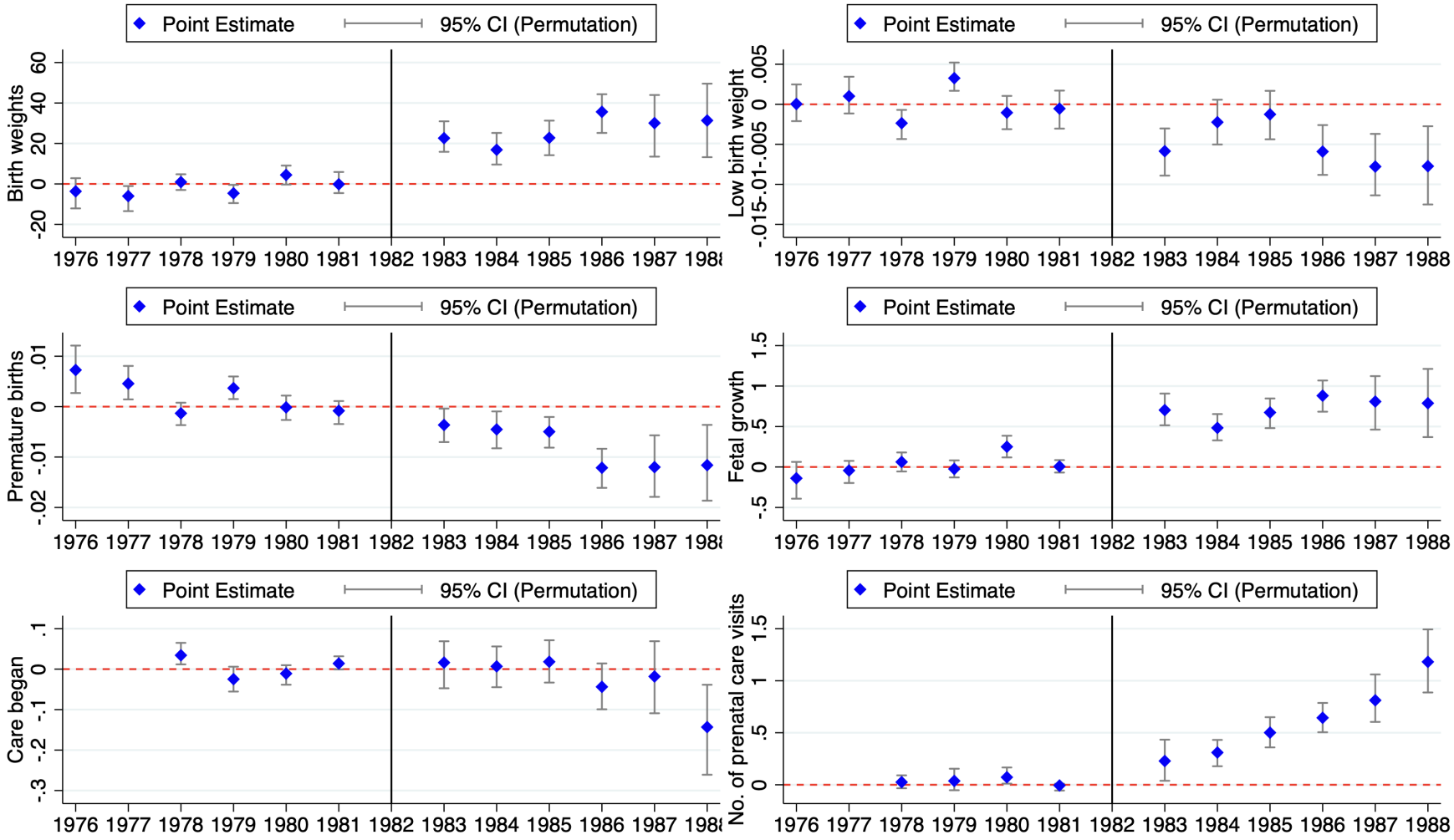


Figure 6: Event Study SDID

Notes: Sample period: 1976-1988 for birth and gestation outcomes; 1978-1988 for prenatal care (due to data availability). Event study SDID following the empirical strategy described in Appendix A.1.

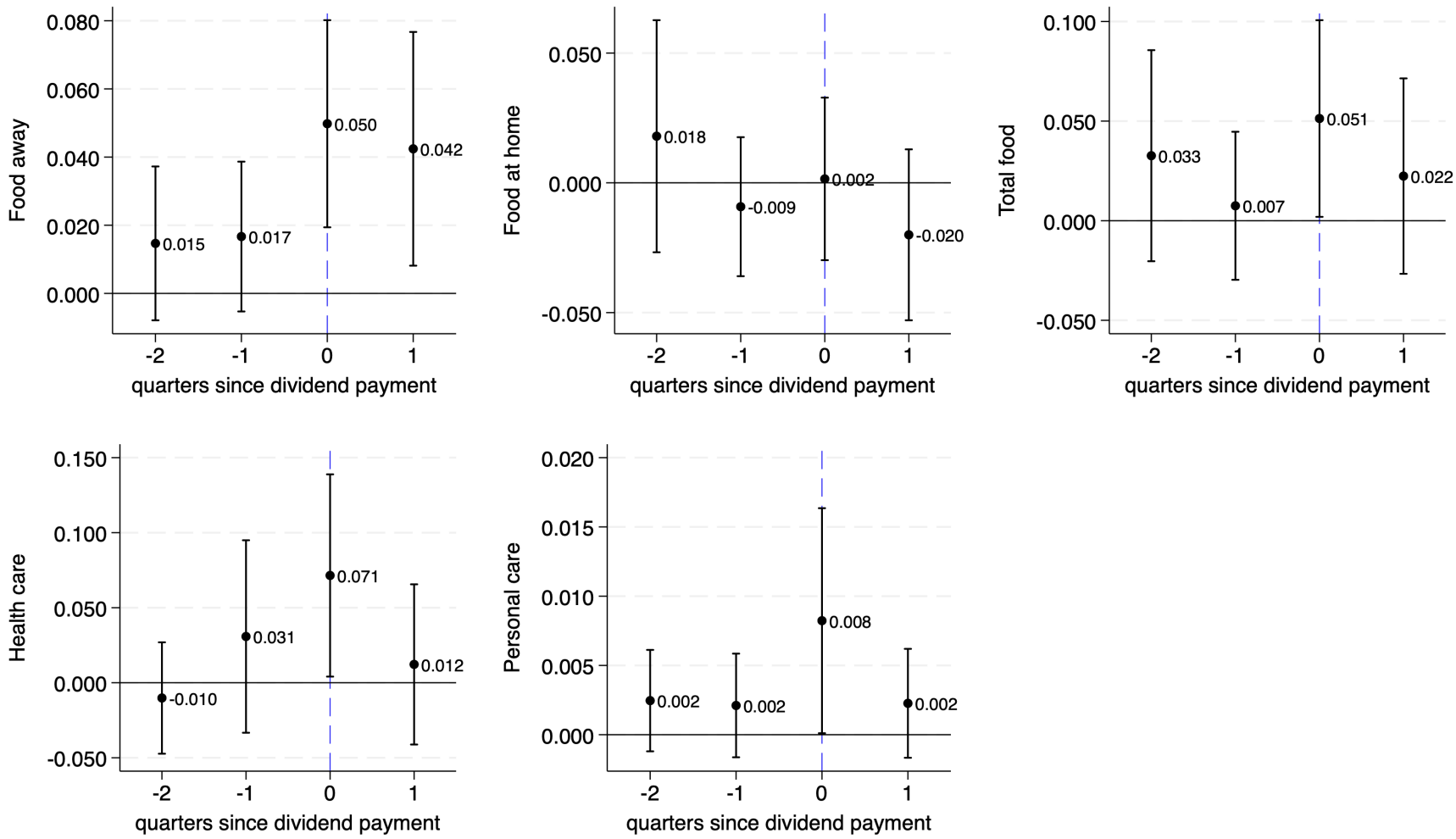


Figure 7: Excess Sensitivity and Lack of Anticipation Effects

Notes: This figure plots estimates and 95% confidence intervals for β_j following Eq. A3, with standard errors clustered at the household level. CEX data cover 1996-2004, the years with publicly available state identifiers. We find excess sensitivity and no anticipation effects, indicating binding liquidity constraints that make payment timing consequential for pregnancy investments. See Appendix A.2.

Table 1: The Effect of the APFD on Birth Outcomes and Mechanisms

	(1)	(2)	(3)	(4)	(5)	(6)
	Birth weight	Low birth weight	Premature births	Fetal growth	Care began	# prenatal care visits
Panel A: Binary Treatment (APFD Implementation)						
Treated	26.566**	-0.005**	-0.008**	0.723***	-0.027	0.613***
p-value	0.025	0.025	0.025	0.000	0.551	0.000
95% CI	[-4.167, 39.118]	[-0.010, 0.002]	[-0.015, 0.000]	[0.164, 1.028]	[-0.157, 0.260]	[0.281, 1.005]
Panel B: Continuous Treatment (Dividend Amount, $1000s)						
Amount	13.339**	-0.003**	-0.004**	0.370**	-0.017	0.303***
p-value	0.025	0.025	0.025	0.025	0.483	0.000
95% CI	[-3.049, 18.925]	[-0.005, 0.001]	[-0.007, 0.001]	[0.076, 0.520]	[-0.079, 0.126]	[0.139, 0.491]
Mean	3450.850	0.045	0.070	87.072	2.901	10.102
N	540	540	540	540	450	450

Notes: All regressions include control variables (pre-processing), state and year fixed effects, and are weighted by SDID unit and time weights ($\hat{\omega}^{\text{sdid}}$, $\hat{\lambda}^{\text{sdid}}$). Sample period: 1976-1988 for birth and gestation outcomes; 1978-1988 for prenatal care (due to data availability). Control variables: maternal education, age, race, parity, infant sex, and lagged unemployment rate. Inference based on permutation tests (1,000 iterations). Panel A shows effects of APFD implementation; Panel B shows effects per $1,000 dividend increase (2024 USD).
* p<0.10, ** p<0.05, *** p<0.01

Table 2: The Effect of APFD on Demographic Composition

	(1)	(2)	(3)	(4)	(5)	(6)
	$<$ Grade 12	$=$ Grade 12	Grades 13-15	Age 15-19	Age 20-24	Age 25-34
Treated	-0.016	0.007	-0.003	0.002	-0.016	0.002
p-value	0.116	0.544	0.759	0.522	0.117	0.813
95% CI	[-0.046, 0.001]	[-0.007, 0.033]	[-0.020, 0.010]	[-0.007, 0.012]	[-0.050, 0.017]	[-0.043, 0.036]
Mean	0.173	0.450	0.230	0.119	0.363	0.474
	(7)	(8)	(9)	(10)	(11)	(12)
	White	Black	First Child	Second Child	Third Child	Male Child
Treated	-0.018	0.005	-0.011	-0.004	-0.006	-0.003
95% CI	[-0.042, 0.009]	[-0.031, 0.028]	[-0.066, 0.016]	[-0.012, 0.008]	[-0.016, 0.014]	[-0.007, 0.001]
p-value	0.127	0.557	0.413	0.340	0.284	0.132
Mean	0.747	0.035	0.342	0.301	0.179	0.518
N	540	540	540	540	540	540

Notes: All regressions include control variables (pre-processing), state, and year fixed effects, and are weighted by the corresponding unit and time weights ($\hat{\omega}^{\text{sdid}}$, $\hat{\lambda}^{\text{sdid}}$). Sample period: 1976–1988. For each demographic characteristic examined, the control variables include all other characteristics except the one being tested. Inference based on permutation tests (1,000 iterations).
* p<0.10, ** p<0.05, *** p<0.01

Table 3: The Effect of APFD on Employment Outcomes by Sample

	Full Sample			No Child			With Child		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Employed	Full-time	Part-time	Employed	Full-time	Part-time	Employed	Full-time	Part-time
Treated	-0.005	-0.020	0.018*	-0.000	-0.004	0.007	0.001	-0.033	0.032**
p-value	0.730	0.197	0.083	1.000	0.871	0.458	0.978	0.114	0.028
95% CI	[-0.043, 0.038]	[-0.051, 0.033]	[0.001, 0.043]	[-0.058, 0.058]	[-0.062, 0.050]	[-0.014, 0.027]	[-0.049, 0.041]	[-0.075, 0.009]	[0.005, 0.067]
Mean	0.592	0.434	0.156	0.620	0.460	0.156	0.566	0.415	0.153
N	561	561	561	561	561	561	561	561	561

Notes: Data for employment is from Current Population Survey (CPS). Sample period: 1977–1988 (due to data availability). The sample includes all 50 states and the District of Columbia and is restricted to women aged 15-44. All regressions include control variables (pre-processing), state, and year fixed effects, and are weighted by the corresponding unit and time weights ($\hat{\omega}^{\text{sdid}}$, $\hat{\lambda}^{\text{sdid}}$). Control variables: maternal education, age, race. Inference based on permutation tests (1,000 iterations).

* p<0.10, ** p<0.05, *** p<0.01

A Appendix

A.1 Event Study

We refer Clarke et al. (2024) to plot event study for our SDID estimates. The event study estimates the dynamic treatment effects by comparing Alaska to the synthetic Alaska at each time period t, relative to a baseline. While standard DID method typically designate a single period (often one year prior to intervention) as the baseline, we define the baseline as a weighted average of all pre-treatment periods:

$$Y^{res}_{ibaseline} = \sum_{t=1}^{T_{pre}} \hat{\lambda}^{sdid}_{t} Y^{res}_{it} \tag{A1}$$

For each time period t, each estimate is defined as:

$$(Y^{res}_{1t} - \sum_{i=2}^{N_{co}} \widehat{\omega}^{sdid}_{i} Y^{res}_{it}) - (Y^{res}_{1baseline} - \sum_{i=2}^{N_{co}} \widehat{\omega}^{sdid}_{i} Y^{res}_{ibaseline}) \tag{A2}$$

The results for event study are shown in Figure 6.

A.2 Consumption Expenditures

In the spirit of Kueng (2018), we leverage data from the CEX to examine household spending responses to the APFD. Our sample period spans from 1996 to 2004. We use all other states as control groups to identify the causal effect of the APFD on household consumption. This window is determined by two data constraints related to the public identification of Alaska: (1) Alaska can be identified in the CEX public use files beginning in 1996; and (2) Alaska can be identified in the natality vital statistics public use files only through 2004.

Our analysis focuses on expenditure categories that are particularly relevant to infant and child health outcomes, including food consumed at home, food away from home, total food expenditures, health care, and personal care. We investigate anticipation effects and excess sensitivity using the following specification:

$$\Delta c_{ist} = \sum_{j=-1}^{2} \beta_j \cdot \text{APFD}_{i,t-j} + \gamma_s + \phi_t + \theta' X_{it} + \varepsilon_{it} \tag{A3}$$

$\Delta c_{ist} = c_{ist} - c_{is,t-1}$ denotes the quarterly change in consumption expenditure for household i in state s, measuring the difference in spending between consecutive quarters (e.g., the change from 1996Q1 to 1996Q2), APFD_{it} is the APFD payments (per dollar) received by all household members at the quarter t (equal to zero for all non-Alaska households and equal to zero in quarters when the dividend is not distributed in Alaska). We include the term $\text{APFD}_{i,t-j}$ to allow for two quarters of leads and one quarter of lags to capture anticipation effects and delayed spending responses. We control for state fixed effects γ_s, time fixed effects ϕ_t. X_{it} includes family size fixed effects, the number of family member changes, age, age square, and education. Standard errors are clustered at the household level. The results are shown in Figure 7.

Appendix Figures and Tables

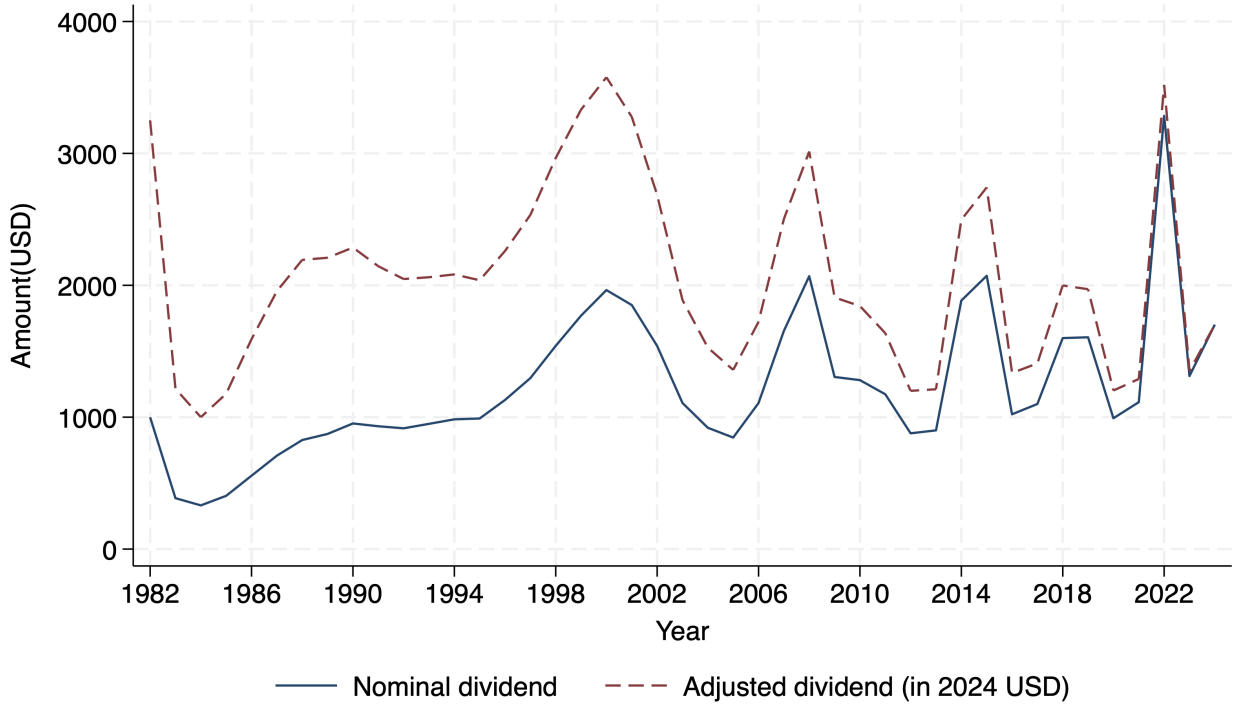


Figure A1: Alaska Permanent Fund Dividend

Notes: The Figure plots dividend amount in 1982-2024. The solid line plots nominal dollars and dashed line plots real amount (in 2024 USD).

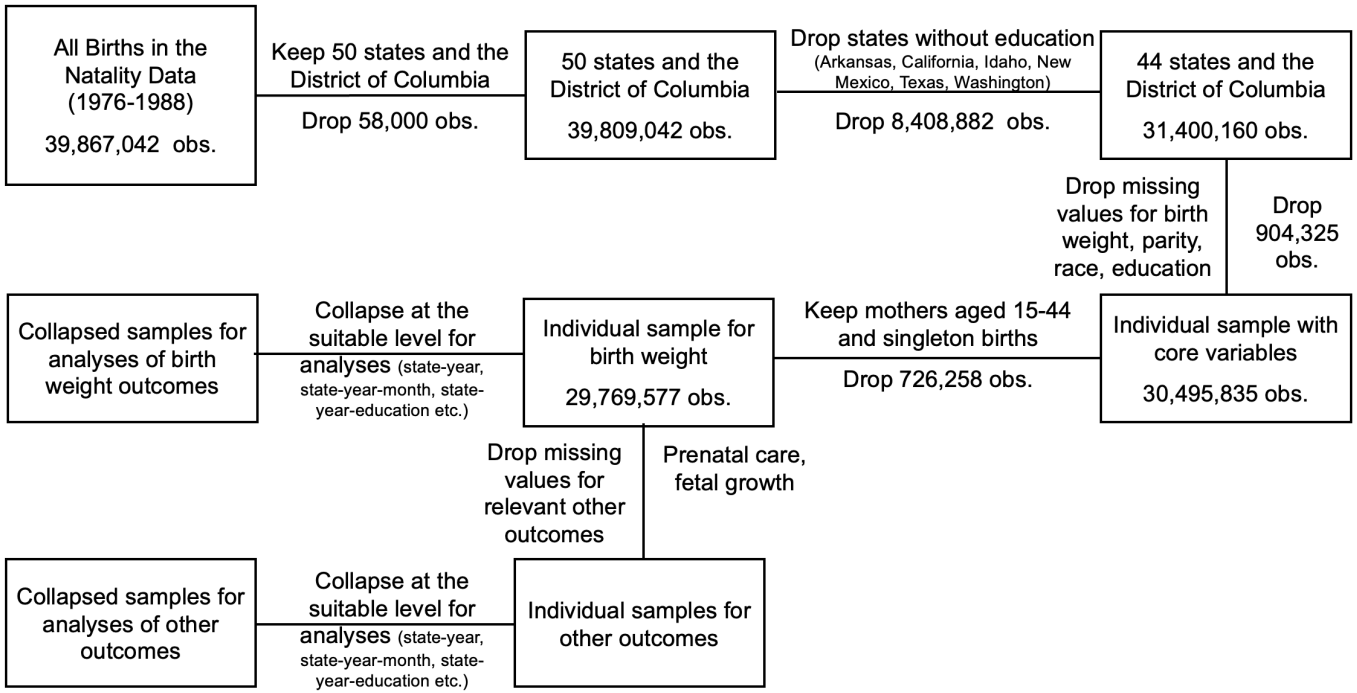


Figure A2: Construction of the Analytic Sample

Notes: This figure summarizes the steps used to construct the analytic dataset from the Natality files (1976–1988). We restrict to the 50 states and D.C., exclude states without maternal education data and observations missing key variables, and keep singleton births to mothers aged 15–44. The resulting core sample includes 29.8 million observations. Separate analytic samples are then created for birth and related outcomes, collapsed to the relevant analysis level. We also conduct sensitivity analyses over a longer time frame and including the excluded states (without education controls) to confirm that results are robust.

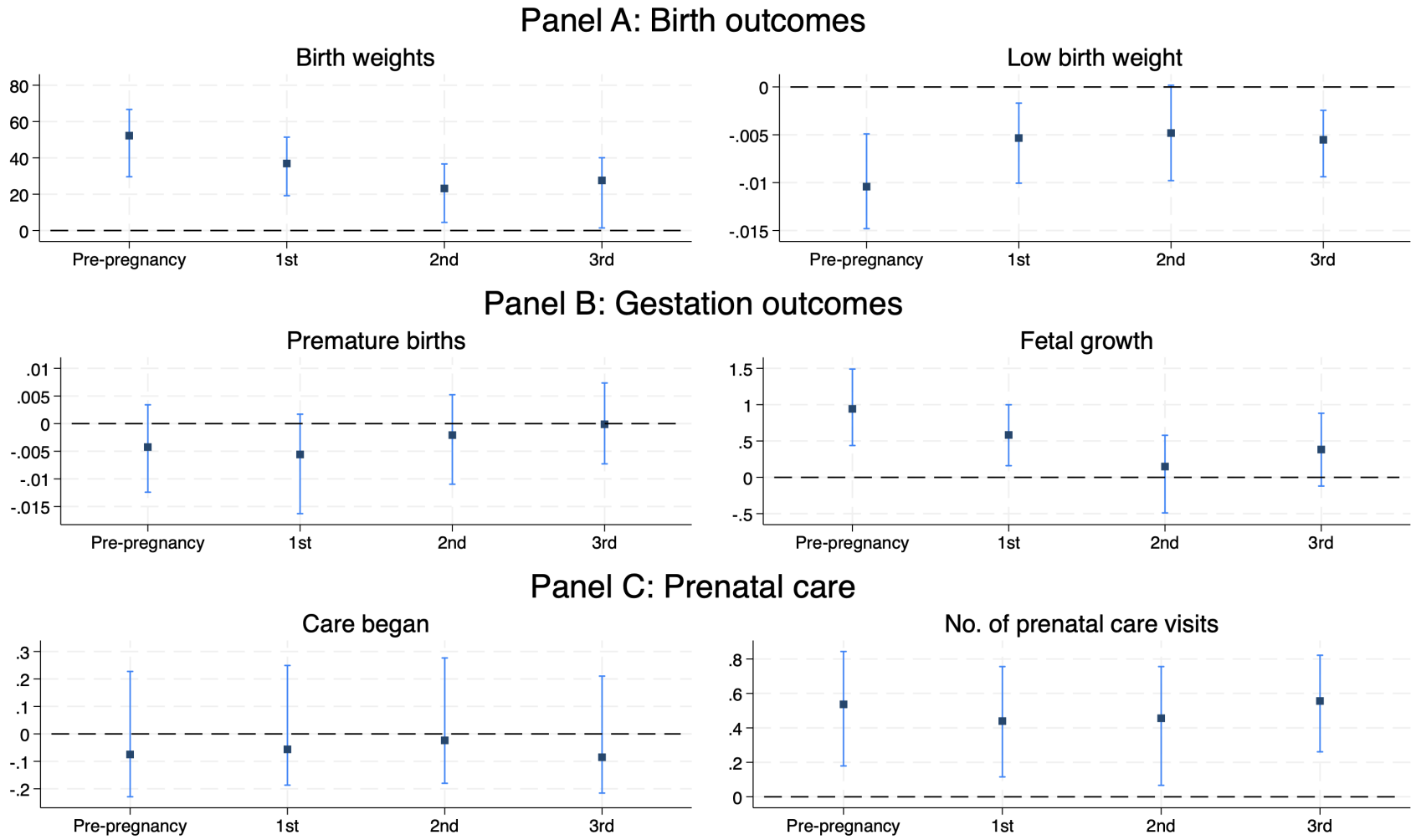


Figure A3: The Timing of APFD on Birth Outcomes and Mechanisms assuming 100% Disbursement in the First Month of Payment

Notes: The figure shows the marginal effects of the APFD on birth outcomes and mechanisms by trimester of receipt, with 95% confidence intervals. The difference from Figure 3 is that we assume that 100% of payments are made in the first month of disbursement (e.g., we posit that all payments are made in September in 1983 and in October in 1984-1988) to obtain binary indicators taking unity for the corresponding pregnancy stage and zero otherwise. We then estimate a specification similar to Eq. (8).

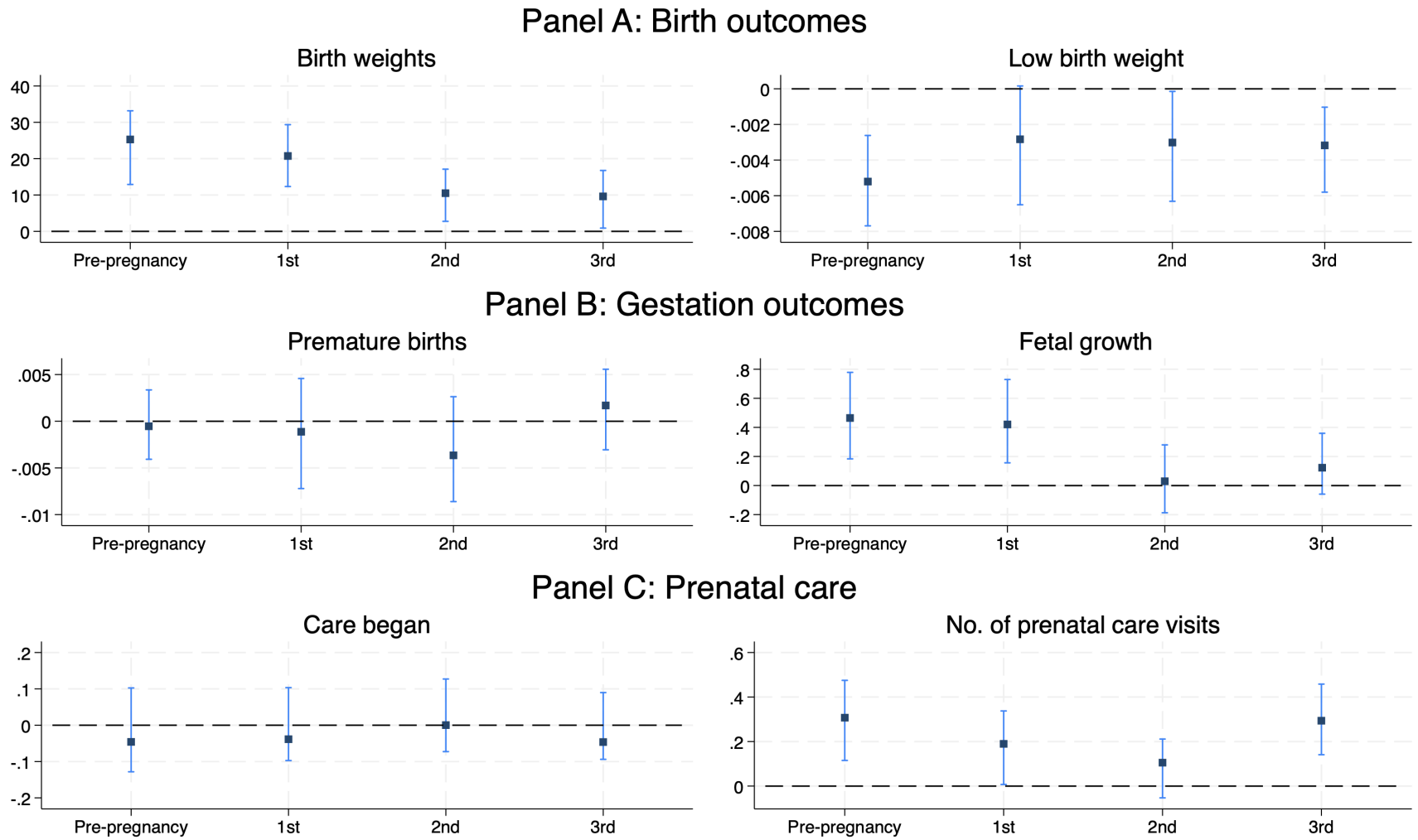


Figure A4: The Timing of APFD on Birth Outcomes and Mechanisms [Amount]

Notes: This graph plots estimates and 95% confidence intervals of the marginal effects of APFD on birth outcomes and underlying mechanisms across trimesters, following the specification in Eq. (8) using transfer amounts.

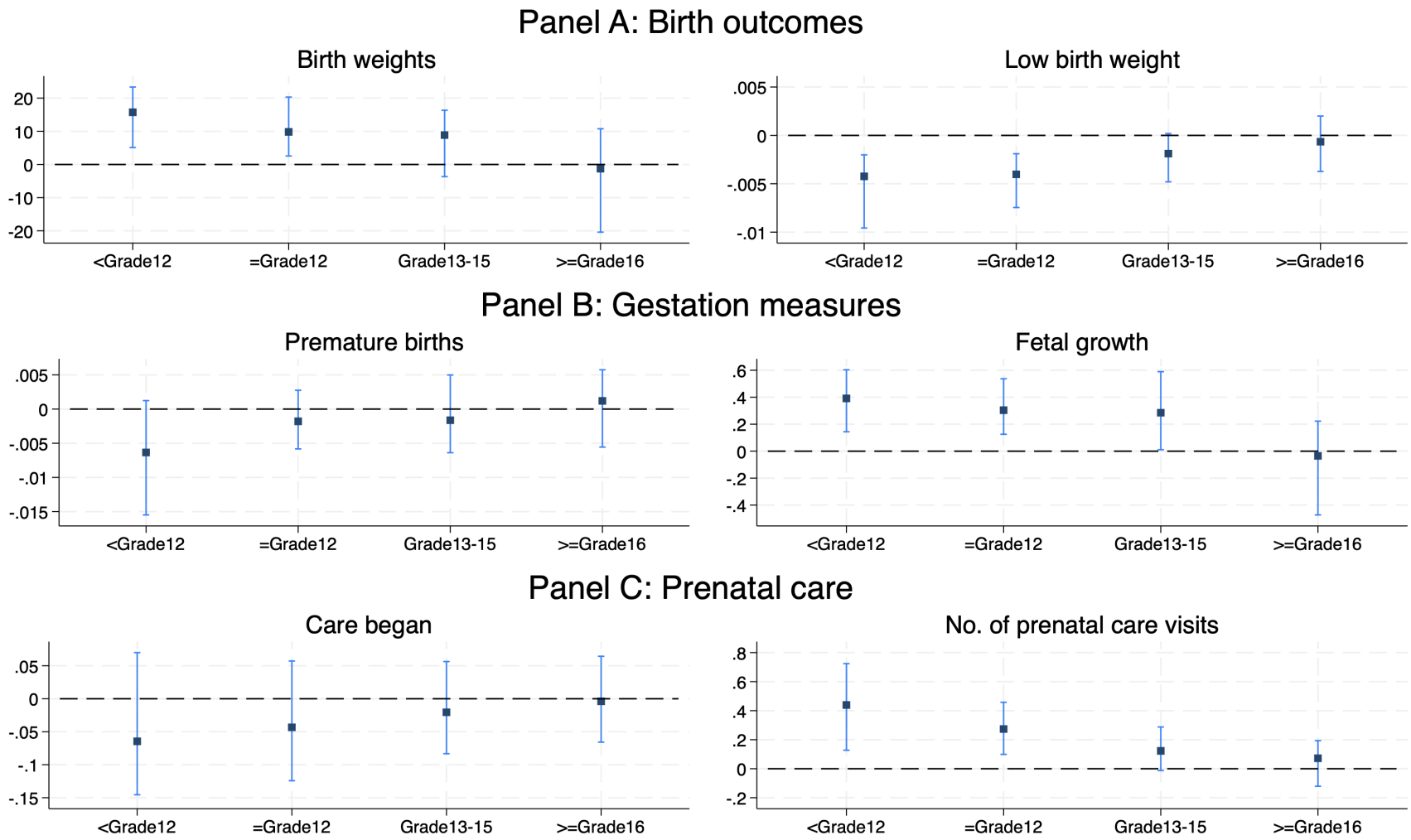


Figure A5: The Effects of APFD on Birth Outcomes and Mechanisms by Maternal Education [Amount]

Notes: This figure shows the marginal effects of dividend amount on birth outcomes and mechanisms by maternal education, with 95% confidence intervals.

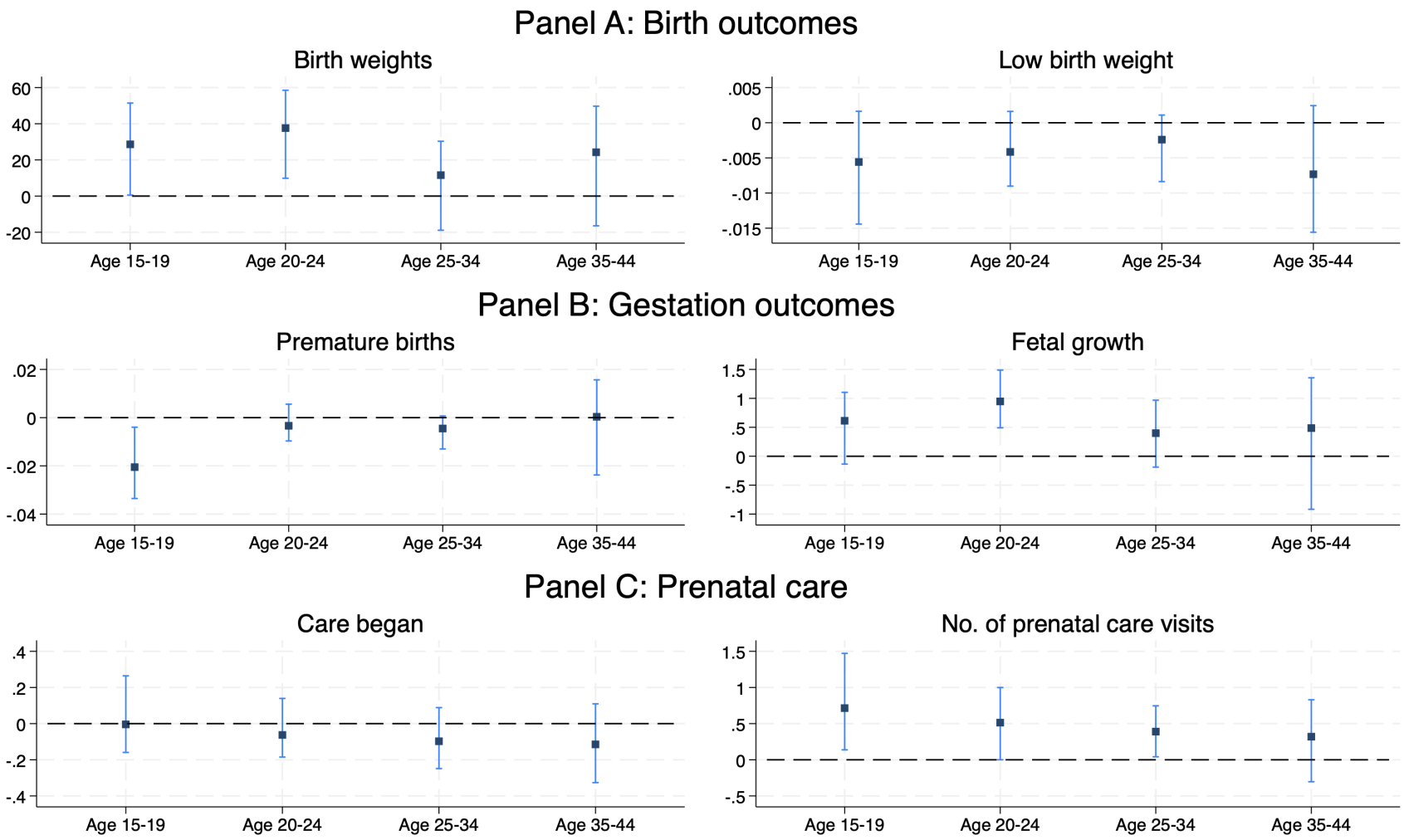


Figure A6: The Effects of APFD on Birth Outcomes and Mechanisms by Maternal Age

Notes: This figure shows the marginal effects of dividend amount on birth outcomes and mechanisms by maternal age groups, with 95% confidence intervals.

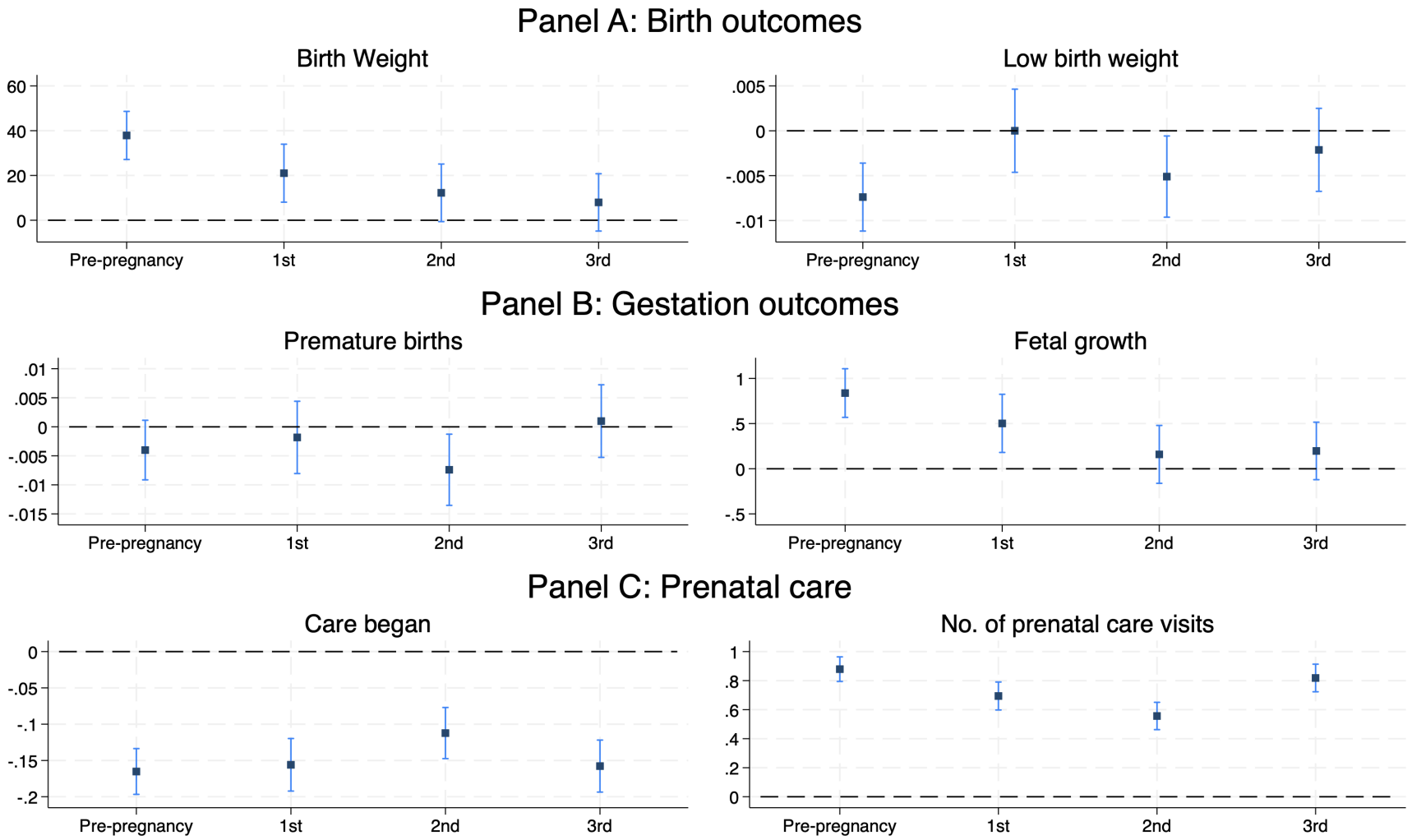


Figure A7: The Timing of APFD on Birth Outcomes and Mechanisms (Washington State as Control)

Notes: The figure shows the marginal effects of the APFD on birth outcomes and mechanism across trimesters using standard DID and Washington State as the control state. Analyses are conducted at the individual-level and do not control for maternal education due to data availability.

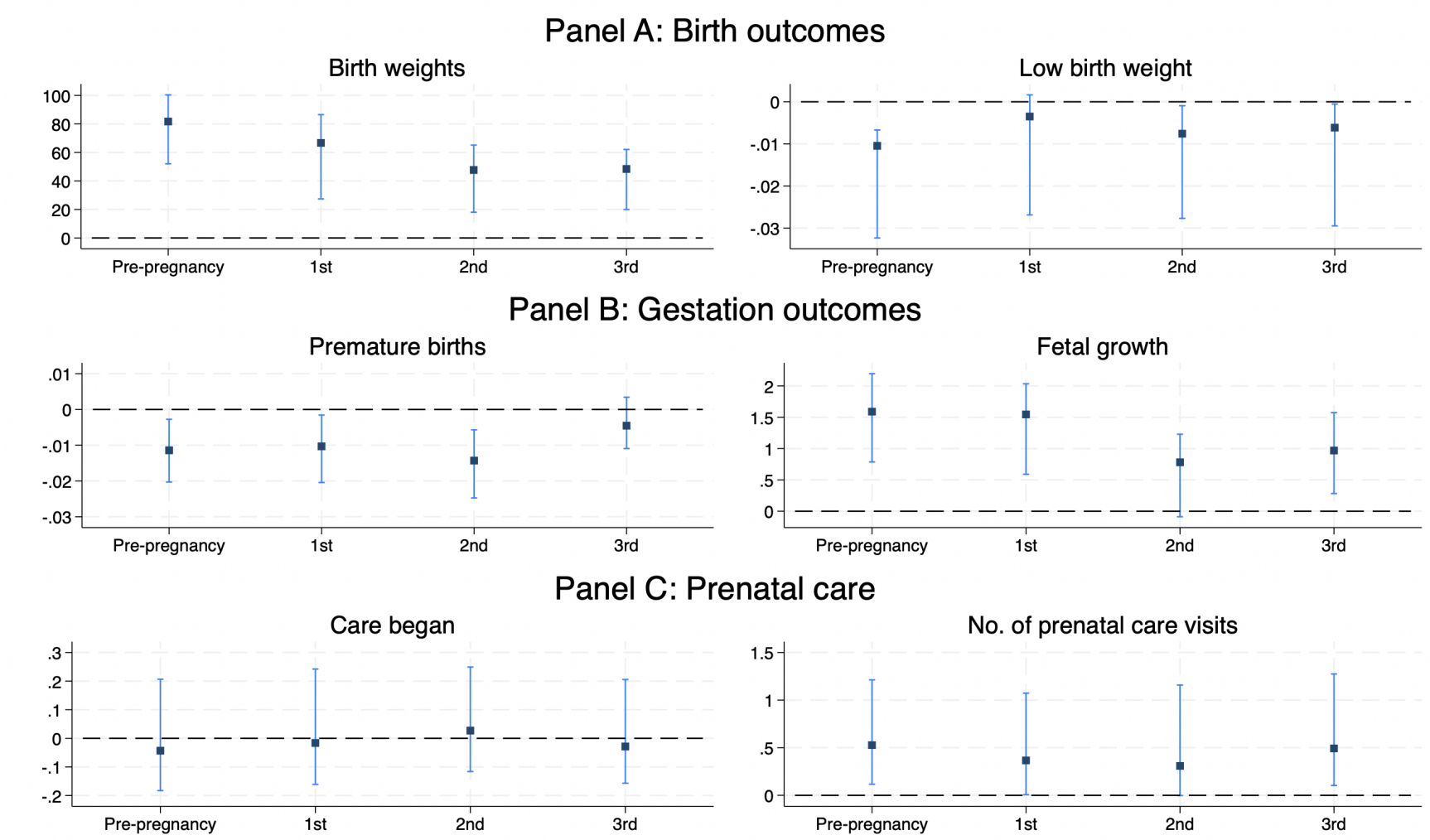


Figure A8: The Timing of APFD on Birth Outcomes and Mechanisms (SCM)

Notes: The figure shows the marginal effects of the APFD on birth outcomes and mechanism across trimesters using SCM.

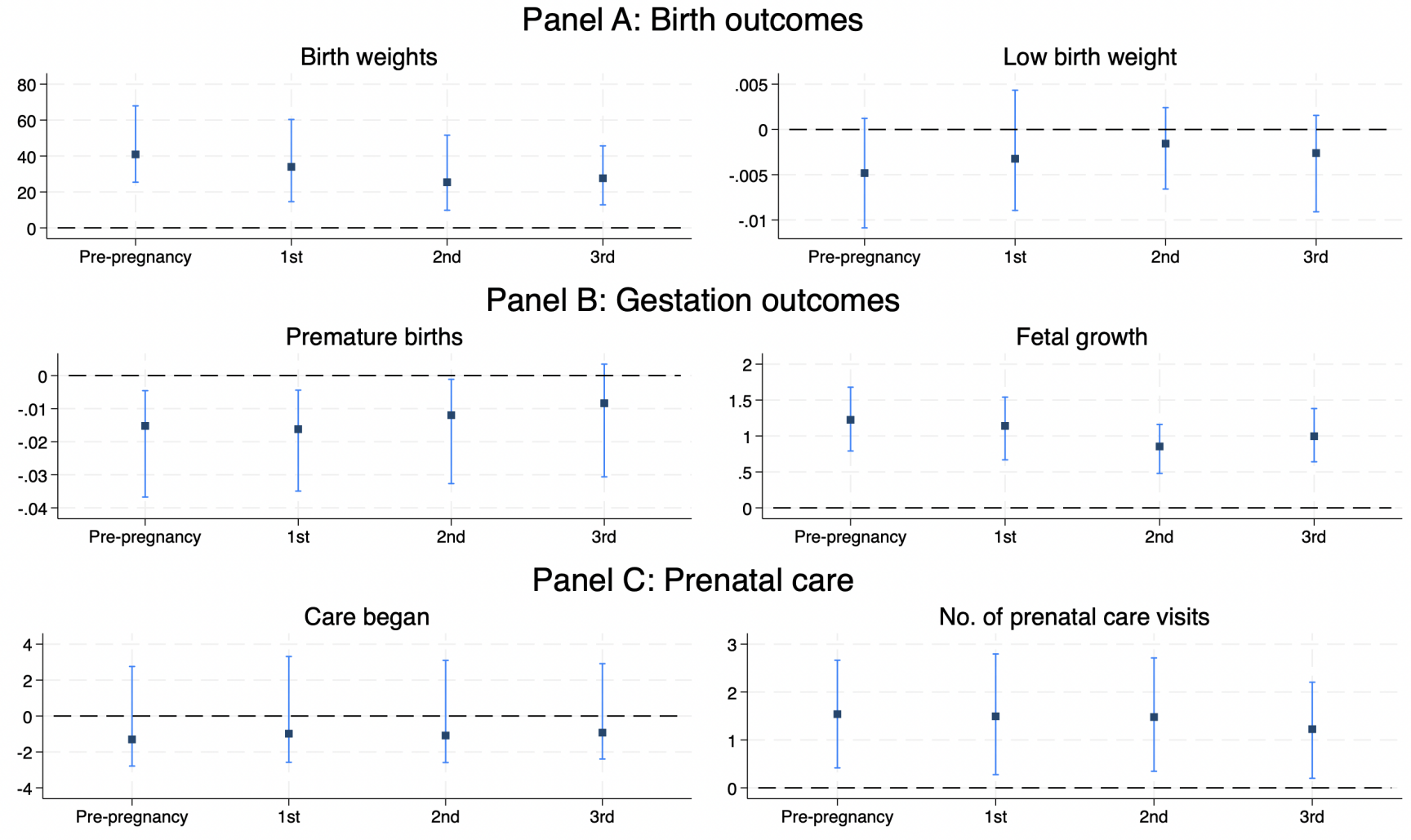


Figure A9: The Timing of APFD on Birth Outcomes and Mechanisms (1968-2004, All States)

Notes: The figure shows the marginal effects of the APFD on birth outcomes and mechanism across trimesters using data from 1968-2004 and all states (1978-2004 for prenatal care due to data availability). We do not control for maternal education and unemployment rate in this SDID specification due to data availability.

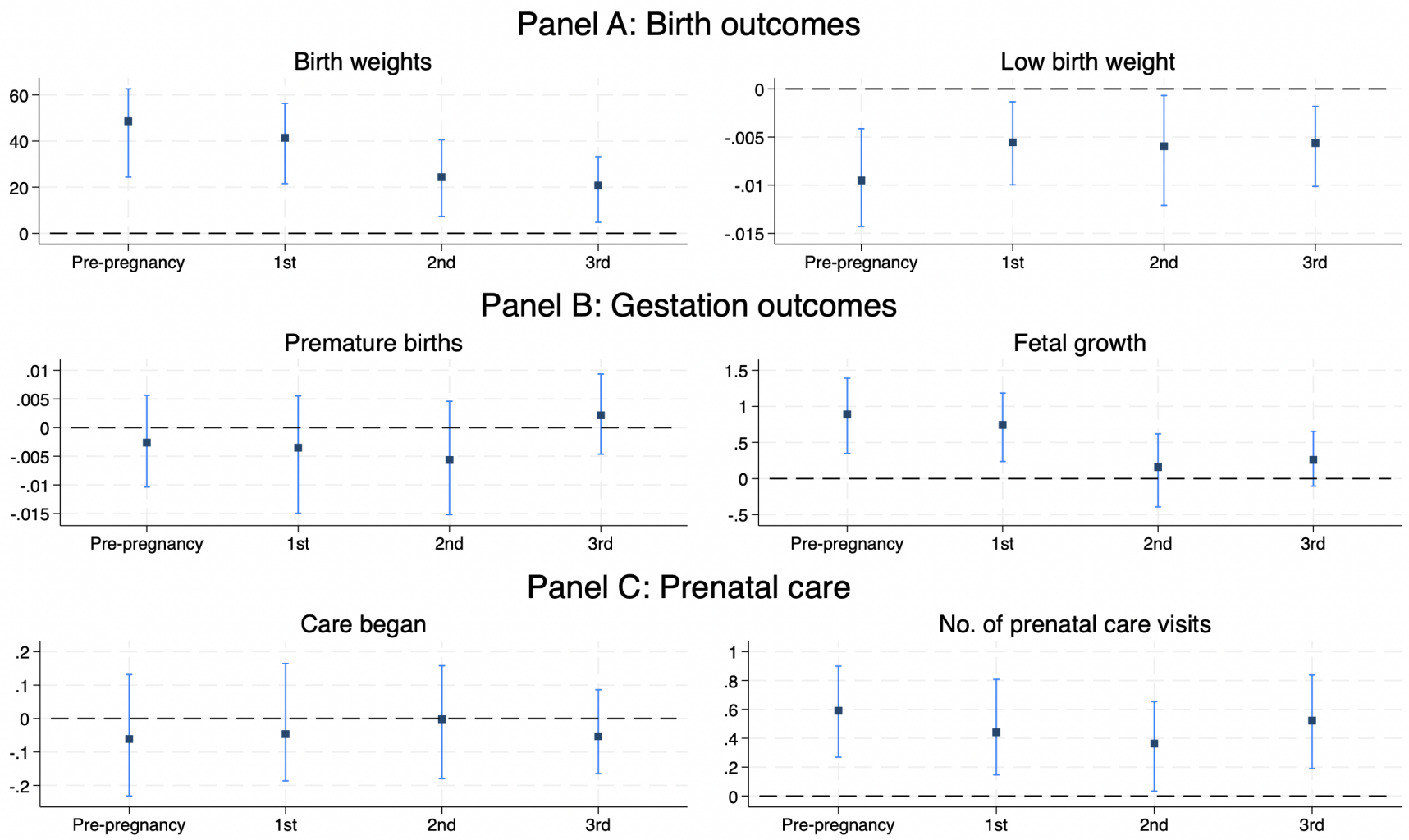


Figure A10: The Timing of APFD on Birth Outcomes and Mechanisms (Individual-level)

Notes: The figure shows the marginal effects of the APFD on birth outcomes and mechanism across trimesters using individual-level data but weighted using the SDID state and time weights.

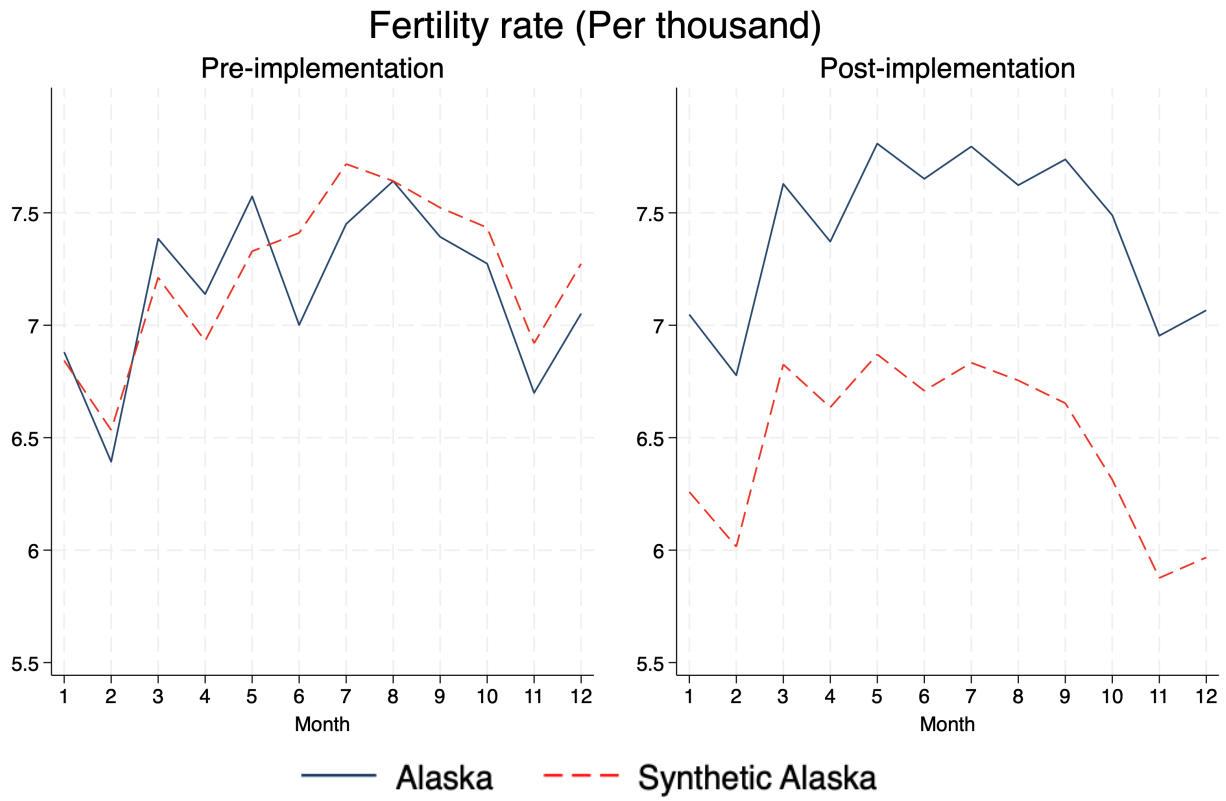


Figure A11: Average Fertility in a Given Month During the Pre-intervention and Post-intervention Periods.

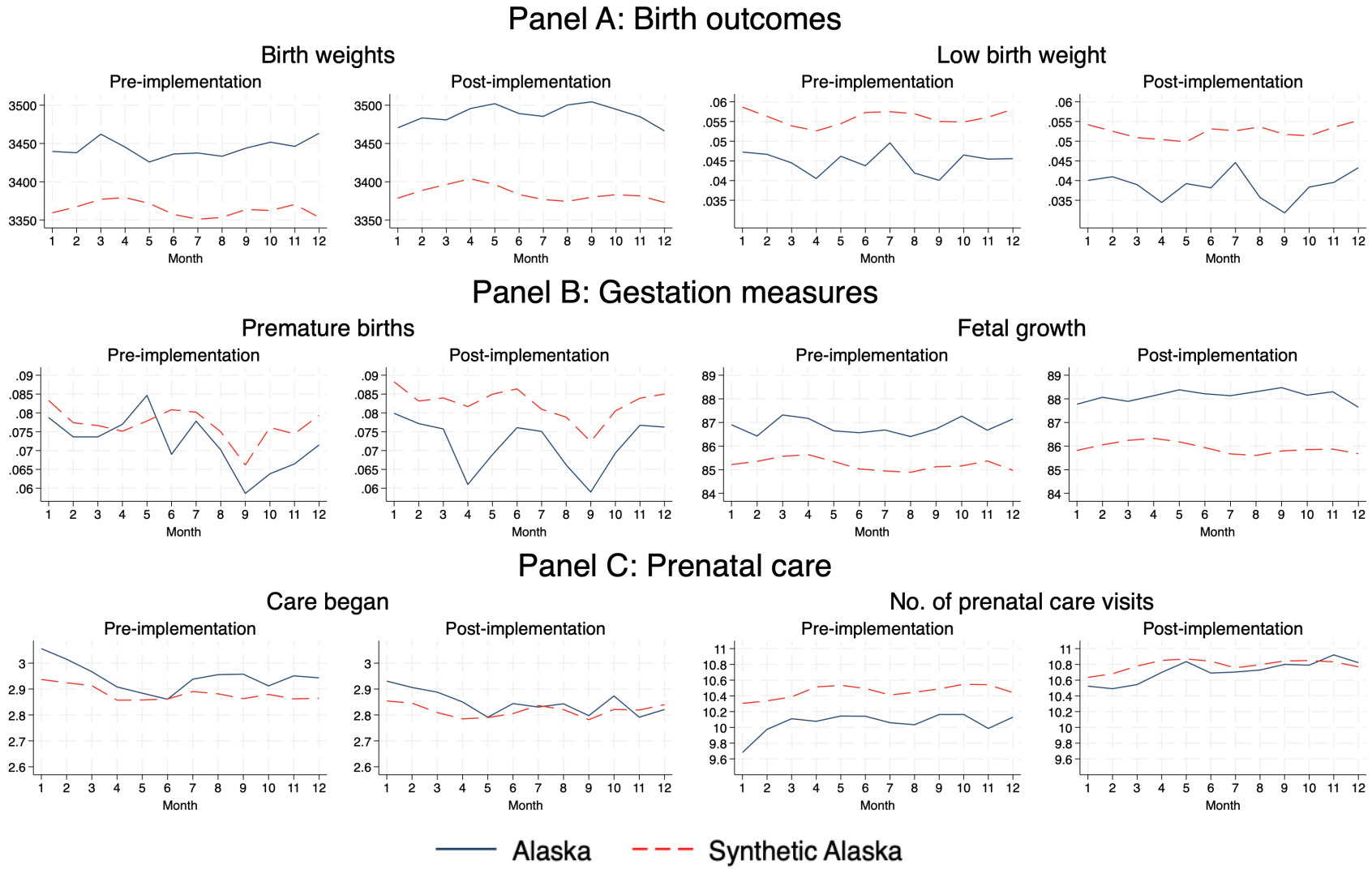


Figure A12: Average Birth Outcomes and Mechanisms in a Given Month During the Pre-intervention and Post-intervention Periods.

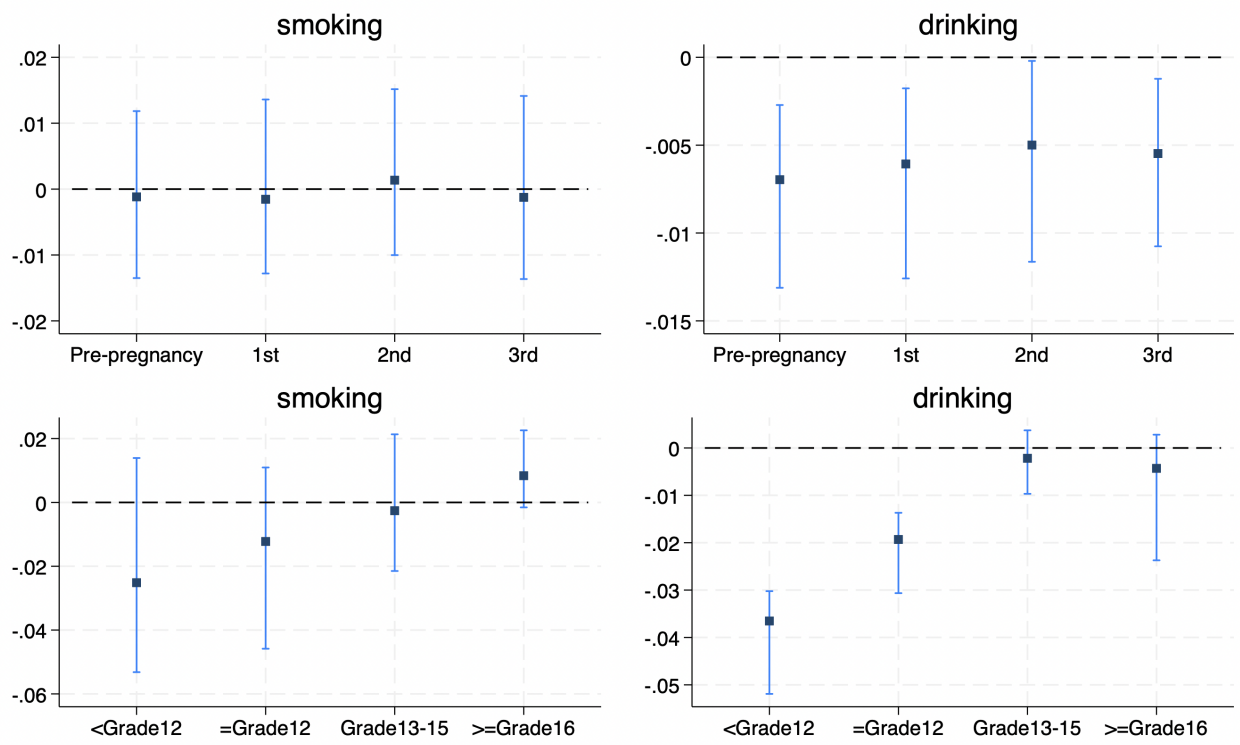


Figure A13: The Effects of APFD on Health-related Behaviors by Trimester and Maternal Education

Notes: This graph plots estimates and 95% confidence intervals for the estimate of the marginal timing effects of APFD on health-related behaviors follow the structure of Eq. (8) using amount and are weighted by the unit and time weights ($\hat{\omega}^{\text{sdid}}$, $\hat{\lambda}^{\text{sdid}}$) obtained from the birth weight outcome. The period covers 1989-2004 due to data availability.

Table A1: Unit Weights from SDID

	(1) Birth Weights	(2) Low Birth Weight	(3) Premature Births	(4) Fetal Growth	(5) Care Began	(6) # Prenatal Care Visits
Alabama	0.001	0.000	0.005	0.018	0.000	0.019
Arizona	0.000	0.000	0.000	0.046	0.000	0.023
Colorado	0.000	0.000	0.000	0.007	0.000	0.026
Connecticut	0.004	0.044	0.000	0.063	0.006	0.032
Delaware	0.000	0.052	0.000	0.000	0.000	0.035
District of Columbia	0.000	0.000	0.000	0.000	0.142	0.087
Florida	0.000	0.006	0.000	0.000	0.000	0.019
Georgia	0.000	0.000	0.000	0.000	0.071	0.057
Hawaii	0.021	0.069	0.000	0.000	0.128	0.000
Illinois	0.013	0.016	0.000	0.037	0.000	0.009
Indiana	0.000	0.014	0.000	0.000	0.000	0.040
Iowa	0.001	0.000	0.011	0.000	0.029	0.000
Kansas	0.022	0.055	0.005	0.057	0.016	0.027
Kentucky	0.000	0.000	0.000	0.000	0.000	0.003
Louisiana	0.101	0.000	0.044	0.167	0.000	0.000
Maine	0.000	0.000	0.053	0.000	0.000	0.000
Maryland	0.000	0.016	0.056	0.022	0.000	0.028
Massachusetts	0.034	0.000	0.000	0.009	0.022	0.016
Michigan	0.000	0.000	0.059	0.000	0.037	0.043
Minnesota	0.010	0.000	0.000	0.041	0.005	0.029
Mississippi	0.000	0.001	0.000	0.000	0.063	0.064
Missouri	0.000	0.000	0.000	0.000	0.043	0.031
Montana	0.051	0.000	0.000	0.050	0.000	0.037
Nebraska	0.028	0.000	0.000	0.072	0.000	0.008
Nevada	0.022	0.034	0.000	0.000	0.000	0.000
New Hampshire	0.096	0.152	0.115	0.081	0.096	0.000
New Jersey	0.013	0.000	0.016	0.013	0.039	0.014
New York	0.029	0.034	0.000	0.042	0.033	0.031
North Carolina	0.000	0.025	0.000	0.000	0.044	0.018
North Dakota	0.100	0.001	0.040	0.062	0.049	0.021
Ohio	0.010	0.000	0.000	0.000	0.033	0.017
Oklahoma	0.044	0.000	0.044	0.018	0.000	0.035
Oregon	0.014	0.063	0.047	0.014	0.011	0.028
Pennsylvania	0.033	0.012	0.046	0.000	0.000	0.014
Rhode Island	0.057	0.121	0.000	0.016	0.000	0.054
South Carolina	0.000	0.000	0.045	0.000	0.002	0.083
South Dakota	0.071	0.058	0.000	0.042	0.044	0.000
Tennessee	0.000	0.000	0.032	0.000	0.051	0.000
Utah	0.010	0.013	0.009	0.000	0.000	0.000
Vermont	0.000	0.059	0.016	0.004	0.000	0.000
Virginia	0.000	0.000	0.030	0.000	0.008	0.010
West Virginia	0.000	0.048	0.000	0.077	0.015	0.020
Wisconsin	0.058	0.015	0.064	0.041	0.011	0.023
Wyoming	0.158	0.092	0.262	0.000	0.000	0.000

Table A2: Time Weights from SDID

	(1) Birth Weights	(2) Low Birth Weight	(3) Premature Births	(4) Fetal Growth	(5) Care Began	(6) # Prenatal Care Visits
1976	0.000	0.000	0.000	0.000	—	—
1977	0.069	0.007	0.186	0.032	—	—
1978	0.485	0.383	0.374	0.013	0.000	0.000
1979	0.014	0.340	0.000	0.188	0.175	0.000
1980	0.022	0.136	0.000	0.000	0.292	0.084
1981	0.410	0.134	0.440	0.767	0.533	0.916
1983	0.166	0.166	0.166	0.166	0.166	0.166
1984	0.166	0.166	0.166	0.166	0.166	0.166
1985	0.166	0.166	0.166	0.166	0.166	0.166
1986	0.166	0.166	0.166	0.166	0.166	0.166
1987	0.166	0.166	0.166	0.166	0.166	0.166
1988	0.166	0.166	0.166	0.166	0.166	0.166

Note: Prenatal care data is available from 1978. The time weight for each post-reform year is $\frac{1}{T_{post}}$.

Table A3: Monthly Distribution of APFD Payments, 1982–1988

								Next Year			
Year	Jun	Jul	Aug	Sep	Oct	Nov	Dec	Jan	Feb	Mar	Apr
1982	6.4%	9.4%	9.4%	9.4%	9.4%	9.4%	9.4%	9.4%	9.4%	9.4%	9.4%
1983				24.6%	53.9%	21.5%					
1984					41.3%	51.2%	7.5%				
1985					48.4%	38.2%	13.4%				
1986					47.1%	37.2%	15.7%				
1987					47.7%	37.2%	15.1%				
1988					38.8%	38.0%	23.2%				

Source: Kueng (2018) Table A1: "This Table shows the estimated monthly distribution of dividend payments via checks. Distributions in 1982 and 1983 are estimated based on historical newspaper articles and Hsieh (2003)."

Table A4: The Probability of Receiving Dividends in Different Trimesters (by Birth Month), 1982-1988

								Next Year				
Year	**Jun**	**Jul**	**Aug**	**Sep**	**Oct**	**Nov**	**Dec**	**Jan**	**Feb**	**Mar**	**Apr**	**May**
Third trimester												
1982		6.40%	15.80%	25.20%	28.20%	28.20%	28.20%	28.20%	28.20%	28.20%	28.20%	28.20%
1983	18.8%	9.4%	0.0%	0.0%	24.6%	78.5%	100.0%	75.4%	21.5%	0.0%	0.0%	0.0%
1984	0.0%	0.0%	0.0%	0.0%	0.0%	41.3%	92.5%	100.0%	58.7%	7.5%	0.0%	0.0%
1985	0.0%	0.0%	0.0%	0.0%	0.0%	48.4%	86.6%	100.0%	51.6%	13.4%	0.0%	0.0%
1986	0.0%	0.0%	0.0%	0.0%	0.0%	47.1%	84.3%	100.0%	52.9%	15.7%	0.0%	0.0%
1987	0.0%	0.0%	0.0%	0.0%	0.0%	47.7%	84.9%	100.0%	52.3%	15.1%	0.0%	0.0%
1988	0.0%	0.0%	0.0%	0.0%	0.0%	38.8%	76.8%	100.0%	61.2%	23.2%	0.0%	0.0%
Second trimester												
1982		0.00%	0.00%	6.40%	15.80%	25.20%	28.20%	28.20%	28.20%	28.20%	28.20%	28.20%
1983	28.20%	28.20%	28.20%	18.80%	9.40%	0.00%	0.00%	24.60%	78.50%	100.00%	75.40%	21.50%
1984	0.00%	0.00%	0.00%	0.00%	0.00%	0.00%	0.00%	0.00%	41.30%	92.50%	100.00%	58.70%
1985	7.50%	0.00%	0.00%	0.00%	0.00%	0.00%	0.00%	0.00%	48.40%	86.60%	100.00%	51.60%
1986	13.40%	0.00%	0.00%	0.00%	0.00%	0.00%	0.00%	0.00%	47.10%	84.30%	100.00%	52.90%
1987	15.70%	0.00%	0.00%	0.00%	0.00%	0.00%	0.00%	0.00%	47.70%	84.90%	100.00%	52.30%
1988	15.10%	0.00%	0.00%	0.00%	0.00%	0.00%	0.00%	0.00%	38.80%	76.80%	100.00%	61.20%
First trimester												
1982	0.00%	0.00%	0.00%	0.00%	0.00%	0.00%	6.40%	15.80%	25.20%	28.20%	28.20%	28.20%
1983	28.20%	28.20%	28.20%	28.20%	28.20%	28.20%	18.80%	0.00%	0.00%	0.00%	24.60%	78.50%
1984	100.00%	75.40%	21.50%	0.00%	0.00%	0.00%	0.00%	0.00%	0.00%	0.00%	0.00%	41.30%
1985	92.50%	100.00%	58.70%	7.50%	0.00%	0.00%	0.00%	0.00%	0.00%	0.00%	0.00%	48.40%
1986	86.60%	100.00%	51.60%	13.40%	0.00%	0.00%	0.00%	0.00%	0.00%	0.00%	0.00%	47.10%
1987	84.30%	100.00%	52.90%	15.70%	0.00%	0.00%	0.00%	0.00%	0.00%	0.00%	0.00%	47.70%
1988	84.90%	100.00%	52.30%	15.10%	0.00%	0.00%	0.00%	0.00%	0.00%	0.00%	0.00%	38.80%
Before pregnancy												
1982		0.00%	0.00%	0.00%	0.00%	0.00%	0.00%	0.00%	0.00%	0.00%	6.40%	15.80%
1983	25.20%	28.20%	28.20%	28.20%	28.20%	28.20%	28.20%	28.20%	28.20%	18.80%	9.40%	0.00%
1984	0.00%	24.60%	78.50%	100.00%	75.40%	21.50%	0.00%	0.00%	0.00%	0.00%	0.00%	0.00%
1985	0.00%	0.00%	41.30%	92.50%	100.00%	58.70%	7.50%	0.00%	0.00%	0.00%	0.00%	0.00%
1986	0.00%	0.00%	48.40%	86.60%	100.00%	51.60%	13.40%	0.00%	0.00%	0.00%	0.00%	0.00%
1987	0.00%	0.00%	47.10%	84.30%	100.00%	52.90%	15.70%	0.00%	0.00%	0.00%	0.00%	0.00%
1988	0.00%	0.00%	47.70%	84.90%	100.00%	52.30%	15.10%	0.00%	0.00%	0.00%	0.00%	0.00%

Note: Proportion of mothers who received dividend payments during each pregnancy stage. For example, 41.3% of mothers of babies born in February 1985 received the dividend during their second trimester, as they were in their second trimester when the October 1984 payment of 41.3% was distributed (see Table A3).

Table A5: The Timing of APFD on Birth Outcomes and Mechanisms

	(1) Birth weight	(2) Low birth weight	(3) Premature births	(4) Fetal growth	(5) Care began	(6) # prenatal care visits
Panel A: Effects Relative to Third Trimester						
Pre-pregnancy	27.841***	-0.004*	-0.005*	0.630***	-0.010	0.050
p-value	0.000	0.085	0.092	0.000	0.730	0.270
95% CI	[14.780, 40.310]	[-0.008, 0.001]	[-0.010, 0.000]	[0.359, 0.964]	[-0.072, 0.049]	[-0.053, 0.169]
First trimester	19.434**	0.000	-0.005	0.473**	0.010	-0.115*
p-value	0.042	0.895	0.270	0.020	0.766	0.064
95% CI	[-2.010, 33.608]	[-0.005, 0.008]	[-0.013, 0.002]	[0.150, 0.835]	[-0.037, 0.057]	[-0.235, -0.016]
Second trimester	1.610	-0.000	-0.008**	-0.125	0.061*	-0.179**
p-value	0.841	1.000	0.040	0.433	0.058	0.015
95% CI	[-12.880, 15.697]	[-0.004, 0.005]	[-0.015, 0.001]	[-0.372, 0.268]	[-0.008, 0.108]	[-0.301, -0.056]
Panel B: Marginal Effects by Trimester						
Pre-pregnancy	50.209***	-0.009**	-0.003	0.896**	-0.086	0.604***
p-value	0.000	0.020	0.549	0.023	0.202	0.000
95% CI	[25.047, 65.790]	[-0.014, -0.005]	[-0.011, 0.005]	[0.357, 1.444]	[-0.248, 0.217]	[0.237, 0.916]
First trimester	41.802**	-0.005**	-0.004	0.739**	-0.067	0.439**
p-value	0.020	0.034	0.461	0.043	0.317	0.024
95% CI	[21.728, 58.013]	[-0.010, -0.001]	[-0.015, 0.005]	[0.237, 1.315]	[-0.198, 0.244]	[0.131, 0.803]
Second trimester	23.978**	-0.006*	-0.006	0.141	-0.015	0.375*
p-value	0.020	0.093	0.175	0.475	0.799	0.056
95% CI	[8.062, 37.871]	[-0.011, 0.001]	[-0.015, 0.004]	[-0.356, 0.645]	[-0.185, 0.284]	[0.007, 0.654]
Third trimester	22.368**	-0.005**	0.002	0.266	-0.076	0.554***
p-value	0.022	0.020	0.712	0.153	0.289	0.000
95% CI	[6.948, 35.971]	[-0.010, -0.002]	[-0.006, 0.009]	[-0.094, 0.684]	[-0.177, 0.213]	[0.252, 0.854]
Mean	3450.850	0.045	0.070	87.072	2.901	10.102
N	6480	6480	6480	6480	5400	5400

Notes: All regressions include control variables, state and year-month fixed effects, and are weighted by SDID unit and time weights ($\hat{\omega}^{\text{sdid}}$, $\hat{\lambda}^{\text{sdid}}$). Sample period: 1976-1988 for birth and gestation outcomes; 1978-1988 for prenatal care (due to data availability). Panel A shows effects relative to third-trimester receipt. Panel B shows marginal effects for each trimester. Control variables: maternal education, age, race, parity, infant sex, and lagged unemployment rate. Inference based on permutation tests (1,000 iterations). * p<0.10, ** p<0.05, *** p<0.01.

Table A6: The Effect of the APFD on Birth Outcomes by Education

	(1) Birth weight	(2) Low birth weight	(3) Premature births	(4) Fetal growth	(5) Care began	(6) # prenatal care visits
Panel A: Effects Relative to College-Educated Mothers						
$< Grade12$	34.638**	-0.007**	-0.011	0.902***	-0.147*	0.792***
p-value	0.020	0.043	0.120	0.000	0.093	0.000
95% CI	[4.101, 59.018]	[-0.013, -0.001]	[-0.027, 0.005]	[0.211, 1.609]	[-0.281, 0.047]	[0.331, 1.199]
$= Grade12$	22.441	-0.007**	-0.003	0.784***	-0.074	0.420**
p-value	0.102	0.045	0.686	0.000	0.170	0.046
95% CI	[0.183, 51.311]	[-0.012, -0.001]	[-0.011, 0.006]	[0.283, 1.440]	[-0.208, 0.029]	[0.094, 0.690]
$Grade13-15$	19.521*	-0.001	-0.002	0.730**	-0.028	0.116
p-value	0.073	0.559	0.601	0.026	0.439	0.278
95% CI	[-0.051, 58.675]	[-0.007, 0.003]	[-0.011, 0.014]	[0.303, 1.652]	[-0.144, 0.026]	[-0.098, 0.283]
Panel B: Marginal Effects by Education Group						
$< Grade12$	33.607**	-0.008*	-0.012*	0.758**	-0.140	0.926***
p-value	0.019	0.062	0.093	0.019	0.177	0.000
95% CI	[12.579, 52.614]	[-0.017, -0.003]	[-0.028, 0.001]	[0.257, 1.321]	[-0.313, 0.126]	[0.339, 1.520]
$= Grade12$	21.410**	-0.007**	-0.004	0.641***	-0.067	0.554**
p-value	0.019	0.019	0.345	0.000	0.284	0.022
95% CI	[7.012, 44.054]	[-0.014, -0.004]	[-0.010, 0.003]	[0.245, 1.157]	[-0.209, 0.144]	[0.192, 0.955]
$Grade13-15$	18.490*	-0.002	-0.003	0.587*	-0.021	0.250
p-value	0.081	0.352	0.477	0.066	0.704	0.181
95% CI	[-9.318, 35.134]	[-0.009, 0.002]	[-0.013, 0.010]	[-0.125, 1.169]	[-0.149, 0.108]	[-0.087, 0.585]
$>= Grade16$	-1.031	-0.001	-0.001	-0.144	0.007	0.134
p-value	0.927	0.830	0.799	0.515	0.955	0.454
95% CI	[-35.271, 21.872]	[-0.005, 0.004]	[-0.011, 0.007]	[-0.954, 0.322]	[-0.112, 0.117]	[-0.295, 0.414]
Mean	3450.850	0.045	0.070	87.072	2.901	10.102
N	2160	2160	2160	2160	1800	1800

Notes: All regressions include control variables (pre-processing), state and year fixed effects, and are weighted by SDID unit and time weights($\hat{\omega}^{\text{sdid}}$, $\hat{\lambda}^{\text{sdid}}$). Sample period: 1976-1988 for birth and gestation outcomes; 1978-1988 for prenatal care (due to data availability). Panel A shows effects relative to college-educated mothers ($\geq$Grade 16). Panel B shows marginal effects for each education group. Control variables: maternal education, age, race, parity, infant sex, and lagged unemployment rate. Inference based on permutation tests (1,000 iterations). * p<0.10, ** p<0.05, *** p<0.01

Table A7: The Effect of APFD on Fertility and Mortality Rates (per 1000)

	(1)	(2)	(3)
	Fertility	Neonatal mortality	Perinatal-related neonatal mortality
Treated	11.440**	0.016	-0.141
p-value	0.013	1.000	0.794
95% CI	[4.927, 15.735]	[-2.530, 1.311]	[-1.402, 0.837]
Mean	86.024	13.802	5.681
N	540	540	540

Notes: Data on the female population are from Survey of Epidemiology and End Results, and mortality data are from Multiple Cause-of-Death Mortality Data. Neonatal mortality refers to deaths within the first 28 days of life. All regressions include control variables (pre-processing), state and year fixed effects, and are weighted by SDID unit and time weights ($\hat{\omega}^{\text{sdid}}$, $\hat{\lambda}^{\text{sdid}}$). Sample period: 1976–1988. Control variables: maternal education, age, race, parity, and infant sex, and the lagged unemployment rate. Inference based on permutation tests (1,000 iterations).
* p<0.10, ** p<0.05, *** p<0.01

Table A8: The Effect of Dividend on Medical Risks

	(1) Hydramnios	(2) Pregnancy-associated Hypertension	(3) Eclampsia	(4) Incompetent Cervix	(5) Uterine Bleeding
Amount	-0.001	-0.006**	0.000	-0.000	-0.000
p-value	0.654	0.022	0.510	0.860	0.759
95% CI	[-0.010, 0.005]	[-0.010, -0.000]	[-0.002, 0.002]	[-0.001, 0.001]	[-0.002, 0.004]
Mean	0.008	0.040	0.005	0.003	0.020
	(6) Rh Sensitization	(7) Anemia	(8) Cardiac Disease	(9) Lung Disease	(10) Diabetes
Amount	-0.002	-0.011***	-0.001	0.001	0.003
p-value	0.148	0.000	0.567	0.526	0.267
95% CI	[-0.006, 0.001]	[-0.020, -0.001]	[-0.002, 0.003]	[-0.005, 0.012]	[-0.004, 0.007]
Mean	0.007	0.055	0.003	0.002	0.028
	(11) Hemoglobinopathy	(12) Chronic Hypertension	(13) Renal Disease	(14) Genital Herpes	(15) Other Medical Risk Factors
Amount	0.000	0.000	-0.001	0.001	-0.034*
p-value	0.535	0.908	0.450	0.489	0.067
95% CI	[-0.001, 0.001]	[-0.003, 0.002]	[-0.005, 0.003]	[-0.001, 0.004]	[-0.055, 0.013]
Mean	0.000	0.007	0.001	0.023	0.092
N	672	672	672	672	672

Notes: All regressions include control variables, state, and year fixed effect, and are weighted by the unit and time weights ($\hat{\omega}^{\text{sdid}}$, $\hat{\lambda}^{\text{sdid}}$) obtained from the birth weight outcome. Sample period: 1989–2004 (due to data availability). We estimate effects per $1,000 (2024 USD) using weights from baseline analysis. Control variables: maternal education, age, race, parity, infant sex, and lagged unemployment rate. Inference based on permutation tests (1,000 iterations). Since there are no pre-intervention periods, the mean is for 1989.
* p<0.10, ** p<0.05, *** p<0.01