The Impact of Diabetes Insurance Mandates on Infant Health

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Abstract

Among the factors thought to contribute to lagging improvements in infant health in recent years are increasing obesity and diabetes prevalence among women of childbearing age. This paper uses a difference-in-difference-in-difference empirical strategy to investigate the impact of mandated insurance coverage for diabetes on adverse pregnancy outcomes. Among educated women, who have high rates of coverage through private insurance that is subject to insurance mandates, diabetes mandates are associated with a reduction in low birth weight and premature birth prevalence. These gains are concentrated among older women and are larger for African-Americans. There is a weaker effect on the prevalence of high birth weight, potentially because of the deleterious effects of an increased probability of pregnancy weight gain in excess of 35 pounds among diabetic women in states with mandates.

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1. INTRODUCTION

Current statistics indicate that the United States ranks 31st in male life expectancy and is falling even further behind other countries with respect to that of women (with the U.S. ranking 35th) (Salomon et al. 2012). Furthermore, between 1990 and 2005 infant health, as proxied by preterm birth and low birth weight prevalence, worsened in the United States (National Center for Health Statistics, NCHS). One explanation for these trends focuses on changes in maternal characteristics, such as diabetes prevalence, because uncontrolled diabetes during pregnancy is associated with a higher incidence of high birth weight (>4000 grams), prematurity (<37 weeks gestation), and low birth weight (<2500 grams). The incidence of diabetes among women of childbearing age doubled between 1980 and 2009, to reach approximately 4%.[1] This mirrors a broader trend of increased diabetes prevalence in the total population.

The high burden on individuals with diabetes, the rapid increase in disease incidence, as well as the potential for diabetes to affect significantly the health of future generations raise the question of whether the health insurance system can effectively address this issue. We investigate this question by analyzing state experimentation with legislation that expanded diabetes insurance coverage in the form of benefits mandates.

Previous research suggests that diabetes mandates are associated with higher obesity rates within the target population, people with diabetes (Klick and Stratmann 2007). In addition, because of its high medical and future costs poor infant health represents a significant component of the impact of maternal diabetes and, thus, diabetes insurance. In this paper, we use 1992-2003 data from the NCHS Vital Statistics Natality Data to evaluate the population impact of diabetes mandates on infant health. Thus, this paper contributes to the literature indicating that

[1] Source: Centers for Disease Control and Prevention, NCHS, Division of Health Interview Statistics, data from the National Health Interview Survey. http://www.cdc.gov/diabetes/statistics/incidence/fig5.htm retrieved 7/28/2011.

concentrating on a policy's target population may underestimate/overestimate its effect when third parties are affected. It also provides the first estimates of diabetes insurance mandates on measures of overall health.

Our econometric approach builds on triple-difference (DDD) procedures commonly used in policy-evaluation literature (Meyer 1995; Gruber 1994a; Bertrand, Duflo, and Mullainathan 2004). We compare changes in the outcomes of infants born to diabetic mothers versus those of infants born to non-diabetic mothers in states that enacted diabetes mandates pre- to post-adoption with changes in the same groups and time periods in states that did not enact such mandates. Our results suggest that diabetes mandates significantly decrease the prevalence of low birth weight and prematurity but do not produce large changes in high birth weight prevalence among births to women with more than 12 years of education, who are more likely to be covered by the type of private insurance subject to mandates. These effects are plausibly larger where more women are covered through private, fully insured health plans and are smaller and insignificant in the case of births to women with 12 or fewer years of education or to non-diabetic women.

Our results contribute to the literature documenting the impact of mandated insurance benefits and provide some insight into the possible effects of the federal health reform. Surprisingly, although, as shown in the literature review below, numerous studies document the effect of insurance mandates on utilization, the literature on the effect on health outcomes is sparse (Klick and Stratmann 2007).[2] This might be because of the potentially long lag between the adoption of mandates and measurable changes in health outcomes. By concentrating on the most fragile individuals, infants, we are able to document that diabetes insurance mandates

[2] Bitler and Carpenter (2012a) also find that mammography mandates are associated with earlier detection, which presumably lowers mortality if the rate of false positives does not increase, but the effect on health is not documented.

significantly improve health for some categories of the population. This is important to document because when insurance coverage allows for better disease management, but also generates incentives for individuals to substitute away from preventive measures, such as healthy eating and exercise, an increase in utilization may not necessarily lead to significant changes in health.

In addition, because the effect of mandates on birth outcomes is driven by the effect on maternal health, our results suggest that birth outcomes may be a good way to identify changes in the health of women of fertile age, when alternative direct measurements of their health are not available, unreliable, or too noisy.

2. BACKGROUND

DIABETES AND PREGNANCY OUTCOMES

During pregnancy, women tend to develop hypoglycemia (plasma glucose mean = 65-75 mg/dL) between meals and during sleep. If the maternal insulin response is inadequate, hyperglycemia results and the mother experiences recurrent postprandial hyperglycemic episodes. These episodes are a significant source of accelerated growth of the fetus, resulting in macrosomia, i.e., high birth weight. Numerous studies find that poor diabetes control during pregnancy is associated with macrosomia (e.g., Jovanovic-Peterson et al. 1991; Ostlund et al. 2003, among many others). In addition, maternal obesity, which is common among people with type 2 diabetes, has a strong effect on fetal macrosomia (Ehrenberg, Mercer, and Catalano 2004; Owens et al. 2010).

Although most fetuses of diabetic mothers exhibit growth acceleration, growth restriction is also common in pregnancies of women with preexisting diabetes. This effect is explained by the underlying maternal vascular disease associated with diabetes. Previous literature finds that maternal diabetes is correlated with low birth weight (McDonald et al. 2010; Rosenberg et al. 2005) and preterm labor (Hedderson, Ferrara, and Sacks 2003). These effects are again compounded by obesity, as researchers report a positive correlation between obesity and both preterm and low weight births (Chen et al. 2009; Naeye 1990).

COSTS OF ABNORMAL BIRTH-WEIGHT BABIES

Numerous papers have investigated the impact of poor birth outcomes, of which we discuss just a few here. Low birth weight and prematurity are associated with higher medical costs (Gilbert, Nesbitt, and Danielson 2003), lower education (Corman and Chaikind 1998), and poorer long-term health (Paneth 1995) and economic outcomes (Behrman and Butler 2007; Behrman and Rosenzweig 2004; Black, Devereux, and Salvanes 2007; Currie and Hyson 1998). The National Academy of Sciences estimates the total cost of preterm births in 2005 as "at least $26.2 billion or $51,600 per infant." Lewit et al. (1995) estimate the cost of low birth weight in 1988 as being between $5.4 and $6 billion. Although these might be overestimates (Almond, Chay, and Lee 2005), overall, the evidence suggests that low birth weight and prematurity are quite costly.

High birth weight is also potentially very costly for society because it represents a risk factor for child obesity (Danielzik et al. 2004), diabetes (Harder et al. 2007), cancer (Harder, Plagemann, and Harder 2008; Hjalgrim et al. 2003), and other deleterious health conditions. There is also some evidence that high birth weight is associated with poorer cognitive function (Richards et al. 2001), reading difficulties (Kirkegaard et al. 2006), and lower test scores (Cesur and Rashad 2010). Thus, the cost of diabetes to third parties could be substantial and the benefit from better control of diabetes significant.

3. INSURANCE MANDATES

Like any insurance problem, there are two possible opposing effects of providing diabetes coverage. Because people can get medical treatment much more cheaply than before, they are more likely to use this treatment and thus become healthier. At the same time, the provision of health insurance has the potential to create moral hazard problems (Pauly 1974; Zweifel and Manning 2000), causing some people to move from non-medical solutions, such as healthy lifestyles, to medical solutions (Kahn 1999). This occurs when the price of insurance is not adjusted for spending on non-medical disease management solutions, or else insurance could in fact encourage better self-protection (Ehrlich and Becker 1972). Should moral hazard occur, some or even all the gains from medication would be offset.

The question of whether gaining access to insurance can improve the health is, thus, not trivial, especially if there are spillovers to third parties. The literature indicates that using medical devices to manage diabetes is sensitive to insurance status. Goldman et al. (2004) find that doubling of copayments reduced the use of antidiabetes medications by patients with diabetes by 23 percent. Evidence from the Oregon Health Insurance Experiment also indicates that insurance is associated with a 15 percent increase in the probability of having one's blood tested for high blood sugar or diabetes (Finkelstein et al. 2012). On the other hand, the same data indicates that two years after being randomly assigned to Medicaid coverage, recipients with diabetes did not fare better than the control group of uninsured (Baicker et al. 2013). This could be because the time investigated was too short to observe measurable changes in overall health. Of course, the challenge to the empirical investigation is to assign causality, which becomes increasingly difficult as the lag between changes in environment and health measurements increases. In this paper, we concentrate on one of the most fragile categories of the population, newborns, which are known to respond significantly to a relatively short period of exposure: changes in the in utero environment.

The source of variation in insurance coverage used in this paper comes from legislative changes in mandated coverage provided by private insurance companies. Diabetes mandates require private insurance companies to provide coverage for medication, equipment, supplies, and sometimes education for at-home treatment. Noting that some states do not define what each of the above terms means, medication usually implies coverage of insulin, glucagon, and also other prescriptive medication, while equipment and supplies could include coverage of insulin syringes, blood glucose monitors, insulin infusion devices, podiatric appliances to prevent complications associated with diabetes, visual reading strips, urine test strips, lancets and lancet devices, and injection aids. Coverage sometimes includes outpatient self-management training, and education and medical nutrition therapy.

The extent to which these mandates are expected to have an impact depends on the institutional contexts in which they were adopted. First, only a subset of women who give birth is likely to be affected by mandates, because state mandates apply only to individuals who have private insurance. In addition, the Employment Retirement Income Security Act (ERISA) limits the role of state mandates. State health insurance laws apply only to the fully insured plans in which mostly individuals and small- and medium-sized businesses and organizations are enrolled. In the empirical analysis, we concentrate on a population subgroup with higher rates of private insurance and subsequently inflate the coefficients by the estimated treatment rates (i.e., the estimated proportion of women in our investigated demographic group having the type of insurance subject to diabetes mandates) to obtain the impact among those subject to mandates.

Second, mandated-benefits laws might cause some employers to reduce offers of health insurance, leading to a decrease in coverage. The empirical evidence to support this claim, however, is mixed (Gruber 1994b; Jensen and Gabel 1992; Sloan and Conover 1998). Diabetes mandates, in particular, are likely to produce only small changes in the cost of insurance. According to a Utah study, this amounts to 2 dollars per year per policyholder, while a Louisiana study finds it amounts to 0.006% of the total cost paid by insurers.[3] These increases are likely too low to lead to significant changes in insurance coverage. In addition, a significant body of literature indicates that various types of mandated benefits lead to increased utilization rates: Mandated benefits are shown to increase utilization rates of diabetes treatment (Li et al. 2010), infertility treatment (Bitler and Schmidt 2012; Schmidt 2007), mental health care (Busch and Barry 2008; Harris, Carpenter, and Bao 2006; Pacula and Sturm 2000), mammography (Bitler and Carpenter 2012a), and Pap tests (Bitler and Carpenter 2012b).

Third, some policies were already covering diabetes. To our knowledge, there are no direct estimates of the effect of diabetes mandates on the coverage of the diabetic population. Pollitz et al. (2005), however, mention several state reports indicating that mandates increase coverage. In addition, existing empirical evidence is consistent with an increase in coverage. Diabetes mandates are associated with higher utilization rates of diabetes management devices (Li et al. 2010), an indication that not all existing private health insurance plans had been including diabetes coverage. The literature also records behavioral changes, such as an increase in obesity rates among people with diabetes, in response to diabetes mandates (Klick and Stratmann 2007).

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[3] "2003 Diabetes Mandate Report" issued by the Utah Insurance Department (October 28, 2003); Louisiana Department of Insurance's "A Study of the Costs Associated with Healthcare Benefits Mandated in Louisiana" issued on 28 February 2003.

Finally, although most state mandates require that all private insurance plans sold include diabetes coverage (a mandate to cover), a minority of state mandates require only that insurers *offer* coverage (i.e. make available for purchase a policy that covers diabetes treatment), but do not require all insurance policies to include diabetes coverage (a mandate to offer). The type of mandate may affect the magnitude of impact.

4. DATA

We use the 1992-2003 NCHS Vital Statistics Detail Natality Data on birth-outcomes: prevalence of low birth weight (<2500 grams or <1500 grams), high birth weight (>4000 grams or >4500 grams), and prematurity (<37 weeks or <32 weeks). Although they do not capture all specific aspects of infant health, these are the primary measures of a newborn's health in most economic research, because they are good indicators of overall health and, as shown in section 2, good predictors of future outcomes. Our data cover all births in the 50 U.S. states and all but one instances of diabetes mandates adoptions (Table 1).[4] Our analysis does not cover Wisconsin's 1988 adoption of diabetes mandates, because the data on mother's diabetic status were not reported in the NCHS Natality Data. As such, the sample in this study is representative of the country as a whole.

Natality data provide information about infant characteristics, such as gender, plurality (single versus higher-order birth), parity (whether it was a first or subsequent birth), and mothers' demographic characteristics, such as age, race, education, marital status, and state of residence. Natality data also provide information about the mother's diabetic status.[5] Although

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[4] If the effective date of the reform was on or after July 1st, the law was coded as belonging to the year after because the law did not apply for most of the year and thus could not affect most births in that year.
[5] The diabetes information is quite comprehensive: There are between 0.5 and 3% missing values in each year investigated. Observations with missing diabetes status were dropped.

pregestational diabetes has more severe consequences for infant outcomes than gestational diabetes, we cannot distinguish between type 1 diabetes, type 2 diabetes, and gestational diabetes in these data. To investigate the differential effects of diabetes mandates on women with pregestational diabetes versus gestational diabetes, we use the Behavioral Risk Factor Surveillance System (BRFSS) to estimate the prevalence of pregestational versus gestational diabetes among women of childbearing age.

Diabetes mandates apply only to private insurance plans, more exactly only to the fully insured plans in which mostly individuals and small- and medium-sized businesses and organizations are enrolled. Because regressions that impose the constraint of an equal effect for the entire population may conclude that the policy had little or no effect (Bitler and Schmidt 2012), we focus on a subsample where mandates are more likely to be binding. The Current Population Survey (CPS) March Supplement data indicate that during the period investigated, 1992-2003, only 57% of women of childbearing age (18-45 years old) with a high-school education or less were covered through private insurance. This is significantly lower than the 81% of the women with more than a high-school diploma covered by private insurance. Among these women, only some are covered by fully insured plans. Using Kaiser/HRET Employer Health Benefits Annual Survey data on the percentage of insured workers in self-insured plans by firm size and the March CPS data regarding the share of 18-45 year-old workers with more than 12 years education by firm size, we find that approximately 45% of women of childbearing age with more than a high-school education were in fully insured plans and, thus, would have had the private insurance subject to this regulation. Using Medical Expenditure Panel Survey (MEPS) data, we find a slightly higher estimate, approximately 46%. Similar calculations indicate that between 34% (HRET estimate) and 35% (MEPS estimate) of women of childbearing age with a high-school education or less would have been subject to this regulation.[6]

As such, diabetes mandates are more likely to affect women with more than a high-school education because a significantly larger proportion will experience a change in insurance coverage. We focus our analysis on this sample. Results obtained using the sample of infants born to mothers with a high-school education or less are discussed in the text when relevant.

The caveat is that these results may not be generalizable to women with less education even if they are privately insured, because education may be a good predictor of individuals' discount rates (Fuchs 1982) and, thus, of their propensity to invest in health. Education may also be correlated with individuals' propensity to use medical care, or it may simply affect the allocation of health inputs (Grossman 2006). Despite these limitations, our study represents an important step in understanding the costs and benefits of insurance mandates.

Summary statistics for the main sample used for our analysis are reported in Table 2. The means and standard errors of the variables are shown for all births and separately for births to diabetic mothers only.[7] These means were calculated for the state-years with no mandates, separated by treatment status in the following year: no diabetes mandate in columns 1 and 4, and diabetes mandates adoption in columns 2 and 5. Columns 3 and 6 report the results of t-tests for the equality of means. Because infant outcomes exhibit time trends and the means calculated in columns 2 and 5 tend to use later data than the means in columns 1 and 4, we report the t-test of equality of means conditional on time fixed effects. Adopting and non-adopting states appear to be virtually identical with respect to the variables characterizing the environment and outcomes of infants born prior to mandate adoptions. Similarly, there is no statistically significant

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[6] More details about these calculations are available in the online Appendix.
[7] The summary statistics for non-diabetic mothers are reported in the online Appendix Table A2.

difference between the characteristics of infants born to diabetic mothers in experimental versus nonexperimental states, suggesting that mandate adoption is not correlated with a state demographic structure prior to adoption.

5. EMPIRICAL STRATEGY

The goal of the empirical analysis is to identify the causal effect of diabetes insurance on the prevalence of adverse birth outcomes. We investigate this question using variation in diabetes insurance coverage resulting from legislative changes in mandated insurance benefits (Table 1) and quasi-experimental methods. A useful approach would be to compare the change before/after in pregnancy outcomes of women with diabetes in diabetes mandate states with the changes in pregnancy outcomes of women with diabetes in all other states. Such a difference-in-difference (DD) estimate of the impact of mandates on the birth outcomes of the target group, however, does not take into account time-varying factors correlated with the timing of the adoption of mandates that differentially affect birth outcomes in mandate versus non-mandate states.

To account for such factors, we use a second source of variation owed to the fact that diabetes mandates target people with diabetes and provide no benefits to those not suffering from diabetes. Since previous research (Klick and Stratmann 2007) confirms that diabetes mandates do not lead to behavioral changes among people that do not suffer from diabetes, the changes in pregnancy outcomes of women without diabetes in mandate states relative to non-mandate states provide an estimate of the magnitude of unobserved factors differentially affecting the birth outcomes in mandate states over this period. If we subtract the DD estimate of the mandates' effect on birth outcomes for non-diabetic women from the DD estimate for women with diabetes, we obtain the difference-in-difference-in-difference (DDD) estimate of the impact of diabetes mandates on the birth outcomes of women with diabetes. This DDD estimate, thus, accounts for general trends in birth outcomes of women with diabetes and for trends specifically affecting women in mandate states.

Because of the very large sample size and the fact that the relevant legislative variable varies only at state/year level, the data are collapsed into state/year/mother diabetic status cells. The data are also divided by mother's age (under 25, 25 to 29, 30 to 34, and 35 and over) and race (Black and White), because trends in diabetes incidence vary by age group (Lawrence et al., 2008) and because African-American women with diabetes tend to have different rates of diabetic complications and are more likely to have low birth weight babies than are Caucasian women (Nicholson et al. 2006).[8] Thus, all observations are averages for the state/year/mother's age/mother's race/mother's diabetes, and we estimate the following regression, weighted by cell size:

$$(1)\ \ Y_{gdst} = \beta_0 + \beta_1 X_{gdst} + \beta_2 S_s + \beta_3 T_t * G_g + \beta_4 D_d + \beta_5 S_s * D_d + \beta_6 T_t * D_d + \beta_7 S_s * T_t + \\ + \beta_8 (Mandate_{s,t-1} * D_d) + \varepsilon_{gdst},$$

where Y_{gdst} are various birth outcomes for women in the age/race demographic group g, of diabetic status d, in state s at time t. X_{gdst} is a vector of time-varying controls, such as female infant, plural birth, first child, mother's education, marital status, and prenatal care. S_s is a vector of state dummies, which control for differences in birth outcomes that are common to people in the same state (for instance, secular differences in the overall level of health resulting from unmeasured cultural factors, such as cuisine specificity or weather). T_t is a vector of year dummies, which captures any time-varying differences in health common to all infants, such as changes in federal-level health-care policies. Because there are different trends in infant

[8] In addition, similar lifestyle choices may or may not have different impacts on birth outcomes by mother's age (Walker, Tekin, and Wallace 2009).

outcomes by mother's age and race, we allow the time effects to vary by age-race demographic groups, G_g. D_d is a dummy for the treatment group (1 if mother was diabetic, 0 otherwise).

The interactions between the time effects and the mother's diabetic status, $T_t *D_d$, account for differential changes over time in the health status of infants born to diabetic mothers (such as those resulting from changes in diabetes management technologies). Interactions between state effects and mother's diabetic status, $S_s *D_d$, are included to control for systematic differences in the outcomes of infants born to diabetic mothers across states. The equation includes state-by-year fixed effects, $S_s *T_t$, that control for differential changes over time in states that adopted mandates. Because practically all means-tested programs (such as Medicaid) are administered at the state level, the state-by-year fixed effects absorb the impact of such programs on infant outcomes and thus any source of bias from the introduction or expansion of such programs.

The variable *Mandate* is a dummy equal to 1 in all state-years with an effective diabetes mandate, and zero otherwise. The subscript, *t-1*, acknowledges the potential lag effect of the mandate because of the lag between exposure and outcome.[9] In this framework β_8, the coefficient of the interaction between diabetic status and being subject to diabetes mandates $(Mandate_{s,t-1} *D_d)$ captures the variation in health specific to infants of diabetic mothers (relative to non-diabetic mothers) in states with diabetes mandates (relative to states without such mandates) in the years after the law (relative to before the law).

In our estimation approach, the unit of observation is more detailed than the level of variation of the variable of interest. Also, the error terms are likely to be correlated within each state over time. To correct for these potential problems, this paper reports robust standard errors

[9] Alternative specifications are available in the online Appendix.

clustered at the state level, a method that allows for an arbitrary autocorrelation process (Bertrand, Duflo, and Mullainathan 2004).

In this framework, we can identify the causal effect of diabetes mandates, provided that the birth outcomes of women without diabetes do not change because of mandates. This is plausible because women without diabetes do not benefit directly from mandates. A positive impact, if any, would be driven by improved access to education about preventive behaviors. To the extent that non-diabetic women benefit from mandates, the DDD will underestimate the true effect of mandates. On the other hand, the DDD will overestimate the true effect if mandates worsen pregnancy outcomes of non-diabetic women. This might happen if mandates resulted in premium changes that affected the decision to obtain coverage or if they reduced incentives to engage in preventive behaviors. As mentioned in section 2, premium changes are likely to be small, and previous literature shows no evidence of moral hazard among people that do not suffer from diabetes (Klick and Stratmann 2007). In the Results section we formally test these assumptions.

Another issue regarding the estimation strategy is that only a subset of women who give birth are likely to be affected by mandates. As explained in the Data section, state mandates apply only to individuals enrolled in fully funded private insurance plans. The coefficients estimated from the specification above are valid estimates of the population impact of diabetes mandates. To determine the impact for those subject to mandates (effect of treatment on the treated), we inflate the coefficients by the estimated treatment rates (i.e., the estimated proportion of women in our investigated demographic group having the type of insurance subject to diabetes mandates). In addition, in the empirical analysis, we test the robustness of the results by exploiting this information about the likelihood of treatment.

6. RESULTS

TESTS FOR THE VALIDITY OF IDENTIFYING ASSUMPTIONS.

First, Figure 1 showing pre- and post-diabetes mandate trends in the incidence of high birth weight and low birth weight separately for diabetic mothers and non-diabetic mothers indicates significant changes in the birth outcomes of women with diabetes. The trends in birth outcomes of non-diabetics before mandates are indistinguishable from the trends after mandates, suggesting that the identification assumption of no changes among women without diabetes is plausible. In addition, there is no indication of a transitory pre-treatment increase in the tails of the distribution, the equivalent of an "Ashenfelter dip" for this case (Ashenfelter 1978), which would suggest that the estimates indicate just mean reversion and thus are falsely attributed to diabetes mandates. At the same time, however, the graphs are consistent with an upward trend in low birth weight and prematurity and a downward trend in high birth weight that may vary by state, highlighting the need to control for state-specific time trends.

Second, we find no evidence of an increase in births to diabetic mothers, suggesting there was no change in the number of diabetic women and thus no change in the definition of the treatment and control groups over time. The estimated association between mandate adoption and the percentage of births to women with diabetes is -0.005 with a p-value of 0.102, and the lag effect is -0.019 with a p-value of 0.113 and thus highly insignificant. This is plausible, because the effect is identified from year-to-year variation and one year is too short a time to develop diabetes.

Third, we test directly whether diabetes mandates affected the birth outcomes of women without diabetes. Reassuringly, using a DD estimation strategy, we find that there is no statistically significant effect on infants born to non-diabetic mothers (Table 3, Panel B), and the lack of an effect is not driven by large standard errors. The estimated coefficients imply very small changes. For instance, our DD estimates imply a ~0.7% decrease in low birth weight (<2500 grams) prevalence and a ~1.6% decrease in prematurity (<32 weeks) (calculated at the mean of the data) among births to women without diabetes. By comparison, the effect of mandates on births to women with diabetes is 6 times larger, ~4.5%, in the case of low birth weight and 9 times larger, ~15%, in the case of prematurity. In addition, the signs of all coefficients obtained for the sample of births to women without diabetes match the signs of the implied effect of mandates on births to women with diabetes. Thus, if mandates have any effect on women without diabetes, our DDD approach would underestimate the true effect of diabetes mandates.

MAIN RESULTS

The results reported in Table 4 suggest that diabetes mandates are associated with a statistically significant 0.34 percentage points, the equivalent of a 4.1-percent decrease (from the mean) in low birth weight (<2500 grams) prevalence among births to women with diabetes, and a 0.19 percentage points reduction, the equivalent of an 14.7-percent reduction of the prevalence of very low birth weight (<1500 grams) among births to women with diabetes. The adoption of diabetes mandates also leads to a 0.22 percentage points decrease, the equivalent of a 12.3-percent decrease, in prematurity (<32 weeks) prevalence among births to women with diabetes. These are estimates of the population impact of diabetes mandates. After inflating the estimated effect by the share of the population investigated subject to mandates, we find that diabetes mandates reduce low birth weight prevalence by 0.73-0.75 percentage points (or approximately 8.9-9.2 percent), very low birth weight prevalence by 0.41-0.42 percentage points (or approximately 31.7-32.7 percent), and prematurity by 0.48-0.49 percentage points (or approximately 26.7-27.5 percent) among diabetic women subject to diabetes mandates.[10]

In contrast, we find no evidence of a statistically significant effect on high birth weight. If anything, it appears that diabetes mandates are positively correlated with macrosomia prevalence. One potential explanation relies on possible heterogeneity of the effect by demographic group when some demographic groups are more likely to have low birth weight or high birth weight babies. Another explanation is that the impact of moral hazard identified by Klick and Stratmann (2007), which increased obesity prevalence among people with diabetes, offsets the effect. In support of this hypothesis, previous research indicates that maternal body-mass index (BMI) has a greater effect on the incidence of large-for-gestational-age (LGA) births than glucose control (Wong et al. 2002). We find evidence in support of both theories.

Because cutting the data to investigate the impact of diabetes mandates by demographic group reduces the available variation, and thus, the power to identify the effect of the law, we use highly detailed, individual-level data to identify the effect of diabetes mandates by age-race demographic group.[11] We find that the benefit of diabetes mandates is concentrated among infants born to women over 30 years old (Table 5). This is consistent with the higher prevalence of adverse infant outcomes and private health insurance among older women, as implied by March CPS data.

To investigate the hypothesis that the effect of better access to medical care is attenuated by behavioral changes, we estimate the effect of diabetes mandates on pregnancy weight gain among diabetic women. Because our data do not report weight before pregnancy, it is difficult to

[10] The lowest estimate is based on the1997 MEPS data on the prevalence of self-funded insurance plans, while the highest estimate is based on the 1996-1998 HRET data on the prevalence of self-funded insurance plans.
[11] The estimates are similar when using state-year cells, but the standard errors are understandably larger. (Results are reported in the online Appendix Table A7.)

assess whether changes in pregnancy weight indicate an improvement or a decline in health. Consequently, we concentrate on a weight gain of at least 35 pounds, which would be problematic for most women in our sample. The Institute of Medicine pregnancy weight gain guidelines are 28-40 pounds for underweight women, 25-35 for women of normal weight, 15-25 for overweight women, and 11-20 for obese women.[12] Given that approximately 59.5% of all women ages 20-39 are overweight, 54.9% of Non-Hispanic White women are overweight, and 78% of Non-Hispanic Black women are overweight (Flegal et al. 2010), while less than 3% of women over 20 years old are underweight (Fryar and Ogden 2010), a pregnancy weight gain of more than 35 pounds would be too much for the large majority of women in our sample. Note that in our data there is heaping at 35 pounds. It could be that women control their weight as to gain exactly 35 pounds, or else misreporting causes the heaping. We believe it is more likely to be caused by under-reporting than over-reporting and treat a weight gain of 35 pounds as problematic.[13] We find that diabetes mandates are in fact associated with an increase in pregnancy weight gain among diabetic women, but the change is statistically significant only among Black women who are 30-35 years old (Table 5). This change in behavior could attenuate the positive effect of diabetes mandates and at least partially explain why we find no effect on the prevalence of macrosomia.

Another way to test this hypothesis is to look at outcomes among first births to women over 35 years old, who would be more likely to avoid any type of behavior that might cause harm to the pregnancy. Among Black women over 35 years old, there is evidence of a significant increase in the probability of significant weight gain (35 pounds or more) only among those who

[12] "Weight Gain During Pregnancy: Reexamining the Guidelines," Institute of Medicine of the National Academies, May 28, 2009 available at www.iom.edu/pregnancyweightgain (downloaded August 2011).

[13] The weight-gain variable has a relatively high non-response rate (details in the online Appendix). There is no evidence, however, that the non-response rate differs between diabetics and non-diabetics. The non-response rate among women with diabetes is 6.516%; among non-diabetics it is 6.215%.

already had a child (coefficient 2.146, with standard error of 1.176 and significant at 5%) but not among those having their first child (coefficient 0.616 with standard error of 4.227). In addition, we find a significant decrease in very high birth weight prevalence among first births to Black women over 35 years old (coefficient -1.782 with standard error of 0.863 and significant at 5%) but a smaller decrease among those having second or subsequent births (coefficient -0.559 with standard error of 0.394). Although the coefficients follow a similar pattern, they are not significant for White women.

It is perhaps surprising that one demographic group that appears to exhibit increased displacement of healthy behaviors, African Americans, also experiences the largest improvement in outcomes. This, however, is fully consistent with some demographic groups having both the highest marginal cost of engaging in healthy behaviors, possibly because of location choices, and the highest marginal benefit from medical care. Even after controlling for socio-economic factors, predominantly African-American neighborhoods have higher fast-food density than other neighborhoods (Block, Scribner, and DeSalvo 2004, Stein and Chakraborty 2010). At the same time, African-Americans are more likely to have low birth weight babies than are Caucasian women (Nicholson et al. 2006). Thus it is quite plausible to find a higher moral hazard and better infant outcomes within this demographic group. An alternative explanation is that mandates have a differential effect on both moral hazard (Cagatay 2005) and outcomes depending on the initial health conditions. It is also possible that various demographic groups are covered by different insurance plans that create different incentives to discourage preventive care (Trujillo et al. 2010).

We further test the hypothesis of moral hazard by investigating how the effect varies by type of mandate. Mandates to cover diabetes likely have a higher potential for moral hazard than mandates to offer coverage for diabetes. We find no statistically significant difference between the two types (Table 6). The results suggest, however, that the positive correlation between mandates and the prevalence of macrosomia is driven by the mandate to provide coverage and not the mandate to offer coverage, consistent with expected patterns of behavioral responses to the two types of mandates.

In addition to the variation by maternal characteristics, it is possible that the difference between the impacts on the left versus the right tail of the birth distribution is driven by the infants' gender. There are more high weight and fewer low weight male births than female births. Because human males are more fragile than females (Kraemer 2000), they may be more sensitive to changes in the fetal environment. Although not statistically significant, the results reported in Table 7 suggest that the positive correlation between diabetes mandates and high birth weight is driven by male births. At the same time, diabetes mandates are associated with a statistically significant reduction in prematurity among female infants but not among male infants. Because both small for gestational age and large for gestational age are associated with a higher risk of stillbirth (Burmeister et al. 2012; Ray and Urquia 2012), a larger decrease in male versus female fetal deaths leading to relatively more premature and more high weight male live births would be consistent with our estimates.[14] Because our data do not have reliable information about stillbirths[15] and miscarriages, we defer this question for future research.

At the same time, we note that mandates lead to a decrease in low weight male births. Interestingly, most of the effect on low birth-weight is concentrated among premature births for both male and female infants. For instance, mandates are associated with a ~10.3% (coefficient -

[14] By fetal deaths, we denote any attrition between conception and live births.
[15] NCHS Fetal Death datasets have missing values for mother's diabetic status for approximately 40% of observations, which makes it unsuitable for our analysis. We find no evidence of a significant change in the sex ratio of infants born to diabetic women in states with mandates. The estimated effect of the diabetes mandate on the proportion of female births is -0.054, with a standard error of 0.251 (both multiplied by 100 to improve readability).

0.446) decrease in prevalence of premature low weight male births but with a ~3.5% (coefficient -0.057) decrease in prevalence of full-term, low weight male births. This is important to document, because prematurity is more important than intra-uterine growth in determining low birth weight, but so far it has proved to be more difficult to manipulate.

VALIDITY CHECKS

EFFECT BY EDUCATION.

If our empirical strategy captures the effect of diabetes mandates, we should observe a lower effect where mandates are less likely to be binding, as is the case of women with a lower level of education, who are less likely to have private health insurance. The coefficients obtained for the sample of women with high-school education or less are indeed not significant and even change signs in some cases (Table 8, Panel A).[16] Moreover, when we restrict the sample to women with 16 or more years of education, of whom according to March CPS data almost 90% have private insurance, the effect is larger and more precisely estimated. These findings provide further reassurance that our identifying strategy successfully isolates the impact of mandates.

EFFECT BY TYPE OF DIABETES: PREGESTATIONAL VERSUS GESTATIONAL

Medical studies indicate that having diabetes early in pregnancy (mostly pregestational diabetes) leads to low birth weight and prematurity (Pedersen, Molsted-Pedersen, and Mortensen 1984), while having diabetes later in pregnancy, as in gestational diabetes, is more likely to lead to macrosomia (Schaefer-Graf et al. 2003). If the identified effect is caused by changes in diabetes management, any improvement in prematurity and low birth weight (high birth weight) should be smaller (larger) where a higher proportion of women have gestational diabetes. To investigate this, we interact the BRFSS estimate of the share of women with gestational diabetes

[16] The coefficients were obtained under the assumption that the timing of the effect of diabetes mandates is the same across subsamples. The results of our investigation of this issue are reported in the online Appendix Table A3.

among 18-45 year-old women with diabetes in each state/year cell with our variable of interest. As expected, the estimated coefficient of this interaction term is negative in the case of high birth weight and positive in the case of low birth weight and prematurity. This is consistent with findings from previous medical studies, thus providing further reassurance that we are identifying the effect of changes in diabetes management resulting from mandates.

EFFECT BY LIKELIHOOD OF TREATMENT AS DETERMINED BY TYPE OF INSURANCE

Even among women with more than a high-school education, not all have private insurance. Our data do not include information about whether the women had private insurance. Instead, we follow Schmidt (2007) and use March CPS data to calculate the share of 18-45 year-old women with more than 12 years of education in each state and year who are covered by private insurance and test whether the effect of diabetes mandates varies with the prevalence of private-insurance coverage. The coefficients of the interaction term between our variable of interest and the share of privately insured women indicate a systematic gradient in the size of the effect by prevalence of private coverage: Mandates lead to larger decreases in both tails of the birth weight distribution where more women are covered by private insurance (Table 9, Column 1).

Among those who have private insurance, only those enrolled in fully funded plans experience a change in coverage, because under ERISA, firms that self-insure are exempt from mandates. Detailed information on the share of employees in such firms is not available. Previous empirical analyses, however, indicate that large firms are more likely to self-insure (Gabel, Jensen, and Hawkins 2003; Park 2000). We follow Schmidt (2007) and use the March CPS share of 18-45 year-old employees with more than 12 years of education who work in firms with less than 500 employees data as a proxy for the share of employment in firms with fully funded plans. Although not all estimates are statistically significant, we find that the decrease in the tails of the birth weight distribution is systematically larger where the share of employment in small- and medium-sized firms is more significant.

Furthermore, we obtain an estimate of the share of the population in fully insured plans and investigate whether we observe a larger effect where more people are enrolled in such plans. For this purpose, we use the Kaiser/HRET Employer Health Benefits Annual Survey data on the share of covered workers in self-insured plans by firm size,[17] along with the share of 18-45 year-old workers with more than 12 years of education by firm size in each state/year cell (March CPS data), to obtain an estimate of the share of workers enrolled in fully funded insurance plans. For an estimate of the share of women of childbearing age (18-45 years old) covered by fully insured plans, we apply the private insurance rates within this population (March CPS data) to the share of fully insured workers. The coefficients of the interaction between the share of fully insured women and our variable of interest show the same pattern of health improvements where the share of the treated population is larger (Table 9, column 3).

Last, we test how the diabetes-mandates effect varies with employment in industries where workers tend to obtain health insurance through fully insured plans: retail, wholesale, service, and finance (Table 9, column 4).[18] We find a similar gradient to that in our previous analyses. All these tests point in the same direction: The effect of diabetes mandates is larger where the likelihood of treatment is greater, providing further support for our identification strategy.

[17] Because the data are not available for all years in our sample, we average 1996 and 1998 data to obtain the 1997 (middle of sample) estimate of the share of workers in self-insured plans by firm size.
[18] Three sources of data were used to classify the industries' function of the share of workers in fully insured plans: Kaiser/HRET Employer Health Benefits Annual Survey data, the Form 5500 filings as described by Brien and Panis (2011), and MEPS data. More details are available in the online Appendix.

SENSITIVITY ANALYSIS

Our results are robust to a wide series of specification tests, which are reported in the online Appendix Table A12. Here we briefly summarize our findings. The results are robust to using a log specification, and to using fewer or more years. While in the main analysis we measure the impact using the diabetes mandates' effective dates, the results are robust to using the enactment date or recoding the law as 1 if diabetes mandates became effective before July 1st of that year, and 0.5 if diabetes mandates became effective in the first week of July. In addition, the estimates are very similar to the results obtained from a DD specification using only the states that adopted mandates, or only the subsample of infants born to women with diabetes, providing reassurance against any concerns that potential secular differences between the infant health of diabetic versus non-diabetic mothers or between adopting and non-adopting states (not already captured by controls) confound our estimates. There is no evidence of endogeneity, as the lead of the effective date of mandates and the lead of the enactment date are not significant predictors of future infant outcomes, providing support for our identifying strategy.

7. CONCLUSIONS

This paper provides new evidence regarding the impact of mandated health insurance benefits on health outcomes. Our results indicate that diabetes mandates are associated with fewer low birth weight and premature births among women with more than 12 years of education, a demographic group known to have the type of insurance coverage subject to mandates.

We find no significant evidence of such a change in the prevalence of high birth weight births. This result appears to be driven by mandates to cover diabetes, and it is not present in the case of mandates to offer coverage for diabetes. Because mandates to cover tend to have more potential for moral hazard, it is possible that behavioral changes in eating and exercise practices differentially affect the two tails of the birth weight distribution. In fact, we find some evidence consistent with the presence of moral hazard: Diabetes mandates are associated with a higher likelihood of excessive weight gain during pregnancy. This effect varies significantly across demographic groups, suggesting that moral hazard varies by demographic factors or by initial health conditions. Thus imposing the constraint of an equal effect for the entire population may obscure the distributional effect of public policies.

We performed numerous falsification and specification tests to verify the validity of the central findings. As expected, the effect is larger where women are more likely to hold private insurance and, thus, are more likely to be affected by diabetes mandates. We also find evidence of a decrease in the prevalence of high birth weight in these areas, suggesting that although the population effect is small, mandates do improve the right tail of the birth weight distribution.

Our results provide strong evidence that there may be significant bias in evaluating the impact of a policy when third parties are affected. Thus a comprehensive investigation of the effects of insurance mandates in the population is necessary to determine the overall efficacy of such policies. We show that mandate-induced changes in diabetes insurance coverage led to fewer premature and low birth weight births. Because mandates are not binding for self-insured firms, our results suggest there is potential for public policy to further improve health and birth outcomes among women with diabetes.

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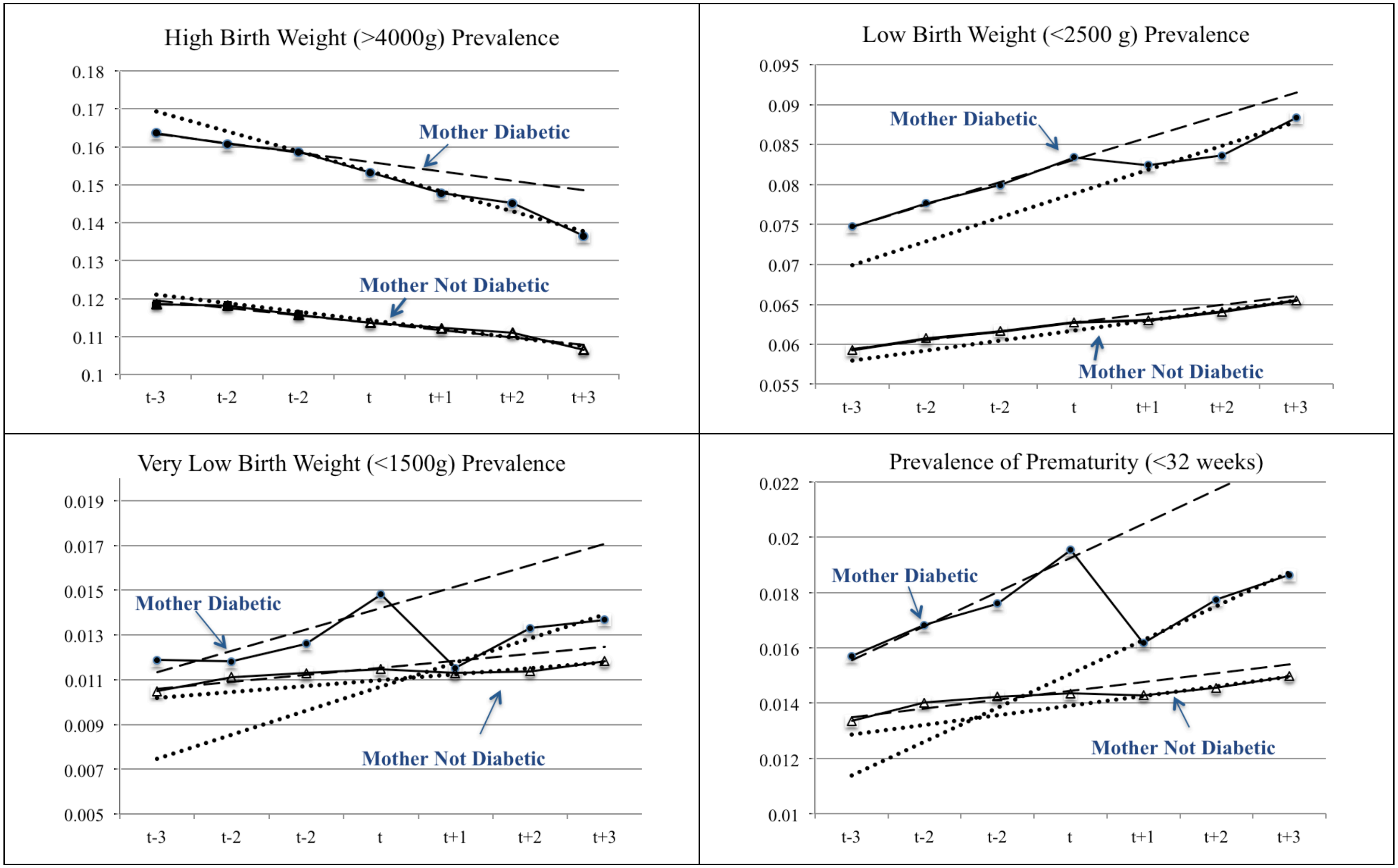

Figure 1. The solid lines represent the prevalence of each outcome among infants born in the years preceding and following diabetes mandates adoption at time t. The long dash line represents the trend in outcomes determined by data 3 years before diabetes mandates became effective and up to and including the year prior to adoption. The short dash line is the trend in outcomes after the adoption determined by data starting from the year of adoption and up to the third year following adoption. The sample is identical to the sample used for the regressions reported in Table 4.

Table 1. Diabetes Mandates Legislation

Effective Date	States
1988	WI
1994	NY
1995	MN
1996	FL, NJ, WV
1997	ME, OK, RI
1998	AR, CT, IN, LA, MD, MO, NV, NH, NM, NC, TN, TX, VT, WA
1999	AZ, CO, IL, KS, KY, MS, PA
2000	CA, IA, NE, SC, SD, VA
2001	AK, DE, DC, MA, MI, UT
2002	HI, MT, OR, WY
2003	GA

Source: National Conference of State Legislatures; State legislatures.
Mississippi, Missouri, and Washington state laws require only that insurers offer coverage for diabetes (mandate to offer). All other states require private insurance policies to cover diabetes treatment.

Table 2: Summary Statistics

	All			Mother Diabetic		
	Non-Adopting States	Adopting States	t	Non-Adopting States	Adopting States	t
	[1]	[2]	[3]	[4]	[5]	[6]
HBW >4000g	12.003	11.551	-0.65	16.491	15.726	-0.29
	(2.989)	(2.828)		(4.166)	(3.573)	
HBW >4500g	1.875	1.797	0.45	3.980	3.822	0.51
	(0.748)	(0.703)		(1.966)	(1.766)	
LBW <2500g	6.125	6.485	0.75	7.563	8.196	-0.43
	(2.379)	(2.253)		(3.535)	(3.637)	
LBW <1500g	1.150	1.256	0.90	1.179	1.322	-1.13
	(0.780)	(0.773)		(1.405)	(1.395)	
Premature <37 weeks	9.548	10.071	0.19	14.407	15.224	-1.21
	(2.867)	(2.677)		(4.813)	(4.528)	
Premature <32 weeks	1.487	1.585	1.17	1.667	1.868	-0.46
	(0.975)	(0.923)		(1.668)	(1.730)	
Female	48.736	48.749	-0.18	47.924	48.295	0.92
	(1.155)	(1.043)		(4.758)	(4.291)	
Plural	3.097	3.404	-0.33	4.249	4.665	-0.56
	(1.081)	(1.182)		(2.759)	(2.630)	
First child	42.604	42.595	-0.21	39.930	39.472	-1.42
	(14.843)	(14.199)		(14.529)	(13.844)	
Mother's Age	29.352	29.603	0.97	31.040	31.295	0.80
	(4.880)	(4.969)		(4.848)	(4.902)	
Mother-Black	11.805	11.783	-0.06	12.751	13.509	0.39
	(32.270)	(32.326)		(33.362)	(34.232)	
Mother's Education	15.035	15.113	1.26	14.992	15.045	1.10
	(0.537)	(0.538)		(0.424)	(0.425)	
Mother Married	84.979	83.957	-1.16	86.857	85.464	-1.32
	(16.651)	(17.088)		(13.301)	(13.833)	
No Prenatal Care	0.548	0.473	-0.60	0.238	0.237	-0.64
	(0.701)	(0.526)		(0.608)	(0.526)	
Log (wage)	2.576	2.638	0.74	2.570	2.638	0.91
	(0.141)	(0.153)		(0.142)	(0.153)	
Physicians/ 1000 pop	2.283	2.494	1.25	2.280	2.554	1.50
	(0.544)	(0.571)		(0.545)	(0.591)	
Hospitals/ 100,000 pop	2.475	2.240	-0.68	2.486	2.216	-0.93
	(1.109)	(0.972)		(1.087)	(0.941)	

All means are calculated for the 1992-2003 sub-sample of infants born to mothers with more than 12 years of education. Columns 1, 2, 4, and 5 report averages of state-year observations with no mandated diabetes benefits. Columns 1 and 4 isolate the observations corresponding to states that did not adopt the diabetes mandate in the following year. Columns 2 and 5 isolate the observations corresponding to states that adopted the diabetes mandate in the following year. Standard errors clustered at state level are reported in parentheses. Columns 3 and 6 report t-test of equality of means conditional on time fixed effects.
*significant at the 10% level, ** significant at the 5% level, *** significant at the 1% level.

Table 3. The Impact of Diabetes Mandates: DD Analysis on Sub-samples

	High Birth Weight		Low Birth Weight		Premature Birth	
	>4000 g	*>4500 g*	*<2500 g*	*<1500 g*	*<37 weeks*	*<32 weeks*
			Panel A: Diabetes			
Coefficient	0.308	0.057	-0.368***	-0.225***	-0.389	-0.270***
Standard Error	(0.266)	(0.116)	(0.136)	(0.079)	(0.289)	(0.091)
% Impact (coeff/mean)	2.014%	1.592%	-4.485%	-17.176%	-2.562%	-14.942%
			Panel B: No diabetes			
Coefficient	0.059	0.022	-0.043	-0.026*	-0.040	-0.024
Standard Error	(0.075)	(0.022)	(0.037)	(0.014)	(0.062)	(0.017)
% Impact (coeff/mean)	0.519%	1.287%	-0.673%	-2.151%	-0.403%	-1.550%

Each cell reports estimates from a separate regression using 1992-2003 data on births to women with more than 12 years of education collapsed into state/year/mother's age/mother's race/ mother's diabetic status cells. Panel A retains only births to women with diabetes. Panel B retains only births to women without diabetes. All regressions cell level controls such as mother's education (percent with a college degree), marital status, and prenatal care (percent women starting prenatal care in 2nd trimester, 3rd trimester, or that did not receive prenatal care), infant gender, plurality, birth order (percent first child); state level controls such as physicians and hospitals; age-race demographic group fixed effects by year fixed effects; and state fixed effects. Robust standard errors clustered at the state level are reported in parentheses. * significant at 10% level; ** significant at 5% level, *** significant at 1% level

Table 4. The Impact of Diabetes Mandates: DDD Analysis

	High Birth Weight		Low Birth Weight		Premature Birth	
	>4000g	>4500g	<2500g	<1500g	<37 weeks	<32 weeks
Coefficient	0.323	0.059	-0.339**	-0.192**	-0.335	-0.223**
Standard Error	(0.238)	(0.113)	(0.151)	(0.080)	(0.281)	(0.099)
% Impact (coeff/mean)	2.112%	1.648%	-4.132%	-14.656%	-2.206%	-12.341%
Coeff. inflated HRET	0.714	0.130	-0.749	-0.424	-0.740	-0.493
Coeff. inflated MEPS	0.692	0.126	-0.727	-0.411	-0.718	-0.478
% Impact inflated HRET	4.707%	3.672%	-9.208%	-32.666%	-4.917%	-27.505%
% Impact inflated MEPS	4.564%	3.561%	-8.929%	-31.673%	-4.768%	-26.669%

Each cell reports estimates from a separate regression using 1992-2003 data on births to women with more than 12 years of education collapsed into state/year/mother's age/mother's race/ mother's diabetic status cells (9352 observations). The average cell size is approximately 2200 births. All regressions control for mother's education (percent with a college degree), marital status, and prenatal care (percent women starting prenatal care in 2nd trimester, 3rd trimester, or that did not receive prenatal care), infant gender, plurality, birth order (percent first child), age-race demographic group fixed effects that are allowed to vary over time. All regressions control for diabetic status of the mother, state fixed effects, year fixed effects, and all their interactions. Robust standard errors clustered at the state level are reported in parentheses.
* significant at 10% level; ** significant at 5% level, *** significant at 1% level

Table 5. The Impact of Diabetes Mandates on Infant Outcomes by Demographic Group: DDD Analysis using Individual Level Data

	High Birth Weight		Low Birth Weight		Premature Birth		Weight
	>4000 g	*>4500 g*	*<2500 g*	*<1500 g*	*<37 weeks*	*<32 weeks*	Gain ≥35
				Panel A: WHITE			
Age <25	-0.501	0.261	0.194	-0.069	0.689	0.321	-0.004
	(0.646)	(0.417)	(0.435)	(0.184)	(0.524)	(0.273)	(0.990)
25≥ Age >30	0.424	-0.235	-0.132	-0.185	-0.299	-0.315**	-0.137
	(0.384)	(0.162)	(0.208)	(0.136)	(0.375)	(0.141)	(0.463)
30≥ Age >35	0.264	0.308**	-0.306	-0.164*	-0.863**	-0.404***	0.041
	(0.431)	(0.141)	(0.194)	(0.090)	(0.383)	(0.110)	(0.345)
Age ≥ 35	0.239	0.041	-0.303	-0.223***	0.459	-0.165	0.397
	(0.347)	(0.219)	(0.253)	(0.082)	(0.362)	(0.130)	(0.425)
				Panel B: BLACK			
Age <25	1.645	0.352	0.406	0.534	0.426	0.410	1.073
	(1.118)	(0.621)	(0.972)	(0.419)	(1.368)	(0.478)	(2.094)
25≥ Age >30	1.157	-1.287	-0.006	0.350	-2.194*	0.226	1.659
	(0.886)	(5.137)	(0.675)	(0.380)	(1.166)	(0.512)	(1.311)
30≥ Age >35	0.005	-0.422	-1.516*	-0.828*	-0.321	-0.374	2.471**
	(0.669)	(0.538)	(0.821)	(0.466)	(0.769)	(0.436)	(1.215)
Age ≥ 35	-0.137	-0.692*	-2.150**	-1.053**	-1.090	-1.227**	2.001*
	(0.906)	(0.346)	(0.915)	(0.517)	(1.283)	(0.594)	(1.019)

Each cell reports estimates from a separate regression using 1992-2003 individual-level data on infants born to mothers with more than 12 years of education. The number of observations differs across regressions from a low 298,593 births to Black women over 35 to a high 5,703,032 births to White women ages 30-35. To improve readability all coefficients and standard errors are multiplied by 100. All regressions control for mother's age, race, education (percent with a college degree), marital status, and prenatal care (if started prenatal care in 2nd trimester, 3rd trimester, or did not receive prenatal care); infant gender; plurality; and birth order (dummy equal to 1 if first child and zero otherwise). All regressions control for diabetic status of the mother, state fixed effects, year fixed effects, and all their interactions. Robust standard errors clustered at the state level are reported in parentheses.
* significant at 10% level; ** significant at 5% level, *** significant at 1% level

Table 6. The Impact of Diabetes Mandates: Mandate to Provide Coverage versus Mandate to Offer Coverage

	High Birth Weight		Low Birth Weight		Premature Birth	
	>4000 g	*>4500 g*	*<2500 g*	*<1500 g*	*<37 weeks*	*<32 weeks*
Mandate to:						
Cover diabetes treatment	0.399*	0.093	-0.328**	-0.184**	-0.309	-0.218**
	(0.230)	(0.112)	(0.150)	(0.080)	(0.286)	(0.100)
Offer coverage for diabetes treatment	-0.770	-0.435	-0.501	-0.307*	-0.708**	-0.289
	(0.740)	(0.293)	(0.343)	(0.155)	(0.318)	(0.205)
F-test of joint significance	2.34	1.70	2.63*	3.24**	2.48*	2.60*
F-test of equality of coefficients	2.47	3.19*	0.29	0.75	1.85	0.13

The results reported here were obtained using the sample and model specification described at Table 4.

Table 7. The Impact of Diabetes Mandates on Infant Outcomes – by Gender

	High Birth Weight		Low Birth Weight		Premature Birth	
	>4000 g	>4500 g	<2500 g	<1500 g	<37 weeks	<32 weeks
Female	0.127	-0.047	-0.148	-0.219**	-0.245	-0.271**
	(0.248)	(0.115)	(0.218)	(0.096)	(0.270)	(0.113)
Male	0.508	0.157	-0.507**	-0.165*	-0.417	-0.178
	(0.309)	(0.168)	(0.196)	(0.096)	(0.404)	(0.109)

Each cell reports estimates from a separate regression using 1992-2003 data on infants born to mothers with more than 12 years of education collapsed into state/year/mother's age/mother's race/mother's diabetic status cells/infant gender cells. The results reported here were obtained using the model specification described at Table 4. Regressions reported in first row use 9142 observations, while regressions reported in second row use 9154 observations.

Table 8. The Impact of Diabetes Mandates on Infant Outcomes - Robustness Tests

	High Birth Weight		Low Birth Weight		Premature Birth	
	>4000 g	*>4500 g*	*<2500 g*	*<1500 g*	*<37 weeks*	*<32 weeks*
	Panel A. Mandates effect by mothers' education					
Sample: ≤ 12 years of education	-0.047	0.039	0.079	-0.018	0.100	0.026
	(0.296)	(0.126)	(0.187)	(0.093)	(0.237)	(0.108)
Sample: ≥16 years of education	0.355	0.114	-0.580***	-0.323***	-0.450	-0.367***
	(0.301)	(0.153)	(0.208)	(0.118)	(0.386)	(0.136)
	Panel B. Mandates effect by type of diabetes					
Diabetes•Mandate	0.362	0.098	-0.304**	-0.206**	-0.328	-0.227**
	(0.237)	(0.116)	(0.141)	(0.078)	(0.271)	(0.100)
Diabetes•Mandate• % Gestational Diabetes	-0.0464***	-0.0083***	0.0093**	0.0044**	-0.0044	0.0045**
	(0.0076)	(0.0029)	(0.0042)	(0.0017)	(0.0062)	(0.0020)

The results reported here were obtained using the model specification described at Table 4.

Table 9. The Impact of Diabetes Mandates by Likelihood of Treatment

	Diabetes• Mandate•Private Insurance	Diabetes• Mandate• Small Firm Employment	Diabetes• Mandate•Fully Insured	Diabetes•Mandate •Mostly Fully Insured Industry
	[1]	[2]	[3]	[4]
High Birth Weight				
>4000g	-0.077***	-0.069*	-0.151***	-0.071*
	(0.025)	(0.035)	(0.041)	(0.035)
>4500g	-0.026*	-0.035*	-0.057**	-0.037**
	(0.015)	(0.020)	(0.025)	(0.017)
Low Birth Weight				
<2500g	-0.013	-0.014	-0.017	-0.009
	(0.017)	(0.025)	(0.032)	(0.025)
<1500g	-0.008	-0.012	-0.011	-0.004
	(0.010)	(0.009)	(0.017)	(0.011)
Premature Birth				
<37 weeks	-0.074***	-0.092**	-0.164***	-0.068
	(0.020)	(0.045)	(0.044)	(0.052)
<32 weeks	-0.002	-0.019	-0.002	-0.027**
	(0.013)	(0.012)	(0.021)	(0.013)

The results reported here were obtained using the sample and model specification described at Table 4.