APPENDIX – NOT FOR PUBLICATION

Natality Data

The National Center for Health Statistics (NCHS) Natality data was collapsed into state/year of birth/mother diabetes status/demographic groups cells, where the demographic groups are defined by age: 25, 25 to 29, 30 to 34, and 35 and over; and race: Black and White, where White includes all non-Black. In the main subsample used in this paper, that of singleton births to women with more than 12 years of education, the average cell size is of approximately 2200 observations. Because our data refers to live births not all demographic cells are represented in all states and all years.

In our data approximately 27.2% of the cells contain less than 50 births and approximately 11.6% of the cells contain fewer than 10 births. Our results however are not driven by noise. As reported in table A5 the results are robust to excluding the small cells.

In the paper we retain 3 measures of infant health: high birth weight, low birth weight and prematurity. Premature births occur at less than 37 weeks of gestation; very premature births occur at less than 32 weeks of gestation; very low birth weight (VLBW) occurs at less than 1500 grams; low birth weight (LBW) occurs at less than 2500 grams, high birth weight (HBW) occurs at greater than 4000 grams, and very high birth weight (VHBW) at greater than 4500 grams.

Natality data also provides an additional measure: the Apgar score.[1] As shown in Table A3 the effect of mandates on the prevalence of low 5-minute Apgar scores (< 8) among infants born to diabetic mothers is negative but not statistically significant at conventional significance levels. Note that this variable has a relatively high non-reporting rate (~18%). Most non-response

[1] The Apgar score is a summary measure of the newborn's condition based on heart rate, respiratory effort, muscle tone, reflex irritability, and skin color. It takes values from 1 to 10, where higher is better. Values of 8 and above are considered normal. Any score lower than 8 indicates the child needs assistance. Source: http://www.nlm.nih.gov/medlineplus/ency/article/003402.htm

(~65%) is driven by one state only, California. All other results are robust to the exclusion of California.

In addition, Natality data contain information about pregnancy weight gain. There is an approximately 18% non-response rate of weight gain. About 65% of the missing values come from California. This may raise the concern of sample selection. There is no evidence, however, that the non-response rate is different for diabetics versus non-diabetics. The non-response rate among women with diabetes is 6.516%; and among non-diabetics it is 6.215%.

We retain all births, including multiple births, and thus our results are valid for the entire population. We find no evidence that diabetes mandates led to any change in the proportion of plural births among women with diabetes relative to non-diabetics (the estimated effect is 0.088, with standard errors of 0.202 and thus highly insignificant). The results hold on the sample of singletons, although the estimates are slightly lower and sometimes with larger standard errors function of specification, which is understandable given that we lose power: More than 25% of low birth-weight babies are plural births. In state cell regressions, this leads to very thin cell sizes for women with diabetes in some age groups. We need this variation, given that we control for a very large number of fixed effects relative to the sample size in our state-level regressions. The results using individual-level data on the sub-sample of singleton births are available on reported in Appendix Table A9.

Diabetes Mandates

There are many legitimate ways to code the legislative data. If the transaction costs associated with insurance contractual arrangements are high enough, the enactment of the law could be enough to prompt insurance companies to include diabetes coverage on new insurance contracts. In this case, there may be an impact on coverage even before the law becomes effective. If additional coverage is costly enough, however, insurance companies may prefer to wait until the law becomes effective to include such additional coverage for diabetes.[2] However, because of the difference between the timing of changes in coverage and the date the effect is recorded, only after birth, it is unlikely that there is any impact on infants born between the enactment and effective dates. For this reason, in the main analysis we measure the impact using the diabetes mandates' effective dates (Table A1). 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 births in that year. In the sensitivity analysis (Table A 12) we show that our results are robust to different ways of coding of the diabetes insurance variable.

In addition, even when using the effective date the effects of diabetes mandates on birth outcomes likely lag at least several months behind their adoption. Small fetal size was correlated with maternal glycemic control during the first trimester (Pedersen et al. 1984; Visser et al. 1985), which suggests a lag of at least 6 months between the adoption of mandates and the effect on low birth weight. On the other hand previous research shows that fetal growth acceleration in large for gestational age fetuses of diabetic mothers begins in the second trimester (Wong et al., 2002), and, in fact, high-glycemia appears to have the most impact on fetal growth in the third trimester (Schaefer-Graf et al., 2003).

In Table A3, we experiment with different specifications and let the data indicate the relevant time period. We find that the effect of diabetes mandates on prematurity and low birth weight lags one year behind the year the legislation became effective, while the effect on high-birth weight is not significant at conventional significance levels at any lag.

[2] Note that the year of enactment and the year the law became effective are identical for some states.

Other Variables

1) Share of women with gestational diabetes

Because Natality data do not include information about the type of diabetes, we use BRFSS data to obtain the relative prevalence of gestational diabetes among 18-45 year-old women with diabetes for each state/year cell. BRFSS includes pregnant women; however the sample-size is very small (an average of ~60 women in a state) thus making the sample of diabetic pregnant women too small (an average of ~3 in each state) for any meaningful estimates of relative prevalence of gestational versus pre-gestational diabetes. Consequently, we obtain estimates for the entire sample of women ages 18-45. There is measurement error in this variable. We do not have a very good estimate of the actual instances of gestational diabetes in a year, because the question asked in BRFSS is, "Have you EVER been told by a doctor that you have diabetes?" Thus, for instance, among women 30-44, some may have had gestational diabetes in their 20s. Nevertheless, the mean estimates appear to be quite similar to the estimates of prevalence of diabetes from other sources. Approximately 2.3% of women aged 18-45 have diabetes, which is slightly higher than the 1.82% pre-gestational diabetes among pregnancies in 2005 (Lawrence et al., 2008). Also, approximately 2.2% of women 18-45 have ever had gestational diabetes, compared to an estimated range of 2 to 10% of pregnancies (CDC, 2011).

2) Share of women treated

Diabetes mandates apply to private insurance plans. In addition, not all women with private insurance will experience a change in their coverage even when living in a state with a diabetes mandate. Under the Employment Retirement Income Security Act of 1974 (ERISA), firms that self-insure are exempt from mandates.

We do not have information about private insurance in the Natality data, nor information about the share of women with private insurance from self-insured plans that are not subject to diabetes mandates. Instead we use data from the Kaiser/HRET Employer Health Benefits Annual Survey to obtain the percentage of insured workers in self-insured plans by firm size (specifically, we averaged the 1996 and 1998 data to obtain self-insurance rates in 1997, the middle of our sample period). This information, along with the data regarding the share of 18-45 year old workers with more than 12 years education by firm size (source: March CPS) allows us to obtain the share of population with more than a high-school education in self-insured plans. Our calculations indicate that approximately 44.6% of insured people with more than a high-school education were in self-insured plans. Using the 1997 Medical Expenditure Panel Survey (MEPS) data we find a slightly lower estimate: ~42.90%. Given that according to March CPS data 81.7% of 18-45 year-old women with more than 12 years of education had private insurance during our sample period, we find that between 45.24% (HRET-based estimate: (100-44.6)*81.7/100) and 46.66% (MEPS-based estimate: (100-42.90)*81.7/100) of educated women of childbearing age would have had private insurance subject to this regulation. These calculations assume that the rates of self-insurance are the same for both group insurance and individually purchased insurance, while in fact individually purchased insurance is more likely to be fully funded (Klick and Stratman, 2007) and thus subject to mandates.

3) Industries where workers tend to obtain insurance through fully insured plans

Both the Kaiser/HRET Employer Health Benefits Annual Survey data and the Form 5500 filings, as described by Brien and Panis (2011), indicate that retail, finance, and service have lower rates of self-insurance than other industries. In addition, Kaiser/HRET data indicate that, in aggregate, mining/construction/wholesale have low rates of self-insurance, but Brien and Panis (2011) find low rates of self-insurance only in wholesale and not in mining and construction. MEPS data confirm high-rates of self-insured plans in mining, but not in construction. However, March CPS data indicate significantly lower rates of private insurance in construction, and thus even if many insured workers may be in fully funded plans, the proportion of total workers in fully funded plans is still lower than that in other industries. These observations made us retain wholesale employment in our analysis but not mining and construction.

Table A1: Diabetes Mandates Legislation

State	Effective Date	Legislation
Alaska	July 27, 2000	ST §21.42.390
Arizona	January 1, 1999	A.R.S. §20-826(P), §20-934
Arkansas	August 1, 1997	ST §23-79-603
California	January 1, 2000	HLTH & S §1367.51
Colorado	July 1, 1998	ST §10-16-104 (subsection 13)
Connecticut	October 1, 1997	§38a-492(d)
Delaware	September 29, 2000	18§3560
District of Columbia	October 21, 2000	DC CODE §31-3001
Florida	July 1, 1995	FL ST §627.65745
Georgia	July 1, 2002	§33-24-59.2
Hawaii	July 1, 2001	HI ST §432:1-612
Illinois	January 1, 1999	215 ILCS 5/356w - (H. 3427)
Indiana	January 1, 1998	IN ST 27-8-14.5-4
Iowa	July 1, 1999	IA ST §514C.18
Kansas	January 1, 1999	KS ST § 40-2,163
Kentucky	July 15, 1998	KY ST §304.17A-148
Louisiana	January 1, 1998	LA R.S. 22:1034
Maine	July 4, 1996	ME ST T. 24 §2332-F:
Maryland	October 1, 1997	MD INSURANCE §15-822
Massachusetts	August 2, 2000	MA ST 118E §10C
Michigan	March 28, 2001	MI ST 500.3406p
Minnesota	August 1, 1994	MN ST §62A.45
Mississippi	January 1, 1999	MS ST § 83-9-46
Missouri	January 1, 1998	MO ST 376.385
Montana	January 1, 2002	MT ST 33-22-129
Nebraska	October 1, 1999	NE ST § 44-790
Nevada	January 1, 1998	NV ST 689A.0427
New Hampshire	January 1, 1998	NH ST §415:6-e
New Jersey	January 5, 1996	NJ ST 17:48-6n
New Mexico	January 1, 1998	NM ST §59A-22-41
New York	January 1, 1994	NY INS §3216:
North Carolina	October 1, 1997	NC ST § 58-51-61:
Oklahoma	November 1, 1996	OK ST T. 36 §6060.2.
Oregon	January 1, 2002	OR ST §743.694.
Pennsylvania	February 13, 1999	40 P.S. §764e.
Rhode Island	January 1, 1997	RI ST §27-18-38
South Carolina	January 1, 2000	SC ST § 38-71-46.
South Dakota	July 1, 1999	SD ST §58-18-83
Tennessee	January 1, 1998	TN ST § 56-7-2605
Texas	January 1, 1998	TX INS §1358.001-TX INS §1358.005
Utah	July 1, 2000	UT ST §31A-22-626.
Vermont	October 1, 1997	VT ST T. 8 §4089c.
Virginia	July 1, 1999	VA ST §38.2-3418.8
Washington	January 1, 1998	WA ST 48.20.391
West Virginia	June 8, 1996	WV ST § 33-15C-1:
Wisconsin	April 7, 1988	WI Stat Ann §632.895[6]
Wyoming	July 1, 2001	WY ST §26-20-201

Source: National Conference of State Legislatures; State legislatures.

Table A2. Summary Statistics

	Mother Not Diabetic		
	Non-Adopting States	Adopting States	t
HBW >4000g	11.879	11.427	-0.71
	(2.851)	(2.707)	
HBW >4500g	1.817	1.737	0.28
	(0.585)	(0.539)	
LBW <2500g	6.085	6.434	0.76
	(2.327)	(2.179)	
LBW <1500g	1.149	1.254	1.00
	(0.756)	(0.747)	
Premature <37 weeks	9.414	9.917	0.20
	(2.671)	(2.442)	
Premature <32 weeks	1.482	1.577	1.23
	(0.949)	(0.887)	
Female	48.758	48.762	-0.28
	(0.852)	(0.753)	
Plural	3.065	3.367	-0.34
	(0.977)	(1.089)	
First child	42.678	42.688	-0.15
	(14.846)	(14.208)	
Mother's Age	29.305	29.553	0.94
	(4.874)	(4.965)	
Mother-Black	11.779	11.731	-0.08
	(32.242)	(0.322)	
Mother's Education	15.036	15.115	1.27
	(0.539)	(0.541)	
Mother Married	84.927	83.912	-1.16
	(16.732)	(17.185)	
No Prenatal Care	0.556	0.480	-0.59
	(0.702)	(0.524)	
Log (wage)	2.576	2.638	0.73
	(0.141)	(0.153)	
Physicians/ 1000 pop	2.283	2.492	1.24
	(0.544)	(0.571)	
Hospitals/ 100,000 pop	2.475	2.241	-0.67
	(1.110)	(0.974)	

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

Table A3. The Impact of Diabetes Mandates on Infant Outcomes: The Timing of the Effect by Mothers' Education (DDD analysis)

	Mother's Education >12			Mother's Education ≤12		
Timing of Impact	*t*	*t+1*	*t+2*	*t*	*t+1*	*t+2*
High Birth Weight						
>4000g	0.435*	0.323	0.223	-0.145	-0.048	0.032
	(0.245)	(0.238)	(0.254)	(0.252)	(0.296)	(0.269)
>4500g	0.091	0.059	-0.063	0.115	0.039	0.110
	(0.114)	(0.113)	(0.109)	(0.086)	(0.126)	(0.137)
Low Birth Weight						
<2500g	-0.128	-0.339**	-0.260**	0.137	0.079	0.185
	(0.119)	(0.151)	(0.129)	(0.167)	(0.188)	(0.158)
<1500g	-0.008	-0.192**	-0.073	-0.052	-0.018	0.007
	(0.089)	(0.080)	(0.072)	(0.087)	(0.093)	(0.078)
Premature Birth						
<37 weeks	-0.215	-0.335	-0.187	0.213	0.100	0.184
	(0.275)	(0.281)	(0.245)	(0.280)	(0.237)	(0.207)
<32 weeks	-0.042	-0.223**	-0.134*	-0.001	0.026	0.039
	(0.100)	(0.099)	(0.080)	(0.099)	(0.108)	(0.092)
5 minutes Apgar Score						
<8	-0.025	-0.088	-0.019	-0.083	0.033	0.101
	(0.140)	(0.135)	(0.117)	(0.087)	(0.116)	(0.112)

Each cell reports estimates from a separate regression using the 1992-2003 data on births to women with more than 12 years of education collapsed into state/year/mother's age/mother's race cells (9352 observations for the sample of births to women with >12 years of education, 9314 observations for the sample of births to women with ≤12 years of education). All regressions control for mother's education, 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 state level are reported in parentheses. * significant at 10% significance level; ** significant at 5% significance level, *** significant at 1% significance level

Table A4. The Impact of Diabetes Mandates on Infant Outcomes by Mother's Education (DDD)

	High Birth Weight		Low Birth Weight		Premature Birth	
	>4000 g	*>4500 g*	*<2500 g*	*<1500 g*	*<37 weeks*	*<32 weeks*
Baseline: >12 years of education	0.323	0.059	-0.339**	-0.192**	-0.335	-0.223**
	(0.238)	(0.113)	(0.151)	(0.080)	(0.281)	(0.099)
Sample: = 12 years of education	-0.319	-0.059	0.121	-0.029	0.256	-0.054
	(0.302)	(0.134)	(0.188)	(0.105)	(0.257)	(0.127)
Sample: < 12 years of education	0.605	0.254	-0.084	0.017	-0.139	-0.214
	(0.372)	(0.188)	(0.251)	(0.091)	(0.349)	(0.120)

Each cell reports estimates from a separate regression using the 1992-2003 data collapsed into state/year/mother's age/mother's race cells. All regressions control for mother's 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 state level are reported in parentheses. * significant at 10% significance level; ** significant at 5% significance level, *** significant at 1% significance level

Table A5. The Impact of Diabetes Mandates: Sensitivity to Small Cell Size (DDD Analysis)

	High Birth Weight		Low Birth Weight		Premature Birth	
	>4000 g	*>4500 g*	*<2500 g*	*<1500 g*	*<37 weeks*	*<32 weeks*
≥10 births	0.326	0.054	-0.333**	-0.196**	-0.354	-0.231**
(8270 obs.)	(0.239)	(0.114)	(0.152)	(0.081)	(0.282)	(0.100)
≥50 births	0.259	0.042	-0.323**	-0.200**	-0.372	-0.221**
(6804 obs.)	(0.258)	(0.116)	(0.157)	(0.081)	(0.294)	(0.101)

The results reported here use the model specification described at Table 4. The sample is births to women with more than 12 years of education. The sample used to obtain first row of estimates drops all cells with fewer than 10 observations. The sample used to obtain the second row of estimates drops all cells with fewer than 50 observations.

Table A6. The Impact of Diabetes Mandates on Different Sub-samples

High Birth Weight		Low Birth Weight		Premature Birth	
>4000 g	*>4500 g*	*<2500 g*	*<1500 g*	*<37 weeks*	*<32 weeks*
Panel A. Subsample Mother Married					
0.221	-0.028	-0.448**	-0.215**	-0.314	-0.265**
(0.246)	(0.114)	(0.166)	(0.084)	(0.311)	(0.115)
Panel B. Subsample Mother Not Married					
0.558	0.519*	0.376	-0.053	-0.414	0.040
(0.370)	(0.268)	(0.331)	(0.145)	(0.477)	(0.159)
Panel C. Subsample Controlling for Risky Behaviors: Smoking, Drinking					
0.209	-0.027	-0.409**	-0.207**	-0.436	-0.247**
(0.269)	(0.137)	(0.160)	(0.092)	(0.275)	(0.105)

The results reported here use the model specification described at Table 4. The sample is births to women with more than 12 years of education.

Table A7. The Impact of Diabetes Mandates on Infant Outcomes by Mother's Age-Race Demographic Group: DDD analysis, state/year/demographic group cells

	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			
All ages	0.271	0.092	-0.213	-0.162**	-0.240	-0.206**	-0.056
	(0.263)	(0.123)	(0.134)	(0.076)	(0.278)	(0.090)	(0.321)
Age <25	-0.951	0.002	0.109	0.070	0.429	0.328	-0.732
	(1.025)	(0.605)	(0.523)	(0.298)	(0.882)	(0.414)	(1.421)
25≥ Age >30	0.321	-0.243	-0.353	-0.172	-0.418	-0.276	-0.382
	(0.550)	(0.258)	(0.289)	(0.212)	(0.547)	(0.219)	(0.726)
30≥ Age >35	0.243	0.355	-0.143	-0.155	-0.789	-0.313*	-0.095
	(0.616)	(0.216)	(0.289)	(0.133)	(0.509)	(0.157)	(0.470)
Age ≥ 35	0.407	0.069	-0.215	-0.219	0.388	-0.157	0.528
	(0.534)	(0.297)	(0.348)	(0.132)	(0.522)	(0.196)	(0.671)
				Panel B: BLACK			
All ages	0.374	-0.213	-1.024**	-0.358	-0.951	-0.320	1.450**
	(0.458)	(0.326)	(0.472)	(0.250)	(0.582)	(0.293)	(0.577)
Age <25	1.224	0.111	0.462	0.252	0.649	-0.008	0.294
	(1.667)	(0.907)	(1.688)	(0.696)	(2.220)	(0.734)	(3.196)
25≥ Age >30	0.706	0.016	-0.045	0.226	-1.477	0.118	0.666
	(1.357)	0.871	(1.027)	(0.552)	(1.742)	(0.794)	(1.790)
30≥ Age >35	0.281	-0.286	-1.650	-0.682	-0.530	-0.383	2.157
	(0.947)	(0.859)	(1.248)	(0.646)	(1.123)	(0.710)	(1.648)
Age ≥ 35	-0.013	-0.470	-2.326*	-0.962	-1.902	-1.086	2.127
	(1.350)	(0.547)	(1.280)	(0.743)	(1.810)	(0.865)	(1.659)

The results reported here use the model specification described at Table 4.

Table A8. The Impact of Diabetes Mandates on Pregnancy Weight-Gain by Age-Race Demographic Group: DDD Analysis using Individual Level Data

	Weight Gain≥35 pounds	Weight Gain>35 pounds	Weight Gain>40 pounds
		Panel A: White	
Age <25	-0.004	-0.240	-0.300
	(0.990)	(0.954)	(0.854)
25≥ Age >30	-0.137	-0.310	-0.389
	(0.463)	(0.465)	(0.436)
30≥ Age >35	0.041	-0.231	-0.193
	(0.345)	(0.375)	(0.326)
Age ≥ 35	0.397	0.750	0.270
	(0.425)	(0.395)*	(0.318)
		Panel B: Black	
Age <25	1.073	2.640	1.886
	(2.094)	(2.122)	(1.801)
25≥ Age >30	1.659	1.844	1.374
	(1.311)	(1.078)*	(0.803)*
30≥ Age >35	2.471	1.929	1.698
	(1.215)**	(0.938)**	(0.967)*
Age ≥ 35	2.001	1.247	0.105
	(1.019)*	(0.920)	(0.664)

These results use the sample and model specification described at Table 5.

Table A9. The Impact of Diabetes Mandates on Singleton Birth Outcomes by Mother's Age-Race 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	-0.503	0.263	0.449	0.158	0.470	0.281	-0.230
	(0.657)	(0.424)	(0.408)	(0.150)	(0.536)	(0.194)	(1.081)
25≥ Age >30	0.446	-0.236	0.022	-0.083	-0.092	-0.122	0.008
	(0.401)	(0.168)	(0.196)	(0.103)	(0.401)	(0.100)	(0.473)
30≥ Age >35	0.278	0.326	-0.227	0.060	-0.690**	-0.153**	0.091
	(0.460)	(0.150)	(0.129)	(0.060)	(0.327)	(0.072)	(0.377)
Age ≥ 35	0.231	0.042	-0.060	0.146**	0.568	0.102	0.615
	(0.374)	(0.237)	(0.230)	(0.068)	(0.341)	(0.091)	(0.396)
				Panel B: BLACK			
Age <25	1.629	0.328	0.187	0.273	0.894	-0.095	1.604
	(1.179)	(0.637)	(0.903)	(0.342)	(1.265)	(0.447)	(2.043)
25≥ Age >30	1.241	-0.140	-0.253	0.065	-2.404**	-0.112	1.575
	(0.936)	(0.536)	(0.740)	(0.337)	(1.152)	(0.464)	(1.300)
30≥ Age >35	0.089	-0.444	-0.996	-0.396	-0.108	-0.185	2.343*
	(0.688)	(0.570)	(0.800)	(0.378)	(0.765)	(0.401)	(1.231)
Age ≥ 35	-0.223	-0.723**	-1.760**	-0.714	-0.813	-1.008**	1.452
	(0.920)	(0.361)	(0.878)	(0.473)	(1.276)	(0.438)	(1.039)

These results use the same model specification described at Table 5. The sample retains only singleton births.

Table A10. Effect of Diabetes Mandates by Likelihood of Treatment: Proportion of Population with Individually Purchased Private Health Insurance

	Diabetes•Mandate•Individually purchased private insurance
High Birth Weight	
>4000g	0.031
	(0.046)
>4500g	0.001
	(0.019)
Low Birth Weight	
<2500g	-0.031
	(0.023)
<1500g	0.010
	(0.013)
Premature Birth	
<37 weeks	-0.015
	(0.052)
<32 weeks	-0.009
	(0.019)

The results reported here use the sample and model specification described at Table 4.

Table A11. The Effect of Diabetes Mandate: Time since Implementation

	High Birth Weight		Low Birth Weight		Premature Birth	
	>4000 g	*>4500 g*	*<2500 g*	*<1500 g*	*<37 weeks*	*<32 weeks*
Diabetes• Mandate	0.104	0.081	-0.286*	-0.168**	-0.520*	-0.205*
	(0.250)	(0.118)	(0.160)	(0.078)	(0.278)	(0.106)
Diabetes• Time since Mandate	0.061	-0.001	-0.050	-0.034	0.111	-0.026
	(0.041)	(0.023)	(0.053)	(0.022)	(0.087)	(0.028)

The results reported here use the sample and model specification described at Table 4. The variable "Time since Mandate" is equal to 1 in the year following the effective date of the mandate, 2 in the second year following the mandate adoption, 3 in the third, etc, and 0 in all state-years with no mandate (the effective date is also coded 0 because of the lag effect of the mandate).

Sensitivity Analysis

First we test if the estimated effect of diabetes mandates is sensitive to changes in functional form and to our choice of coding the law. We find the results are robust to using log dependent variable[3] and to coding of the timing of impact. For instance, because in many states the effective date of diabetes mandates was either exactly on July 1st (the cut-off used to distinguish between a year with mandates and one without) (9 states) or immediately afterwards, in row 3 of Table A12 the diabetes mandates variable is coded 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. Reassuringly, the results obtained using this specification are substantially the same. Similarly, we find that our results are robust to using the enactment date. Note that statistically significant effects occur two years after enactment, to be expected given the lag between enactment and effective date.

We also test if secular differences in trends between adopting and non-adopting states (not already captured by controls) confound our results. In row 5 of Table A12, we find that our results are robust in regressions restricted to states that passed diabetes mandates. In Table 3 we also reported that the difference-in-difference estimates obtained on the sample of infants born to diabetic mothers are substantially the same with our main estimates. These results remove any concerns that potential secular differences between infant health of diabetic versus non-diabetic mothers (not already captured by controls) confound our estimates.

We further investigate whether our estimates are biased by potential endogeneity due to pre-existing trends by using leads of diabetes mandates in our main specification. The relevant timing of any effect from environment to adoption, however, is the time the law was enacted

[3] Because the dependent variable can take zero values, we add 1 (one) to the dependent variable before taking the log. We prefer the specification in levels because it is known the estimates are somewhat sensitive to the value added before taking the log.

because the problem is presumably that changes in outcomes led to the adoption of diabetes mandates. We find no evidence of a significant correlation between the enactment of diabetes mandates and prior infant outcomes providing support for our identification (Table A12, row 6).

We also check the correlation between infant outcomes and the leads of the effective date. It should be noted that if the effective date was after July 1 the law was coded 1 in the following year. Thus, for a not insignificant proportion of states[4] the lead of the effective date picks up 5-6 months in which the law was effective and thus affected pregnancies in their third trimester. In fact for 23 states the lead effective date covers at least some months in which the law was effective. Because high-glucose exposure in the third trimester drives high birth weight it is likely that the lead effect of the effective date captures some of the effect of actual exposure and thus is not a very good test of endogeneity.

We find the neither the first nor the second lead of the effective date are significant at conventional significance levels. However the coefficient of the first lead appear to be very similar with the coefficient of the effective date and significant at 10% significance levels in the case of high birth weight. It is possible that by picking up only the effect only during late pregnancy and none of the effect of care during early pregnancy the lead mandates actually shows what happens if medical care is provided only during late pregnancy. That would suggest that most of the gains from medical care might come from care during early pregnancy or from earlier diagnosis of diabetes.

The results are also robust to the sample choice. In the introduction we mentioned that the incidence of diabetes in population has increased. This could raise the concern that there may be changes in the characteristics of the treatment group before and after the introduction of

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[4] 10 states have a law becoming effective on July 1, and a total of 15 states have laws becoming effective in July-August

mandates that may confound our estimates.[5] To eliminate this concern in row 9 of Table A12 we concentrate on a smaller sample of years before and after the period when most diabetes mandates laws became effective, 1998-1999. Estimates obtained using the 1995-2001 data are substantially the same as those obtained using the entire sample, providing support for our identifying strategy.[6]

In addition, we test the robustness of the results to adding years. The initial choice of sample reflected the need to cover as many instances of reforms while maintaining a manageable sample to test our results on individual level data. Here we show that adding one year at the beginning of the sample and one year at the end does not alter the results.

[5] We also find no evidence that the mandates led to a change in share of births to diabetic mothers The estimated instantaneous association between the mandate adoption and 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.
[6] In addition, we found no evidence that the time passed since implementation matters for the magnitude of the effect (Table A11).

Table A12. Robustness Checks

	High Birth Weight		Low Birth Weight		Premature Birth	
	>4000 g	>4500 g	<2500 g	<1500 g	<37 weeks	<32 weeks
[1] Main	0.323	0.059	-0.339**	-0.192**	-0.335	-0.223**
	(0.238)	(0.113)	(0.151)	(0.080)	(0.281)	(0.099)
[2] Log Dependent Variable	0.018	0.011	-0.033*	-0.073**	-0.020	-0.073**
	(0.014)	(0.026)	(0.017)	(0.030)	(0.016)	(0.033)
[3] Different Law Coding	0.356	0.078	-0.322**	-0.183**	-0.304	-0.203*
	(0.247)	(0.116)	(0.144)	(0.087)	(0.295)	(0.107)
[4] Enactment, t+2	0.363	0.062	-0.288*	-0.178**	-0.348	-0.207**
	(0.237)	(0.115)	(0.146)	(0.080)	(0.283)	(0.100)
[5] Adopting states only	0.176	0.025	-0.286*	-0.135**	-0.176	-0.185*
	(0.218)	(0.117)	(0.164)	(0.065)	(0.299)	(0.100)
[6] Lead 1 enactment	0.163	0.023	0.049	-0.077	-0.296	-0.009
	(0.269)	(0.124)	(0.137)	(0.093)	(0.232)	(0.085)
[7] Lead 1	0.488*	0.193*	-0.076	-0.038	-0.164	0.000
	(0.249)	(0.112)	(0.121)	(0.092)	(0.255)	(0.095)
[8] Lead 2	0.159	0.001	0.013	-0.091	-0.287	-0.020
	(0.264)	(0.122)	(0.136)	(0.091)	(0.228)	(0.083)
[9] 1995-2001	0.202	-0.034	-0.365*	-0.271***	-0.091	-0.253**
	(0.250)	(0.147)	(0.187)	(0.080)	(0.292)	(0.121)
[10] 1991-2004	0.307	0.024	-0.301**	-0.193**	-0.315	-0.206**
	(0.251)	(0.115)	(0.141)	(0.075)	(0.289)	(0.083)

All regressions retain the sub-sample of infants born to mothers with more than 12 years of education. Unless otherwise specified these regressions use data from the period 1992-2003. All regressions control for mother's education, 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 and 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 state level are reported in parentheses.
* significant at 10% significance level; ** significant at 5% significance level, *** significant at 1% significance level

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