Diabetes Treatments and Moral Hazard

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Abstract

In the face of rising rates of diabetes, many states have passed laws requiring health insurance plans to cover medical treatments for the disease. Although supporters of the mandates expect them to improve the health of diabetics, the mandates have the potential to generate a moral hazard to the extent that medical treatments might displace individual behavioral improvements. Another possibility is that the mandates do little to improve insurance coverage for most individuals, as previous research on benefit mandates has suggested that mandates often duplicate what plans already cover. To examine the effects of these mandates, we employ a triple-differences methodology comparing the change in the gap in body mass index (BMI) between diabetics and nondiabetics in mandate and nonmandate states. We find that mandates do generate a moral hazard problem, with diabetics exhibiting higher BMIs after the adoption of these mandates.

1. Introduction

Diabetes is a growing concern in the United States. The Centers for Disease Control and Prevention (CDC) estimates that more than 17 million people have diabetes, and the incidence of the disease has been growing throughout the past decade (CDC 2002). Among the complications induced by the disease are blindness, kidney disease, amputations, cardiovascular disease, and a host of other life-threatening problems, placing diabetes as the sixth leading cause of death in the United States. The American Diabetes Association (ADA) estimates that the total cost of diabetes in 2002 in terms of direct medical care and indirect productivity losses amounted to $132 billion in the United States (ADA 2003).

In addition, analysts estimate that there are another 12 million Americans with a condition known as prediabetes (Benjamin et al. 2003). The term pre-

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Table 1
Adoption of Diabetes Mandate

State	Year	State	Year	State	Year
Alaska	2000	Maine	1996	Oregon	2001
Arizona	1998	Massachusetts	2000	Pennsylvania	1998
California	1981	Michigan	2000	Rhode Island	1996
Colorado	1998	Minnesota	1994	South Carolina	1999
Connecticut	1997	Mississippi	1998	South Dakota	1999
Delaware	2000	Montana	2001	Tennessee	1997
Florida	1995	Nebraska	1999	Texas	1997
Georgia	1998	Nevada	1997	Utah	2000
Hawaii	2000	New Hampshire	1997	Vermont	1997
Illinois	1998	New Jersey	1996	Virginia	1998
Indiana	1997	New Mexico	1997	Washington	1997
Iowa	1984	New York	1993	West Virginia	1996
Kentucky	1998	North Carolina	1997	Wisconsin	1987
Louisiana	1997	Oklahoma	1996	Wyoming	2001

diabetes covers individuals who are at a high risk for developing type 2 diabetes.[1] The upward trend of obesity witnessed over the past 2 decades suggests that the incidence of diabetes and prediabetes will continue to grow (Mokdad et al. 2003).

In this context, the legislatures of a majority of states have passed laws mandating that health insurance providers cover supplies, services, medications, and equipment for treating diabetes as part of their basic coverage without charging higher premiums for the coverage (see Table 1). Given the high cost of diabetes treatments, advocates such as the ADA view these mandates as necessary for ensuring that diabetics receive adequate health care.

As with most insurance coverage, these mandates have the potential to induce moral hazard problems. That is, because type 2 diabetes can largely be avoided through fastidious diet and exercise regimens, individuals facing the costs associated with diabetes have strong incentives to engage in healthful behavior. When the cost of medical treatments declines because of state mandates, the relative cost of behavioral prevention increases, inducing individuals to engage in worse diet and exercise practices. On the margin, this moral hazard increases the obesity incidence and eventually the diabetes incidence.

However, the mandates include coverage for self-management and education programs that have the potential to improve the health of diabetics. Mandated coverage for testing supplies has the potential to give diabetics improved awareness of their condition, inducing them to be more vigilant in their behavior. The education provisions of mandates might improve access for diabetics to dieticians and diabetes educators.

[1] The phase between normal blood sugar levels and levels denoting type 2 diabetes is classified as impaired glucose tolerance (IGT) or impaired fasting glucose (IFG). With IGT, the blood sugar level is elevated (in the range of 140–199 milligrams per deciliter after a 2-hour oral glucose tolerance test) but does not meet the standard for a type 2 diabetes diagnosis. With IFG, the fasting blood sugar level is elevated (in the range of 110–125 milligrams per deciliter after an overnight fast) but does not reach the type 2 diabetes threshold.

A third alternative is that these mandates do not actually change the coverage available to people, as some previous research suggests that insurers often already cover the benefits included in the mandates. If this is the case, we might expect that mandates do not change behavior unless passage of the mandate provides individuals with better information regarding the coverage they already have. Thus, the net effect of these mandates on individual health is ambiguous.

In this paper, we examine the health effects of diabetes mandates by focusing on individuals' body mass indexes (BMIs) for the period 1996–2000, during which 34 states adopted mandates, by employing a triple-differences research design in which we compare the change in the BMI gap between diabetics and nondiabetics when a mandate is passed relative to the contemporaneous change in the diabetic/nondiabetic gap in nonmandate states. We find that mandates generate a statistically significant increase in the BMI of diabetics and that the effect is of practical significance. Specifications that insufficiently control for factors that lead to the adoption of mandates generate spurious positive (that is, decreases in BMI) treatment effects.[2]

In Section 2 of the paper, we discuss the existing literature on the economics of obesity and diabetes. Section 3 provides the theoretical context for the expected effect of diabetes mandates on behavior. Section 4 discusses our data and research design. Results are presented in Sections 5 and 6, followed by concluding remarks.

2. Economics of Obesity and Diabetes

Perhaps owing to the recent trends in body weight, the topic of obesity has gained much attention in the economics literature lately. Philipson and Posner (2003) argue that the increase in obesity witnessed in the United States and worldwide is a function of technological progress. That is, as technology has lowered the price of food and has reduced the amount of on-the-job exercise that typically takes place in modern American occupations, individuals consume relatively more calories compared with the calories they expend than they did in the past. This net increase in caloric intake more than offsets the effects of increased dieting and recreational exercise.

In an extension of the basic Philipson and Posner framework, Lakdawalla and Philipson (2002) test the major implications of the technological model of obesity. They find strong evidence that lower food prices, resulting from improvements in agricultural technology, do lead to a statistically significant increase in body weights. Further, they provide some evidence that declining occupational physical activity is also an important contributor to the increase in body weights.

Cutler, Glaeser, and Shapiro (2003) also adopt the technological explanation for the rise in obesity, but they focus on the distribution of the increases in body weights. They identify that the biggest technologically based increase in calorie

[2] A previous version of this paper did not sufficiently control for this endogeneity and reported only the spurious treatment effects.

consumption is exhibited in the heavy tail of the weight distribution. That is, the increases in weight have been most pronounced for relatively heavy individuals. To explain this, they invoke a self-control model in which overweight individuals have difficulties limiting their consumption when food prices decrease. They argue that price decreases are actually welfare reducing for this segment of the population.

Chou, Grossman, and Saffer (2004) provide a separate economic explanation for the increase in U.S. obesity rates. They use state data on the number of restaurants in an individual's home state and information regarding the price of meals in various restaurants to explain a large proportion of the variation in individuals' BMIs. Although, as the authors admit, this approach potentially suffers from a simultaneity bias, their results suggest that individuals facing markets with relatively many restaurants and low food prices exhibit higher BMIs and obesity incidence. They go on to argue that changing labor market opportunities for women are at the root of this effect. Basically, in years past, mothers controlled the diets of families fairly effectively, but as more women entered the workforce, families substituted with more preprepared and restaurant meals, which are relatively unhealthful. They also attribute a large portion of the increase in obesity to declining smoking rates.

The rise in obesity is not troubling per se. However, it is viewed as a public health problem to the extent that obesity is a strong predictor for a number of costly health problems. Although obesity is linked with a host of physical problems, its connection with diabetes is especially strong. In fact, type 2 diabetes is almost completely limited to the overweight and obese. This implies that the economic models of obesity also indirectly apply to diabetes.

Diabetes does present some interesting questions that are distinct from the general issue of obesity. Specifically, while exercise and healthful diets can lower the likelihood of both obesity and diabetes, there are also medical substitutes for these behavioral treatments in the case of diabetes. Kahn (1999) highlights how both behavioral modifications and medical treatments have significantly improved the quality of life for diabetics. One particular concern for Kahn is the possibility that diabetic individuals substitute medical treatments for behavioral modifications. That is, do medicated diabetics become less fastidious in various behaviors that increase their chances of developing complications from diabetes, such as smoking and eating behaviors? While Kahn finds no evidence of this substitution in his analysis, he notes that clinical diabeticians express concern that improved access to medications for diabetes might lull individuals into a false sense of security, causing them to ignore behavioral prescriptions.

Similar offsetting behavior has been documented in many other contexts in the economics literature (see, for example, Peltzman 1975; Viscusi 1984). In the case of diabetes, the possibility of offsetting behavior raises questions about the ultimate aggregate effect of increasing access to medical treatments for diabetes. Specifically, since complications from diabetes represent the costs of poor health habits, the prospect of developing diabetes induces individuals, on the margin, to engage in more healthful behavior. Laws requiring insurers to cover medical treatments for diabetes effectively subsidize less healthful behavior, potentially leading more individuals to develop prediabetes and diabetes than would be the case in the absence of these laws.[3]

3. Diabetic Behavior

We model a diabetic's behavior as involving a choice to manage his or her disease either through behavior modification or through medical treatments. For simplicity, we constrain behavior modification to involve simply a choice regarding how many units of unhealthful food (f) to consume at the nominal price p_f.[4] Consumption of unhealthful food also increases the utility cost of diabetes (D). The diabetic also chooses how many units of medical treatments to consume (m) at price p_m. Medical treatments do not enter the utility function directly, but they lower the utility cost of diabetes. Thus, the diabetic individual with income I faces the following optimization problem:

$$\max_{f,m} U(f) - D(f, m) + \lambda(I - p_f \times f - p_m \times m), \tag{1}$$

which yields the following first-order conditions:

$$\frac{\partial U}{\partial f} - \frac{\partial D}{\partial f} - \lambda p_f = 0 \tag{2}$$

and

$$-\frac{\partial D}{\partial m} - \lambda p_m = 0. \tag{3}$$

Substituting equation (3) into equation (2) yields

$$\frac{\partial U}{\partial f} - \frac{\partial D}{\partial f} + \frac{p_f}{p_m}\frac{\partial D}{\partial m} = 0. \tag{4}$$

By the implicit function theorem, then

$$\frac{\partial f}{\partial p_m} = -\left(p_f \times \frac{\partial D}{\partial m}\right) \bigg/ \left(\frac{\partial U^2}{\partial^2 f} - \frac{\partial D^2}{\partial^2 f}\right), \tag{5}$$

which implies that, as long as the individual's utility function is concave in food consumption and the incremental effect of food consumption on the severity of diabetes costs is either constant or increasing (or even decreasing at a relatively

[3] This is simply an application of the concept of moral hazard. Empirical analyses of the potential for moral hazard in the insurance context can be found in Klick and Stratmann (2003, 2006). For a general discussion of moral hazard arising from regulatory activity, see Klick and Mitchell (2006).

[4] The intuition of the model does not change if we allow for a choice over healthful and unhealthful foods or if we add an exercise component.

low rate), as the cost of medical treatments declines, the diabetic individual will consume more unhealthful food. That is, we get the intuitive result that as the price of medical treatments drops in relative terms, a rational individual will substitute away from behavior modifications as a way of managing diabetes.[5] Mandates requiring that medical treatments for diabetes are included in basic insurance coverage effectively lower the price of those treatments. Thus, we might expect that mandates produce deleterious health effects.

However, given the high cost of providing medical treatments for diabetes,[6] insurers may focus much of their efforts on the proactive aspects of the mandates, such as the coverage of consultations with dieticians and the provision of self-management supplies. Because mandates restrict insurers from pricing the diabetes risk into their premiums, insurers might engage in active preventive management to mitigate the risk posed by diabetes mandates.[7] Active management has the potential to reap large cost savings with respect to diabetes since behavioral modifications significantly reduce diabetes incidence.[8] Improving access to devices that monitor an individual's blood sugar level has the potential to make diabetics more aware of their condition, improving their compliance with the diet and exercise directives issued by doctors. Further, covering the cost of education programs could make a doctor more likely to suggest that a patient visit a professional dietician or diabetes educator. Even if doctors regularly suggest education programs, insurance coverage might make it more likely that patients will follow through on the suggestion (Guglielmo 2001).

However, with respect to self-management and education, if these options are effective in improving the behavior of diabetics, arguably, insurers would be likely to cover them even in the absence of a mandate. As indicated above, complications from diabetes, which would generally be covered by an insurer even if it excluded direct diabetes treatments, tend to be very expensive, making prevention and mitigation potentially good investments. Thus, it could be the case that mandating coverage for self-management supplies and education is superfluous.

Specifically, assume that there is a preventive treatment that costs an insurer c per period to provide. This treatment guarantees that its customer will not

[5] Gary Becker has recently offered a similar explanation of why Americans in general remain fat. In effect, he argues that individuals rationally expect science to advance to the point where medical technology can alleviate the negative health effects of obesity (Reuters News Service 2005).

[6] Peele, Lave, and Songer (2002) estimate that health care expenditures by insurers were 3 times higher for diabetics compared with all consumers in the examined health plans.

[7] Another avoidance strategy is raised by Summers (1989). He argues that, in the presence of mandates, if employers cannot adjust wages to account for differential benefit costs, they will seek to hire low-risk employees. Jensen, Cotter, and Morrisey (1995) demonstrate that another avoidance strategy employed by firms is to self-insure so that state mandates are preempted by the Employee Retirement Income Security Act of 1974 (Pub. L. No. 93-406, 88 Stat. 829), although their results suggest that firms had stopped moving toward self-insurance as a strategy to avoid the burden of state mandates by the mid-1980s.

[8] Hu et al. (2001) find that more than 90 percent of cases of type 2 diabetes could be prevented by the adoption of a more healthful lifestyle.

develop diabetes. Further, assume that in the event the customer does not receive the preventive treatment, his or her likelihood of developing diabetes is represented by the probability distribution function $p(t)$. That is, the likelihood of developing diabetes is only a function of time (t) and $\partial p / \partial t > 0$.

If the customer develops diabetes, the insurer will incur per-period cost m. If the insurer provides the preventive treatment, it can charge an additional premium of h.[9] Assuming the insurer has discount rate r and the insured customer is covered by the insurer from period 0 to period T, the insurer's decision rule for whether it provides the preventive treatment is to provide the treatment when the following condition is met:

$$\int_0^T e^{-rt} m p(t)\, dt \geqslant \int_0^T e^{-rt} (c - h)\, dt. \tag{6}$$

In work examining other kinds of insurance mandates, Gruber (1994a) has found that mandates generally do not expand coverage because employers already often cover the services that are the subject of the mandate.[10] If plans already cover diabetes treatments, the mandates could still have an effect if customers are generally ignorant about their coverage and mandates make them aware that they do have coverage.[11]

Diabetes coverage might be slightly different in this regard, however. That is, given the structure of the disease, preventive efforts that might be cost justified over a patient's lifetime might not be a good investment from the standpoint of an insurer. Because the major costs of diabetes complications arise primarily in old age, insurers might rationally calculate that the benefits of preventive treatments will be reaped by Medicare rather than accrue to the insurer. Even if it is likely that the complication will arise before the customer reaches Medicare age, insurers might hesitate to cover preventive care if there is substantial movement in and out of insurance plans.[12]

Under these conditions, it will not be possible for a given insurer to internalize the benefits of preventive care. In that case, mandates may serve as a coordination mechanism inducing insurers to cover preventive treatments that are cost justified in a social sense.

[9] Note it may not be possible to set h at the level at which all cost-justified preventive treatments are provided because of regulatory constraints on pricing or differentials between the discount rates of the customers and those of the insurer.

[10] Gruber's research did not include diabetes mandates, and there is some limited evidence that such mandates are different in this regard. For example, Pollitz et al. (2005, pp. 36–37) document a number of state reports that find that diabetes benefit mandates will increase coverage for state residents because the mandates go beyond what insurers already cover in general, although they note that insurers in Maine did not expect to have to change coverage very much.

[11] Consumer ignorance of coverage can impede patients from availing themselves of important preventive treatments. See, for example, Parente, Salkever, and DaVanzo (2005).

[12] Pollitz et al. (2005) note that the majority of individual health insurance policies are held for less than 2 years.

4. Research Design

The adoption of diabetes mandates provides us with the opportunity to examine the incentive effects of increased treatment access on the behavior of individuals. In general, isolating the causal effect of treatment availability is difficult, since improved health technology represents a shock in availability to everyone, which leaves analysts without a control group against which to measure the marginal effect of improved access. If one focuses not on technology but rather on price changes, as is the case in expanded insurance coverage, there is the potential that election of insurance and personal health behaviors are jointly determined.

With the adoption of mandates, however, the exogenous increase in access to diabetes treatments that applies to individuals in the adopting state also provides us with an interesting quasi-experiment. Specifically, within a state, we can examine the change occasioned by passage of a mandate in the gap between BMI exhibited by diabetics controlling for contemporaneous changes in the state as observed in nondiabetics in the state. Further, we can control for time effects that are unrelated to the adoption of insurance mandates by using diabetics and nondiabetics in nonmandate states as controls.

We use individual-level data from the Behavioral Risk Factor Surveillance System (BRFSS) for the years 1996–2000 to analyze the effects of diabetes mandates. We chose 1996 as our starting point because it represents the first year that all states took part in the BRFSS.[13] Our measure of health is BMI.[14] Body mass index is a normalized weight metric used to classify an individual's weight status. Individuals with BMIs 25 and above are considered overweight, while patients with a BMI of 30 or greater are considered obese.

We estimate the regression

$$\begin{aligned}\text{BMI}_{ijt} = {} & \alpha \times \text{Diabetic}_{it} \times \text{Mandate}_{jt} + \beta \times \text{Mandate}_{jt} \\ & + \delta \times \text{Diabetic}_{it} + \Theta \times \boldsymbol{X}_{it} + \rho_i + \tau_t + \upsilon_j + \varepsilon_{ijt},\end{aligned} \tag{7}$$

where BMI represents individual i's BMI calculated from his or her survey responses regarding height and weight at time t. The Diabetic × Mandate interaction takes the value of one if the individual's state of residence (j) has a mandate in effect during survey year t and if the individual has diabetes. The Mandate variable takes the value of one if the individual's state has a mandate in effect regardless of whether the individual has diabetes (and is affected by the mandate) or not (and is not affected by the mandate). The variable Diabetic takes the value of one if the individual is diabetic to control for the fact that diabetics, whether covered by mandates or not, tend to exhibit higher BMIs.

[13] Centers for Disease Control and Prevention, Behavioral Risk Factor Surveillance System (http://www.cdc.gov/brfss/). We chose 2000 as our endpoint because after that year some of the variables we use in our analysis were no longer collected.

[14] BMI = [(Weight in pounds) / (Height in inches)^2] × 703.

The vector $\boldsymbol{X}$ has individual-level covariates, ρ represents a time-invariant race effect corresponding to i's reported race, τ represents the effect of year t that is common to all individuals surveyed in the same year as i, and v represents a time-invariant state effect that is common for all individuals living in state j. We also examine specifications in which we control for state-specific trends and other specifications for which we allow for state-specific year dummies.

For our covariates, we include the individual's age and age squared, recognizing that individuals tend to gain weight as they age but then reach an age at which weight actually declines. We also include income and income squared, expecting that thinness is a normal good in the United States but that at some point the effect of food being a normal good as well might overwhelm the demand for thinness.[15] We include the individual's education level since education serves as a proxy for an individual's subjective discount rate (Fuchs 1982). We expect that individuals with low discount rates will invest in both education and health. We also control for whether an individual is unemployed since unemployed individuals are likely to be less active than their employed counterparts, conditional on income levels.

We also control for the individual's insurance status, recognizing that the choice to buy insurance might correlate with health preferences. Another measure of health preferences that we include is whether the individual smokes cigarettes. Finally, we control for a number of other lifestyle attributes such as whether the individual is married, separated, or divorced, the number of children the individual has, the gender of the individual, and whether the individual is pregnant at the time of the survey. Descriptive statistics are presented in Table 2.

If the moral hazard effect of the diabetes mandates dominates, we should observe a positive coefficient on the Diabetic × Mandate interaction term, and we might expect a positive coefficient on the mandate term in general if nondiabetics rely on their expectation of insurance coverage in the event that they develop diabetes in the future. However, if the mandates are successful in improving the health of diabetics, we should observe a negative coefficient on the Diabetic × Mandate term.

5. Results

We present the results of the regressions described in Table 3. In the specification including general year dummies (column 1), the treatment group (Diabetic × Mandate) exhibits a BMI reduction of .4, which represents a decrease of about 2 percent, and the result is statistically significant at the 1 percent level. Interestingly, the nondiabetic population in mandate states appears to exhibit the effects of moral hazard, as the passage of the mandate increases BMI among this group by .07. Although the effect is statistically significant at the 1

[15] Philipson and Posner (2003) argue that the quadratic will imply increasing weight at low income levels and decreasing weight at higher income levels. However, given the relative wealth of the United States, we do not expect to find such a relationship in this data.

Table 2
Descriptive Statistics

Variable	Description	Mean	SD
BMI	Body mass index		
Total sample		26.052	5.117
Diabetics excluded		25.850	4.957
Diabetics only		29.611	6.415
Diabetic	Equals one if diabetic	.054	.226
Mandate	Equals one if in a mandate state	.563	.496
Diabetic × Mandate	Equals one if individual lives in a mandate state and is diabetic	.032	.178
Income	Income ($1,000s)	38.649	21.643
Age	Age (years)	46.669	17.378
Female	Equals one if female	.590	.492
Pregnant	Equals one if currently pregnant	.014	.119
Education	Education level reported (scale of 1–6)	4.665	1.097
Smoker	Equals one if smoker	.237	.447
Married	Equals one if married	.542	.498
Separated/divorced	Equals one if divorced or separated	.157	.364
Children	Number of children (ages 18 and under)	.734	1.138
Unemployed	Equals one if currently unemployed	.034	.181
Insured	Equals one if insured	.877	.329
Contribution prohibition	Equals one if state currently prohibits corporations from making campaign contributions to state legislators	.407	.491
Term limit	Equals one if state currently limits the amount of time an individual can serve in the state legislature's lower house	.177	.382

Source. All data are from Centers for Disease Control and Prevention, Behavioral Risk Factor Surveillance System (http://www.cdc.gov/brfss/), for the years 1996–2000, except for contribution prohibition (Feigenbaum and Palmer 1980–2000) and term limit (National Conference of State Legislatures 2006).

percent level, the relative effect is very small (.2 percent). The coefficients on the covariates all yield the expected results. In total, the regression explains almost 10 percent of the variation in BMI.

We introduce state-specific trends in the specification presented in column 2. The Diabetic × Mandate interaction coefficient does not change in size or statistical significance, as it still implies a treatment effect of the mandates of about a 2 percent reduction in BMI. The moral hazard effect in the nondiabetic population of mandate states, however, loses statistical significance. The results for the other coefficients are unaffected, and we continue to explain about 10 percent of the variation in the data.

Because of the large size of our data set, we are able to include an additional specification that controls for state-specific year effects. We present results with these controls in column 3. Again, we find a treatment effect among diabetics in mandate states of about 2 percent. This reduction is statistically significant at the 1 percent level. We continue to explain about 10 percent of the data's variation, and the coefficients on the covariates are largely robust to this specification.

Table 3
Effect of Diabetes Mandates on Body Mass Index

Variable	(1) Coefficient	(1) Robust SE	(2) Coefficient	(2) Robust SE	(3) Coefficient	(3) Robust SE
Diabetic × Mandate	−.404	.092	−.411	.092	−.404	.092
Mandate	.071	.027	−.047	.034	. . .	
Diabetic	3.043	.067	3.047	.067	3.041	.067
Income	−.021	.002	−.022	.002	−.022	.002
Income^2	.004	.002	.004	.002	.004	.002
Age	.312	.003	.313	.003	.312	.003
Age^2	−.003	.000	−.003	.000	−.003	.000
Female	−1.407	.015	−1.389	.015	−1.408	.015
Pregnant	.963	.065	.956	.065	.963	.065
Education	−.289	.008	−.290	.008	−.289	.008
Smoker	−.704	.016	−.700	.016	−.703	.016
Married	.065	.020	.060	.020	.063	.020
Separated/divorced	−.394	.025	−.399	.025	−.396	.025
Children	.056	.007	.054	.007	.055	.007
Unemployed	.094	.045	.091	.045	.092	.045
Insurance	.063	.024	.064	.024	.065	.024
Time control	Year dummies		State trends		State-year dummies	
Adjusted R^2	.098		.098		.098	

Note. The dependent variable is BMI as reported in Centers for Disease Control and Prevention, Behavioral Risk Factor Surveillance System (http://www.cdc.gov/brfss/), for the years 1996–2000. All regressions include state and race effects. The coefficient for Income^2 has been multiplied by 100 for presentation. $N =$ 466,805.

6. Is the Effect Causal?

The identification strategy used above relies on the exogenous adoption of mandates by states. That is, if the decision to adopt a diabetes mandate depends on the expectations of a state legislature regarding the health of diabetics in their state, then our treatment effect would suffer from a simultaneity bias. For example, if a legislature observes indications that the health of diabetics is getting worse and it decides to pass a mandate to mitigate the health problems of diabetics on that basis, then the estimated treatment effect would exhibit a downward bias. On the other hand, if insurers tend to fight benefit mandates that are costly to them, mandates might pass only in those states in which insurers observe indications that the health of diabetics is getting better. In that case, the estimated treatment effect would exhibit an upward bias.

To rule out the potential for simultaneity, we exploit the differences-in-differences-in-differences model (DDD) introduced by Gruber (1994b). This model imposes less restrictive assumptions regarding the exogeneity of the policy shock in that it controls for trends that are specific to diabetics as well as any idiosyncratic attributes that differentiate the diabetics in mandate states from diabetics in nonmandate states.

Following Gruber, we initially focus attention on two subsets of states: (1) the treatment group includes those eight states that adopted mandates in 1998, which

Table 4

Effect of Diabetes Mandates on Body Mass Index: Nonadopting States and States Adopting in 1988

Variable	Coefficient	Robust SE
β_8 (Treatment effect)	1.716	.296
β_7 (Diabetics in mandate states)	−1.827	.200
β_6 (Diabetics 1998+ effect)	1.502	.208
β_5 (Mandate states 1998+ effect)	.105	.049
β_4 (Diabetics)	2.786	.173
β_3 (Mandate state effect)	−.037	.036
β_2 (1998+ Effect)	−.617	.032

Note. Results are from a triple-differences model using only nonadopting states and states that adopted mandates in 1998. Data are from Centers for Disease Control and Prevention, Behavioral Risk Factor Surveillance System (http://www.cdc.gov/brfss/), for the years 1996–2000. In addition to the controls presented here, this model includes the covariates presented in Table 3, and the estimated coefficients were qualitatively similar. $N = 174{,}318$; $R^2 = .096$.

is the midpoint of our sample, and (2) the eight states that did not adopt mandates before or during our sample period. We then estimate the following model:

$$\begin{aligned}\mathrm{BMI}_{ijt} = {} & \beta_1 \boldsymbol{X}_{ijt} + \beta_2\tau_t + \beta_3\delta_j + \beta_4\mathrm{Diabetic}_i + \beta_5(\delta_j \times \tau_t) \\ & + \beta_6(\tau_t \times \mathrm{Diabetic}_i) + \beta_7(\delta_j \times \mathrm{Diabetic}_i) + \beta_8(\tau_t \times \delta_j \times \mathrm{Diabetic}_i),\end{aligned} \tag{8}$$

where i indexes individuals, t indexes the time period (where zero stands for years before the mandate passes in 1998 and one stands for 1998 and later), and j indexes states (where one stands for states that pass a diabetes mandate in 1998 and zero stands for states that do not pass mandates). Collapsing our data into these groupings (as does Gruber) allows for a more direct application of the treatment/control framework. The vector $\boldsymbol{X}$ stands for the observable variables we control for in Table 3; τ represents a fixed post-treatment-year effect common to all observations occurring in 1998 or later, and δ controls for fixed differences between states that adopt mandates and states that do not and is common to all observations in states that pass mandates in 1998. The variable Diabetic again measures whether an individual is diabetic and therefore captures any fixed BMI differences between diabetics and nondiabetics. The interaction carrying the β_5 coefficient controls for any time effect that is common to all individuals in mandate states after adoption of the mandate. The β_6 coefficient controls for any time effect that is common to all diabetic individuals after adoption of the mandate. The β_7 coefficient controls for any idiosyncratic differences common to diabetic individuals in mandate states that are constant pre- and postadoption. Thus, β_8 represents the causal treatment effect, as it isolates the effect of the mandate on a mandate-state diabetic.

We present the results of this model in Table 4. Interestingly, this more powerful model indicates that the treatment effect of diabetes mandates is to increase the BMI of affected diabetics by 1.7 points, which is an increase of almost 6 percent,

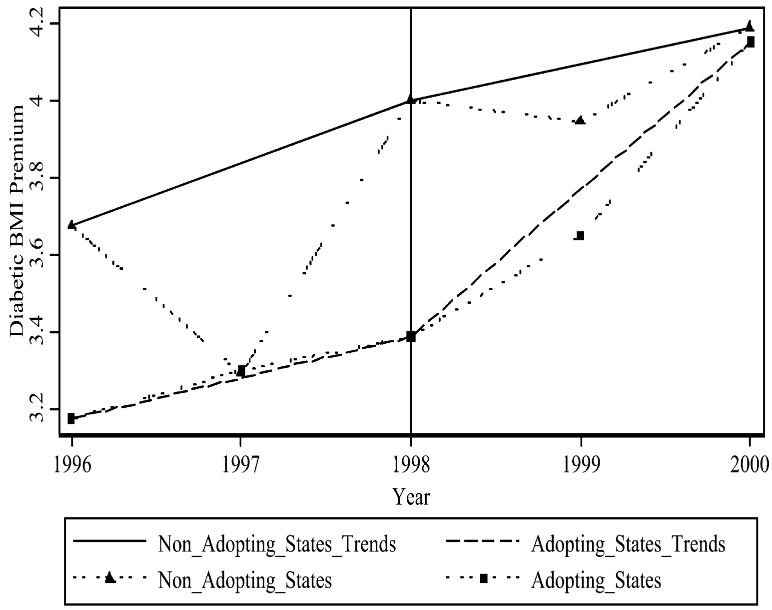

Figure 1. Trends in diabetic body mass index premium

and the effect is statistically significant at the 1 percent level. Examining the coefficients of the interactions provides some insight into the bias present in our earlier estimates. Specifically, it appears as though the mandate states as a group had diabetic residents who were relatively healthy compared with the diabetics in nonmandate states. Further confounding our results were the facts that mandate states experienced an upward trend in BMI among nondiabetic residents and that diabetics in general exhibited increases in BMI. This is also demonstrated in Figure 1, which provides two piecewise linear graphs[16] of the "diabetes premium" in BMI (that is, average diabetic BMI − nondiabetic BMI) for the eight states adopting mandates in 1998 and the eight states that did not adopt mandates during our time period.[17]

Although these results are, at a minimum, evidence that our earlier results contain a serious bias, the question of whether these mandates generally created a moral hazard problem deserves more attention. It could be the case that

[16] We generated linear trends for both groups for the periods 1996–98 and 1998–2000 to identify the change in trends occurring in 1998 when the adopters implemented mandates.

[17] We also graphed the diabetes premiums for each of the eight adopting states to ensure that the effect is not completely driven by a single state. From those graphs, it appears as though five of the adopting states exhibit the pattern (Arizona, Georgia, Illinois, Mississippi, and Pennsylvania), while one appears to simply continue an upward trend around 1998 (Virginia), and the two remaining states exhibit the opposite effect (Colorado and Kentucky). These graphs are available from the authors on request.

Table 5
Effect of Diabetes Mandates on Body Mass Index: All States

Variable	Coefficient	Robust SE
ψ_6 (Treatment effect)	.401	.126
ψ_5 (Diabetics in mandate states)	−1.104	.137
ψ_3 (Diabetic)	3.135	.160

Note. Results are from a triple-differences model using all states. Data are from Centers for Disease Control and Prevention, Behavioral Risk Factor Surveillance System (http://www.cdc.gov/brfss/), for the years 1996–2000. In addition to the controls presented here, this model included the covariates presented in Table 3, and the estimated coefficients were qualitatively similar. This model also includes diabetic-specific year dummies and state-specific year dummies. $N = 466{,}805$; $R^2 = .099$.

restricting our attention to only 16 states distorts our view of what effect mandates have. Perhaps these states were systematically different than other states. Further, we fail to exploit some available variation by compressing our 5 years of data into a simple before and after structure. Also, collapsing all states into the distinction between mandate and no-mandate states disregards any idiosyncratic differences that exist within the groups.

To mitigate these concerns, we use Gruber's DDD intuition, but we drop the data structure he uses. Instead, we examine all states using the following model:

$$\begin{aligned}\mathrm{BMI}_{ijt} = {} & \psi_1 \boldsymbol{X}_{ijt} + \psi_2(\tau_t \times \delta_j) + \psi_3 \mathrm{Diabetic}_i + \psi_4(\tau_t \times \mathrm{Diabetic}_i) \\ & + \psi_5(\delta_j \times \mathrm{Diabetic}_i) + \psi_6(\tau_t \times \delta_j \times \mathrm{Diabetic}_i),\end{aligned} \tag{9}$$

where the model has a structure similar to that of the DDD model presented above, where i indicates an individual, t denotes a year (that is, we no longer collapse all years into pre- or post-1998), and j indicates the state of residence of the individual (that is, we do not collapse into mandate or nonmandate states). Again we control for observable differences across individuals with the $\boldsymbol{X}$ vector. Instead of time and state effects, in this model we allow for state-specific year effects with the ψ_2 interaction. We again control for a diabetic-specific effect with ψ_3. We also allow for separate diabetes year effects with ψ_4. This control will capture any national changes in diabetic treatment such as innovations in diabetes pills or new diet directives from the CDC. The variable ψ_5 controls for baseline differences in the diabetic populations of states that eventually adopt mandates, and ψ_6 isolates our treatment effect (that is, the change in diabetic BMI after adoption of a mandate relative to contemporaneous changes relative to BMI baseline in the state as a whole and relative to contemporaneous changes in diabetic BMI nationally, conditional on variation in the observed covariates).

We present the results from this less restrictive model in Table 5. Our estimated treatment effect is an increase in BMI among diabetics in mandate states of .4 points, which represents an increase of 1.4 percent. This result is statistically significant at the 1 percent level.

Thus, our more powerful statistical models indicate that the true causal effect of passing diabetes mandates is to generate a moral hazard such that diabetics rely more on medical treatments for their disease than on improvements in their diets or exercise patterns.

It is likely that our estimated treatment effect is biased toward zero since mandates apply only to a subset of a state's population because of federal preemption under the Employee Retirement Income Security Act of 1974 (ERISA), which largely exempts self-insured employers' health insurance plans from state mandates. Unfortunately, it is impossible to know from the data collected in BRFSS whether an individual's health insurance is covered under ERISA. Also, there are no comprehensive state-level data tracking the proportion of a state's population that falls under ERISA, which makes it impossible to design a credible index for a more precise mandate variable (Klick and Markowitz 2006). In effect then, our estimated treatment effect should be viewed as a pooled estimate in which the effect of mandates on the individuals to which the mandate applies is averaged with a zero effect for all the individuals falling under ERISA preemption. It is likely then that the true causal effect is somewhat larger than the BMI increase described above.

The BRFSS does contain one potential proxy for ERISA status. During the years of our analysis, the BRFSS asked individuals where they obtained their insurance. If we assume that those individuals who answered that they received their coverage through their employer (or spouse's employer) are less likely to fall under state mandates because of ERISA preemption relative to those individuals who indicated that they bought their insurance independently (as indicated by Jensen, Cotter, and Morrisey [1995]), we might be able to estimate a more precise treatment effect if we use the self-purchase individuals in mandate states as the treatment group, with employer-insured and uninsured individuals as the within-state control.[18] For this analysis, we focus only on diabetics since it is only non-ERISA-preempted diabetics who are affected by state mandates. Because this restriction limits our sample size, it is not possible to estimate the less restrictive DDD model presented in equation (3). Instead, we once again employ Gruber's pooling method, estimating

$$\begin{aligned}\mathrm{BMI}_{ijt} = {} & \gamma_1 \boldsymbol{X}_{ijt} + \gamma_2 \tau_t + \gamma_3 \delta_j + \gamma_4 \text{Independent Insurance}_i + \gamma_5 (\delta_j \times \tau_t) \\ & + \gamma_6 (\tau_t \times \text{Independent Insurance}_i) + \gamma_7 (\delta_j \times \text{Independent Insurance}_i) \\ & + \gamma_8 (\tau_t \times \delta_j \times \text{Independent Insurance}_i), \end{aligned} \tag{10}$$

in which, once again, the time dimension is collapsed into two periods, pre-1998 and 1998 onward (with γ_2 measuring the period effect), and we restrict

[18] Note that mandates in general did not exempt nongroup policies from the coverage requirements. For summaries and statute citations for the relevant laws, see National Conference of State Legislatures, State Laws Mandating Diabetes Health Coverage (http://www.ncsl.org/programs/health/diabetes.htm).

Table 6

Effect of Diabetes Mandates on Body Mass Index with Independent Insurance as a Proxy for Mandate Coverage

Variable	Coefficient	Robust SE
γ_8 (Treatment effect)	2.922	1.432
γ_7 (Independently insured in mandate states)	−1.674	1.102
γ_6 (Independently insured 1998+ effect)	2.026	1.101
γ_5 (Mandate states 1998+ effect)	−.143	.321
γ_4 (Independently insured)	−1.822	.773
γ_3 (Mandate state effect)	−.218	.239
γ_2 (1998+ Effect)	−1.088	.251

Note. Results are from a triple-differences model using only diabetics in nonadopting states and states that adopted mandates in 1998. Data are from Centers for Disease Control and Prevention, Behavioral Risk Factor Surveillance System (http://www.cdc.gov/brfss/), for the years 1996–2000. In addition to the controls presented here, this model included the covariates presented in Table 3 except diabetic, and the estimated coefficients were qualitatively similar. $N = 6{,}814$; $R^2 = .165$.

attention to only those eight states passing mandates in 1998 and states that passed no mandate before or during our period of analysis. As before, states are treated as falling within the mandate or nonmandate group; thus γ_3 measures the time-invariant group effect. Independent Insurance represents a dummy variable that indicates that the individual purchased his or her insurance independently of his or her employer or spouse's employer. The variable γ_5 captures the mandate group postmandate time effect, γ_6 controls for the independent insurance post-1998 time effect, and γ_7 controls for any idiosyncratic differences regarding the independent insurance group in mandate states; γ_8 then represents the treatment effect.

If our non-ERISA proxy does provide us with more precision regarding who is covered by state mandates, we should estimate a treatment effect that exceeds the increase in BMI of 1.7 points that we estimated in Table 4. We present the results of this potentially more precise model in Table 6. Our estimates suggest a moral hazard effect of 2.9 points among the independently insured individuals affected by mandates. This result is statistically significant at the 5 percent level, and it represents a relative BMI increase of almost 10 percent.

One final robustness check that we performed involved endogenizing the adoption of a diabetes mandate. Given that the treatment effect estimated in our earlier models involved interaction terms to allow us to use unaffected individuals in that state as a within-state control group, we will not be able to duplicate those models in an instrumental variables (IV) framework. To implement an IV model, we restricted our attention to diabetics only, performing a simple difference-in-difference model that compares the change in diabetic BMI occasioned by the passage of a benefit mandate relative to contemporaneous BMI changes in the diabetic population of nonmandate states. For our instruments, we investigate the use of (1) an indicator measuring whether the state had restrictions in place that bar corporations from making campaign contributions to state legislators and (2) an indicator for whether the state has term limits in place for its state legislature.[19]

The intuition behind our first instrument involves the fact that insurers generally oppose benefit mandates and are likely to lobby against them. If insurers are prohibited from making contributions to legislators, their lobbying efforts will be less likely to be successful. The second instrument captures the likelihood that term limits change legislators' incentives. Specifically, while facing the discipline of elections, legislators may be more beholden to populist interests, like expanding health insurance coverage. However, if legislators are faced with the prospect of having to find a job in the private sector or go into business for themselves when term limits bind, they may be less willing to impose additional costs on businesses through mandated benefits.

While intuitively it seems as though these two variables are unrelated to the health characteristics of state residents, one might worry that these variables correlate with general political preferences in a state that also affect the health of residents. If that were the case, the instruments would not be orthogonal to BMI. Fortunately, both of these variables are highly influenced by actions undertaken by state courts, which are much less likely to be correlated with other political and policy characteristics within the state. Specifically, for a nontrivial number of states, both campaign contributions and term limits have been invalidated by state courts after legislatures adopted them. For term limits, the possibility is further attenuated by the fact that most term limits are adopted many years before they actually go into effect (and, therefore, before they show up in our coding). Furthermore, it is likely that state fixed effects in the first stage of the IV regression will mitigate the effects of any generic political and policy characteristics that could be correlated with both the adoption of these policies and state health characteristics. Finally, we also provide diagnostic tests suggesting that the instruments are rightfully excluded in the second stage of the IV regression.

We present the results of our IV analysis in Table 7. Our instruments perform well in the first-stage regression, generating a first-stage F-statistic for joint significance of 279, well above the standard cutoff of 10. Each instrument is individually statistically significant in the predicted direction as well.[20] In the second stage, we estimate that passage of a mandate increases the BMI of diabetics by

[19] We focus on binding term limits (that is, the variable does not take the value of one as soon as the state passes the term limit; instead, it takes the value of one starting in the first year in which the limit will have an effect on who may run for the legislature). We do this to limit the correlation between current voter preferences and the effect of term limits on legislator decision making. Since these laws are generally passed many years before they take effect, any correlation between the existence of a binding term limit law and current political preferences in a state will be attenuated, especially given that we control independently for state fixed effects in both stages of the instrumental variables analysis.

[20] The existence of term limits decreases the likelihood of adopting a diabetes mandate by nearly 15 percent ($p = .000$), and a prohibition on corporate campaign contributions is associated with an increase in the likelihood of mandate adoption of about 36 percent ($p = .000$).

Table 7

Effect of Diabetes Mandates on Body Mass Index: Diabetics Only

Variable	Coefficient	Robust SE	*p*-Value
Mandate	2.389	1.027	
Contribution prohibition, first stage	.360	.016	
Term limits, first stage	−.147	.011	
F-statistic for instruments in first stage	278.970		.000
Hansen *J*-statistic	.519		.471

Note. Results are from instrumental variables analysis examining only diabetics. Data are from Centers for Disease Control and Prevention, Behavioral Risk Factor Surveillance System (http://www.cdc.gov/brfss/), for the years 1996–2000. In addition to the instruments presented here, the first-stage equation included all covariates presented in Table 3 except diabetic. Full first-stage results are available on request. $N = 18{,}700$; $R^2 = .090$.

more than 2 points, an increase of about 8 percent. The increase is statistically significant at the 2 percent level. Further, our test of overidentifying restrictions suggests that our instruments are orthogonal to BMI.

One possible alternate hypothesis for our result is that passage of a diabetes mandate induces diabetics who are relatively less healthy to move into the state to receive diabetes benefits. While the BRFSS does not provide data that could help us rule out this possibility (such as an indicator for how long an individual has lived in the state), because we do find such a large effect (6 percent) in such a short period of time (less than 3 years for results presented in Table 4), it would seem unlikely that migration could be completely driving our result, given the costs of moving and changing jobs. If migration were driving our results, we might expect to observe an increase in the number of diabetics in mandate states after the mandate goes into effect. The BRFSS data do not show any such relationship.[21]

7. Conclusion

The incidence of diabetes is on the rise. The nearly $100 billion cost of diabetes and its complications represents only a small fraction of the true burden of this disease that is the sixth leading cause of death in the United States. Believing that this burden is likely to grow, a majority of the states have passed mandates requiring insurers to cover medical treatments for the disease.

This increased access to treatment could induce a moral hazard problem whereby individuals rationally substitute away from preventive measures such as a healthful diet and exercise routine when the effective price of medical treatments is lowered. However, among diabetics, mandates have the potential to improve access to self-management supplies and educational resources. Thus, the net public health effect of mandates is ambiguous.

[21] The Behavioral Risk Factor Surveillance System data indicate that when a state passes a diabetes mandate, the percentage of its population with diabetes increases by .0007, and the result is not statistically significant at even the 50 percent level.

Using microdata from the BRFSS in a DDD framework, we find that the passage of diabetes benefit mandates worsens the health of diabetics relative to nondiabetics within mandate states, controlling for contemporaneous changes in the gap between diabetics and nondiabetics in nonmandate states. This suggests that diabetes benefit mandates might be counterproductive in improving the health of diabetics. At a minimum, it suggests that any cost-benefit analysis of these mandates needs to account for this offsetting behavior.

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