Diabetes Treatments and Moral Hazard

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Abstract: In the face of rising diabetes rates, many states 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, they 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 often mandates 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 non-diabetics in mandate and non-mandate states. We find that mandates do generate a moral hazard problem with diabetics exhibiting higher BMIs after the adoption of these mandates.

JEL Classification: I12; I18; J32; J38
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1. Introduction

Diabetes is a growing concern in the United States. The Centers for Disease Control (CDC) estimates that more than 17 million people have diabetes and the incidence of the disease has been growing throughout the past decade (CDC 2003). 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 fifth leading cause of death in the United States. The American Diabetes Association 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 U.S. (ADA 2003).

Additionally, analysts estimate that there are another 12 million Americans with a condition known as pre-diabetes (Benjamin, Valdez, Geiss, Rolka, and Narayan 2003). Pre-diabetes is a condition covering individuals who are at a high risk for developing Type 2 diabetes.[1] The upward trend of obesity witnessed over the past two decades suggests that the incidence of diabetes and pre-diabetes will continue to grow (Mokdad, Ford, Bowman, Dietz, Vinicor, Bales, and Marks 2003).

In this context, the legislatures of 46 states have passed laws mandating that health insurance providers cover supplies, services, medications, and equipment for treating diabetes as

[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 to 199 milligrams per deciliter after a two-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 to 125 milligrams per deciliter after an overnight fast) but does not reach the Type 2 diabetes threshold.

part of their basic coverage without charging higher premiums for the coverage.[2] Given the high cost of diabetes treatments, advocates such as the American Diabetes Association 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 which 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.

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

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[2]Information on which states have diabetes mandates and the year of passage is provided in Table 1.

ambiguous.

In this paper, we examine the health effects of diabetes mandates by focusing on individuals' body mass indexes (BMI) for the period 1996-2000, during which 34 states adopted mandates, by employing a triple difference research design in which we compare the change in the BMI gap between diabetics and non-diabetics when a mandate is passed relative to the contemporaneous change in the diabetic/non-diabetic gap in non-mandate states. We find that mandates generate a statistically significant increase in the BMI of diabetics and the effect is of practical significance. Specifications that insufficiently control for factors that lead to the adoption of mandates generate spurious positive (i.e., decreases in BMI) treatment effects.[3]

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. The 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 U.S. 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 to the calories they expend than they did in the past. This net increase in

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

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 (forthcoming) 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.

In related work, Klick and Stratmann (2005) attempt to examine the short term effect of food price variation on individual behavior by looking at relative food prices. That is, while it is true that food prices have been declining in recent decades, until the mid-1980s the prices of healthful and unhealthful foods had been dropping at a similar rate. However, at that time, the price decline of unhealthful food accelerated relative to that of healthful food in the U.S.. Given this change, Klick and Stratmann demonstrate that not only did individuals eat more food in the aggregate as prices dropped, they also switched their consumption bundle to favor relatively unhealthful foods, generating a significant increase in BMI around the time the U.S. obesity epidemic is thought to have begun.

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 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, as well as 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 work force, families substituted toward more pre-prepared 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 which 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.[4] 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 pre-diabetes and diabetes than would be the case in the absence of these laws.[5]

3. Offsetting Behavior and the Insurer's Response

For simplicity we model an individual's food consumption decision assuming that the only cost of eating is an increase in the likelihood that the individual will develop diabetes. That is, an individual chooses to eat f identical units of food which imposes no financial cost on him,

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[4]See, for example, Peltzman (1975) and Viscusi (1984).

[5]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 moral general discussion of moral hazards arising from regulatory activity, see Klick and Mitchell (2006).

but consumption does increase the probability (p) he will develop diabetes.[6] Diabetes brings a utility cost of D which is independent of the consumption decision and is a decreasing function of available medical treatments (m).[7] The individual faces the following maximization problem:

$$\underset{f}{Max}\, U(f) - p(f)D(m)$$

yielding the following first order condition:

$$\frac{\partial U}{\partial f} - \frac{\partial p}{\partial f} D(m) = 0$$

which implies that the individual eats up to the point where his marginal increase in utility from food consumption equals the marginal increase in the probability that he will develop diabetes multiplied by the utility cost of diabetes. If the individual experiences a positive exogenous shock in access to medical treatments, the effect on his choice of f is represented by:

$$-\left[\frac{-\partial p / \partial f \,.\, \partial D / \partial m}{\partial^2 U / \partial f^2 - \partial^2 p / \partial f^2} \right]$$

The effect of an increase in medical treatments is to increase the individual's food intake, as long

[6]Allowing for choices among foods with varying levels of healthfulness would not change the primary result of this model.

[7]Allowing for varying losses from diabetes, either instead of or in addition to allowing p to be a function of f, does not change the model qualitatively.

as utility is concave in food consumption and the probability function has a constant or increasing slope with respect to food consumption. This implies that the prospect of improved diabetes treatment access will induce a worsening in the diet and, by extension, the exercise habits of individuals who fear the prospect of developing diabetes.[8] Mandates requiring that medical treatments for diabetes are included in the basic insurance coverage effectively increase access to those treatments. Thus, we might expect that mandates produce deleterious health effects.

However, given the high cost of providing medical treatments for diabetes,[9] insurers may focus much of their efforts on the pro-active 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. Active management has the potential to reap large cost savings with respect to diabetes since behavioral modifications significantly reduce diabetes incidence.[10] 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

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

[9]Peele, Lave, and Songer (2002) estimate that healthcare expenditures by insurers were three times higher for diabetics compared to all consumers in the examined health plans.

[10]In one study, Hu, Manson, Stampfer, Colditz, Liu, Solomon, and Willett (2001) find that more than 90 percent of cases of Type 2 diabetes could be prevented by the adoption of healthier lifestyles.

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. In work examining other kinds of insurance mandates, Gruber (1994) has found that mandates generally do not expand coverage because employers already often cover the services that are the subject of the mandate. 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,

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

insurers might hesitate to cover preventive care if there is substantial movement in and out of insurance plans. 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.

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. Generally, isolating the causal effect of treatment availability is difficult, since improved health technology represents a shock in availability to everyone, leaving 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 within the state as observed in non-diabetics in the state. Further, we can control for time effects that are unrelated to the adoption of insurance mandates by using diabetics and non-diabetics in non-mandate 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.[12] Our measure of health is the body mass index (BMI).[13] The BMI is a normalized weight metric used to classify an individual's weight status. Individuals with BMIs 25 and above are considered overweight, while a BMI of 30 or greater are considered obese.

We estimate the regression:

(1) $$BMI_{ijt} = \alpha \cdot diabetic_{it} * mandate_{jt} + \beta \cdot mandate_{jt} + \delta \cdot diabetic_{it} + \Theta \cdot X_{it} + \rho_i + \tau_t + \upsilon_j + \varepsilon_{ijt}$$

where BMI represents individual *i*'s BMI calculated from his 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). *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. The vector 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* which is common to all individuals surveyed in the same year as *i*, and υ 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 where we allow for state-specific year dummies.

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[12]We chose 2000 as our end point because after that year, some of the variables we use in our analysis were no longer collected.

[13] $$BMI = \left(\frac{\text{Weight in pounds}}{(\text{Height in inches})^2} \right) \cdot 703$$

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 where weight actually declines. We also include income and income squared, expecting that thinness is a normal good in the U.S., but at some point the effect of food being a normal good as well might overwhelm the demand for thinness.[14] 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 or not 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 are whether or not the individual smokes cigarettes. Lastly, 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, sex of the individual, whether the individual is unemployed at the time of the survey, 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 non-diabetics rely on their expectation of insurance coverage in the event that they develop diabetes in the future. However, if the mandates are

[14]Philipson and Posner 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 U.S., we do not expect to find such a relationship in this data.

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 above in Table 3. In the specification including general year dummies (column i), the treatment group (*diabetic*mandate*) exhibits a BMI reduction of 0.4, which represents a decrease of about 2 percent, and the result is statistically significant at the 1 percent level. Interestingly, the non-diabetic population in mandate states appears to exhibit the effects of moral hazard as the passage of the mandate increases BMI among this group by 0.07. Although the effect is statistically significant at the 1 percent level, the relative effect is very small (0.2 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 ii. 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 BMI reduction. The moral hazard effect in the non-diabetic 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 dataset, we are able to include an additional specification that controls for state-specific year effects. We present results with these controls in column iii. 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.

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 upon 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 only pass in those states where 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 (1994). 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 non-mandate 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, the mid-point 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:

$$\text{(2)}\quad \begin{aligned} BMI_{ijt} = \beta_1 X_{ijt} + \beta_2\tau_t + \beta_3\delta_j + \beta_4 Diabetic_i + \beta_5\left(\delta_j \times \tau_t\right) + \\ \beta_6\left(\tau_t \times Diabetic_i\right) + \beta_7\left(\delta_j \times Diabetic_i\right) + \beta_8\left(\tau_t \times \delta_j \times Diabetic_i\right) \end{aligned}$$

where *i* indexes individuals, *t* indexes the time period (where 0 stands for years before the mandate passes in 1998 and 1 stands for 1998 and later), and *j* indexes states (where 1 stands for states that pass a diabetes mandate in 1998 and 0 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. *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. 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. Diabetic again measures whether an individual is diabetic and therefore captures any fixed BMI differences between diabetics and non-diabetics. 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 post adoption. 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 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 to the diabetics in non-mandate states. Further confounding our results was the fact that mandate states experienced an upward trend in BMI among non-diabetic residents and diabetics in general exhibited increases in BMI.

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 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 mandate/no mandate distinction 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:

$$\begin{aligned} BMI_{ijt} &= \psi_1 X_{ijt} + \psi_2\left(\tau_t \times \delta_j\right) + \psi_3 Diabetic_i + \psi_4\left(\tau_t \times Diabetic_i\right) + \\ &\psi_5\left(\delta_j \times Diabetic_i\right) + \psi_6\left(\tau_t \times \delta_j \times Diabetic_i\right) \end{aligned} \tag{3}$$

where the model has a similar structure as the DDD model presented above, where *i* indicates an individual, t denotes a year (i.e., we no longer collapse all years into pre/post 1998), and j indicates the state of residence of the individual (i.e., we do not collapse into mandate states vs. non-mandate states). Again we control for observable differences across individuals with the X matrix. 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. ψ_5 controls for baseline differences in the diabetic populations of states that eventually adopt mandates and ψ_6 will isolate our treatment effect (i.e., 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 0.4 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 only apply to a subset of a state's population due to 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 or not. Also, there are no comprehensive state level data tracking the proportion of a state's population that falls under ERISA or not, making it impossible to design a credible index for a more precise mandate variable (Klick and Markowitz, 2003). 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 answering that they received their coverage through their employer (or spouse's employer) are less likely to fall under state mandates due to ERISA preemption relative to those individuals indicating that they bought their insurance independently, 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. 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 above. Instead, we once again employ Gruber's pooling method, estimating:

$$\begin{aligned} & BMI_{ijt} = \gamma_1 X_{ijt} + \gamma_2 \tau_t + \gamma_3 \delta_j + \gamma_4 IndependentInsurance_i + \gamma_5 \left(\delta_j \times \tau_t\right) + \\ (4)\quad & \gamma_6 \left(\tau_t \times IndependentInsurance_i\right) + \gamma_7 \left(\delta_j \times IndependentInsurance_i\right) + \\ & \gamma_8 \left(\tau_t \times \delta_j \times IndependentInsurance_i\right) \end{aligned}$$

in which, once again, the time dimension is collapsed into two period, pre-1998 and 1998-onward (with γ_2 measuring the period effect), and we restrict attention to only those 8 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 non-mandate group; thus γ_3 measures the time-invariant group effect. Independent insurance represents a dummy variable indicating that the individual purchased his insurance independently of his or his spouse's employer. γ_5 captures the mandate group post-mandate 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 among the independently insured individuals affected by mandates of 2.9 points. 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 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 framework. To implement an IV model, we restricted our attention to diabetics only, performing a simple difference in difference model comparing the change in diabetic BMI occasioned by the passage of a benefit mandate relative to contemporaneous BMI changes in the diabetic population of non-mandate states. For our instruments, we use 1) an indicator measuring whether or not the state had restrictions in place that bar corporations from making campaign contributions to state legislators and 2) a measure of the percentage of workers in a state who work for firms with more than 500 employees. 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 fact that larger firms' insurance plans are more likely to be governed by ERISA. Thus, in a state dominated by large firms, the practical effect of passing a mandate will be relatively low, .

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 statistics for joint significance of 323.9, well above the standard cut-off of 10. Each instrument is individually statistically significant in the predicted direction as well. In the second stage, we estimate that passage of a mandate increases the BMI of diabetics by almost 2 points, an increase of about 7 percent. The increase is statistically significant at the 6 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 relatively less healthy diabetics 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 three 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.[15]

7. Conclusion

The incidence of diabetes is on the rise. The nearly $100 billion cost of diabetes and its complications represents only a fraction of the true burden of this disease that is the sixth leading cause of death in the U.S. 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.

Using micro data from the BRFSS in a DDD framework, we find that the passage of diabetes benefit mandates worsens the health of diabetics relative to non-diabetics within mandate states, controlling for contemporaneous changes in the diabetic/non-diabetic gap in non-mandate 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.

[15]The BRFSS data indicate that when a state passes a diabetes mandate, the percentage of its population with diabetes increases by 0.0007 and the result is not statistically significant at even the 50 percent level.

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Table 1
Mandate Adoption

State	Year
Alaska	2000
Arizona	1998
California	1981
Colorado	1998
Connecticut	1997
Delaware	2000
Florida	1995
Georgia	1998
Hawaii	2000
Illinois	1998
Indiana	1997
Iowa	1984
Kentucky	1998
Louisiana	1997
Maine	1996
Massachusetts	2000
Michigan	2000
Minnesota	1994
Mississippi	1998
Montana	2001
Nebraska	1999
Nevada	1997
New Hampshire	1997
New Jersey	1996
New Mexico	1997
New York	1993
North Carolina	1997
Oklahoma	1996
Oregon	2001

Pennsylvania	1998
Rhode Island	1996
South Carolina	1999
South Dakota	1999
Tennessee	1997
Texas	1997
Utah	2000
Vermont	1997
Virginia	1998
Washington	1997
West Virginia	1996
Wisconsin	1987
Wyoming	2001

Table 2
Descriptive Statistics

Variable	Description	Mean	Std Dev
BMI (Total Sample)	Body Mass Index	26.052	5.117
BMI (Diabetics Excluded)	Body Mass Index	25.850	4.957
BMI (Diabetics Only)	Body Mass Index	29.611	6.415
Diabetic	= 1 if individual has diabetes	0.054	0.226
Mandate	= 1 if individual lives in a mandate state	0.563	0.496
Diabetic*Mandate	= 1 if individual lives in a mandate state and is diabetic	0.032	0.178
Income	Income in $1,000s	38.649	21.643
Age	Age in years	46.669	17.378
Female	= 1 if individual is female	0.590	0.492
Pregnant	= 1 if individual is currently pregnant	0.014	0.119
Education	Education level reported on scale of 1-6	4.665	1.097
Smoker	Indicator = 1 if individual currently smokes	0.237	0.447
Married	Indicator = 1 if individual is currently married	0.542	0.498
Separated/Divorced	Indicator = 1 if individual is divorced or separated	0.157	0.364
Children	Number of children (ages 18 and under) individual has	0.734	1.138
Unemployed	= 1 if individual indicated he is currently unemployed	0.034	0.181
Insured	= 1 if individual indicated he is insured	0.877	0.329
Contribution Prohibition	= 1 if state currently prohibits corporations from making campaign contributions to state legislators	0.407	0.491
Percent Large Firm	Percent of workers in state who are employed by firms with 500 or more employees	0.476	0.053

Note: All data come from the Behavioral Risk Factor Surveillance System (http://www.cdc.gov/brfss/) for the years 1996-2000, except for Contribution Prohibition (Source:) and Percent Large Firm (Source: U.S. Small Business Administration).

Table 3
Effect of Diabetes Mandates on BMI
(Robust Standard Errors in Parentheses)

Variable	(i)	(ii)	(iii)
Diabetic*Mandate	-0.404 (0.092)	-0.411 (0.092)	-0.404 (0.092)
Mandate	0.071 (0.027)	-0.047 (0.034)	–
Diabetic	3.043 (0.067)	3.047 (0.067)	3.041 (0.067)
Income	-0.021 (0.002)	-0.022 (0.002)	-0.022 (0.002)
Income^2	0.004 (0.002)	0.004 (0.002)	0.004 (0.002)
Age	0.312 (0.003)	0.313 (0.003)	0.312 (0.003)
Age^2	-0.003 (0.000)	-0.003 (0.000)	-0.003 (0.000)
Female	-1.407 (0.015)	-1.389 (0.015)	-1.408 (0.015)
Pregnant	0.963 (0.065)	0.956 (0.065)	0.963 (0.065)
Education	-0.289 (0.008)	-0.290 (0.008)	-0.289 (0.008)
Smoker	-0.704 (0.016)	-0.700 (0.016)	-0.703 (0.016)
Married	0.065 (0.020)	0.060 (0.020)	0.063 (0.020)
Separated/Divorced	-0.394 (0.025)	-0.399 (0.025)	-0.396 (0.025)
Children	0.056 (0.007)	0.054 (0.007)	0.055 (0.007)
Unemployed	0.094 (0.045)	0.091 (0.045)	0.092 (0.045)
Insurance	0.063 (0.024)	0.064 (0.024)	0.065 (0.024)
State Effects	Yes	Yes	Yes
Race Effects	Yes	Yes	Yes
Time Control	Year Dummies	State Trends	State-Year Dummies
Observations	466,805	466,805	466,805
Adjusted R^2	0.098	0.098	0.098

Note: The dependent variable is Body Mass Index (BMI) as reported in the BRFSS for the years 1996-2000. The coefficient for Income^2 has been multiplied by 100 for presentation.

Table 4
Effect of Diabetes Mandates on BMI
DDD Model Using Only Non-Adopting States
and States that Adopted Mandates in 1998
(Robust Standard Errors in Parentheses)

β_8 (Treatment Effect)	1.716 (0.296)
β_7 (Diabetics in Mandate States)	-1.827 (0.200)
β_6 (Diabetics 1998+ Effect)	1.502 (0.208)
β_5 (Mandate States 1998+ Effect)	0.105 (0.049)
β_4 (Diabetics)	2.786 (0.173)
β_3 (Mandate State Effect)	-0.037 (0.036)
β_2 (1998+ Effect)	-0.617 (0.032)
Observations	174,318
R^2	0.096

Note: Data come from BRFSS for 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.

Table 5
Effect of Diabetes Mandates on BMI
DDD Model Using All States
(Robust Standard Errors in Parentheses)

ψ_6 (Treatment Effect)	0.401 (0.126)
ψ_5 (Diabetics in Mandate States)	-1.104 (0.137)
ψ_3 (Diabetic)	3.135 (0.160)
Observations	466,805
R^2	0.099

Note: Data come from BRFSS for 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

Table 6
Effect of Diabetes Mandates on BMI
Independent Insurance as a Proxy for Mandate Coverage
DDD Model Using Only Diabetics in Non-Adopting States
and States that Adopted Mandates in 1998
(Robust Standard Errors in Parentheses)

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)	-0.143 (0.321)
γ_4 (Independently Insured)	-1.822 (0.773)
γ_3 (Mandate State Effect)	-0.218 (0.239)
γ_2 (1998+ Effect)	-1.088 (0.251)
Observations	6,814
R^2	0.165

Note: Data come from BRFSS for 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. This model also includes diabetic-specific year dummies and state-specific year dummies

Table 7
Effect of Diabetes Mandates on BMI
Instrumental Variables Analysis
Examining Only Diabetics
(Robust Standard Errors in Parentheses)

Mandate	1.978 (1.032)
Contribution Prohibition First Stage	0.321 (0.014)
Percent Large Firm First Stage	-4.506 (0.386)
F Statistic for Instruments in First Stage	323.900 (p = 0.000)
Hansen J Statistic	0.034 (p = 0.854)
Observations	18,700
R^2	0.093

Note: Data come from BRFSS for 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 upon request.