Are Restaurants Really Supersizing America?*

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Abstract

Regulating specific inputs into health and safety production functions is unlikely to be effective when optimizing consumers can compensate along other margins. This paper examines the implications of this principle in the context of economic policies targeted at reducing obesity. Well-established cross-sectional and time-series correlations between average body weight and eating out have convinced many researchers and policymakers that restaurants are a leading cause of obesity in the United States. But a basic identification problem challenges these conclusions: do more restaurants cause obesity, or do preferences for greater food consumption lead to an increase in restaurant density? To answer this question, we design a natural experiment in which we manipulate the effective price of restaurants and examine the impact on consumers' body mass. We use the presence of Interstate Highways in rural areas as an instrument for the supply of restaurants. The instrument strongly predicts restaurant access, and robustness tests support its validity. The results find no evidence of a causal link between restaurants and obesity, and the estimates are precise enough to rule out any meaningful effect. Analysis of food intake micro data suggests that although consumers eat larger meals at restaurants than at home (even after accounting for selection), they offset these calories at other times of day. We conclude that public health policies targeting restaurants are unlikely to reduce obesity but could negatively affect consumer welfare.

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"There's nobody at McDonald's shoving fries in your mouth."

Bonnie Modugno, McDonald's Chief Nutritionist[1]

1. Introduction

Obesity is the second leading underlying cause of death in the United States, and obesity rates have been growing rapidly in recent years (Mokdad et al. 2004). While 15 percent of Americans were obese in 1980 (defined as having a body mass index of at least 30), 34 percent were obese in 2004 (CDC 2007). Medical research has linked obesity to diabetes, heart disease, stroke, and certain cancers. Treating these diseases is expensive; health care spending attributed to obesity reached $78.5 billion in 1998 and continues to grow (Finkelstein et al. 2003). Although obesity is a serious and growing problem, its causes are not well understood.

One popular idea among public health advocates is that eating fast food causes obesity.[2] Fast-food – and restaurant food in general – has high caloric content, and the standard portion sizes served are relatively large (Young and Nestle 2002). Concerned policymakers are turning to new regulations on restaurants in efforts to fight obesity, ranging from bills recently advanced in New York City and California that require the display of nutritional information on food menus to strict limits on the number of fast-food restaurants in neighborhoods like South Los Angeles (Abdollah 2007). It is possible that large portions and effective marketing (presentation and pricing) lead people to eat more when they go to restaurants than when they eat at home. But it is not obvious that the empirical link between eating at restaurants and obesity is causal. If consumers' lifestyles are increasingly conducive to excess energy intake and positive energy balance, the increasing prevalence of restaurants may simply reflect a greater demand for calories.

The case against restaurants centers on well-known correlations showing that the frequency of eating out is positively associated with greater fat, sodium, and total energy intake as well as with greater body fat. These correlations have been established using a broad range of datasets and

[1] *San Francisco Chronicle*, March 7, 2004.

[2] Other hypotheses include changes in food prices, increasingly sedentary lifestyles, and technological change in food production (Lakdawalla and Philipson 2002; Cutler, Glaeser, and Shapiro 2003; Lakdawalla, Philipson, and Bhattacharya 2005; Bleich et al 2007).

study populations (for examples, see Clemens, et al. 1999; McCrory, et al. 1999; Binkley, et al. 2000; French, et al. 2000; Kant and Graubard 2004; Maddock 2004). Furthermore, the prevalence of both restaurants and obesity have been rising for a number of decades, leading some researchers to propose a connection between these trends. Chou, Grossman, and Saffer (2004) present state-year correlations that suggest the growth in restaurant density accounts for as much as 65% of the rise in the percentage of Americans who are obese. At this point, there appears to be broad consensus among participants in the national discussion on obesity policy that restaurants increase weight (U.S. Surgeon General 2001; Mello, Studdert, and Brennan 2006; Becker 2007).[3]

But simple correlations between restaurants and overeating may conflate the impact of changes in supply and demand. People choose where and how much to eat. A key question is whether the growth in restaurant supply, in terms of both number of establishments and portion sizes, is contributing to the obesity epidemic, or whether it merely reflects changes in consumer preferences. Whether or not consumers maximize utility in the classical sense, surely some restaurant growth is driven by secular increases in time costs and consumers' demand for calories.

We present a simple neoclassical model of an optimizing consumer that shows that a rational agent who consumes excess calories at a restaurant will cut back on other caloric intake. An implication from this framework is that eating at restaurants may have little or no causal impact on obesity. The model suggests that consumer preferences for increased caloric intake may explain the observed correlations of restaurant eating and obesity.

To assess the nature of the connection between restaurants and obesity, we design a natural experiment in which we effectively manipulate the price of restaurants (by changing some consumers' travel costs of accessing a restaurant) and examine the impact on consumers' body mass. In rural areas, Interstate Highways provide a shock to the supply of restaurants that is uncorrelated with consumer demand. To serve the large market of highway travelers passing through, a disproportionate number of restaurants locate immediately adjacent to these highways. For residents of these communities, we find that the highway boosts the supply of restaurants

[3] In fact, many public health advocates have shifted to designing policies aimed at reducing the impact of restaurants on obesity, even while they acknowledge that such a link has not been proven (for example, see Keystone 2006).

(and reduces the travel cost associated with accessing a restaurant) in a manner that is plausibly uncorrelated with demand or general health practices. We compare the prevalence of obesity in communities located immediately adjacent to Interstate Highways to those located slightly farther away to uncover the causal effect of restaurants on obesity.

The estimates suggest that restaurants have little effect on obesity. The distributions of BMI in highway and non-highway areas are virtually identical, and point estimates of the causal effect of restaurants on obesity are close to zero and precise enough to rule out any meaningful effects. But given that more calories are consumed at a typical restaurant meal than at home, how is it that lowering restaurant prices does not increase obesity? Our neoclassical model of a rational consumer points to two characteristics of consumer preferences: desired caloric intake and satiation. We examine food intake data collected by the U.S. Department of Agriculture and confirm these effects. First, there is selection bias in who eats at restaurants; people who eat at restaurants also consume more calories when they eat at home. Second, when eating relatively large portions at restaurants, people tend to reduce other calorie consumption at other times during the day. After accounting for these factors, eating a meal at a restaurant is associated with only 24 additional calories.

Our results indicate that policies focused on reducing caloric intake at restaurants are unlikely to substantially reduce obesity. The results also have broader implications for obesity policy and general health and safety regulation. Economic theory implies that regulating specific inputs in the health production function may not improve outcomes if consumers can compensate along other margins. This proposition is supported by economic studies in a variety of empirical settings. For example, Peltzman (1975) contends that mandating automobile safety devices does not reduce traffic fatalities because motorists respond by driving less carefully. More recently, Adda and Cornaglia (2006) argue that smokers react to cigarette taxes by smoking fewer cigarettes more intensively. In the case of obesity, consumers can choose from multiple sources of cheap calories. Restricting a single source – restaurants – is therefore unlikely to affect obesity, as confirmed by our findings. This mechanism may also underlie the apparent failure of so many targeted obesity interventions (Kolata 2006). Despite their ineffectiveness, such policies have the potential to generate considerable deadweight loss. We measure the potential deadweight loss of policies targeted at restaurants and find it to be as high as $33 billion annually.

The remainder of the paper is organized as follows. Section 2 develops a model of caloric intake for an optimizing consumer facing the option of eating at home or at a restaurant. The theoretical impact of restaurants on obesity is ambiguous because an optimizing consumer will compensate for larger portion sizes by eating less elsewhere. Section 3 describes the data, and Section 4 presents our quasi-experimental methodology using a simple graphical analysis. Section 5 presents estimates of the causal impact of restaurants on obesity. We find that reducing the price of restaurant food has little or no impact on obesity. Section 6 explores competing hypotheses that explain why restaurants do not affect obesity and analyzes the welfare effects of a potential restaurant "sin tax." Section 7 concludes.

2. Theoretical framework

Obesity may be the consequence of lifetime-utility-maximizing consumers making informed decisions about eating and exercising. But self-control issues are also likely to lead some consumers to overeat (Cutler et al. 2003). While food brings immediate gratification, the health costs of over-consumption occur in the future. If consumer preferences are time inconsistent, then regulation that decreases obesity may benefit at-risk individuals. The goal of this paper is not to evaluate how time inconsistency affects the optimality of decisions regarding caloric intake. Rather, taking reducing obesity as a public policy objective, this paper aims to evaluate whether increasingly prevalent regulations focused on raising the effective price of restaurants are likely to succeed in reducing obesity.[4]

Recognizing that consumers are optimizing agents reveals other characteristics of consumer preferences that are likely to undermine the efficacy of these regulations. To illustrate these challenges for public policy, we present a simple model of an optimizing consumer's decision of how much to eat. For simplicity, we abstract away from issues related to time inconsistency and focus on the impact of neoclassical characteristics of consumer preferences that are present even in a static model.

Consider a rational agent who chooses how many calories to consume during each of two periods – mealtime (c_1) and snack-time (c_2). Meals can be consumed either at home or at a restaurant.

[4] A strong argument in favor of government intervention is that the costs of treating obesity are not fully internalized by consumers. Finkelstein et al. (2003) estimate that Medicare and Medicaid alone spent $37.6 billion covering obesity-related illnesses in 1998 ($55.6 billion in 2007 dollars, inflated with CPI Medical Care Services index).

Eating at home costs the agent p_{1H} per calorie consumed as well as a fixed cost f_{1H}, representing the time it takes to prepare the meal. Some days the agent is busier than others, and f_{1H} is a random variable drawn daily from a support of $[0,\infty)$. Alternatively, the agent can eat out during mealtime. Eating at a restaurant costs the agent f_{1R} for a set quantity of food k, including the price of the meal and the time cost of traveling to and from the restaurant. Eating at snack-time costs the agent p_2 per calorie.

For simplicity, suppose calories consumed at a restaurant are prefect substitutes for calories consumed at home and that the agent has quasi-linear preferences in caloric consumption and another composite good x:[5]

$$U = u\,(c_{1H} + c_{1R}\,, c_2) + x$$

Caloric intake at mealtime is a substitute for caloric intake at snack-time in the sense that eating more at mealtime decreases the marginal utility from eating more at snack-time and vice versa, $u_{c_1c_2} < 0$. Suppose that the consumer's income Y is great enough that she consumes a positive amount of the composite good. An optimizing consumer chooses how much to eat to maximize her utility subject to her budget constraint:

$$Y - \mathrm{I}(c_{1H} > 0)(f_{1H} + p_{1H}\, c_{1H}) - \mathrm{I}(c_{1R} > 0)(f_{1R}) - p_2\, c_2 - x \geq 0$$

Following Young and Nestle (2002), assume that restaurant portion sizes are relatively large (i.e., larger than the agent would choose to eat at home, $k > c_{1H}$*). Depending on idiosyncratic circumstances on a particular day (her draw of f_{1H}), the agent will eat the meal either at home or a restaurant but not both. Let c_{1H}* and c_2*(H) denote the chosen levels of caloric consumption at mealtime and snack-time, respectively, when the agent eats the meal at home, and let c_{1R}* and c_2*(R) denote the chosen levels when the agent eats the meal at a restaurant. Three results immediately follow from this framework.

Result 1: c_{1R}* > c_{1H}*. On days when the agent eats at a restaurant, she eats more at mealtime than on days when she prepares the meal at home. The agent eats more at a restaurant, because the

[5] Quasi-linear preferences are a plausible assumption for a consumer whose income is sufficiently large and for whom food is only a small part of his total budget.

marginal cost of additional caloric intake is lower than at home.[6] At a restaurant, the fixed pricing scheme leads the agent not to internalize marginal production costs; she eats until she either finishes the portion, $c_{1R}* = k$, or is completely satiated, $u_{c_{1R}^*} = 0$. At home, she stops eating sooner, when marginal utility equals marginal cost, $u_{c_{1R}^*} = p_{1H}$. The agent "overeats" in restaurants in the sense that she consumes calories for which her marginal utility exceeds the marginal production cost.

Result 2: $c_2*(R) < c_2*(H)$. On days when the agent eats at a restaurant, she eats less at snack-time than on days when she eats the meal at home. At snack-time, the agent eats until marginal utility equals marginal cost, $u_{c_2^*} = p_2$. Because calories at mealtime and snack-time are substitutes, $u_{c_1 c_2} < 0$, the agent compensates for the larger portions at restaurants by consuming less throughout the rest of the day. Adding together calories consumed at mealtime and snack-time, total caloric intake is not necessarily greater on days when the agent eats at a restaurant versus at home.

Result 3: Total caloric intake depends on f_{1H}, f_{1R}, and $u(\cdot)$. Average total caloric intake may vary from person to person, depending on both the relative cost of eating at a restaurant versus at home and the agent's preferences. The model highlights at least two possible sources for heterogeneity in body mass across individuals. First, by decreasing the effective price of eating out, proximity to restaurants may lead to increased caloric intake if offsetting reductions in snack-time consumption are not complete. Second, variation in consumer preferences for caloric intake may lead some individuals to eat more than others – whether they are eating at a restaurant or at home. If consumers with preferences for increased caloric intake patronize restaurants more frequently than others, the empirical association of restaurants and obesity may not reflect a causal relationship.

Whether restaurants actually increase obesity is an empirical question. It is possible that access to large portions with low marginal costs at restaurants leads people to overeat. On the other hand, if rational consumers compensate for large restaurant portions by eating less elsewhere, raising

[6] For simplicity, we don't allow the agent to save unconsumed food purchased from the restaurant for other meals or snacks. As long as this sort of transfer between meals is not costless (for example, if food quality is reduced or if there is a chance of spoilage), the results are qualitatively unchanged.

restaurants' effective prices may have no impact on total caloric intake or obesity. The empirical analysis that follows addresses this important question.

3. Data and Descriptive Statistics

The obesity data used in this study come from the Behavioral Risk Factor Surveillance System (BRFSS). BRFSS is an ongoing, large-scale telephone survey that interviews hundreds of thousands of individuals each year regarding their health behaviors.[7] In addition to questions about demographic characteristics and health behaviors, BRFSS asks each individual to report his or her weight and height.

Two features of BRFSS are important for our study. First, BRFSS generally oversamples less populous states. Since our analysis focuses on rural areas, this sampling frame works to our advantage. Second, although consolidated BRFSS data are publicly available from the Centers for Disease Control (CDC), CDC does not release geographic identifiers at a finer level than the county. To complete our study, we therefore approached 23 State Departments of Health and requested confidential BRFSS extracts that include a much finer geographic identifier: telephone area code and exchange (i.e., the first 6 digits of a 10 digit telephone number).[8] Ultimately, 11 states – Arkansas, Colorado, Iowa, Kansas, Maine, Missouri, North Dakota, Nebraska, Oklahoma, Utah, and Vermont – cooperated with our requests. Sample years vary by state and overall cover 1990 to 2005.

Our measures of obesity include Body Mass Index (BMI) – defined as weight in kilograms divided by height in meters squared – and an overweight indicator that equals unity if BMI is greater than 25. These measures are standard in the obesity literature, and the overweight indicator is of particular interest because mortality risk increases as BMI exceeds 25 (Adams et al. 2006).[9]

Restaurant establishment data are from the United States Census ZIP Code Business Patterns. These data include counts of full-service and limited-service restaurants for every ZIP code in the

[7] BRFSS randomly samples phone numbers using a disproportionate stratified sample method. Within each household, individuals over the age of 18 are randomly selected for interviews. Business and nonworking phone numbers are omitted (BRFSS Operational and User's Guide 2006).
[8] Prospective states were chosen on two criteria intended to maximize the usable number of observations: the BRFSS sampling rate and the number of towns that qualified for our research design.
[9] Repeating the analysis using an obese indicator – BMI greater than 30 – generates similar conclusions.

United States.[10] Because the restaurant data are identified by ZIP code, while the obesity data are identified by telephone exchange, it is impossible to create an exact link between the two data sets.[11] Instead, our analysis relies on two-sample-instrumental-variables techniques, which use separate samples to estimate the effect of the instrument on each of the two endogenous variables, obesity and restaurant access. The link between the two data sets thus runs through the instrument, proximity to an Interstate Highway.

Table 1 presents summary statistics for our data sets. The first two columns present unweighted means and standard deviations for the analytic sample, which consists of all telephone exchanges or ZIP codes in cooperative states located less than 10 miles from an Interstate Highway, more than 30 miles from an urban area, and with a population density of less than 80 persons per square mile. Our analysis focuses on rural areas because the population density in urban areas guarantees that almost everyone has easy access to one or more restaurants. (We also present estimates for a smaller sample of rural areas that have population density of less than 40 persons per square mile; the results are qualitatively unchanged.) The last set of columns in Table 1 present the same statistics for the full national sample.

Panel A, based on BRFSS data, reveals that mean BMI and percent overweight in the analytic sample closely match national averages. However, the analytic sample is slightly older and somewhat less educated than the national sample. Panel B, based on Census data, demonstrates that the rural analytic sample has fewer minorities and a lower average income than the national sample. The analytic sample also has substantially fewer restaurants per ZIP code than the national sample, primarily because the average population per ZIP code is much lower.

4. Graphical Analysis

Our analysis exploits the location of Interstate Highways in rural areas as a natural experiment and compares two groups of small towns: those directly adjacent to an Interstate Highway (0-5

[10] The distinction between full service and limited service restaurants is based on the timing of payment: in full-service restaurants, the customer pays after eating; in limited-service restaurants, the customer pays before eating. Roughly speaking, full service restaurants can be thought of as "sit down" restaurants and limited service restaurants can be thought of "fast food" restaurants.

[11] Though both measures are geographically precise, telephone exchange is the finer of the two. There are approximately 35,000 ZIP codes and 130,000 telephone exchanges in the U.S. However, the differential in geographic area is not as large as it appears since telephone exchanges in urban areas can overlap whereas ZIP codes do not.

miles away) and those slightly further (5-10 miles away). For convenience, we refer to these two sets of towns as "adjacent" and "nonadjacent," respectively. The Interstate System was designed in the 1940s "to connect by routes, direct as practical, the principal metropolitan areas, cities, and industrial centers" (U.S. Department of Transportation 2002).[12] As an unintended consequence, the system lowered transportation costs for rural counties that happened to lie between major cities. Previous work has studied the effect of highways on rural county-level economic outcomes (Chandra and Thompson 2000; Michaels 2007). Our study, however, uses a much finer level of geographic detail – ZIP codes and telephone exchange areas.[13] This geographic detail enables us to limit our sample to ZIP codes and exchanges whose centers lie within 10 miles of an Interstate Highway. We therefore expect – and find – no systematic differences in economic outcomes between the two groups of towns in our sample.

Nevertheless, for a large group of individuals – through travelers on Interstate Highways – adjacent towns represent a more convenient service option than nonadjacent towns that are slightly further away. Since these individuals have many choices along their route of travel, their demand is highly elastic with respect to distance from the highway. Proximity to an Interstate thereby produces a positive shock to the supply of restaurants in towns adjacent to Interstates, relative to towns that are not immediately adjacent, for a reason that is independent of local demand. Comparing the two sets of towns, ZIP codes located 0 to 5 miles from Interstates are approximately 38 percent (19 percentage points) more likely to have restaurants than ZIP codes located 5 to 10 miles from Interstates. This is true for both fast-food and full-service restaurants.

Figure 1 plots the distribution of distance to the nearest restaurant for adjacent and nonadjacent ZIP codes. For ZIP codes without restaurants, we use the distance to the nearest ZIP code with a restaurant.[14] But the average distance for residents of ZIP codes that contain restaurants is not zero. We calculate the distribution of the distance from each Census block to the nearest restaurant for a stratified random sample of 21 ZIP codes that contain restaurants (see Appendix A1 for details). Residents of these ZIP codes live, on average, 2.5 miles from their nearest

[12] The other two goals of the Interstate System were to aid national defense and to connect, at suitable border points, major routes to Canada and Mexico.

[13] The average U.S. county contains approximately 1,030 square miles while the average US ZIP code contains approximately 80 square miles (U.S. Census Bureau 2002a,b).

[14] Appendix A1 presents calculations of the exact distance from each Census block to the nearest restaurant for a stratified random sample of 11 ZIP codes not containing restaurants. This analysis confirms that the distance measures used are generally accurate representations of the distance from the nearest restaurant for the average resident of the ZIP code.

restaurant. To construct Figure 1, we sample (with replacement) from the empirical distribution of restaurant distance for each sample ZIP code that contains a restaurant.

Figure 1 shows that the distance to the nearest restaurant is much lower for residents of ZIP codes that are adjacent to an Interstate highway than for residents of nonadjacent ZIP codes. Most residents of adjacent ZIP codes live 0 to 5 miles from the nearest restaurant, whereas residents of nonadjacent ZIP codes are more likely to lie 5 to 20 miles away. These distances represent a sizable barrier to restaurant access; they correspond to greater roundtrip travel times by up to 40 minutes.[15]

Figure 2 shows the distribution of BMI for towns adjacent to an Interstate and towns further from an Interstate. The two distributions match up exactly, implying that restaurants have no discernable effect on obesity. The tight correspondence between the two distributions also suggests that unobserved factors that affect BMI are balanced across both groups of towns. If such factors were not balanced, they would have to exactly offset not only the effect of restaurants on mean BMI, but the effect of restaurants at every quantile of the BMI distribution. This effect would likely be heterogeneous, so it would take a complex and improbably distributed set of confounders to exactly offset it. Nevertheless, we analyze an observable set of BMI predictors to confirm that adjacent and nonadjacent towns are comparable across factors that predict BMI.

Figure 3 plots the distribution of an index of predicted BMI for both groups of towns. The index consists of the fitted values from a regression of BMI on a set of observed covariates: gender, a quadratic in age, indicators for educational attainment, employment, unemployment, and marital status, and a full set of state-by-year fixed effects. This index summarizes all of the covariates, weighting them in relation to their correlation with BMI, and provides a more powerful test of covariate balance than examining each covariate individually.[16] The plot in Figure 3 reveals that risk factors for BMI are balanced across the adjacent and distant towns – the two distributions match up precisely. This balance occurs without controlling for any covariates – not even state or

[15] While in theory travel times may be overstated if individuals work in restaurant-dense areas and eat out near work, analysis presented below shows that the effects are similar for individuals who are employed and those who are not employed.

[16] Statistical tests of each covariate individually also find no significant differences between individuals residing in adjacent and nonadjacent towns.

year dummies – suggesting that our research design successfully approximates a randomized experiment.[17]

5. Statistical Framework and Estimation Results

While the figures presented above suggest that restaurants do not have a causal effect on obesity, they do not deliver a framework suited for statistical inference. This section presents an estimation framework based on instrumental variables that formally estimates the causal effect of restaurants on obesity.

The following model describes the causal relationship to be estimated. For individual i living in town j during year t, we can write

$$b_{ijt} = \beta_0 + \beta_1 p_{jt} + \eta_t + \varepsilon_{ijt}, \tag{2}$$

where b_{ijt} is individual i's BMI, p_{jt} is the restaurant price, η_t are time effects, and ε_{ijt} contains unobserved determinants of BMI that vary at both the time and individual levels.[18] We define p_{jt} broadly to include not only menu prices, service charges, and taxes, but also travel and time costs. It is the latter which we observe in our data, and our analysis focuses on this source of price variation.

An analysis that assumes p_{jt} is exogenously determined is unattractive. Both restaurants and people choose where to locate, so restaurant availability is likely to be correlated with potential BMI outcomes at the individual level. Since the BMI data are coded by telephone exchange, while the restaurant data are coded by ZIP code, combining b_{ijt} and p_{jt} in a single sample is also infeasible. We address these issues by finding an instrument z_j that affects restaurant availability but is uncorrelated with other determinants of potential BMI outcomes, ε_{ijt}. On theoretical and empirical grounds, we conclude that our instrument – proximity to an Interstate Highway – satisfies these two criteria.

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[17] To demonstrate that the BMI risk index has predictive power, Appendix Figure A1 plots the BMI distributions for individuals with a predicted BMI of less than 25 (predicted normal weight) and individuals with a predicted BMI of greater than 25 (predicted overweight). As expected, individuals predicted to be overweight are substantially heavier than individuals predicted to be normal weight.

[18] Although the equation is linear with constant coefficients, these assumptions are not necessary for the estimates to have a legitimate causal interpretation. If p_{st} were randomly assigned, then β_1 would be a weighted average of individual causal effects along the length of the causal response function (Angrist and Imbens, 1995).

There is no theoretical reason to believe that proximity to Interstate Highways is correlated with the determinants of body mass. Small towns that lie directly adjacent to Interstates do so only by historical accident, and all towns in our sample enjoy the lower transportation costs associated with easy access to highways (Chandra and Thompson 2000; Michaels, forthcoming). Nevertheless, in principle people can sort across towns: individuals with preferences for eating out might sort into towns adjacent to Interstates, and these individuals may have higher or lower unobserved determinants of BMI, ε_{ijt}. But results from disaggregated Census and BRFSS data, reported in Tables 2 and 3, show no evidence of sorting across adjacent and nonadjacent towns in our sample. Given that all observable characteristics are balanced, it is likely that unobservable characteristics are balanced as well.

Table 2 presents the relationship between demographic characteristics and both restaurant availability and Interstate proximity using ZIP code level extracts from the 2000 U.S. Census of Population and Housing. Each estimate represents the results of a separate regression and controls for a full set of state-by-year fixed effects. The first column reports the coefficients from regressions that run different dependent variables on an indicator that is unity if a ZIP code contains any restaurants and zero otherwise. This sample contains all ZIP codes in the states that we study. ZIP codes with restaurants contain a disproportionate number of females and minorities, and their residents tend to be better educated with higher incomes. These estimates are statistically significant, with *t*-statistics ranging from 9.0 to 20.3. The results indicate that an OLS analysis – regressing BMI on restaurant availability using all ZIP codes – would likely yield misleading estimates of the causal effects of restaurant availability.

The second column of Table 2 reports the coefficients from a series of regressions running the same dependent variables on the instrument, proximity to an Interstate Highway. This sample contains only rural ZIP codes lying within 10 miles of an Interstate Highway (the analytic sample). In contrast to the first column, no regression returns a statistically significant coefficient. More importantly, the lack of statistical significance occurs because of a sharp drop in the magnitude of the coefficients, not because of an increase in the standard errors.[19] Relative to the first column, coefficient magnitudes decrease by factors ranging from 3 to 94 times, with an average decrease in magnitude of 27.9 times. Overall, important demographic characteristics seem to be well-balanced across areas that are adjacent and nonadjacent to Interstate highways.

[19] In fact, all but one of the coefficients in Column (2) would be statistically insignificant even when using the smaller standard errors from Column (1).

Table 3 presents similar results using individual-level BRFSS data. These results reinforce the conclusion that the instrument is uncorrelated with other determinants of BMI. The "BMI Risk Index" consists of the fitted values from a regression of BMI on a set of observed covariates, as defined above in Section 4. The effect of the instrument on the BMI Risk Index is statistically and economically insignificant; the coefficient of -0.038 corresponds to less than 0.01 standard deviations of BMI. The "Overweight Risk Index" is constructed similarly to the BMI Risk Index, but the covariates are used to predict overweight status rather than BMI. Again, there is no statistically significant or economically meaningful relationship between the Overweight Risk Index and proximity to an Interstate Highway.

Some states included additional variables in the BRFSS extracts they provided us. We do not include these variables when estimating the BMI and Overweight Risk Indices because the sample size would be severely reduced. However, individual tests for each variable also support the validity of our identification strategy. Regression estimates for these additional variables are presented in the last four rows of Table 3. There is no significant relationship between proximity to an Interstate and average income or smoking rates. More importantly, individuals living in adjacent and nonadjacent towns have similar desired weights and exercise with similar frequency.

While proximity to an Interstate Highway appears uncorrelated with observed determinants of BMI, it does affect restaurant availability. Table 4 reports first-stage results for a variety of restaurant availability measures. Column (1) presents results for the full analytic sample. To illustrate that the estimates are not sensitive to a particular cutoff for population density, we report results in Column (2) for a smaller sample that includes only areas with less than 40 people per square mile (versus less than 80 in the full sample). Proximity to an Interstate Highway has a positive and significant effect on restaurant availability in all regressions, regardless of the measure or choice of sample.

The first row of Table 4 shows estimates of the impact of Interstate proximity on the distance to the nearest ZIP code with a restaurant. The results indicate that ZIP codes adjacent to Interstates are, on average, 1.50 miles closer to the nearest ZIP code with a restaurant than ZIP codes further from Interstates. This effect is highly significant; the t-statistic of 3.9 corresponds to a first-stage F-statistic of 15.6. Although 1.50 miles may not sound far, it is important to note that this effect primarily operates through the differential in ZIP codes containing any restaurants. Proximity to an Interstate makes a ZIP code more likely to have a restaurant, reducing the average distance to the nearest ZIP code with a restaurant from 10.2 miles to 2.5 miles.[20] Thus, although the majority of the sample is unaffected, those that are affected experience a large reduction in travel costs. (Appendix A1 verifies the accuracy of our ZIP code level distance measure and describes how the 2.5 mile figure is calculated.)

Our instrumental variables estimates focus on travel distance as the relevant measure of restaurant access because it has a direct economic interpretation. Nevertheless, other estimates, reported in Table 4, show that the relationship between Interstate proximity and restaurant availability is robust across different measures. ZIP codes adjacent to Interstates are 17.5 percentage points more likely to contain at least one restaurant (t = 4.2). This effect holds both for full-service and limited-service restaurants.[21] Although the effect for full-service restaurants is larger in raw percentage point terms (19.6 percentage point increase versus 15.4 percentage point increase), the effect for limited-service restaurants is larger in proportional terms (45.6 percent increase versus 57.9 percent increase). ZIP codes adjacent to Interstates also have significantly more restaurants per capita.

The effect of interest for public policy is the response in BMI to changes in total restaurant price. We translate the distance measure reported in the first row of Table 4 into a price measure using a set of conservative assumptions regarding vehicle operating costs and travel time valuation. Vehicle operating costs come from annual publications of *American Automobile Association: Your Driving Costs*. We include per mile gasoline costs at full value, but we reduce maintenance and depreciation costs by 50 percent under the assumption that many drivers may underestimate these costs. We also exclude tire wear and expected accident costs from our calculations. Under these conservative assumptions, we compute perceived vehicle operating costs at 9.7 cents per mile during our sample period.[22]

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[20] In the terminology of Angrist, Imbens, and Rubin (1996), these ZIP codes are the "compliers." This group accounts for 18.9 percent of towns in distant ZIP codes. Differences in group means do not exactly equal reported regression coefficients because regressions contain state fixed effects.

[21] The determining factor in whether a restaurant is classified as full-service or limited-service is whether the customer pays prior to eating or after eating. For example, McDonald's is limited-service and Denny's is full-service.

[22] All values in this section are expressed in 2007 dollars. Costs are computed as weighted averages from 1990 to 2005, with each year weighted by the number of observations that it contributes to our analytic sample. If we used true vehicle operating costs and included expected accident costs, the estimate would be approximately 2.5 times as large.

The best available estimates of travel time valuation come from Lam and Small (2001). They use the revealed preferences of Southern California toll lane users to estimate an average time value of $29.28 per hour. These results, however, are estimated from urban commuters and may not generalize to rural, non-work trips. We therefore draw upon an original data set of automobile speeds on unobstructed rural roadways to estimate the degree to which rural drivers travel at higher speeds that trade off gasoline consumption for time savings. These data reveal that rural motorists value their time at a minimum of $12.11 per hour, or 31.1 cents per mile (see Appendix A2 for full details).

We estimate total perceived travel costs (including both vehicle operating costs and travel time) at 40.8 cents per mile. The last row of Table 4 indicates that the perceived cost differential in restaurant access for ZIP codes adjacent to Interstates versus ZIP codes further from Interstates is $1.22.[23] As explained above, this effect operates through the differential in ZIP codes containing any restaurants. Proximity to an Interstate reduces the total restaurant price by an average of $6.18 for areas that would not have a restaurant if not for the highway. This figure amounts to almost 80 percent of the average cost of a restaurant meal.

Table 5 reports the reduced-form effect of Interstate proximity on obesity. The first row shows estimates of the impact of Interstate proximity on an overweight indicator (BMI > 25), and the second row shows estimates of the impact on BMI.[24] Column (1) reports estimates from the full analytic sample and includes controls for state-by-year fixed effects but no other covariates.[25] The regressions are precisely estimated. The estimated coefficient from the overweight regression indicates that proximity to a highway has no significant effect on the probability of being overweight. In fact, the point estimate is negative (-0.7 percentage points). Estimates from the BMI regression also show that proximity to highways does not affect obesity; the coefficient is statistically insignificant and implies that Interstate proximity increases BMI by only 0.002 points.

[23] We compute this number by doubling the coefficient in the first row of Table 4 (to reflect round-trip travel) and multiplying it by 40.1 cents per mile.
[24] The former measure is generally of more interest to public health practitioners as increases in BMI are not considered unhealthy until the individual crosses the threshold of 25.
[25] County fixed effects are infeasible because some states only reported the distance to the Interstate Highway – rather than the exact location of the telephone exchange – in order to preserve the anonymity of the survey respondents.

The reduced-form results are robust to various adjustments to the econometric specification. Column (2) reports results from regressions that contain no state or year fixed effects. The coefficients are close to those reported in Column (1) and remain statistically indistinguishable from zero.[26] Retaining state-by-year fixed effects does however make the estimates more precise. Column (3) presents results from regressions that contain state-by-year fixed effects but are estimated on a smaller sample that is not missing observations for a large set of economic variables (which are included as controls in subsequent specifications). The coefficients are insignificant and remain close to those reported in Columns (1) and (2). Column (4) presents results from regressions that add flexible controls for age, education, marital status, employment status, and gender. The addition of controls has little effect on the coefficients; they increase slightly from the estimates reported in Column (3), but they remain close to zero and statistically insignificant.[27] Column (5) estimates the same model as Column (4) but reduces the sample to include only individuals in the most sparsely populated areas (40 people per square mile or less).[28] The results again indicate that proximity to highways has no effect on obesity.

Because information on obesity (the outcome) and restaurant access (the endogenous right-hand variable) are not contained in the same sample, estimation via the traditional instrumental variables technique is infeasible. Instead, we apply the Two-Sample Two-Stage Least Squares estimator (TS2SLS) discussed in Inoue and Solon (2005), a variant of the two sample instrumental variables strategy used by Angrist (1990) and Angrist and Krueger (1992). The first-stage estimating equation is:

$$d_{jst} = \pi_0 + \pi_1 z_{jst} + \chi_{st} + u_{jst} \tag{3}$$

where d_{jst} is distance to the nearest ZIP code with a restaurant from ZIP code j in state s and year t, z_{jst} is an indicator for proximity to an Interstate, χ_{st} are state-by-year fixed effects, and u_{jst} is the least squares residual. The results for this regression are in Table 4, Column (1).

We implement the TS2SLS estimator by applying the coefficient estimates from equation (3) – estimated using data from ZIP Code Business Patterns – to predict the value of d_{jst} for observations in the BRFSS data:

$$\hat{d}_{jst} = \hat{\pi}_0 + \hat{\pi}_1 z_{jst} + \hat{\chi}_{st}. \tag{4}$$

We then run the second-stage regression

[26] Although the coefficient in the BMI regression is an order of magnitude greater than the estimate reported in Column (1), this increase represents less than 0.005 standard deviations of BMI.

[27] Running the regressions separately for males and females reveals no significant differences in the effect.

[28] This sample corresponds to the first-stage regressions reported in Table 4, Column (2).

$$b_{ijst} = \beta_0 + \beta_1 \hat{d}_{jst} + \lambda_{st} + \varepsilon_{ijst} \tag{5}$$

to estimate the effect of distance to the nearest restaurant on BMI. The standard errors are adjusted to reflect the fact that the first-stage coefficients are estimated rather than known (Inoue and Solon 2005, p. 6).

We make several conservative assumptions when estimating the first-stage coefficient, π_1. In particular, we assume that the entire differential in restaurant access operates through distance to the nearest restaurant. However, the results in Table 4 indicate that Interstate proximity also has a positive effect on restaurant density, potentially increasing the variety of restaurants available to consumers. By ignoring this channel, we understate the true effects of Interstate proximity on restaurant availability. We also underestimate vehicle operating costs and travel time valuation when translating distance measures into travel cost measures – this is equivalent to underestimating π_1 when the measure of restaurant access is travel cost instead of distance.

An alternative expression for the TS2SLS estimate makes it clear that these assumptions bias the magnitude of $\hat{\beta}_1$ upwards, overstating the impact of restaurant prices on obesity. Because the model is exactly identified, the TS2SLS estimates are directly implied by the ratio of the reduced-form and first-stage estimates. Let $\hat{\alpha}_1$ be the coefficient obtained from estimating the reduced form equation:

$$b_{ikst} = \alpha_0 + \alpha_1 z_{kst} + \phi_{st} + v_{ikst} \tag{6}$$

where b_{ikst} is the BMI of person i in telephone exchange k of state s in year t, z_{kst} is an indicator for proximity to an Interstate, ϕ_{st} are state-by-year fixed effects, and v_{ikst} is the least squares residual. The results for this regression are in Table 5, Column (1). Because the model is exactly identified, the TS2SLS estimate is:

$$\hat{\beta}_1 = \frac{\hat{\alpha}_1}{\hat{\pi}_1}. \tag{7}$$

By conservatively estimating π_1, we therefore ensure that our estimates of the effect of restaurant prices on obesity are, if anything, too large.

Table 6 presents TS2SLS results for the effect of restaurant access on obesity. Column (1) reports estimates for regressions using the full analytic sample. Shifting one mile closer to a restaurant is associated with a 0.5 percentage point reduction in the probability of being overweight and a 0.001 point increase in BMI. Both of these effects are statistically and economically insignificant. Panel B presents the estimated effects of decreasing restaurant prices by one dollar. These estimates utilize the per-mile driving costs computed above. For example, the effect on BMI of decreasing restaurant prices by one dollar is

(0.0014 BMI per mile) / (2 * 0.40 dollars per mile) = 0.0018 BMI per dollar[29]

Lowering restaurant costs by one dollar is associated with no increase in the probability of being overweight and a 0.002 point increase in BMI. Both effects are statistically insignificant and correspond to changes of less than 0.01 standard deviations in the respective outcomes.

The remaining columns in Table 6 report TS2SLS results for alternative samples. Column (2) presents estimates for the smaller sample that contains only areas with 40 people per square mile or less. All of the estimated coefficients are statistically insignificant with negative point estimates, reinforcing the conclusion that cheaper access to restaurants does not increase obesity. Column (3) presents estimates for the sub-sample of individuals who are not employed. Some employed persons who live in areas without easy access to restaurants may commute to areas that have easier access to restaurants. For these individuals, we may overestimate the cost of accessing food away from home. To address this possibility, we re-estimate the TS2SLS coefficients for the subsample of individuals who are not employed. In each regression, the coefficient in Column (3) is less than the coefficient in Column (1), indicating that access to restaurants at work is not confounding our results. Column (4) presents estimates for a sample of individuals who are both not employed and live in areas with less than 40 residents per square mile. Again, all coefficients remain statistically insignificant with negative point estimates.

6. Discussion of Policy and Welfare Implications

The results presented above suggest that access to restaurants has no appreciable causal effect on BMI or the prevalence of overweight individuals. All point estimates are close to zero, precisely estimated, and statistically insignificant. These findings suggest that policies targeted at restaurants are unlikely to lower the prevalence of obesity. Nevertheless, examples of these policies are common across many jurisdictions. Modest policies include mandatory posting of calorie counts on menus – advanced in New York City, Seattle, and California – and limits on fast-food advertisements – adopted in Quebec and Great Britain (Baylis and Dhar 2007; Davies

[29] The scaling factor of 2 in the denominator is present to account for the roundtrip nature of the trips. Figures are rounded to 0.001 and 0.002 in Table 6.

2007; Gardiner 2007; Ostrom 2007; Vyse 2007).[30] More ambitious proposals include zoning regulations that limit the number of fast-food restaurants – put forward in New York City and Los Angeles – and restaurant "fat taxes" – proposed by public health officials and city leaders in New Zealand, Great Britain, and the United States (Claridge 2003; Whiteside 2004; Crowley 2005; Fernandez 2006; Abdollah 2007).

Here, we consider the effects of the most ambitious proposal, a hypothetical restaurant "fat tax." Existing "sin taxes" on alcohol and tobacco vary by state; the median tax is 2.1 percent for beer and 48.3 percent for cigarettes (Boon 2007; Tax Foundation 2007; U.S. Department of Labor 2007).[31] We therefore consider a restaurant tax of 50 percent to be at the upper limit of plausible "fat taxes." Since the average restaurant meal costs \$7.94 (excluding tax and tip), a 50 percent tax implies an increase of \$3.97 in average meal prices (U.S. Census Bureau, 2005, p.12).[32]

The point estimates from the first column of Table 6 imply that a 50 percent (\$3.97) increase in restaurant prices would not negatively affect the probability of being overweight and would reduce BMI by only 0.007 points on average.[33] Even if the true effect were one standard error greater than the estimated coefficient, a 50 percent tax would reduce the probability of being overweight by only 1.3 percentage points and decrease average BMI by 0.42 points. These effects correspond to 2 percent of the base rate of overweight adults and 0.08 standard deviations of BMI. At the extreme tails of the 95 percent confidence intervals, a 50 percent tax would at most reduce the probability of being overweight by 4.6 percentage points and decrease average BMI by 0.81 points. Thus, even when combining the strongest feasible policy with the largest possible coefficient values, there is only a small decrease in the prevalence of overweight adults.

Given the established correlation between eating out and obesity, as well as the simple fact that restaurant portions have grown markedly over the past several decades, it may appear surprising that restaurant access has no causal effect on obesity. Based on our theoretical framework presented above, there are three possible reasons why varying the effective price of restaurants

[30] The New York City ordinance was enacted but overturned on a legal technicality (and is expected to be re-enacted soon), and the California bill expired after the governor failed to sign it into law. The laws in Seattle, Quebec, and Great Britain are in effect or will come into effect within the next year.
[31] The cigarette tax includes both federal and state taxes. There is no federal tax on alcohol.
[32] This price may seem low – it is important to note that it excludes tax and tip and includes fast-food meals, which make up the majority of restaurant meals served. The average full-service restaurant meal is \$12.30, excluding tax and tip.
[33] Table 6 coefficients have been rounded; the exact coefficient for BMI regression in Panel B is 0.0018.

would not affect weight. First, restaurant demand may be highly inelastic. This hypothesis, however, is not supported by existing estimates of the own-price elasticity of food away from home, which generally range from -1.0 to -2.0 (Park et al. 1996; Piggott 2003). Second, after accounting for selection, individuals may not consume substantially more calories when they eat out than they do at home. Third, even if people do consume more calories at restaurants, they may offset the additional restaurant consumption by eating less during the rest of the day. The latter two hypotheses imply that a restaurant tax could have substantial effects on social welfare. To explore their empirical relevance, we examine food intake data collected by the U.S. Department of Agriculture.

The food intake data come from the Continuing Survey of Food Intake by Individuals, conducted from 1994 to 1996. These data include detailed information on all food items consumed by several thousand adults over two nonconsecutive days. We limit our analysis to individuals who live outside of metropolitan areas to be more representative of the subjects in our natural experiment.[34] We also drop a small number of observations with obvious coding errors, leaving an analytic sample of 2,452 individuals.

We conduct two types of analyses using the food intake data. First, we examine how caloric intake differs for meals eaten at restaurants as compared to meals eaten at home. Then, we examine how caloric intake changes on days in which individuals eat at a restaurant as compared to days in which they do not eat at a restaurant. As our theoretical model implies, these two quantities may not be equal if individuals can substitute calories intertemporally throughout the day. In particular, if individuals engage in this type of compensatory behavior, we expect restaurants to have a larger effect on calories consumed at a given meal than they do on calories consumed throughout the day.

Table 7 presents coefficient estimates from the regression

$$c_{it} = \tau_0 + \tau_1 r_{it} + X_{it}\theta + w_{jt}, \tag{7}$$

where c_{it} is calories consumed by individual i during meal or day t, r_{it} is a binary indicator for whether the individual eats at a restaurant during meal or day t, X_{it} is a set of controls that

[34] Using the full sample of all adults leads to similar conclusions (see note 38).

includes indicators for lunch, dinner, and the day of the week, and w_{jt} is the least squares residual. The sample includes days in which individuals eat either zero or one meals at a restaurant.[35]

Panel A reports results from the meal-level analysis. The sample ate 11.4 percent of their meals at restaurants (Column 1). Column (2) presents results from a random effects model, which uses both between- and within-individual variation in restaurant dining to estimate the effect of restaurants on caloric intake. On average, individuals consume 242 more calories per meal at restaurants than at home. This estimate is statistically significant and corresponds to a 35 percent increase over the size of the average meal.

Column (3) presents results from a between estimator, which uses only between-individual variation in restaurant dining to estimate the effect of restaurants on caloric intake. On average, individuals that eat at restaurants consume 305 more calories per meal than individuals that do not. Some of this relationship may be due to selection – people who frequent restaurants may eat more than those who do not, even when they are not eating out. To address this possibility, Column (4) presents results for a model that includes individual fixed effects. These results show that, when a given individual eats out, he consumes 233 more calories per meal than when he eats at home.[36] While this estimate controls for the type of selection described above, it does not capture any compensatory reductions that may occur at other meals or at snack-time.[37] Both the between and fixed effects estimates are upwardly-biased estimates of the effect of restaurant meals on total caloric intake – the between estimate because of selection and the fixed effects

[35] While our theoretical model allows an individual to eat at a restaurant once per day, there are days when people eat out more than once. In an extension of the model where we allow restaurant consumption in both periods of the day, we find that when an individual chooses to eat multiple meals at restaurants in a given day, the marginal cost of a calorie at the daily level falls, daily caloric intake rises, and substitution takes place between adjacent days rather than between meals during the same day. Since we do not have food intake data for consecutive days, we limit the empirical analysis to days on which individuals eat out once or not at all – these days comprise 93 percent of the sample. Our ultimate conclusions about selection and compensatory behavior also hold in an analysis that includes all days (see note 38).

[36] All estimates in Panel A rely upon individuals' assignment of food items to specific meals. This measure is subjective and may confound the results if individuals group food items into meals differently at restaurants than they do at home. We explore the sensitivity of the results by instead assigning food items to meals using the time at which the item was consumed. Items consumed from midnight to 11 am are assigned to "breakfast," items consumed from 11 am to 4 pm are assigned to "lunch," and items consumed from 4 pm to midnight are assigned to "dinner." Regressions using this meal assignment (not shown) produce results qualitatively similar to those in Panel A.

[37] The between estimates, in contrast, do capture compensatory behavior at other meals within the same day, because all meals are effectively averaged to the individual-level before the cross-sectional comparisons are made.

estimate because it does not capture compensatory behavior. Accurately measuring the effect of restaurants on total caloric intake requires a daily-level analysis.

Panel B of Table 7 applies the same econometric models to data measured at the daily-level rather than the meal-level. If calories consumed throughout the day are substitutes, then our theoretical model suggests that people will compensate for larger portions at restaurants by consuming less throughout the rest of the day. Consistent with this prediction, the coefficient in the daily-level fixed effects regression is substantially less than the corresponding estimate at the meal-level. In fact, eating out increases daily intake by only 24 calories – compared to an average caloric intake of 1,944 calories per day. This effect is statistically insignificant and represents a decline of almost 90 percent from the corresponding meal-level estimate. The result suggests that, although individuals tend to eat more at restaurants, they compensate to a substantial degree by eating less throughout the rest of the day. Meal-level estimates therefore overestimate the net effect of restaurants on total caloric intake. The between-individual coefficient is significantly larger than the fixed effects coefficient (235 versus 24), implying that individuals who frequent restaurants also eat more at home. This difference suggests that selection may explain why a number of observational studies have found a link between caloric intake and food away from home. Of course, even with individual fixed effects, the decision to eat at a restaurant is not exogenous. Given the size of restaurant portions, we suspect that consumers tend to eat at restaurants on days when they are hungrier. The 24 calorie per meal estimate therefore represents an upper bound and suggests that restaurant meals do not have a substantive causal effect on total caloric intake.

How much weight gain is associated with an additional 24 calories per restaurant meal? A one calorie increase, repeated every day in perpetuity, raises weight by approximately 0.08 pounds (Cutler, Glaeser, and Shapiro 2003). Banning restaurants entirely might therefore reduce average weight by 0.55 pounds (that is, 23.9 calories per meal * 0.286 restaurant meals per day * 0.08 pounds per steady-state calorie). More practical public policies would likely have even smaller effects. Assuming the own price elasticity of demand is -1, a one dollar (13 percent) increase in restaurant prices might reduce weight by 0.07 pounds, or about 0.01 BMI points. This estimate closely matches the BMI effects presented in the first column of Table 6, suggesting that the results of the natural experiment are consistent with demand for restaurant food that is unit elastic or greater.[38]

[38] This conclusion is robust to various analytical extensions. When an individual eats out multiple times in the same day, there is less scope for compensatory behavior at the daily-level (see note 35). Replicating the

Although a restaurant "fat tax" would have little effect on obesity, it could produce substantial deadweight loss. Here we estimate the costs of such a tax and compare them to the upper range of the potential health benefits (medical cost avoidance). To compute the welfare costs, we need to know the own-price elasticity of demand for restaurants. Table 8 reports the deadweight loss associated with a 50 percent tax under three different restaurant own-price elasticities of demand that fall within the range suggested by the literature: -0.5, -1.0, and -2.0. Using a constant elasticity demand curve, the value of consumer welfare lost ranges from $99.4 billion to $134.1 billion per year, and the total deadweight loss ranges from $12.3 billion to $33.1 billion per year.[39]

Next we compute a conservative estimate of the potential benefits of the restaurant tax to compare to the deadweight loss. The results from our natural experiment (presented in Section 5) suggest that taxing restaurant consumption would provide minimal benefit to public health. The point estimates, reported in Table 6, are close to zero and precisely estimated. To be conservative, assume that the effect of restaurant prices on body mass is one standard error greater than our point estimate (this corresponds to approximately the 85^{th} percentile). In that scenario, a 50 percent tax would reduce the prevalence of overweight individuals by 1.3 percentage points – compared to the 66 percent of Americans who were overweight in 2004. Using results from Finkelstein et al. (2003), a 1.3 percentage point decrease in the prevalence of overweight individuals reduces covered medical expenditures by $2.3 billion (reported in the

entire analysis including days on which individuals eat out twice or more implies that a one dollar decrease in restaurant prices could increase BMI by 0.04 points. Although this effect is several times larger than the effect reported above, it remains trivial in magnitude and is statistically indistinguishable from the results of our natural experiment. The total effect of a restaurant meal on these days is still less than half the magnitude of the effect implied by the meal-level results in Panel A, and the overall effect on weight remains minimal because it is rare to eat out more than once per day. Expanding the sample to include urban and suburban consumers also generates similar conclusions. The effect of a single restaurant meal is somewhat larger, and the effect of two or more restaurant meals is somewhat smaller; however, the changes are statistically insignificant, and the net effect of a one dollar decrease in restaurant prices remains minimal (0.06 BMI points).

[39] Because the compensated demand curve is unobservable, we integrate the area under the uncompensated demand curve between $7.94 (the average restaurant price) and $11.91 (the counterfactual price under a 50 percent tax) to compute the amount of consumer welfare lost. We specify the demand curve as $ln(q) = a - \varepsilon \cdot ln(p)$, where ε is the own-price elasticity of demand, and a is determined by the 2002 equilibrium of p = $7.94 and q = $37.57 billion. Because spending on food away from home accounts for only two percent of national income, income effects are likely negligible, and the uncompensated demand curve provides a reasonable approximation of consumer welfare lost (Hines 1999).

second-to-last column of Table 8).[40] The last column of Table 8 combines this estimate of the benefits with estimates of the deadweight loss from the previous paragraph to compute the ratio of the welfare costs to the potential benefits associated with a 50 percent restaurant tax. In all cases, the costs dominate the benefits, and the cost-benefit ratio ranges from 5.3-to-1 to 14.4-to-1.

While the deadweight loss associated with a tax policy is substantial, the deadweight loss associated with a zoning policy against restaurants, such has those proposed in New York City and Los Angeles, is likely even greater. With a tax policy, the government recaptures all of the out-of-pocket price increase from consumers. But with zoning regulations, only part of the effective price increase is recaptured by nearby firms while the rest is dissipated in increased time and fuel expenditures by consumers who travel further to access their nearest restaurant and wait in longer lines when they arrive.

7. Conclusion

At this point, many policymakers and public health advocates have shifted to designing policies intended to reduce the impact of restaurants on obesity, even while they acknowledge that convincing evidence of such a link has proven elusive. For example, the FDA recently organized a forum in which participants focused on proposing implementable solutions to the challenge of obesity in the context of away-from-home foods, even while the organizers cautioned that "there does not exist a conclusive body of evidence establishing a causal link between the availability or consumption of away-from-home foods and obesity" (Keystone 2006).

Our findings indicate that the causal link between the availability of restaurant foods and obesity is minimal at best. Manipulating the distance to the nearest restaurant using Interstate Highway proximity as an instrument demonstrates that restaurants have no significant effect on BMI or overweight status. These results are precisely estimated and robust to different specifications and samples. Translating the distance measure into an economic cost, point estimates imply that a 50 percent reduction in restaurant prices would have no positive effect on the prevalence of

[40] Finkelstein et al. (2003) estimate that private insurers, Medicare, and Medicaid spent $65.7 billion covering obesity-related illnesses in 1998 ($97.2 billion in 2007 dollars, inflated using the CPI Medical Care Services index). The prevalence of overweight individuals was 53.6 percent in 1998, implying total costs of $1.8 billion per percentage point (2007 dollars). We exclude out-of-pocket costs in this calculation because those costs are likely already internalized by consumers. Nevertheless, this number may overstate the relevant savings if employers discriminate against obese individuals because they have higher health care costs. In this scenario, some costs paid by insurers would already be internalized by consumers.

overweight individuals. Even the extreme tail of a 95 percent confidence interval implies that this policy would not reduce the overweight prevalence by more than 4.6 percentage points (while 66 percent of Americans were overweight in 2004). Similar conclusions hold when using BMI or an obese indicator as the outcome of interest.

Detailed analyses with food intake data reveal that, although restaurant meals are associated with greater caloric intake, many of these additional calories are offset by reductions in eating throughout the rest of the day. We also find evidence of selection – individuals that frequent restaurants appear to be those who eat more even when at home. These facts indicate that previous research demonstrating positive correlations between eating out and obesity or caloric intake may be confounded by a lack of exogenous variation in restaurant access. Although our results apply specifically to rural consumers, the central conclusions are likely to generalize to urban consumers as well. The summary statistics in Table 1 indicate that our analytic sample is similar to the national sample in terms of BMI and overweight prevalence, and our observations on offsetting behavior and selection hold for urban as well as rural consumers.

These results, combined with work in the context of traffic safety (Peltzman 1975) and tobacco (Adda and Cornaglia 2006), suggest that regulating specific inputs into the health and safety production functions can be ineffective when optimizing consumers can compensate along other margins. Although restaurants conveniently deliver calories at a low marginal cost, they are only one source among many.[41] The same principle may apply to other targeted obesity interventions as well. For example, two recent large-scale, multi-state randomized trials of school-based programs that improved the nutritional content of cafeteria meals found no effect on student weight (Nader et al. 1999; Caballero et al. 2003). One principal investigator notes, in retrospect, that the intervention could not control what the children ate outside of school (Kolata 2006). Future research and policy proposals may find greater success if it is designed to account for the optimizing behavior of the targeted subjects.

[41] The relative price of food is at a historic low (Lakdawalla and Philipson 2002), and inexpensive snack foods are prevalent (Cutler et al. 2003). Bleich et al. (2007) show that food is so readily available in developed countries that consumers are literally throwing it away at increasing rates.

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Appendix

A1. Detailed Analysis of Distance to Nearest Restaurant

We conduct a detailed analysis of 32 randomly sampled ZIP codes with two objectives: (1) to check the accuracy of our ZIP code level distance measure and (2) to estimate the average distance to a restaurant when a ZIP code contains a restaurant. The ZIP code detail sample is stratified by state and contains 11 ZIP codes without restaurants and 21 ZIP codes with restaurants. The unit of observation in this analysis is the Census block; these 32 ZIP codes contain a total of 6,096 Census blocks.

For each Census block, we compute the distance to the nearest restaurant (identified via a search of the *Yahoo! Yellow Pages*) along the United States road network using ArcGIS. We also record driving time to the nearest restaurant using posted speed limits; we inflate these speed limits by 30 percent to account for speeding. Finally, we calculate the average distance (and driving time) to the nearest restaurant for each ZIP code as a population-weighted average of the distances for each Census block within the ZIP code.

For the sample of 11 ZIP codes without restaurants, the average distance to the nearest ZIP code containing a restaurant is 9.6 miles (standard error of 1.2 miles). Using the Census block data, we compute an average distance of 8.8 miles to the nearest ZIP code containing a restaurant (standard error of 0.6 miles). In spite of the small sample of ZIP codes, the two estimates differ by less than 10 percent. We cannot reject equality of the two estimates, and we conclude that the ZIP code level distance calculations are reasonably accurate.[42]

The sample of 21 ZIP codes with restaurants reports an average distance of 2.5 miles to the nearest ZIP code containing a restaurant. This estimate is fairly precise (standard error of 0.3 miles), and we use it to estimate the average distance to the nearest restaurant for residents of ZIP codes containing a restaurant.

[42] Note that differences of 10 or 20 percent would not change the qualitative conclusions of this paper.

A2. Travel Time Valuation

Little evidence exists regarding rural motorists' valuation of travel time. Previous studies whose results have relied on estimates of this quantity, e.g. Ashenfelter and Greenstone (2004), have generally used the average hourly wage to approximate the value of travel time. However, these estimates may be too large if individuals find traveling to be less unpleasant than working. To establish a conservative lower bound on the value of travel time, we collect a unique data set of automobile speeds on an unobstructed rural roadway. Based on these speeds, we measure the degree to which drivers trade off travel time reductions for increased gasoline consumption.

Few drivers achieve the Environmental Protection Agency (EPA) mileage figures for their vehicles, in spite of downward adjustments that the EPA makes to its actual measured results (CNN/Money 2004). The primary reason for this real-world shortfall is that most drivers accelerate harder and drive faster than EPA laboratory tests assume.[43] At speeds in excess of 60 miles per hour (mph), fuel economy declines at an average of 1.5 percent per 1 mph (Davis and Diegel 2007).[44] The average driver therefore trades off 3.7 gallons of gasoline for each hour of time savings when traveling at 73 mph (the median speed in our data set).[45]

Our data consist of a sample of 200 vehicles surveyed on East Pacheco Boulevard between Los Banos, California, and Chowchilla, California, on October 22, 2007. The speed limit on this rural four-lane roadway is 65 mph, and its location makes it unlikely to be traversed by intercity travelers traveling between the major metropolitan areas of San Francisco, Los Angeles, and Sacramento. Vehicles were randomly surveyed by a radar-qualified officer from a major San Francisco Bay Area police department using a U.S. Radar Phantom handheld unit (accurate to +/-

[43] Acceleration is more important for city mileage, and cruising speed is more important for highway mileage. The EPA highway test runs vehicles at an average speed of 48 mph and a top speed of 60 mph. Recognizing that most drivers exceed these speeds, the EPA is revising its tests in 2008 to include a "high speed" segment that tests at speeds of up to 80 mph (Davis and Diegel 2007).

[44] This fact underlies the passage of the 55 mph national speed limit in 1974, an energy conserving measure enacted in response to rising oil prices.

[45] At 73 mph, increasing speed by 1 mph to 74 mph reduces the time needed to travel 73 miles by 0.0135 hours (49 seconds), but significantly increases fuel consumption. The fleet average EPA highway mileage over the last decade is 23.0 mpg (U.S. EPA 2007). Assume this is a reasonable estimate for fuel consumption at 65 mph; then average fuel consumption at 73 mph is approximately 20.2 mpg (based on fuel economy declining by 1.5 percent per 1 mph). At 20.2 mpg, increasing speed from 73 mph to 74 mph raises the amount of gas needed to travel 73 miles by 0.0495 gallons. On the margin, therefore, gas is exchanged for travel time at 0.0495 gallons/0.0135 hours = 3.67 gallons/hour when traveling at 73 mph.

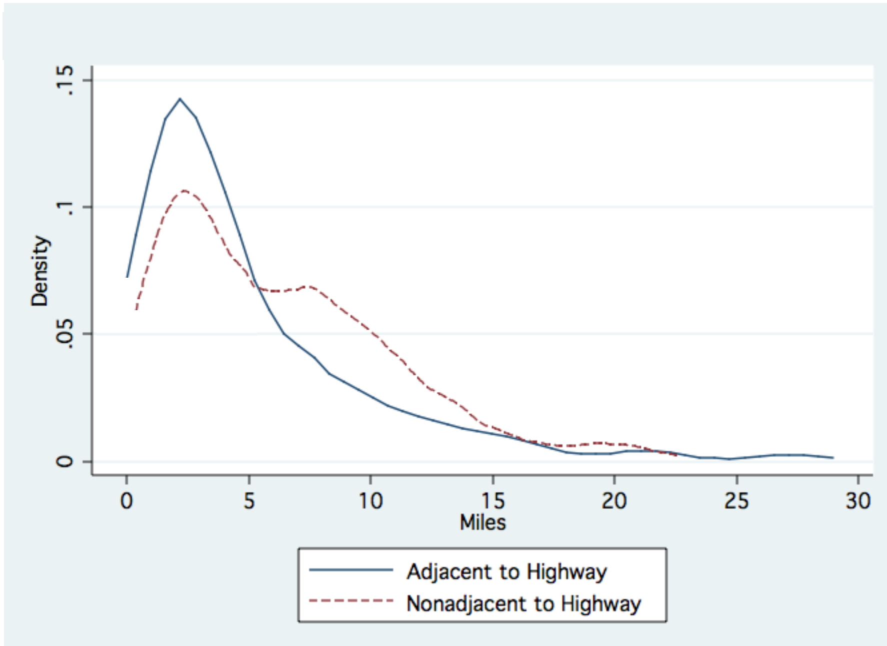
0.1 mph).[46] The survey vehicle was not visible from the roadway, so drivers were unlikely to have modified their speeds.[47]

The median driver's speed on this rural roadway was 73 mph.[48] With local regular unleaded gas priced at $3.30 per gallon at the time of the survey, the median driver traded gas for travel time savings at a rate of 3.67 gallons/hour * $3.30/gallon = $12.11 per hour. Lower quantiles also demonstrated a high valuation of time. The 25th percentile of vehicles in our data set traveled at 69 mph, and the 10th percentile of vehicles traveled at 67 mph. Even the 5th percentile of vehicles traveled at 65 mph (the posted maximum speed).

The $12.11 per hour estimate is conservative in at least two ways. First, it does not account for the increased risk of injury or death that drivers face when traveling at higher speeds. Furthermore, it does not account for the increased probability of receiving a speeding citation. We therefore interpret this estimate as a lower bound on the value of travel time.

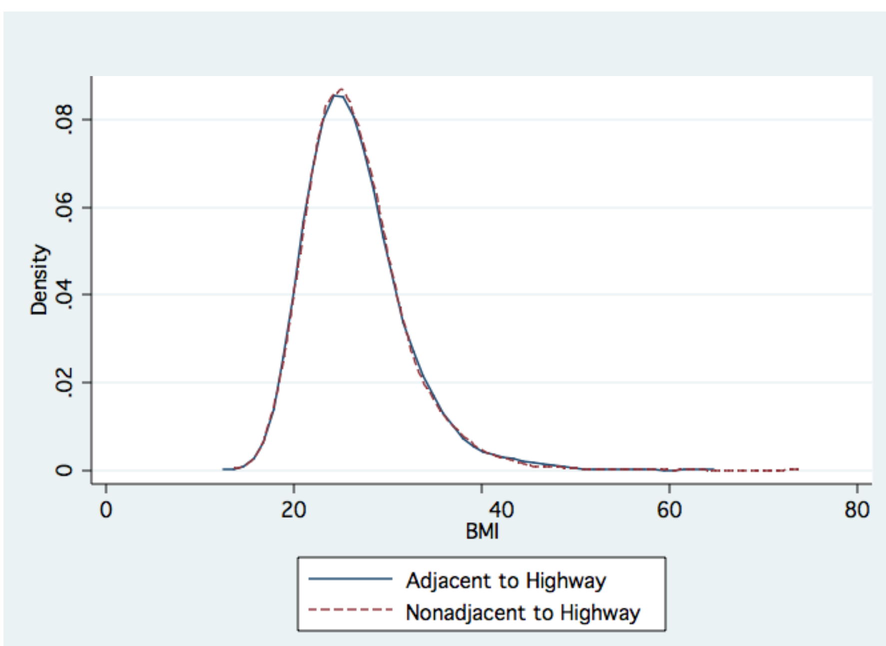
[46] A cosine correction of 1.02 was applied to adjust for the exact angle at which the vehicles were surveyed.
[47] To the extent that any drivers with radar detectors slowed down from their preferred speed, our estimates understate the true valuation of travel time.
[48] The maximum speed was 94 mph.

Figure 1. Distribution of Distance to Nearest Restaurant



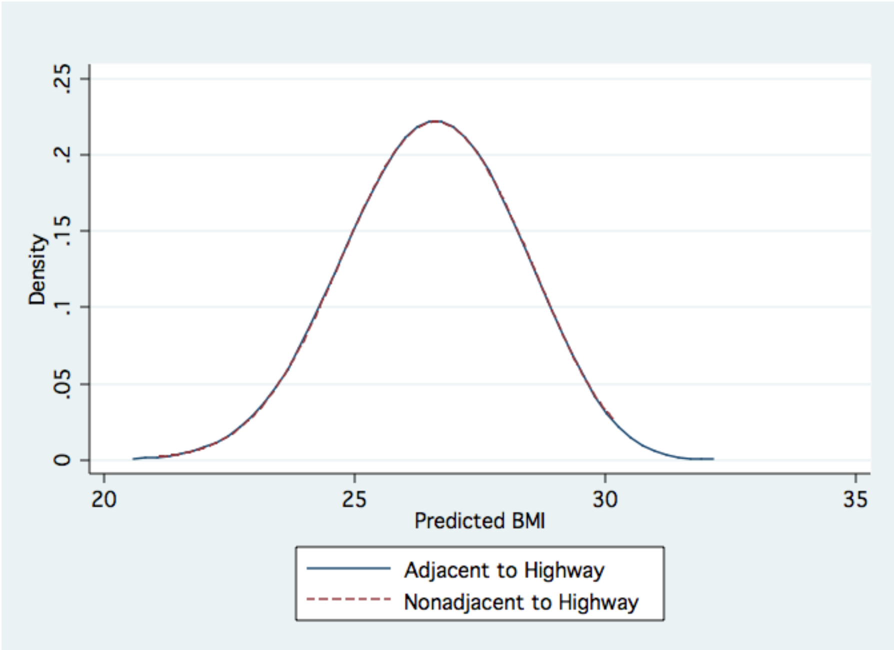
Note: Distribution of distance to the nearest restaurant for ZIP codes that are adjacent and nonadjacent to Interstate Highways. See Section 4 and Appendix A1 for details.

Figure 2. Distribution of Body Mass Index



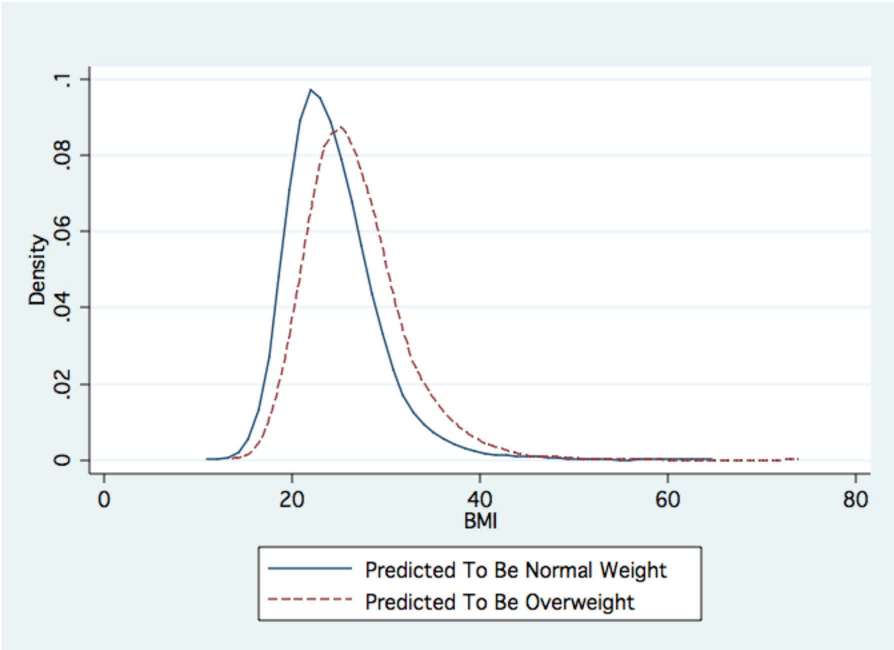
Note: Distribution of body mass index (BMI) for residents of towns that are adjacent and nonadjacent to Interstate Highways.

Figure 3. Distribution of BMI Risk



Note: Distribution of an index of predicted BMI for residents of towns that are adjacent and nonadjacent to Interstate Highways. The index consists of the fitted values from a regression of BMI on a set of observed characteristics. See Section 4 for details.

Appendix Figure A1. Distribution of Body Mass Index



Note: Distribution of BMI for individuals with a predicted BMI of less than 25 (predicted to be normal weight) and greater than 25 (predicted to be overweight) using the BMI risk index depicted in Figure 3.

Table 1: Summary Statistics

	Analytic Sample			National Sample		
	Mean	Std Deviation	Sample Size	Mean	Std Deviation	Sample Size
Panel A: *Individual Level BRFSS Data*						
BMI	26.57	5.24	13,470	26.49	5.39	2,608,980
Overweight (BMI ≥ 25)	0.58	0.49	13,470	0.56	0.50	2,608,980
Female	0.58	0.49	13,470	0.59	0.49	2,608,980
Age	50.6	17.7	13,429	48.2	17.3	2,597,839
Employed	0.61	0.49	13,458	0.61	0.49	2,602,142
College	0.51	0.50	12,237	0.57	0.49	2,604,957
Panel B: *ZIP Code Level Census Data*						
White	0.93	0.10	551	0.83	0.21	39,960
College	0.43	0.13	551	0.46	0.17	39,917
Median HH Income	$34,689	$7,728	551	$39,676	$16,715	40,021
Any Restaurant	0.61	0.49	551	0.78	0.41	40,021
Any Full Service Restaurant	0.55	0.50	551	0.74	0.44	40,021
Any Limited Service Restaurant	0.36	0.48	551	0.64	0.48	40,021
Number Full Service Restaurants	2.38	4.83	551	9.45	15.83	40,021
Number Limited Service Restaurants	1.64	3.51	551	8.36	12.84	40,021

Note: Unweighted summary statistics for the analytic sample and the full national sample are reported. The analytic sample consists of all telephone exchanges or ZIP codes in Arkansas, Colorado, Iowa, Kansas, Maine, Missouri, North Dakota, Nebraska, Oklahoma, Utah, and Vermont that are located less than 10 miles from an Interstate Highway, more than 30 miles from an urban area, and have population density less than 80 persons per square mile. The data in Panel A are from the BRFSS, and the standard deviations are calculated at the individual level. The data in Panel B are from the Census, and the standard deviations are calculated at the ZIP code level.

Table 2: ZIP Code Level Covariate Balance

	Near Restaurant	Near Interstate
Dependent variable		
i) Percent Male	-0.009	-0.003
	(0.001)	(0.002)
ii) Percent White	-0.039	-0.001
	(0.003)	(0.007)
iii) Percent Under 21	-0.0015	-0.0001
	(0.0015)	(0.0038)
iv) Percent Over 65	-0.0017	-0.0004
	(0.0014)	(0.0037)
v) Percent With Some College or More	0.061	0.002
	(0.003)	(0.009)
vi) Median Household Income	2,736	29
	(253)	(593)
State-by-year fixed effects	Yes	Yes
Sample:		
Description	Full sample	Analytic sample
Number of ZIP Codes	7,105	551

Note: Each coefficient represents a separate regression; rows corresponds to different dependent variables, and columns correspond to different samples of ZIP codes and regression specifications. In the first column ("Near Restaurant"), the reported coefficients are from regressions on an indicator variable for whether a ZIP code contains one or more restaurants; estimates in this column are based on a full sample of ZIP codes in the states represented in the analytic sample. In the second column ("Near Interstate"), the reported coefficients are from regressions on an indicator variable for whether a ZIP code is adjacent to an Interstate Highway; estimates in this column are based on the analytic sample (defined in the note to Table 1). All regressions contain state-by-year fixed effects. Robust standard errors are reported in parentheses.

Table 3: Individual Level Covariate Balance

	Coefficient	Sample Size
<u>*Dependent Variable*</u>		
i) BMI Risk Index	-0.038	12,797
	(0.029)	
ii) Overweight (BMI ≥ 25) Risk Index	-0.0037	12,797
	(0.0031)	
iii) Average Income ($)	-482	10,560
	(936)	
iv) Ever Smoked	-0.019	9,180
	(0.012)	
v) Desired Weight (in kg)	-0.280	4,154
	(0.425)	
vi) Exercised in Last Month	0.012	6,731
	(0.016)	
State-by-year fixed effects	Yes	

Note: Each coefficient represents a separate regression. The reported coefficients are from regressions of the indicated dependent variables on an indicator for whether the respondent's telephone prefix is adjacent to an Interstate Highway. All regressions contain state-by-year fixed effects. Standard errors corrected for within-prefix correlation in the error term are reported in parentheses. BMI (Overweight) risk index consists of the fitted values from a regression of BMI (Overweight) on a set of observed covariates: gender, a quadratic in age, indicators for educational attainment, employment, unemployment, and marital status, and a full set of state-by-year fixed effects.

Table 4: First-Stage - Effect of Interstate Proximity on Restaurant Access

	(1)	(2)
Dependent Variable		
i) Miles to nearest ZIP with restaurant	-1.50	-1.38
	(0.39)	(0.45)
ii) Any Restaurant	0.175	0.165
	(0.042)	(0.047)
iii) Any Full Service Restaurant	0.196	0.188
	(0.042)	(0.046)
iv) Any Limited Service Restauarant	0.154	0.131
	(0.040)	(0.043)
v) Restaurants per 1,000 people	0.55	0.63
	(0.19)	(0.21)
vi) Travel Cost	-$1.22	-$1.13
	(0.32)	(0.37)
Sample:		
Population Density Cutoff (People Per Sq Mile)	80	40
Number of ZIP Codes	551	460

Note: Each coefficient represents a separate regression. The reported coefficients are from regressions of the indicated dependent variables on an indicator for whether the respondent's ZIP code is adjacent to an Interstate Highway. All regressions contain state-by-year fixed effects. Robust standard errors are reported in parentheses.

Table 5: Reduced Form - Effect of Interstate Proximity on Obesity

	(1)	(2)	(3)	(4)	(5)
<u>*Dependent Variable*</u>					
i) Overweight Status	-0.007	-0.004	-0.007	-0.003	0.001
(BMI ≥ 25)	(0.011)	(0.012)	(0.011)	(0.011)	(0.012)
ii) BMI	0.002	0.026	0.010	0.047	-0.003
	(0.127)	(0.143)	(0.133)	(0.124)	(0.136)
State-By-Year Effects	Yes	No	Yes	Yes	Yes
Covariates	No	No	No	Yes	Yes
Sample Size	13,470	13,470	12,175	12,175	7,956

Note: Each coefficient represents a separate regression. The reported coefficients are from regressions of the indicated dependent variables on an indicator for whether the respondent's telephone prefix is adjacent to an Interstate Highway and a set of controls. Where indicated, the controls include state-by-year fixed effects and/or the following covariates: gender, a quadratic in age, and indicators for educational attainment, employment, unemployment, and marital status. Standard errors corrected for within-prefix correlation in the error term are reported in parentheses.

Table 6: Effect of Restaurant Access on Obesity

	All Individuals		Not Employed	
	(1)	(2)	(3)	(4)
Panel A: *Effect of Being One Mile Closer to a Restaurant on:*				
Dependent Variable				
i) Overweight Status	-0.005	-0.004	-0.008	-0.005
(BMI ≥ 25)	(0.007)	(0.009)	(0.011)	(0.014)
ii) BMI	0.001	-0.074	-0.006	-0.055
	(0.085)	(0.102)	(0.126)	(0.163)
Panel B: *Effect of Lowering Restaurant Prices by $1 on:*				
Dependent Variable				
i) Overweight Status	-0.006	-0.005	-0.009	-0.006
(BMI ≥ 25)	(0.009)	(0.011)	(0.013)	(0.017)
ii) BMI	0.002	-0.091	-0.008	-0.068
	(0.104)	(0.125)	(0.154)	(0.200)
Sample:				
Population Density Cutoff (People Per Sq Mile)	80	40	80	40
Observations	13,470	8,575	5,208	3,368

Note: Two-sample two-stage least squares estimates are reported. The estimates represent different dependent variables, samples, and econometric specifications for the effect of restaurant access. All estimates control for state-by-year fixed effects and use an indicator for proximity to an Interstate Highway as an instrument for restaurant access. Standard errors corrected for within-prefix correlation in the error term are reported in parentheses.

Table 7: Relationship Between Restaurants and Caloric Intake

	Percent at Restaurant (1)	Random Effects (2)	Between Estimator (3)	Fixed Effects (4)	Hausman *p* -value (5)	Sample Size (6)
Panel A: *Meal-Level* (mean = 665.1 calories)						
Eat at Restaurant	11.4	241.7	305.3	233.3	0.092	10,440
		(17.5)	(35.1)	(17.9)		
Panel B: *Daily-Level* (mean = 1,944.1 calories)						
Eat at Restaurant	28.6	104.7	235.2	23.9	0.000	4,316
		(27.3)	(39.5)	(36.1)		

Note: This table presents an analysis of caloric intake by rural individuals based on data collected by the U.S. Department of Agriculture. The sample includes individuals aged 18 or older on days in which the person ate either zero or one meals at a restaurant. Column (1) shows the percent of observations that include some restaurant food. Columns (2) through (4) report coefficients from regressions of caloric intake. The number of calories consumed during a given meal or day is regressed on an indicator for whether the food was from a restaurant and a set of controls. The controls include indicators for lunch, dinner, and the day of the week. Standard errors corrected for within-household correlation in the error term are reported in parentheses. Column (5) reports *p* -values associated with Hausman tests for the hypotheses that the Random Effects and Fixed Effects estimates are equal (p-values are not adjusted for within-household correlation).

Table 8: Potential Deadweight Loss From 50% Restaurant Tax

	Cost						
Demand Elasticity	Consumer Welfare Loss ($ billion)	–	Government Revenue ($ billion)	=	Deadweight Loss ($ billion)	Benefit ($ billion)	Cost-Benefit Ratio
-0.5	134.1		121.8		12.3	2.3	5.3 : 1
-1.0	121.0		99.4		21.5	2.3	9.3 : 1
-2.0	99.4		66.3		33.1	2.3	14.4 : 1

Note: This table reports estimates of the health benefits (medical cost avoidance) and welfare loss associated with a hypothetical 50 percent tax on restaurant food. The deadweight loss (consumer welfare loss net of government revenue) is calculated using a constant-elasticity demand curve and a range of assumptions for the restaurant own-price elasticity of demand. (The difference between consumer welfare loss and government revenue may not exactly equal the deadweight loss due to rounding.) The benefit is calculated under the optimistic assumption that the tax would reduce the prevalence of overweight individuals by 1.3 percentage points (one standard error greater than the point estimate from Table 6) and using estimates of the external costs of treating obesity-related illnesses from Finkelstein et al. (2003). The cost-benefit ratio equals the deadweight loss divided by the benefit.