Are Restaurants Really Supersizing America?*

Michael Anderson
UC Berkeley
mlanderson@berkeley.edu

David A. Matsa
Northwestern University
dmatsa@northwestern.edu

December 18, 2008

Abstract

Regulating specific inputs into health and safety production functions is unlikely to be effective when optimizing consumers can compensate in other ways. 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: does greater availability of 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 exploit exogenous variation in 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 frequency of consumption, 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.

JEL codes: D12, H25, I12, I18

Keywords: economics of regulation, health production, obesity, fat tax

* We thank David Card, Carlos Dobkin, Michael Greenstone, Alex Mas, Amalia Miller, Enrico Moretti, Kathryn Shaw, May Wang, and seminar participants at Northwestern University, the NBER Summer Institute, Stanford University, Tel Aviv University, and UC Berkeley for helpful comments. Ellen Kersten, David Reynoso, Tammie Vu, and Kristie Wood provided excellent research assistance. We appreciate the cooperation of the Departments of Health of Arkansas, Colorado, Iowa, Kansas, Maine, Missouri, North Dakota, Nebraska, Oklahoma, Utah, and Vermont in providing the confidential data used in this study; their cooperation does not imply endorsement of the conclusions of this paper. We are also grateful to an anonymous police department for assistance in collecting data on motor vehicle speed.

"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). Whereas 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). The time series of obesity rates in the United States, plotted in Figure 1 (solid line), reveals that the rate of increase over the past quarter-century has been substantially greater than during the preceding two decades. 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. For example, in response to high obesity rates in low-income neighborhoods, the Los Angeles City Council unanimously approved a law on July 29, 2008, banning the opening of new fast-food restaurants in a 32-square mile area containing 500,000 residents (Abdollah 2007; Hennessy-Fiske and Zahniser 2008). If large portions and effective marketing (presentation and pricing) lead people to eat more when they go to restaurants than when they eat at home, then these regulations may be effective. 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.

[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).

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 data sets and 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; Babey et al. 2008). Furthermore, the number of restaurants and the prevalence of obesity have been rising for a number of decades. In addition to the obesity rate, Figure 1 shows the growth of restaurant density in the United States over the past half-century (dashed line). The close correspondence between these series has led some researchers to propose that there is 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 percent of the rise in the percentage of Americans who are obese. Despite some evidence contradicting these findings (Simmons et al. 2005; Jeffery et al. 2006), there appears to be broad consensus among the health policy community that greater availability of restaurants increases body 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, leaving restaurant consumption correlated with other dietary practices associated with weight gain (Newby et al. 2003; Paeratakul et al. 2003). A key question is whether the growth in the number of establishments and in portion sizes is contributing to the obesity epidemic, or whether these changes merely reflect consumer preferences. Whether or not consumers maximize utility in the classical sense, surely some increase in the prevalence of restaurants and portion sizes is driven by secular increases in time costs and consumers' demand for calories. To the extent that changes in preferences are leading consumers to eat out more, regulating restaurants may only lead consumers to shift consumption to other sources rather than to reduce total caloric intake.

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 of this framework is that eating at restaurants may have little or no causal impact on

[3] In fact, many public health advocates have moved to design 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).

obesity. The model suggests that consumers' preference for high caloric intake may explain the observed correlations between restaurant eating and obesity.

To assess the nature of the connection between restaurants and obesity, we design a natural experiment in which we exploit exogenous variation in the effective price of restaurants (due to the different costs to consumers of traveling to 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 (and reduces the travel cost associated with visiting a restaurant) in a manner that is plausibly uncorrelated with demand or general health practices. Using original survey data based on a smaller sample, we show that differences in travel costs generate large differences in fast-food consumption. To uncover the causal effect of restaurants on obesity, we then compare the prevalence of obesity in communities located immediately adjacent to Interstate Highways with the prevalence of obesity in communities located slightly farther away.

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 the prevalence of obesity are close to zero and precise enough to rule out any meaningful effects. But given that a typical restaurant meal contains more calories than a home-cooked meal, why does lowering restaurant prices 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 than other consumers when they eat at home. Second, people who eat relatively large portions in restaurants tend to reduce their calorie consumption at other times during the day. After accounting for these factors, we find that the existence of restaurants increases BMI by only 0.2 BMI points for the typical obese consumer.

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 in other ways. 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) have argued that smokers react to cigarette taxes by smoking fewer cigarettes more intensively. In the case of obesity, consumers have access to multiple sources of cheap calories. Restricting a single source – such as restaurants – is therefore unlikely to affect obesity, as our findings confirm. This mechanism may also underlie the apparent failure of so many interventions targeted at reducing obesity (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 model shows that an optimizing consumer will compensate for larger portion sizes at restaurants by eating less at other times. In this scenario, OLS estimates of the relationship between restaurants and caloric intake are likely to give misleading results. Section 3 describes the data we use in our study. Section 4 describes our quasi-experimental methodology and presents the main results using both graphical and regression analyses. These results imply that eating out has little to no causal effect on obesity. Section 5 explores and rules out three alternative interpretations of our results. Section 6 discusses the causal mechanisms that might explain why restaurants do not affect obesity and presents evidence of inter-meal calorie offsetting. Based on these results, Section 7 analyzes the welfare effects of a potential restaurant "sin tax," and Section 8 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 aimed at decreasing 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 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 about 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. This modeling approach is similar to that used by Jeitschko and Pecchenino (2006), who argue that the socially optimal size of restaurant meals is larger than the size of the average home-cooked meal – even though the larger portion size leads some consumers to overeat. Our model is also related to the dynamic theory of weight management developed by Lakdawalla and Philipson (2002), which also models food intake by an optimizing consumer. While they use a dynamic model to examine how changes in average food prices, income, and physical activity affect steady-state weight, we use a simpler static model to illustrate how changes in the *relative* prices of specific foods affect *total* food consumption. Our conclusions extend to the dynamic weight-management framework.

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. 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 perfect substitutes for calories consumed at home and that the agent has quasi-linear preferences in caloric consumption and another composite good x (Jeitschko and Pecchenino 2006):[5]

[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).

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

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 of eating more at snack-time and vice versa, $u_{c_1 c_2} < 0$. 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:

$$Y - \mathrm{I}(c_{1H} > 0)(f_{1H} + p_{1H}\, c_{1H}) - \mathrm{I}(c_{1R} > 0)(f_{1R}) - p_2\, c_2 - x \geq 0 \qquad (2)$$

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 at a restaurant, but not in both places. 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 of caloric consumption 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 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.

[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.

[6] For simplicity, we do not model explicitly the agent's option to save unconsumed food purchased from the restaurant for other meals or snacks. Implicitly, this sort of transfer between meals is one form of calorie offsetting addressed by Result 2, and it works to undermine a causal effect of restaurant portion size on obesity. Nevertheless, to the extent that physically transferring food between meals is costly (for example, if food quality is reduced or if there is a chance of spoilage), this sort of direct offsetting is unlikely to be complete.

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. In this framework, decreasing the price of restaurant food, f_{1R}, makes the agent more likely to eat at a restaurant, which increases consumption at that meal but may or may not increase total caloric intake.

Result 3: Total caloric intake depends on $u(\cdot)$. Even if food prices are constant across the population, total caloric intake varies from person to person, depending on the agent's preferences. 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 a preference for high 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, and OLS estimates of the relationship between restaurants and obesity are likely to give misleading results. 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 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 a confidential extract of 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

[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).

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 overweight and obese indicators that equal unity if BMI is greater than 25 or 30, respectively. These measures are standard in the obesity literature, and the obese indicator is of particular interest because mortality risk increases as BMI exceeds 30 (Adams et al. 2006). Data on height and weight in the BRFSS are self-reported. Although some respondents may misreport this information, Cawley (1999) and Ezzati et al. (2006) find the degree of misreporting to be minimal, and there is no reason to suspect that misreporting would be more or less prevalent in rural towns adjacent to Interstate Highways (our instrument for restaurant proximity) than in other nearby towns.

Restaurant establishment data are from the United States Census ZIP Code Business Patterns. These data include counts of full-service ("sit-down") and limited-service ("fast-food") restaurants for every ZIP code in the United States.[9] Fast-food restaurants are usually busiest at lunchtime, and their most popular menu type is hamburgers (43 percent of sales), followed by pizza (13 percent), sandwich/sub shop (10 percent), chicken (9 percent), and Mexican (8 percent) (U.S. Census Bureau 2005). A majority of these establishments (65 percent of sales) operate

[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] 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.

under a trade name. U.S. sales at McDonald's, the largest chain, totaled almost $29 billion in 2007 – over three times more than its closest rival (Technomic 2008). Other fast-food chains with over $5 billion in U.S. sales include Burger King, Subway, Wendy's, Taco Bell, and KFC. Sit-down restaurants, on the other hand, are usually busiest at dinnertime, rarely operate under a trade name (12 percent of sales), and tend to serve "American" food (47 percent). The largest "sit-down" chains include Pizza Hut, Applebee's, Chili's, and Olive Garden. The average price of a sit-down meal is more than twice that of fast food – $12.30 versus $5.51, excluding tax and tip (U.S. Census Bureau 2005).

Because the restaurant data are identified by ZIP code and the obesity data are identified by telephone exchange, it is impossible to create an exact link between the two data sets.[10] 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 cooperating 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.[11] (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, percent overweight, and percent obese in the analytic sample closely match national averages. However, the analytic sample is slightly

---

[10] 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.

[11] The 80 persons per square mile threshold was chosen because it represents the 10th percentile of the population density distribution. The 10th percentile corresponds to the percentage of the U.S. population living in non-metro counties with urban populations of less than 20,000 (U.S. Department of Agriculture 2004). Our results are robust to alternative threshold values, including lowering the threshold to 40-persons per square mile (reported below) and other variations in the parameters (reported in Appendix Figure A1).

older and somewhat less educated than the national sample. Panel B, based on Census data, shows 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. Restaurant Proximity and Body Mass

Our goal is to measure the causal effect of restaurant prices on body mass. For individual *i* living in town *j* during year *t*, we can write the relationship between restaurant prices and body mass as

$$b_{ijt} = \beta_0 + \beta_1 p_{jt} + \eta_t + \varepsilon_{ijt}, \tag{3}$$

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.[12] We define p_{jt} comprehensively to include not only menu prices, service charges, and taxes, but also travel and time costs. It is the latter that 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 (Waldfogel 2006). Furthermore, since the BMI data are coded by telephone exchange and the restaurant data are coded by ZIP code, combining b_{ijt} and p_{jt} in a single sample is infeasible. We address these issues by finding an instrument z_j that satisfies two essential properties: First, it affects restaurant availability, and second, it is uncorrelated with other determinants of potential BMI outcomes, ε_{ijt}.

Our instrument, z_j, exploits the location of Interstate Highways in rural areas as a natural experiment. We compare two groups of small towns: those directly adjacent to an Interstate Highway (0-5 miles away) and those slightly farther from an Interstate (5-10 miles away).[13] For

[12] 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 causal response function (Angrist and Imbens 1995).

[13] Because of confidentiality concerns, some states (Iowa and Kansas) were only willing to release categorical distance variables, rather than a continuous distance variable. Before examining the first-stage or reduced-form regressions, we chose 5 miles as the relevant cutoff for the highway distance variable and requested the data based on that cutoff. We made this choice because the average ZIP code is 80 square miles, corresponding to a circle with a 5-mile radius. On average, a ZIP code whose centroid is within 5

convenience, we refer to these two sets of towns as "adjacent" and "nonadjacent," respectively. The Interstate Highways were designed in the 1940s "to connect by routes, direct as practical, the principal metropolitan areas, cities, and industrial centers" of the United States (U.S. Department of Transportation 2002).[14] As an unintended consequence, the highways lowered transportation costs for rural towns that happened to lie on highway routes running between major cities. By using straight-line distance to the highway in constructing our instrument, we identify an exogenous source of variation in highway access. We show that this variation is sufficient to generate substantial differences in restaurant access and frequency of fast-food consumption. We avoid using distance to the nearest highway exit in constructing the instrument because the placement of exits is likely endogenously determined by town characteristics (nevertheless, our results are robust to either measure).[15]

Previous work has studied the effects of highways on rural county-level economic outcomes (Chandra and Thompson 2000; Michaels 2008). These studies conclude that highways can affect county-level economic outcomes. To avoid this problem, our study uses a much finer level of geographic detail – ZIP codes and telephone exchange areas.[16] 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.[17]

---

miles of an Interstate is thus likely to contain that Interstate. Our results are robust to alternative cutoff values, as shown in Appendix Figure A1.

[14] 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.

[15] We draw the distinction between highway exits and highway placement because of large differences in their associated costs. Highway exits can be added at little cost to through drivers, but relocating a highway to run through an entirely different town is costly in terms of both construction costs and travel time for through drivers. In fact, all stretches of Interstate Highways that we examine in this study have had no major re-routings since their original construction, and no new Interstates were added in any of our study areas during the study period. Nevertheless, our results are unchanged if we use distance to the nearest highway exit as the instrument rather than distance to the nearest highway. The first stage is of identical magnitude and statistically significant, and the reduced-form relationship between obesity and distance to the nearest highway exit is economically and statistically insignificant for all three of our obesity measures. However, the sample size decreases because we must exclude Iowa and Kansas because of the confidentiality issues (see note 13).

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

[17] In contrast to the ZIP code-level data analyzed in our study, we expect and find statistically and economically significant county-level differences in demographic variables between counties with highways and counties without highways. This remains true even if we focus only on counties that contain highways and estimate the relationship between total highway exits and demographic factors that predict obesity. Nevertheless, to demonstrate the robustness of our results, we explore the relationship between obesity and highway exits at the county level as suggested in a recent working paper by Dunn (2008).

4.1. First-Stage Relation

For a large group of individuals – through travelers on Interstate Highways – adjacent towns represent a more convenient service option than nonadjacent towns that are even slightly farther 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. In a comparison of 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 2 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.[18] But of course 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 restaurant. To construct Figure 2, we sample (with replacement) from the empirical distribution of restaurant distance for each sample ZIP code that contains a restaurant.

Figure 2 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

---

Specifically, we regress BMI on county Interstate exits and a large set of covariates using county-level BRFSS data. We find that the relationship between BMI and highway exits in the pooled 1996-2005 BRFSS data set is statistically and economically insignificant. Furthermore, when estimating the results separately by year, the relationship is statistically insignificant in every year except 2005, when the relationship is marginally significant (2005 is the one year of data analyzed in Dunn 2008). The null relationship is also robust to a wide range of sample restrictions and specifications, including detailed controls for demographic characteristics, county population density, and county economic indicators (all results are available from the authors upon request). We thus conclude that, even at the county level, there is no relationship between BMI and restaurants when using Interstate Highways as an instrument.

[18] 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.

nonadjacent ZIP codes are more likely to live 5 to 15 miles away. These distances correspond to additional roundtrip travel times of 10 to 40 minutes.[19] Given the extensive evidence in economics and marketing that even small distances can have large effects on shopping patterns (e.g., McFadden 1974; Blaylock 1989; Chiou 2008), these distances represent a sizable barrier to restaurant access.

Regression analyses confirm the statistical significance of the relationship between highway proximity and restaurant availability. Table 2 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 2 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 farther 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 seem 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 have much lower travel costs. (Section 5.3 presents analysis verifying the accuracy of our ZIP code-level distance measures.)

Our instrumental variables results focus on travel distance as the relevant measure of restaurant access because it has a direct economic interpretation. Nevertheless, other estimates, reported in

---

[19] In theory, travel times may be overstated if individuals work in restaurant-dense areas and eat out near work. Nevertheless, we demonstrate in Section 5.1 that highway proximity (the instrument) has a large, statistically significant effect on the frequency of fast-food consumption. Furthermore, results presented in Table 3 show that the effects of highway proximity on obesity are similar for individuals who are employed and those who are not employed.

[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 include state fixed effects.

Table 2, 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 for both full-service and limited-service (fast-food) 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 a greater density of restaurants, as measured by 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 2 into a price measure using conservative estimates of vehicle operating costs and travel time valuation. (See Appendix A2 for a description of the methodology.) We estimate total travel costs, including both vehicle operating costs and travel time, at 70.1 cents per mile. The last row of Table 2 indicates that the average cost differential in restaurant access for ZIP codes adjacent to Interstates versus ZIP codes farther from Interstates is $2.10.[22] 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 $10.80 for areas that would not have a restaurant if not for the highway. This figure corresponds to almost twice the average menu price of a fast-food meal and to 88 percent of the average menu price of a sit-down restaurant meal.

4.2. Reduced-Form Relation

Figure 3 presents the distribution of BMI for towns adjacent to an Interstate and towns farther 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 also the effect of restaurants at every quantile of the BMI distribution. This effect would likely be heterogeneous, so it would take a complex and

[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] We compute this number by doubling the coefficient in the first row of Table 2 (to reflect round-trip travel) and multiplying it by 70.1 cents per mile.

improbably distributed set of confounders to exactly offset it. Nevertheless, we analyze an observable set of BMI predictors in Section 5.2 and confirm that adjacent and nonadjacent towns are comparable across factors that predict BMI.

Table 3 reports regression coefficients measuring the reduced-form effect of Interstate proximity on body mass. The first row shows estimates of the impact of Interstate proximity on an obese indicator (BMI > 30), the second row shows estimates of the impact on an overweight indicator (BMI > 25), and the third row shows estimates of the impact on BMI.[23] Column (1) reports estimates from the full analytic sample and includes controls for state-by-year fixed effects but no other covariates.[24] The regressions are precisely estimated. The estimated coefficient from the obese regression indicates that proximity to a highway has no significant effect on the probability of being obese. In fact, the point estimate is negative (-0.1 percentage points). Estimates from the overweight and BMI regressions also show that proximity to highways does not affect body weight; the BMI coefficient is statistically insignificant and implies that Interstate proximity increases BMI by only 0.002 points.[25] Interstate proximity also has no economically or statistically significant effect on Type II obesity (BMI > 35) or severe obesity (BMI > 40); results available from authors upon request).

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

[23] The first measure is usually of most interest to public health practitioners because mortality risk does not increase substantially until BMI crosses the threshold of 30.
[24] Including county fixed effects is infeasible because some states only report the distance to the Interstate Highway – rather than the exact location of the telephone exchange – in an effort to preserve the anonymity of survey respondents.
[25] Specifying BMI in logs, rather than levels, also generates a regression coefficient that is close to zero (0.0001) and statistically insignificant.
[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.

slightly (less than 0.01 standard deviations in all cases) from the estimates reported in Column (3), but they remain close to zero and statistically insignificant. 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).[27] The results again indicate that proximity to highways has no effect on obesity. There is also no evidence of effects for any demographic subgroup. Estimating models separately by gender, age, education, or income yields economically and statistically insignificant results in all cases (see Appendix Table A1 for detailed results).

4.3. Instrumental Variables Results

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:

$$d_{jst} = \pi_0 + \pi_1 z_{jst} + \chi_{st} + u_{jst} \tag{4}$$

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 reported in Table 2, Column (1).

We implement the TS2SLS estimator by applying the coefficient estimates from equation (4) – estimated using data from ZIP Code Business Patterns – to predict the value of d_{jst} for observations in the BRFSS data:

$$\hat{d}_{jst} = \hat{\pi}_0 + \hat{\pi}_1 z_{jst} + \hat{\chi}_{st}. \tag{5}$$

We then run the second-stage regression

$$b_{ijst} = \beta_0 + \beta_1 \hat{d}_{jst} + \lambda_{st} + \varepsilon_{ijst} \tag{6}$$

to estimate the effect of distance to the nearest restaurant on BMI.[28] 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).[29]

---

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

[28] Note that we do not include covariates in either regression (other than state-by-year effects) because the same set of covariates is not available in both samples. If we were to include covariates in the second stage

We make several conservative assumptions that lead $\hat{\beta}_1$ to overstate the effect of restaurant proximity on BMI. First, we assume that the entire differential in restaurant access operates through distance to the nearest restaurant. However, the results in Table 2 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 overstate the true effects of restaurant proximity on BMI. Second, we use relatively low estimates of vehicle operating costs when translating distance measures into travel cost measures – this is equivalent to underestimating the first-stage coefficient, π_1, when we measure restaurant access by travel cost instead of by 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:

---

but not the first stage, the second-stage coefficient estimates would be inconsistent. The reduced-form results in Table 3, however, indicate that the addition of covariates has no significant effect on the relationship between highway proximity and obesity. Furthermore, in Appendix Table A2 we limit our sample to individuals for whom we know the exact ZIP code of residence. This restriction allows us to estimate conventional 2SLS models. Our results remain unchanged (precisely estimated null effects for all obesity outcomes), and including covariates in these models has no meaningful impact on the estimated coefficients.

[29] An additional complication arises due to the two-sample nature of the TS2SLS estimator. Since TS2SLS uses two samples, the sample upon which the first stage is estimated will not exactly match the sample upon which the reduced form is estimated. Because BRFSS respondents are not necessarily evenly distributed across ZIP codes, a given ZIP code can appear at different frequencies in the two samples. To check the sensitivity of our results to this issue, we weight each ZIP code in the first-stage sample by the expected frequency at which it occurs in the reduced-form sample. We must weight by the expected frequency rather than the exact frequency because some telephone exchanges (the geographic identifier for many observations in the reduced-form sample) map into multiple ZIP codes (the geographic identifier in the first-stage sample). Nevertheless, the expected frequency is measured with a high degree of accuracy in most cases. For states in which we have telephone exchange or ZIP code information (85 percent of cases), we can identify the ZIP code of residence with 100 percent confidence in 67 percent of cases, 90 percent or better confidence in 84 percent of cases, and 80 percent or better confidence in 93 percent of cases. Weighting each ZIP code in the first-stage sample by the expected frequency at which it occurs in the reduced-form sample has a minimal effect on the first-stage coefficient – the effect of highway proximity on distance to the nearest restaurant changes from 1.50 miles to 1.37 miles. For transparency, we report the unweighted TS2SLS models, but incorporating the weighted first stage does not affect the reported TS2SLS coefficients (the induced change is less than rounding error) and has a minimal affect on the standard errors. We further explore this issue in Appendix Table A2 by limiting our sample to individuals for whom we know the exact ZIP code of residence. This restriction allows us to estimate conventional 2SLS models, and our conclusions remain unchanged.

$$b_{ikst} = \alpha_0 + \alpha_1 z_{kst} + \phi_{st} + v_{ikst} \tag{7}$$

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 reported in Table 3, Column (1). Because the model is exactly identified, the TS2SLS estimate is:

$$\hat{\beta}_1 = \frac{\hat{\alpha}_1}{\hat{\pi}_1}. \tag{8}$$

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

Table 4 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 0.1 percentage point reduction in the probability of being obese, a 0.5 percentage point reduction in the probability of being overweight, and a 0.001 point increase in BMI. All 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 described above (and detailed in Appendix A2). For example, the effect on BMI of decreasing restaurant prices by one dollar is

(0.0014 BMI per mile) / (2 * 0.701 dollars per mile) = 0.001 BMI per dollar.[30]

Lowering restaurant access costs by one dollar is associated with no increase in the probability of being obese and a 0.001 point increase in BMI. All effects are statistically insignificant and correspond to changes of less than 0.01 standard deviations in the respective outcomes.

The remaining columns in Table 4 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 eating out. To address this possibility, we re-estimate the TS2SLS coefficients for the sub-sample 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

[30] The scaling factor of 2 in the denominator is present to account for the roundtrip nature of the trips.

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.

5. Analysis of Alternative Interpretations

The clear null relationship between Interstate proximity and body mass suggests that the availability of restaurants does not affect obesity. However, there are three alternative explanations for the null relationship that merit consideration. First, although Interstate proximity correlates with restaurant availability, it is possible that the instrument has no effect on the frequency of restaurant consumption (formally, this is equivalent to an absence of the necessary first-stage relationship). Second, residents of towns adjacent to the highway may differ from residents in nonadjacent towns along dimensions that affect body mass. In that case, it is possible that a positive effect of restaurants on body mass is masked by negative effects of other factors on body mass (formally, this is equivalent to a failure of the IV exclusion restriction). Finally, subtle forms of measurement error may attenuate the reduced form relationship between body mass and Interstate proximity in spite of a significant relationship between restaurant access and Interstate proximity. In this section, we analyze all three possibilities in turn.

5.1. Does Highway Proximity Increase Restaurant Consumption?

The first-stage relation estimated in Section 4.1 demonstrates that residents of nonadjacent towns live significantly farther from their nearest restaurant than residents of adjacent towns. Does this difference actually affect restaurant consumption? Restaurant demand, for example, might be highly inelastic with respect to travel distance, or optimizing consumers might choose to eat in a restaurant on days when they already travel to restaurant towns for other reasons. To validate the first-stage relationship between highway proximity and fast-food consumption, we conducted an original survey in a rural area that is representative of our study population. We surveyed customers at every fast-food restaurant lying within a 3,000 square-mile corridor of Interstate 5 in northern California. These data reveal that restaurant proximity indeed has a strong effect on frequency of consumption.

The area of northern California that we analyze is approximately two-thirds the size of Connecticut. Centered on Interstate 5 (I-5) between Dunnigan and Corning, CA, the study area is approximately 80 miles long and 40 miles wide, and contains 23 fast-food restaurants, including McDonald's, Burger King, Carl's Jr., Jack in the Box, Taco Bell, Kentucky Fried Chicken, Quiznos, and Subway. We chose this area because it was the only continuous Interstate corridor with comparable population density to our main analytic sample located within a 200-mile radius of either Berkeley, CA, or Evanston, IL.[31] Over 11 nonconsecutive days in June and July 2008, we approached 2,040 customers at all of these 23 restaurants and asked for their town and ZIP code of residence.[32] Ninety-three percent of those approached responded to our short oral survey.

Using these data and ZIP code populations from the US Census, we estimate the relative frequency of fast-food consumption for each ZIP code in the study area. The sampling scheme for these data is different than for the Census or BRFSS data since we sample at the point of consumption (the restaurant) rather than at the point of residence (the ZIP code or telephone exchange area). Nevertheless, because we sample from the entire universe of restaurants in the study area, both schemes should produce similar estimates of per capita fast-food consumption (up to sampling error). As an example, suppose that we wish to measure the number of California residents and Nevada residents attending the 2009 Annual Meeting of the American Economic Association (AEA) in San Francisco. One alternative would be to telephone a random sample of California and Nevada residents and ask, "Did you register for and attend the 2009 AEA Annual Meeting?" The other alternative would be to stand at the AEA registration desk and ask each person who registers, "What state are you from?" Both alternatives are valid and would yield the same answer asymptotically. Logistically, however, in both the AEA scenario and our actual survey, it is far less expensive to gather an equivalent number of observations using a direct customer survey than a telephone survey. For this reason, we conduct a direct customer survey. The two methods will produce different results only if we fail to capture a representative sample of the universe of restaurants, either spatially or temporally. (In the AEA scenario, the direct survey might fail if it only covered one of multiple registration desks or if it only sampled conference attendees from 8 a.m. to 12 p.m.; these issues would result in bias if California and Nevada residents were more or less likely to use a particular registration desk or to arrive in the afternoon.) Robustness checks, described below, indicate that neither of these issues affects our results.

---

[31] The population density of the three counties containing the corridor is 19 persons per square mile. The population density of the areas containing our main analytic sample is 25 persons per square mile.

[32] When surveying a given restaurant, we covered all possible entrances, including drive-throughs, and approached every customer entering the restaurant. Employees were not surveyed.

Table 5 presents summary statistics for the survey area and for the main analytic sample used in Section 4. The survey area is more racially diverse, younger, less educated, and less wealthy than the average ZIP code in our BRFSS sample. Because the effect of highways on obesity does not vary by demographic group (see Appendix Table A1), we do not expect these differences to bias our conclusions. ZIP codes in the survey area are also 10 to 16 percentage points more likely to contain some type of restaurant than the average ZIP code in our main study.

The first-stage relationship between highway proximity and restaurant access is roughly similar in the survey area and in our main study. For example, highway proximity increases the likelihood of having a restaurant by 21 percentage points in the survey area and 17.5 percentage points in our main analytic sample. Highway proximity reduces the average distance to travel to the nearest restaurant by 2.05 miles in the survey area and 1.50 miles in our main analytic sample.

The survey data indicate that restaurants located in towns adjacent to the highway are much more likely to serve long-distance travelers than restaurants located in nonadjacent towns. Fifty percent of customers in adjacent restaurants live more than one hour from the restaurant (as measured by Google Maps), compared to only 17 percent of customers in nonadjacent restaurants. This differential is statistically significant (t = 6.8) and supports the idea that restaurant density is greater adjacent to highways because these areas have greater demand from highway travelers passing through.

The relationship between the frequency of fast-food consumption and highway proximity is economically and statistically significant. Residents of towns located 0 to 5 miles from I-5 visit restaurants at a rate of 128 daily visits per 1,000 residents.[33] Residents of towns located 5 to 10 miles from I-5 visit restaurants at a rate of 68 daily visits per 1,000 residents.[34] This 47 percent decrease in frequency of fast-food consumption is highly significant (t = 8.4) and robust to the

[33] To convert our survey results to daily visit rates, we multiply by the ratio of business hours divided by surveyed hours. Business hours are assumed to start at 7 a.m. and end at 10 p.m., while surveyed hours began at 11 a.m. and ended by 8 p.m. If the average store is busier during the afternoon and evening than during the morning and late at night, then daily rates may be overstated. However, because the same multiplication factor is applied to the results from all towns, the conclusions of this section (which focus on relative comparisons between towns) are unaffected by the choice of multiplier.

[34] In theory, households in off-highway ZIP codes could send a single household member to bring back food for multiple household members. In this case, the number of people visiting fast-food restaurants from these ZIP codes would appear to be low, but the number of parties (i.e., distinct groups of customers) visiting fast-food restaurants from these ZIP codes would not appear to be low. Our data cast doubt on this possibility: the relative differential between highway and non-highway ZIP codes is identical if we perform the analysis using parties per 1,000 residents instead of people per 1,000 residents.

choice of sampling weights.[35] The relationship between fast-food consumption and restaurant proximity is also strong and statistically significant. Residents of towns that contain a fast-food restaurant visit restaurants at a rate of 127 daily visits per 1,000 residents, while residents of towns that do not contain a fast-food restaurant visit restaurants at a rate of 39 daily visits per 1,000 residents (t = 14.1).

Panel A of Table 6 reports the fast-food visit rate per 1,000 residents disaggregated by highway proximity and restaurant proximity. All differences between any two quadrants in Panel A of Table 6 are statistically significant. The fast-food visit rate for residents of adjacent towns that contain fast-food restaurants is 137 daily visits per 1,000 residents, whereas the fast-food visit rate for residents of nonadjacent towns that contain fast-food restaurants is 95 daily visits per 1,000 residents. Restaurant proximity is associated with greater restaurant consumption: both figures are significantly higher than the fast-food visit rate for residents of adjacent towns that do not contain fast-food restaurants (71 daily visits per 1,000 residents). Nevertheless, even when focusing on towns with no restaurants, highway residents visit fast-food restaurants more frequently than off-highway residents, presumably because living on the highway provides quicker access to towns with fast-food restaurants. The fast-food visit rate for residents of nonadjacent towns that do not contain fast-food restaurants is just 18 daily visits per 1,000 residents. Overall, the figures in Table 6 demonstrate that both restaurant and highway proximity are highly correlated with frequency of restaurant consumption.

As discussed above, our sampling scheme may overstate the correlation between highway proximity and fast-food consumption if we fail to capture the universe of fast-food restaurants, either temporally or spatially. Temporally, we may fail to appropriately measure the universe of restaurant consumption if relative visit rates differ throughout the day, because we only sample from the beginning of lunchtime until the end of dinnertime (11 a.m. through 8 p.m.). If off-highway residents eat a disproportionate number of their fast-food meals during breakfast hours, then we may be overstating the relationship between highway proximity and fast-food consumption. Breakfast accounts for 11 percent of fast-food meals (U.S. Census Bureau 2005, p. 69). We assess the robustness of our results by assuming that off-highway residents consume

[35] Each fast-food restaurant is sampled on at least one weekday and one weekend day, but logistical constraints led some restaurants to be sampled for longer durations than others. The sampling weights account for these differences by reweighting the data in proportion to the inverse of the hours surveyed per restaurant. The results are not sensitive to the exact choice of weights. For example, even when making no adjustments at all for sampling duration, we find that fast-food consumption decreases by 41 percent in nonadjacent towns relative to adjacent towns.

fast-food breakfasts at double the implied rate from our survey. Under this assumption, the fast-food consumption rate of off-highway residents is still 41 percent lower than that of on-highway residents.[36]

In the spatial dimension, we may fail to capture the universe of fast-food restaurant consumption because we do not sample every fast-food restaurant in the state of California. Instead, the estimates reported above assume that consumers do not travel more than 20 minutes to obtain fast food (more precisely, there is no difference in consumption rates between on- and off-highway residents at that distance). For residents of the ZIP codes analyzed in Table 6, the minimum driving time to a non-surveyed fast-food restaurant ranges from 21 minutes to 48 minutes. We check the sensitivity of our results to the 20-minute cutoff by re-estimating them on samples in which we further restrict the minimum driving time to a non-surveyed fast-food restaurant. If patronage of non-surveyed restaurants is driving the on-highway/off-highway consumption differential, then we should expect the differential to narrow as we further restrict the minimum driving time. Instead, we observe the opposite pattern.

When restricting the sample to ZIP codes in which residents must drive at least 30 minutes to reach the nearest non-surveyed fast-food restaurant, the frequency of fast-food consumption of off-highway residents is 52 percent lower than that of on-highway residents (t = 5.4). The disaggregated results are presented in Panel B of Table 6. When restricting the sample to ZIP codes in which residents must drive at least 40 minutes to reach the nearest non-surveyed fast-food restaurant, the frequency of fast-food consumption of off-highway residents is 91 percent lower than that of on-highway residents. The disaggregated results, reported in Panel C of Table 6, reveal that the large differential between on-highway and off-highway fast-food consumption arises because the off-highway sample now has no ZIP codes that contain restaurants.[37] Restricting the sample to residents who live very far from the nearest non-surveyed restaurant

[36] Our survey implies that off-highway residents consume fast food at 53 percent of the rate of on-highway residents. Doubling this figure for breakfast implies that off-highway residents consume fast-food breakfasts at 2*0.53 = 106 percent of on-highway residents. Because breakfasts comprise 11 percent of fast-food meals, the relative fast-food consumption rate of off-highway versus on-highway residents is 0.53*(89 percent of meals) + 1.06*(11 percent of meals) = 0.59.

[37] To maintain a reasonable sample size, we expand the off-highway group in Panel C to contain all ZIP codes between 5 to 20 miles from I-5. Limiting the sample to ZIP codes between 5 to 10 miles generates qualitatively similar results.

indicates that a failure to capture the entire universe of restaurants is not leading us to overstate the correlation between highway proximity and fast-food consumption.[38]

Overall, the results from the fast-food consumption survey suggest that residents in ZIP codes located 5 to 10 miles from the highway may consume fast food at only half the rate of residents in ZIP codes located 0 to 5 miles from the highway. The implied demand response to a one dollar change in travel costs is similar to existing estimates of the demand response to a one dollar change in menu prices (Park et al. 1996; Piggott 2003).[39] Even if the exact magnitudes estimated from the survey data do not generalize to our main analytic sample, the strong economic and statistical significance of the survey results verify that highway proximity indeed induces meaningful changes in fast-food consumption.

5.2. Can Residential Sorting across Towns Explain the Results?

There is little theoretical reason to believe that proximity to Interstate Highways in the range we examine 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 2008). Nevertheless, in principle people can choose where to live: individuals with a preference for eating out might choose to live in towns adjacent to Interstates, and these individuals may have higher or lower unobserved determinants of BMI, ε_{ijt}.

---

[38] Another possibility is that consumers in towns without fast-food restaurants perfectly substitute consumption to local full-service restaurants. However, 89 percent of towns without fast-food restaurants in the restricted sample also lack full-service restaurants. Trimming the sample to exclude residents of towns that lack fast-food restaurants but contain full-service restaurants indicates that highway proximity reduces fast-food consumption by 60 percent (t = 7.1). In this sample, we can be confident that consumers in towns with no fast food are not substituting consumption to either non-surveyed fast-food restaurants or to local full-service restaurants. It thus appears likely that on-highway residents are consuming not only more fast food than off-highway residents, but more total restaurant food as well.

[39] Using our travel cost estimate of 70.1 cents per mile, the survey results suggest that a $2.87 change in travel costs reduces fast-food consumption by 47 percent (the $2.87 figure is computed as 70.1 cents per mile * 2.05 miles extra travel distance each direction * 2 directions = $2.87). This implies that a one dollar increase in travel costs reduces fast-food consumption by 16.4 percent. For comparison, estimates from the literature suggest that a one dollar increase in menu prices reduces fast-food consumption by approximately 18 percent. Park et al. (1996) estimate an own-price demand elasticity of −1 for restaurant food. The average fast-food meal menu price is $5.51 (U.S. Census Bureau 2005), so a one dollar increase in menu prices (18.1 percent) should reduce fast-food consumption by approximately 18 percent.)

The BMI distributions plotted in Figure 3 indicate that it is very unlikely that selective sorting of individuals into highway towns offsets a positive effect of restaurants on body mass. Since the BMI distributions of the adjacent towns and the nonadjacent towns match up exactly, any positive restaurant effect must be offset not just at the mean, but also at every quantile of the BMI distribution. The probability of this occurring is very low. To confirm that unobserved factors are not offsetting a positive effect from restaurants, however, we also analyze a wide range of covariates from disaggregated Census and BRFSS data. These analyses, reported in Figure 4 and Tables 7 and 8, show no evidence that people sort themselves according to proximity to an Interstate. Given that all observable characteristics are balanced, it is likely that unobservable characteristics are balanced as well.

Using BRFSS data, Figure 4 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.[40] The plot in Figure 4 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 year dummies – suggesting that our research design successfully approximates a randomized experiment.[41]

Table 7 presents regression coefficients quantifying 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

[40] Statistical tests of each covariate individually also find no significant differences between individuals residing in adjacent and nonadjacent towns.
[41] To demonstrate that the BMI risk index has predictive power, Appendix Figure A2 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.

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 7 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 important, 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.[42] 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.

Table 8 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 the context of Figure 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 "Obese Risk Index" and the "Overweight Risk Index" are constructed similarly to the BMI Risk Index, but the covariates are used to predict obese status or overweight status rather than BMI. There is no statistically or economically significant relationship between either 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, Obese, or Overweight Risk Indices because doing so would severely reduce our sample size. 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 8. There is no significant relationship between proximity to an Interstate and average income or smoking rates. More important, individuals living in towns adjacent to and nonadjacent to an Interstate have similar desired

[42] 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).

weights and exercise with similar frequency. Based on these analyses, we conclude that there is no evidence of selection across adjacent and nonadjacent towns.

5.3. Can Measurement Error Explain the Results?

Our instrument assigns location using the centroid of a restaurant's or individual's ZIP code. This coding implies that actual distance to the Interstate Highway or to the nearest restaurant is measured with error. This measurement error has different implications, however, for distance to the Interstate Highway than it does for distance to the nearest restaurant. In the case of distance to the Interstate Highway, the measurement error does not affect the interpretation of the TS2SLS estimates as long as the first stage induces a statistically and economically significant change in restaurant access.[43] The first-stage estimates and the fast-food survey results leave little doubt that the instrument as coded is correlated with differences in restaurant access.

In the case of distance to the nearest restaurant, two features of our data could cause us to mismeasure the relationship between restaurant access and the instrument as coded. First, we assign distance to the nearest restaurant based on the ZIP code where a resident lives rather than the actual distance from his or her house. This fact could cause us to overstate (or understate) the true average distance to the nearest restaurant for residents of any given ZIP code, exaggerating (or attenuating) our TS2SLS results. Second, some of the BRFSS data are identified by telephone exchange rather than ZIP code. In principle this should reduce measurement error in much of the reduced-form sample because telephone exchanges are typically assigned to smaller geographic areas than ZIP codes, but in practice a small number of telephone exchanges also map into multiple ZIP codes. We analyze both of these issues in this section.

To check the accuracy of our ZIP code-level distance measures, we conduct a detailed analysis of 32 randomly sampled ZIP codes. This 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. Census blocks are geographically precise – the 32 ZIP codes contain a total of

[43] Imagine that we can measure the endogenous regressor – distance for individual i to his or her nearest restaurant – with perfect accuracy. The fact that distance to the Interstate Highway might be measured with error is then irrelevant as long as the instrument as coded induces a significant change in distance to the nearest restaurant, as it does in our case (the two sufficient conditions for a valid instrument – that it be correlated with the endogenous regressor and uncorrelated with unobserved factors that affect BMI – are met). Measurement error in distance to the Interstate would only become relevant if it were so large as to eliminate any significant effect of the instrument on the endogenous regressor.

6,096 Census blocks. For each Census block, we compute the distance to the nearest restaurant along the United States road network.[44] We then calculate the average distance to the nearest restaurant for the residents of each ZIP code as a population-weighted average of the distances for each Census block within the ZIP code.

For the 11 ZIP codes without restaurants, the average road-network centroid-to-centroid 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 actual 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 size of this sample, the two estimates differ by less than 10 percent. We cannot reject equality of the two estimates, and even differences of 20 percent would be too small to affect our conclusion that restaurants have minimal impact on obesity.[45] We conclude that the ZIP code-level distance calculations are reasonably accurate.

The Census block analysis indicates that measurement error at the ZIP code level is not generating our null result. For some BRFSS observations, however, location is assigned using telephone exchange rather than ZIP code. To check whether this assignment procedure attenuates our reduced-form results, we estimate the reduced-form relationship between highway proximity and obesity using only individuals for whom we observe the correct ZIP code with complete accuracy. Approximately 60 percent of our BRFSS sample meets this condition. The results confirm that there is no relationship between Interstate proximity and obesity. For example, regressing an obese indicator on Interstate proximity (the instrument) generates a coefficient of 0.013 (standard error of 0.012); regressing an overweight indicator on Interstate proximity generates a coefficient of -0.006 (standard error of 0.014); and regressing BMI on Interstate proximity generates a coefficient of 0.11 (standard error of 0.17).[46] These coefficients are economically and statistically insignificant – in each case the coefficient's magnitude is less than 0.03 standard deviations of the dependent variable. 2SLS estimates from this sample also reveal

[44] We identify restaurant locations using the *Yahoo! Yellow Pages*. Distances along the United States road network are computed using ArcGIS.
[45] If our first-stage estimates were overstated by 20 percent, then the estimated TS2SLS coefficient would be 17 percent too low (see equation (8)). Increasing the TS2SLS coefficient by 17 percent would imply that decreasing restaurant prices by one dollar increases body mass by only 0.0012 BMI points, which is still less than 0.001 standard deviations.
[46] All regressions contain state-by-year fixed effects. The corresponding full sample estimates are in Column (1) of Table 3.

no economically or statistically significant effect of restaurant access on any measure of obesity (see Appendix Table A2).

In summation, no form of measurement error can explain our conclusion that restaurants have little or no effect on obesity.

6. Why don't restaurants affect obesity?

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. To reconcile these facts, this section presents analysis of the causal mechanisms behind the limited effect of restaurant consumption on obesity. As illustrated by the theoretical framework developed above, there are two possible reasons why varying the effective price of restaurants would not affect body weight. First, after accounting for selection based on caloric demand, individuals may not consume substantially more calories when they eat out than they do at home. Second, 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. To explore the empirical relevance of these potential mechanisms, 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 about all of the food items consumed by several thousand adults over two nonconsecutive days. We focus our analysis on obese and overweight individuals who live outside of metropolitan areas because they are more representative of the subjects in our natural experiment.[47] We also drop a small number of observations with obvious coding errors, leaving an analytic sample of 854 individuals. Some individuals in nutrition studies likely underreport the amount of food they eat (Bingham 1987; Mertz et al. 1991; Schoeller 1990). Because our analysis is based on relative comparisons of between-individual and within-individual estimators, the presence of some underreporting is unlikely to affect our conclusions.

[47] Expanding the sample to include all geographic areas or non-overweight individuals leads to similar conclusions (see note 54). Expanding the sample in both dimensions increases the sample size to 5,713 individuals.

Respondents reported eating at a restaurant about once every three days. On days when survey respondents reported eating "more than usual," they were asked to explain why. The most popular reasons for eating more than usual were a social occasion or special day (5.4 percent of days) and because they were hungrier (1.3 percent of days). On only five out of every 1,000 days did people report eating more because they had eaten at a restaurant. These responses suggest that eating out does not actually increase daily caloric intake. We examine the food intake micro data to explore whether selection and/or offsetting can explain why intake does not increase.

We conduct two types of analyses using the food intake micro data. First, we examine how caloric intake differs for meals eaten at restaurants and meals eaten at home. Then, we examine how caloric intake changes on days in which individuals eat at a restaurant rather than exclusively at home. As our theoretical model implies, these two quantities may not be equal if individuals can substitute calories inter-temporally 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 9 presents coefficient estimates from the regression

$$c_{it} = \tau_0 + \tau_1 r_{it} + X_{it}\theta + w_{jt}, \qquad (9)$$

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 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, one, or two meals at a restaurant.[48]

Panel A reports results from the meal-level analysis. The sample ate 16.3 percent of their meals at restaurants (Column 1). Column (2) presents results from a between-individual estimator, which uses between-individual variation in restaurant dining to estimate the effect of restaurants on caloric intake. On average, individuals who eat at restaurants consume 339 more calories per meal than individuals who do not. This estimate is statistically significant and sizeable: the

[48] 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, daily caloric intake rises, and substitution takes place between adjacent days rather than between meals during the same day. This effect is even stronger if an individual eats all meals (breakfast, lunch, and dinner) at a restaurant. Since we do not have food intake data for consecutive days, we limit the empirical analysis to days on which individuals eat out once, twice, or not at all – these days comprise 99.3 percent of the sample. Our ultimate conclusions about selection and compensatory behavior also hold in an analysis that includes all days (see note 54).

average restaurant meal contains 85 percent more calories than the average home-cooked meal. If assigned a causal interpretation, an increase of 339 calories per restaurant-meal would imply that the existence of restaurants increases BMI by approximately 1.7 points (Column 3).[49] Many of the findings in the public health literature linking restaurants and obesity rely on this sort of cross-sectional variation (e.g., Clemens et al. 1999; McCrory et al. 1999; Binkley et al. 2000; French et al. 2000; Kant and Graubard 2004).

But some of the observed relationship between restaurants and caloric intake across individuals 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 use within-individual variation in restaurant dining to estimate the effect of restaurants on caloric intake. On average, when a given individual eats out, he consumes 238 more calories per meal than when he eats at home.[50] If assigned a causal interpretation, an increase of 238 calories per restaurant meal would imply that the existence of restaurants increases BMI by approximately 1.2 points (Column 5).[51]

While the fixed-effects 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.[52] Both the between and fixed-effects estimates are therefore upwardly biased estimates of the effect of restaurant meals on total caloric intake – the between estimate because of selection and the fixed-

[49] A one calorie increase, repeated every day in perpetuity, raises steady-state weight by approximately 0.08 pounds (Cutler, Glaeser, and Shapiro 2003). This calculation is based on calibrations from the medical literature that account for calories burned through basal metabolism, physical activity, and the thermic effect (Schofield, Schofield, and James 1985; Whitney and Cataldo 1983). Banning restaurants entirely might therefore reduce average weight by 10.9 pounds (that is, 338.8 calories per meal * 16.3 percent of meals at restaurants * 2.48 meals per day * 0.08 pounds per steady-state calorie). For a person of average height (5 feet, 7 inches), 10.9 pounds corresponds to 1.7 BMI points.

[50] 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 a.m. are assigned to "breakfast," items consumed from 11 a.m. to 4 p.m. are assigned to "lunch," and items consumed from 4 p.m. to midnight are assigned to "dinner." Regressions using this meal assignment (not shown) produce results qualitatively similar to those in Panel A.

[51] Banning restaurants entirely would reduce average weight by 7.7 pounds (that is, 237.6 calories per meal * 16.3 percent of meals at restaurants * 2.48 meals per day * 0.08 pounds per steady-state calorie). For a person of average height (5 feet, 7 inches), 7.7 pounds corresponds to 1.2 BMI points.

[52] The between estimates, in contrast, do capture compensatory behavior at other meals (but not snacks) within the same day, because all meals are effectively averaged to the individual level before the cross-sectional comparisons are made.

effects 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 9 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 intake over the entire day by only 35 calories – compared to an average daily caloric intake of 2,062 calories. Column (5) translates the coefficient into the long-term effect on BMI – an increase of 35 calories per restaurant meal implies that the existence of restaurants increases BMI by less than 0.2 points.[53] This effect is statistically insignificant and represents a decline of almost 90 percent from the between-individual 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, presented in Column (2), is significantly larger than the fixed-effects coefficient (214 versus 35), 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 especially hungry. The 35-calorie-per-meal fixed-effects estimate therefore represents an upper bound and implies that restaurant meals do not have a substantive causal effect on total caloric intake.

The food intake results suggest that banning restaurants entirely would reduce BMI by less than 0.2 points. This effect is economically insignificant – it represents 0.03 standard deviations of BMI. Banning restaurants entirely, however, is an unrealistic policy. More practical public policies would have even smaller effects; for example, reducing restaurant consumption by 25

[53] Banning restaurants entirely would reduce average weight by 1.1 pounds (that is, 34.6 calories per meal * 0.408 restaurant meals per day * 0.08 pounds per daily steady-state calorie). For a person of average height (5 feet, 7 inches), 1.1 pounds corresponds to 0.2 BMI points.

percent would reduce BMI by less than 0.05 points. These estimates are close to zero, closely matching the BMI effects from the natural experiment presented above in Table 4.[54]

Consumer calorie offsetting has also been shown to be a robust feature of food intake in controlled laboratory and field experiments in which individuals are offered meals of varying caloric content. Those offered more caloric meals tend to compensate by eating less later in the day, while those offered less caloric meals compensate by eating more later in the day (Foltin et al. 1990; Foltin et al. 1992; Lawton et al. 1998). These behaviors may be rooted in physiological mechanisms identified by the medical literature on the epidemiology of obesity (see, for example, Cummings and Schwartz 2003). The human body is understood to have a physiological system that regulates body weight in a manner analogous to that by which a thermostat controls ambient temperature. Each individual's target body weight is determined by a combination of genetic and environmental factors, and the body's weight regulation system, known as energy homeostasis, triggers compensatory changes in appetite and energy expenditure that resist weight changes outside a narrow range around the target. In this model, people with higher weight targets may eat out more to satisfy their greater demand for calories, but the causal physiological response to eating a large meal at a restaurant – holding constant an individual's target weight – is to offset those additional calories in other ways.

7. Policy implications

The results presented above suggest that access to restaurants has no appreciable causal effect on BMI or the prevalence of obese 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, such policies have recently been put forward in many jurisdictions. Proposals include zoning regulations that limit the number of fast-food restaurants – enacted in Los Angeles and under consideration in New York City – and restaurant "fat taxes" – proposed by public health officials and city leaders in the

[54] 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 48). Replicating the analysis including days on which individuals eat out three times or more implies that a 25 percent decrease in restaurant consumption could reduce BMI by 0.06 points. This effect is almost identical to the effect reported above. Expanding the sample to include urban and suburban consumers also generates similar conclusions. Estimates from this sample imply that reducing restaurant consumption by 25 percent might reduce BMI by 0.10 points. This effect is larger than the effect reported above, but it remains trivial in magnitude and is statistically indistinguishable from the results of our natural experiment.

United States, New Zealand, and Great Britain (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).[55] We therefore consider a restaurant tax of 50 percent to be at the upper limit of a plausible "fat tax." Since the average restaurant meal costs \$7.94 (excluding tax and tip), a 50 percent tax would imply an increase of \$3.97 in the average meal price (U.S. Census Bureau 2005, p. 12).[56]

The point estimates from the first column of Table 4 imply that a 50 percent (\$3.97) increase in restaurant prices would have no effect on the probability of being overweight or obese and would reduce BMI by only 0.004 points on average. 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 or obese by only 0.8 percentage points and decrease average BMI by 0.24 points. These effects correspond to 0.02 standard deviations of the obese and overweight indicators and 0.05 standard deviations of BMI. Thus, even when combining the strongest feasible policy with coefficient values substantially larger than those present in our data, there is still only a small decrease in the prevalence of obesity.

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 10 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 (Park et al. 1996; Piggott 2003). Using a constant elasticity demand curve, the value of consumer welfare lost ranges from

[55] The cigarette tax includes both federal and state taxes. There is no federal tax on alcohol.
[56] 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.

$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.[57]

The point estimates from our natural experiment (presented in Section 4) imply that a restaurant tax would not reduce the prevalence of obese or overweight individuals, so the cost-benefit ratio of such a policy would be infinite. To confirm that the costs are substantially higher than the benefits under any reasonable scenario, we compute an optimistic estimate of the potential benefits of the restaurant tax to compare to the deadweight loss. Specifically, we assume that the effect of restaurant prices on body mass is one standard error greater than our point estimate in Table 4. In that scenario, a 50 percent tax would reduce the prevalence of overweight and obese individuals by 0.8 percentage points from the 66 percent of Americans who were obese or overweight in 2004. Using results from Finkelstein et al. (2003), a 0.8 percentage point decrease in the prevalence of obese and overweight individuals would reduce covered medical expenditures by $1.4 billion (reported in the second-to-last column of Table 10).[58] The last column of Table 10 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 8.8-to-1 to 23.6-to-1.

While the deadweight loss associated with a tax policy is substantial, the deadweight loss associated with a zoning policy against restaurants, such as those adopted in Los Angeles and proposed in New York City, 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

[57] 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 2 percent of national income, income effects are likely negligible, and the uncompensated demand curve provides a reasonable approximation of consumer welfare lost (Hines 1999).

[58] Finkelstein et al. (2003) estimate that private insurers, Medicare, and Medicaid spent $65.7 billion covering overweight- and obesity-related illnesses in 1998 ($97.2 billion in 2007 dollars, inflated using the CPI Medical Care Services index). The prevalence of obese and 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.

time and fuel expenditures by consumers who must travel farther to their nearest restaurant and wait in longer lines when they arrive.

8. Conclusion

Many policymakers and public health advocates design policies intended to reduce the impact of restaurants on obesity, even while they acknowledge that convincing evidence of such a link has proven to be elusive. For example, the Food and Drug Administration recently organized a forum in which participants proposed 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, p.6).

Our findings indicate that the causal link between the availability of restaurant foods and obesity is minimal at best. Exploiting variation in the distance to the nearest restaurant due to Interstate Highway proximity shows that restaurant access and restaurant consumption have no significant effects on BMI, obesity, 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 obese individuals. Similar conclusions hold with respect to BMI and the prevalence of overweight individuals.

Detailed analyses of 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 also eat more when they eat at home. Furthermore when eating at home, obese individuals consume almost 30 percent of their calories in the form of "junk food."[59] Because obese individuals consume so many calories from nutritionally deficient sources at home, it may not be surprising that replacing restaurant consumption with home consumption does not improve health (as measured by BMI). These facts indicate that previous research demonstrating positive

[59] In this calculation, "junk food" includes ice cream, processed cheese, bacon, baked sweets (such as muffins, cakes, cookies, and pastries), crackers, potato chips and fries, candies, soft drinks, and beer. The estimate is based on the 1994-1996 Continuing Survey of Food Intake by Individuals.

between-individual correlations between eating out and obesity or caloric intake may be confounded by a lack of exogenous variation in restaurant consumption.

Although the results from our natural experiment apply directly to rural consumers, there are indications that the central conclusions are likely to generalize to urban consumers as well. As illustrated in Table 1, our analytic sample is similar to the national sample in terms of BMI and obesity prevalence. And though residents of rural areas are slightly older and less educated than urban residents, we find that restaurants have no effect on obesity for any demographic subpopulation in our sample defined by gender, age, education, or income (see Appendix Table A1). The lack of an effect among any of these subgroups suggests that our estimates may generalize to out-of-sample populations with different demographic characteristics. Furthermore, the mechanisms of sample selection and calorie offsetting that we document in Section 6 to explain our findings hold empirically for urban consumers as well as rural consumers.

Our results are also consistent with recent findings that focus on specific subpopulations. Currie et al. (2008) examine the correlation between fast-food restaurant openings and body weight among 9th grade schoolchildren and pregnant women in four states. Among these subpopulations, they find a negligible relationship between fast-food restaurants and obesity.[60]

Our 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 in other ways. Although restaurants conveniently deliver calories at a low marginal cost, they are only one source among many.[61] While taxing restaurant meals might cause obese consumers to change where they eat, our results suggest that a tax would be unlikely to affect their underlying tendency to overeat. The same principle would apply to other targeted obesity interventions as well. For example, two recent large-scale, multi-state randomized trials of school-based

[60] Currie et al. have no data on fast-food consumption, making direct comparisons with our results difficult. Nevertheless, their preferred specifications imply that the complete elimination of fast-food restaurants from all school neighborhoods might change obesity rates among 9th graders by +0.6 percentage points (from a base rate of 32.9 percent; an increase of approximately 0.01 standard deviations), and the complete elimination of fast-food restaurants from the average pregnant woman's neighborhood might change her BMI by -0.007 points (approximately 0.001 standard deviations). Because fast-food restaurant openings may be correlated with increases in demand for unhealthy food, we interpret these small estimates as upper bounds on the causal relationship between fast-food and obesity for the studied subpopulations.

[61] 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.

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 noted, 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 they are designed to account for the optimizing behavior of the targeted subjects.

References

Abdollah, Tami. 2007. “Limits Proposed on Fast-Food Restaurants.” *Los Angeles Times*, September 10.

Adams, K. F., A. Schatzkin, T. B. Harris, V. Kipnis, T. Mouw, R. Ballard-Barbash, A. Hollenbeck, and M. F. Leitzmann. 2006. “Overweight, Obesity, and Mortality in a Large Prospective Cohort of Persons 50 to 71 Years Old.” *New England Journal of Medicine* 355, no. 8: 763-78.

Adda, Jerome, and Francesca Cornaglia. 2006. “Taxes, Cigarette Consumption, and Smoking Intensity.” *American Economic Review* 96, no. 4: 1013-28.

Angrist, Joshua D. 1990. “Lifetime Earnings and the Vietnam Era Draft Lottery: Evidence from Social Security Administrative Records.” *American Economic Review* 80, no. 3: 313-36.

Angrist, Joshua D., and Alan B. Krueger. 1992. “The Effect of Age at School Entry on Educational Attainment: An Application of Instrumental Variables with Moments from Two Samples.” *Journal of the American Statistical Association* 87, no. 418: 328-36.

Ashenfelter, Orley, and Michael Greenstone. 2004. “Using Mandated Speed Limits to Measure the Value of a Statistical Life.” *Journal of Political Economy* 112, no. 1: S226-67.

Babey, Susan H., Allison L. Diamant, Theresa A. Hastert, Stefan Harvey, Harold Goldstein, Rebecca Flournoy, Rajni Banthia, Victor Rubin, and Sarah Treuhaft. 2008. “Designed for Disease: The Link between Local Food Environments and Obesity and Diabetes.” California Center for Public Health Advocacy, PolicyLink, and the UCLA Center for Health Policy Research.

Baylis, Cathy, and Tirtha Dhar. 2007. “Effect of the Quebec Advertising Ban on Junk Food Expenditure.” University of British Columbia.

Becker, Gary. “Social Causes of the Obesity 'Epidemic'.” 2007. In *The Becker-Posner Blog*. http://www.becker-posner-blog.com/archives/2007/08/social_causes_o.html.

Bingham, S. A. 1987. “The Dietary Assessment of Individuals: Methods, Accuracy, New Techniques and Recommendations.” *Nutrition Abstracts and Reviews* 57, no. 10: 705-42.

Binkley, J. K., J. Eales, and M. Jekanowski. 2000. “The Relation between Dietary Change and Rising U.S. Obesity.” *International Journal of Obesity* 24, no. 8: 1032-39.

Blaylock, J. R. 1989. “An Economic Model of Grocery Shopping Frequency.” *Applied Economics* 21, no. 6: 843-52.

Bleich, Sara, David M. Cutler, Christopher J. Murray, and Alyce Adams. 2007. “Why Is the Developed World Obese?” NBER Working Paper, no. W12954.

Boon, Ann. 2007. “State Cigarette Prices, Taxes, and Costs per Pack.” Washington, DC.

Caballero, A. Enrique. 2003. “Endothelial Dysfunction in Obesity and Insulin Resistance: A Road to Diabetes and Heart Disease.” *Obesity Research* 11, no. 11: 1278-89.

Cawley, John H. 1999. “Rational Addiction, the Consumption of Calories, and Body Weight.” University of Chicago..

Centers for Disease Control and Prevention. 2007. “New CDC Study Finds No Increase in Obesity among Adults; but Levels Still High.” CDC National Center for Health Statistics.

Chandra, Amitabh, and Eric Thompson. 2000. “Does Public Infrastructure Affect Economic Activity? Evidence from the Rural Interstate Highway System.” *Regional Science and Urban Economics* 30, no. 4: 457-90.

Chiou, Lesley. Forthcoming. “Empirical Analysis of Competition between Wal-Mart and Other Retail Channels” *Journal of Economics and Management Strategy*.

Chou, Shin Yi, Michael Grossman, and Henry Saffer. 2004. “An Economic Analysis of Adult Obesity: Results from the Behavioral Risk Factor Surveillance System.” *Journal of Health Economics* 23, no. 3: 565-87.

Claridge, Anna. 2003. “Researcher Backs 'Fat Tax' Idea.” *The Press* (Christchurch, New Zealand), June 26, 2003.

Clemens, L. H., D. L. Slawson, and R. C. Klesges. 1999. “The Effect of Eating Out on Quality of Diet in Premenopausal Women.” *Journal of the American Dietetic Association* 99, no. 4: 442-44.

Crowley, Patrick. 2005. “Food Fight.” *Newsweek* Web Exclusive, May 18. http://www.newsweek.com/id/51712?tid=relatedcl.

Currie, Janet, Stefano DellaVigna, Enrico Moretti, and Vikram Pathania. 2008. “The Effect of Fast Food Restaurants on Obesity.” Columbia University and UC Berkeley.

Cutler, David M., Edward L. Glaeser, and Jesse M. Shapiro. 2003. “Why Have Americans Become More Obese?” *Journal of Economic Perspectives* 17, no. 3: 93-118.

Davies, Haley. 2007. “Sacramento Restaurants May Be Forced to Serve Up Nutritional Data.” *San Francisco Chronicle*, September 12.

Davis, Stacy C., and Susan W. Diegel. 2007. “Transportation Energy Data Book: Edition 26.” Oak Ridge, TN: Oak Ridge National Laboratory.

Dunn, Richard A. 2008. “Obesity and the Availability of Fast-Food: An Instrumental Variables Approach.” University of Wisconsin-Madison.

Ezzati, M., H. Martin, S. Skjold, S. Vander Hoorn, and C. J. Murray. 2006. “Trends in National and State-Level Obesity in the USA after Correction for Self-Report Bias: Analysis of Health Surveys.” *Journal of the Royal Society of Medicine* 99, no. 5: 250-57.

Fernandez, Manny. 2006. “Pros and Cons of a Zoning Diet: Fighting Obesity by Limiting Fast-Food Restaurants.” *New York Times*, September 24.

Finkelstein, E. A., I. C. Fiebelkorn, and G. Wang. 2003. “National Medical Spending Attributable to Overweight and Obesity: How Much, and Who's Paying?” *Health Affairs* Web Exclusive: W3-219-26. http://content.healthaffairs.org/cgi/content/short/hlthaff.w3.219v1

Flegal, Katherine M., Margaret D. Carroll, Cynthia L. Ogden, and Clifford L. Johnson. 2002. “Prevalence and Trends in Obesity Among US Adults, 1999-2000.” *Journal of the American Medical Association* 288: 1723-1727.

Foltin, R. W., M. W. Fischman, T. H. Moran, B. J. Rolls, and T. H. Kelly. 1990. “Caloric Compensation for Lunches Varying in Fat and Carbohydrate Content by Humans in a Residential Laboratory.” *American Journal of Clinical Nutrition* 52, no. 6: 969-80.

Foltin, R. W., B. J. Rolls, T. H. Moran, T. H. Kelly, A. L. McNelis, and M. W. Fischman. 1992. “Caloric, but Not Macronutrient, Compensation by Humans for Required-Eating Occasions with Meals and Snack Varying in Fat and Carbohydrate.” *American Journal of Clinical Nutrition* 55, no. 2: 331-42.

French, S. A., L. Harnack, and R. W. Jeffery. 2000. “Fast Food Restaurant Use among Women in the Pound of Prevention Study: Dietary, Behavioral and Demographic Correlates.” *International Journal of Obesity* 24, no. 10: 1353-59.

Gardiner, Jill. 2007. “Council May Well Step into Fight over Fast Food.” *New York Sun*, February 26.

Hennessy-Fiske, Molly, and David Zahniser. 2008. “Council Bans New Fast-Food Outlets in South L.A.” *Los Angeles Times*, July 30.

Inoue, Atsushi, and Gary Solon.. 2005. “Two Sample-Instrumental Variables Estimators “ NBER Technical Working Paper, no. 311.

Jeitschko, Thomas D., and Rowena A. Pecchenino. 2006. “Do You Want Fries with That? An Exploration of Serving Size, Social Welfare, and Our Waistlines.” *Economic Inquiry* 44, no. 3: 442-50.

Jeffery, R. W., J. Baxter, M. McGuire, and J. Linde. 2006. “Are Fast Food Restaurants an Environmental Risk Factor for Obesity?” *International Journal of Behavioral Nutrition and Physical Activity* 3: 2.

Kant, A. K., and B. I. Graubard. 2004. “Eating Out in America, 1987-2000: Trends and Nutritional Correlates.” *Preventive Medicine* 38, no. 2: 243-49.

Keystone Center. 2006. “The Keystone Forum on Away-from-Home Foods: Opportunities for Preventing Weight Gain and Obesity—Final Report.” Washington, DC.

Kolata, Gina. 2006. “Thinning the Milk Does Not Mean Thinning the Child.” *New York Times*, February 12.

Lakdawalla, Darius, and Tomas Philipson. 2002. “The Growth of Obesity and Technological Change: A Theoretical and Empirical Examination.” NBER Working Paper, no. W8946.

Lakdawalla, Darius, Tomas Philipson, and Jay Bhattacharya. 2005. “Welfare-Enhancing Technological Change and the Growth of Obesity.” *American Economic Review* 95, no. 2: 253-57.

Lam, Terence C., and Kenneth A. Small. 2001. “The Value of Time and Reliability: Measurement from a Value Pricing Experiment.” *Transportation Research Part E: Logistics and Transportation Review* 37, no. 2-3: 231-51.

Lawton, C. L., H. J. Delargy, F. C. Smith, V. Hamilton, and J. E. Blundell. 1998. “A Medium-Term Intervention Study on the Impact of High- and Low-Fat Snacks Varying in Sweetness and Fat Content: Large Shifts in Daily Fat Intake but Good Compensation for Daily Energy Intake.” *British Journal of Nutrition* 80, no. 2: 149-61.

Maddock, J. 2004. “The Relationship between Obesity and the Prevalence of Fast Food Restaurants: State-Level Analysis.” *American Journal of Health Promotion* 19, no. 2: 137-43.

McCrory, M. A., P. J. Fuss, N. P. Hays, A. G. Vinken, A. S. Greenberg, and S. B. Roberts. 1999. “Overeating in America: Association between Restaurant Food Consumption and Body Fatness in Healthy Adult Men and Women Ages 19 to 80.” *Obesity Research* 7, no. 6: 564-71.

McFadden, Daniel. 1974. “The Measurement of Urban Travel Demand.” *Journal of Public Economics* 3, no. 4: 303-28.

Mello, M. M., D. M. Studdert, and T. A. Brennan. 2006. “Obesity—the New Frontier of Public Health Law.” *New England Journal of Medicine* 354, no. 24: 2601-10.

Mertz, W., J. C. Tsui, J. T. Judd, S. Reiser, J. Hallfrisch, E. R. Morris, P. D. Steele, and E. Lashley. 1991. “What Are People Really Eating? The Relation between Energy Intake Derived from Estimated Diet Records and Intake Determined to Maintain Body Weight.” *American Journal of Clinical Nutrition* 54, no. 2: 291-95.

Michaels, Guy. 2008. “The Effect of Trade on the Demand for Skill – Evidence from the Interstate Highway System.” *Review of Economics and Statistics* 90, no. 4: 683-701.

Mokdad, Ali H., James S. Marks, Donna F. Stroup, and Julie L. Gerberding. 2004. “Actual Causes of Death in the United States, 2000.” *Journal of the American Medical Association* 291, no. 10: 1238-45.

Nader, P. R., E. J. Stone, L. A. Lytle, C. L. Perry, S. K. Osganian, S. Kelder, L. S. Webber, J. P. Elder, D. Montgomery, H. A. Feldman, M. Wu, C. Johnson, G. S. Parcel, and R. V. Luepker. 1999. “Three-Year Maintenance of Improved Diet and Physical Activity: The Catch Cohort. Child and Adolescent Trial for Cardiovascular Health.” *Archives of Pediatrics & Adolescent Medicine* 153, no. 7: 695-704.

Newby, P. K., D. Muller, J. Hallfrisch, N. Qiao, R. Andres, and K. L. Tucker. 2003. “Dietary Patterns and Changes in Body Mass Index and Waist Circumference in Adults.” *American Journal of Clinical Nutrition* 77, no. 6: 1417-25.

Ostrom, Carol M. 2007. “New Rules: Menus Must Say What's in Your Meal.” *Seattle Times*, July 25.

Paeratakul, S., D. P. Ferdinand, C. M. Champagne, D. H. Ryan, and G. A. Bray. 2003. “Fast-Food Consumption among US Adults and Children: Dietary and Nutrient Intake Profile.” *Journal of the American Dietetic Association* 103, no. 10: 1332-38.

Park, John L., Rodney B. Holcomb, Kellie Curry Raper, and Oral Capps, Jr. 1996.“A Demand Systems Analysis of Food Commodities by U.S. Households Segmented by Income.” *American Journal of Agricultural Economics* 78, no. 2: 290-300.

Peltzman, Sam. 1975. “The Effects of Automobile Safety Regulation.” *Journal of Political Economy* 83, no. 4: 677-726.

Piggott, Nicholas E. 2003. “The Nested Piglog Model: An Application to U.S. Food Demand.” *American Journal of Agricultural Economics* 85: 1-15.

Schoeller, D. A. 1990. “How Accurate Is Self-Reported Dietary Energy Intake?” *Nutrition Reviews* 48, no. 10: 373-79.

Schofield, W. N., C. Schofield, and W. P. T. James. 1985. "Basal Metabolic Rate—Review and Prediction Together with Annotated Bibliography of Source Material." *Human Nutrition: Clinical Nutrition* 39C, no. Supplement 1: 5-96.

Simmons, D., A. McKenzie, S. Eaton, N. Cox, M. A. Khan, J. Shaw, and P. Zimmet. 2005. "Choice and Availability of Takeaway and Restaurant Food Is Not Related to the Prevalence of Adult Obesity in Rural Communities in Australia." *International Journal of Obesity* 29, no. 6: 703-10.

Tax Foundation. 2007. "Tax Data: State Sales, Gasoline, Cigarette, and Alcohol Tax Rates by State, 2006." Washington, DC.

Technomic Information Services. 2008. *Technomic Top 500 Chain Restaurant Report*. Chicago.

U.S. Census Bureau. 2005. "Accommodation and Food Services Subject Series: Miscellaneous Subjects, 2002." 2002 Economic Census, 2005.

———. 2002a. "Government Organization." 2002 Census of Governments.

———. 2002b. "Summary File 1." Census 2000.

U.S. Department of Agriculture. 2004. "Measuring Rurality: Rural-Urban Continuum Codes." Economic Research Service.

U.S. Department of Health and Human Services. 2001. "The Surgeon General's Call to Action to Prevent and Decrease Overweight and Obesity." Rockville, MD: Public Health Service, Office of the Surgeon General.

U.S. Department of Labor. 2008. "Occupational Employment Statistics: Nonmetropolitan Area Cross-Industry Estimates." Bureau of Labor Statistics, 2008.

———. 2007. "Average Price Data." Bureau of Labor Statistics.

U.S. Department of Transportation. 2002. "The Dwight D. Eisenhower National System of Interstate and Defense Highways." Federal Highway Administration.

U.S. Environmental Protection Agency. 2007. "Light-Duty Automotive Technology and Fuel Economy Trends: 1975 through 2007." Office of Transportation and Air Quality.

Vyse, Leah. 2007. "U.K. Junk Food Advertising Ban Comes into Force." *Food Business Review Online*, April 2. http://www.food-business-review.com/article_news.asp?guid=671D964 F-534F-456F-8686- FB5C0C575153

Waldfogel, Joel. 2006. "Who Benefits Whom in the Neighborhood? Demographics and Retail Product Geography." University of Pennsylvania.

Whiteside, Philip. 2004. "Fast Food Tax to Cut the Calories; from Vat to Fat." *Daily Star* (UK), February 20.

Whitney, Eleanor Noss, and Corinne Balog Cataldo. 1983. *Understanding Normal and Clinical Nutrition*. St. Paul: West Publishing Company.

Young, L. R., and M. Nestle. 2002. "The Contribution of Expanding Portion Sizes to the Us Obesity Epidemic." *American Journal of Public Health* 92, no. 2: 246-49.

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 (see Section 5.3) 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 (in 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, inflating 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.

The sample of 21 ZIP codes with restaurants reports an average distance of 2.5 miles to the nearest restaurant. This estimate is precisely estimated (standard error of 0.3 miles). We use this figure and the underlying distribution to estimate the average distance to the nearest restaurant for residents of ZIP codes containing a restaurant and to construct Figure 2.

A2. Travel Cost Valuation

Consumer travel costs include both vehicle operating costs and the opportunity cost of time spent in transit. Our estimates of vehicle operating costs come from annual publications of *American Automobile Association: Your Driving Costs*. We include per mile gasoline, maintenance, and depreciation costs. We exclude tire wear and expected accident costs from our calculations. Under these assumptions, we compute vehicle operating costs at 31.7 cents per mile during our sample period.[62] This estimate is likely conservative – for example, the IRS Standard Mileage Rate during the same period was 28 percent higher (40.7 cents per mile).

[62] 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

To estimate time costs, we follow Ashenfelter and Greenstone (2004) and use average hourly wages to approximate the value of travel time. Based on estimates from the Occupational Employment Statistics survey (U.S. Department of Labor 2008), the average wage in rural areas of our sample states is $14.91 per hour, or 38.4 cents per mile.[63] We thus estimate that the average marginal cost of travel for rural consumers is 70.1 cents per mile – the sum of vehicle operating costs and time costs.[64]

To benchmark our estimate of travel time valuation using an independent source, we collected an original data set of automobile speeds on an unobstructed rural roadway.[65] Based on these speeds, we measure the degree to which rural drivers trade off travel time reductions for increased gasoline consumption, and find an estimate similar to that using wages. 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 used by drivers 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 +/- 0.1 mph).[66] We arranged for the survey vehicle to be invisible from the roadway, so that drivers would not modify their speed in response to seeing the survey vehicle.[67]

The median driver's speed on this rural roadway was 73 mph.[68] 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

---

sample. Including tire wear and expected accident costs would increase the estimated costs by approximately 40 percent.

[63] We convert the travel time valuation of $14.91 per hour to 38.4 cents per mile using an average speed of 38.9 miles per hour, which is 30 percent greater than the average posted speed limit in our detailed sample of ZIP codes described in Appendix A1.

[64] This estimate is consistent with estimates from Chiou (forthcoming) for how far a rural consumer is willing to travel to save one dollar on a DVD purchase.

[65] Lam and Small (2001) use the revealed preferences of southern California toll lane users to estimate an average travel time valuation of $29.28 per hour, but these results are estimated from urban commuters and may not generalize to rural, non-work trips.

[66] A cosine correction of 1.02 was applied to adjust for the exact angle at which the vehicles were surveyed.

[67] To the extent that any drivers with radar detectors slowed down from their preferred speed, our estimates understate the true valuation of travel time.

[68] The maximum speed was 94 mph.

2007).[69] The average driver therefore trades off 4.01 gallons of gasoline for each hour of time savings when traveling at 73 mph.[70] 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 4.01 gallons/hour * $3.30/gallon = $13.23 per hour. Other speed quantiles also demonstrated a high valuation of time. The 90th and 75th speed percentiles demonstrated implied time valuations of $15.90 and $14.62 per hour respectively, while the 25th and 10th speed percentiles demonstrated implied time valuations of $11.93 and $11.20 per hour respectively.

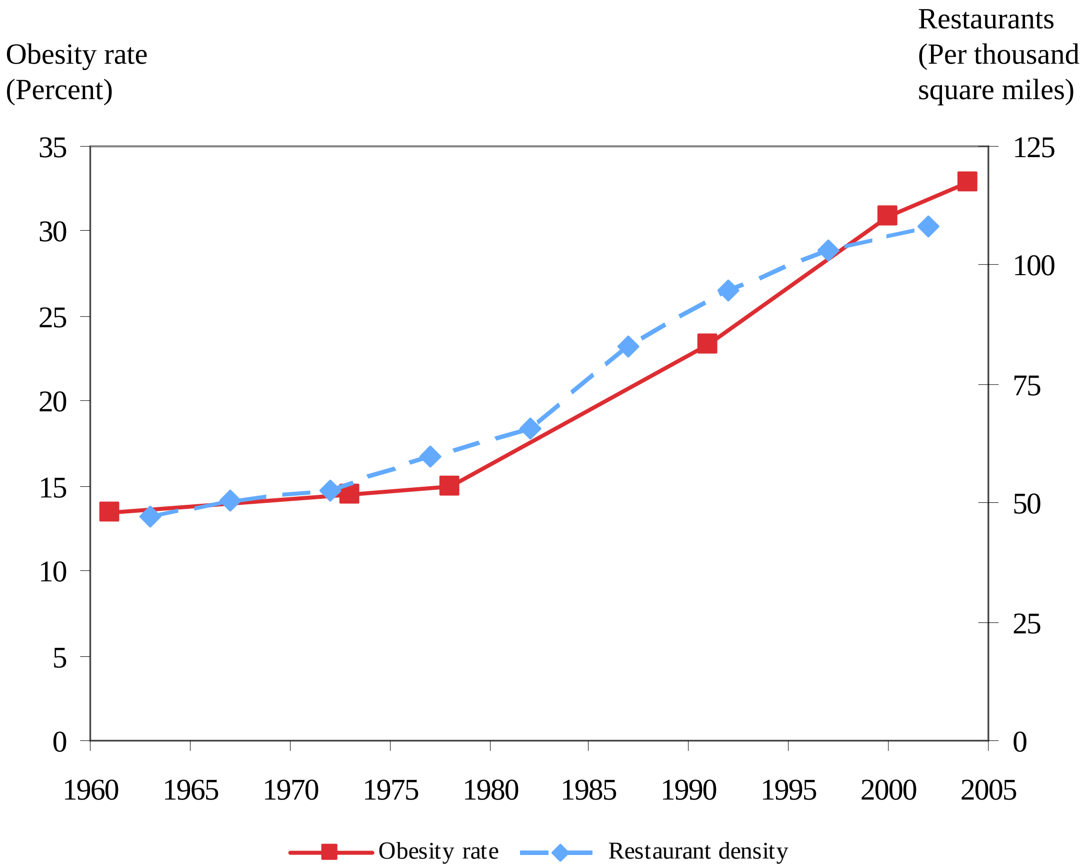
While the speed data provide a useful robustness check, these estimates rely on a number of assumptions. First, the drivers must understand the trade-off between travel time reductions and higher gasoline consumption. Second, the estimate does not account for the increased probability of receiving a speeding citation. Third, it does not account for the greater risk of injury or death that drivers face when traveling at higher speeds. Nevertheless, the estimates suggest that the average rural wage provides a plausible estimate of rural motorists' value of travel time.

---

[69] 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.

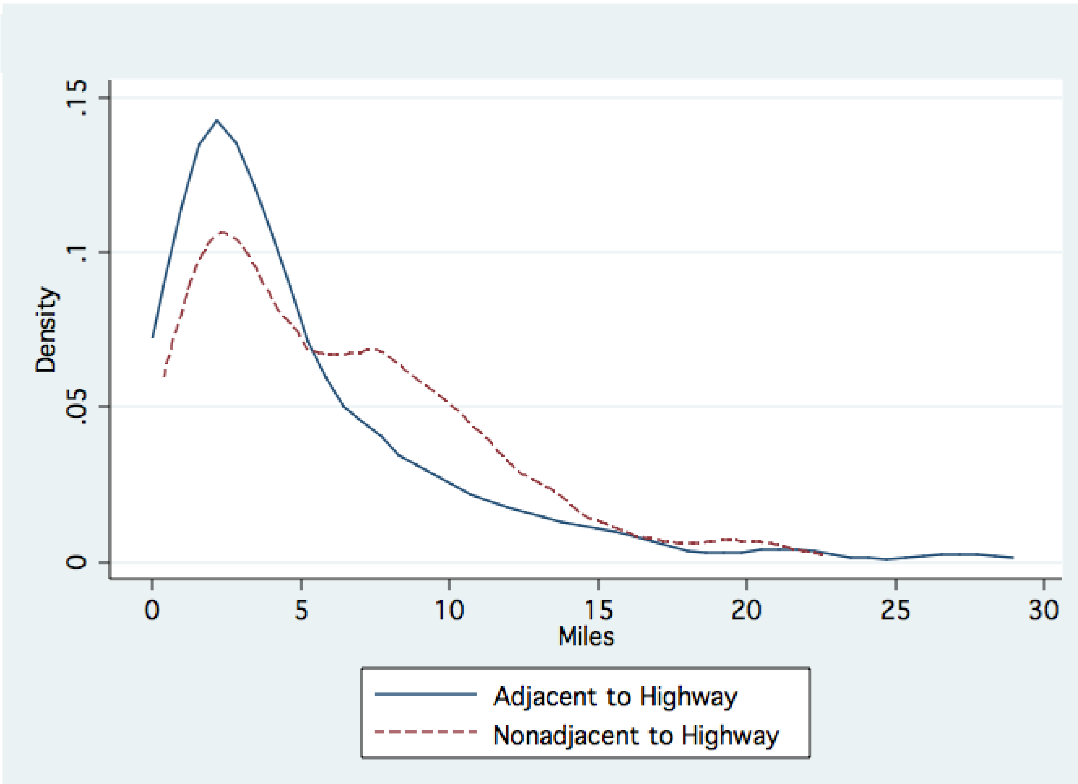
[70] At 73 mph, increasing speed by 1 mph to 74 mph reduces the time needed to travel 73 miles by 0.0135 hours (49 seconds), but significantly increases fuel consumption. The fleet average EPA highway mileage over the last decade is 23.0 mpg (U.S. EPA 2007). Assuming this is a reasonable estimate for 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 0.0542 gallons. On the margin, therefore, gas is exchanged for travel time at 0.0542 gallons/0.0135 hours = 4.01 gallons/hour when traveling at 73 mph.

Figure 1. Obesity Rates and Restaurant Density, 1960-2004



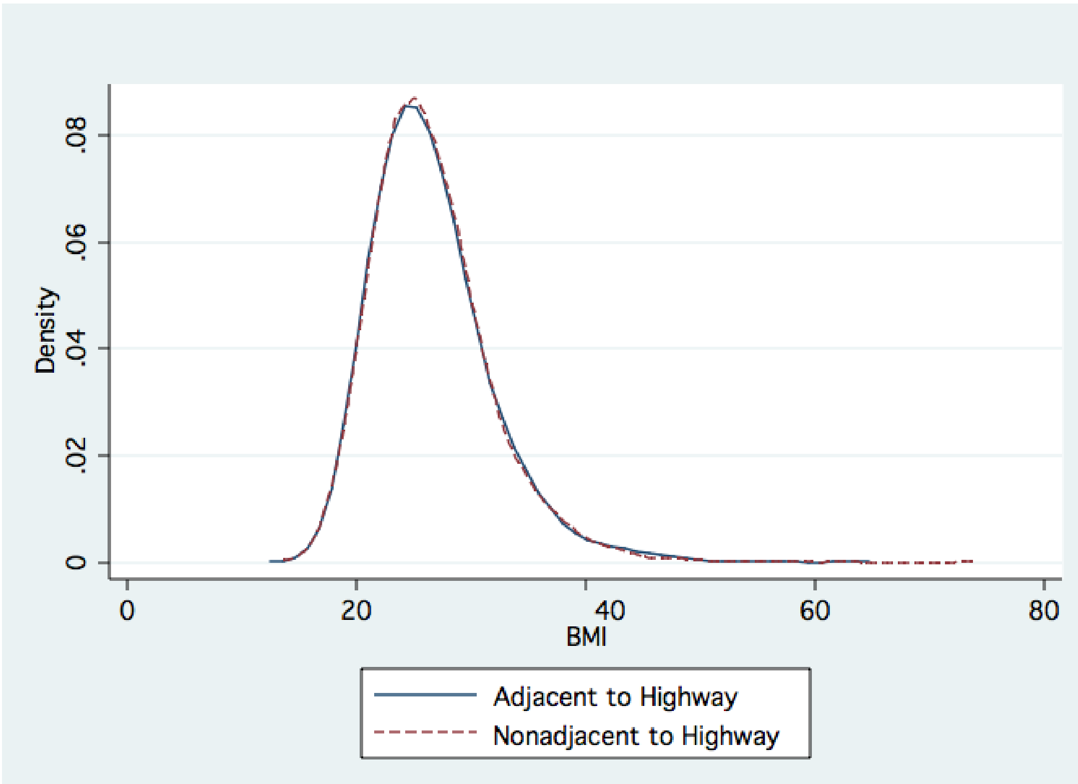
This figure plots the obesity rate and restaurant density in the United States from 1960 through 2004. The obesity rates are age-adjusted estimates for the percent of adults aged 20 to 74 with body mass index greater than or equal to 30, based on the National Health and Nutrition Examination Survey (Flegal et al. 2002; CDC 2007). The restaurant density rates are the number of full-service and limited-service restaurant establishments per thousand square miles, reported by the Economic Census.

Figure 2. Distribution of Distance to Nearest Restaurant in Towns Adjacent and Nonadjacent to Interstate Highways



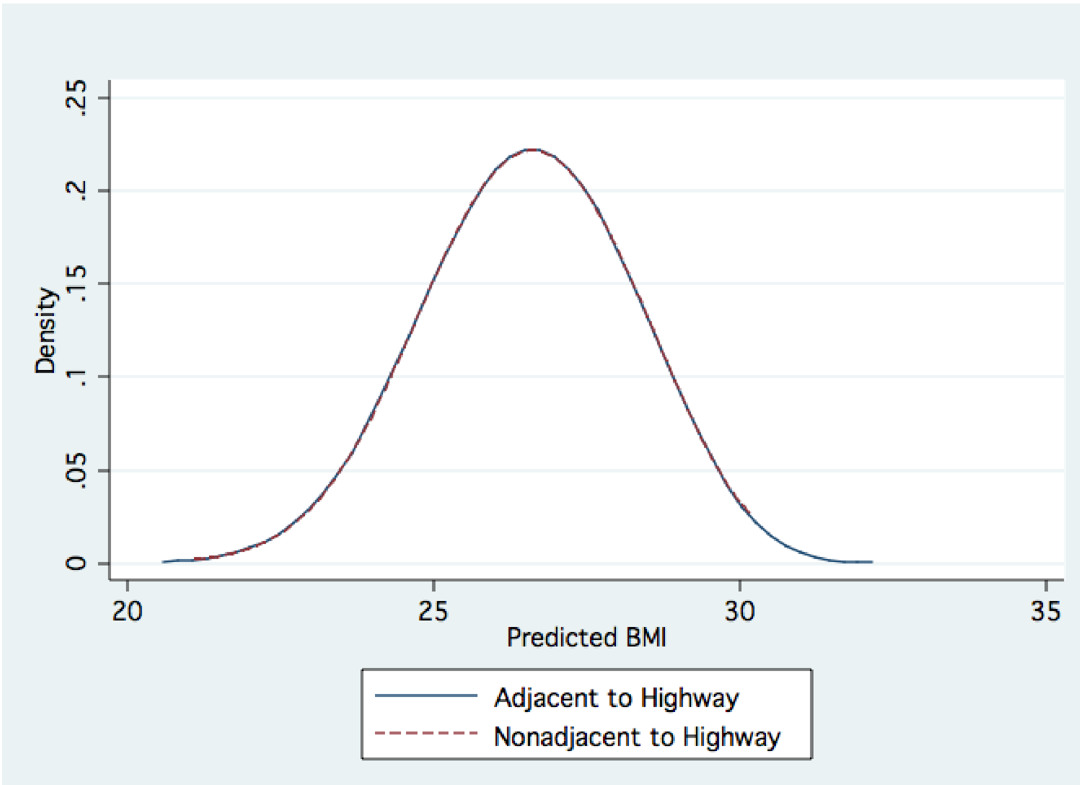
This figure plots the distribution of distance to the nearest restaurant for towns that are adjacent and nonadjacent to Interstate Highways. For ZIP codes without restaurants, we use distance to the nearest ZIP code with a restaurant. For ZIP codes with restaurants, we sample (with replacement) from the empirical distribution of distance to the nearest restaurant for each Census block in a stratified random sample of 21 ZIP codes (see Appendix A1 for details). Residents of adjacent towns are likely to live 0 to 5 miles from the nearest restaurant, whereas residents of nonadjacent towns are more likely to live 5 to 15 miles away. These distances correspond to additional roundtrip travel times of 10 to 40 minutes.

Figure 3. Distribution of Body Mass Index in Towns Adjacent and Nonadjacent to Interstate Highways



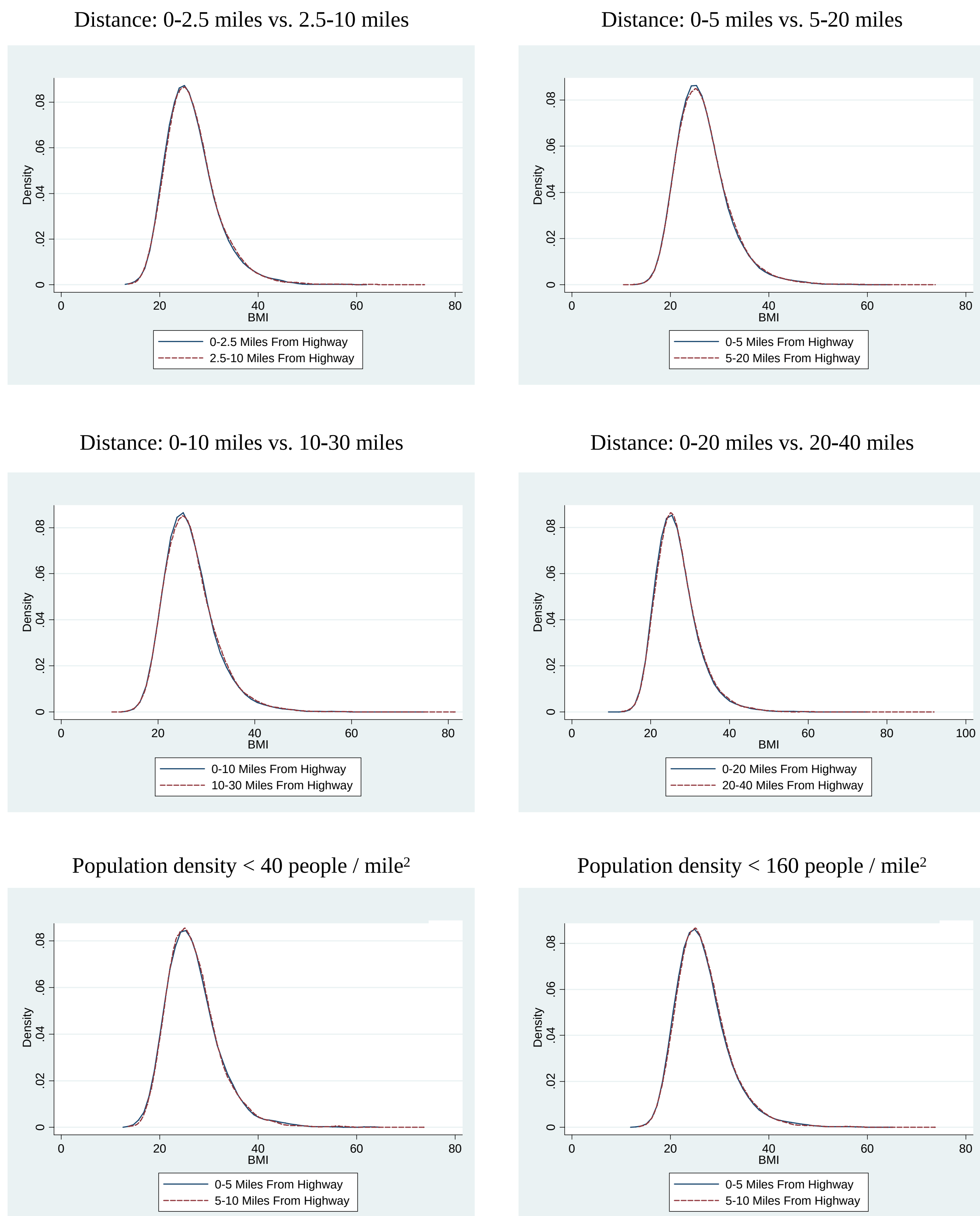
This figure plots the distribution of body mass index (BMI) for residents of towns that are adjacent and nonadjacent to Interstate Highways. While residents of adjacent towns have greater access to restaurants (see Figure 1; Table 4) and eat more fast-food (see Table 10), they have similar body mass to residents of nonadjacent towns. The two distributions match up almost exactly, implying that restaurants have no discernable effect on obesity at any quantile of the BMI distribution.

Figure 4. Distribution of BMI Risk in Towns
Adjacent and Nonadjacent to Interstate Highways



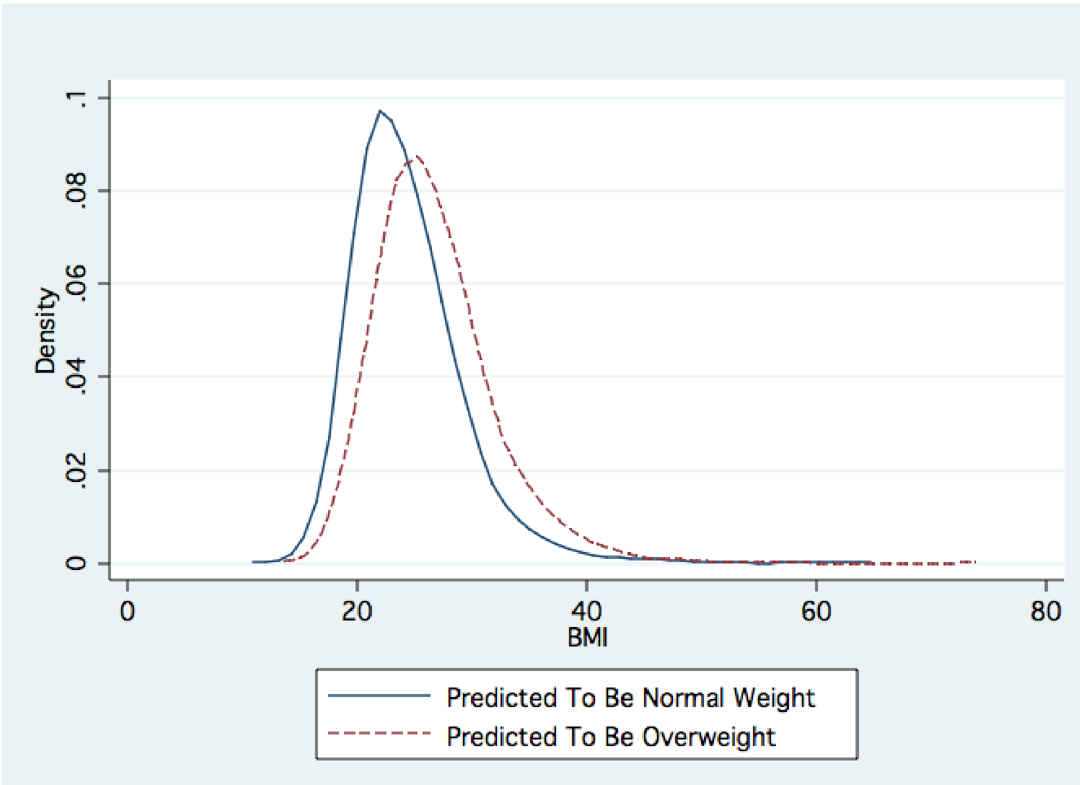
This figure plots the distribution of an index of predicted BMI for residents of towns that are adjacent and nonadjacent to Interstate Highways. The index consists of the fitted values from a regression of BMI on a set of observed characteristics: gender, a quadratic in age, indicators for educational attainment, employment, unemployment, and marital status, and a full set of state-by-year fixed effects. The two distributions match up almost exactly, implying that the risk factors for BMI are balanced across adjacent and distant towns. This balance occurs without controlling for any covariates – not even state or year dummies – suggesting that our research design successfully approximates a randomized experiment.

Appendix Figure A1. Robustness of Highway Analysis to Alternative Specifications:
Distribution of Body Mass Index in Towns Adjacent and Nonadjacent to Interstate Highways



This figure examines the robustness of Figure 2 to alternative specifications for highway proximity and population density. The results suggest that there is no relationship between highway (and restaurant) proximity and body mass; in every specification, the distributions of body mass among residents of adjacent and nonadjacent towns match up almost exactly.

Appendix Figure A2. Distribution of Body Mass Index for Individuals Predicted to Be Normal Weight versus Overweight by the BMI Risk Index



This figure plots the 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. Individuals predicted to be overweight are substantially heavier than individuals predicted to be normal weight, implying that the BMI risk index has predictive power.

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
Obese (BMI ≥ 30)	0.21	0.41	13,470	0.19	0.40	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: This table reports unweighted summary statistics for the analytic sample and the full national sample. 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 a population density of 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: 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	-$2.10	-$1.94
	(0.55)	(0.64)
Sample:		
Population Density Cutoff (People per Sq Mile)	80	40
Number of ZIP Codes	551	460

Note: In this table, 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. The results indicate that residents of ZIP codes adjacent to Interstates have greater access to restaurants -- in any number of dimensions -- than do residents of nonadjacent ZIP codes.

Table 3: Reduced Form Effect of Interstate Proximity on Obesity

	(1)	(2)	(3)	(4)	(5)
Dependent Variable					
i) Obese	-0.001	0.000	0.000	0.002	0.000
(BMI ≥ 30)	(0.009)	(0.010)	(0.009)	(0.009)	(0.011)
ii) Overweight	-0.007	-0.004	-0.007	-0.003	0.001
(BMI ≥ 25)	(0.011)	(0.012)	(0.011)	(0.011)	(0.012)
iii) 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:					
Population Density Cutoff (People per Sq Mile)	80	80	80	80	40
Observations	13,470	13,470	12,175	12,175	7,956

Note: In this table, 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. The results, which are precisely estimated, indicate that proximity to highways does not affect average body weight.

Table 4: Effect of Restaurant Access on Obesity (TS2SLS Models)

	All Individuals		Not Employed	
	(1)	(2)	(3)	(4)
Panel A: *Effect of Being One Mile Closer to a Restaurant on:*				
Dependent Variable				
i) Obese	-0.001	-0.004	-0.008	-0.015
(BMI ≥ 30)	(0.006)	(0.008)	(0.009)	(0.013)
ii) Overweight	-0.005	-0.004	-0.008	-0.005
(BMI ≥ 25)	(0.007)	(0.009)	(0.011)	(0.014)
iii) 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) Obese	0.000	-0.003	-0.005	-0.010
(BMI ≥ 30)	(0.004)	(0.006)	(0.007)	(0.009)
ii) Overweight	-0.003	-0.003	-0.006	-0.003
(BMI ≥ 25)	(0.005)	(0.006)	(0.008)	(0.010)
iii) BMI	0.001	-0.053	-0.005	-0.039
	(0.060)	(0.073)	(0.090)	(0.116)
Sample:				
Population Density Cutoff (People per Sq Mile)	80	40	80	40
Observations	13,470	8,575	5,208	3,368

Note: This tables reports estimates from two-sample two-stage least squares regressions. Each estimate represents a different combination of dependent variable, sample, and econometric specification 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. The results, which are precisely estimated, indicate that reducing the cost of visiting restaurants does not affect average body weight.

Table 5: Summary Statistics

	Northern California Study Area			Main Analytic Sample		
	Mean	Std Deviation	Sample Size	Mean	Std Deviation	Sample Size
White	0.69	0.19	26	0.93	0.10	551
Under 21	0.35	0.06	26	0.31	0.05	551
Over 65	0.12	0.04	26	0.16	0.05	551
College	0.36	0.12	25	0.43	0.13	551
Median HH Income	$31,653	$9,243	26	$34,689	$7,728	551
Any Restaurant	0.77	0.43	26	0.61	0.49	551
Any Full-Service Restaurant	0.65	0.49	26	0.55	0.50	551
Any Limited-Service Restaurant	0.46	0.51	26	0.36	0.48	551
Number of Full-Service Restaurants	2.85	3.90	26	2.38	4.83	551
Number of Limited-Service Restaura	2.04	3.01	26	1.64	3.51	551

Note: This table reports unweighted summary statistics for the rural northern California survey area (used in Section 5.1) and the main analytic sample (used in Section 4). The rural northern California survey area consists of the 26 ZIP codes lying within 20 miles of the I-5 corridor from Dunnigan to Corning, CA. The analytic sample consists of all 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 a population density of less than 80 persons per square mile. All data are from the Census, and standard deviations are calculated at the ZIP code level.

Table 6: Average Number of Daily Visits to Fast-Food Restaurants per 1,000 Residents

	Restaurant in ZIP Code	
	Restaurant	No Restaurant
Panel A: *At Least 20 Mins to Nearest Non-Surveyed Restaurant*		
Distance from highway		
0 - 5 miles	137.3	71.0
	(5.5)	(9.4)
5 - 10 miles	95.2	17.9
	(7.8)	(4.7)
Panel B: *At Least 30 Mins to Nearest Non-Surveyed Restaurant*		
Distance from highway		
0 - 5 miles	157.6	138.1
	(11.7)	(28.1)
5 - 10 miles	89.7	5.5
	(12.5)	(6.2)
Panel C: *At Least 40 Mins to Nearest Non-Surveyed Restaurant*		
Distance from highway		
0 - 5 miles	157.6	138.1
	(11.7)	(28.1)
5 - 20 miles	N/A	13.3
		(7.0)

Note: In this table, each number represents the average number of daily visits to fast-food restaurants per 1,000 residents. Numbers in the "Restaurant" column correspond to residents living in ZIP codes that contain a fast-food restaurant. Numbers in the "No Restaurant" column correspond to residents living in ZIP codes without a fast-food restaurant. Numbers in the rows correspond to residents living in ZIP codes that lie within the indicated number of miles from the highway. Standard errors are reported in parentheses. The results indicate that both restaurant and highway proximity are highly correlated with the frequency of restaurant consumption.

Table 7: 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: In this table, each coefficient represents a separate regression; rows corresponds to different dependent variables, and columns correspond to different samples of ZIP codes and regression specifications. All regressions contain state-by-year fixed effects, and robust standard errors are reported in parentheses. 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. The results indicate that the characteristics of ZIP codes with restaurants are different than the characteristics of restaurants without restaurants, suggesting 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. 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). In contrast to the first column, no regression returns a statistically significant coefficient; the results suggest that demographic characteristics seem to be well balanced across areas that are adjacent and nonadjacent to Interstate Highways.

Table 8: Individual-Level Covariate Balance

	Coefficient	Sample Size
Dependent Variable		
i) BMI Risk Index	-0.038	12,797
	(0.029)	
ii) Obese (BMI ≥ 30) Risk Index	-0.0019	12,797
	(0.0016)	
iii) Overweight (BMI ≥ 25) Risk Index	-0.0037	12,797
	(0.0031)	
iv) Average Income ($)	-482	10,560
	(936)	
v) Ever Smoked	-0.019	9,180
	(0.012)	
vi) Desired Weight (in kg)	-0.280	4,154
	(0.425)	
vii) Exercised in Last Month	0.012	6,731
	(0.016)	
State-by-year fixed effects	Yes	

Note: In this table, 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, underweight) risk index consists of the fitted values from a regression of BMI (overweight, underweight) 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. There is no statistically or economically significant relationship between any of these variables and proximity to an Interstate Highway.

Table 9: Relationship between Restaurant and Caloric Intake for Obese & Overweight Individuals

	Frequency of Restaurant Meals	Between Estimator		Fixed Effects		Sample Size
		Calories	Long-run BMI Effect	Calories	Long-run BMI Effect	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: *Meal-Level* (mean = 697.8 calories)						
Eat at Restaurant	0.163	338.8	1.74	237.6	1.22	3,920
		(46.0)	(0.24)	(23.8)	(0.12)	
Panel B: *Daily-Level* (mean = 2,061.8 calories)						
Eat at Restaurant	0.408	214.2	1.12	34.6	0.18	1,591
		(53.0)	(0.28)	(41.1)	(0.21)	

Note: This table presents an analysis of caloric intake by obese and overweight 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, one, or two meals at a restaurant. Column (1) shows the frequency of restaurant meals (percent of meals at restaurants in Panel A and average number of restaurant meals per day in Panel B). Columns (2) and (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 and dinner (meal-level regressions only), the day of the week, and whether an individual reported eating more because of a social occasion or extreme hunger. Standard errors corrected for within-household correlation in the error term are reported in parentheses. Columns (3) and (5) translate the coefficients from columns (2) and (4) into the total effect of eating at restaurants on BMI for the average obese or overweight rural individual. The formulas used for translation are described in the Section 6. The results reveal why restaurants do not cause obesity, even though the portions consumed in restaurants are relatively large: (1) people who frequent restaurants eat more than those who do not, even when they are not eating out; and (2) people who eat at restaurants compensate for larger restaurant portions by consuming less throughout the rest of the day.

Table 10: Potential Deadweight Loss from 50% Restaurant Tax

	Cost						
Demand Elasticity	Consumer Welfare Loss ($ billion)	–	Government Revenue ($ billion)	=	Deadweight Loss ($ billion)	Optimistic Benefit ($ billion)	Cost-Benefit Ratio
-0.5	134.1		121.8		12.3	1.4	8.8 : 1
-1.0	121.0		99.4		21.5	1.4	15.3 : 1
-2.0	99.4		66.3		33.1	1.4	23.6 : 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 because of rounding.) The benefit is calculated under the optimistic assumption that the tax would reduce the prevalence of overweight and obese individuals by 0.8 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. In all cases, the costs of a tax on restaurant food are significantly greater than the benefits.

Table A1: Reduced Form Effect of Interstate Proximity on Obesity by Demographic Group

Sample:	Males	Females	Under 50 Years Old	Over 50 Years Old	College Educated	High School Educated	Income Under \$40K/Year	Income Over \$40K/Year
Dependent Variable								
i) Obese	0.000	-0.001	0.009	-0.009	0.004	-0.004	-0.010	0.005
(BMI ≥ 30)	(0.013)	(0.011)	(0.010)	(0.013)	(0.013)	(0.012)	(0.014)	(0.012)
ii) Overweight	-0.019	0.002	0.002	-0.010	-0.006	-0.008	-0.011	0.015
(BMI ≥ 25)	(0.014)	(0.014)	(0.014)	(0.015)	(0.014)	(0.014)	(0.014)	(0.015)
iii) BMI	-0.057	0.052	0.069	-0.029	0.004	0.011	-0.037	0.137
	(0.146)	(0.166)	(0.155)	(0.169)	(0.169)	(0.150)	(0.186)	(0.163)
Observations	5,613	7,857	6,827	6,643	6,232	6,005	5,475	4,635

Note: In this table, each coefficient represents a separate regression. Each row corresponds to a different dependent variable, and each column corresponds to a different demographic group. 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 state-by-year fixed effects. Standard errors corrected for within-prefix correlation in the error term are reported in parentheses. The results indicate that proximity to highways has no significant effect on obesity for any of these demographic subgroups.

Table A2: Effect of Restaurant Access on Obesity (2SLS Models)

	(1)	(2)	(3)	(4)
Panel A: *Effect of Being One Mile Closer to a Restaurant on:*				
Dependent Variable				
i) Obese	0.007	0.007	0.008	0.011
(BMI ≥ 30)	(0.007)	(0.007)	(0.008)	(0.009)
ii) Overweight	-0.003	-0.003	0.002	0.006
(BMI ≥ 25)	(0.007)	(0.007)	(0.007)	(0.007)
iii) BMI	0.054	0.054	0.043	0.099
	(0.090)	(0.090)	(0.080)	(0.083)
Panel B: *Effect of Lowering Restaurant Prices by $1 on:*				
Dependent Variable				
i) Obese	0.005	0.005	0.006	0.008
(BMI ≥ 30)	(0.005)	(0.005)	(0.005)	(0.006)
ii) Overweight	-0.002	-0.002	0.001	0.004
(BMI ≥ 25)	(0.005)	(0.005)	(0.005)	(0.005)
iii) BMI	0.039	0.039	0.031	0.070
	(0.064)	(0.064)	(0.057)	(0.059)
Covariates	No	Yes	No	Yes
Sample:				
Population Density Cutoff (People per Sq Mile)	80	80	40	40
Observations	8,266	8,266	4,709	4,262

Note: This table reports estimates from two-stage least squares regressions using observations for which we know the exact ZIP code of residence with 97 percent confidence or greater. Each coefficient represents a separate regression. 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. Regressions with covariates include the following controls: gender, a quadratic in age, indicators for educational attainment, employment, unemployment, and marital status. Standard errors corrected for within-ZIP code correlation in the error term are reported in parentheses. These estimates indicate that there is no economically or statistically significant effect of restaurant access on body mass or obesity.