Online Appendix for "Are Restaurants Really Supersizing America?"

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A. Construction of Main Analytic Sample

The obesity data used in this study come from a confidential extract of the Behavioral Risk Factor Surveillance System (BRFSS), an ongoing, large-scale telephone survey. BRFSS randomly samples phone numbers using a disproportionate stratified sample method. Within each household, individuals aged 18 or older are randomly selected for interviews. Business and nonworking phone numbers are omitted (CDC 2006).

The Centers for Disease Control (CDC) does not release geographic identifiers for the BRFSS 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). 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.

The analytic sample consists of all telephone exchanges 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. The 80 persons per square mile threshold was chosen because it

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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 or raising the threshold to 160-persons per square mile (reported below) and other variations in the parameters (reported in Appendix Figure A3).

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). 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 miles of an Interstate is thus likely to contain that Interstate. We chose 10 miles as the maximum distance cutoff for nonadjacent towns. Although raising this cutoff would increase the sample size, we worried ex ante that it may reduce the credibility of the exclusion restriction. Our results are in fact robust to any number of alternative cutoff values, as shown in Appendix Figure A3.

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). We draw the distinction between highway exits and highway placement because of large differences in their associated adjustment 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 confidentiality issues (see above).

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). 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 (p = 0.100; 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.

B. 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 Appendix E) 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 in the paper.

C. 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.[1] 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).

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.[2] 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.[3]

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.[4] 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

[1] All values in this section are expressed in 2007 dollars. Costs are computed as weighted averages from 1990 to 2005, with each year weighted by the number of observations that it contributes to our analytic sample. Including tire wear and expected accident costs would increase the estimated costs by approximately 40 percent.

[2] 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 B.

[3] This estimate is consistent with estimates from Chiou (2009) for how far a rural consumer is willing to travel to save one dollar on a DVD purchase.

[4] 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.

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).[5] We arranged for the survey vehicle to not be visible from the roadway, so that drivers would not modify their speed in response to seeing the survey vehicle.[6]

The median driver's speed on this rural roadway was 73 mph.[7] At speeds in excess of 60 miles per hour (mph), fuel economy declines at an average of 1.5 percent per 1 mph (Davis and Diegel 2007).[8] The average driver therefore trades off 4.01 gallons of gasoline for each hour of time savings when traveling at 73 mph.[9] 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.

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

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

[7] The maximum speed was 94 mph.

[8] 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.

[9] 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.

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.

D. Details of TS2SLS Estimator

Because TS2SLS uses two samples, the sample upon which the first stage is estimated does 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 in the paper, 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 A4 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.

E. Can Measurement Error Explain the Null Relationship Between Restaurants and Obesity?

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.[10] The first-stage estimates and the

[10] 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.

restaurant 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, attenuating (or exaggerating) 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 6,096 Census blocks. For each Census block, we compute the distance to the nearest restaurant along the United States road network.[11] 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).

[11] We identify restaurant locations using the *Yahoo! Yellow Pages*. Distances along the United States road network are computed using ArcGIS.

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.[12] (Note that distance for any given ZIP code may still be measured with error, but that this does not affect our conclusions. As long as the distance is measured correctly on average, the first-stage estimates of the effect of Interstate proximity on the mean distance to the nearest restaurant will be correct.) 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 certainty. 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).[13] These coefficients are economically and

[12] If our first-stage estimates were overstated by 20 percent, then the estimated TS2SLS coefficient would be 17 percent too low (see equation 9). 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.

[13] All regressions contain state-by-year fixed effects. The corresponding full sample estimates are in Column (1) of Table 3 in the paper.

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 no economically or statistically significant effect of restaurant access on any measure of obesity (see Appendix Table A4).

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

F. Restaurant Survey Instrument

To conduct the restaurant survey, we hired three UC Berkeley undergraduate students. Above-market wages were paid to ensure a large pool of candidates, and the students were selected based upon strong recommendations from previous employers and demonstrated communications ability. We explained to the students that the aim of the survey was to document the geographic distribution of restaurant customers, but we did not inform them of our study's larger goals. After several trial runs under direct supervision at local restaurants, the students were dispatched in teams of two to conduct surveys in the survey area.

When surveying a restaurant, one student surveyed drive-through customers while the other student surveyed walk-in customers. Under this system, 100 percent of customers that entered the restaurant during the survey period were approached (and 93 percent agreed to the survey). Only ZIP code and town of residence information was solicited from customers; a waiver of informed consent was obtained from the UC Berkeley Committee for the Protection of Human Subjects based upon the fact the survey responses contained no individually identifying information. Restaurant employees were not surveyed. We did not solicit permission from the restaurants prior to conducting the surveys, but the restaurant employees and management were cooperative nevertheless.

G. Policy implications

The results presented in the paper suggest that eating at restaurants has no appreciable causal effect on BMI or obesity. 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 taxes on restaurant food and zoning regulations that limit the number of restaurants. Los Angeles has banned new fast food restaurants in some high obesity neighborhoods and similar policies have been proposed in New York and other U.S. cities (Fernandez 2006; Abdollah 2007).[14]

Here we consider the effects of a hypothetical tax on restaurant food such as that proposed by Ebberling, Pawlak, and Ludwig (2002) in a leading medical journal. The tax rates imposed under existing "sin taxes," such as on alcohol and tobacco, vary by state, with a median tax of 2.1 percent for beer and 48.3 percent for cigarettes (Boon 2007; Tax Foundation 2007; U.S. Department of Labor 2007).[15] We therefore consider a tax of 50 percent to be at the upper limit of a plausible restaurant sin tax, and ultimately conclude that even such an extreme policy would have minimal effect on obesity. Since the average restaurant meal costs $7.94 (excluding

[14] Many small towns throughout the country also restrict or ban fast-food and chain restaurants (Abdollah 2007). These earlier regulations were often tied to aesthetics or to the protection of smaller businesses, rather than to health concerns.
[15] The cigarette tax includes both federal and state taxes. There is no federal tax on alcohol.

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).[16]

The point estimates from the first column of Table 4 in the paper 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 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). We calculate the deadweight loss of a tax as the decrease in consumer surplus minus the government revenue collected. (If the tax also reduces firm profits – for example, because of a reduction in returns to scale – then the deadweight loss would be even greater.) To compute the consumer welfare loss, we use estimates for the own-price elasticity of demand for restaurants. Appendix Table A7 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

[16] 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.

welfare lost ranges from \$99.4 billion to \$134.1 billion per year, and the total deadweight loss ranges from \$12.3 billion to \$33.1 billion per year.[17]

Our instrumental variables point estimates (presented in Section III of the paper) 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 Appendix Table A7).[18] The last column of Appendix Table A7 combines this estimate of the benefits with estimates of the deadweight loss from the previous paragraph to compute the ratio of the welfare

[17] 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).

[18] 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.

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.[19]

Although we find that restaurant taxes and zoning regulations are unlikely to reduce obesity, the estimates presented in this paper do not rule out the effectiveness of other possible restaurant regulations. For example, it is possible that posting calorie content or regulating restaurant advertising might help reduce body weight (Bollinger, Leslie, and Sorenson 2009; Elbel, et al. 2009).[20] However, our results on calorie source substitution suggest that the effectiveness of these regulations likely relies on whether they can influence overall eating behaviors, not just at restaurants but also at home. It is also possible that there are benefits to regulating restaurant food besides encouraging weight loss. Banning trans fats, for example, may improve cardiac health even if it does not reduce body weight. Quantifying the magnitude of such possible non-obesity health effects of regulating restaurants is beyond the scope of this research.

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

[20] Using differences-in-differences research designs, Bollinger, Leslie, and Sorenson (2009) and Elbel et al. (2009) find that the New York City calorie posting law had a modest impact or no impact on restaurant purchases.

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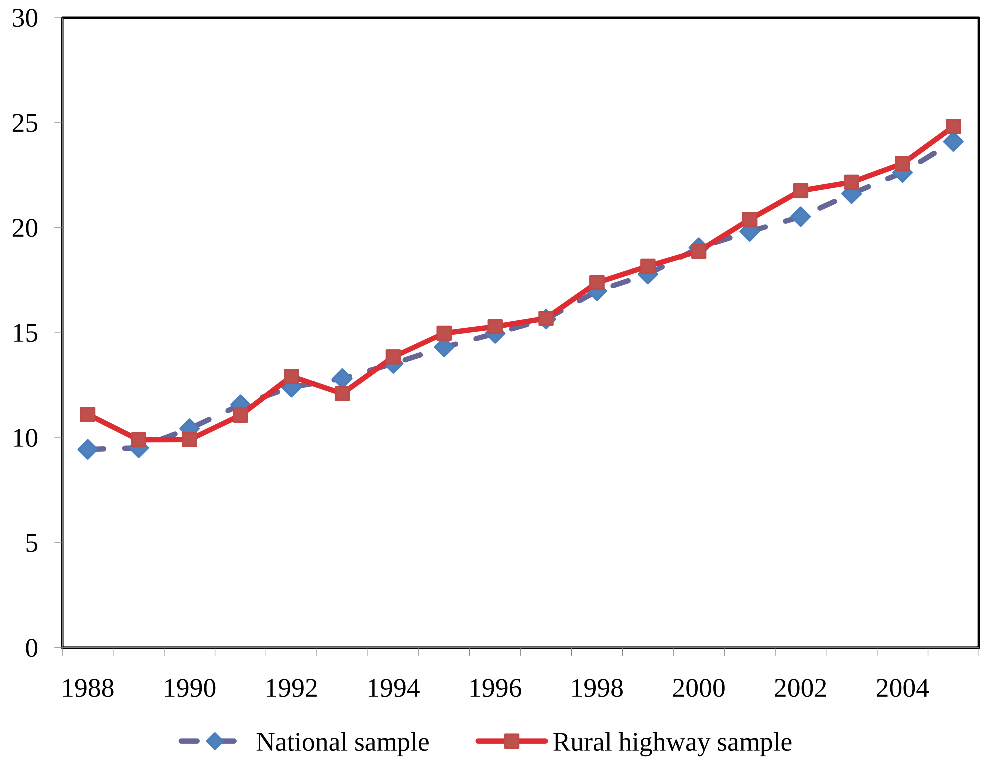
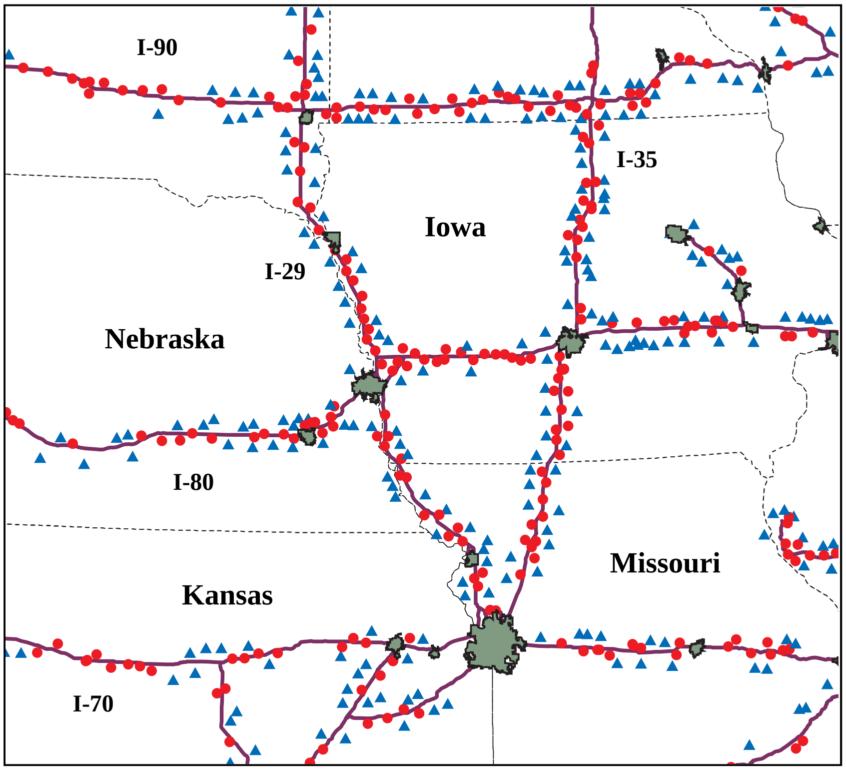


APPENDIX FIGURE A1. OBESITY RATES IN THE RURAL HIGHWAY AND NATIONAL SAMPLES, 1988-2005

Notes: This figure plots the obesity rates in the United States overall and in a rural highway sample from 1988 through 2005, based on the Behavioral Risk Factor Surveillance System. The rural highway sample is restricted to areas that lie in our sample states, contain highways, and have population density below 80-persons per square mile.

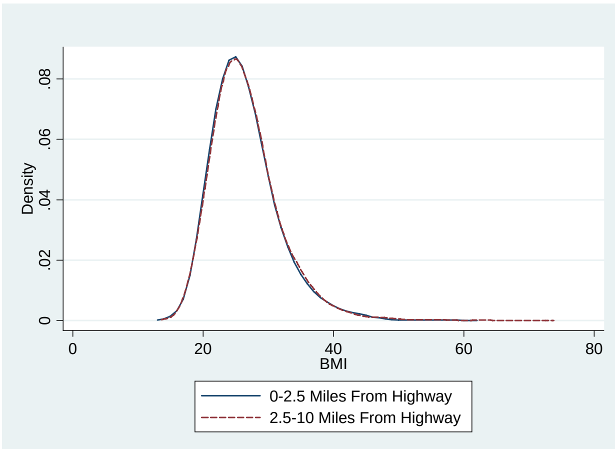


● Adjacent to highway ▲ Nonadjacent to highway

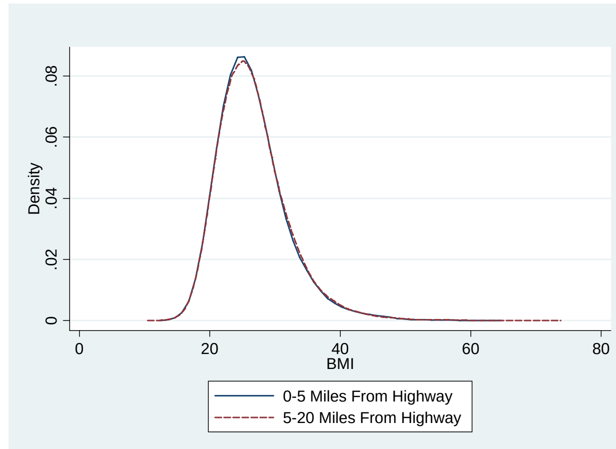
APPENDIX FIGURE A2. SAMPLE DESIGN

Notes: This figure shows rural towns in several states that meet the eligibility requirements for our study. We compare two groups of towns: "adjacent" towns are 0-5 miles from an Interstate Highway and "nonadjacent" towns are 5-10 miles away. All sample towns are also located at least 30 miles from an urban area and have a population density of less than 80 persons per square mile.

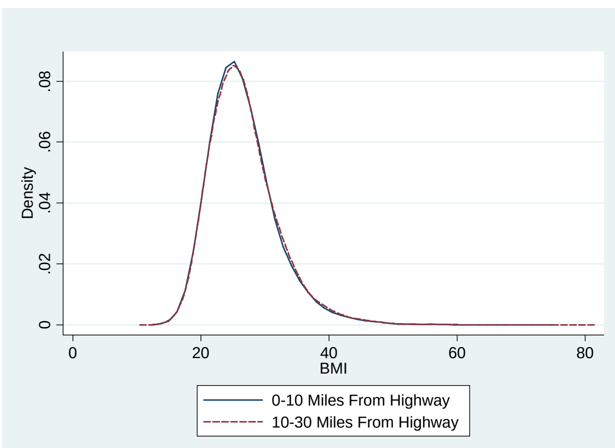
Distance: 0-2.5 miles vs. 2.5-10 miles



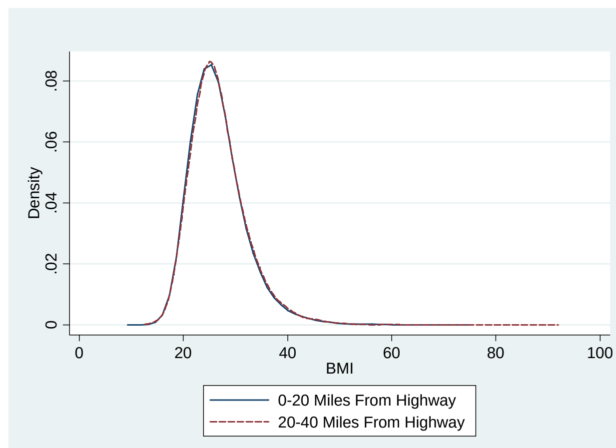
Distance: 0-5 miles vs. 5-20 miles



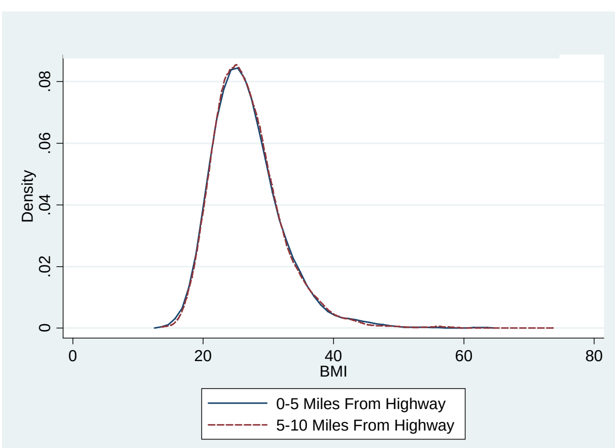
Distance: 0-10 miles vs. 10-30 miles



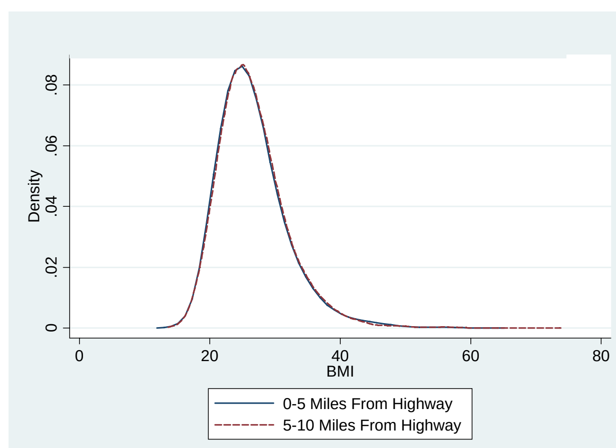
Distance: 0-20 miles vs. 20-40 miles



Population density < 40 people / mile^2

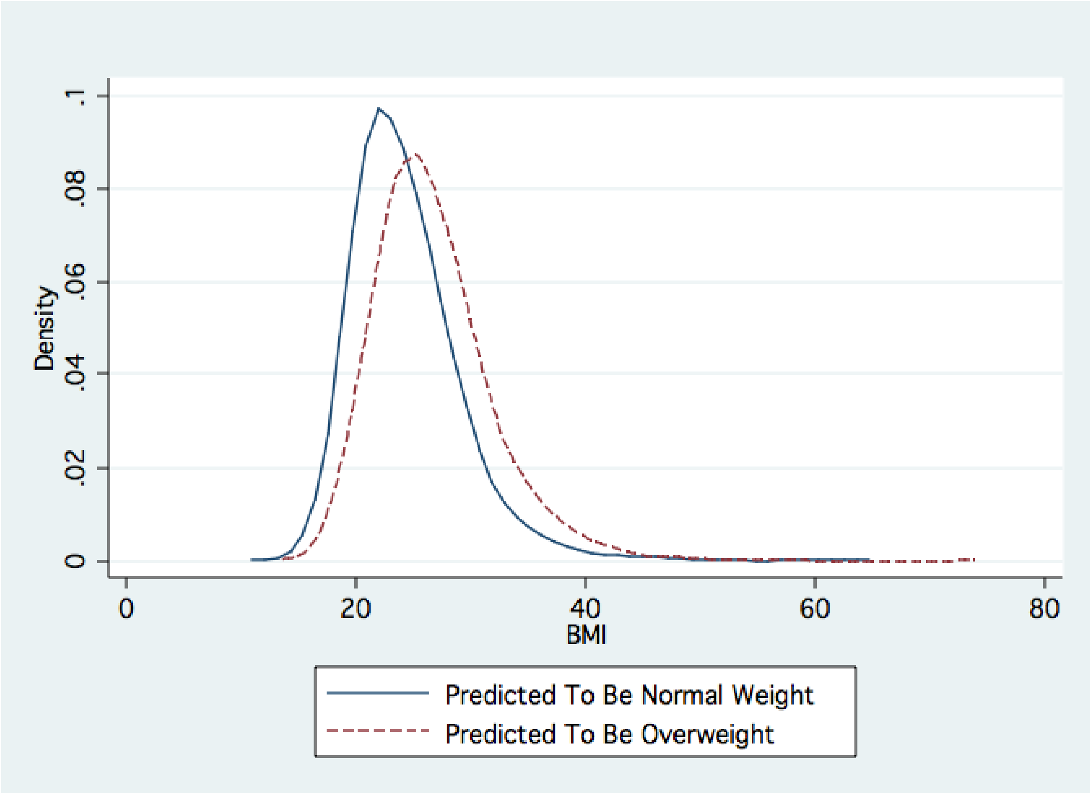


Population density < 160 people / mile^2



APPENDIX FIGURE A3. ROBUSTNESS OF HIGHWAY ANALYSIS TO ALTERNATIVE SPECIFICATIONS: DISTRIBUTION OF BODY MASS INDEX IN TOWNS ADJACENT AND NONADJACENT TO INTERSTATE HIGHWAYS

Notes: This figure examines the robustness of Figure 2 to alternative specifications for highway proximity and population density.



APPENDIX FIGURE A4. DISTRIBUTION OF BODY MASS INDEX FOR INDIVIDUALS PREDICTED TO BE NORMAL WEIGHT VERSUS OVERWEIGHT BY THE BMI RISK INDEX

Notes: 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.

Appendix Table A1: Relationship between Restaurants and Caloric Intake
for Rural and Urban Individuals

	Frequency of Meals at Restaurant Type	Calories Per Meal	
		Between Estimator	Fixed Effects Estimator
	(1)	(2)	(3)
Rural Consumers			
Eat Meal at Full-Service Restaurant	0.077	289.8	256.1
		(46.5)	(28.0)
Eat Meal at Fast-Food Restaurant	0.083	238.5	206.6
		(49.7)	(22.8)
Urban/Suburban Consumers			
Eat Meal at Full-Service Restaurant	0.088	171.5	207.9
		(23.7)	(13.1)
Eat Meal at Fast-Food Restaurant	0.098	248.3	112.5
		(23.0)	(11.5)

Note: This table presents an analysis of caloric intake based on data collected by the U.S. Department of Agriculture. The sample includes individuals aged 18 or older. Column (1) shows the share of meals that are full-service and fast-food restaurant meals. Columns (2) and (3) report coefficients from regressions of caloric intake on indicators for whether a meal was eaten at a full-service or fast-food restaurant. Column (2) presents coefficients from between-individual regressions while Column (3) presents coefficients from within-individual (individual fixed effects) regressions. Other controls include indicators for lunch and dinner, 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.

Appendix Table A2: Correlation Between Obesity and Restaurants Per Capita

	National Sample	Restricted Rural Highway Sample
Dependent Variable: BMI		
Restaurants Per Capita	0.681	0.544
	(0.075)	(0.178)
Restaurants Per Capita Squared	-0.015	-0.012
	(0.002)	(0.006)
Dependent Variable: Obese		
Restaurants Per Capita	0.0392	0.0349
	(0.0043)	(0.0160)
Restaurants Per Capita Squared	-0.0009	-0.0008
	(0.0001)	(0.0005)
Observations	578,896	24,799

Note: This table reports estimates from least squares regressions of obesity measures on restaurants per 10,000 residents and the square of restaurants per 10,000 residents. These regressions replicate the preferred specification from Chou, Grossman, and Saffer (2004). All regressions control for state fixed effects, age, gender, education, and marital status. Standard errors corrected for within-state correlation in the error term are reported in parentheses.

Appendix Table A3: Reduced Form Effect of Interstate Proximity on Obesity by Demographic Group

Sample:	All	Males	Females	Under 50 Years Old	Over 50 Years Old	College Educated	High School Educated	Income Under $40K/Year	Income Over $40K/Year
<u>*Dependent Variable*</u>									
i) Obese	-0.001	0.000	-0.001	0.009	-0.009	0.004	-0.004	-0.010	0.005
(BMI ≥ 30)	(0.009)	(0.013)	(0.011)	(0.010)	(0.013)	(0.013)	(0.012)	(0.014)	(0.012)
ii) Overweight	-0.007	-0.019	0.002	0.002	-0.010	-0.006	-0.008	-0.011	0.015
(BMI ≥ 25)	(0.011)	(0.014)	(0.014)	(0.014)	(0.015)	(0.014)	(0.014)	(0.014)	(0.015)
iii) BMI	0.002	-0.057	0.052	0.069	-0.029	0.004	0.011	-0.037	0.137
	(0.127)	(0.146)	(0.166)	(0.155)	(0.169)	(0.169)	(0.150)	(0.186)	(0.163)
iv) Class II Obesity	0.003	0.001	0.004	0.006	0.000	-0.005	0.013	0.008	-0.002
(BMI ≥ 35)	(0.005)	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)	(0.008)
v) Severe Obesity	0.002	0.002	0.003	0.004	0.001	0.002	0.005	0.004	0.006
(BMI ≥ 40)	(0.002)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)
Observations	13,470	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 specified 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.

Appendix Table A4: Effect of Restaurant Access on Obesity (2SLS Models)

	(1)	(2)
Panel A: *Effect of Being One Mile Closer to a Restaurant on:*		
Dependent Variable		
i) Obese	0.007	0.007
(BMI ≥ 30)	(0.007)	(0.007)
ii) Overweight	-0.003	-0.003
(BMI ≥ 25)	(0.007)	(0.007)
iii) BMI	0.054	0.054
	(0.090)	(0.090)
Panel B: *Effect of Lowering Restaurant Prices by $1 on:*		
Dependent Variable		
i) Obese	0.005	0.005
(BMI ≥ 30)	(0.005)	(0.005)
ii) Overweight	-0.002	-0.002
(BMI ≥ 25)	(0.005)	(0.005)
iii) BMI	0.039	0.039
	(0.064)	(0.064)
Covariates	No	Yes
Observations	8,266	8,266

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.

Appendix Table A5: Effect of Restaurant Access on Obesity (TS2SLS Models)

Panel A: *Effect of Having Any Restaurant on:*	
Dependent Variable	
i) Obese	-0.005
(BMI ≥ 30)	(0.051)
ii) Overweight	-0.039
(BMI ≥ 25)	(0.062)
iii) BMI	0.013
	(0.726)
Panel B: *Effect of Having Any Limited-Service Restaurant on:*	
Dependent Variable	
i) Obese	-0.006
(BMI ≥ 30)	(0.058)
ii) Overweight	-0.045
(BMI ≥ 25)	(0.070)
iii) BMI	0.014
	(0.825)
Observations	13,470

Note: This table reports estimates from two-sample two-stage least squares regressions. Each estimate represents the effect of restaurant access (or limited-service restaurant access) on a different dependent variable. 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.

Appendix Table A6: Effects of Restaurants on BMI from Food Recall Data

	All Restaurants (Rural Areas)	All Restaurants (All Areas)	Full-Service (Rural Areas)	Fast-Food (Rural Areas)
Overweight Individuals	0.18	0.40	0.02	0.17
	(0.21)	(0.12)	(0.15)	(0.14)
Obese Individuals	0.16	0.33	0.21	0.00
	(0.41)	(0.21)	(0.31)	(0.26)
All Individuals	0.24	0.35	0.08	0.17
	(0.16)	(0.09)	(0.11)	(0.11)

Note: This table presents the total effect of restaurants on BMI by geographic area, restaurant type, and body type. The underlying results come from an analysis of caloric intake 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. The number of calories consumed during a given day is regressed on the number of meals consumed at a restaurant that day and a set of controls. The controls include individual fixed effects, indicators for the day of the week, and an indicator for whether an individual reported eating more because of a social occasion or extreme hunger. The caloric effects of eating at restaurants are translated into steady-state BMI effects using the formulas described in Section 6. Standard errors corrected for within-household correlation in the error term are reported in parentheses.

Appendix Table A7: 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 4). It uses 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.